READINGS ABOUT

The Social Animal

Eleventh Edition

EDITED BY *Joshua Aronson and Elliot Aronson*

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The Social Animal

Edited by

Joshua Aronson
New York University

Elliot Aronson
University of California, Santa Cruz

Worth Publishers

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Acquisitions Editor: Erik Gilg
Marketing Manager: Jennifer Bilello
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To my mentors, Ned Jones (1927–1993),
Claude Steele, and Elliot Aronson (of course)
—JA

To my bigger, smarter brother,
Jason Aronson (1929–1961)
—EA

"*The most incomprehensible thing about the world is that it's comprehensible.*"—Albert Einstein

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Contents

PREFACE XI
AN OPEN LETTER TO THE READER XIII

I. INTRODUCTION: REFLECTIONS ON THE RESEARCH PROCESS 1

1. Research in Social Psychology as a Leap of Faith 3
   Elliot Aronson
2. On Baseball and Failure 10
   Elliot Aronson

II. CONFORMITY AND OBEDIENCE 15

3. Opinions and Social Pressure 17
   Solomon E. Asch
4. Behavioral Study of Obedience 27
   Stanley Milgram
5. "From Jerusalem to Jericho": A Study of Situational and Dispositional Variables in Helping Behavior 41
   John M. Darley and C. Daniel Batson

6. A Focus Theory of Normative Conduct: Recycling the Concept of Norms to Reduce Littering in Public Places 56
Robert B. Cialdini, Raymond R. Reno and Carl A. Kallgren

7. Making Sense of the Nonsensical: An Analysis of Jonestown 80
Neal Osherow

III. MASS COMMUNICATION, PROPAGANDA, AND PERSUASION 99

8. Effects of Varying the Recommendations in a Fear-Arousing Communication 101
James M. Dabbs Jr., and Howard Leventhal

9. Attribution Versus Persuasion as a Means for Modifying Behavior 112
Richard L. Miller, Philip Brickman, and Diana Bolen

10. The Impact of Mass Media Violence on U.S. Homicides 132
David P. Phillips

IV. SOCIAL COGNITION 147

11. Contrast Effects and Judgments of Physical Attractiveness: When Beauty Becomes a Social Problem 149
Douglas T. Kenrick and Sara E. Gutierres

12. The Effect of Attitude on the Recall of Personal Histories 164
Michael Ross, Cathy McFarland, and Garth J. O. Fletcher

13. The Peculiar Longevity of Things Not So Bad 176
Daniel T. Gilbert, Matthew D. Lieberman, Carey K. Morewedge, and Timothy D. Wilson

14. Strangers to Ourselves: Discovering the Adaptive Unconscious 189
Timothy D. Wilson

15. Self-Theories of Intelligence 201
Carol S. Dweck

V. SELF-JUSTIFICATION 217

16. Dissonance, Hypocrisy, and the Self-Concept 219
Elliot Aronson

17. Compliance Without Pressure: The Foot-in-the-Door Technique 237
Jonathan L. Freedman and Scott C. Fraser

18. Reducing Weight by Reducing Dissonance: The Role of Effort Justification in Inducing Weight Loss 249
Danny Axsom and Joel Cooper

19. Dishonest Behavior as a Function of Differential Levels of Induced Self-Esteem 263
Elliot Aronson and David R. Mettee

20. Using Cognitive Dissonance to Encourage Water Conservation 275
Chris Ann Dickerson, Ruth Thibodeau, Elliot Aronson, and Dayna Miller

21. A Self-Affirmation Intervention to Reduce the Racial Achievement Gap 288
Geoffrey L. Cohen, Julio Garcia, Nancy Apfel, and Allison Master

VI. HUMAN AGGRESSION 301

22. The Effects of Observing Violence 303
Leonard Berkowitz

23. Does Venting Anger Feed or Extinguish the Flame? Catharsis, Rumination, Distraction, Anger, and Aggressive Responding 315
Brad J. Bushman

24. Peacetime Casualties: The Effects of War on the Violent Behavior of Noncombatants 329
Dane Archer and Rosemary Gartner

25. Deindividuation and Anger-Mediated Interracial Aggression: Unmasking Regressive Racism 341
Ronald W. Rogers and Steven Prentice-Dunn

26. Predictors of Naturalistic Sexual Aggression 357
Neil M. Malamuth

VII. PREJUDICE 379

27. The Nonverbal Mediation of Self-Fulfilling Prophecies in Interracial Interaction 381
Carl O. Word, Mark P. Zanna, and Joel Cooper

28. Low Numbers: Stereotypes and the Underrepresentation of Women in Math and Science 394
Joshua Aronson

29. Experiments in Group Conflict 415
Muzafer Sherif

30. Jigsaw Groups and the Desegregated Classroom: In Pursuit of Common Goals 425
Elliot Aronson and Diane Bridgeman

31. Branch Rickey, Jackie Robinson, and the Social Psychology of Affirmative Action 436
Anthony R. Pratkanis and Marlene E. Turner

32. Prejudice as Self-Image Maintenance: Affirming the Self Through Derogating Others 445
Steven Fein and Steven J. Spencer

33. The Positive Feedback Bias 475
Kent Harber

VIII. LIKING, LOVING, AND INTERPERSONAL SENSITIVITY 495

34. Lending a Hand: Social Regulation of the Neural Response to Threat 497
James A. Coan, Hillary S. Schaefer, and Richard J. Davidson

35. The Search for a Romantic Partner: The Effects of Self-Esteem and Physical Attractiveness on Romantic Behavior 512
Sara B. Kiesler and Roberta L. Baral

36. Social Perception and Interpersonal Behavior: On the Self-Fulfilling Nature of Social Stereotypes 521
Mark Snyder, Elizabeth Decker Tanke, and Ellen Berscheid

37. Some Evidence for Heightened Sexual Attraction Under Conditions of High Anxiety 536
Donald G. Dutton and Arthur P. Aron

38. Cognitive, Interpersonal, and Behavioral Effects of Social Exclusion: How People Respond to Rejection 550
Roy F. Baumeister and Dianne M. Tice

39. Arbitrary Social Norms Influence Sex Differences in Romantic Selectivity 568
Eli J. Finkel and Paul W. Eastwick

NAME INDEX 581

SUBJECT INDEX 594

Preface

In *The Social Animal,* we attempt to paint a clear picture of the current state of our social-psychological knowledge and how such knowledge might be applied to alleviate some problems plaguing us in the world today. *The Social Animal* is intended to be concise, brisk, and lively. It is almost totally unencumbered by graphs, charts, tables, statistical analysis, or detailed methodological discussions. Although that kind of presentation provides an easy and even enjoyable introduction to the world of social psychology, many readers express a need to delve more deeply into the details of the research that forms the backbone of *The Social Animal.* To meet that need, we have edited this book, *Readings About the Social Animal.*

We have selected the readings that appear in this book in such a way that they both complement and supplement the material contained in the eleventh edition of *The Social Animal.* Not only are the sections organized to coincide with chapters in *The Social Animal,* but the specific readings also represent an attempt to amplify and elaborate on the major themes covered in that book. Moreover, we have been especially careful to choose readings that provide a mixture of classic and contemporary research. Some of the articles have been classics for decades. Others are quite recent. This combination will expose the reader to a glimpse of the history of social psychology as well as its future.

There is another way of classifying the articles in this collection. Most are reports of specific research as originally published in technical journals; others are more general pieces summarizing several studies on a given topic written by one of the major contributors to that area. A specific report, though not always easy to read, has the advantage of providing the detail necessary for enabling the reader to gain some understanding of exactly what goes into a piece of research. The summary article is usually less technical and, therefore, easier to read, offering a more panoramic overview of the subject by the people who know it best. In effect,

it enables the reader to look over an investigator's shoulder and see how he or she views an array of research on a given topic.

For this, the eleventh edition, we continue the process of blending the classic with the contemporary—and the specific with the panoramic. As the years roll by, it is gratifying to note that some of the articles first selected as contemporary for the 1972 edition of this book have now taken their place as genuine classics in 2011. Our hope is that *today's* contemporaries will likewise take their place as classics in the decades to come.

Joshua Aronson,
New York, New York

Elliot Aronson,
Santa Cruz, California

An Open Letter to the Reader

Welcome backstage. As mentioned in the Preface, there are two kinds of articles contained in this volume. Some selections are descriptions of research programs. These make exciting reading in that they describe in some detail a series of experiments aimed at explicating or extending a single idea. Other selections are reports of individual pieces of empirical research. These are equally exciting but sometimes get rather technical. We are sure some of you (teachers, graduate students, statisticians, and other dedicated types) will want to understand thoroughly every sentence of every article on the following pages, perhaps in the hope of planning some research of your own. *Bon voyage!* For your benefit, we have not abridged or changed a line of the original.

Our guess is that most of you do not require that amount of detail. Chances are, what you would like to get out of these articles is an understanding of what the investigator was trying to find out, how he or she went about the task, and how successful the outcome was. There is no better way to understand the research process than to read original reports. The adventure of reading an original report lies in your ability as the reader to put yourself in the shoes of the investigator as he or she transforms an idea into a viable set of research operations and tries to make sense of the results, which occasionally do not conform precisely to the predictions. Each of the original research reports contains four principal sections: (1) First there is an *introduction,* in which the author states the idea, where it came from, and why it's important, and distills the idea down to a hypothesis or series of hypotheses. (2) This is followed by a *procedure* section, in which the author tests the hypothesis by translating the idea into a concrete set of operations. In social psychology this frequently becomes a full-blown scenario designed to provide the participant (or subject) with a reasonable justification for responding to events without being allowed to know the true purpose of the procedure. The procedure section of a good piece of research is often the most interesting part because it requires a great deal of ingenuity for the

investigator to achieve precision without sacrificing realism or impact. (3) In the *results* section the investigator states as clearly and succinctly as possible what the findings were. The investigator uses various statistical procedures to ascertain the extent to which the data are reliable. (4) Finally there is a *discussion* section, in which the researcher evaluates and interprets the data presented in the preceding section and tries to make sense of them in the context of previous research. The creative researcher can also use this section to speculate about the implications of the data and to point the way toward future research.

To those of you who do not yet have much experience in reading research reports, we offer a few suggestions about which parts of the study to read carefully and which parts to skim. If the article contains a summary (either at the beginning or at the end), we recommend that you read that first to familiarize yourself with the general idea behind the piece of research and to learn quickly what the results were. Next, read the introduction carefully to learn the history of the idea and to understand the hypothesis thoroughly. Then read the procedure section pretty carefully. Finally, skim the results section just to see the extent to which the findings agreed with the predictions. If the findings did not fit the predictions, look closely at the discussion section to see how the author made sense of the results he or she did get and whether or not the explanation seems plausible to you. Unless you are adept at and/or intrigued by statistical analyses, we would advise you to skim that section. For those of you who have little or no knowledge of statistical procedures, it would be terribly frustrating and would serve no useful purpose to bog yourself down in some of the details of the statistical analyses. These articles were selected because they were well done. It is probably safe for you to accept on faith that the analysis was performed competently.

I

INTRODUCTION: REFLECTIONS ON THE RESEARCH PROCESS

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1

Research in Social Psychology as a Leap of Faith

Elliot Aronson

It is an honor and a pleasure to be here among such a distinguished group of dedicated experimental social psychologists. When I was asked to deliver an Invited Address at this meeting, the suggested topic was: "What Ever Became of Elliot Aronson?" I'm sure the committee selecting the title had their tongues at least partly in their cheeks. At the same time, I think there may have been a kernel of seriousness embedded in that choice of a title. Because I haven't published a laboratory experiment in a few years, I suspect that there may be a feeling abroad that I might have decided to abandon the laboratory in favor of doing research in the imprecise domain of what some of us laughingly call "the real world." And I suppose that the folks who invited me might have been tweaking my nose about that alleged decision. Well, whether or not my nose was being tweaked, I decided to take the request seriously. Initially, my decision was to describe the research project in the "real world" that has me so excited that it has occupied a large proportion of my

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This chapter was delivered as an invited address at the 1976 Meeting of the Society of Experimental Social Psychologists at the University of California at Los Angeles. A slightly modified version appeared in *Personality and Social Psychology Bulletin*, Vol. 3, No. 2, Spring 1977, pp. 190–195, by permission of Division 8 of the American Psychological Association and of Sage Publications. Copyright © Sage Publications. Reprinted by permission.

research time for the past four or five years. This is an action research project aimed at exploring the consequences of building cooperative learning groups in elementary schools, and is a project I'm very excited about: Among other things, we are finding that the loss of self-esteem among ethnic children following desegregation (discovered by Gerard and Miller, 1975) is largely a function of the competitiveness of the traditional classroom. When we placed kids in cooperative learning groups (of a special kind) we reversed this trend and produced sharp increases in self-esteem and test performance among black and Chicano children (Aronson et al., 1975; Lucker et al., 1976; Aronson et al., 1966).

But after I arrived here, I realized that a heavy dose of data would be inappropriate for an after-dinner speech. My data might be too heavy a dessert. Besides, ever since I've been here, several of you have been urging me to keep it light and perhaps even to make it humorous. So, I decided to talk to you about the philosophy of science. After all, graduate students consider my approach to the philosophy of science to be extraordinarily light and some even find it to be hilarious.

In 1968 Merrill Carlsmith and I wrote a chapter in *The Handbook of Social Psychology* on experimentation in social psychology. We spent most of that chapter talking about sticky problems: the trials and tribulations involved in designing and conducting an experiment in this area. We wrote about ethical issues, about experimenter bias, about random samples, experimental realism, debriefing, and all kinds of things to watch out for. After we finished it, we read the thing over and we realized that it made experimentation sound like a terrible drag. Further, we realized that this impression didn't reflect our own excitement and enthusiasm about doing research in social psychology. So we included another paragraph in which we said, in effect, "Hey, it *does* sound like doing research in social psychology is difficult, problematical, and occasionally a pain in the ass—and this is all true—but we will have really misled you if we don't convey our major feeling about doing research in social psychology—and that is that, all things considered, it is great fun."

It's now several years since that chapter was published, and I think that it's about time that I explained at least some of what we meant by the statement that experimentation is fun. And I'd like to do it in terms of a metaphor.

The metaphor I'd like to make use of occurs in a novel by Camus called *The Plague.* The backdrop of the novel involves bubonic plague that is ravaging a town on the coast of Algeria. One of the major characters is Monsieur Grand—an amiable sort of man who is writing a book. He wants that book to be absolutely perfect. He wants every sentence to be flawless, every paragraph to be magnificent, every page to be fantastically beautiful. He wants it to be perfect to the extent that, when he sends it to a publisher, the publisher will read the first sentence and be so struck by it that he will stand up and say to his colleagues, "Ladies and Gentlemen, hats off."

Monsieur Grand spends a lot of time writing that first sentence—the sentence is about a woman riding a horse in the park, but not just any woman, horse, or park. "One fine morning in the month of May an elegant young horsewoman might have been seen riding a handsome sorrell mare along the flowery avenues of the Bois de Boulogne."

But he's not satisfied. Does each noun have too many qualifiers? Too few? Does each qualifier convey precisely what he intends? Does each word properly convey the rhythm of a cantering horse? Would "flower-strewn" be preferable to "flowery"? Eight or nine months go by and he's still working on that first sentence—he has 50 manuscript pages all on that first sentence, because, as you know, he wants to be so good that the publisher will say to his colleagues, "Ladies and Gentlemen, hats off."

One day as he is working on his manuscript, he becomes ill; as his symptoms develop, it soon becomes clear that he has contracted bubonic plague. His physician—who is also his friend—examines him and says, "I'm really sorry to have to tell you this, but you're going to die; you don't have much longer to live." So Monsieur Grand orders his physician to destroy his manuscript. He issues this order with such assertiveness and such strength that the physician immediately takes the manuscript—this 50 pages of one sentence meticulously honed and sharpened—and throws it into the fire.

The next day Monsieur Grand recovers. And he says to his friend, "I think I acted too hastily." This is incredibly ironic, of course; the one action on his part that was spontaneous was in the interest of *destroying* the thing that he had created with anything but spontaneity.

So much for the metaphor. Basically, I believe that there are two very different ways to do science. One is in the slow, methodical, one-step-at-a-time manner exemplified by Monsieur Grand. This involves a great deal of meticulous honing, sharpening, and polishing of the design and operations. This may require several months. When the researcher is ready to leave the drawing board, he runs the study, and after he runs a few subjects he realizes that there are some things about it that are not perfect. So he stops and brings it back to the drawing board and works some more, sharpening and honing. He then runs a few more subjects and notices something else that can be improved. Several years later he may have the kind of experiment that, when he submits it to the journal, the editor will say to his colleagues, "Ladies and Gentlemen, hats off."[1]

There's another way to do science. This involves, in effect, sketching a study in quick, broad strokes, pilot testing it—seeing where you went wrong, recasting it, and then running it as best as you can at the moment. As you finish the study and begin to write it up, you may realize that if you had it all to do over again you would have done it better. Of course; learning always takes place as a function of experience—even in the researcher. But in this approach, instead of going back to the drawing board to design that mythical "perfect" study that will produce a "hats off" response, you finish the write-up and submit it for publication—relying on the notion that science is a self-corrective enterprise. Self-corrective in the following sense: I know that if I do a study that *isn't* perfect, it will soon be improved upon by others. Thus, my goal is to get it into the literature to give my colleagues a chance to look at it, be stimulated by it, be provoked by it, annoyed by it, and then go ahead and do it better—even if their intent

[1]Even under these circumstances the "hats off" response is unlikely; chances are the editor will find some flaws that the author overlooked.

is to prove me wrong, and even if they succeed in proving me wrong. That's the exciting thing about science; it progresses by people taking off on one another's work. This is what William James (1956) called the "leap of faith." I have faith that if I do an imperfect piece of work, someone will read it and will be provoked to demonstrate this imperfection in a really interesting way. This will almost always lead to a greater understanding of the phenomenon under investigation. And maybe, after that other person does his research, the editor and the publisher and the world at large will say, "Hey, hats off." And that's OK.

Needless to say, I prefer the second way—as those of you who are familiar with my imperfect research can readily gather. And this leap of faith forms a vital aspect of my philosophy of science. Namely, that we don't have to make things perfect before we expose them to other people's thinking, criticism, and actions. It's more exciting, it's more yeasty, it's more provocative to treat science in big, broad brush strokes rather than meticulously polishing and honing and taking several years before coming out with a finished product. Since I believe that science is a self-correcting enterprise, I would prefer to be provocative than right. Of course, it goes without saying that I do not attempt to be wrong or sloppy. I attempt to do the best I can at the moment and share that less than perfect product with you—my colleagues and critics. William James maintains that there is an immense class of cases where faith creates its own verification. To use one of James's examples—if while climbing a mountain you must leap over a precipice, your belief that you can do it increases the probability of a successful outcome. To extrapolate our confidence that others will be excited by our research and provoked to carry it further or prove it wrong induces us to conduct research in a manner and publish it at a time that maximizes the probability of the research providing the yeast for this kind of empirical convocation.

It is now several years since Carlsmith and I wrote that chapter in the *Handbook* in which we very blithely announced that experimentation in social psychology was fun. And as I read the journals, it seems to me that a lot of the fun has gone out of social psychology. One of the reasons for this, I think, is that we've gotten much too cautious, much too careful, much too afraid to be wrong, and it's taken a lot of the zest and a lot of the yeast out of the research in social psychology.

One of the by-products of excessive caution is excessive self-consciousness. Indeed, one of the most characteristic aspects of contemporary social psychology is a recent trend toward discipline-wide self-consciousness, handwringing, and "kvetching." By conservative estimate, in the past five or six years I must have been invited to at least a half dozen symposia with titles like "Where Is Social Psychology Heading?" or "What Must We Do About Social Psychology?" or "Whither Social Psychology?" (with or without the "h" in whither). This kind of self-consciousness is a bore. Let me be clear. I'm not opposed to a certain degree of self-consciousness on an individual level. Indeed, I believe that any individual scientist should take stock of herself every few years and give herself the opportunity to re-order her priorities. Every few years it is probably quite useful to ask: What is important? What are my ethical and societal concerns? Although this process is important for an individual, when an entire discipline does it, the implication is that there

is a particular place we should all be going, a particular methodology we should all be using, particular topics we should all be studying. I think that that kind of self-consciousness is deadly and stultifying. The proper question is "What might *individuals* be doing?", not "What should the *field* be doing?" I like to think of the discipline of social psychology as a really large circus tent where a lot of different acts are going on and the acts occasionally cross, intermingle, and overlap. In that way each individual is doing what that person thinks is the most interesting or useful thing to do *and* is being challenged all the time by the existence of other people in the science who are engaged in overlapping topics employing different methods or different topics employing overlapping methods.

Watch out—I feel another metaphor coming on. There is a short story by J. D. Salinger entitled *Seymour: An Introduction* in which Buddy Glass, who is a budding writer, presents his short stories as he writes them to his older brother, Seymour, for criticism. Seymour, who is a wise and good man, generally writes these criticisms in the form of a letter. After one story, Seymour writes Buddy a letter (which Buddy keeps for many years afterwards) in which he says, in effect "You really are a terrific craftsman. You really know how to write. You really know how to put a sentence together; you've mastered the technique. You know how to take these sentences and string them together into paragraphs. Your stories are constructed beautifully. The one thing you haven't learned yet is what to write about, and that's a really important problem. As I was thinking about it, I came up with a solution that was so simple and so direct and so "obvious" that it boggles the mind. The solution is this: Just remember that before you were a writer you were a reader. Then, all you have to do is think about the one story, the one thing that you always wanted to read, and then sit down and write it."

As a scientist, I continually find myself trying to make some use of Seymour's advice. In effect, I say to myself: "Hey, remember that before you were a researcher you were a consumer of research. If you want to know what to do research on, just think of the one experiment on human social behavior that you always wanted to read about, and then go out and do it." I have always tried to follow my translation of Seymour's advice, and occasionally I have succeeded. It would be very dangerous to say to a group like this that I always succeed—that every experiment I do involves the one question that I always wanted to know the answer to—because that would open me up to ridicule. I can just hear you saying, "You mean the one thing you always wanted to know about human nature is what happens when people spill coffee all over themselves?" (Aronson, Willerman, and Floyd, 1966). It's quite a dangerous statement even if it were true—and it's not true.

But I've had my moments, and I can tell when I'm following Seymour's advice because I can feel my excitement rise, and I think I'm onto one of those questions now. My current research questions are: How can we turn the educational experience of millions of elementary school children into a less dehumanizing experience? How can we counteract the trend in American education toward lowering self-concepts of minority group people? How can we teach skills of cooperation painlessly and easily in the ordinary classroom situation? How can we make learning an exciting, interesting,

social psychological, as well as educational, venture? Since public schools are institutions that 95 percent of us and 95 percent of our children and 95 percent of our grandchildren are going to be going through, I see these as important issues.

But I promised you I wouldn't dwell on the research itself. What I do want to dwell on are my failures as a scientist—the times when I don't follow Seymour's advice. Occasionally I do research that I'm not particularly excited about. How come? Sometimes I find myself between ideas. Or sometimes the research I'm interested in is too hard to do or is taking a lot of time to set up. When this happens, instead of doing nothing, I get scared. Scared of what? Let me backtrack. Yesterday when we had a symposium about the editorial policy of our journals, my friend and former student, Darwyn Linder, talked to us eloquently about what he thought the major function of journals should be. He presented a discussion of three separate functions: One is the archival function; the journal serves as a permanent body of knowledge so that 50 years from now when people want to know what was going on in social psychology in the 1970s, they can look in the back issues of the journals. A second function it serves is as a way of exchanging information; if you want to know what is going on in various labs across the country right now at this moment, or at least what went on three years ago when the researchers did the experiment that finally got published, you can look in the current journal. The third function that Linder mentioned was that for the younger social psychologists—people who are teaching at universities without tenure—it serves a pragmatic function. That is, publishing in journals convinces deans that the young social psychologist is doing his job by piling up vast numbers of publications. But what I want to say is that the third function is not one that is limited to younger people who are desirous of tenure. I think it is something that afflicts some of us old guys, too, in a very different way. Whereas our jobs aren't dependent on publications, something else is. And that something—for a lack of a better term—I would call collegial esteem. What keeps me from consistently doing research on the one thing that I always wanted to find out about is that I get scared once in a while that if I'm not always active, not always producing things—anything—then perhaps some of my colleagues might think that I've lost it, and they may begin to ask questions like "Whatever became of Elliot Aronson?" which was indeed the suggested topic of this talk. It is conceivable to me that I'm not the only person here who has experienced that fear. If my suspicion is correct, then perhaps by discussing that hobgoblin we can, if nothing else, get rid of what Harry Stack Sullivan called "the fallacy of uniqueness." This may help us all to lay the hobgoblin to rest and get on with our proper business: for each of us in his own way to pursue as best as we can the answer to the one question he always wanted to know.

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2

On Baseball and Failure

Elliot Aronson

Acceptance Speech, SESP Distinguished Scientific Achievement Award, October 1994

I am deeply touched by this honor. I cannot imagine a more meaningful honor for a social psychologist than having one's scientific research recognized by *this* group. I have always believed that experimentation in social psychology is a family business. And, if that's true, then you are the family. The people in this room are the best scientists in this business. No one knows better than you about what it takes to do a good experiment in this area. Every person in this room is well aware that there is an art to this wonderful science of ours. And it is a demanding art, indeed.

It is arguably more difficult to do an experiment in social psychology than in any other scientific discipline for one simple and powerful reason: In social psychology, we are testing our theories and hypotheses on participants who are almost always intelligent, curious, experienced human beings who have spent their entire lives in a social environment—and have, therefore, formed their own theories and hypotheses about precisely the behaviors we are trying to study in the laboratory. That is to say, everyone in the world, including the participants in our experiments, is a social psychological theorist. That simple fact makes doing experiments in social psychology both challenging and exciting.

Our challenge is to find a way to circumvent or neutralize the theories that our participants walk in with—so that we can observe their true behavior—not how they think they should behave if their own theory were true. Our excitement? Ahhh, that's what the rest of this so-called speech is about! Actually, it's not a speech at all. As a chronic and habitual storyteller, it seems appropriate for me to

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This speech was prepared for delivery at the annual convention of the Society of Experimental Social Psychologists in 1994 by the recipient of its 12th annual Distinguished Scientific Achievement Award. Portions of this speech were published in *Dialogue* (a publication of the Society for Personality and Social Psychology), Spring 1995, pp. 4–5.

tell you a story. The story takes place early in 1941, when I was nine years old. That was an important year for me; it was the year I discovered social psychology.

I grew up in Revere, Massachusetts—a small, run-down, blue-collar city near Boston. Ours was the only Jewish family in a rough, passionately anti-Semitic neighborhood. When I was a child, my parents insisted that I attend Hebrew school, in addition to public school. The Hebrew classes were held very late in the afternoon, four days a week. Alas, the Hebrew school building was located clear on the other side of town, which meant that I used to have to walk through my own hostile neighborhood carrying my Hebrew books. I was like a walking advertisement, a beacon to the anti-Semitic kids: "Hey, look at me—I'm Jewish!" Simply getting to and from Hebrew school every day was something of an adventure: I was forever trying to find creative routes, zig-zag paths that would take me away from the greatest areas of danger. But, in spite of my best efforts, I was frequently waylaid, pushed around, and occasionally roughed up by gangs of teenaged tough guys who yelled anti-Semitic slogans.

One of my most vivid childhood memories involves sitting on a curb, nursing a bloody nose and a split lip, and wondering how these kids could possibly hate me so much when they didn't even know me. I wondered if they were born hating Jews, or if they needed to be taught such hatred; I wondered if there might be any way to get them to hate me less—like, if they had gotten to know me better as a person, before they knew I was Jewish, would that have diminished their hatred?

I didn't realize it at the time, of course, but these were profound social psychological questions. And some 10 years later, as an economics major at Brandeis, I wandered into an Intro. Psych. class being taught by Abraham Maslow—quite by accident. As it happened, Maslow was lecturing on the social psychology of prejudice, and I was astonished and delighted to discover that there was an entire profession where people actually got paid for thinking about those things and raising the same kinds of questions that I had raised when I was nine years old. I could hardly believe my ears! Wow, I thought, what a racket! And I immediately switched my major to psychology. And I've been thinking, Wow, what a racket, ever since! For over 35 years, I have been having so much fun doing social psychology that I keep expecting to hear a knock on my door in the middle of the night; the cops are going to arrest me for accepting money under false pretenses!

What's more, to my great personal satisfaction, some 30 years after I was sitting on that curb nursing my bloody nose and my split lip, I was able to invent a social psychological strategy for reducing prejudice in the schools—a strategy that not only teaches us something about human nature and human behavior, but one that really works—that really reduces prejudice and violence and thereby improves human lives. I feel blessed.

One of the things I love about social psychology is precisely that: It affords us the opportunity of doing both of those things simultaneously—of illuminating something important about human nature and doing some good in the world.

In my judgment, it is part and parcel of the mission of social psychology to uncover a lot of unappetizing things about human behavior—about mindless

conformity, about blind obedience to authority, about violence, about prejudice, and the like. This could be depressing if it were not for the fact that this discipline of ours offers us the opportunity for redemption in the form of application: in the form of finding ways to reduce mindless conformity, blind obedience, prejudice, and violence. And, of course, this redemption—this applied research—if it is done well, comes full circle and informs our theories and basic research. That is what I love about social psychology.

Needless to say, social psychology was not my first love. When I was nine years old, sitting on that curbstone in Revere, I sure as hell did not want to be a social psychologist. I did not even know there *was* such a thing. But even if I had known about social psychology, at that time I wanted to be a professional baseball player. Actually, my desire was much more specific than that: I wanted to play center field for the Boston Red Sox. (Of course, the Sox already had a center fielder at that time; but I was not greedy or impatient: I was willing to wait until he retired; I figured that would take about 10 years, and, by then, I would be ready!)

When I was a little kid, I worked hard at achieving my dream. Every spring, starting in late February, even before the snow had completely melted off the baseball field, a few of us were out there, shagging flies—hour, after hour, after hour.

And, with all that practice, I got pretty good at shagging flies—but I have a confession: In spite of hundreds of hours of practice, I never made the Red Sox. As a matter of fact, I could not even get to play center field on my high school team. I was much too slow a runner for that, and besides, I could not hit a curve ball to save my life. Indeed, as a ballplayer, I reached my peak at about the age of 14, since then, it has been downhill all the way! It is kind of sad to reach a peak at anything at age 14, but there you have it.

Fortunately, as I said, I eventually discovered social psychology. As a social psychologist, I expect to reach my peak any time now—or as we Red Sox fans are fond of saying, maybe next year!

Although I never made it as a ballplayer, I learned a few things playing baseball that proved to be useful to me as a social psychologist. It goes without saying that the game of baseball is vastly overused as a metaphor—but what the hell, why should I let a little thing like *that* stop me?

One of the things I like most about baseball is contained in the box score of a game: In baseball, they tally runs, hits, and errors. I like that because, generally speaking, we live in a society that is contemptuous of failure and punishes mistakes of all kinds. Because of this, we are under enormous pressure to deny our mistakes or minimize their importance. The danger of this attitude is that it makes people afraid to make mistakes and when people are afraid to make mistakes, they usually do not take many risks but play it close to the vest. Aronson's second law: People who are afraid to make mistakes are unlikely to make anything of great value. Moreover, if we are unselfconscious about our mistakes, we can look at them carefully and perhaps learn something important from them.

Playing baseball taught me how to deal with failure. It was a lesson I learned well when I was very young and for that, I will always be grateful. It goes without saying that I've made more than my share of mistakes—in life and in social psychology. (I will spare you my confessions of all my important failures as a human being and simply make reference to my failures in the lab.) In doing experiments, I screw up a lot—usually because I'm not as smart or skillful as I would like to be. Occasionally, I screw up merely because I'm trying to tackle a hypothesis that is elusive or difficult to test. I don't ever want to be deterred from trying to tackle a difficult hypothesis by my fear of screwing up.

In baseball, by including errors in the box score, they are saying, in effect, that it's not such a terrible thing to screw up—it's not desirable, of course, but screwing up is part of the game: a part of the game of baseball, a part of the game of life, a part of the game of social psychology. I've often thought that we should have a journal devoted to publishing interesting ideas that didn't work—and that people should submit these interesting failures with pride.

There are at least three good reasons for this: (1) Someone else may be able to pick up our fumble and run with it. (Whoops, a mixed athletic metaphor!) (2) It would be good for the self-confidence of our graduate students if they saw some of our failures on display as well as our successes. When grad students read the current journals, it creates the illusion that we old-timers are much more clever than we are. This would be harmless, except for the fact that they might get discouraged by this—concluding, erroneously, that they themselves would have a lot of difficulty measuring up. (3) Most important, it would make failure a legitimate part of the social psychology game—not just success. Like in baseball, errors are certainly not sought out—they are not desirable, per se—but the fear of error should not keep us from going after the difficult problems.

In the baseball box score, errors are tallied in the fielding part of the game—but the same kind of tolerance for imperfection is reflected in hitting. Consider this: If, as a batter, a ballplayer screws up two out of three times at bat, that will give him a lifetime batting average of .333; with a batting average of .333, he is likely to make it to the Hall of Fame!

When I thought about that for a little while, I realized that while I could never do nearly that well in hitting a baseball, .333 is just about my average as an experimentalist—that is, in designing and conducting experiments, I fail about twice as often as I succeed. If this honor you have so graciously given me today can be considered social psychology's equivalent of baseball's Hall of Fame, then I guess that means that I made it after all—even though I still can't hit a curve ball to save my life! I am very grateful that you found a way to make the dreams of a nine-year-old kid come true—after a fashion!

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II

CONFORMITY AND OBEDIENCE

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3

Opinions and Social Pressure

Solomon E. Asch

Exactly what is the effect of the opinions of others on our own? In other words, how strong is the urge toward social conformity? The question is approached by means of some unusual experiments.

That social influences shape every person's practices, judgments, and beliefs is a truism to which anyone will readily assent. A child masters his "native" dialect down to the finest nuances; a member of a tribe of cannibals accepts cannibalism as altogether fitting and proper. All the social sciences take their departure from the observation of the profound effects that groups exert on their members. For psychologists, group pressure upon the minds of individuals raises a host of questions they would like to investigate in detail.

How, and to what extent, do social forces constrain people's opinions and attitudes? This question is especially pertinent in our day. The same epoch that has witnessed the unprecedented technical extension of communication has also brought into existence the deliberate manipulation of opinion and the "engineering of consent." There are many good reasons why, as citizens and as scientists, we should be concerned with studying the ways in which human beings form their opinions and the role that social conditions play.

Studies of these questions began with the interest in hypnosis aroused by the French physician Jean Martin Charcot (a teacher of Sigmund Freud) toward the end of the nineteenth century. Charcot believed that only hysterical patients could be fully hypnotized, but this view was soon challenged by two other physicians, Hyppolyte Bernheim and A. A. Liébault, who demonstrated that they could put

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most people under the hypnotic spell. Bernheim proposed that hypnosis was but an extreme form of a normal psychological process which became known as "suggestibility." It was shown that monotonous reiteration of instructions could induce in normal persons in the waking state involuntary bodily changes such as swaying or rigidity of the arms, and sensations such as warmth and odor.

It was not long before social thinkers seized upon these discoveries as a basis for explaining numerous social phenomena, from the spread of opinion to the formation of crowds and the following of leaders. The sociologist Gabriel Tarde summed it all up in the aphorism: "Social man is a somnambulist."

When the new discipline of social psychology was born at the beginning of this century, its first experiments were essentially adaptations of the suggestion demonstration. The technique generally followed a simple plan. The subjects, usually college students, were asked to give their opinions or preferences concerning various matters; some time later they were again asked to state their choices, but now they were also informed of the opinions held by authorities or large groups of their peers on the same matters. (Often the alleged consensus was fictitious.) Most of these studies had substantially the same result: confronted with opinions contrary to their own, many subjects apparently shifted their judgments in the direction of the views of the majorities or the experts. The late psychologist Edward L. Thorndike reported that he had succeeded in modifying the esthetic preferences of adults by this procedure. Other psychologists reported that people's evaluations of the merit of a literary passage could be raised or lowered by ascribing the passage to different authors. Apparently the sheer weight of numbers or authority sufficed to change opinions, even when no arguments for the opinions themselves were provided.

Now the very ease of success in these experiments arouses suspicion. Did the subjects actually change their opinions, or were the experimental victories scored only on paper? On grounds of common sense, one must question whether opinions are generally as watery as these studies indicate. There is some reason to wonder whether it was not the investigators who, in their enthusiasm for a theory, were suggestible, and whether the ostensibly gullible subjects were not providing answers which they thought good subjects were expected to give.

The investigations were guided by certain underlying assumptions, which today are common currency and account for much that is thought and said about the operations of propaganda and public opinion. The assumptions are that people submit uncritically and painlessly to external manipulation by suggestion or prestige, and that any given idea or value can be "sold" or "unsold" without reference to its merits. We should be skeptical, however, of the supposition that the power of social pressure necessarily implies uncritical submission to it: independence and the capacity to rise above group passion are also open to human beings. Further, one may question on psychological grounds whether it is possible as a rule to change a person's judgment of a situation or an object without first changing his knowledge or assumptions about it.

FIGURE 3.1
Experiment is repeated in the Laboratory of Social Relations at Harvard University. Seven student subjects are asked by the experimenter *(right)* to compare the length of lines. Six of the subjects have been coached beforehand to give unanimously wrong answers. The seventh *(sixth from the left)* has merely been told that it is an experiment in perception. (Photograph by William Vandivert.)

In what follows I shall describe some experiments in an investigation of the effects of group pressure which was carried out recently with the help of a number of my associates. The tests not only demonstrate the operations of group pressure upon individuals but also illustrate a new kind of attack on the problem and some of the more subtle questions that it raises.

A group of seven to nine young men, all college students, are assembled in a classroom for a "psychological experiment" in visual judgment. The experimenter informs them that they will be comparing the lengths of lines. He shows two large white cards (see Figure 3.2). On one is a single vertical black line—the standard whose length is to be matched. On the other card are three vertical lines of various lengths. The subjects are to choose the one that is of the same length as the line on the other card. One of the three actually is of the same length; the other two are substantially different, the difference ranging from three quarters of an inch to an inch and three quarters.

The experiment opens uneventfully. The subjects announce their answers in the order in which they have been seated in the room, and on the first round every person chooses the same matching line. Then a second set of cards is exposed; again the group is unanimous. The members appear ready to endure politely another boring experiment. On the third trial there is an unexpected disturbance. One person near the end of the group disagrees with all the others in his selection of the matching line. He looks surprised, indeed incredulous, about the disagreement. On the following trial he disagrees again, while the others remain unanimous in their choice. The dissenter becomes more and more worried and hesitant

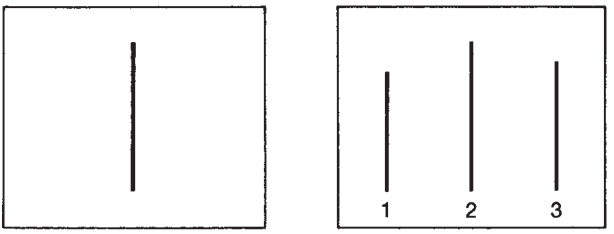

FIGURE 3.2
Subjects were shown two cards. One bore a standard line. The other bore three lines, one of which was the same length as the standard. The subjects were asked to choose this line.

as the disagreement continues in succeeding trials; he may pause before announcing his answer and speak in a low voice, or he may smile in an embarrassed way.

What the dissenter does not know is that all the other members of the group were instructed by the experimenter beforehand to give incorrect answers in unanimity at certain points. The single individual who is not a party to this prearrangement is the focal subject of our experiment. He is placed in a position in which, while he is actually giving the correct answers, he finds himself unexpectedly in a minority of one, opposed by a unanimous and arbitrary majority with respect to a clear and simple fact. Upon him we have brought to bear two opposed forces: the evidence of his senses and the unanimous opinion of a group of his peers. Also, he must declare his judgments in public, before a majority which has also stated its position publicly.

The instructed majority occasionally reports correctly in order to reduce the possibility that the naive subject will suspect collusion against him. (In only a few cases did the subject actually show suspicion; when this happened, the experiment was stopped and the results were not counted.) There are 18 trials in each series, and on 12 of these the majority responds erroneously. How do people respond to group pressure in this situation? I shall report first the statistical results of a series in which a total of 123 subjects from three institutions of higher learning (not including my own, Swarthmore College) were placed in the minority situation described above.

Two alternatives were open to the subject: he could act independently, repudiating the majority, or he could go along with the majority, repudiating the evidence of his senses. Of the 123 put to the test, a considerable percentage yielded to the majority. Whereas in ordinary circumstances individuals matching the lines will make mistakes less than 1 percent of the time, under group pressure the minority subjects swung to acceptance of the misleading majority's wrong judgments in 36.8 percent of the selections.

Of course individuals differed in response. At one extreme, about one quarter of the subjects were completely independent and never agreed with the erroneous

judgments of the majority. At the other extreme, some individuals went with the majority nearly all the time. The performances of individuals in this experiment tend to be highly consistent. Those who strike out on the path of independence do not, as a rule, succumb to the majority even over an extended series of trials, while those who choose the path of compliance are unable to free themselves as the ordeal is prolonged.

The reasons for the startling individual differences have not yet been investigated in detail. At this point we can only report some tentative generalizations from talks with the subjects, each of whom was interviewed at the end of the experiment. Among the independent individuals were many who held fast because of staunch confidence in their own judgment. The most significant fact about them was not absence of responsiveness to the majority but a capacity to recover from doubt and to reestablish their equilibrium. Others who acted independently came to believe that the majority was correct in its answers, but they continued their dissent on the simple ground that it was their obligation to call the play as they saw it.

Among the extremely yielding persons we found a group who quickly reached the conclusion: "I am wrong, they are right." Others yielded in order "not to spoil your results." Many of the individuals who went along suspected that the majority were "sheep" following the first responder, or that the majority were victims of an optical illusion; nevertheless, these suspicions failed to free them at the moment of decision. More disquieting were the reactions of subjects who construed their difference from the majority as a sign of some general deficiency in themselves, which at all costs they must hide. On this basis they desperately tried to merge with the majority, not realizing the longer-range consequences to themselves. All the yielding subjects underestimated the frequency with which they conformed.

Which aspect of the influence of a majority is more important—the size of the majority or its unanimity? The experiment was modified to examine this question. In one series the size of the opposition was varied from one to fifteen persons. The results showed a clear trend. When a subject was confronted with only a single individual who contradicted his answers, he was swayed little: he continued to answer independently and correctly in nearly all trials. When the opposition was increased to two, the pressure became substantial: minority subjects now accepted the wrong answer 13.6 percent of the time. Under the pressure of a majority of three, the subjects' errors jumped to 31.8 percent. But further increases in the size of the majority apparently did not increase the weight of the pressure substantially. Clearly the size of the opposition is important only up to a point.

Disturbance of the majority's unanimity had a striking effect. In this experiment the subject was given the support of a truthful partner—either another individual who did not know of the prearranged agreement among the rest of the group, or a person who was instructed to give correct answers throughout.

The presence of a supporting partner depleted the majority of much of its power. Its pressure on the dissenting individual was reduced to one-fourth: that is, subjects answered incorrectly only one-fourth as often as under the pressure of a unanimous majority (see Figure 3.6). The weakest persons did not yield as readily. Most interesting were the reactions to the partner. Generally the feeling toward him was one of warmth and closeness; he was credited with inspiring confidence. However, the subjects repudiated the suggestion that the partner decided them to be independent.

Was the partner's effect a consequence of his dissent, or was it related to his accuracy? We now introduced into the experimental group a person who was instructed to dissent from the majority but also to disagree with the subject. In some experiments the majority was always to choose the worst of the comparison lines and the instructed dissenter to pick the line that was closer to the length of the standard one; in others the majority was consistently intermediate and the dissenter most in error. In this manner we were able to study the relative influence of "compromising" and "extremist" dissenters.

Again the results are clear. When a moderate dissenter is present, the effect of the majority on the subject decreases by approximately one-third, and extremes of yielding disappear. Moreover, most of the errors the subjects do make are moderate, rather than flagrant. In short, the dissenter largely controls the choice of errors. To this extent the subjects broke away from the majority even while bending to it.

On the other hand, when the dissenter always chose the line that was more flagrantly different from the standard, the results were of quite a different kind. The extremist dissenter produced a remarkable freeing of the subjects; their errors dropped to only 9 percent. Furthermore, all the errors were of the moderate variety. We were able to conclude that dissent *per se* increased independence and moderated the errors that occurred, and that the direction of dissent exerted consistent effects.

In all the foregoing experiments each subject was observed only in a single setting. We now turned to studying the effects upon a given individual of a change in the situation to which he was exposed. The first experiment examined the consequences of losing or gaining a partner. The instructed partner began by answering correctly on the first six trials. With his support the subject usually resisted pressure from the majority: eighteen of twenty-seven subjects were completely independent. But after six trials the partner joined the majority. As soon as he did so, there was an abrupt rise in the subjects' errors. Their submission to the majority

FIGURE 3.3
Experiment proceeds as follows: In the top picture, the subject *(center)* hears rules of experiment for the first time. In the second picture, he makes his first judgment of a pair of cards, disagreeing with the unanimous judgment of the others. In the third, he leans forward to look at another pair of cards. In the fourth, he shows the strain of repeatedly disagreeing with the majority. In the fifth, after twelve pairs of cards have been shown, he explains that "he has to call them as he sees them." This subject disagreed with the majority on all twelve trials. Seventy-five percent of experimental subjects agree with the majority in varying degrees. (Photographs by William Vandivert.)

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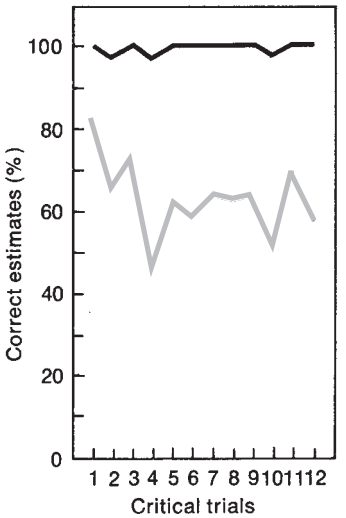

FIGURE 3.4
Error of 123 subjects, each of whom compared lines in the presence of six to eight opponents, is plotted in the gray curve. The accuracy of judgment not under pressure is indicated in black.

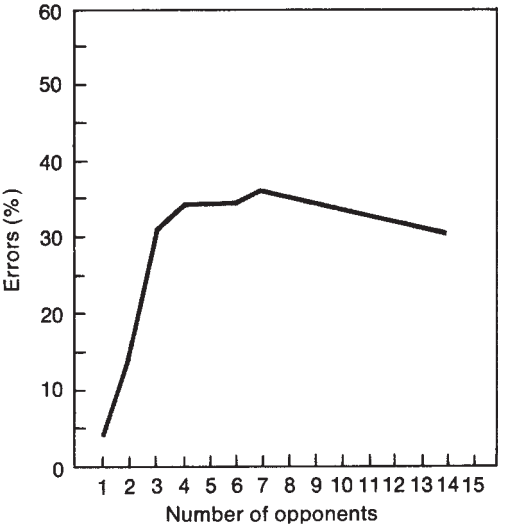

FIGURE 3.5
Size of majority that opposed them had an effect on the subjects. With a single opponent, the subject erred only 3.6 percent of the time; with two opponents he erred 13.6 percent; with three, 31.8 percent; with four, 35.1 percent; with six, 35.2 percent; with seven, 37.1 percent; with nine, 35.1 percent; with fifteen, 31.2 percent.

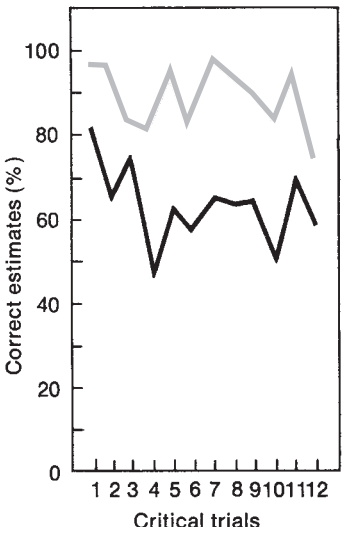

FIGURE 3.6
Two subjects supporting each other against a majority made fewer errors *(gray curve)* than one subject did against a majority *(black curve)*.

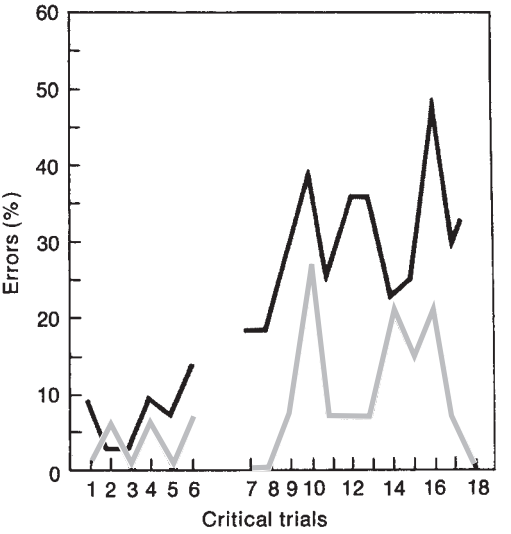

FIGURE 3.7
Partner left subject after six trials in a single experiment. The gray curve shows the error of the subject when the partner "deserted" to the majority. The black curve shows error when partner merely left the room.

was just about as frequent as when the minority subject was opposed by a unanimous majority throughout.

It was surprising to find that the experience of having had a partner and of having braved the majority opposition with him had failed to strengthen the individuals' independence. Questioning at the conclusion of the experiment suggested that we had overlooked an important circumstance; namely, the strong specific effect of "desertion" by the partner to the other side. We therefore changed the conditions so that the partner would simply leave the group at the proper point. (To allay suspicion it was announced in advance that he had an appointment with the dean.) In this form of the experiment, the partner's effect outlasted his presence. The errors increased after his departure, but less markedly than after a partner switched to the majority.

In a variant of this procedure the trials began with the majority unanimously giving correct answers. Then they gradually broke away until on the sixth trial the naive subject was alone and the group unanimously against him. As long as the subject had anyone on his side, he was almost invariably independent, but as soon as he found himself alone, the tendency to conform to the majority rose abruptly.

As might be expected, an individual's resistance to group pressure in these experiments depends to a considerable degree on how wrong the majority is. We varied the discrepancy between the standard line and the other lines systematically, with the hope of reaching a point where the error of the majority would be so glaring that every subject would repudiate it and choose independently. In this we regretfully did not succeed. Even when the difference between the lines was seven inches, there were still some who yielded to the error of the majority.

The study provides clear answers to a few relatively simple questions, and it raises many others that await investigation. We would like to know the degree of consistency of persons in situations which differ in content and structure. If consistency of independence or conformity in behavior is shown to be a fact, how is it functionally related to qualities of character and personality? In what ways is independence related to sociological or cultural conditions? Are leaders more independent than other people, or are they adept at following their followers? These and many other questions may perhaps be answerable by investigations of the type described here.

Life in society requires consensus as an indispensable condition. But consensus, to be productive, requires that each individual contribute independently out of his experience and insight. When consensus comes under the dominance of conformity, the social process is polluted and the individual at the same time surrenders the powers on which his functioning as a feeling and thinking being depends. That we have found the tendency to conformity in our society so strong that reasonably intelligent and well-meaning young people are willing to call white black is a matter of concern. It raises questions about our ways of education and about the values that guide our conduct.

Yet anyone inclined to draw too pessimistic conclusions from this report would do well to remind himself that the capacities for independence are not to be underestimated. He may also draw some consolation from a further observation: those

who participated in this challenging experiment agreed nearly without exception that independence was preferable to conformity.

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4

Behavioral Study of Obedience

Stanley Milgram

This chapter describes a procedure for the study of destructive obedience in the laboratory. It consists of ordering a naive S to administer increasingly more severe punishment to a victim in the context of a learning experiment. Punishment is administered by means of a shock generator with 30 graded switches ranging from Slight Shock to Danger: Severe Shock. The victim is a confederate of the E. The primary dependent variable is the maximum shock the S is willing to administer before he refuses to continue further. Twenty-six Ss obeyed the experimental commands fully, and administered the highest shock on the generator. Fourteen Ss broke off the experiment at some point after the victim protested and refused to provide further answers. The procedure created extreme levels of nervous tension in some Ss. Profuse sweating, trembling and stuttering were typical expressions of this emotional disturbance. One unexpected sign of tension—yet to be explained—was the regular occurrence of nervous laughter, which in some Ss developed into uncontrollable seizures. The variety of interesting behavioral dynamics observed in the experiment, the reality of the situation for the S, and the possibility of parametric variation within the framework of the procedure, point to the fruitfulness of further study.

Obedience is as basic an element in the structure of social life as one can point to. Some system of authority is a requirement of all communal living, and it is only the man dwelling in isolation who is not forced to respond, through defiance or submission, to the commands of others. Obedience, as a determinant of behavior, is of particular relevance to our time. It has been reliably established that from 1933–1945 millions of innocent persons were systematically slaughtered on command. Gas chambers were built, death camps were guarded, daily quotas of corpses were produced with the same efficiency as the manufacture of appliances. These inhumane policies may have originated in the mind of a single person, but they could only be carried out on a massive scale if a very large number of persons obeyed orders.

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Obedience is the psychological mechanism that links individual action to political purpose. It is the dispositional cement that binds men to systems of authority. Facts of recent history and observation in daily life suggest that for many persons obedience may be a deeply ingrained behavior tendency, indeed, a prepotent impulse overriding training in ethics, sympathy, and moral conduct. C. P. Snow (1961) points to its importance when he writes:

> When you think of the long and gloomy history of man, you will find more hideous crimes have been committed in the name of obedience than have ever been committed in the name of rebellion. If you doubt that, read William Shirer's "Rise and Fall of the Third Reich." The German Officer Corps were brought up in the most rigorous code of obedience . . . in the name of obedience they were party to, and assisted in, the most wicked large scale actions in the history of the world [p. 24].

While the particular form of obedience dealt with in the present study has its antecedents in these episodes, it must not be thought all obedience entails acts of aggression against others. Obedience serves numerous productive functions. Indeed, the very life of society is predicated on its existence. Obedience may be ennobling and educative and refer to acts of charity and kindness, as well as to destruction.

General Procedure

A procedure was devised which seems useful as a tool for studying obedience (Milgram, 1961). It consists of ordering a naive subject to administer electric shock to a victim. A simulated shock generator is used, with 30 clearly marked voltage levels that range from 15 to 450 volts. The instrument bears verbal designations that range from Slight Shock to Danger: Severe Shock. The responses of the victim, who is a trained confederate of the experimenter, are standardized. The orders to administer shocks are given to the naive subject in the context of a "learning experiment" ostensibly set up to study the effects of punishment on memory. As the experiment proceeds the naive subject is commanded to administer increasingly more intense shocks to the victim, even to the point of reaching the level marked Danger: Severe Shock. Internal resistances become stronger, and at a certain point the subject refuses to go on with the experiment. Behavior prior to this rupture is considered "obedience," in that the subject complies with the commands of the experimenter. The point of rupture is the act of disobedience. A quantitative value is assigned to the subject's performance based on the maximum intensity shock he is willing to administer before he refuses to participate further. Thus for any particular subject and for any particular experimental condition the degree of obedience may be specified with a numerical value. The crux of the study is to systematically vary the factors believed to alter the degree of obedience to the experimental commands.

The technique allows important variables to be manipulated at several points in the experiment. One may vary aspects of the source of command, content and form

of command, instrumentalities for its execution, target object, general social setting, etc. The problem, therefore, is not one of designing increasingly more numerous experimental conditions, but of selecting those that best illuminate the *process* of obedience from the sociopsychological standpoint.

Related Studies

The inquiry bears an important relation to philosophic analyses of obedience and authority (Arendt, 1958; Friedrich, 1958; Weber, 1947), an early experimental study of obedience by Frank (1944), studies in "authoritarianism" (Adorno, Frenkel-Brunswik, Levinson, and Sanford, 1950; Rokeach, 1961), and a recent series of analytic and empirical studies in social power (Cartwright, 1959). It owes much to the long concern with *suggestion* in social psychology, both in its normal forms (e.g., Binet, 1900) and in its clinical manifestations (Charcot, 1881). But it derives, in the first instance, from direct observation of a social fact; the individual who is commanded by a legitimate authority ordinarily obeys. Obedience comes easily and often. It is a ubiquitous and indispensable feature of social life.

METHOD

Subjects

The subjects were 40 males between the ages of 20 and 50, drawn from New Haven and the surrounding communities. Subjects were obtained by a newspaper advertisement and direct mail solicitation. Those who responded to the appeal believed they were to participate in a study of memory and learning at Yale University. A wide range of occupations is represented in the sample. Typical subjects were postal clerks, high school teachers, salesmen, engineers, and laborers. Subjects ranged in educational level from one who had not finished elementary school, to those who had doctorate and other professional degrees. They were paid $4.50 for their participation in the experiment. However, subjects were told that payment was simply for coming to the laboratory, and that the money was theirs no matter what happened after they arrived. Table 4.1 shows the proportion of age and occupational types assigned to the experimental condition.

Personnel and Locale

The experiment was conducted on the grounds of Yale University in the elegant interaction laboratory. (This detail is relevant to the perceived legitimacy of the

TABLE 4.1
Distribution of age and occupational types in the experiment

Occupations	20–29 years *n*	30–39 years *n*	40–50 years *n*	Percentage of total (occupations)
Workers, skilled and unskilled	4	5	6	37.5
Sales, business, and white-collar	3	6	7	40.0
Professional	1	5	3	22.5
Percentage of total (age)	20	40	40	

Note: Total $N = 40$.

experiment. In further variations, the experiment was dissociated from the university, with consequences for performance.) The role of experimenter was played by a 31-year-old high school teacher of biology. His manner was impassive, and his appearance somewhat stern throughout the experiment. He was dressed in a gray technician's coat. The victim was played by a 47-year-old accountant, trained for the role; he was of Irish-American stock, whom most observers found mild-mannered and likable.

Procedure

One naive subject and one victim (an accomplice) performed in each experiment. A pretext had to be devised that would justify the administration of electric shock by the naive subject. This was effectively accomplished by the cover story. After a general introduction on the presumed relation between punishment and learning, subjects were told

> But actually, we know very *little* about the effect of punishment on learning, because almost no truly scientific studies have been made of it in human beings.
>
> For instance, we don't know how *much* punishment is best for learning—and we don't know how much difference it makes as to who is giving the punishment, whether an adult learns best from a younger or an older person than himself—or many things of that sort.
>
> So in this study we are bringing together a number of adults of different occupations and ages. And we're asking some of them to be teachers and some of them to be learners.
>
> We want to find out what effect different people have on each other as teachers and learners, and also what effect *punishment* will have on learning in this situation.

> Therefore, I'm going to ask one of you to be the teacher here tonight and the other one to be the learner.
>
> Does either of you have a preference?

Subjects then drew slips of paper from a hat to determine who would be the teacher and who would be the learner in the experiment. The drawing was rigged so that the naive subject was always the teacher and the accomplice always the learner. (Both slips contained the word "Teacher.") Immediately after the drawing the teacher and learner were taken to an adjacent room and the learner was strapped into an "electric chair" apparatus.

The experimenter explained that the straps were to prevent excessive movement while the learner was being shocked. The effect was to make it impossible for him to escape from the situation. An electrode was attached to the learner's wrist, and electrode paste was applied "to avoid blisters and burns." Subjects were told that the electrode was attached to the shock generator in the adjoining room.

To improve credibility the experimenter declared, in response to a question by the learner: "Although the shocks can be extremely painful, they cause no permanent tissue damage."

Learning task. The lesson administered by the subject was a paired-associate learning task. The subject read a series of word pairs to the learner, and then read the first word of the pair along with four terms. The learner was to indicate which of the four terms had originally been paired with the first word. He communicated his answer by pressing one of four switches in front of him, which in turn lit up one of four numbered quadrants in an answer-box located atop the shock generator.

Shock generator. The instrument panel consists of 30 lever switches set in a horizontal line. Each switch is clearly labeled with a voltage designation that ranges from 15 to 450 volts. There is a 15-volt increment from one switch to the next going from left to right. In addition, the following verbal designations are clearly indicated for groups of four switches going from left to right: Slight Shock, Moderate Shock, Strong Shock, Very Strong Shock, Intense Shock, Extreme Intensity Shock, Danger: Severe Shock. (Two switches after this last designation are simply marked XXX.)

Upon depressing a switch: a pilot light corresponding to each switch is illuminated in bright red; an electric buzzing is heard; an electric blue light, labeled "voltage energizer," flashes; the dial on the voltage meter swings to the right; various relay clicks are sounded.

The upper left-hand corner of the generator is labeled Shock Generator, Type ZLB, Dyson Instrument Company, Waltham, Mass. Output 15 Volts–450 Volts.

Details of the instrument were carefully handled to insure an appearance of authenticity. The panel was engraved by precision industrial engravers, and all

components were of high quality. No subject in the experiment suspected that the instrument was merely a simulated shock generator.[1]

Sample shock. Each naive subject is given a sample shock on the shock generator, prior to beginning his run as teacher. This shock is always 45 volts, and is applied by pressing the third switch of the generator. The shock is applied to the wrist of the naive subject, and has its source in a 45-volt battery wired into the generator. This further convinces the subject of the authenticity of the generator.

Shock instructions. The subject is told to administer a shock to the learner each time he gives a wrong response. Moreover—and this is the key command—the subject is instructed to "move one lever higher on the shock generator each time the learner flashes a wrong answer." He is also instructed to announce the voltage level before administering a shock. This serves to continually remind subjects of the increasing intensity of shocks administered to the learner.

Preliminary and regular run. Pretests revealed that the procedure of reading words and administering shocks requires some practice before it can be handled smoothly. Therefore, immediately preceding the regular run, the teacher is given a preliminary series of 10 words to read to the learner. There are 3 neutral words in the practice series (i.e., words that the learner answers correctly), so that shocks are administered for 7 of the words, with the maximum shock at 105 volts (moderate shock). Almost all subjects master the procedure by the time the preliminary run is over.

Subjects are then presented with a second list, and are told that the procedure is the same as for the first list; the experimenter adds, however,

> When you get to the bottom of the list, repeat it over again, and continue giving shocks, until the learner has learned all the pairs correctly.

The experimenter instructs the subject to

> start from 15 volts and increase the shock level one step each time the learner gives a wrong answer.

Feedback from the victim. In all conditions the learner gives a predetermined set of responses to the word pair test, based on a schedule of approximately three wrong answers to one correct answer. In the present experimental condition

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[1]A related technique, making use of a shock generator, was reported by Buss (1961) for the study of aggression in the laboratory. Despite the considerable similarity of technical detail in the experimental procedures, each investigator proceeded in ignorance of the other's work. Milgram provided plans and photographs of his shock generator, experimental procedure, and first results in a report to the National Science Foundation in January 1961. This report received only limited circulation. Buss reported his procedure six months later, but to a wider audience. Subsequently, technical information and reports were exchanged. The present article was first received in the editor's office on December 27, 1961; it was resubmitted with deletions on July 27, 1962.

no vocal response or other sign of protest is heard from the learner until Shock Level 300 is reached. When the 300-volt shock is administered, the learner pounds on the wall of the room in which he is bound to the electric chair. The pounding can be heard by the subject. From this point on, the learner's answers no longer appear on the four-way panel.

At this juncture, subjects ordinarily turn to the experimenter for guidance. The experimenter instructs the subject to treat the absence of a response as a wrong answer, and to shock the subject according to the usual schedule. He advises the subjects to allow 5–10 seconds before considering no response as a wrong answer, and to increase the shock level one step each time the learner fails to respond correctly. The learner's pounding is repeated after the 315-volt shock is administered; afterwards he is not heard from, nor do his answers reappear on the four-way signal box.

Experimenter feedback. At various points in the experiment the subject may turn to the experimenter for advice on whether he should continue to administer shocks. Or he may indicate that he does not wish to go on. It was important to standardize the experimenter's responses to these inquiries and reactions.

If the subject indicated his unwillingness to go on, the experimenter responded with a sequence of "prods," using as many as necessary to bring the subject into line.

Prod 1: Please continue. *or* Please go on.
Prod 2: The experiment requires that you continue.
Prod 3: It is absolutely essential that you continue.
Prod 4: You have no other choice, you *must* go on.

The prods were always made in sequence: Only if Prod 1 had been unsuccessful, could Prod 2 be used. If the subject refused to obey the experimenter after Prod 4, the experiment was terminated. The experimenter's tone of voice was at all times firm, but not impolite. The sequence was begun anew on each occasion that the subject balked or showed reluctance to follow orders.

Special prods. If the subject asked if the learner was liable to suffer permanent physical injury, the experimenter said:

> Although the shocks may be painful, there is no permanent tissue damage, so please go on. [Followed by Prods 2, 3, and 4 if necessary.]

If the subject said that the learner did not want to go on, the experimenter replied:

> Whether the learner likes it or not, you must go on until he has learned all the word pairs correctly. So please go on. [Followed by Prods 2, 3, and 4 if necessary.]

Dependent Measures

The primary dependent measure for any subject is the maximum shock he administers before he refuses to go any further. In principle this may vary from 0 (for

a subject who refuses to administer even the first shock) to 30 (for a subject who administers the highest shock on the generator). A subject who breaks off the experiment at any point prior to administering the thirtieth shock level is termed a *defiant* subject. One who complies with experimental commands fully, and proceeds to administer all shock levels commanded, is termed an *obedient* subject.

Further records. With few exceptions, experimental sessions were recorded on magnetic tape. Occasional photographs were taken through one-way mirrors. Notes were kept on any unusual behavior occurring during the course of the experiments. On occasion, additional observers were directed to write objective descriptions of the subjects' behavior. The latency and duration of shocks were measured by accurate timing devices.

Interview and dehoax. Following the experiment, subjects were interviewed; open-ended questions, projective measures, and attitude scales were employed. After the interview, procedures were undertaken to assure that the subject would leave the laboratory in a state of well being. A friendly reconciliation was arranged between the subject and the victim, and an effort was made to reduce any tensions that arose as a result of the experiment.

RESULTS

Preliminary Notions

Before giving an account of the experimental results, it is instructive to consider how persons predict others will perform when commanded to administer powerful shocks to another person. Fourteen Yale seniors, all psychology majors, were provided with a detailed description of the experimental situation. They were asked to reflect carefully on it, and to predict the behavior of 100 hypothetical subjects. More specifically, they were instructed to plot the distribution of obedience of "100 Americans of diverse occupations, and ranging in age from 20 to 50 years," who were placed in the experimental situation.

There was considerable agreement among the respondents on the expected behavior of hypothetical subjects. All respondents predicted that only an insignificant minority would go through to the end of the shock series. (The estimates ranged from 0 to 3 percent; i.e., the most "pessimistic" member of the class predicted that of 100 persons, 3 would continue through to the most potent shock available on the shock generator—450 volts.) The class mean was 1.2 percent. The question was also posed informally to colleagues of the author, and the most general feeling was that few if any subjects would go beyond the designation Very Strong Shock.

Result of Experiment I

Subjects accept situation. With few exceptions subjects were convinced of the reality of the experimental situation, that they were administering shocks to another person, and the most potent shocks were extremely painful. In the post-experimental interview subjects were asked: "How painful to the learner were the last few shocks that you administered to him?" Subjects were instructed to indicate their answers on a printed 14-point scale ranging from "Not at all painful" to "Extremely painful." The modal response was 14 (Extremely painful) with the mean at 13.42.

Signs of extreme tension. Many subjects showed signs of nervousness in the experimental situation, and especially upon administering the more powerful shocks. In a large number of cases the degree of tension reached extremes that are rarely seen in sociopsychological laboratory studies. Subjects were observed to sweat, tremble, stutter, bite their lips, groan, and dig their fingernails into their flesh. These were characteristic rather than exceptional responses to the experiment.

One sign of tension was the regular occurrence of nervous laughing fits. Fourteen of the 40 subjects showed definite signs of nervous laughter and smiling. The laughter seemed entirely out of place, even bizarre. Full-blown, uncontrollable seizures were observed for 3 subjects. On one occasion we observed a seizure so violently convulsive that it was necessary to call a halt to the experiment. The subject, a 46-year-old encyclopedia salesman, was seriously embarrassed by his untoward and uncontrollable behavior. In the postexperimental interviews subjects took pains to point out that they were not sadistic types, and that the laughter did not mean they enjoyed shocking the victim.

Distribution of scores. It had been conjectured that persons would not, in general, agree to administer shocks beyond the level designated as Very Strong Shock. The distribution of maximum shocks for this experiment is shown in Table 4.2. The obtained distribution of scores deviates radically from the prediction.

Upon command of the experimenter, each of the 40 subjects went beyond the expected break-off point. No subject stopped prior to administering Shock Level 20. (At this level—300 volts—the victim kicks on the wall and no longer provides answers to the teacher's multiple-choice questions.)

Of the 40 subjects, 5 refused to obey the experimental commands beyond the 300-volt level. Four more subjects administered one further shock, and then refused to go on. Two broke off at the 330-volt level, and 1 each at 345, 360, and 375 volts. Thus a total of 14 subjects defied the experimenter. It is instructive to reprint their remarks at the point of defiance, as transcribed from the tape record.

> [0124] I think he's trying to communicate, he's knocking . . . Well it's not fair to shock the guy . . . these are terrific volts. I don't think this is very humane . . . Oh, I

TABLE 4.2
Distribution of break-off points

Verbal designation and voltage indication	Number of subjects for whom this was maximum shock
Slight Shock	
15	0
30	0
45	0
60	0
Moderate Shock	
75	0
90	0
105	0
120	0
Strong Shock	
135	0
150	0
165	0
180	0
Very Strong Shock	
195	0
210	0
225	0
240	0
Intense Shock	
255	0
270	0
285	0
300	5
Extreme Intensity Shock	
315	4
330	2
345	1
360	1
Danger: Severe Shock	
375	1
390	0
405	0
420	0
XXX	
435	0
450	26

can't go on with this; no, this isn't right. It's a hell of an experiment. The guy is suffering in there. No, I don't want to go on. This is crazy. [Subject refused to administer more shocks.]

[0123] He's banging in there. I'm gonna chicken out. I'd like to continue, but I can't do that to a man . . . I'm sorry I can't do that to a man. I'll hurt his heart. You take your check . . . No really, I couldn't do it.

These subjects were frequently in a highly agitated and even angered state. Sometimes, verbal protest was at a minimum, and the subject simply got up from his

chair in front of the shock generator, and indicated that he wished to leave the laboratory.

Of the 40 subjects, 26 obeyed the orders of the experimenter to the end, proceeding to punish the victim until they reached the most potent shock available on the shock generator. At that point, the experimenter called a halt to the session. (The maximum shock is labeled 450 volts, and is two steps beyond the designation: Danger: Severe Shock.) Although obedient subjects continued to administer shocks, they often did so under extreme stress. Some expressed reluctance to administer shocks beyond the 300-volt level, and displayed fears similar to those who defied the experimenter; yet they obeyed.

After the maximum shocks had been delivered, and the experimenter called a halt to the proceedings, many obedient subjects heaved sighs of relief, mopped their brows, rubbed their fingers over their eyes, or nervously fumbled cigarettes. Some shook their heads, apparently in regret. Some subjects had remained calm throughout the experiment, and displayed only minimal signs of tension from beginning to end.

DISCUSSION

The experiment yielded two findings that were surprising. The first finding concerns the sheer strength of obedient tendencies manifested in this situation. Subjects have learned from childhood that it is a fundamental breach of moral conduct to hurt another person against his will. Yet, 26 subjects abandon this tenet in following the instructions of an authority who has no special powers to enforce his commands. To disobey would bring no material loss to the subject; no punishment would ensue. It is clear from the remarks and outward behavior of many participants that in punishing the victim they are often acting against their own values. Subjects often expressed deep disapproval of shocking a man in the face of his objections, and others denounced it as stupid and senseless. Yet the majority complied with the experimental commands. This outcome was surprising from two perspectives: first, from the standpoint of predictions made in the questionnaire described earlier. (Here, however, it is possible that the remoteness of the respondents from the actual situation, and the difficulty of conveying to them the concrete details of the experiment, could account for the serious underestimation of obedience.)

But the results were also unexpected to persons who observed the experiment in progress, through one-way mirrors. Observers often uttered expressions of disbelief upon seeing a subject administer more powerful shocks to the victim. These persons had a full acquaintance with the details of the situation, and yet systematically underestimated the amount of obedience that subjects would display.

The second unanticipated effect was the extraordinary tension generated by the procedures. One might suppose that a subject would simply break off or continue

as his conscience dictated. Yet, this is very far from what happened. There were striking reactions of tension and emotional strain. One observer related

> I observed a mature and initially poised businessman enter the laboratory smiling and confident. Within 20 minutes he was reduced to a twitching, stuttering wreck, who was rapidly approaching a point of nervous collapse. He constantly pulled on his earlobe, and twisted his hands. At one point he pushed his fist into his forehead and muttered: "Oh God, let's stop it." And yet he continued to respond to every word of the experimenter, and obeyed to the end.

Any understanding of the phenomenon of obedience must rest on an analysis of the particular conditions in which it occurs. The following features of the experiment go some distance in explaining the high amount of obedience observed in the situation.

1. The experiment is sponsored by and takes place on the grounds of an institution of unimpeachable reputation, Yale University. It may be reasonably presumed that the personnel are competent and reputable. The importance of this background authority is now being studied by conducting a series of experiments outside of New Haven, and without any visible ties to the university.
2. The experiment is, on the face of it, designed to attain a worthy purpose—advancement of knowledge about learning and memory. Obedience occurs not as an end in itself, but as an instrumental element in a situation that the subject construes as significant, and meaningful. He may not be able to see its full significance, but he may properly assume that the experimenter does.
3. The subject perceives that the victim has voluntarily submitted to the authority system of the experimenter. He is not (at first) an unwilling captive impressed for involuntary service. He has taken the trouble to come to the laboratory presumably to aid the experimental research. That he later becomes an involuntary subject does not alter the fact that, initially, he consented to participate without qualification. Thus he has in some degree incurred an obligation toward the experimenter.
4. The subject, too, has entered the experiment voluntarily, and perceives himself under obligation to aid the experimenter. He has made a commitment, and to disrupt the experiment is a repudiation of this initial promise of aid.
5. Certain features of the procedure strengthen the subject's sense of obligation to the experimenter. For one, he has been paid for coming to the laboratory. In part this is canceled out by the experimenter's statement that

> Of course, as in all experiments, the money is yours simply for coming to the laboratory. From this point on, no matter what happens, the money is yours.[2]

6. From the subject's standpoint, the fact that he is the teacher and the other man the learner is purely a chance consequence (it is determined by drawing lots) and he, the subject, ran the same risk as the other man in being assigned the role of

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[2]Forty-three subjects, undergraduates at Yale University, were run in the experiment without payment. The results are very similar to those obtained with paid subjects.

learner. Since the assignment of positions in the experiment was achieved by fair means, the learner is deprived of any basis of complaint on this count. (A similar situation obtains in Army units, in which—in the absence of volunteers—a particularly dangerous mission may be assigned by drawing lots, and the unlucky soldier is expected to bear his misfortune with sportsmanship.)

7. There is, at best, ambiguity with regard to the prerogatives of a psychologist and the corresponding rights of his subject. There is a vagueness of expectation concerning what a psychologist may require of his subject, and when he is overstepping acceptable limits. Moreover, the experiment occurs in a closed setting, and thus provides no opportunity for the subject to remove these ambiguities by discussion with others. There are few standards that seem directly applicable to the situation, which is a novel one for most subjects.

8. The subjects are assured that the shocks administered to the subject are "painful but not dangerous." Thus they assume that the discomfort caused the victim is momentary, while the scientific gains resulting from the experiment are enduring.

9. Through Shock Level 20 the victim continues to provide answers on the signal box. The subject may construe this as a sign that the victim is still willing to "play the game." It is only after Shock Level 20 that the victim repudiates the rules completely, refusing to answer further.

These features help to explain the high amount of obedience obtained in this experiment. Many of the arguments raised need not remain matters of speculation, but can be reduced to testable propositions to be confirmed or disproved by further experiments.[3]

The following features of the experiment concern the nature of the conflict which the subject faces.

10. The subject is placed in a position in which he must respond to the competing demands of two persons: the experimenter and the victim. The conflict must be resolved by meeting the demands of one or the other; satisfaction of the victim and the experimenter are mutually exclusive. Moreover, the resolution must take the form of a highly visible action, that of continuing to shock the victim or breaking off the experiment. Thus the subject is forced into a public conflict that does not permit any completely satisfactory solution.

11. While the demands of the experimenter carry the weight of scientific authority, the demands of the victim spring from his personal experience of pain and suffering. The two claims need not be regarded as equally pressing and legitimate. The experimenter seeks an abstract scientific datum; the victim cries out for relief from physical suffering caused by the subject's actions.

12. The experiment gives the subject little time for reflection. The conflict comes on rapidly. It is only minutes after the subject has been seated before the

[3]A series of recently completed experiments employing the obedience paradigm is reported in Milgram (1965).

shock generator that the victim begins his protests. Moreover, the subject perceives that he has gone through but two-thirds of the shock levels at the time the subject's first protests are heard. Thus he understands that the conflict will have a persistent aspect to it, and may well become more intense as increasingly more powerful shocks are required. The rapidity with which the conflict descends on the subject, and his realization that it is predictably recurrent may well be sources of tension to him.

13. At a more general level, the conflict stems from the opposition of two deeply ingrained behavior dispositions: first, the disposition not to harm other people, and second, the tendency to obey those whom we perceive to be legitimate authorities.

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5

"From Jerusalem to Jericho": A Study of Situational and Dispositional Variables in Helping Behavior

John M. Darley and C. Daniel Batson

The influence of several situational and personality variables on helping behavior was examined in an emergency situation suggested by the parable of the Good Samaritan. People going between two buildings encountered a shabbily dressed person slumped by the side of the road. Subjects in a hurry to reach their destination were more likely to pass by without stopping. Some subjects were going to give a short talk on the parable of the Good Samaritan, others on a nonhelping relevant topic; this made no significant difference in the likelihood of their giving the victim help. Religious personality variables did not predict whether an individual would help the victim. However, if a subject did stop to offer help, the character of the helping response was related to his type of religiosity.

Helping other people in distress is, among other things, an ethical act. That is, it is an act governed by ethical norms and precepts taught to children at home, in school, and in church. From Freudian and other personality theories, one would expect individual differences in internalization of these standards that would lead to differences between individuals in the likelihood with which they would help others. But recent research on bystander intervention in emergency situations (Bickman, 1969; Darley and Lantané, 1968; Korte, 1969; but see also Schwartz and Clausen, 1970) has had bad luck in finding personality determinants of helping behavior. Although personality variables that one might expect to correlate with helping behavior have been measured (Machiavellianism, authoritarianism, social de-

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sirability, alienation, and social responsibility), these were not predictive of helping. Nor was this due to a generalized lack of predictability in the helping situation examined, since variations in the experimental situation, such as the availability of other people who might also help, produced marked changes in rates of helping behavior. These findings are reminiscent of Hartshorne and May's (1928) discovery that resistance to temptation, another ethically relevant act, did not seem to be a fixed characteristic of an individual. That is, a person who was likely to be honest in one situation was not particularly likely to be honest in the next (but see also Burton, 1963).

The rather disappointing correlation between the social psychologist's traditional set of personality variables and helping behavior in emergency situations suggests the need for a fresh perspective on possible predictors of helping and possible situations in which to test them. Therefore, for inspiration we turned to the Bible, to what is perhaps the classical helping story in the Judeo-Christian tradition, the parable of the Good Samaritan. The parable proved of value in suggesting both personality and situational variables relevant to helping.

> "And who is my neighbor?" Jesus replied, "A man was going down from Jerusalem to Jericho, and he fell among robbers, who stripped him and beat him, and departed, leaving him half dead. Now by chance a priest was going down the road; and when he saw him he passed by on the other side. So likewise a Levite, when he came to the place and saw him, passed by on the other side. But a Samaritan, as he journeyed, came to where he was; and when he saw him, he had compassion, and went to him and bound his wounds, pouring on oil and wine; then he set him on his own beast and brought him to an inn, and took care of him. And the next day he took out two dennarii and gave them to the innkeeper, saying, 'Take care of him; and whatever more you spend, I will repay you when I come back.' Which of these three, do you think, proved neighbor to him who fell among the robbers?" He said, "The one who showed mercy on him." And Jesus said to him, "Go and do likewise." [Luke 10: 29–37 RSV]

To psychologists who reflect on the parable, it seems to suggest situational and personality differences between the nonhelpful priest and Levite and the helpful Samaritan. What might each have been thinking and doing when he came upon the robbery victim on that desolate road? What sort of persons were they?

One can speculate on differences in thought. Both the priest and the Levite were religious functionaries who could be expected to have their minds occupied with religious matters. The priest's role in religious activities is obvious. The Levite's role, although less obvious, is equally important: The Levites were necessary participants in temple ceremonies. Much less can be said with any confidence about what the Samaritan might have been thinking, but, in contrast to the others, it was most likely not of a religious nature, for Samaritans were religious outcasts.

Not only was the Samaritan most likely thinking about more mundane matters than the priest and Levite, but, because he was socially less important, it seems likely that he was operating on a quite different time schedule. One can imagine the priest and Levite, prominent public figures, hurrying along with little black

books full of meetings and appointments, glancing furtively at their sundials. In contrast, the Samaritan would likely have far fewer and less important people counting on him to be at a particular place at a particular time, and therefore might be expected to be in less of a hurry than the prominent priest or Levite.

In addition to these situational variables, one finds personality factors suggested, as well. Central among these, and apparently basic to the point that Jesus was trying to make, is a distinction between types of religiosity. Both the priest and Levite are extremely "religious." But it seems to be precisely their type of religiosity that the parable challenges. At issue is the motivation for one's religion and ethical behavior. Jesus seems to feel that the religious leaders of his time, though certainly respected and upstanding citizens, may be "virtuous" for what it will get them, both in terms of the admiration of their fellowmen and in the eyes of God. New Testament scholar R. W. Funk (1966) noted that the Samaritan is at the other end of the spectrum.

> The Samaritan does not love with side glances at God. The need of neighbor alone is made self-evident, and the Samaritan responds without other motivation [pp. 218–219].

That is, the Samaritan is interpreted as responding spontaneously to the situation, not as being preoccupied with the abstract ethical or organizational do's and don'ts of religion as the priest and Levite would seem to be. This is not to say that the Samaritan is portrayed as irreligious. A major intent of the parable would seem to be to present the Samaritan as a religious and ethical example, but at the same time to contrast his type of religiosity with the more common conception of religiosity that the priest and Levite represent.

To summarize the variables suggested as affecting helping behavior by the parable, the situational variables include the content of one's thinking and the amount of hurry in one's journey. The major dispositional variable seems to be differing types of religiosity. Certainly these variables do not exhaust the list that could be elicited from the parable, but they do suggest several research hypotheses.

Hypothesis 1. The parable implies that people who encounter a situation possibly calling for a helping response while thinking religious and ethical thoughts will be no more likely to offer aid than persons thinking about something else. Such a hypothesis seems to run counter to a theory that focuses on norms as determining helping behavior because a normative account would predict that the increased salience of helping norms produced by thinking about religious and ethical examples would increase helping behavior.

Hypothesis 2. Persons encountering a possible helping situation when they are in a hurry will be less likely to offer aid than persons not in a hurry.

Hypothesis 3. Concerning types of religiosity, persons who are religious in a Samaritan-like fashion will help more frequently than those religious in a priest or Levite fashion.

Obviously, this last hypothesis is hardly operationalized as stated. Prior research by one of the investigators on types of religiosity (Batson, 1971), however, led us to differentiate three distinct ways of being religious: *(a)* for what it will gain one (cf. Freud, 1927, and perhaps the priest and Levite), *(b)* for its own intrinsic value (cf. Allport and Ross, 1967), and *(c)* as a response to and quest for meaning in one's everyday life (cf. Batson, 1971). Both of the latter conceptions would be proposed by their exponents as related to the more Samaritanlike "true" religiosity. Therefore, depending on the theorist one follows, the third hypothesis may be stated like this: People *(a)* who are religious for intrinsic reasons (Allport and Ross, 1967) or *(b)* whose religion emerges out of questioning the meaning of their everyday lives (Batson, 1971) will be more likely to stop to offer help to the victim.

The parable of the Good Samaritan also suggested how we would measure people's helping behavior—their response to a stranger slumped by the side of one's path. The victim should appear somewhat ambiguous—ill-dressed, possibly in need of help, but also possibly drunk or even potentially dangerous.

Further, the parable suggests a means by which the incident could be perceived as a real one rather than part of a psychological experiment in which one's behavior was under surveillance and might be shaped by demand characteristics (Orne, 1962), evaluation apprehension (Rosenberg, 1965), or other potentially artifactual determinants of helping behavior. The victim should be encountered not in the experimental context but on the road between various tasks.

METHOD

In order to examine the influence of these variables on helping behavior, seminary students were asked to participate in a study on religious education and vocations. In the first testing session, personality questionnaires concerning types of religiosity were administered. In a second individual session, the subject began experimental procedures in one building and was asked to report to another building for later procedures. While in transit, the subject passed a slumped "victim" planted in an alleyway. The dependent variable was whether and how the subject helped the victim. The independent variables were the degree to which the subject was told to hurry in reaching the other building and the talk he was to give when he arrived there. Some subjects were to give a talk on the jobs in which seminary students would be most effective, others, on the parable of the Good Samaritan.

Subjects

The subjects for the questionnaire administration were 67 students at Princeton Theological Seminary. Forty-seven of them, those who could be reached by tele-

phone, were scheduled for the experiment. Of the 47, 7 subjects' data were not included in the analyses—3 because of contamination of the experimental procedures during their testing and 4 because of suspicion of the experimental situation. Each subject was paid $1 for the questionnaire session and $1.50 for the experimental session.

Personality Measures

Detailed discussion of the personality scales used may be found elsewhere (Batson, 1971), so the present discussion will be brief. The general personality construct under examination was religiosity. Various conceptions of religiosity have been offered in recent years based on different psychometric scales. The conception seeming to generate the most interest is the Allport and Ross (1967) distinction between "intrinsic" versus "extrinsic" religiosity (cf. also Allen and Spilka, 1967, on "committed" versus "consensual" religion). This bipolar conception of religiosity has been questioned by Brown (1964) and Batson (1971), who suggested three-dimensional analyses instead. Therefore, in the present research, types of religiosity were measured with three instruments which together provided six separate scales: *(a)* a *doctrinal orthodoxy* (D-O) scale patterned after that used by Glock and Stark (1966), scaling agreement with classic doctrines of Protestant theology; *(b)* the Allport-Ross *extrinsic* (AR-E) scale, measuring the use of religion as a means to an end rather than as an end in itself; *(c)* the Allport-Ross *intrinsic* (AR-I) scale, measuring the use of religion as an end in itself; *(d)* the *extrinsic external* scale of Batson's Religious Life Inventory (RELI-EE), designed to measure the influence of significant others and situations in generating one's religiosity; *(e)* the *extrinsic internal* scale of the Religious Life Inventory (RELI-EI), designed to measure the degree of "driveness" in one's religiosity; and *(f)* the *intrinsic* scale of the Religious Life Inventory (RELI-I), designed to measure the degree to which one's religiosity involves a questioning of the meaning of life arising out of one's interactions with his social environment. The order of presentation of the scales in the questionnaire was RELI, AR, D-O.

Consistent with prior research (Batson, 1971), a principal-component analysis of the total scale scores and individual items for the 67 seminarians produced a theoretically meaningful, orthogonally rotated three-component structure with the following loadings:

Religion as means received a single very high loading from AR-E (.903) and therefore was defined by Allport and Ross's (1967) conception of this scale as measuring religiosity as a means to other ends. This component also received moderate negative loadings from D-O (−.400) and AR-I (−.372) and a moderate positive loading from RELI-EE (.301).

Religion as end received high loadings from RELI-EI (.874), RELI-EE (.725), AR-I (.768), and D-O (.704). Given this configuration, and again following Allport

and Ross's conceptualization, this component seemed to involve religiosity as an end in itself with some intrinsic value.

Religion as quest received a single very high loading from RELI-I (.945) and a moderate loading from RELI-EE (.75). Following Batson, this component was conceived to involve religiosity emerging out of an individual's search for meaning in his personal and social world.

The three religious personality scales examined in the experimental research were constructed through the use of complete-estimation factor score coefficients from these three components.

Scheduling of Experimental Study

Since the incident requiring a helping response was staged outdoors, the entire experimental study was run in 3 days, December 14–16, 1970, between 10 A.M. and 4 P.M. A tight schedule was used in an attempt to maintain reasonably consistent weather and light conditions. Temperature fluctuation according to the *New York Times* for the 3 days during these hours was not more than 5 degrees Fahrenheit. No rain or snow fell, although the third day was cloudy, whereas the first two were sunny. Within days the subjects were randomly assigned to experimental conditions.[1]

Procedure

When a subject appeared for the experiment, an assistant (who was blind with respect to the personality scores) asked him to read a brief statement which explained that he was participating in a study of the vocational careers of seminary students. After developing the rationale for the study, the statement read

> What we have called you in for today is to provide us with some additional material which will give us a clearer picture of how you think than does the questionnaire material we have gathered thus far. Questionnaires are helpful, but tend to be somewhat oversimplified. Therefore, we would like to record a 3–5-minute talk you give based on the following passage. . . .

Variable 1: Message. In the task-relevant condition the passage read

> With increasing frequency the question is being asked: What jobs or professions do seminary students subsequently enjoy most, and in what jobs are they most effective? The answer to this question used to be so obvious that the question was not

[1]An error was made in randomizing that increased the number of subjects in the intermediate-hurry conditions. This worked against the prediction that was most highly confirmed (the hurry prediction) and made no difference to the message variable tests.

> even asked. Seminary students were being trained for the ministry, and since both society at large and the seminary student himself had a relatively clear understanding of what made a "good" minister, there was no need even to raise the question of for what other jobs seminary experience seems to be an asset. Today, however, neither society nor many seminaries have a very clearly defined conception of what a "good" minister is or of what sorts of jobs and professions are the best context in which to minister. Many seminary students, apparently genuinely concerned with "ministering," seem to feel that it is impossible to minister in the professional clergy. Other students, no less concerned, find the clergy the most viable profession for ministry. But are there other jobs and/or professions for which seminary experience is an asset? And, indeed, how much of an asset is it for the professional ministry? Or, even more broadly, can one minister through an "establishment" job at all?

In the helping-relevant condition, the subject was given the parable of the Good Samaritan exactly as printed earlier in this article. Next, regardless of condition, all subjects were told

> You can say whatever you wish based on the passage. Because we are interested in how you think on your feet, you will not be allowed to use notes in giving the talk. Do you understand what you are to do? If not, the assistant will be glad to answer questions.

After a few minutes the assistant returned, asked if there were any questions, and then said

> Since they're rather tight on space in this building, we're using a free office in the building next door for recording the talks. Let me show you how to get there [draws and explains map on 3 × 5 card]. This is where Professor Steiner's laboratory is. If you go in this door [points at map], there's a secretary right here, and she'll direct you to the office we're using for recording. Another of Professor Steiner's assistants will set you up for recording your talk. Is the map clear?

Variable 2: Hurry. In the high-hurry condition the assistant then looked at his watch and said, "Oh, you're late. They were expecting you a few minutes ago. We'd better get moving. The assistant should be waiting for you so you'd better hurry. It shouldn't take but just a minute." In the intermediate-hurry condition he said, "The assistant is ready for you, so please go right over." In the low-hurry condition he said, "It'll be a few minutes before they're ready for you, but you might as well head on over. If you have to wait over there, it shouldn't be long."

The incident. When the subject passed through the alley, the victim was sitting slumped in a doorway, head down, eyes closed, not moving. As the subject went by, the victim coughed twice and groaned, keeping his head down. If the subject stopped and asked if something was wrong or offered to help, the victim, startled and somewhat groggy, said, "Oh, thank you [cough]. . . . No, it's all right. [Pause] I've got this respiratory condition [cough]. . . . The doctor's given me these pills to take, and I just took one. . . . If I just sit and rest for a few minutes I'll be O.K. . . . Thanks very much for stopping though [smiles weakly]." If the subject

persisted, insisting on taking the victim inside the building, the victim allowed him to do so and thanked him.

Helping ratings. The victim rates each subject on a scale of helping behavior, as follows:

> 0 = failed to notice the victim as possibly in need at all; 1 = perceived the victim as possibly in need but did not offer aid; 2 = did not stop but helped indirectly (e.g., by telling Steiner's assistant about the victim); 3 = stopped and asked if victim needed help; 4 = after stopping, insisted on taking the victim inside and then left him.

The victim was blind to the personality scale scores and experimental conditions of all subjects. At the suggestion of the victim, another category was added to the rating scales, based on his observations of pilot subjects' behavior:

> 5 = after stopping, refused to leave the victim (after 3–5 minutes) and/or insisted on taking him somewhere outside experimental context (e.g., for coffee or to the infirmary).

(In some cases it was necessary to distinguish Category 0 from Category 1 by the postexperimental questionnaire and Category 2 from Category 1 on the report of the experimental assistant.)

This 6-point scale of helping behavior and a description of the victim were given to a panel of 10 judges (unacquainted with the research) who were asked to rank order the (unnumbered) categories in terms of "the amount of helping behavior displayed toward the person in the doorway." Of the 10, 1 judge reversed the order of Categories 0 and 1. Otherwise there was complete agreement with the ranking implied in the presentation of the scale above.

The speech. After passing through the alley and entering the door marked on the map, the subject entered a secretary's office. She introduced him to the assistant who gave the subject time to prepare and privately record his talk.

Helping behavior questionnaire. After recording the talk, the subject was sent to another experimenter, who administered "an exploratory questionnaire on personal and social ethics." The questionnaire contained several initial questions about the interrelationship between social and personal ethics, and then asked three key questions: *(a)* "When was the last time you saw a person who seemed to be in need of help?" *(b)* "When was the last time you stopped to help someone in need?" *(c)* "Have you had experience helping persons in need? If so, outline briefly." These data were collected as a check on the victim's ratings of whether subjects who did not stop perceived the situation in the alley as one possibly involving need or not.

When he returned, the experimenter reviewed the subject's questionnaire, and, if no mention was made of the situation in the alley, probed for reactions to it and then phased into an elaborate debriefing and discussion session.

Debriefing

In the debriefing, the subject was told the exact nature of the study, including the deception involved, and the reasons for the deception were explained. The subject's reactions to the victim and to the study in general were discussed. The role of situational determinants of helping behavior was explained in relation to this particular incident and to other experiences of the subject. All subjects seemed readily to understand the necessity for the deception, and none indicated any resentment of it. After debriefing, the subject was thanked for his time and paid, then he left.

RESULTS AND DISCUSSION

Overall Helping Behavior

The average amount of help that a subject offered the victim, by condition, is shown in Table 5.1. The unequal-*N* analysis of variance indicates that while the hurry variable was significantly ($F = 3.56$, $df = 2/34$, $p < .05$) related to helping behavior, the message variable was not. Subjects in a hurry were likely to offer less help than were subjects not in a hurry. Whether the subject was going to give a speech on the parable of the Good Samaritan or not did not significantly affect his helping behavior on this analysis.

TABLE 5.1
Means and analysis of variance of graded helping responses

M

	Hurry			
Message	Low	Medium	High	Summary
Helping relevant	3.800	2.000	1.000	2.263
Task relevant	1.667	1.667	.500	1.333
Summary	3.000	1.818	.700	

Analysis of variance

Source	*SS*	*df*	*MS*	*F*
Message (A)	7.766	1	7.766	2.65
Hurry (B)	20.884	2	10.442	3.56*
A × B	5.237	2	2.619	.89
Error	99.633	34	2.930	

Note: $N = 40$.
*$p < .05$.

Other studies have focused on the question of whether a person initiates helping action, rather than on scaled kinds of helping. The data from the preset study can also be analyzed on the following terms: Of the 40 subjects, 16 (40%) offered some form of direct or indirect aid to the victim (Coding Categories 2–5), 24 (60%) did not (Coding Categories 0 and 1). The percentages of subjects who offered aid by situational variable were, for low hurry, 63% offered help, intermediate hurry 45%, and high hurry 10%; for helping-relevant message 53%, task-relevant message 29%. With regard to this more general question of whether help was offered, an unequal-*N* analysis of variance (arc sine transformation of percentages of helpers, with low- and intermediate-hurry conditions pooled) indicated that again only the hurry main effect was significantly ($F = 5.22$, $p < .05$) related to helping behavior; the subjects in a hurry were more likely to pass by the victim than were those in less of a hurry.

Reviewing the predictions in the light of these results, the second hypothesis, that the degree of hurry a person is in determines his helping behavior, was supported. The prediction involved in the first hypothesis concerning the message content was based on the parable. The parable itself seemed to suggest that thinking pious thoughts would not increase helping. Another and conflicting prediction might be produced by a norm salience theory. Thinking about the parable should make norms for helping salient and therefore produce more helping. The data, as hypothesized, are more congruent with the prediction drawn from the parable. A person going to speak on the parable of the Good Samaritan is not significantly more likely to stop to help a person by the side of the road than is a person going to talk about possible occupations for seminary graduates.

Since both situational hypotheses are confirmed, it is tempting to stop the analysis of these variables at this point. However, multiple regression analysis procedures were also used to analyze the relationship of all of the independent variables of the study and the helping behavior. In addition to often being more statistically powerful because of the use of more data information, multiple regression analysis has an advantage over analysis of variance in that it allows for a comparison of the relative effect of the various independent variables in accounting for variance in the dependent variable. Also, multiple regression analysis can compare the effects of continuous as well as nominal independent variables on both continuous and nominal dependent variables (through the use of point biserial correlations, r_{pb}) and shows considerable robustness to violation of normality assumptions (Cohen, 1965, 1968). Table 5.2 reports the results of the multiple regression analysis using both help versus no help and the graded helping scale as dependent measures. In this table the overall equation *F*s show the *F* value of the entire regression equation as a particular row variable enters the equation. Individual variable *F*s were computed with all five independent variables in the equation. Although the two situational variables, hurry and message condition, correlated more highly with the dependent measure than any of the religious dispositional variables, only hurry was a significant predictor of whether one will help (column 1) or of the overall amount

TABLE 5.2
Stepwise multiple regression analysis

Help vs. no help					Graded helping				
	Individual variable		Overall equation			Individual variable		Variable equation	
Step	*r*[a]	*F*	*R*	*F*	Step	*r*	*F*	*R*	*F*
1. Hurry[b]	−.37	4.537*	.37	5.884*	1. Hurry	−.42	6.665*	.42	8.196**
2. Message[c]	.25	1.495	.41	3.834*	2. Message	.25	1.719	.46	5.083*
3. Religion as quest	−.03	.081	.42	2.521	3. Religion as quest	−.16	1.297	.50	3.897*
4. Religion as means	−.03	.003	.42	1.838*	4. Religion as means	−.08	.018	.50	2.848*
5. Religion as end	.06	.000	.42	1.430	5. Religion as end	−.07	.001	.50	2.213

Note: $N = 40$. Helping is the dependent variable. $df = 1/34$.
[a]Individual variable correlation coefficient is a point biserial where appropriate.
[b]Variables are listed in order of entry into stepwise regression equations.
[c]Helping-relevant message is positive.
*$p < .05$.
**$p < .01$.

of help given (column 2). These results corroborate the findings of the analysis of variance.[2]

Notice also that neither form of the third hypothesis, that types of religiosity will predict helping, received support from these data. No correlation between the various measures of religiosity and any form of the dependent measure ever came near statistical significance, even though the multiple regression analysis procedure is a powerful and not particularly conservative statistical test.

Personality Difference Among Subjects Who Helped

To further investigate the possible influence of personality variables, analyses were carried out using only the data from subjects who offered some kind of help to the victim. Surprisingly (since the number of these subjects was small, only 16) when this was done, one religiosity variable seemed to be significantly related to the kind of helping behavior offered. (The situational variables had no significant effect.) Subjects high on the religion as quest dimension appear likely, when they stop for the victim, to offer help of a more tentative or incomplete nature than are subjects scoring low on this dimension ($r = -.53$, $p < .05$).

This result seemed unsettling for the thinking behind either form of Hypothesis 3. Not only do the data suggest that the Allport-Ross–based conception of religion as end does not predict the degree of helping, but the religion as quest component is a significant predictor of offering less help. This latter result seems counterintuitive and out of keeping with previous research (Batson, 1971), which found that this type of religiosity correlated positively with other socially valued characteristics. Further data analysis, however, seemed to suggest a different interpretation of this result.

It will be remembered that one helping coding category was added at the suggestion of the victim after his observation of pilot subjects. The correlation of religious personality variables with helping behavior dichotomized between the added category (1) and all of the others (0) was examined. The correlation between religion as quest and this dichotomous helping scale was essentially unchanged ($r_{pb} = -.54$, $p < .05$). Thus, the previously found correlation between the helping scale and religion as quest seems to reflect the tendency of those who score low on the quest dimension to offer help in the added helping category.

What does help in this added category represent? Within the context of the experiment, it represented an embarrassment. The victim's response to persistent offers of help was to assure the helper he was all right, had taken his medicine, just needed to rest for a minute or so, and, if ultimately necessary, to request the helper

[2]To check the legitimacy of the use of both analysis of variance and multiple regression analysis, parametric analyses, on this ordinal data, Kendall rank correlation coefficients were calculated between the helping scale and the five independent variables. As expected, τ approximated the correlation quite closely in each case and was significant for hurry only (hurry, $\tau = -.38$, $p < .001$).

to leave. But the *super* helpers in this added category often would not leave until the final appeal was repeated several times by the victim (who was growing increasingly panicky at the possibility of the arrival of the next subject). Since it usually involved the subject's attemping to carry through a preset plan (e.g., taking the subject for a cup of coffee or revealing to him the strength to be found in Christ), and did not allow information from the victim to change that plan, we originally labeled this kind of helping as rigid—an interpretation supported by its increased likelihood among highly doctrinal orthodox subjects ($r = .63$, $p < .01$). It also seemed to have an inappropriate character. If this more extreme form of helping behavior is indeed effectively less helpful, then the second form of Hypothesis 3 does seem to gain support.

But perhaps it is the experimenters rather than the super helpers who are doing the inappropriate thing; perhaps the best characterization of this kind of helping is as different rather than as inappropriate. This kind of helper seems quickly to place a particular interpretation on the situation, and the helping response seems to follow naturally from this interpretation. All that can safely be said is that one style of helping that emerged in this experiment was directed toward the presumed underlying needs of the victim and was little modified by the victim's comments about his own needs. In contrast, another style was more tentative and seemed more responsive to the victim's statements of his need.

The former kind of helping was likely to be displayed by subjects who expressed strong doctrinal orthodoxy. Conversely, this fixed kind of helping was unlikely among subjects high on the religion as quest dimension. These latter subjects, who conceived their religion as involving an ongoing search for meaning in their personal and social world, seemed more responsive to the victim's immediate needs and more open to the victim's definitions of his own needs.

CONCLUSION AND IMPLICATIONS

A person not in a hurry may stop and offer help to a person in distress. A person in a hurry is likely to keep going. Ironically, he is likely to keep going even if he is hurrying to speak on the parable of the Good Samaritan, thus inadvertently confirming the point of the parable. (Indeed, on several occasions, a seminary student going to give his talk on the parable of the Good Samaritan literally stepped over the victim as he hurried on his way!)

Although the degree to which a person was in a hurry had a clearly significant effect on his likelihood of offering the victim help, whether he was going to give a sermon on the parable or on possible vocational roles of ministers did not. This lack of effect of sermon topic raises certain difficulties for an explanation of helping behavior involving helping norms and their salience. It is hard to think of a context in which norms concerning helping those in distress are more salient than for a person thinking about the Good Samaritan, and yet it did not significantly increase helping behavior. The results were in the direction suggested by the norm

salience hypothesis, but they were not significant. The most accurate conclusion seems to be that salience of helping norms is a less strong determinant of helping behavior in the present situation than many, including the present authors, would expect.

Thinking about the Good Samaritan did not increase helping behavior, but being in a hurry decreased it. It is difficult not to conclude from this that the frequently cited explanation that ethics becomes a luxury as the speed of our daily lives increases is at least an accurate description. The picture that this explanation conveys is of a person seeing another, consciously noting his distress, and consciously choosing to leave him in distress. But perhaps this is not entirely accurate, for, when a person is in a hurry, something seems to happen that is akin to Tolman's (1948) concept of the "narrowing of the cognitive map." Our seminarians in a hurry noticed the victim in that in the postexperiment interview almost all mentioned him as, on reflection, possibly in need of help. But it seems that they often had not worked this out when they were near the victim. Either the interpretation of their visual picture as a person in distress or the empathic reactions usually associated with that interpretation had been deferred because they were hurrying. According to the reflections of some of the subjects, it would be inaccurate to say that they realized the victim's possible distress, then chose to ignore it; instead, because of the time pressures, they did not perceive the scene in the alley as an occasion for an ethical decision.

For other subjects it seems more accurate to conclude that they decided not to stop. They appeared aroused and anxious after the encounter in the alley. For these subjects, what were the elements of the choice that they were making? Why were the seminarians hurrying? Because the experimenter, *whom the subject was helping,* was depending on him to get to a particular place quickly. In other words, he was in conflict between stopping to help the victim and continuing on his way to help the experimenter. And this is often true of people in a hurry; they hurry because somebody depends on their being somewhere. Conflict, rather than callousness, can explain their failure to stop.

Finally, as in other studies, personality variables were not useful in predicting whether a person helped. But in this study, unlike many previous ones, considerable variations were possible in the kinds of help given, and these variations did relate to personality measures—specifically to religiosity of the quest sort. The clear light of hindsight suggests that the dimension of kinds of helping would have been the appropriate place to look for personality differences all along; *whether* a person helps is an instant decision likely to be situationally controlled. How a person helps involves a more complex and considered number of decisions, including the time and scope to permit personality characteristics to shape them.

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6

A Focus Theory of Normative Conduct: Recycling the Concept of Norms to Reduce Littering in Public Places

Robert B. Cialdini, Raymond R. Reno, and Carl A. Kallgren

Past research has generated mixed support among social scientists for the utility of social norms in accounting for human behavior. We argue that norms do have a substantial impact on human action; however, the impact can only be properly recognized when researchers (a) separate two types of norms that at times act antagonistically in a situation—injunctive norms (what most others approve or disapprove) and descriptive norms (what most others do)—and (b) focus Ss attention principally on the type of norm being studied. In five natural settings, focusing Ss on either the descriptive norms or the injunctive norms regarding littering caused Ss littering decisions to change only in accord with the dictates of the then more salient type of norm.

Although social norms have a long history within social psychology, support for the concept as a useful explanatory and predictive device is currently quite mixed. Some researchers have used and championed the concept as important to a proper understanding of human social behavior (e.g., Berkowitz, 1972; Fishbein and Ajzen, 1975; McKirnan, 1980; Pepitone, 1976; Sherif, 1936; Staub, 1972; Triandis, 1977). Others have seen little of value in it, arguing that the concept is vague and overly general, often contradictory, and ill-suited to empirical testing (e.g., Darley and Latané, 1970; Krebs, 1970; Krebs and Miller, 1985; Marini, 1984). In

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addition, a parallel controversy has developed within academic sociology where ethnomethodological and constructionist critics have faulted the dominant normative paradigm of that discipline (Garfinkel, 1967; Mehan and Wood, 1975).

The effect of these criticisms has been positive in pointing out problems that must be solved before one can have confidence in the utility of normative explanations. One such problem is definitional. Both in common parlance and academic usage, *norm* has more than one meaning (Shaffer, 1983). When considering normative influence on behavior, it is crucial to discriminate between the *is* (descriptive) and the *ought* (injunctive) meaning of social norms, because each refers to a separate source of human motivation (Deutsch and Gerard, 1955). The descriptive norm describes what is typical or *normal.* It is what most people do, and it motivates by providing evidence as to what will likely be effective and adaptive action: "If everyone is doing it, it must be a sensible thing to do." Cialdini (1988) has argued that such a presumption offers an information-processing advantage and a decisional shortcut when one is choosing how to behave in a given situation. By simply registering what most others are doing there and by imitating their actions, one can usually choose efficiently and well. Researchers have repeatedly found that the perception of what most others are doing influences subjects to behave similarly, even when the behaviors are as morally neutral as choosing a consumer product (Venkatesan, 1966) or looking up at the sky (Milgram, Bickman, and Berkowitz, 1969). The injunctive meaning of norms refers to rules or beliefs as to what constitutes morally approved and disapproved conduct. In contrast to descriptive norms, which specify what is done, injunctive norms specify what ought to be done. That is, rather than simply informing one's actions, these norms enjoin it through the promise of social sanctions. Because what is approved is often what is typically done, it is easy to confuse these two meanings of norms. However, they are conceptually and motivationally distinct, and it is important for a proper understanding of normative influence to keep them separate, especially in situations where both are acting simultaneously.

A second source of confusion surrounding the concept of social norms is that, although they are said to characterize and guide behavior within a society, they should not be seen as uniformly in force at all times and in all situations. That is, norms should motivate behavior primarily when they are activated (i.e., made salient or otherwise focused on); thus, persons who are dispositionally or temporarily focused on normative considerations are most likely to act in norm-consistent ways (Berkowitz, 1972; Berkowitz and Daniels, 1964; Gruder, Romer, and Korth, 1978; Miller and Grush, 1986; Rutkowski, Gruder, and Romer, 1983; Schwartz and Fleishman, 1978). Of course, salience procedures should be effective for both descriptive and injunctive norms. In fact, in situations with clear-cut descriptive and injunctive norms, focusing individuals on *is* versus *ought* information should lead to behavior change that is consistent only with the now more salient type of norm.

One purpose of this research was to test this assertion as it applies to individuals' decisions to litter in public places. The choice of littering behavior for this study occurred for several reasons: (a) it provides a clearly observable action that

is governed by a widely held injunctive norm (Bickman, 1972; Heberlein, 1971; Keep America Beautiful, Inc., 1968) and (b) it constitutes a growing social problem of considerable aesthetic, financial, and health-related costs to the culture. In California alone, for example, litter has increased by 24% over a recent span of 15 years, requiring $100 million annually in cleanup costs (California Waste Management Board, 1988) and posing health threats to humans and wildlife through water pollution, fire hazards, rodent and insect infestations, highway accidents, and thousands of injuries suffered from discarded cans and broken bottles (Geller, Winett, and Everett, 1982). Thus, a better understanding of the normative factors moderating deliberate littering would be of both conceptual and practical value.

A common finding in the literature on littering is that the act is significantly more likely in a littered setting than in a clean setting (e.g., Finnie, 1973; Geller, Witmer, and Tuso, 1977; Heberlein, 1971; Krauss, Freedman, and Whitcup, 1978; Reiter and Samuel, 1980). Although this finding is congruent with the normative view that, in most settings, individuals tend to act in accordance with the clear behavioral norm there (Krauss et al., 1978), it is also consistent with other motivational accounts. For example, it might be argued that the tendency to litter more in a littered environment is due to simple imitation. Or, it might be argued that individuals are more likely to litter into a littered environment because they perceive that their litter will do less damage to the state of the environment than if it were clean.

STUDY 1

In our first experiment, subjects were given the opportunity to litter into either a previously clean or a fully littered environment after witnessing a confederate who either littered into the environment or walked through it. By varying the state of the environment (clean vs. littered), we sought to manipulate the perceived descriptive norm for littering in the situation. By manipulating whether the confederate dropped litter into the environment, we sought to affect the extent to which subjects were drawn to focus attention on the state of the environment and, consequently, on the relevant descriptive norm there.

We had two main predictions: First, we expected that subjects would be more likely to litter into an already littered environment than into a clean one. This expectation is consistent with the findings of prior research on littering (e.g., Krauss et al., 1978; Reiter and Samuel, 1980) and with the view that, in most settings, individuals are at least marginally aware of the existing norms and tend to act in accordance with them. Second, and more important, we expected the effect of the descriptive norm for littering in the situation (as indicated by the state of the environment) to be significantly enhanced when subjects' attention was drawn to the environment by a littering other. This expectation was predicated on considerable prior evidence (see Fiske and Taylor, 1984, for a review) indicating that substantial psychological impact can result from salience procedures involving simple shifts in the visual prominence of stimulus information, including

normative information (Feldman, Higgins, Karlovac, and Ruble, 1976; Ferguson and Wells, 1980; Manis, Dovalina, Avis, and Cardoze, 1980; Ruble and Feldman, 1976; Trope and Ginnosar, 1986). Specifically, then, we predicted an interaction such that subjects who saw the confederate litter into a fully littered environment would litter more than those who saw no such littering; whereas subjects who saw the confederate litter into a clean environment would litter less than those who saw no such littering.

Should we obtain this interaction, we would have good support for our focus model of normative conduct. It should be noted that the second component of this predicted interaction adds important conceptual weight to our test in that it is contrary to what would be anticipated by rival accounts. It is opposite to what would be expected if subjects were motivated simply by a greater reluctance to litter into a clean versus littered environment because of the greater relative damage to the respective environments that such littering would cause; by that account, subjects should be more likely to litter after observing littering in a clean environment because the environment will have already been damaged. Similarly, the second component of our predicted interaction pits the norm focus/salience interpretation against a straightforward imitation formulation, in which an unpunished litterer would be expected to increase the littering tendencies of observers in either type of environment. By postulating that a littering other will concentrate attention on evidence of what the majority of people have done, thereby highlighting normative considerations, only the (descriptive) norm focus/salience account predicts that observed littering will reduce subsequent littering in a clean environment.

METHOD

Subjects and Procedure

Norm salience. Subjects were 139 visitors to a university-affiliated hospital who were returning to their cars in an adjacent, multilevel parking garage during the daylight hours of 5 days within a period of 8 consecutive days. Approximately 5 s after emerging from an elevator, subjects encountered an experimental confederate of college age walking toward them. In half of the instances, the confederate appeared to be reading a large, 21.6 × 35.6 cm (8½ × 14 in.) handbill, which he or she dropped into the environment approximately 4.5 cm (5 yd) before passing the subjects (high norm salience). A second confederate judged whether a subject had noticed the littering incident and, consequently, had deflected his or her attention at least momentarily to the parking garage floor. The great majority (93%) were judged to have done so, and only they were examined as to their subsequent littering behavior. In the other half of the instances, the confederate merely walked past the subject without carrying a handbill, so as to provide an equivalent degree of social contact (low norm salience).

Existing descriptive norm. For some of the subjects, the floor of the parking structure had been heavily littered by the experimenters with an assortment of handbills, candy wrappers, cigarette butts, and paper cups (existing prolittering norm). For the remaining subjects, the area had been cleaned of all litter (existing antilittering norm). The state of the environment (littered or clean) was alternated in 2-hr blocks, with the initial state determined randomly at the start of each day. On arriving at their cars, subjects encountered a large handbill that was tucked under the driver's side windshield wiper so as to partially obscure vision from the driver's seat. The handbill, identical to that dropped by the confederate, carried a stenciled message that read, "THIS IS AUTOMOTIVE SAFETY WEEK. PLEASE DRIVE CAREFULLY." A similar handbill had been placed on all other cars in the area as well.

Measure of littering. From a hidden vantage point, an experimenter noted the driver's sex, estimated age, and whether the driver littered the handbill. Littering was defined as depositing the handbill in the environment outside of the vehicle. Because there were no trash receptacles in the area, all subjects who failed to litter did so by taking and retaining the handbill inside their vehicles before driving away.

Analyses

Analyses in this and subsequent studies were conducted using the SPSS-X loglinear program, wherein tests for effects within dichotomous data are examined through the nesting of hierarchical models. This technique allows the testing of individual parameters by comparing the differences in the likelihood ratio chi-square of a pair of nested models. The differenced likelihood ratio is reported as a chi-square.

RESULTS AND DISCUSSION

Gender and age differences in littering have sometimes been found in past research (see Geller et al., 1982, for a review). Therefore, before proceeding to tests of our theoretical hypotheses, we explored the data for gender or age differences. None were found; consequently, neither variable was included in subsequent analyses.

Figure 6.1 depicts the amount of littering that occurred in each of the four experimental conditions. Loglinear analysis of those data produced a set of results that conforms to that predicted by our norm focus model. First, as expected, there was a main effect for the existing descriptive norm, in that subjects littered more in a littered environment than in a clean environment (41% vs. 11%), $\chi^2(1, N = 139) = 17.06$, $p < .001$. Second, this effect occurred to a much greater extent under conditions of high norm salience, when subjects' attention was drawn to the existing de-

scriptive norm for the environment. That is, the size of the existing descriptive-norm effect when the confederate littered (6% vs. 54%), $\chi^2(1, N = 55) = 16.52$, $p < .001$, was significantly greater than when the confederate did not litter (14% vs. 32%), $\chi^2(1, N = 84) = 3.99$, $p < .05$; the resultant interaction was tested as a planned comparison that proved highly reliable, $\chi^2(1, N = 139) = 20.87$, $p < .001$. The significant interaction provides confirmation of our hypothesis that procedures designed to shift attention within a setting to just one type of operative norm—in this case, the descriptive norm—will generate behavior change that is consistent only with that type of norm. Apparently, this is so even when the behavior in question is governed by an injunctive norm—in this case, the antilittering norm—that is strongly and widely held in the society (Bickman, 1972; Heberlein, 1971; Keep America Beautiful, Inc., 1968).

The pattern of results also supported the directional predictions made from our model. That is, under conditions of high (descriptive) norm salience, subjects littered more in a littered environment (54% vs. 32%) but less in a clean one (6% vs. 14%), although neither simple effect was statistically significant, χ^2s = 2.76 and 1.18, respectively.

It is this latter finding, showing the least littering among subjects in the high norm salience/clean environment condition, that seems the most provocative of our study and, therefore, worthy of pursuit. After all, from an applied standpoint, we should be principally interested in strategies for litter abatement. Moreover, the fact that the least littering occurred among subjects who observed prior littering into a clean environment is of considerable conceptual interest, as it supports norm focus predictions over those that spring from a straightforward imitation or environmental damage account. Good reason exists, however, for caution in drawing strong conceptual conclusions from this finding. Although part of a theoretically

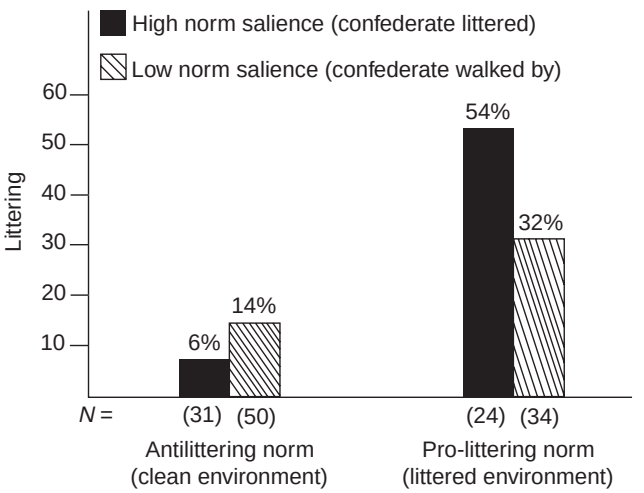

FIGURE 6.1
Percentages of subjects littering as a function of norm salience, and the direction of the descriptive norm regarding littering: Study 1.

predicted, significant interaction, the drop in littering due to high norm salience in the clean environment was far from significant by itself. Of course, this lack of significance might well have occurred because of a floor effect, owing to the low level of littering (14%) in the low norm salience/clean environment condition; nonetheless, in the interest of enhanced statistical confidence, a replication seemed warranted.

STUDY 2

In planning to replicate and extend our initial study, we recognized a pair of testable implications that flowed from our earlier analysis. First, consistent with the outcomes of Study 1, a subject who witnessed evidence of littering in an otherwise clean environment should litter less as a result; however, the evidence would not have to take the form, as it did in Study 1, of observed littering action. That is, the consequence of such action—a single piece of litter lying in an otherwise clean environment—should have the same effect, because of its conspicuousness, by drawing attention to an environment whose descriptive norm (except for one aberrant litterer) was clearly antilitter. Second, as the amount of litter increases progressively in a setting, so should the likelihood that a subject will litter into it because, by definition, that litter will change the descriptive norm for the setting. The upshot of this pair of implications of our normative analysis is a nonintuitive prediction: The likelihood that an individual will litter into an environment bearing various pieces of perceptible, extant litter will be described by a checkmark-shaped function. Little littering should occur in a clean environment; still less should occur with a sole piece of litter in an otherwise clean environment, but progressively greater littering should occur as litter accumulates and the descriptive norm for the situation changes from antilitter to prolitter.

METHOD

Subjects and Procedure

Subjects were 358 visitors to an amusement park in a large southwestern city during the evening hours of a pair of weekends in early summer. Immediately before turning a particular corner on a park walkway, subjects encountered a college-age experimental confederate passing out handbills that read “DON’T MISS TONIGHT’S SHOW,” which referred to an entertainment program sponsored by the park on weekend nights. The confederate was instructed to give a handbill, at 1-min intervals, to the first passing adult walking alone or to one adult (the physically closest) in the first passing group. On turning the walkway corner, subjects, who were no longer visible to the confederate, faced a path of approximately 55 m (60 yd) from which no exit was possible except at its ends.

State of the environment. All litter had been removed from the path except for varying numbers of handbills of the sort that subjects had just been given by a confederate. Depending on the experimental condition, the path contained 0, 1, 2, 4, 8, or 16 handbills that were visible from the path entrance.

Measurement of littering. Because no litter receptacles were available on the path, a subject who deposited a handbill into the environment at any point along the path's length was considered a litterer. Subjects' littering behavior was covertly observed by a hidden, second experimental confederate, who also timed subjects' latency to litter (failure to litter was given a score of 100 s) and who removed any newly littered handbills from the path. On exiting the path, subjects turned a corner to find a pair of previously unseen litter receptacles; virtually all subjects who had not littered to that point dropped their handbills into one of the receptacles.

RESULTS AND DISCUSSION

As in Study 1, we first examined the littering data for age and gender differences. No significant effects were obtained because of subject age. However, we did find a significant tendency for men to litter more frequently than women (31% vs. 19%), $\chi^2(1, N = 358) = 7.41$, $p < .01$.

Figure 6.2 depicts the percentage of litterers in each of the experimental conditions of Study 2. The data pattern closely reflects the predicted checkmark shape of our normative analysis. The checkmark function hypothesis was tested in a two-step process. First, we constructed a planned comparison using trend weights that

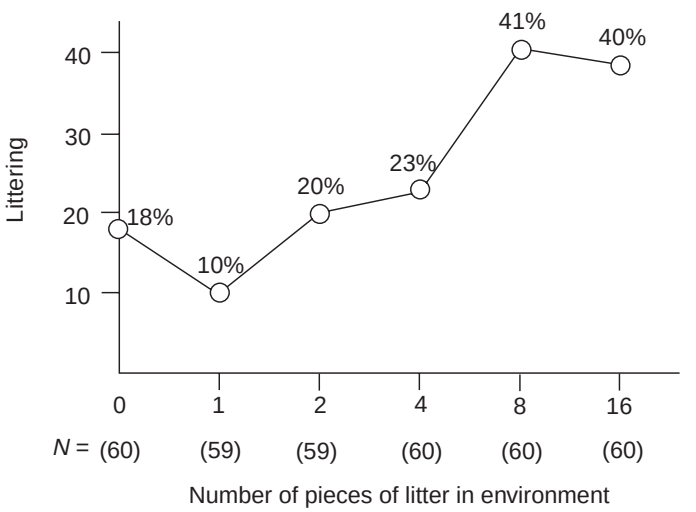

FIGURE 6.2
Percentages of subjects littering as a function of the number of pieces of litter in the environment: Study 2.

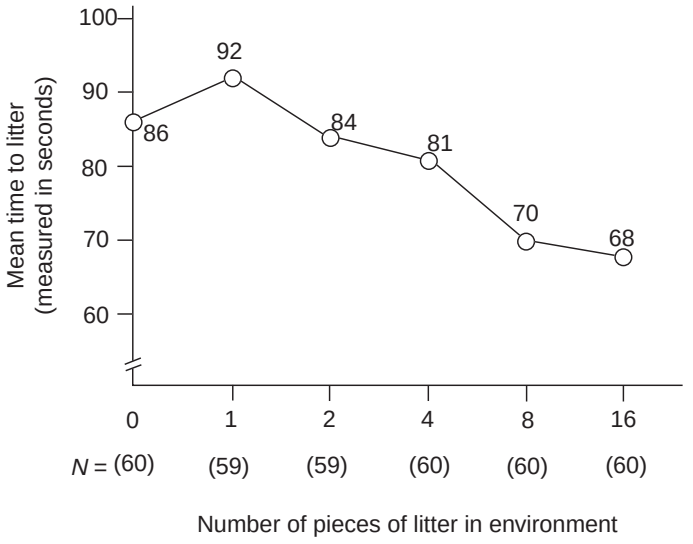

FIGURE 6.3
Mean latency to litter as a function of the number of pieces of litter in the environment: Study 2.

modeled the checkmark shape (−2, −4, −1, 1, 2, 4). It proved significant, $\chi^2(1, N = 358) = 21.80$, $p < .01$. A second planned comparison was then performed to test whether a difference in littering occurred between the zero littering condition and the one-piece-of-environmental-litter condition. No significant difference was found, $\chi^2(1, N = 229) = 1.64$, $p < .20$. Comparable analyses were conducted on the latency to litter data shown in Figure 6.3. As with frequency to litter, the first contrast proved significant, $F(1, 352) = 20.65$, $p < .01$, whereas the second did not ($F < 1$). There was no significant interaction between any of these contrasts and gender.

STUDY 3

Even though the general form of the findings of Study 2 confirmed our predictions, one crucial feature of the results offered only ambiguous support. The hypothesized decline in littering from the clean environment condition to the one-piece-of-litter condition of the study, although present (18% vs. 10%), was not conventionally significant, allowing the possibility that it may have been the overall linearity of the checkmark pattern, rather than its elbow-like bend, that accounted for the significance of our general planned comparison. This ambiguity is especially frustrating because, as in Study 1, it appears that a floor effect in the data may have prevented a clear demonstration of reduced littering under the circumstances predicted by our formulation. It is difficult to generate significantly less littering than that of a clean environment when the clean environment generates so little littering itself.

Consequently, we decided to conduct a conceptual replication of the theoretically relevant conditions of Study 2 that was designed to overcome the floor-effect problem. One way to deal with a floor effect of the sort that faced us is to increase the statistical power associated with our significance tests by increasing the number of subjects run in each condition. Thus, we used an experimental setting that would allow us to record the littering decisions of large numbers of subjects in a relatively short period of time. Additionally, in an attempt to sharpen the impact of our single-piece-of-litter manipulation, we chose a more conspicuous single piece of litter than we had used in Study 2.

Specifically, subjects were college dormitory residents who found a public service flier in their mailboxes. The environment in front of the mailboxes had been arranged so that it contained (a) no litter, (b) one piece of highly conspicuous litter (a hollowed-out, end piece of watermelon rind), or (c) a large array of various types of litter, including the watermelon rind. The dependent variable was subjects' tendencies to litter with the fliers. On the basis of our normative analysis and the pattern of results of Studies 1 and 2, we made a pair of predictions. First, we anticipated that subjects would litter more into a fully littered environment than into a clean one. Second, we expected that they would litter least into an otherwise clean environment that contained a single, attention-focusing piece of litter.

METHOD

Subjects

Subjects were 484 residents of a densely populated, high-rise women's dormitory on the campus of a large state university.

Procedure

The residents' mailboxes were located in rows at one corner of the dormitory's main lobby. The mailbox area was cut off visually from most of the lobby by a translucent partition. Once past the partition, subjects encountered an open area that fronted the mailboxes. During a 10 a.m. to 4 p.m. schoolday period, residents who opened their mailboxes to find a public service flier placed there as part of the experiment were counted as subjects, provided that no one else was simultaneously in the area getting her mail.

Depending on the experimental condition, subjects passing through the open area in front of their mailboxes encountered an environment that contained no litter or a single piece of litter (a hollowed-out, heel section of watermelon rind), or a large number of pieces of litter of various kinds (e.g., discarded fliers, cigarette butts, paper cups, candy wrappers, and soft drink cans), including the watermelon rind. A subject was considered to have littered if she deposited the flier anywhere

in the environment (all waste containers had been removed) before exiting the lobby onto an elevator or through a set of doors leading to the campus. Of those subjects who littered, the great majority were observed by an unobtrusively placed experimenter to do so in the area in front of the mailboxes.

RESULTS AND DISCUSSION

The percentages of littering in the three experimental conditions are presented in Figure 6.4. Their pattern accords well with predictions based on our normative perspective; indeed, the expected quadratic trend was highly significant, $\chi^2(1, N = 484) = 23.12$, $p < .001$. Moreover, planned contrast tests of our two experimental predictions were supportive at conventional levels of significance. First, subjects were more likely to litter into a fully littered environment than into an unlittered one (26.7% vs. 10.7%), $\chi^2(1, N = 291) = 12.62$, $p < .001$. Second, subjects were less likely to litter into an environment when it contained a single, salient piece of litter than when it was unlittered (3.6% vs. 10.7%), $\chi^2(1, N = 335) = 6.79$, $p < .01$.

Theoretical Implications

To this point, we have reported data from three experiments in three different natural settings that seem to converge sufficiently to allow the generation of statements about the conceptual and pragmatic value of those data. On the conceptual side, it appears that norms can be influential in directing human action; however,

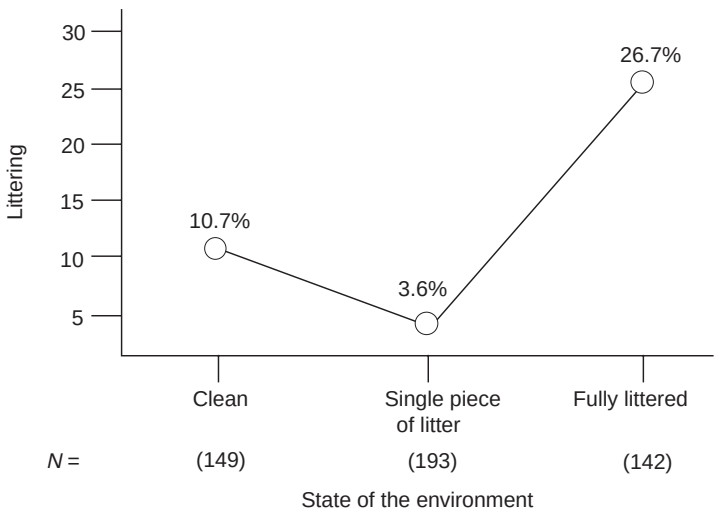

FIGURE 6.4
Percentages of subjects littering as a function of the amount of litter in the environment: Study 3.

in keeping with the spirit of prior criticism of normative explanations, it is necessary for norm theorists to be specific about both the type of norm (injunctive or descriptive) thought to be acting in a situation and about the conditions under which it is likely to act. Distinguishing between injunctive and descriptive norms is crucial, because both types can exist simultaneously in a setting and can have either congruent or contradictory implications for behavior. For example, in Study 1 we showed that through procedures designed to highlight differing descriptive norms, we could enhance or undermine compliance with the societywide injunctive norm against littering. Such a finding should not be interpreted to mean that descriptive norms are, in this instance or in general, more powerful than injunctive norms. Rather, it is the differential focusing of attention on one or the other sort of norm that is the key. Indeed, even within the same type of norm, it seems to be the case from our findings that focus of attention is an important component. In all three experiments, exposing subjects to a single piece of litter in an otherwise clean environment—a procedure designed to draw subjects' attention to what most people had done in the setting (i.e., the descriptive norm)—reduced littering there.

Practical Applications

Because littering is a social problem, it is appropriate to consider the potential practical applications of our data as well. The finding of greatest applied value appears to be that subjects in three different settings littered least after encountering a single piece of litter in an otherwise unlittered place. At first glance, such a result might seem to suggest that individuals seeking to retard the accumulation of litter in a particular environment might affix a single, prominent piece of litter there. On closer consideration, however, it becomes clear that such an approach would be inferior to beginning with a totally clean environment. Examination of Figures 6.2 and 6.3, showing the average likelihood and latency of littering among subjects in our amusement park study, illustrates the point. Subjects who encountered a perfectly clean environment tended not to litter there, resulting in long delays before anyone despoiled it with a handbill. Once a single handbill appeared in the setting, subjects were even less likely to litter, generating even longer latencies before the second piece of litter appeared. At that point, with two pieces of litter visible in the environment, the descriptive norm began to change, and subjects' reluctance to litter into the setting began to deteriorate steadily, leading to shorter and shorter littering latencies with increasing accumulations of litter. Anyone wishing to preserve the state of a specific environment, then, should begin with a clean setting so as to delay for the greatest time the appearance of two pieces of litter there, because those two pieces of litter are likely to begin a slippery-slope effect that leads to a fully littered environment and to a fully realized perception that "everybody litters here." This logic further suggests that environments will best be able to retard littering if they are subjected to frequent and thorough litter pickups that return them to the optimal litter-free condition.

In considering the practical implications of our data, we recognized a weakness in our decision to focus subjects' attention on the descriptive rather than injunctive norm for littering: Procedures that focus subjects on the descriptive norm will only reduce littering when the environment is wholly or virtually unspoiled. Indeed, as was suggested in the data of Study 1, a descriptive norm focus when the environment is substantially littered will tend to increase littering there—hardly a desirable outcome for any but theory-testing purposes. A descriptive norm-focusing procedure, then, should only have socially beneficial effects in environments that do not need much help. The circumstances are different, however, when the injunctive norm is made salient and when, consequently, individuals are focused on what people typically approve and disapprove rather than on what they typically do in a situation. By making the injunctive norm against littering more prominent, we should expect reduced littering even in a heavily littered environment.

A test of this hypothesis seemed instrumental to a pair of potentially valuable goals. First, on the practical level, it might establish norm focus procedures that could be used for litter abatement in a variety of environments. Second, on the conceptual level, it would generate evidence for or against our contention that focusing attention on either *is* or *ought* information will lead to behavior change that is consistent only with the now more salient type of norm; to this point in the research program, we had examined only half of that contention by concentrating just on descriptive norms.

STUDY 4

Recall that in Study 1, we argued that a confederate's act of dropping a flier into the environment would draw subjects' attention to that environment and to clear evidence (that we had manipulated) concerning whether people typically littered there. In this way, we sought to manipulate focus of attention to the existing descriptive norm regarding littering in the setting. Presumably, if instead the environment were to give clear evidence of what is societally approved or disapproved there, the same attention-focusing device would function as an injunctive norm activator, because societally based approval or disapproval is the distinguishing characteristic of injunctive norms (Birnbaum and Sagarin, 1976; Marini, 1984; Sherif and Sherif, 1969).

The question of what clear approval/disapproval cue could be placed effectively in a natural environment to test our formulation was answered serendipitously while conducting Study 1. That study was run in a parking garage whose walls rose only halfway from the floor to the roof at each level. On one especially windy day, the litter we had distributed all around the garage floor in the fully littered environment condition was blown against an inside wall, as if someone had swept it there in a neat line. When a confederate dropped a handbill into that environment, virtually no subjects littered, whereas, on previous days the majority of subjects in that experimental condition had littered. In the course of puzzling over the discrepancy,

we realized that the littering tendency of windy-day subjects may have declined when attention was called to the considerable litter in the environment because that litter gave the (mistaken) impression of having been swept—a clear disapproval cue.

Armed with this potential insight, we decided to conduct a partial replication and extension of Study 1, in which subjects saw a confederate who either did or did not drop a handbill into an environment that contained a large amount of either swept or unswept litter. In the case of unswept litter, we expected to replicate the data pattern of Study 1 for the comparable experimental cells; that is, we anticipated that by dropping a handbill, the confederate would focus subjects' attention on the environment and its evidence that people typically litter there, which should cause littering to increase. By dropping a handbill into a setting where prior litter had been swept (into piles), we anticipated that the confederate would once again focus subjects' attention on the environment. But in this instance, subjects would encounter a mixed message, composed of a descriptive norm cue (abundant litter) that would incline them toward littering and an injunctive norm cue (swept litter) that would incline them against it. Accordingly, we predicted that the difference in littering found in the unswept conditions would be reversed or at least reduced. Statistically, then, we expected an interaction between our two independent variables of whether a confederate dropped a handbill into the environment (high or low norm salience) and whether the environment contained swept or unswept litter (presence or absence of an injunctive norm cue). Furthermore, we expected a specific form for that interaction, such that any difference in littering found between the swept and unswept litter conditions under low-norm salience procedures would be significantly enhanced under high-norm salience procedures. That is, it was our belief that, under the low salience conditions, the normative forces present would be registered only minimally by subjects, resulting in only a minimal swept/unswept difference. However, under high salience conditions with normative issues now focal, the effect would be magnified.

METHOD

Subjects and Procedure

Norm salience. Subjects were 127 visitors to a university-affiliated hospital during the late afternoon and early evening hours of 6 days within a 13-day period. They underwent the same norm salience procedures as subjects in Study 1. That is, after emerging from a parking garage elevator, they encountered a college-age confederate who either dropped a distinctively colored handbill onto the floor in subjects' view or simply walked past without carrying a handbill.

Presence of an injunctive norm cue. For some subjects, the floor of the parking structure had been heavily littered by the experimenters, with the litter

distributed across the environment in a fashion identical to that of Study 1. For the remaining subjects, all of this ambient litter had been swept into three large piles situated approximately 9 m (10 yd) apart in a line. In the high-norm salience/swept litter condition, the confederate dropped a handbill onto the floor approximately 1.5 m (5 ft) after passing the piles of litter. It was decided to have the confederate drop the handbill immediately in front, but in full view, of the litter piles to avoid an imitation explanation for our predicted effect. That is, if subjects had seen the confederate drop a handbill into one of the piles, then the predicted reduction in subjects' subsequent littering could be interpreted as simple modeling of a decision not to litter. The swept or unswept litter conditions were run in alternating 2-hr blocks, with the first run of the day determined randomly.

Measure of littering. Littering was assessed as it was in Study 1.

RESULTS AND DISCUSSION

The influence of age and gender on littering rates was examined in an initial analysis; no significant effects occurred. Thus, these variables were not included in further analyses.

The percentage of subjects who littered in each of the experimental conditions of our design is displayed in Figure 6.5. Those percentages occurred in a pattern consistent with the form of the interaction that we were led to anticipate from our norm focus formulation. Using loglinear analyses, we tested that interaction with a planned comparison that contrasted the difference between the two low-norm salience cells (29% vs. 33%), $\chi^2(1, N = 68) = 0.18$, *ns*, against the difference be-

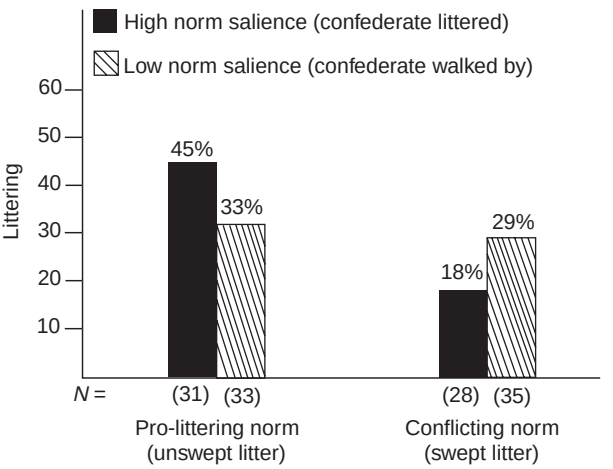

FIGURE 6.5
Percentages of subjects littering as a function of norm salience, and the configuration of litter in the environment: Study 4.

tween the two high-norm salience cells (18% vs. 45%), $\chi^2(1, N = 59) = 5.19$, $p < .02$. That interaction test proved significant, $\chi^2(1, N = 127) = 4.91$, $p < .03$.

Looking at the interaction pattern in another way, we can see that it is composed of two opposing trends—neither significant by itself, but significantly different in contrast to one another—both instigated by the same attention-focusing procedure. That is, when a dropped handbill drew attention to an unswept environment that, by its fully littered nature, gave evidence of a clear descriptive norm favoring littering there, littering tendencies rose (33% vs. 45%). However, when the same device drew attention to an environment that included a clearly conflicting injunctive norm cue as well, littering tendencies were reversed (29% vs. 18%). This pattern of effects accords well with each of the goals we set for Study 4. First, it supports our theoretical assertion that both descriptive and injunctive norms can elicit behavior change, with the prominence of one or the other type of norm accounting for the direction of the change. Second, it offers grounds for hope that certain kinds of undesirable action (littering, drinking and driving, tax cheating, highway speeding, etc.) can be restrained by the use of procedures that temporarily focus individuals on injunctive norms in the settings where the action is most likely to take place.

STUDY 5

To this point in our research program, we have examined the validity of our norm focus formulation by using an attention-focusing procedure designed to make subjects mindful of a specific descriptive norm (Studies 1–3) or of conflicting descriptive and injunctive norms (Study 4) governing littering in a situation. The first three studies found resultant behavior changes wholly in line with the descriptive norm. The fourth study, which added evidence of a contradicting injunctive norm to the perception of the existing descriptive norm, broke the dominance of the descriptive norm over subjects' behavior; it actually produced a (nonsignificant) reduction of littering in an environment where a clear, prolittering descriptive norm existed. It seemed to us that the logical next step in this progression was to conduct one additional study that removed any prolittering descriptive norm focus and that concentrated subjects exclusively on the injunctive, antilittering norm. It was our expectation that such an uncontaminated, injunctive norm focus would then lead to a significant reduction in littering.

We saw another reason for conducting an additional experiment. In Studies 1 through 4, our norm-focusing manipulation involved the dropping of a noticeable piece of litter into an environment (either by a seen or an unseen individual) so as to draw subjects' attention to the normative information present in that environment. There were several advantages of using that particular attention-focusing device, including the ability to make certain nonintuitive predictions that would not have flowed from rival theoretical accounts. We also recognized, however, that there would be certain drawbacks to using the same procedure yet again.

First, the generality of our conceptual argument could be seen as untested beyond the range of our specific norm salience manipulation. More important, though, using littering to highlight the norms related to littering could create interpretational ambiguities. That is, the littering act itself is not neutral. It carries social meanings (depending on the situation in which it occurs) that are likely to generate various kinds of perceptions of the littering agent. It is possible that one or another of these perceptions could have acted to incline subjects to follow or reject the litterer's lead. For instance, although it is unlikely that someone who littered into a fully littered environment, as occurred in Study 4, would be seen positively by subjects, someone who littered into an environment of neatly swept litter might be seen in an especially negative light; it is possible that this more negative view may have accounted for the reduction in littering among such subjects in Study 4. Similarly, it is conceivable that subjects in Studies 1 through 3 may have had an unpleasant reaction to any litterer who would litter into a previously clean setting and, hence, may have failed to litter so as to distance themselves from such an unsavory person.

To avoid interpretations of this sort, which are based on subjects' perceptions of a litterer, it was necessary to design a focus shift manipulation that would draw subjects' attention to the injunctive norm against littering but would do so without the action of a littering agent. To this end, in Study 5 we relied on the device of cognitive priming, wherein one concept can be activated in an individual by focusing that individual's attention on a related concept (see Higgins and Bargh, 1987, for a review). Most, although not all (cf. Ratcliff and McKoon, 1988), explanations of priming effects incorporate the notion of spreading activation, which posits that similar concepts are linked together in memory within a network of nodes and that activation of one concept results in the spreading of the activation along the network to other related concepts (Anderson, 1976, 1983; Collins and Loftus, 1975; McClelland and Rumelhart, 1981). A key determinant of whether the presentation of one concept will cause activation of another is their semantic or conceptual proximity.

If, as research by Harvey and Enzle (1981) indicates, norms are concepts stored in a network format, then focusing subjects on a particular norm should activate other norms that are perceived to be semantically close to it. Moreover, the greater the semantic proximity, the stronger should be the resultant activation. To test this possibility, we first had a large number of norms rated as to their similarity to the antilittering norm. Next, on the basis of those ratings, we selected three norms that, although alike in rated normativeness, differed in their perceived similarity (conceptual proximity) to the antilittering norm. Finally, we included reference to one or another of the norms on handbills that we placed on car windshields in a local library parking lot. We expected that the handbills containing a message reminding subjects of the most distant norm from the antilittering norm (voting) would be littered relatively often but that as the handbill messages referred to norms rated closer (energy conservation) and closer (recycling) to the antilittering norm, fewer and fewer subjects would litter them. We also expected that handbills containing

no normative message would be littered most of all, whereas handbills containing the target, antilittering message would be littered least.

METHOD

Preliminary Ratings Study

A list of 35 norms that had been generated by the researchers and their colleagues (e.g., "Driving at a safe speed," "Recycling," "Paying taxes," and "Not littering") were shown to 95 undergraduate psychology students during a class session at a large state university. The students were asked to indicate the extent to which they found each item on the list to be normative or nonnormative on 9-point scales, anchored by the labels *extremely normative* (1) and *not at all normative* (9); the scale midpoint was labeled *somewhat normative* (5). A definition of norms was provided at the top of the list that read "Norms are shared beliefs within a culture as to what constitutes socially appropriate conduct."

A second list was shown to a different class of 87 undergraduate psychology students at the same university during a meeting of their class. In addition to the definition of norms at the top of the list, this list contained comparisons of each of the selected norms with the norm against littering. Subjects were asked to "indicate how closely related you believe each of the pairs of norms are" on 9-point scales anchored by the labels *identical* (1) and *unrelated* (9); the scale midpoint was labeled *somewhat close* (5). Examples of the comparison items are "The norm against littering and the norm for recycling" and "The norm against littering and the norm for returning library books on time."

Selection of the experimental norms. Means for both types of ratings were computed. The norm for not littering was rated as 4.25 on the 9-point normativeness scale. We then limited our choices for the additional experimental norms to those that had means for both male and female subjects within one scale point of 4.25 on rated normativeness. From this pool and on the basis of the similarity scale ratings, we selected three norms to be close to, moderately close to, and far from the norm against littering. Those three norms and their rated distances from the norm against littering were, respectively, the norm for recycling (3.57), the norm for turning off lights when last to leave a room (5.74), and the norm for voting (7.12).

Generating the normative messages. For each of the four experimental norms, a message was constructed that was suitable for presentation on a handbill. For the antilittering norm (identical to the target norm), it read, "April is Keep Arizona Beautiful Month. Please Do Not Litter." For the recycling norm (close to the target norm), it read, "April is Preserve Arizona's Natural Resources Month. Please Recycle." For the turning off lights norm (moderately close to the target norm), it read, "April is Conserve Arizona's Energy Month. Please Turn Off Unnecessary

Lights." For the voting norm (far from the target norm), it read, "April is Arizona's Voter Awareness Month. Please Remember That Your Vote Counts." Finally, a control message was constructed that carried no injunctive norm; it read, "April is Arizona's Fine Art's Month. Please Visit Your Local Art Museum."

Subjects and Procedure

Participants were 133 female patrons and 126 male patrons of a municipal public library branch who parked their cars in the library lot. After leaving the library and returning to their cars, subjects found on the driver's side of the windshield a handbill that had been placed there by an experimenter. The handbill carried one of the five experimental messages designed to focus subjects differentially on the norm against littering. Drivers' decisions to litter the handbill were recorded by an unobtrusively placed observer. Typically, subjects who littered did so immediately after reading the handbill message and virtually always within 5 s of having done so. Consequently, we felt confident that the priminglike effects we anticipated were well within the range of priming-effect durations found by other investigators (see Higgins and Bargh, 1987, for a review). No efforts were made to change the moderate amount of naturally occurring litter on the library grounds and parking lot, which consisted of a variety of cigarette butts and an occasional paper cup or soft drink can.

RESULTS AND DISCUSSION

In tests for gender effects within the data, only the main effect was significant, $\chi^2(1, N = 259) = 3.92$, $p < .05$, indicating that men littered more frequently than women (22% vs. 14%). To examine our hypothesis that as the conceptual distance between the antilittering norm and the handbill messages increased, littering rates would increase commensurately, we conducted a trend analysis. Only the predicted, linear trend (displayed in Figure 6.6) proved significant, $\chi^2(1, N = 259) = 5.48$, $p < .02$. Within the five experimental message means, only one comparison was significant, that between the target, antilittering norm (10%) and the no-norm control message (25%), $\chi^2(1, N = 118) = 4.87$, $p < .03$.

As in Study 4, focusing subjects differentially on the injunctive norm against littering, this time through the processes of priming and spreading activation, led to littering rates corresponding to the predicted degree of injunctive norm focus. Thus, as expected, subjects in Study 5 (a) littered least after encountering a message focusing them directly on the antilittering norm, (b) littered progressively more frequently as the encountered (equally normative) messages directed focus progressively away from the antilittering norm, and (c) littered most when the encountered message was not normative.

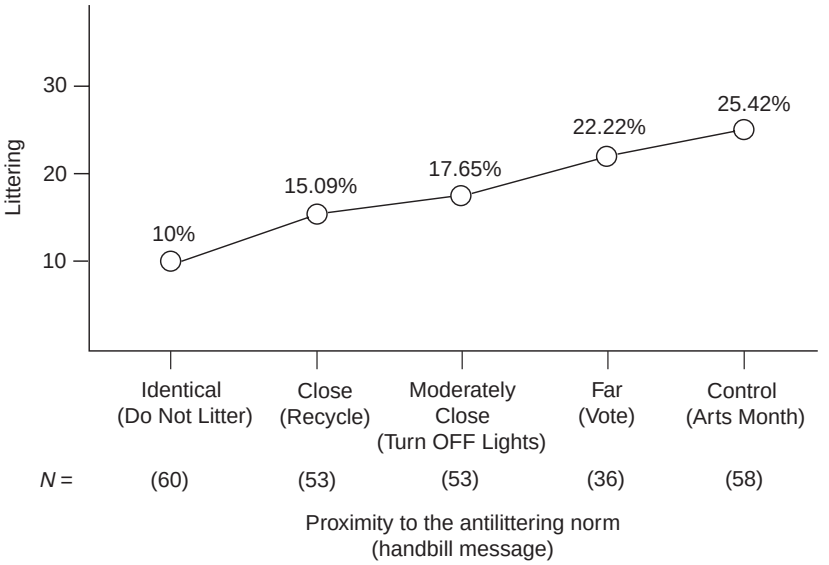

FIGURE 6.6
Percentages of subjects littering a handbill message as a function of its proximity to the injunctive norm against littering: Study 5.

GENERAL DISCUSSION

We began this article by reporting the mixed support for the utility of social norms in accounting for much of human behavior; the claim that the concept, as traditionally conceived, possesses great explanatory power currently has strong proponents and equally strong opponents. From the perspective of the research we have presented, it would appear that both camps are right. Norms clearly do have a considerable impact on behavior, but the force and form of that impact can only be usefully understood through conceptual refinements that have not been traditionally or rigorously applied. That is, to predict properly the likelihood of norm-consistent action requires, first, that one specify the type of norm—descriptive or injunctive—said to be operating. Second, one must take into account the various conditions that would incline individuals to focus attention on or away from the norm.

We have argued that our experimental manipulations worked to focus subjects on descriptive norms in Studies 1 through 3, on descriptive and injunctive norms in Study 4, and on injunctive norms in Study 5. Although the patterns of results in those studies are consistent with that argument, there is certainly room for alternative views. For example, it could be contended that, for subjects in Studies 1–3, seeing litter in an otherwise clean environment did not simply engage the descriptive norm against littering but engaged the injunctive norm as well. That is, a single piece of litter may have reminded subjects of societal objections to littering, and

thus it may have been the activation of the injunctive norm that produced reduced littering in those studies. Alternative accounts of this sort for specific segments of our data, although not parsimonious in explaining the overall pattern of results, remain conceivable nonetheless.

That is so in part because our work was conducted in naturally occurring field settings where it was not possible to assess the precision and effectiveness of our norm-focus manipulations through the methods typically available to laboratory investigators. Detailed checks on the strength, specificity, and functional impact of a subject's attentional focus could not have been practicably administered in our research situations. The consequent absence of such measures allows questions to arise as to whether our experimental manipulations worked as planned. Without the corroboration of these measures, one may have less confidence that the type of norm we intended to be functional actually mediated our findings. Fortunately, the effectiveness of injunctive social norms, about which there has been doubt in the scientific community (Darley and Latané, 1970; Garfinkel, 1967; Krebs, 1970; Krebs and Miller, 1985; Marini, 1984), has the clearest support in our data. That is, although it does seem possible to explain our data patterns without recourse to the well-established concept of descriptive norms, it does not seem plausible to do so without recourse to the more disputed and interesting concept of injunctive social norms, especially in Studies 4 and 5. Nonetheless, future research should be done in ways that allow direct assessments of the mediating processes presumed to be active in the present work.

Throughout this research program, we have exposed subjects to acute situational conditions designed to focus them on or away from particular norms. We recognize, however, that enduring cultural and dispositional conditions may also influence one's normative focus. This distinction among cultural, situational, and dispositional factors strikes us as important in the realm of norms. In thinking about the concept, we have been led to speculate that norms function at the cultural/societal level, the situational level, and the individual level. Although they may not have developed such a tripartite conceptualization, norm theorists have recognized normative influences at each of these levels. At the first (cultural/societal) level, the influence of global norms on behavior within a culture or social group has often been noted (Birnbaum and Sagarin, 1976; Paicheler, 1976; Pepitone, 1976; Triandis, 1977; Triandis, Marin, Lisansky, and Betancourt, 1984). Indeed, many definitions of norms refer exclusively to this level. For example, Ross (1973) considered norms to be "cultural rules that guide behavior within a society" (p. 105). At the second level, others have recognized that cultural norms may not apply equally to all situations (Peterson, 1982). Consequently, definitions of norms often include an explicit situational component. For example, Popenoe (1983) defined social norms as expectations "of how people are supposed to act, think, or feel in specific situations" (p. 598). Finally, other social scientists have evidence that norms exist at the individual level as

well. Most notable in this regard is the groundbreaking work of Schwartz (1973, 1977) on the concept of personal norms.

Our view is that what is normative (i.e., most often done or approved or both) in a society, in a setting, and within a person will, in each case, have demonstrable impact on action, but that the impact will be differential depending on whether the actor is focused on norms of the culture, the situation, or the self. Research is planned to test the implications of this conception.

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7

Making Sense of the Nonsensical: An Analysis of Jonestown

Neal Osherow

Those who do not remember the past are condemned to repeat it.

—quotation on placard over Jim Jones's rostrum at Jonestown

Close to one thousand people died at Jonestown. The members of the Peoples Temple settlement in Guyana, under the direction of the Reverend Jim Jones, fed a poison-laced drink to their children, administered the potion to their infants, and drank it themselves. Their bodies were found lying together, arm in arm; more than 900 perished.

How could such a tragedy occur? The image of an entire community destroying itself, of parents killing their own children, appears incredible. The media stories about the event and full-color pictures of the scene documented some of its horror but did little to illuminate the causes or to explain the processes that led to the deaths. Even a year afterward, a CBS Evening News broadcast asserted that "it was widely assumed that time would offer some explanation for the ritualistic suicide/murder of over 900 people. . . . One year later, it does not appear that any lessons have been uncovered" (CBS News, 1979).

The story of the Peoples Temple is not enshrouded in mystery, however. Jim Jones had founded his church more than 20 years before, in Indiana. His preaching stressed the need for racial brotherhood and integration, and his group helped feed the poor and find them jobs. As his congregation grew, Jim Jones gradually increased the discipline and dedication that he required from the members. In 1965, he moved to northern California; about 100 of his faithful relocated with him. The membership began to multiply, new congregations were formed, and the headquarters was established in San Francisco.

I am very grateful to Elliot Aronson for his assistance with this essay. His insights, suggestions, and criticism were most valuable to its development. Also, my thanks to Elise Bean for her helpful editing.

Behind his public image as a beloved leader espousing interracial harmony, "Father," as Jones was called, assumed a messiah-like presence in the Peoples Temple. Increasingly, he became the personal object of the members' devotion, and he used their numbers and obedience to gain political influence and power. Within the Temple, Jones demanded absolute loyalty, enforced a taxing regimen, and delivered sermons forecasting nuclear holocaust and an apocalyptic destruction of the world, promising his followers that they alone would emerge as survivors. Many of his harangues attacked racism and capitalism, but his most vehement anger focused on the "enemies" of the Peoples Temple—its detractors and especially its defectors. In mid-1977, publication of unfavorable magazine articles, coupled with the impending custody battle over a six-year-old Jones claimed as a "son," prompted emigration of the bulk of Temple membership to a jungle outpost in Guyana.

In November, 1978, Congressman Leo Ryan responded to charges that the Peoples Temple was holding people against their will at Jonestown. He organized a trip to the South American settlement; a small party of journalists and "Concerned Relatives" of Peoples Temple members accompanied him on his investigation. They were in Jonestown for one evening and part of the following day. They heard most residents praise the settlement, expressing their joy at being there and indicating their desire to stay. Two families, however, slipped messages to Ryan that they wanted to leave with him. After the visit, as Ryan's party and these defectors tried to board planes to depart, the group was ambushed and fired upon by Temple gunmen—five people, including Ryan, were murdered.

As the shootings were taking place at the jungle airstrip, Jim Jones gathered the community at Jonestown. He informed them that the Congressman's party would be killed and then initiated the final ritual: the "revolutionary suicide" that the membership had rehearsed on prior occasions. The poison was brought out. It was taken.

Jonestown's remoteness caused reports of the event to reach the public in stages. First came bulletins announcing the assassination of Congressman Ryan along with several members of his party. Then came rumors of mass-deaths at Jonestown, then confirmations. The initial estimates put the number of dead near 400, bringing the hope that substantial numbers of people had escaped into the jungle. But as the bodies were counted, many smaller victims were discovered under the corpses of larger ones—virtually none of the inhabitants of Jonestown survived. The public was shocked, then horrified, then incredulous.

Amid the early stories about the tragedy, along with the lurid descriptions and sensational photographs, came some attempts at analysis. Most discussed the charisma of Jim Jones and the power of "cults." Jones was described as "a character Joseph Conrad might have dreamt up" (Krause, 1978), a "self-appointed messiah" whose "lust for dominion" led hundreds of "fanatic" followers to their demise (Special Report: The Cult of Death, *Newsweek*, 1978a).

Although a description in terms of the personality of the perpetrator and the vulnerability of the victims provides some explanation, it relegates the event to the category of being an aberration, a product of unique forces and dispositions. Assuming such a perspective distances us from the phenomenon. This might be comforting,

but I believe that it limits our understanding and is potentially dangerous. My aim in this analysis is not to blunt the emotional impact of a tragedy of this magnitude by subjecting it to academic examination. At the same time, applying social psychological theory and research makes it more conceivable and comprehensible, thus bringing it closer (in kind rather than in degree) to processes each of us encounters. Social psychological concepts can facilitate our understanding: The killings themselves, and many of the occurrences leading up to them, can be viewed in terms of obedience and compliance. The processes that induced people to join and to believe in the Peoples Temple made use of strategies involved in propaganda and persuasion. In grappling with the most perplexing questions—Why didn't more people leave the Temple? How could they actually kill their children and themselves?—the psychology of self-justification provides some insight.

CONFORMITY

The character of a church . . . can be seen in its attitude toward its detractors.

—Hugh Prather, *Notes to Myself*

At one level, the deaths at Jonestown can be viewed as the product of obedience, of people complying with the orders of a leader and reacting to the threat of force. In the Peoples Temple, whatever Jim Jones commanded, the members did. When he gathered the community at the pavilion and the poison was brought out, the populace was surrounded by armed guards who were trusted lieutenants of Jones. There are reports that some people did not drink voluntarily but had the poison forced down their throats or injected (Winfrey, 1979). Although there were isolated acts of resistance and suggestions of opposition to the suicides, excerpts from a tape, recorded as the final ritual was being enacted, reveal that such dissent was quickly dismissed or shouted down:

> JONES: I've tried my best to give you a good life. In spite of all I've tried, a handful of people, with their lies, have made our life impossible. If we can't live in peace then let's die in peace. (Applause) . . . We have been so terribly betrayed. . . .
>
> What's going to happen here in the matter of a few minutes is that one of the people on that plane is going to shoot the pilot—I know that. I didn't plan it, but I know it's going to happen. . . . So my opinion is that you be kind to children, and be kind to seniors, and take the potion like they used to in ancient Greece, and step over quietly, because we are not committing suicide—it's a revolutionary act. . . . We can't go back. They're now going back to tell more lies. . . .
>
> FIRST WOMEN: I feel like that as long as there's life, there's hope.
>
> JONES: Well, someday everybody dies.
>
> CROWD: That's right, that's right!

JONES: What those people gone and done, and what they get through will make our lives worse than hell. . . . But to me, death is not a fearful thing. It's living that's cursed. . . . Not worth living like this.

FIRST WOMAN: But I'm afraid to die.

JONES: I don't think you are. I don't think you are.

FIRST WOMAN: I think there were too few who left for 1,200 people to give them their lives for those people who left. . . . I look at all the babies and I think they deserve to live.

JONES: But don't they deserve much more—they deserve peace. The best testimony we can give is to leave this goddam world. (Applause)

FIRST MAN: It's over, sister. . . . We've made a beautiful day. (Applause)

SECOND MAN: If you tell us we have to give our lives now, we're ready. (Applause) [*Baltimore Sun,* 1979.]

Above the cries of babies wailing, the tape continues, with Jones insisting upon the need for suicide and urging the people to complete the act:

JONES: Please get some medication. Simple. It's simple. There's no convulsions with it. . . . Don't be afraid to die. You'll see people land out here. They'll torture our people. . . .

SECOND WOMAN: There's nothing to worry about. Everybody keep calm and try to keep your children calm. . . . They're not crying from pain; it's just a little bitter tasting . . .

THIRD WOMAN: This is nothing to cry about. This is something we could all rejoice about. (Applause)

JONES: Please, for God's sake, let's get on with it. . . . This is a revolutionary suicide. This is not a self-destructive suicide. (Voices praise "Dad." Applause)

THIRD MAN: Dad has brought us this far. My vote is to go with Dad. . . .

JONES: We must die with dignity. Hurry, hurry, hurry. We must hurry. . . . Stop this hysterics. Death is a million times more perferable to spending more days in this life. . . . If you knew what was ahead, you'd be glad to be stepping over tonight . . .

FOURTH WOMAN: It's been a pleasure walking with all of you in this revolutionary struggle. . . . No other way I would rather go than to give my life for socialism. Communism, and I thank Dad very much.

JONES: Take our life from us. . . . We didn't commit suicide. We committed an act of revolutionary suicide protesting against the conditions of an inhuman world [*Newsweek*, 1978b, 1979].

If you hold a gun at someone's head, you can get that person to do just about anything. As many accounts have attested,[1] by the early 1970s the members of the Peoples Temple lived in constant fear of severe punishment—brutal beatings

[1]The reports of ex-Peoples Temple members who defected create a very consistent picture of the tactics Jim Jones employed in his church. Jeanne Mills (1979) provides the most comprehensive personal account, and there are affidavits about the Peoples Temple sworn to by Deborah Blakey (May 12, 1978 and June 15, 1978) and Yolanda Crawford (April 10, 1978). Media stories about the Peoples Temple, which usually rely on interviews with defectors, and about Jonestown, which are based on interviews with survivors, also corroborate one another. (See especially Kilduff and Tracy [1977], *Newsweek* [1978a], Lifton [1979], and Cahill [1979].)

coupled with public humiliation—for committing trivial or even inadvertent offenses. But the power of an authority need not be so explicitly threatening in order to induce compliance with its demands, as demonstrated by social psychological research. In Milgram's experiments (1963), a surprisingly high proportion of subjects obeyed the instructions of an experimenter to administer what they thought were very strong electric shocks to another person. Nor does the consensus of a group need be so blatantly coercive to induce agreement with its opinion, as Asch's experiments (1955) on conformity to the incorrect judgments of a majority indicate.

Jim Jones utilized the threat of severe punishment to impose the strict discipline and absolute devotion that he demanded, and he also took measures to eliminate those factors that might encourage resistance or rebellion among his followers. Research showed that the presence of a "disobedient" partner greatly reduced the extent to which most subjects in the Milgram situation (1965) obeyed the instructions to shock the person designated the "learner." Similarly, by including just one confederate who expressed an opinion different from the majority's, Asch (1955) showed that the subject would also agree far less, even when the "other dissenter's" judgment was also incorrect and differed from the subject's. In the Peoples Temple, Jones tolerated no dissent, made sure that members had no allegiance more powerful than to himself, and tried to make the alternative of leaving the Temple an unthinkable option.

Jeanne Mills, who spent six years as a high-ranking member before becoming one of the few who left the Peoples Temple, writes: "There was an unwritten but perfectly understood law in the church that was very important: 'No one is to criticize Father, his wife, or his children' " (Mills, 1979). Deborah Blakey, another long-time member who managed to defect, testified

> Any disagreement with [Jim Jones's] dictates came to be regarded as "treason." . . . Although I felt terrible about what was happening, I was afraid to say anything because I knew that anyone with a differing opinion gained the wrath of Jones and other members. [Blakey, June 15, 1978.]

Conditions in the Peoples Temple became so oppressive, the discrepancy between Jim Jones's stated aims and his practices so pronounced, that it is almost inconceivable that members failed to entertain questions about the church. But these doubts went unreinforced. There were no allies to support one's disobedience of the leader's commands and no fellow dissenters to encourage the expression of disagreement with the majority. Public disobedience or dissent was quickly punished. Questioning Jones's word, even in the company of family or friends, was dangerous—informers and "counselors" were quick to report indiscretions, even by relatives.

The use of informers went further than to stifle dissent; it also diminished the solidarity and loyalty that individuals felt toward their families and friends. While Jones preached that a spirit of brotherhood should pervade his church, he made it clear that each member's personal dedication should be directed to "Father." Fami-

lies were split: First, children were seated away from parents during services; then, many were assigned to another member's care as they grew up; and ultimately, parents were forced to sign documents surrendering custody rights. "Families are part of the enemy system," Jones stated, because they hurt one's total dedication to the "Cause" (Mills, 1979). Thus, a person called before the membership to be punished could expect his or her family to be among the first and most forceful critics (Cahill, 1979).

Besides splitting parent and child, Jones sought to loosen the bonds between wife and husband. He forced spouses into extramarital sexual relations, which were often of a homosexual or humiliating nature, or with Jones himself. Sexual partnerships and activities not under his direction and control were discouraged and publicly ridiculed.

Thus, expressing any doubts or criticism of Jones—even to a friend, child, or partner—became risky for the individual. As a consequence, such thoughts were kept to oneself, and with the resulting impression that nobody else shared them. In addition to limiting one's access to information, this "fallacy of uniqueness" precluded the sharing of support. It is interesting that among the few who successfully defected from the Peoples Temple were couples such as Jeanne and Al Mills, who kept together, shared their doubts, and gave each other support.

Why didn't more people leave? Once inside the Peoples Temple, getting out was discouraged; defectors were hated. Nothing upset Jim Jones so much; people who left became the targets of his most vitriolic attacks and were blamed for any problems that occurred. One member recalled that after several teen-aged members left the Temple, "We hated those eight with such a passion because we knew any day they were going to try bombing us. I mean Jim Jones had us totally convinced of this" (Winfrey, 1979).

Defectors were threatened: Immediately after she left, Grace Stoen headed for the beach at Lake Tahoe, where she found herself looking over her shoulder, checking to make sure that she hadn't been tracked down (Kilduff and Tracy, 1977). Jeanne Mills reports that she and her family were followed by men in cars, their home was burglarized, and they were threatened with the use of confessions they had signed while still members. When a friend from the Temple paid a visit, she quickly examined Mills's ears—Jim Jones had vowed to have one of them cut off (Mills, 1979). He had made ominous predictions concerning other defectors as well: Indeed, several ex-members suffered puzzling deaths or committed very questionable "suicides" shortly after leaving the Peoples Temple (Reiterman, 1977; Tracy, 1978).

Defecting became quite a risky enterprise, and, for most members, the potential benefits were very uncertain. They had little to hope for outside of the Peoples Temple; what they had, they had committed to the church. Jim Jones had vilified previous defectors as "the enemy" and had instilled the fear that, once outside of the Peoples Temple, members' stories would not be believed by the "racist, fascist" society, and they would be subjected to torture, concentration camps, and execution. Finally, in Guyana, Jonestown was surrounded by dense jungle, the few trails

patrolled by armed security guards (Cahill, 1979). Escape was not a viable option. Resistance was too costly. With no other alternatives apparent, compliance became the most reasonable course of action.

The power that Jim Jones wielded kept the membership of the Peoples Temple in line, and the difficulty of defecting helped to keep them in. But what attracted them to join Jones's church in the first place?

PERSUASION

Nothing is so unbelievable that oratory cannot make it acceptable.

—Cicero

Jim Jones was a charismatic figure, adept at oratory. He sought people for his church who would be receptive to his messages and vulnerable to his promises, and he carefully honed his presentation to appeal to each specific audience.

The bulk of the Peoples Temple membership was comprised of society's needy and neglected: the urban poor, the black, the elderly, and a sprinkling of ex-addicts and ex-convicts (Winfrey, 1979). To attract new members, Jones held public services in various cities. Leaflets would be distributed:

> PASTOR JIM JONES . . . Incredible! . . . Miraculous! . . . Amazing! . . . The Most Unique Prophetic Healing Service You've Ever Witnessed! Behold the Word Made Incarnate In Your Midst!
>
> God works as tumorous masses are passed in every service. . . . Before your eyes, the crippled walk, the blind see! [Kilduff and Javers, 1978.]

Potential members first confronted an almost idyllic scene of blacks and whites living, working, and worshipping together. Guests were greeted and treated most warmly and were invited to share in the group's meal. As advertised, Jim Jones also gave them miracles. A number of members would recount how Jones had cured them of cancer or other dread diseases; during the service Jones or one of his nurses would reach into the member's throat and emerge with a vile mass of tissue—the "cancer" that had been passed as the person gagged. Sometimes Jim Jones would make predictions that would occur with uncanny frequency. He also received revelations about members or visitors that nobody but those individuals could know—what they had eaten for dinner the night before, for instance, or news about a far-off relative. Occasionally, he performed miracles similar to more well-established religious figures:

> There were more people than usual at the Sunday service, and for some reason the church members hadn't brought enough food to feed everyone. It became apparent that the last fifty people in line weren't going to get any meat. Jim announced, "Even though there isn't enough food to feed this multitude, I am blessing the food that we have and multiplying it—just as Jesus did in biblical times."

> Sure enough, a few minutes after he made this startling announcement, Eva Pugh came out of the kitchen beaming, carrying two platters filled with fried chicken. A big cheer came from the people assembled in the room, especially from the people who were at the end of the line.
>
> The "blessed chicken" was extraordinarily delicious, and several of the people mentioned that Jim had produced the best-tasting chicken they had ever eaten. [Mills, 1979.]

These demonstrations were dramatic and impressive; most members were convinced of their authenticity and believed in Jones's "powers." They didn't know that the "cancers" were actually rancid chicken gizzards, that the occurrences Jones "forecast" were staged, or that sending people to sift through a person's garbage could reveal packages of certain foods or letters of out-of-town relatives to serve as grist for Jones' "revelations" (Kilduff and Tracy, 1977; Mills, 1979). Members were motivated to believe in Jones; they appreciated the racial harmony, sense of purpose, and relief from feelings of worthlessness that the Peoples Temple provided them (Winfrey, 1979; Lifton, 1979). Even when suspecting that something was wrong, they learned that it was unwise to voice their doubts:

> One of the men, Chuck Beikman . . . jokingly mentioned to a few people standing near him that he had seen Eva drive up a few moments earlier with buckets from the Kentucky Fried Chicken stand. He smiled as he said, "The person that blessed this chicken was Colonel Sanders."
>
> During the evening meeting Jim mentioned the fact that Chuck had made fun of his gift. "He lied to some of the members here, telling them that the chicken had come from a local shop," Jim stormed. "But the Spirit of Justice has prevailed. Because of his lie Chuck is in the men's room right now, wishing that he was dead. He is vomiting and has diarrhea so bad he can't talk!"
>
> An hour later a pale and shaken Chuck Beikman walked out of the men's room and up to the front, being supported by one of the guards. Jim asked him, "Do you have anything you'd like to say?"
>
> Chuck looked up weakly and answered, "Jim, I apologize for what I said. Please forgive me."
>
> As we looked at Chuck, we vowed in our hearts that we would never question any of Jim's "miracles"—at least not out loud. Years later, we learned that Jim had put a mild poison in a piece of cake and given it to Chuck. [Mills, 1979.]

Although most members responded to presentations that were emotional, one-sided, and almost sensational in tone, those who eventually assumed positions of responsibility in the upper echelons of the Peoples Temple were attracted by different considerations. Most of these people were white and came from upper-middle-class backgrounds—they included lawyers, a medical student, nurses, and people representing other occupations that demanded education and reflected a strong social consciousness. Jones lured these members by stressing the social and political aspects of the church, its potential as an idealistic experiment with integration and socialism. Tim Stoen, who was the Temple's lawyer, stated later, "I wanted utopia so damn bad I could die" (Winfrey, 1979). These members had the information and intelligence to see through many of Jones's ploys, but, as Jeanne Mills explains repeatedly in her book, they dismissed their qualms and dismissed Jones's deception

as being necessary to achieve a more important aim—furthering the Cause: "For the thousandth time, I rationalized my doubts. 'If Jim feels it's necessary for the Cause, who am I to question his wisdom?' " (Mills, 1979).

It turned out to be remarkably easy to overcome their hesitancy and calm their doubts. Mills recalls that she and her husband initially were skeptical about Jones and the Peoples Temple. After attending their first meeting, they remained unimpressed by the many members who proclaimed that Jones had healed their cancers or cured their drug habits. They were annoyed by Jones' arrogance, and they were bored by most of the long service. But in the weeks following their visit, they received numerous letters containing testimonials and gifts from the Peoples Temple, they had dreams about Jones, and they were attracted by the friendship and love they had felt from both the black and the white members. When they went back for their second visit, they took their children with them. After the long drive, the Mills family was greeted warmly by many members and by Jones himself. "This time . . . my mind was open to hear his message because my own beliefs had become very shaky" (Mills, 1979). As they were driving home afterward, the children begged their parents to join the church:

> We had to admit that we enjoyed the service more this time and we told the children that we'd think it over. Somehow, though, we knew that it was only a matter of time before we were going to become members of the Peoples Temple. [Mills, 1979.]

Jim Jones skillfully manipulated the impression that his church would convey to newcomers. He carefully managed its public image. He used the letter-writing and political clout of hundreds of members to praise and impress the politicians and press that supported the Peoples Temple, as well as to criticize and intimidate its opponents (Kasindorf, 1978). Most important, Jones severely restricted the information that was available to the members. In addition to indoctrinating members into his own belief system through extensive sermons and lectures, he inculcated a distrust of any contradictory messages, labelling them the product of enemies. By destroying the credibility of their sources, he inoculated the membership against being persuaded by outside criticism. Similarly, any contradictory thoughts that might arise within each member were to be discredited. Instead of seeing them as having any basis in reality, members interpreted them as indications of their own shortcomings or lack of faith. Members learned to attribute the apparent discrepancies between Jones's lofty pronouncements and the rigors of life in the Peoples Temple to their personal inadequacies rather than blaming them on any fault of Jones. As ex-member Neva Sly was quoted: "We always blamed ourselves for things that didn't seem right" (Winfrey, 1979). A unique and distorting language developed within the church, in which "the Cause" became anything that Jim Jones said (Mills, 1979). It was spoken at Jonestown, where a guard tower was called the "playground" (Cahill, 1979). Ultimately, through the clever use of oratory, deception, and language, Jones could speak of death as "stepping over," thereby camouflaging a hopeless act of self-destruction as a noble and brave act of "revolutionary suicide," and the members accepted his words.

SELF-JUSTIFICATION

Both salvation and punishment for man lie in the fact that if he lives wrongly he can befog himself so as not to see the misery of his position.

—Tolstoy, "The Kreutzer Sonata"

Analyzing Jonestown in terms of obedience and the power of the situation can help to explain why the people *acted* as they did. Once the Peoples Temple had moved to Jonestown, there was little the members could do other than follow Jim Jones's dictates. They were comforted by an authority of absolute power. They were left with few options, being surrounded by armed guards and by the jungle, having given their passports and various documents and confessions to Jones, and believing that conditions in the outside world were even more threatening. The members' poor diet, heavy workload, lack of sleep, and constant exposure to Jones's diatribes exacerbated the coerciveness of their predicament; tremendous pressures encouraged them to obey.

By the time of the final ritual, opposition or escape had become almost impossible for most of the members. Yet even then, it is doubtful that many *wanted* to resist or to leave. Most had come to believe in Jones—one woman's body was found with a message scribbled on her arm during the final hours: "Jim Jones is the only one" (Cahill, 1979). They seemed to have accepted the necessity, and even the beauty, of dying—just before the ritual began, a guard approached Charles Garry, one of the Temple's hired attorneys, and exclaimed, "It's a great moment . . . we all die" (Lifton, 1979). A survivor of Jonestown, who happened to be away at the dentist, was interviewed a year following the deaths:

> If I had been there, I would have been the first one to stand in that line and take that poison and I would have been proud to take it. The thing I'm sad about is this; that I missed the ending. [Gallagher, 1979.]

It is this aspect of Jonestown that is perhaps the most troubling. To the end, and even beyond, the vast majority of the Peoples Temple members *believed* in Jim Jones. External forces, in the form of power or persuasion, can exact compliance. But one must examine a different set of processes to account for the members' internalizing those beliefs.

Although Jones's statements were often inconsistent and his methods cruel, most members maintained their faith in his leadership. Once they were isolated at Jonestown, there was little opportunity or motivation to think otherwise—resistance or escape was out of the question. In such a situation, the individual is motivated to rationalize his or her predicament; a person confronted with the inevitable tends to regard it more positively. For example, social psychological research has shown that when children believe that they will be served more of a vegetable they dislike, they will convince themselves that it is not so noxious (Brehm, 1959), and

when a person thinks that she will be interacting with someone, she tends to judge a description of that individual more favorably (Darley and Berscheid, 1967).

A member's involvement in the Temple did not begin at Jonestown—it started much earlier, closer to home, and less dramatically. At first, the potential member would attend meetings voluntarily and might put in a few hours each week working for the church. Though the established members would urge the recruit to join, he or she felt free to choose whether to stay or to leave. Upon deciding to join, a member expended more effort and became more committed to the Peoples Temple. In small increments, Jones increased the demands made on the member, and only after a long sequence did he escalate the oppressiveness of his rule and the desperation of his message. Little by little, the individual's alternatives became more limited. Step by step, the person was motivated to rationalize his or her commitment and to justify his or her behavior.

Jeanne Mills, who managed to defect two years before the Temple relocated in Guyana, begins her account, *Six Years With God* (1979), by writing "Every time I tell someone about the six years we spent as members of the Peoples Temple, I am faced with an unanswerable question: 'If the church was so bad, why did you and your family stay in for so long?'" Several classic studies from social psychological research investigating processes of self-justification and the theory of cognitive dissonance (see Aronson, 1980, chapter 4; Aronson, 1969) can point to explanations for such seemingly irrational behavior.

According to dissonance theory, when a person commits an act or holds a cognition that is psychologically inconsistent with his or her self-concept, the inconsistency arouses an unpleasant state of tension. The individual tries to reduce this "dissonance," usually by altering his or her attitudes to bring them more into line with the previously discrepant action or belief. A number of occurrences in the Peoples Temple can be illuminated by viewing them in light of this process. The horrifying events of Jonestown were not due merely to the threat of force, nor did they erupt instantaneously. That is, it was *not* the case that something "snapped" in people's minds, suddenly causing them to behave in bizarre ways. Rather, as the theory of cognitive dissonance spells out, people seek to *justify* their choices and commitments.

Just as a towering waterfall can begin as a trickle, so too can the impetus for doing extreme or calamitous actions be provided by the consequences of agreeing to do seemingly trivial ones. In the Peoples Temple, the process started with the effects of undergoing a severe initiation to join the church, was reinforced by the tendency to justify one's commitments, and was strengthened by the need to rationalize one's behavior.

Consider the prospective member's initial visit to the Peoples Temple, for example. When a person undergoes a severe initiation to gain entrance into a group, he or she is apt to judge that group as being more attractive, in order to justify expending the effort or enduring the pain. Aronson and Mills (1959) demonstrated that students who suffered greater embarrassment as a prerequisite for being allowed to participate in a discussion group rated its conversation (which actually was quite boring) to be significantly more interesting than did those students who

experienced little or no embarrassment in order to be admitted. Not only is there a tendency to justify undergoing the experience by raising one's estimation of the goal—in some circumstances, choosing to experience a hardship can go so far as to affect a person's perception of the discomfort or pain he or she felt. Zimbardo (1969) and his colleagues showed that when subjects volunteered for a procedure that involves their being given electric shocks, those thinking that they had more choice in the matter reported feeling less pain from the shocks. More specifically, those who experienced greater dissonance, having little external justification to account for their choosing to endure the pain, described it as being less intense. This extended beyond their impressions and verbal reports; their performance on a task was hindered less, and they even recorded somewhat lower readings on a physiological instrument measuring galvanic skin responses. Thus, the dissonance-reducing process can be double-edged: Under proper guidance, a person who voluntarily experiences a severe initiation not only comes to regard its ends more positively, but may also begin to see the means as less aversive: "We begin to appreciate the long meetings, because we were told that spiritual growth comes from self-sacrifice" (Mills, 1979).

Once involved, a member found ever-increasing portions of his or her time and energy devoted to the Peoples Temple. The services and meetings occupied weekends and several evenings each week. Working on Temple projects and writing the required letters to politicians and the press took much of one's "spare" time. Expected monetary contributions changed from "voluntary" donations (though they were recorded) to the required contribution of a quarter of one's income. Eventually, a member was supposed to sign over all personal property, savings, social security checks, and the like to the Peoples Temple. Before entering the meeting room for each service, a member stopped at a table and wrote self-incriminating letters or signed blank documents that were turned over to the church. If anyone objected, the refusal was interpreted as denoting a "lack of faith" in Jones. Finally, members were asked to live at Temple facilities to save money and to be able to work more efficiently, and many of their children were raised under the care of other families. Acceding to each new demand had two repercussions: In practical terms, it enmeshed the person further into the Peoples Temple web and made leaving more difficult; on an attitudinal level, it set the aforementioned processes of self-justification into motion. As Mills (1979) describes:

> We had to face painful reality. Our life savings were gone. Jim had demanded that we sell the life insurance policy and turn the equity over to the church, so that was gone. Our property had all been taken from us. Our dream of going to an overseas mission was gone. We thought that we had alienated our parents when we told them we were leaving the country. Even the children whom we had left in the care of Carol and Bill were openly hostile toward us. Jim had accomplished all this in such a short time! All we had left now was Jim and the Cause, so we decided to buckle under and give our energies to these two.

Ultimately, Jim Jones and the Cause would require the members to give their lives.

What could cause people to kill their children and themselves? From a detached perspective, the image seems unbelievable. In fact, at first glance, so does the idea of so many individuals committing so much of their time, giving all of their money, and even sacrificing the control of their children to the Peoples Temple. Jones took advantage of rationalization processes that allow people to justify their commitments by raising their estimations of the goal and minimizing its costs. Much as he gradually increased his demands, Jones carefully orchestrated the members' exposure to the concept of a "final ritual." He utilized the leverage provided by their previous commitments to push them closer and closer to its enactment. Gaining a "foot in the door" by getting a person to agree to a moderate request makes it more probable that he or she will agree to do a much larger deed later, as social psychologists—and salespeople—have found (Freedman and Fraser, 1966). Doing the initial task causes something that might have seemed unreasonable at first appear less extreme in comparison, and it also motivates a person to make his or her behavior appear more consistent by consenting to the larger request as well.

After indoctrinating the members with the workings of the Peoples Temple itself, Jones began to focus on broader and more basic attitudes. He started by undermining the members' belief that death was to be fought and feared and set the stage by introducing the possibility of a cataclysmic ending for the church. As several accounts corroborate (see Mills, 1979; Lifton, 1979; Cahill, 1979), Jones directed several "fake" suicide drills, first with the elite Planning Commission of the Peoples Temple and later with the general membership. He would give them wine and then announce that it had been poisoned and that they would soon die. These became tests of faith, of the members' willingness to follow Jones even to death. Jones would ask people if they were ready to die and on occasion would have the membership "decide" its own fate by voting whether to carry out his wishes. An ex-member recounted that one time, after a while

> Jones smiled and said, "Well, it was a good lesson. I see you're not dead." He made it sound like we needed the 30 minutes to do very strong, introspective type of thinking. We all felt strongly dedicated, proud of ourselves. . . . [Jones] taught that it was a privilege to die for what you believed in, which is exactly what I would have been doing. [Winfrey, 1979.]

After the Temple moved to Jonestown, the "White Nights," as the suicide drills were called, occurred repeatedly. An exercise that appears crazy to the observer was a regular, justifiable occurrence for the Peoples Temple participant. The reader might ask whether this caused the members to think that the actual suicides were merely another practice, but there were many indications that they knew that the poison was truly deadly on that final occasion. The Ryan visit had been climactic, there were several new defectors, the cooks—who had been excused from the prior drills to prepare the upcoming meal—were included, Jones had been growing increasingly angry, desperate, and unpredictable, and, finally, everyone could see the first babies die. The membership was manipulated, but they were not unaware that this time the ritual was for real.

A dramatic example of the impact of self-justification concerns the physical punishment that was meted out in the Peoples Temple. As discussed earlier, the threat of being beaten or humiliated forced the member to comply with Jones's orders: A person will obey as long as he or she is being threatened and supervised. To affect a person's *attitudes,* however, a mild threat has been demonstrated to be more effective than a severe threat (Aronson and Carlsmith, 1963) and its influence has been shown to be far longer lasting (Freedman, 1965). Under a mild threat, the individual has more difficulty attributing his or her behavior to such a minor external restraint, forcing the person to alter his or her attitudes to justify the action. Severe threats elicit compliance, but, imposed from the outside, they usually fail to cause the behavior to be internalized. Quite a different dynamic ensues when it is not so clear that the action is being imposed upon the person. When an individual feels that he or she played an active role in carrying out an action that hurts someone, there comes a motivation to justify one's part in the cruelty by rationalizing it as necessary or by derogating the victim by thinking that the punishment was deserved (Davis and Jones, 1960).

Let's step back for a moment. The processes going on at Jonestown obviously were not as simple as those in a well-controlled laboratory experiment; several themes were going on simultaneously. For example, Jim Jones had the power to impose any punishments that he wished in the Peoples Temple, and, especially toward the end, brutality and terror at Jonestown were rampant. But Jones carefully controlled how the punishments were carried out. He often called upon the members themselves to agree to the imposition of beatings. They were instructed to testify against fellow members, bigger members told to beat up smaller ones, wives or lovers forced to sexually humiliate their partners, and parents asked to consent to and assist in the beatings of their children (Mills, 1979; Kilduff and Javers, 1978). The punishments grew more and more sadistic, the beatings so severe as to knock the victim unconscious and cause bruises that lasted for weeks. As Donald Lunde, a psychiatrist who has investigated acts of extreme violence, explains:

> Once you've done something that major, it's very hard to admit even to yourself that you've made a mistake, and subconsciously you will go to great lengths to rationalize what you did. It's very tricky defense mechanism exploited to the hilt by the charismatic leader. [*Newsweek,* 1978a.]

A more personal account of the impact of this process is provided by Jeanne Mills. At one meeting, she and her husband were forced to consent to the beating of their daughter as punishment for a very minor transgression. She relates the effect this had on her daughter, the victim, as well as on herself, one of the perpetrators:

> As we drove home, everyone in the car was silent. We were all afraid that our words would be considered treasonous. The only sounds came from Linda, sobbing quietly in the back seat. When we got into our house, Al and I sat down to talk with Linda. She was in too much pain to sit. She stood quietly while we talked with her. "How do you feel about what happened tonight?" Al asked her.

> "Father was right to have me whipped," Linda answered. "I've been so rebellious lately, and I've done a lot of things that were wrong. . . . I'm sure Father knew about those things, and that's why he had me hit so many times."
>
> As we kissed our daughter goodnight, our heads were spinning. It was hard to think clearly when things were so confusing. Linda had been the victim, and yet we were the only people angry about it. She should have been hostile and angry. Instead, she said that Jim had actually helped her. We knew Jim had done a cruel thing, and yet everyone acted as if he were doing a loving thing in whipping our disobedient child. Unlike a cruel person hurting a child, Jim had seemed calm, almost loving, as he observed the beating and counted off the whacks. Our minds were not able to comprehend the atrocity of the situation because none of the feedback we were receiving was accurate. [Mills, 1979.]

The feedback one received from the outside was limited, and the feedback from inside the Temple member was distorted. By justifying the previous actions and commitments, the groundwork for accepting the ultimate commitment was established.

CONCLUSION

Only months after we defected from Temple did we realize the full extent of the cocoon in which we'd lived. And only then did we understand the fraud, sadism, and emotional blackmail of the master manipulator.

—Jeanne Mills, *Six Years with God*

Immediately following the Jonestown tragedy, there came a proliferation of articles about "cults" and calls for their investigation and control. From Synanon to Transcendental Meditation, groups and practices were examined by the press, which had a difficult time determining what constituted a "cult" or differentiating between those that might be safe and beneficial and those that could be dangerous. The Peoples Temple and the events at Jonestown make such a definition all the more problematic. A few hours before his murder, Congressman Ryan addressed the membership: "I can tell you right now that by the few conversations I've had with some of the folks . . . there are some people who believe this is the best thing that ever happened in their whole lives" (Krause, 1978). The acquiescence of so many and the letters they left behind indicate that this feeling was widely shared—or at least expressed—by the members.

Many "untraditional"—to mainstream American culture—groups or practices, such as Eastern religions or meditation techniques, have proven valuable for the people who experience them but may be seen as very strange and frightening to others. How can people determine whether they are being exposed to a potentially useful alternative way of living their lives or if they are being drawn to a dangerous one?

The distinction is a difficult one. Three questions suggested by the previous analysis, however, can provide important clues: Are alternatives being provided or taken away? Is one's access to new and different information being broadened or denied? Finally, does the individual assume personal responsibility and control or is it usurped by the group or by its leader?

The Peoples Temple attracted many of its members because it provided them an alternative way of viewing their lives; it gave many people who were downtrodden a sense of purpose, and even transcendence. But it did so at a cost, forcing them to disown their former friendships and beliefs and teaching them to fear anything outside of the Temple as "the enemy." Following Jones became the *only* alternative.

Indeed, most of the members grew increasingly unaware of the possibility of any other course. Within the Peoples Temple, and especially at Jonestown, Jim Jones controlled the information to which members would be exposed. He effectively stifled any dissent that might arise within the church and instilled a distrust in each member for contradictory messages from outside. After all, what credibility could be carried by information supplied by "the enemy" that was out to destroy the Peoples Temple with "lies"?

Seeing no alternatives and having no information, a member's capacity for dissent or resistance was minimized. Moreover, for most members, part of the Temple's attraction resulted from their willingness to relinquish much of the responsibility and control over their lives. These were primarily the poor, the minorities, the elderly, and the unsuccessful—they were happy to exchange personal autonomy (with its implicit assumption of personal responsibility for their plights) for security, brotherhood, the illusion of miracles, and the promise of salvation. Stanley Cath, a psychiatrist who has studied the conversion techniques used by cults, generalizes: "Converts have to believe only what they are told. They don't have to think, and this relieves tremendous tensions" (*Newsweek*, 1978a). Even Jeanne Mills, one of the better-educated Temple members, commented:

> I was amazed at how little disagreement there was between the members of this church. Before we joined the church, Al and I couldn't even agree on whom to vote for in the presidential election. Now that we all belonged to a group, family arguments were becoming a thing of the past. There was never a question of who was right, because Jim was always right. When our large household met to discuss family problems, we didn't ask for opinions. Instead, we put the question to the children, "What would Jim do?" It took the difficulty out of life. There was a type of "manifest destiny" which said the Cause was right and would succeed. Jim was right and those who agreed with him were right. If you disagreed with Jim, you were wrong. It was as simple as that. [Mills, 1979.]

Though it is unlikely that he had any formal exposure to the social psychological literature, Jim Jones utilized several very powerful and effective techniques for controlling people's behavior and altering their attitudes. Some analyses have compared his tactics to those involved in "brainwashing," for both include the control of communication, the manipulation of guilt, and dispensing power over people's existence (Lifton, 1979), as well as isolation, an exacting regimen, physical pressure,

and the use of confessions (Cahill, 1979). But using the term brainwashing makes the process sound too esoteric and unusual. There *were* some unique and scary elements in Jones' personality—paranoia, delusions of grandeur, sadism, and a preoccupation with suicide. Whatever his personal motivation, however, having formulated his plans and fantasies, he took advantage of well-established social psychological tactics to carry them out. The decision to have a community destroy itself was crazy, but those who performed the deed were "normal" people who were subjected to a tremendously influential situation, the victims of powerful internal forces as well as external pressures.

POSTSCRIPT

Within a few weeks of the deaths at Jonestown, the bodies had been transported back to the United States, the remnants of the Peoples Temple membership were said to have disbanded, and the spate of stories and books about the suicide/murders had begun to lose the public's attention. Three months afterward, Michael Prokes, who had escaped from Jonestown because he was assigned to carry away a box of Peoples Temple funds, called a press conference in a California motel room. After claiming that Jones had been misunderstood and demanding the release of a tape recording of the final minutes [quoted earlier], he stepped into the bathroom and shot himself in the head. He left behind a note, saying that if his death inspired another book about Jonestown, it was worthwhile (*Newsweek,* 1979).

POSTSCRIPT

Jeanne and Al Mills were among the most vocal of the Peoples Temple critics following their defection, and they topped an alleged "death list" of its enemies. Even after Jonestown, the Millses had repeatedly expressed fear for their lives. More than a year after the Peoples Temple deaths, they and their daughter were murdered in their Berkeley home. Their teen-aged son, himself an ex-Peoples Temple member, has testified that he was in another part of the large house at the time. At this writing, no suspect has been charged. There are indications that the Millses knew their killer—there were no signs of forced entry, and they were shot at close range. Jeanne Mills had been quoted as saying, "It's going to happen. If not today, then tomorrow." On the final tape of Jonestown, Jim Jones had blamed Jeanne Mills by name, and had promised that his followers in San Francisco "will not take our death in vain" (*Newsweek,* 1980).

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III

MASS COMMUNICATION, PROPAGANDA, AND PERSUASION

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8

Effects of Varying the Recommendations in a Fear-Arousing Communication

James M. Dabbs Jr. and Howard Leventhal

It has been suggested that divergent effects of fear arousal on attitude change can be caused by variations in the recommendations in a persuasive communication. In a three-way factorial design Ss were presented with communications manipulating fear of tetanus and the perceived effectiveness and painfulness of inoculation against tetanus. Inoculation was recommended for all Ss. It was expected that more Ss would take shots described as highly effective and not painful, and that this tendency would change as level of fear was increased. The manipulations of effectiveness and painfulness were perceived as intended, but they did not affect intentions to take shots or shot-taking behavior. The fear manipulation influenced both intentions and behavior, with higher fear producing greater compliance with the recommendations.

A number of studies have investigated the effects of fear arousal on persuasion. Although the majority report that fear increases persuasion, the picture is not completely clear. Facilitating effects of fear on persuasion have been reported in studies of dental hygiene practices (Haefner, 1965; Leventhal and Singer, 1966; Singer, 1965), tetanus inoculations (Leventhal, Jones, and Trembly, 1966; Leventhal, Singer, and Jones, 1965), safe driving practices (Berkowitz and Cottingham, 1960; Leventhal and Niles, 1965), and cigarette smoking (Insko, Arkoff, and Insko, 1965; Leventhal and Watts, 1966; Niles, 1964). However, under increasing levels of fear Janis and Feshbach (1953) observed no increase in acceptance of beliefs about the

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proper type of toothbrush to use, and Leventhal and Niles (1964) observed some decrease in acceptance of a recommendation to stop smoking. All these results are based on verbal measures of *attitude* change.

The picture is even less clear when one considers actual *behavior* change. The study on tetanus by Leventhal et al. (1965) reports that some minimal amount of fear is necessary for behavior change, but that further increases in fear do not affect change. The later study by Leventhal et al. (1966) reports a slight tendency for increases in fear to increase behavior change. In the studies of dental hygiene practices, Janis and Feshbach (1953) reported decreased behavior change under high fear, while Singer (1965) found no main effect of fear. In the study on smoking by Leventhal and Watts (1966) high fear simultaneously increased compliance with a recommendation to cut down on smoking and decreased compliance with a recommendation to take an X-ray.

The present study attempted to account for some of these divergent findings. It is reasonable to expect that the behavior being recommended by a persuasive communication is of critical importance (Leventhal, 1965). Recommendations that are seen as effective in controlling danger may be accepted more readily as fear is increased, while ineffective recommendations may be rejected rationally or may produce reactions of denial (Janis and Feshbach, 1953) or aggression (Janis and Terwilliger, 1962). Additionally, rejection of a recommended behavior may occur if subjects have become afraid of the behavior itself (Leventhal and Watts, 1966).

Recommendations presented as part of fear-arousing communications vary in their effectiveness in controlling danger and in the unpleasantness associated with them. For example, brushing the teeth offers no guarantee of preventing decay, while taking a chest X-ray can lead to the unpleasant discovery of lung cancer. An audience might well reject recommendations which are ineffective in warding off danger or which are difficult, painful, or apt to bring unpleasant consequences. Recommendation factors have been invoked post hoc to explain research findings, but have not been manipulated and studied directly.

In the present study fear was manipulated by presenting differing discussions of the danger of tetanus. Inoculations and booster shots were recommended for protection against tetanus. Under high and low levels of fear, inoculation was portrayed so that it would be seen as more or less *effective* in preventing tetanus and more or less *painful* to take (these manipulations were orthogonal). Subjects' intentions to take shots and their actual shot-taking behavior were used to measure compliance with the recommendations.

It was expected that compliance would be greater when shots were highly effective or not painful. These factors might produce simple main effects or they might interact with level of fear. It seemed more likely that the latter would be the case—that increased fear would make subjects either more or less sensitive to differences in the recommendations.

METHOD

Subjects and Design

Letters were sent to all Yale College seniors asking them to participate in a study to be conducted jointly by the John Slade Ely Center, a local research organization, and the Department of University Health. The study was presented as a survey of student health practices at Yale and an evaluation of some health-education materials. An attempt was then made to contact all seniors by telephone for scheduling.

Of approximately 1000 students who received letters, 274 were scheduled and run in the experiment. Seventy-seven of these were excluded because they had been inoculated since the preceding academic year, and 15 were excluded because of suspicion, involvement in compulsory inoculation programs, allergic reactions to inoculation, or religious convictions against inoculation. The final usable N was 182.

Each subject received a communication which was intended to manipulate perceived effectiveness and painfulness of inoculation. Three levels of fear (including a no-fear control level), two levels of effectiveness, and two levels of pain were combined in a $3 \times 2 \times 2$ factorial design. The n's for the resulting 12 conditions ranged from 11 to 20, with smaller n's in the no-fear control conditions.

Procedure

Experimental sessions were conducted in a classroom with groups ranging in size from 1 to 12. Subjects within each session were randomly assigned to conditions. Control (no-fear) conditions were run separately because of the brevity of the control communications.

Questionnaires containing medical items and personality premeasures were administered at the beginning of the session. Subjects then read a communication on tetanus and gave their reactions to it in a second questionnaire. They were assured that all their responses would be kept confidential.

Communications

Communications were 10-page pamphlets which discussed the danger of tetanus and the effectiveness and painfulness of inoculation.[1] All pamphlets gave specific

[1]Communications have been deposited with the American Documentation Institute. Order Document No. 9011 from ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington, D.C. 20540. Remit in advance $2.00 for microfilm or $3.75 for photocopies and make checks payable to: Chief, Photoduplication Service, Library of Congress.

instructions on how to become inoculated and were similar in style and content to those used by Leventhal et al. (1965) and Leventhal et al. (1966).

Fear. Low-fear material described the very low incidence of tetanus and indicated that bleeding from a wound usually flushes the poison-producing bacilli out of the body. A case history was included which reported recovery from tetanus following mild medication and throat-suction procedures. High-fear material indicated that tetanus can be contracted through seemingly trivial means and that if contracted the chances of death are high. A high-fear case history was included that reported death from tetanus despite heavy medication and surgery to relieve throat congestion. Black-and-white photographs were included in the low-fear material and color photographs in the high-fear material. The discussion of tetanus and case history were omitted entirely from control (no-fear) communications.

Effectiveness. The effectiveness manipulation stressed either the imperfections or the unusual effectiveness of inoculation. Low-effectiveness material stated that inoculation is generally effective and about as adequate as the measures available to deal with other kinds of danger. It pointed out, however, that no protection is perfect and that there is a possibility that even an inoculated person will contract tetanus. High-effectiveness material described inoculation as almost perfect and as far superior to methods available to deal with other kinds of danger. It emphasized that inoculation reduces the chances of contracting tetanus, for all practical purposes, to zero. All communications reported that a new type of inoculation was available at the Department of University Health which would provide protection against tetanus for a period of 10 years.

Pain. To produce fear of the recommended behavior, it was pointed out that inoculation against tetanus has always been painful. Subjects were told that the new inoculation requires a deep intramuscular injection of tetanus toxoid and alum precipitate, making the injection even more painful than before and the local reaction longer lasting. The discussion of pain was presented to subjects as a forewarning so that the discomfort would not take them by surprise. This discussion was omitted from pamphlets in the no-pain conditions.

Specific instructions on how to get a shot and a map showing the location of the Department of University Health were included in all pamphlets. Subjects were encouraged to get a shot or at least to check on whether they needed one.

Measures

Most questions on the pre- and postcommunication questionnaires were answered on 7-point rating scales. The precommunication questionnaire contained

medical questions and four personality measures (susceptibility, coping, anxiety, and self-esteem). The susceptibility measure was made up of three items which asked subjects how susceptible they felt toward common illnesses, toward unusual diseases, and toward illness and disease in general. Three items used to measure coping asked subjects whether they tended to tackle problems actively or to postpone dealing with them. The problems concerned subjects' health habits, their everyday lives as students, and their decisions regarding summer activities. The anxiety scale was made up of 10 true-false items from the Taylor (1953) Manifest Anxiety scale.

The self-esteem measure was similar to that used by Dabbs (1964). Subjects rated themselves on 20 adjectives and descriptive phrases and then rated each of the 20 items as to its desirability. Eight items were classified as desirable and 12 as undesirable on the basis of mean ratings from the entire sample of subjects. Using this group criterion of desirability, each subject's self-esteem score was defined as the sum of his ratings on the desirable items. (It was subsequently discovered that 12 subjects in the present study had participated in the earlier study by Dabbs, and the correlation between their self-esteem scores in the two studies was .62, $p < .01$. This correlation, despite a lapse of 3 years and changes in the measuring instrument, suggests this type of measure is reasonably stable.)

The postcommunication questionnaire included checks on each of the experimental manipulations, a 10-item mood adjective check list, and questions on intentions to take shots, the importance of shots, and the likelihood of contracting tetanus. The subject's evaluation of the pamphlet and the date and place of his last tetanus shot were obtained on this questionnaire.

A measure of behavioral compliance with the recommendations was obtained from shot records of the Department of University Health. Subjects were counted as complying if they took tetanus shots between the experimental sessions and the end of the semester, about one month later. When contacted by letter and phone, no subjects reported receiving shots at places other than the Department of University Health. A few reported that they had tried to take shots and had been told they did not need any. These subjects were counted as having taken shots but their data are presented separately in footnote 2 on the next page.

RESULTS

Main Effects of the Manipulations

Compliance with the recommendations was unaffected by the manipulations of effectiveness and pain. The manipulation of fear, however, influenced both

TABLE 8.1
Compliance with recommendations

	Control (no fear)	Low fear	High fear
Mean intentions to take shots	4.12	4.73	5.17
Proportion of Ss taking shots	.06	.13	.22
N	48	62	72

intentions to take shots ($F_{2,179} = 4.85$, $p < .01$) and actual shot-taking behavior[2] ($F = 3.39$, $p < .05$). The effects were linear (Table 8.1), with compliance being greatest under high fear. The consistency of subjects' responses is indicated by high biserial correlations between intention and behavior measures within high-fear ($r_b = .62$, $p < .01$) and low-fear ($r_b = .68$, $p < .01$) conditions; no correlation was computed for the control conditions since only three control subjects took shots.

Table 8.2 shows that the fear manipulation increased feelings of fear, as it was intended to do. It also increased feelings of interest and nausea, belief in the severity of tetanus and the importance of taking shots, and desire to have additional information. None of these measures were affected by the manipulations of effectiveness or pain.

Both effectiveness and pain were successfully manipulated. Check questions showed that subjects in the high-effectiveness conditions felt inoculation was more effective than did those in the low-effectiveness conditions ($\bar{X}_{\text{high}} = 6.7$, $\bar{X}_{\text{low}} = 5.8$, $F_{1,170} = 105.85$, $p < .01$). Subjects in the pain conditions felt shots would be more painful ($\bar{X}_{\text{pain}} = 4.1$, $\bar{X}_{\text{no pain}} = 2.3$, $F_{1,170} = 74.56$, $p < .01$) and reported more "mixed feelings" about taking shots ($F_{1,170} = 7.20$, $p < .01$) than did subjects in the no-pain conditions. But the clear perception of differences in effectiveness did not affect subjects' intentions to take shots, nor did increasing the anticipated painfulness of shots decrease intentions to take them. In fact, there was a slight tendency for painfulness to strengthen intentions to be inoculated ($F_{1,170} = 2.72$, $p = .10$).

[2]"Shot-taking behavior" combines 20 subjects who took shots and 7 who reported trying to take shots. These two categories are distributed similarly across the fear treatment conditions: 2, 6, and 12 subjects took shots and 1, 2, and 4 subjects tried to do so.

An arc sine transformation of the proportions in Table 8.1 was used (Winer, 1962). This made it possible to test the significance of the differences between groups against the *baseline variance* of the transformation (see Gilson and Abelson, 1965). Baseline variance has a theoretical value which does not depend on computations from observed data. In the present case this value is given by the reciprocal of the harmonic mean number of cases on which each proportion is based, or 1/59 = .0169.

TABLE 8.2
Other reactions to fear manipulation

	Control	Low fear	High fear	F^a
Fear[b]	7.74	9.25	12.19	21.50**
Feelings of nausea	1.27	1.24	1.90	7.95**
Feelings of interest	4.48	4.70	5.39	6.12**
Evaluation of the severity of tetanus	4.64	4.83	5.34	3.86*
Feelings of susceptibility to tetanus	3.46	3.99	4.25	3.31*
Evaluation of the importance of shots	5.85	6.16	6.52	6.44**
Desire for more information about tetanus	3.08	2.68	3.57	3.90*

[a]*F* ratios are computed from three-way analyses of variance. In each analysis *df* = 2/170 (approximately).
[b]"Fear" represents the sum of three items: feelings of fear, fear of contracting tetanus, and fear produced by the pamphlet.
*$p < .05$.
**$p < .01$.

Correlations Among Responses

Only within the high-fear condition did reported fear correlate with intentions to take shots ($r = .23$, $p < .01$). A scatterplot of scores revealed that the high-fear condition increased the range of reported fear and that the positive correlation could be attributed to subjects in the extended portion of the range (those scoring higher than 13 on a 19-point composite scale). These extreme subjects all showed strong intentions to take shots, while other subjects in the high-fear condition sometimes did and sometimes did not intend to take shots. This pattern suggests that fear and acceptance of a recommendation are more closely associated when fear is relatively high, though acceptance may occur at any level of reported fear.

Unlike fear, anger was negatively associated with intent to take shots. The overall within-class correlation between anger and intentions was −.18 ($p < .05$). This correlation remained essentially the same within different levels of the fear treatment, but became increasingly negative as the recommendation was portrayed as less effective and more painful (Table 8.3). Both the row and column differences in

TABLE 8.3
Correlations between anger and intentions to take shots under varying portrayal of recommendations

	Effectiveness	
	Low	High
Pain		
Low	−.29	.15
High	−.52	−.18

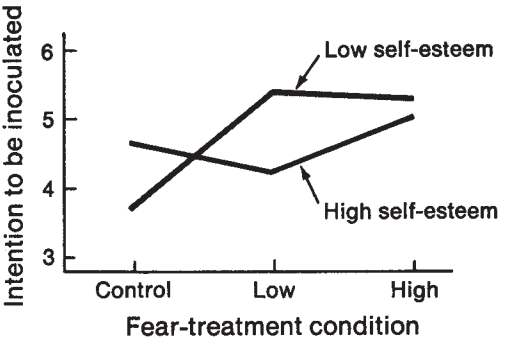

FIGURE 8.1
Self-esteem differences in reactions to the communications on tetanus.

Table 8.3 are significant[3] (for rows, $CR = 2.02$, $p < .05$; for columns, $CR = 2.81$, $p < .01$). It should be emphasized that these are correlational differences only. Low effectiveness and high pain did not increase the mean level of anger or decrease intentions (or decrease actual shot taking). The ranges of anger and intention scores also did not differ among the four conditions.

Personality Differences

None of the premeasures on personality (susceptibility, coping, anxiety, self-esteem) were significantly correlated with intentions to take shots, nor did the correlations vary systematically across the 12 experimental conditions. However, differences were observed when subjects were split at the median into groups high and low on self-esteem subjects. Figure 8.1 shows intentions to take shots among high– and low–self-esteem subjects. The only significant effect within this data (other than the main effect of fear) is the interaction between self-esteem and fear level ($F_{2,166} = 4.74$, $p < .01$). Subjects low in self-esteem increased their intentions to take shots from control to low-fear conditions, then showed no further increase under high fear. Subjects high in self-esteem, on the other hand, showed increased intentions only from low- to high-fear conditions.

DISCUSSION

A positive relationship between fear arousal and persuasion was observed. Increases in the intensity of the fear manipulation were associated with increases in

[3]Significance of these differences was tested after applying Fisher's z' transformation to the correlation coefficients.

attitude and behavior change, with high correlations between intentions to take shots and actual shot taking. These findings are similar to those of Leventhal et al. (1966), who reported a slight tendency for shot taking to increase as fear was raised from low to high levels.

Subjects' beliefs about the effectiveness of inoculation did not affect their compliance; they responded equally well to recommendations portrayed as low and high in effectiveness. This may be because even the low-effective recommendation was rated relatively high in effectiveness (5.8 on a 7-point scale). However, the manipulation was sufficient for subjects to perceive significant differences between low and high effectiveness, and the failure of this variable to influence compliance suggests caution in using it to reconcile divergent results of studies on fear arousal and persuasion (Janis and Leventhal, 1967; Leventhal, 1965; Leventhal and Singer, 1966).

The description of pain produced mixed feelings about shots, but did not prevent subjects from taking them. Perhaps this is because the discomfort of inoculation is negligible in comparison with the pain of tetanus itself. A stronger manipulation of anticipated "painfulness" was unintentionally introduced in the study by Leventhal and Watts (1966), who created fear of smoking by showing a film in which a chest X-ray led to the discovery of cancer and to surgical removal of a lung. The authors suggested that decreased X-ray taking in this condition was more likely caused by fear of the consequences of an X-ray than by defensive reactions to the fear-arousing material on cancer. The present findings do not invalidate their conclusion, but they limit the range of situations to which such an explanation might apply. Subjects apparently do not respond to small variations in the effectiveness or unpleasantness of a recommended course of action. Unless a compelling deterrent exists, people who anticipate danger prefer to do something rather than nothing.

This last statement is qualified by differences in the behavior of high– and low–self-esteem subjects. Low–self-esteem subjects showed high compliance with the recommendations in both high- and low-fear conditions, while high–self-esteem subjects showed high compliance only in the high-fear condition. In addition, personality measures of self-esteem and coping were significantly correlated ($r = .49$, $p < .01$) in the present study, as they were in the study reported by Dabbs (1964). One might conclude that high–self-esteem subjects are more active and aggressive in dealing with their environment and have developed more skill in meeting dangers with appropriate protective actions. Thus, they may recognize inoculation to be more appropriate when the danger of tetanus is greater, while low–self-esteem subjects may accept the position of the communication that inoculation is appropriate regardless of the magnitude of danger. An alternative possibility is that some differences characteristically associated with self-esteem simply disappear when there is an urgent need to combat danger (as there was with the high-fear communication).

In the conditions where inoculation was depicted as ineffective or painful, anger was negatively associated with intentions to be inoculated. It is possible that

increased anger in these conditions would have lowered compliance. But since anger and compliance did not covary as the fear treatment was increased, there appears to be no causal relationship between them. It seems more likely that anger does not decrease compliance, but that under certain conditions it provides the justification for noncompliance. Under other conditions—as when recommended behaviors are highly effective, not painful, and not a reasonable target for anger—noncompliance may have to be justified in some other manner.

All the present findings could have been influenced by several factors that were not varied. For example, all subjects received the manipulations of perceived danger, effectiveness, and pain in the same order. Learning about the dangers of tetanus first may have caused some subjects to ignore differences in the effectiveness and painfulness of inoculation. In addition, all subjects were asked after reading the communications whether they intended to be inoculated. One might expect that stating an intention to be inoculated would commit the subject to following this course of behavior later on. However, the findings of Leventhal et al. (1965) and Leventhal et al. (1966) indicate that specific instructions must be present before intentions will be translated into behavior. Since all subjects in the present study did receive specific instructions, the high correlations between intentions and behavior may be due to this factor.

Finally, while fear and persuasions were associated, the evidence of any causal relationship between them is tenuous. As in most studies of fear arousal and attitude change, the communications used were complex ones. They discussed the damage tetanus can cause and the likelihood of contracting it. They differed in fearfulness, interest, and feelings of nausea evoked. They also differed in length, type of language used, and amount of information about tetanus. With such confounding it is impossible to attribute increases in attitude change solely to increases in fear. This is an inherent difficulty when fear is manipulated by the use of differing descriptions of danger. Another approach should be developed by anyone wishing to manipulate fear independently of other aspects of a persuasive communication.

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9

Attribution Versus Persuasion as a Means for Modifying Behavior

Richard L. Miller, Philip Brickman, and Diana Bolen

The present research compared the relative effectiveness of an attribution strategy with a persuasion strategy in changing behavior. Study 1 attempted to teach fifth graders not to litter and to clean up after others. An attribution group was repeatedly told that they were neat and tidy people, a persuasion group was repeatedly told that they should be neat and tidy, and a control group received no treatment. Attribution proved considerably more effective in modifying behavior. Study 2 tried to discover whether similar effects would hold for a more central aspect of school performance, math achievement and self-esteem, and whether an attribution of ability would be as effective as an attribution of motivation. Repeatedly attributing to second graders either the ability or the motivation to do well in math proved more effective than comparable persuasion or no-treatment control groups, although a group receiving straight reinforcement for math problem-solving behavior also did well. It is suggested that persuasion often suffers because it involves a negative attribution (a person should be what he is not), while attribution generally gains because it disguises persuasive intent.

Despite the volume of research on attitude change and persuasion, there is surprisingly little evidence that persuasion can be effective, particularly if a criterion of persistence of change over time is applied (Festinger, 1964; Greenwald, 1965b;

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This work would have been impossible without the enthusiasm, cooperation, and enlightened support of Irene Timko, principal of Anderson Elementary School. Great thanks are also due to the teachers who participated in this project. In Study 1, thanks go to Catherine Lebenyi, Wendy Weiss, and Ira Mizell for willingly helping to initiate a new behavioral modification technique. In Study 2, where an incredible number of experimental manipulations were administered by the teachers, special thanks go to Margaret Paffrath, Ronnie Briskman, June Butalla, and Susan Levie. Thanks are also due to the eighth-grade assistants who administered our tests.

Rokeach, 1968; Zimbardo and Ebbesen, 1969; Cook, 1969). The failure of persuasive efforts to produce lasting change may be taken as evidence that subjects have not integrated the new information into their own belief systems (Kelman, 1958) or taken it as the basis for making an attribution about themselves (Kelley, 1967). We might expect that a persuasive communication specifically designed to manipulate the attributions a person made about himself would be more effective in producing and maintaining change. This research was designed to test the relative importance of attribution manipulations to persuasive attempts by comparing a normal persuasion treatment and an attribution treatment.

The persuasion conditions of the present research were designed to be maximally effective through their use of a variety of techniques that have been found to be helpful, at least on occasion, in past research. Past research has shown that an optimal persuasive manipulation should involve a high-credibility source (Hovland and Weiss, 1951) delivering a repeated message (Staats and Staats, 1958) with an explicitly stated conclusion (Hovland and Mandell, 1952) that is supported by arguments pointing out the benefits of change (Greenwald, 1965a) and overlearned by the audience (Cook and Wadsworth, 1972). Face-to-face communication by the source (Jecker, Maccoby, Breitrose, and Rose, 1964), reinstatement of the source at the time of attitude assessment (Kelman and Hovland, 1953), and active role playing or participation by the audience in the message (Janis and King, 1954) are also helpful.

The attribution techniques were also designed to be maximally effective through their use of all three factors specified by Kelley (1967) as conducive to making a stable attribution: consistency of the evidence over time, consistency of the evidence over modalities, and consistency or consensus across sources.

STUDY 1: LITTERING BEHAVIOR

Study 1 attempted to modify children's littering behavior. Behavior was monitored before and after treatment and again after a 2-week period of nontreatment. It was hypothesized that both the attribution and the persuasion conditions would result in initial posttreatment behavioral change but that the attribution condition would show greater persistence as a result of altering the basic self-concept of the subjects in a direction inconsistent with littering.

METHOD

Participants

The research took place in three fifth-grade classrooms in an inner-city Chicago public school. Two fifth-grade classrooms were randomly assigned to the experimental conditions, while a third was designated a control group. Three female

experimenters, all undergraduate psychology majors at Northwestern University, were randomly assigned to a different classroom for each test.

Experimental Manipulations

There were a total of 8 days of attribution and persuasion treatments dealing with littering, with discussion intended to average about 45 minutes per day.

Attribution condition. On Day 1, the teacher commended the class for being ecology-minded and not throwing candy wrappers on the auditorium floor during that day's school assembly. Also on Day 1, the teacher passed on a comment ostensibly made by the janitor that their class was one of the cleanest in the building. On Day 2, after a visiting class had left the classroom, the teacher commented that paper had been left on the floor but pointed out that "our class is clean and would not do that." The students at this point disagreed pointedly and remarked that they would and did indeed litter. On Day 3, one student picked up some paper discarded on the floor by another and after disposing it in the wastebasket was commended by the teacher for her ecology consciousness. On Day 4, Row 1 was pointed out as being the exceptionally neat row in the room by the teacher. Also on Day 4, the principal visited the class and commented briefly on how orderly it appeared. After the principal left the room, the students castigated the teacher for her desk being the only messy one in the room. On Day 5, a large poster of a Peanuts character saying "We are Andersen's Litter-Conscious Class" was pinned to the class bulletin board. Also on Day 5, the teacher gave a lesson on ecology and talked about what we "the class" are doing to help. On Day 6, the principal sent the following letter to the class: "As I talked to your teacher, I could not help but notice how very clean and orderly your room appeared. A young lady near the teacher's desk was seen picking up around her desk. It is quite evident that each of you are very careful in your section." On Day 7, the teacher talked about why "our class" was so much neater. In the interchange the students made a number of positive self-attributions concerning littering. On Day 8, the janitors washed the floor and ostensibly left a note on the blackboard saying that it was easy to clean.

Persuasion condition. On Day 1 during a field trip, the children were told about ecology, the dangers of pollution, and the contribution of littering to pollution. They were then asked to role play being a trash collector and to pick up litter as they came across it. On Day 2, inside the school lunchroom the teacher talked about garbage left by students and gave reasons why it should be thrown away: it looked terrible, drew flies, and was a danger to health. On Day 3, the teacher gave a lecture on ecology, pollution, and litter and discussed with the class how the situation could be improved. Also on Day 3, the teacher passed on a comment ostensibly from the school janitor that they needed help from the students in keeping the

floors clean, implying here as elsewhere that nonlittering would lead to approval and commendation by various adult authorities. On Day 4, the teacher told the students that everyone should be neat, mentioning aesthetics among other reasons for neatness. Also on Day 4, the principal visited the class and commented briefly about the need for clean and tidy classrooms. On Day 5, the teacher told the students that they should not throw candy wrappers on the floor or the playground but should dispose of them in trash cans. Also on Day 5, a large poster of a Peanuts character saying "Don't be a litterbug" with "Be neat" and "Don't Litter" bordering it was pinned to the class bulletin board. On Day 6, the principal sent the following letter to the class: "As I talked to your teacher, I could not help but notice that your room was in need of some cleaning. It is very important that we be neat and orderly in the upkeep of our school and classrooms. I hope each of you in your section will be very careful about litter." On Day 7, the teacher appointed several children in each row to watch and see if people were neat outside the building as well as in the classroom. On Day 8, a note was left on the board ostensibly from the janitors to remind the children to pick up papers off the floor.

Measurement of Littering

Pretest. To discover any existing differences among the three classrooms with respect to their tendency to litter, a specially marked reading assignment which had previously been turned in to the teachers was returned to the students 5 minutes before the end of the school day. The students were then instructed to throw the assignment away after the bell rang for dismissal. After school the experimenters counted the number of assignments thrown in the wastebasket versus left on the floor or on the shelf under the students' seats. Less than 20% of the students in each class disposed of their assignments in the wastebaskets. The precise percentages were 20% for the control group ($n = 31$), 16% for the persuasion group ($n = 26$), and 15% for the attribution group ($n = 27$).

Posttest. A two-part behavioral test was designed to tap the two aspects of the ecology-littering problem, nonlittering and cleaning up the litter of others.

On the morning of the tenth day, minutes before the first recess, each teacher introduced the experimenter for her classroom as a marketing representative of a local candy manufacturing firm and left the experimenter in charge of the room. The experimenter explained that she was testing the tastiness of a new brand of candy and passed out one piece of candy to each student. The candy was wrapped in colored cellophane with a different color used for each classroom. Following the taste test, the class was dismissed for recess. During recess the experimenters counted the number of candy wrappers in the wastebaskets, on the floor, and in the desk seats. The experimenters then relittered the classroom entrance area with seven specially marked candy wrappers. After recess the experimenters checked

the hallway and playground for discarded candy wrappers. During the lunch break, which came 1 hour after recess, the experimenters reentered the classroom and determined the disposition of the specially marked candy wrappers.

The second posttest followed the first by a period of 2 weeks. During this time no mention of ecology or littering was permitted in any of the classrooms. The second test was very similar to the first except that this time, the experimenters did not interact with the students. Ten minutes before the afternoon recess the teacher passed out toy puzzles as Christmas presents from the Parent Teachers Association. The students were asked to try to work the puzzles before recess. Each puzzle was wrapped in a color-coded container with a different color assigned to each class. During recess the experimenters entered the classroom and determined the disposition of the containers left there. They then relittered the entrance way. After recess the experimenters searched the hallway and playground for other containers. After school the experimenters reentered the classroom and determined the disposition of the relittered containers.

RESULTS AND DISCUSSION

Littering Behavior

Figure 9.1 charts the percentage of items in each group which were discarded in the wastebasket on each test. A chi-square test was used to compare frequency of littering in the three groups on the immediate and delayed posttests. Although the measures directly reflect items of litter rather than individual subjects, it was observed by the experimenters that subjects independently discarded their own candy wrappers in the wastebaskets. The three groups were significantly different at both the immediate posttest, χ^2 (2) = 18.14, $p < .001$, and at the delayed posttest, χ^2 (2) = 20.99, $p < .001$. The attribution group was significantly superior to the persuasion group on both the immediate posttest, χ^2 (1) = 7.19, $p < .01$, and the delayed posttest, χ^2 (1) = 16.15, $p < .01$. Although the persuasion group appeared to show an immediate increase in litter-conscious behavior, it was not significantly different from the control group even on the immediate posttest, χ^2 (1) = 2.57, *ns.*

Cleanup Behavior

All seven items were picked up by members of the attribution group on both the immediate and the delayed posttest. Persuasion group members picked up four items on the immediate posttest and two on the delayed posttest, while control

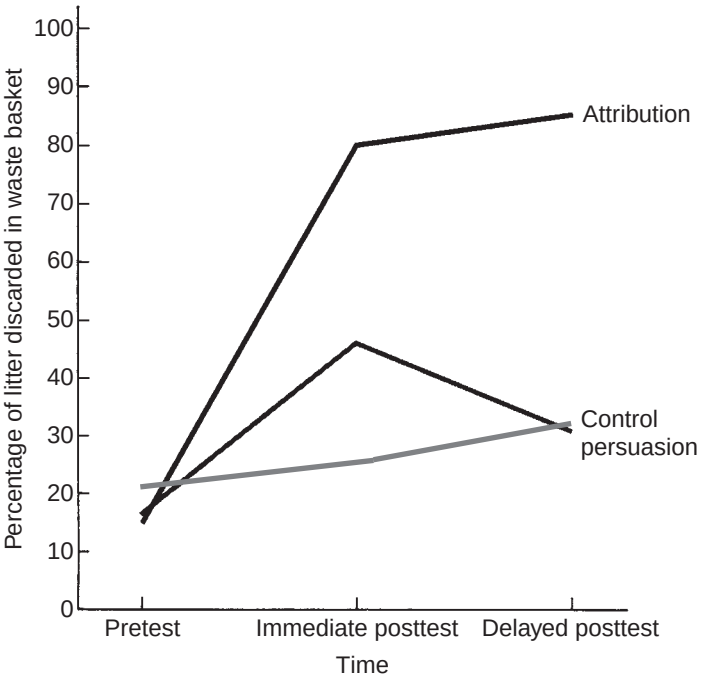

FIGURE 9.1
Nonlittering behavior of the attribution, persuasion, and control groups over time (Study 1).

group members picked up two items on the immediate posttest and three on the delayed test. Since the total number of wrappers left was only seven and more than one wrapper may sometimes have been picked up by a single individual, a chi-square test is not fully appropriate. Nonetheless, such a test would be significant at the .05 level at each posttest, which supports the appearance of differences favoring the attribution group.

After this study the attribution teacher was advised that the nonlittering behavior could perhaps be maintained if the students were occasionally reminded of the attribution "You are neat." Three months later the teacher reported that her class was still significantly neater than it had been prior to treatment.

The results for both the littering test and the cleanup test support the hypothesis that attribution is a more effective technique than persuasion for inducing stable behavioral change. We would like to show, however, that this effect holds for other kinds of behavior. Furthermore, it would be desirable to overcome a weakness in Study 1 that arose from the fact that treatments were nested within classrooms. It is possible, if relatively unlikely, that the differences emerging over time were due to teacher differences rather than treatment differences. Study 2 avoids this by including all treatment and control conditions in each classroom.

STUDY 2: MATH ACHIEVEMENT

The results of Study 1 are certainly encouraging for an attributional approach to modifying behavior. However, although ecology consciousness and nonlittering are of some social importance, they are not the primary focus of the schools, which is, at least theoretically, to teach skills. Will attribution and persuasion techniques show the same pattern of effectiveness in generating a skilled behavior, like math achievement, as they have in generating a socially desirable but unskilled behavior, like disposing of trash in wastebaskets? Furthermore, littering behavior would certainly be only a weakly valenced aspect of self, while most of the skills taught in schools would be highly valenced and of considerable import for a student's self-concept. Will attribution be an aspect? The first purpose of Study 2 was to answer just these questions. It might also be noted that attributions in Study 2, in addition to being more central to self-concept, are specifically directed to particular individuals rather than addressed to a group as a whole.

The second purpose of Study 2 was to test the relative effectiveness of attributions of motivation versus attributions of ability in changing behavior. Both perceptions of ability and motivation are essential to the belief that a person will attain a given goal (Heider, 1958). Study 1, however, would seem to have involved primarily the attribution of motivation, since the children presumably began with a common belief that they had the ability to be neat. In the case of a skilled behavior like arithmetic, however, it would seem more likely that motivation and belief in one's motivation to do well are more common than ability and belief in one's ability to do well (Katz, 1964), so that attributions of ability would be of greater value than attributions of motivation. Nonetheless, enhancing people's perceptions of their motivation for a task may also benefit their performance. Study 2 attempts to separate ability and motivation as the bases of attribution and the targets of persuasive appeal.

So far we have only considered cognitive strategies for modifying attitudes and behavior. Staats (1965) has shown that even young children will engage in complex learning tasks if they are simply given appropriate reinforcement. According to Bandura (1969), a successful reinforcement strategy for behavior modification requires a valued reinforcer which is contingent upon the desired behavior and a reliable procedure for eliciting the desired behavior. In the present study both verbal praise and extrinsic rewards were used as reinforcers for efforts at mathematical achievement, and a number of overlapping procedures were used to elicit these efforts.

To compare the relative efficacy of the attribution and reinforcement techniques with standard attitude change approaches, a persuasive manipulation was devised similar to the one used in Study 1. The only changes were that audience participation and role playing were deleted, since neither was appropriate to the treatment conditions, while public labeling was added. It appears from the study of deviance (Becker, 1963) that public labeling of a person can lead that person to redefine

himself along the lines of the label. While it was felt in Study 1 that children had to be convinced of the benefits of nonlittering in the persuasion condition, the advantages of math achievement as a means for obtaining rewards in school seemed too obvious to need pointing out.

The present study attempted to modify children's math-related self-esteem and their math scores on skill tests. The six conditions were attribution ability, attribution motivation, persuasion ability, persuasion motivation, reinforcement control, and a no-message control. It was hypothesized that all three basic techniques (attribution, persuasion, and reinforcement) should have an initial positive effect on the self-esteem and math behavior of the subjects but that attribution should have the most enduring effect over time.

METHOD

Participants

The research took place in four second-grade classrooms of the same inner-city Chicago public school involved in Study 1. Second-grade students were picked, since it was felt on the basis of Rosenthal and Jacobson (1968) that their school-related self-concepts would be more malleable than at a later age. In all, 96 students took part in the study. All five experimental conditions and one control condition were present in each of the four classrooms. From each class list of approximately 30 students, 24 were randomly assigned to the six possible conditions. Thus 4 students in each classroom appeared in each condition.

Overview of Procedure

All subjects first received a mathematics pretest and a self-esteem pretest. Subsequent treatments consisted of 8 days of attribution, persuasion, or reinforcement. Immediately following the treatment, math and self-esteem posttests were administered. Delayed posttests were given after a 2-week period of no treatment. The control group received the pretests and the immediate and delayed posttests but no treatment. Student absences for both treatments and tests were made up on the day the student returned to school.

Experimental Manipulations

Five treatment techniques were used with all groups: verbal comments, written comments, letters from the teacher, letters from the principal, and medals. The above order is followed in discussing these techniques for each experimental

condition. It should also be noted that in the attribution and persuasion conditions, students were initially called to the principal's office in groups of eight, where they received a treatment-related message. The principal discontinued these treatments after the third day of the experiment, however, on the grounds that they were too time-consuming for her and that she found the false attribution treatments too difficult to deliver.

All treatment techniques were prepackaged. Before the experiment began, teachers' treatment packages were prepared that listed the treatment techniques and their recipients for each day. The order in which each subject received the treatments was randomized for classroom "A" and repeated in each of the other classrooms.

Attribution ability. The general focus of this treatment was attributing to the students skill and knowledge in mathematics. Three different verbal comments were made by the teacher to each student on different days: "You are doing very well in arithmetic," "You are a very good arithmetic student," and "You seem to know your arithmetic assignments well." Three different written notes were tied to assignments on different days and handed back to the students: "You're doing very well," "excellent work," and "very good work." The letters from the teachers and principal underscored the students' excellent work in math and were sent home on days when a verbal or written note was not scheduled. The letter from the teacher included the phrases "very good student," "does all his assignments well," and "excellent arithmetic ability." The letter from the principal used the phrases "excellent ability," "knows his assignments," and "very good student." The medals awarded to the attribution ability students featured the words "good student—math."

Attribution motivation. The general focus of this treatment was attributing hard work and consistent trying to the student. Three appropriate verbal comments were made by the teacher to each child privately, and three written notes were appended to a test or assignment. The verbal comments were the following: "You really work hard in arithmetic," "You're working harder in arithmetic," and "You're trying more in arithmetic." The written comments were as follows: "You're working harder, good!" "You're trying more, keep at it!" and "Keep trying harder!" The letters from the teacher and principal underscored the child's application in math. The teacher's letter used the phrases "working hard," "trying," and "applying himself." The principal's letter used the phrases "working harder," "applying himself," and "trying harder." The medals awarded to the attribution motivation students featured the words "hard worker—math."

Persuasion ability. The general focus of this treatment was to persuade the student that he should be good in arithmetic and doing well in that subject. Three

verbal comments and three written comments summarizing that message were made in the same manner as that for the attribution messages. The three verbal comments were "You should be good at arithmetic," "You should be a good arithmetic student," and "You should be doing well in arithmetic." The three written comments were "should be better," "should be good at arithmetic," and "should be getting better grades." The letters from the teacher and principal used ostensible "aptitude test scores" to inform parents that their child should be making good grades in math. The teacher's letter included the injunctions "should be doing well," "should be getting high grades," and "should be becoming a good arithmetic student." The principal's letter used the phrases "should get good grades," "should do very well," and "should be a good arithmetic student." The medals awarded to these students contained the words "do better—math."

Persuasion motivation. The general focus of this treatment was to persuade the student that he should be working harder and spending more time on math. The three verbal and the three written comments asked the child to try more at math and were made in the same way as they were in the other treatment conditions. The three verbal comments were "You should spend more time on arithmetic," "You should work harder at arithmetic," and "You should try more on arithmetic." The three written notes were "Try harder," "Work more on arithmetic," and "Work harder." The letters from the teachers and the principal informed parents that their child should spend more time on math and to pass that idea along to him. The teachers' letter included the injunctions "should be trying harder," "should spend more time on arithmetic," and "should be applying himself." The principal's letter used the phrases "spend more time" and "try harder." The medals awarded the persuasion motivation students contained the words "work harder—math."

Reinforcement. The reinforcement condition also followed the same format as the attribution and persuasion conditions except that it added two additional methods and deleted the principal's comments. Three verbal comments indicating pride in the student's good work were made by the teacher. They were as follows: "I'm proud of your work," "I'm pleased with your progress in arithmetic," and "very good." Three written comments of simple praise were appended to the student's math work. They were "excellent" "very good," and "I'm very happy with your work." The letters from the teachers and principal indicated pride and satisfaction in the child's work to the parent. The teacher's letter used the phrases "proud of his work" " good grades," and "happy with his work." The principal's letter used the phrases "excellent progress," "doing very well," and "proud of him." The medals awarded those students contained the words "math award." On Days 2, 5, and 7, students were praised verbally if they chose to work on an extra math problem rather than a reading exercise. On two other days the students received silver stars by solving a problem from a math assignment.

Control. The control condition received no treatment whatsoever but took part in all tests of mathematical ability and self-esteem.

MEASUREMENTS

Self-Esteem

The self-esteem pretest was an adaptation of the questionnaire originally developed by Rogers and Dymond (1954) that includes items that measure self-esteem with regard to peers and parents, school interests, and personal interests. Four new items were added which dealt specifically with math-related self-esteem. All items were declarative sentences generally of the "I am ——" form, from which the children were asked to say whether it was "like me" or "unlike me." The self-esteem pretest and posttest were both administered individually to each student by one of a dozen eighth-grade assistants, who asked each question privately to the second graders and personally recorded the subject's answer. The self-esteem posttest was given in the morning on the day after treatment ended.

Mathematics

The pretest and all subsequent math tests were 20-item tests consisting of 25% review questions, 50% current material, and 25% new material. The math pretest was administered by the teachers as a regular arithmetic quiz. The immediate posttest was administered in the afternoon on the day after treatment ceased. The final posttest was administered after a 2-week period in which no treatment took place.

RESULTS

Self-Esteem

The mean math self-esteem scores for all six conditions on the pretest and the posttest are presented in Figure 9.2. Analysis of variance of the pretest scores indicated that there were no significant differences among the five treatment groups and the control group in math self-esteem before treatment began, F (5, 90) = .79, *ns.*

The major analysis of the math self-esteem scores was a repeated measures analysis of variance using the six experimental conditions and the two times of measurement as factors. Results indicated both a main effect of time, F (1, 90) = 10. 84, $p < .01$, and a significant Time $\times$ Treatment interaction, F (5, 90) = 2.41,

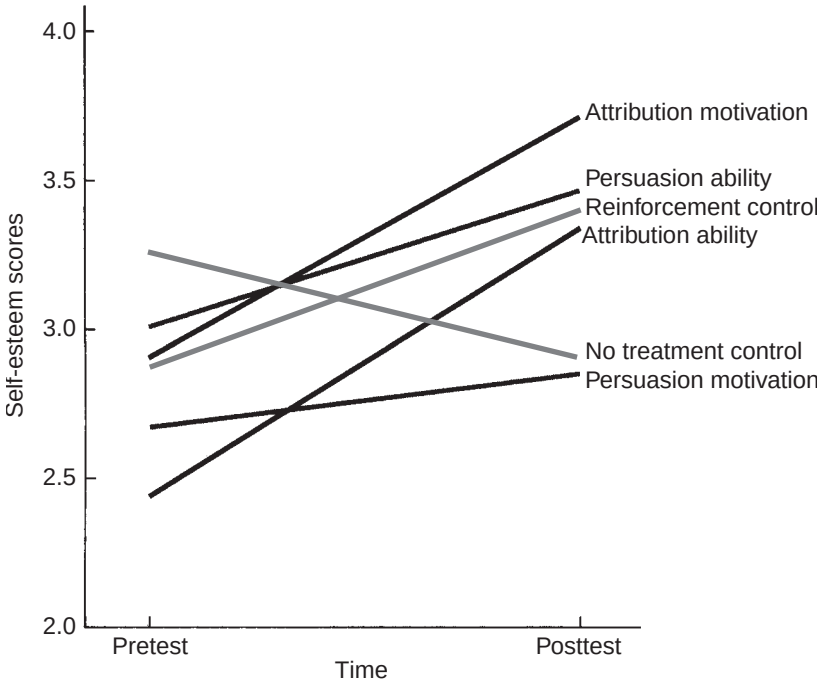

FIGURE 9.2
Self-esteem scores for the attribution, persuasion, and control groups over time (Study 2).

$p < .05$. As can be seen from Figure 9.2, all treatment groups show an increase in self-esteem, while the control group shows a decrease, which is probably due to the lack of any treatment for control students in the face of a number of public treatments visible to them in their classroom.

A subsidiary analysis of variance was performed using only the attribution and the persuasion groups, with mode of treatment (attribution vs. persuasion), basis of treatment (ability manipulation vs. motivation manipulation), and time as the three independent variables. Results again indicated a main effect of time, F (1, 60) = 19.02, $p < .001$, and a significant Time $\times$ Mode of Treatment interaction, F (1, 60) = 4.76, $p < .05$. The math self-esteem of the attribution groups increased more sharply from pretest (2.66) to posttest (3.50) then did the math self-esteem of the persuasion groups (2.84 at pretest, 3.13 at posttest). Only the attribution ability and attribution motivation groups were significantly different from the no-treatment control group in their change from pretest to posttest, F (1, 30) = 7.58, $p < .01$ for attribution ability, F (1, 30) = 7.66, $p < .01$ for attribution motivation. This difference approached but fell short of significance for the persuasion ability group, F (1, 30) = 3.12, $p < .11$. Similar analyses showed no significant effects on general school-related and non-school-related self-esteem.

Mathematics

The mean total math scores for all six conditions on the pretest, the immediate posttest, and the delayed posttest are presented in Figure 9.3. Analysis of variance of the pretest scores indicated that there were no significant differences among the five treatment groups and the control group in their total math scores before treatment began, F (5, 90) = .54, *ns.*

The major analysis of the math test scores was a repeated measures analysis of variance using the six experimental conditions and the three times of measurement as factors. The results show both a main effect of time, F (2, 180) = 36.69, $p < .001$, and a significant Time $\times$ Treatment interaction, F (10, 180) = 5.57, $p < .001$. From Figure 9.3 it can be seen that both attribution conditions show marked increases on the immediate posttest followed by a slight tendency to enlarge that increase after the 2-week delay. Both persuasion conditions appear to show an increase on the immediate posttest but fail to maintain that increase over the 2-week delay. The reinforcement condition shows a pattern similar to that of the attribution conditions but with a lesser degree of improvement.

To assess the extent to which attribution and persuasion differed in their initial effectiveness versus the extent to which they differed in their ability to maintain

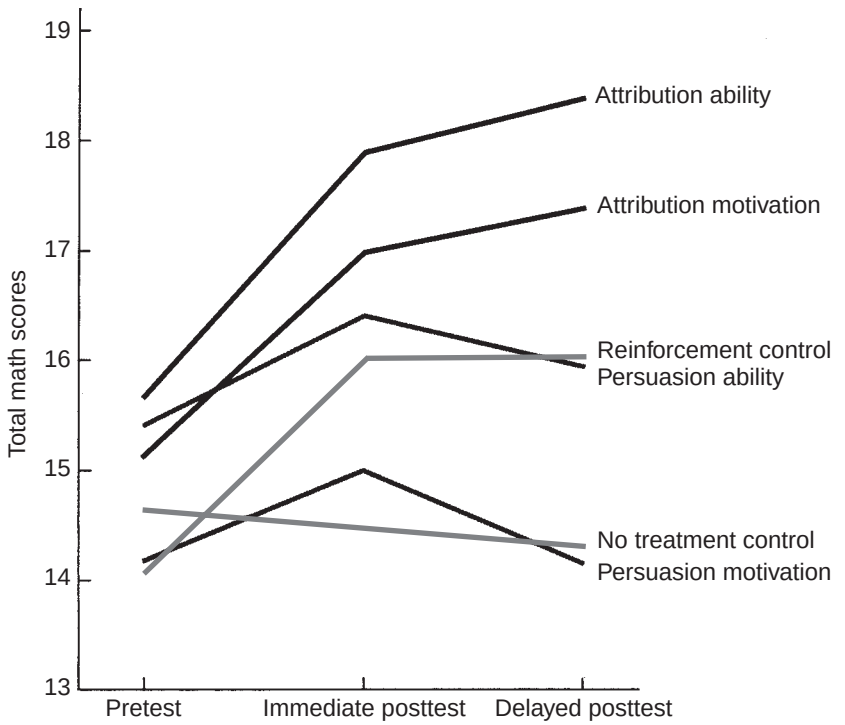

FIGURE 9.3
Total math scores of the attribution, persuasion, and control groups over time (Study 2).

their effectiveness over time, separate analyses were made of changes from the pretest to the immediate posttest and from the immediate posttest to the delayed posttest. Mode of treatment (attribution vs. persuasion) and basis of treatment (ability vs. motivation) were used as factors along with time of test. For change from pretest to the immediate posttest, a significant Mode $\times$ Time interaction, F (1, 60) = 11.97, $p < .001$, indicated that attribution was significantly more effective than persuasion in inducing change. Only the attribution ability and attribution motivation groups were significantly different from the no-treatment control group in their change from pretest to immediate posttest, F (1, 30) = 14.75, $p < .01$ for attribution ability; F (1, 30) = 11.42, $p < .01$ for attribution motivation. This difference approached but fell short of significance for the persuasion ability group, F (1, 30) = 2.92, $p < .10$.

For change from the immediate posttest to the delayed posttest, another significant Mode $\times$ Time interaction, F (1, 60) = 13.67, $p < .001$, indicated that the attribution treatments were also superior to the persuasion treatment in maintaining what change they produced. The attribution treatments show an increase of .50 from the immediate to the delayed posttest, while the persuasion treatments show a decrease of .63.

Finally, to make a preliminary test as to whether attribution and persuasion were similar in their effects on high- and low-ability students, subjects in each condition were divided at the cell median according to their math pretest performance. An analysis of variance on posttest math scores was conducted using pretest ability (high vs. low), treatment (attribution vs. persuasion, ignoring basis of treatment), and posttest time (immediate vs. delayed) as factors. Initially more able students continued to outperform initially less able students, F (1, 60) = 46.01, $p < .001$, on the posttests. Attribution was more effective than persuasion for both groups, F (1, 60) = 19.39, $p < .001$, but the attribution-persuasion difference was significantly larger for the low-ability students than for the high-ability students, interaction F (1, 60) = 5.66, $p < .05$. Average posttest scores for the high-ability students were 18.8 for attribution and 17.8 for persuasion, while for low-ability students they were 16.5 for attribution and 13.0 for persuasion.

DISCUSSION

In both studies the attribution treatments caused a significant change which persisted over time. These treatments were strong enough to overcome counterarguing by subjects in Study 1 and a history of at best modest success among many subjects in Study 2. Both attributions based upon subjects' ability to do something and those based upon subjects' motivation to do it appeared effective. The effects of persuasion were, in general, insignificant and dissipated over time.

The fact that the superiority of attribution treatments over persuasion treatments was demonstrated in two different field experiments using different behaviors

(nonlittering and math problem solving), two different subject populations (fifth graders and second graders), and two different designs (a between-classroom design and a within-classroom design) gives us some confidence that these effects are real and generalizable. Neither study is without weakness. The nesting of treatments within classrooms in Study 1 leaves teacher or group differences as a possible, if unlikely, alternative explanation for the treatment effects that emerged over time. The public nature of the treatments in each classroom in Study 2 means that treatment effects may have been aided by implicit comparisons students were making between their own condition and other treatments, a process which must lie behind the unexpected drop in math self-esteem shown by the no-treatment control group. (This last result must also be counted among the ethical costs of a within-classroom design.) The weaknesses of the two studies, however, are quite different, while their effects are quite similar, which suggests that the results are not due to idiosyncrasies of design.

The present research provides a general framework into which previous work concerning the effects of teacher expectancies on pupil performance (Beez, 1968; Rosenthal and Jacobson, 1968; Seaver, 1973) can be fit. The means by which teacher expectancies are communicated is at best a dependent variable in previous studies (Meichenbaum, Bowers, and Ross, 1969) and often mysteriously unobserved. The present study made the communication of expectancies, in the form of attributions, its central manipulation. The fact that this programmed communication of expectancies worked as it did provides some support for the essential validity of the often elusive teacher expectancy effect.

Effects of Attribution

That attributions based upon ability and attributions based upon motivation did not differ in their effectiveness implies that direct linkage of skill-specific attributions to the self-system is more important than the basis on which this linkage is made. The message "You are a particular kind of person" is more important than the specification of "why." It should be noted, however, that the present research contrasted only two kinds of internal attributions, ability and motivation. Attributions made to a person on the basis of external factors ("You are neat because I am watching you") would presumably be less effective in producing lasting change.

Attribution can, of course, involve elements of persuasion. As we have seen, the statement "You are a neat person" may be a most effective means of persuading someone to be neat. Nonetheless, such attributional statements need not involve persuasive intent but may instead be simple statements of fact. Indeed, their guise as truth statements may be thought of as their most effective advantage. Not only does this enable them to work directly on a person's self-concept, as noted, but it

may also enable them to slip by the defenses a person ordinarily employs against persuasive attempts. It may be effective because it is less easily recognized as persuasion, and hence less likely to arouse resistance, counterarguing, or reactance.

In Study 1 the attribution treatment did elicit counterarguing by the students, which suggests a possible reactance (Brehm, 1972) or boomerang effect (McGuire, 1969), but there was no evidence of such an effect by the end of the experiment. It is possible that reactance, like other attitude-change forces, dissipates over time as the issue is worked through and the treatment is maintained. It is also possible that the elementary school students who were subjects in the present studies are less likely to perceive the manipulative intent of an attribution treatment and less likely to show reactance than a comparable adult group.

Attribution and Persuasion

In accounting for the relative ineffectiveness of persuasion, we may note first of all that persuasive communications urging a person to do something do not necessarily tap the internal self-concept of their target. Worse yet, to the extent that they do implicate self-concept, they may involve the negative attribution that the person is not the kind of individual who engages in the recommended behavior. An appeal to be neat or an appeal to work hard can involve the implicit attribution that the person is not currently the sort of individual who is neat or works hard. If convincing people that they are neat or hard working is the key to making them neat or hard working, a naive persuasive attempt can cancel out its own message. At best it attributes to its target the potential for becoming the sort of person recommended, but this is clearly much weaker than the attribution that the person already embodies the desired behavior.

In this study, moreover, the persuasive messages implied something negative about the subjects, while the attribution treatments implied something positive. Although blame as well as praise has been shown to be effective in eliciting improved performance (see Kennedy and Willcutt, 1964, for a review of the mixed results in this area), one of the best designed studies (Hurlock, 1925) found that the improvement elicited by blame dissipated over time, while that elicited by praise persisted. It may be speculated that persuasion and punishment both remain effective in motivating behavior only so long as the actors feel that they can accomplish what is being called for. As they accept the implicit negative attributions of a persuasive message, the effectiveness of the message diminishes. This may explain why persuasion was relatively more effective for high-ability students than for low-ability students. The high-ability students may have been better able to respond to the appeal to do better without becoming discouraged by the implicit attribution (which they could to some extent discount) that they were not currently demonstrating appropriate accomplishment.

The implicit attributions of the persuasion treatments in this study were negative because the behaviors they were calling for (which the subjects were presumably not emitting) were positive. Likewise, the attribution treatments were positive because the behaviors they attributed to the subjects were positive. For practical and ethical reasons, all of the treatments in the present study were aimed at producing positive or socially desirable behaviors. It is our idea that the implicit or explicit labeling itself, and not merely the rewardingness or punishingness of the labels, made the present attribution treatments effective and the persuasion treatments ineffective. This can only be known for sure, however, by a study that aims at producing undesirable behavior, in which case attributions of the behavior would have negative implications, which persuasion to the behavior would have implicit positive implications for the present self-concept. If people were responding to the attributions in the present experiment only because they were rewarding, we would expect that attributions of an undesirable behavior would have reverse effects and, indeed, that persuasion under these circumstances might even be more effective. The sociological literature on deviance (e.g., Schur, 1971), however, suggests that negative attributions (e.g., labeling as delinquent) can indeed produce or support the attributed behavior, as does a recent experimental study of labeling "charitable" or "uncharitable" behaviors (Kraut, 1973). We may suspect, then, that the positive implications of the attributions in the present study were not the sole key to their effectiveness and that if a suitable experiment could be designed in which the target behavior were socially undesirable, attribution would continue to be more effective than persuasion in generating that behavior.

Attribution and Reinforcement

A straightforward reward contingency program seems to modify behavior because it makes that behavior worthwhile to the subject. However, the separation between reinforcement and attribution seems somewhat confounded. Symbolic reinforcement, as used in the present study, has some attributional aspects. Simple praise is often interpreted as a "You are X" statement. Furthermore, attribution can contain elements of reinforcement especially when socially desirable behaviors are the focus of the attributional process. Thus to some extent a reinforcement procedure that produces enduring change may require elements of attribution, while a successful attribution treatment may involve elements of reinforcement.

The remaining feature distinguishing attribution from reinforcement would seem to be the noncontingent nature of the attribution. In this regard it is interesting to note that Kazdin (1973) has recently found that under circumstances where the desired behavior was emitted at a fairly high base rate and subjects believed that reinforcement was contingent, noncontingent reinforcements were as effective as contingent ones in modifying behavior. Attribution treatments, however, may

have the very important further advantage over simple reinforcement of serving to elicit behavior (like modeling; see Bandura, 1969) as well as to maintain it.

Practical and Ethical Implications

The present study supports the idea that an effort to improve the child's academic self-concept will help improve academic performance, if only because the improved self-image will make actual success less inconsistent, less unexpected, and less likely to be discounted or rejected (Brickman, 1972). A distinguishing feature of the present attribution treatments is that they focused on raising self-esteem in a specific area of skill rather than raising global or general self-esteem. The failure of more general "cultural enrichment" or general "self-concept enhancement" programs may be due in part to the fact that these global manipulations have only vague and diffuse implications for particular areas of academic performance (but cf. deCharms, 1972).

Nonetheless, there are a number of reasons to be cautious about considering these results as the basis for a solution to any social problem. It is unlikely that long-standing individual differences in accomplishment will be overcome by short-term manipulation of motivation, incentive, or self-regard. A gain of three problems solved after a week of treatment is not very substantial in terms of life chances; and there is no reason to assume that 10 weeks of treatment will necessarily result in a gain of 30 problems. Second, the attribution treatments in Study 2 were difficult to administer and, unlike the treatments in Study 1, did not produce any immediate and visible indications of success to sustain teacher enthusiasm. Although the teachers involved in Study 1 were quite positive toward the study, those involved in Study 2 had decidedly mixed feelings about the value of the time and energy involved. More seriously, the false attributions came increasingly to be felt by at least one teacher and by the principal as an intolerable risk to their credibility. As indicated, the principal terminated her office meetings with the students after the third day of treatment mainly on these grounds. While future research could tailor attributions not to be too discrepant from individual pretest baselines, the practical and ethical difficulties involved in maintaining such attributions will not thereby be eliminated. All of these matters warn against an uncritical application of the present results to matters of educational importance.

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10

The Impact of Mass Media Violence on U.S. Homicides

David P. Phillips

The impact of mass media violence on aggression has almost always been studied in the laboratory; this paper examines the effect of mass media violence in the real world. The paper presents the first systematic evidence indicating that a type of mass media violence triggers a brief, sharp increase in U.S. homicides. Immediately after heavyweight championship prize fights, 1973–1978, U.S. homicides increased by 12.46 percent. The increase is greatest after heavily publicized prize fights. The findings persist after one corrects for secular trends, seasonal, and other extraneous variables. Four alternative explanations for the findings are tested. The evidence suggests that heavyweight prize fights stimulate fatal, aggressive behavior in some Americans.

Since 1950 more than 2500 studies have attempted to discover whether mass media violence triggers additional aggressive behavior (Comstock et al., 1978; Murray and Kippax, 1979; Roberts and Bachen, 1981; National Institutes of Mental Health, 1982). With few exceptions (reviewed in Phillips, 1982b), researchers have studied aggression *in the laboratory,* and there is consensus that media violence can trigger additional aggression in the laboratory setting. However, policy makers, unlike researchers, have been primarily concerned with violence *outside* the laboratory, particularly with serious, fatal violence like homicide. Studies of media effects on homicide have been extremely rare and there is no systematic evidence to date indi-

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Direct all correspondence to: David P. Phillips, Department of Sociology, University of California, La Jolla, CA 92093.

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cating that mass media violence elicits additional murders.[1] As Andison has noted (1980:564), we do not know whether "there are deaths and violence occurring in society today because of what is being shown on the TV screen."

This paper presents what may be the first systematic evidence suggesting that some homicides are indeed triggered by a type of mass media violence. The current study builds on earlier research (Phillips, 1974, 1977, 1978, 1979, 1980, 1982a) which showed that: (1) U.S. suicides increase after publicized suicide stories. This finding has been replicated with American (Bollen and Phillips, 1982) and Dutch (Ganzeboom and de Haan, 1982) data. (2) The more publicity given to the suicide story, the more suicides rise thereafter. (3) The rise occurs mainly in the geographic area where the suicide story is publicized. (4) California (Phillips, 1979), Dutch (Ganzeboom and de Haan, 1982), and Detroit (Bollen and Phillips, 1981) auto fatalities all increase just after publicized suicide stories. (5) The more publicity given to the stories, the greater the increase, and (6) the increase occurs mainly in the area where the story is publicized. (7) Single-car crash fatalities increase more than other types, and (8) the driver in these crashes is significantly similar to the person described in the suicide story, while the passengers are not. These results are statistically significant and persist after correction for day-of-the-week and seasonal fluctuations, holiday weekends, and linear trends. After testing alternative explanations, Phillips concluded that suicide stories appear to elicit additional suicides, some of which are disguised as auto accidents.

It would be interesting to discover whether *homicide* stories elicit additional homicides. But it is difficult to conduct such a study because, unlike suicide stories, homicide stories occur so often that it is very difficult to separate the effect of one story from the effect of the others. However, some other types of violent stories occur much less often, and it is possible to discover whether these types of stories trigger a rise in U.S. homicides.

MASS MEDIA VIOLENCE AND U.S. HOMICIDES

In reviewing the literature on media effects, Comstock (1977) concluded that violent stories with the following characteristics were most likely to elicit aggression: When the violence in the story is presented as (1) rewarded, (2) exciting, (3) real, and (4) justified; when the perpetrator of the violence is (5) not criticized for his behavior and is presented as (6) intending to injure his victim.[2]

One type of story that meets all of these criteria is the heavyweight prize fight, which is almost universally presented as highly rewarded, exciting, real, and

[1]Some anecdotal data link a particular murder with subsequent murders or murder attempts (e.g., the Tylenol "copycat" crimes). But I know of only one *systematic* study of the topic (Berkowitz and Macaulay, 1971). This study found no increase in homicides after three publicized murder stories.

[2]Comstock also notes that a story is more likely to be imitated if the aggressor in the story is like the person exposed to the story, and if the victim in the story is like the imitator's victim. These points will be taken up later in this paper.

justified. Furthermore, the participants are not criticized for their aggressive behavior and are presented as trying to injure each other.

In a well-known series of studies, Berkowitz and various associates (1963, 1966, 1967, 1973) examined the impact of a filmed prize fight in the laboratory. They found that angered laboratory subjects behaved more aggressively after seeing a filmed prize fight scene. In contrast, angered laboratory subjects exposed to a track meet film displayed a significantly lower level of aggression.

In sum, the heavyweight prize match is a promising research site because (1) it meets Comstock's criteria for stories most likely to elicit aggression, and (2) it is known to elicit aggression in the laboratory.

DATA SOURCES

An exhaustive list of championship heavyweight prize fights and their dates was obtained from *The Ring Book Boxing Encyclopedia,* which is the standard reference on the topic. The period 1973–1978 has been chosen for analysis because, for this period, daily counts of all U.S. homicides are publicly available from the National Center for Health Statistics.[3]

METHOD OF ANALYSIS

A standard time-series regression analysis is used.[4] Homicides are known to fluctuate significantly by day of the week, by month, and by year (Conklin, 1981). In addition, as we will see, homicides rise markedly on public holidays. All these "seasonal" effects must be corrected before one can assess the effect of prize fights on homicides.

A 0–1 dummy variable was constructed for all days that were Mondays, another dummy variable was coded for Tuesdays, and in general a different dummy variable was assigned to each day of the week, with Sunday being the omitted variable. Similarly, a 0–1 variable was coded for each month of the year (with January being the omitted variable), and for each year (with 1978 being the omitted variable). In

[3]Data for 1973–1977 consist of computerized death certificates generated by the National Center for Health Statistics and made available by the Inter-University Consortium for Political Science Research. As of this writing, 1978 computerized death certificates are not yet publicly available. Consequently, for 1978, a published table (National Center for Health Statistics, 1978: Table I-30) has been used instead. A 50 percent sample of 1972 deaths is also available but will not be analyzed, because its inclusion with the complete, 100 percent sample data for 1973–1978 would violate the assumption of homoscedasticity required in the analysis that follows. It is theoretically possible to correct for this type of heteroscedasticity and then include the 1972 data in the analysis. But it was judged unnecessary to do so, because the data set is already very large even without the 1972 information. In all, there are 2192 data points for the daily data, 1973–1978.

[4]For the application of this approach to daily mortality data, see Bollen and Phillips (1981, 1982). For general introductions to time-series regression techniques, see Ostrom (1978), Rao and Miller (1971) and Johnston (1972).

addition, a dummy variable was assigned to each of the public holidays (New Year's Day, Memorial Day, Independence Day, Labor Day, Thanksgiving, and Christmas). Finally, a dummy variable, PFIGHT(X), was used to indicate the presence of a championship prize fight. The regression coefficient of PFIGHT(X) gives the effect of a prize fight on homicides X days later (i.e., the effect of a prize fight lagged X days). Initially, the effect of the prize fight is examined for the 10-day period following it; later, a longer period is studied.

RESULTS

Table 10.1 gives the size and statistical significance of each coefficient.[5] This table shows that, after the average championship prize fight, homicides increase markedly on the third day (by 7.47) and on the fourth day (by 4.15), for a total increase of 11.62.[6] The rise in homicides after the prize fight is statistically significant.[7]

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[5]These statistical significances are biased if there is serial correlation among the regression residuals. The conventional test for serial correlation, the Durbin-Watson test, is appropriate when a lagged dependent variable is included in the regression model (Nerlove and Wallis, 1966), as is the case in Table 10.1. A common alternative test, using Durbin's h statistic, cannot be used here for reasons described in Bollen and Phillips (1982: fn. 7). Consequently, another test for serial correlation proposed by Durbin (1970) was used instead. This test reveals no evidence of first-order autocorrelation. Autocorrelation of higher orders was sought by the methods described in Bollen and Phillips (1982), with no evidence of serial correlation being uncovered. One other feature of Table 10.1 should be mentioned briefly. The table shows that homicides increase markedly on all U.S. public holidays, except Memorial Day. To my knowledge, this finding has not been previously demonstrated with U.S. daily homicide data.

[6]The coefficient of .12 for HOMICIDE(1) indicates that there is a small, lagged endogenous effect. This implies, for example, that each of the lagged prize fight dummies has its impact distributed over more than one day. Therefore, the effect of prize fights on homicides one day later (for example) does not take place one day later, but is realized over a longer period. The small coefficient for the endogenous variable (.12) means that the long-run effects of PFIGHT(X) and other variables decay very rapidly and aren't much more than their immediate ones, but the distributed effects do exist. Thus, the pattern of lags is more complicated than is immediately apparent from Table 10.1. In sum, because of small, lagged endogenous effects, the impact of one PFIGHT(X) variable overlaps to a small extent with the impact of another. However, the presence of the lagged endogenous variable does not affect the validity of the statistical tests of the hypotheses (see also footnote 7).

[7]In Table 10.1 we are examining the series of 11 coefficients, PFIGHT(0), PFIGHT(1), . . . PFIGHT(10). Under the null hypothesis, none of these 11 prize fight coefficients is likely to be very large. On the other hand, under the alternative hypothesis that prize fights trigger homicides, one or more of these 11 coefficients is likely to be large and positive. If one or more of the PFIGHT(X) coefficients is sufficiently large, we can reject the null hypothesis in favor of the alternative. One way to discover whether H_0 can be rejected is to proceed as follows. Because the covariance matrix indicates that the estimates of the coefficients for PFIGHT(X) are uncorrelated, and because of the asymptotic normality of the coefficient estimates, it follows that these coefficient estimates are in fact independent. This in turn implies that the *t*-statistics for each of these coefficients are independent. Under these circumstances, one can use the binomial test to evaluate the probability of finding that r or more of the PFIGHT(X) coefficients are statistically significant at a given level. Table 10.1 indicates that there are two PFIGHT(X) coefficients — PFIGHT(3), PFIGHT(4) — which are statistically significant at .025 or better. For $n = 11$, $P = 0.25$, $r \geqslant 2$, the binomial test indicates that the probability of finding two or more significance levels of .025 in 11 independent trials is .0296. Thus, we can reject the null hypothesis on the joint evidence provided by the 11 PFIGHT(X) coefficients.

TABLE 10.1
U.S. homicides regressed on heavyweight prize fight, controlling for daily, monthly, yearly, and holiday effects, 1973–1978

Regressand	R^2	$\bar{R}^2$	D.F.	N
HOMICIDES	.671	.665	2148	2190

Regressor	Regression coefficient	*t*-statistic
Intercept	55.34*	30.16
HOMICIDE(l)	.12*	5.64
PFIGHT(−1)	1.97	.94
PFIGHT(0)	1.95	.93
PFIGHT(l)	−.26	−.13
PFIGHT(2)	1.32	.63
PFIGHT(3)	7.47***	3.54
PFIGHT(4)	4.15†	1.97
PFIGHT(5)	−.60	−.29
PFIGHT(6)	3.28	1.57
PFIGHT(7)	.35	.17
PFIGHT(8)	.99	.47
PFIGHT(9)	3.10	1.48
PFIGHT(10)	2.28	1.09
Monday	−16.46*	−21.74
Tuesday	−16.71*	−17.97
Wednesday	−18.42*	−19.13
Thursday	−15.81*	−15.88
Friday	−8.02*	−8.41
Saturday	14.54*	16.95
February	1.88**	1.99
March	1.13	1.23
April	.43	.46
May	−.69	−.73
June	1.61	1.74
July	4.16*	4.46
August	4.46*	4.83
September	3.91*	4.16
October	2.79*	3.02
November	3.04*	3.25
December	5.86*	6.30
1973	−1.11	−1.70
1974	1.71*	2.62
1975	1.28	1.96
1976	−3.01*	−4.60
1977	−1.73*	−2.62
New Year's Day	41.08*	10.29
Memorial Day	−1.05	−.28
Independence Day	21.61*	5.89
Labor Day	16.92*	4.56
Thanksgiving	18.34*	4.98
Christmas	10.25*	2.79

Note: The variable HOMICIDE(l) indicates homicides lagged one day. Two-tailed *t*-tests are used for all seasonal variables; one-tailed *t*-tests for prize fight variables.

*Significant at .01 or better.

**Significant at .05 or better.

***Significant at .0002.

†Significant at .025.

Table 10.1 shows that the third day displays by far the largest peak in homicides. It is interesting to note that this "third-day peak" appears not only in the present study but also, repeatedly, in several earlier investigations: California auto fatalities peak on the third day after publicized suicide stories (Phillips, 1979), as do Detroit auto fatalities (Bollen and Phillips, 1981) and U.S. noncommercial airplane crashes (Phillips, 1978, 1980). At present we do not know the precise psychosocial mechanisms producing the third day lag, but this phenomenon has now been replicated so often in different data sets that it seems to be a relatively stable effect which will repay future investigation.

The observed peak in homicides after a prize fight cannot be ascribed to day-of-the-week, monthly, yearly, or holiday effects, because all of these "seasonal" variables were corrected for in the regression analysis. In addition, one cannot plausibly ascribe the homicide peak to random fluctuations, because the peak is statistically significant.

SOME ALTERNATIVE EXPLANATIONS FOR THE PEAK IN HOMICIDES

Two different explanations can be tested with the data in Table 10.2. For each fight, this table indicates: (1) The number of homicides observed three days after the prize fight. (2) The number of homicides expected on the third day, under the null hypothesis that prize fights have no effect on homicides.[8] (3) The difference between the observed and expected number of homicides. (A positive difference indicates that homicides are higher than expected just after the prize fight.) (4) Whether the fight was held outside the United States. (5) Whether the fight was discussed on the network evening news.

TABLE 10.2
Fluctuation of U.S. homicides three days after each heavyweight prize fight, 1973–1978

Name of fight	Observed no. of homicides	Expected no. of homicides	Observed minus expected	Fight held outside U.S.?	On network evening news?
Foreman/Frazier	55	42.10	12.90	yes	yes
Foreman/Roman	46	49.43	−3.43	yes	no
Foreman/Norton	55	54.33	.67	yes	no
Ali/Foreman	102	82.01	19.99	yes	yes
Ali/Wepner	44	46.78	−2.78	no	yes
Ali/Lyle	54	47.03	6.97	no	yes
Ali/Bugner	106	82.93	23.07	yes	no
Ali/Frazier	108	81.69	26.31	yes	yes
Ali/Coopman	54	45.02	8.98	yes	no
Ali/Young	41	43.62	−2.62	no	no
Ali/Dunn	50	41.47	8.53	yes	yes
Ali/Norton	64	52.57	11.43	no	yes
Ali/Evangelista	36	42.11	−6.11	no	no
Ali/Shavers	66	66.86	−.86	no	no
Spinks/Ali	89	78.96	10.04	no	yes
Holmes/Norton*	53	48.97	4.03	no	no
Ali/Spinks	59	52.25	6.75	no	yes
Holmes/Evangelista*	52	50.24	1.76	no	no

*Sponsored by World Boxing Council; all other fights sponsored by the World Boxing Association.

[8]Under the null hypothesis, PFIGHT(X) has no impact on the number of homicides; thus, for Table 10.2 the expected number of homicides under H_0 is calculated by omitting PFIGHT(X) from the regression variables and rerunning the regression equation.

"Personal experience" hypothesis. Perhaps the prize fight affects only those actually attending the fight, not those experiencing it through the mass media. If this is so, one cannot claim that *mass media* violence is triggering a rise in homicides.

If one must personally experience the prize fight in order to be affected by it, then prize fights occurring outside the United States should trigger few if any U.S. homicides. In contrast, prize fights held inside the United States should elicit much larger rises in homicides. The evidence in Table 10.2 contradicts these predictions. After the average "foreign" fight, U.S. homicides rise by 12.128, while a much *smaller* rise, 2.862, occurs after the average U.S. fight.[9] Thus, the "personal experience" hypothesis does not seem plausible.

"Modeling" hypothesis—first test. A different hypothesis can also be tested with the data in Table 10.2. Prize fights may trigger some homicides through some type of modeling of aggression. If this is so, then prize fights receiving much publicity should have a greater effect than prize fights receiving less publicity.

One way to test this hypothesis is to see whether prize fights discussed on the network evening news are followed by relatively large increases in homicides, while relatively small increases occur after the remaining, less-publicized prize fights.[10] The evidence in Table 10.2 is consistent with this "modeling" explanation. Homicides rise by 11.127 after the average "publicized" fight, and by only 2.833 after the average unpublicized one. The difference between these two figures is statistically significant at .0286 (two-sample *t*-test, one-tailed).[11]

It is perhaps worth noting that the most touted of all the prize fights in this period, the so-called "Thrilla in Manilla" between Ali and Frazier, displays the largest third-day peak in homicides.

"Modeling" hypothesis—second test. The modeling hypothesis can also be tested in another way. The laboratory literature on the modeling of mass media aggression (see footnote 2) repeatedly suggests that (1) a person is more likely to imitate an aggressor on the screen if he is similar to that aggressor; and (2) a person is more likely to aggress against a target victim if his target is similar to the victim on the screen.[12] In sum, the laboratory literature suggests that there is modeling of both the *aggressor* and of the aggressor's *victim.*

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[9]At present, we do not know why U.S. homicides rise so much more after foreign than domestic fights. Perhaps a detailed study of the characteristics of these fights would help to resolve this question.

[10]A thorough analysis of this topic is desirable but would be extremely laborious. Future studies might attempt to measure the additional publicity derived from advertisements in all the media, not only at the time of the fight, but also in the weeks and months preceding it. In addition, one might wish to measure closed circuit television receipts, corrected for inflation.

[11]The formula used for this particular *t*-test does not require that the two compared populations have equal variances. For a description of this test, see Brownlee (1965:299–303). One might prefer to substitute the Mann-Whitney for the *t*-test. When this is done, $P = .0211$.

[12]Berkowitz and associates (1963, 1966, 1967, 1973) have shown this in a series of ingenious studies particularly relevant to this paper. They showed that laboratory subjects were most likely to inflict shocks on a target if that target had the same name as the losing boxer on the screen.

If *aggressor* modeling exists after a prize fight, then after a young, black male wins a boxing match, murders by young, black males should increase (but murders by young, *white* males should not). Conversely, after a young, white male wins a boxing match, the opposite findings should occur. Unfortunately, *aggressor* modeling cannot be studied with the death certificates examined in this paper, because these certificates do not reveal the identity of the murderer, only of the victim.

However, it is possible to use these death certificates to discover whether *victim* modeling exists after a heavyweight prize fight. If such modeling occurs, then, just after a prize fight, homicide victims should be unusually similar to the losing boxer. Specifically, after a young, *white* male is beaten in a prize fight, the homicide deaths of young, white male victims should increase; no such increase should appear for young, *black* male victims. Conversely, after a young, *black* male is beaten in a prize fight, the homicide deaths of young, *black* male victims should increase, while the homicide deaths of young, *white* males should not.

These predictions can be tested with the information in Tables 10.3 and 10.4, which distinguish between the impact of "black loser" prize fights (in which a black is beaten) and "white loser" prize fights (in which a white is beaten).[13] The detailed mortality data necessary to generate these tables can be found only in the computerized death certificates cited in footnote 3. These are available only for 1973–1977. Thus, it should be stressed that the period to be examined in the remainder of this paper is 1973–1977, not 1973–1978, as in Tables 10.1 and 10.2.

Table 10.3 examines the impact of "white loser" and "black loser" prize fights on the homicides of young, *white* male victims. The evidence supports the hypothesis of victim modeling. *White* loser prize fights are followed by significant increases in young, white male homicide deaths; in contrast, *black* loser prize fights do not seem to trigger young, white male homicide deaths.[14]

White homicides increase significantly on the day of the prize fight (by 3.86 per fight), two days thereafter (by 3.14 per fight), and eight days after the fight (by 2.97 per fight). Thus young, white male homicides rise by a total of 9.97 (= 3.86 + 3.14 + 2.97)

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[13]In the period under study (1973–1977) nearly all the losing boxers were 20–34.9 years of age; consequently I have defined "young males" as men in this age range. Nearly all the losing boxers were white (Wepner, Bugner, Coopman, Dunn) or black (Frazier, Norton, Foreman, Lyle, Young, Shavers). However, two of the losing boxers were Hispanic Americans (one Uruguayan and one Puerto Rican). There is no separate classification for Hispanic Americans on the computerized death certificate, and it is unclear whether one can treat these fighters as either white or black. Consequently, these two fights have been excluded from the analysis that follows.

[14]The analysis described in footnote 7 (and applied to Table 10.1) can be reapplied to the results in Table 10.3. Examining the coefficients, WL(0), . . . WL(10), we see that three are statistically significant at .0251 or better. The covariance analysis indicates that the estimates of the coefficients WL(X) are uncorrelated. Because of this and the asymptotic normality of the coefficient estimates, we can treat as independent the *t*-statistics for WL(0), . . . WL(10). Using the binomial test, with $n = 11$, $P = .0251$, $r \geqslant 3$, one finds that the probability of finding three or more coefficients significant at .0251 in 11 independent trials is .0022. Hence, the homicides of young, white males increase significantly just after "White Loser" prize fights.

TABLE 10.3
Impact of "White Loser" (WL) and "Black Loser" (BL) prize fights on the homicides of young, white male victims, U.S., 1973–1977[a]

Regressand	R^2	$\bar{R}^2$	D.F.	N
HOMICIDES	.3781	.360	1772	1825

Regressor	Regression coefficient	*t*-statistic
Intercept	10.43*	23.78
HOMICIDE(1)	.01	.23
WL(−1)	.70	.46
WL(0)	3.86**	2.54
WL(1)	.30	.20
WL(2)	3.14**	2.07
WL(3)	.48	.31
WL(4)	.57	.37
WL(5)	−.29	−.19
WL(6)	.93	.61
WL(7)	.35	.23
WL(8)	2.97†	1.96
WL(9)	.53	.35
WL(10)	.58	.39
BL(−1)	1.36	1.27
BL(0)	.04	.03
BL(1)	−1.44	−1.34
BL(2)	−.59	−.55
BL(3)	1.20	1.11
BL(4)	.69	.64
BL(5)	−1.06	−.99
BL(6)	1.61	1.50
BL(7)	−.14	−.13
BL(8)	.32	.30
BL(9)	.28	.26
BL(10)	−.53	−.49

[a]As in Table 10.1 the effect of prize fight variables is calculated, controlling for seasonal variables. For reasons of clarity, the coefficients for these seasonal variables have not been displayed in Table 10.2 since the prime purpose of this table is to contrast the impact of "White Loser" and "Black Loser" prize fights. One-tailed *t*-tests are used for the prize fight variables; two-tailed *t*-tests for all other variables.

*Significant at less than .001. **Significant at .006. ***Significant at .019. †Significant at .0251.

per white loser prize fight. Interestingly, the typical white loser prize fight has a larger total impact (9.97) than almost any other variable in the table. Of the 27 "seasonal" variables examined, only one (New Year's Day) has a larger impact on young, white male homicides.[15] This suggests that the impact of a white loser prize fight is not only statistically significant, but practically significant as well. At present, it is not known why this type of prize fight seems to exert so large an effect.

[15]The coefficient for this holiday is 15.75. Although the effect of any given prize fight is large compared with the effect of seasonal variables, the cumulative effect of all prize fights combined is not large compared with the cumulative effect of all seasonal variables combined. This is because there are relatively few prize fights.

TABLE 10.4
Impact of "White Loser" (WL) and "Black Loser" (BL) prize fights on the homicides of young, black male victims, U.S., 1973–1977

Regressand	R^2	$\bar{R}^2$	D.F.	N
HOMICIDES	.452	.436	1772	1825

Regressor	Regression coefficient	*t*-statistic
Intercept	10.59*	20.79
HOMICIDE(1)	.04	1.63
WL(−1)	.83	.48
WL(0)	−1.30	−.75
WL(1)	−1.60	−.93
WL(2)	.19	.11
WL(3)	2.82	1.59
WL(4)	−.82	−.47
WL(5)	−1.19	−.69
WL(6)	−1.66	−.96
WL(7)	2.80	1.62
WL(8)	−.78	−.45
WL(9)	1.62	.94
WL(10)	.59	.34
BL(−1)	−.25	−.21
BL(0)	1.19	.98
BL(1)	−.60	−.49
BL(2)	.18	.15
BL(3)	.67	.54
BL(4)	2.68**	2.19
BL(5)	2.28***	1.86
BL(6)	−.22	−.18
BL(7)	.04	.03
BL(8)	−.76	−.62
BL(9)	1.50	1.23
BL(10)	.30	.25

*Significant at less than .001. **Significant at .014. ***Significant at .032.
See also footnotes to Table 10.3.

Table 10.4 examines the impact of "white loser" and "black loser" prize fights on the homicides of young, *black* male victims. Once again, the evidence supports the hypothesis of victim modeling. Black loser prize fights are followed by significant increases in young, black male homicide deaths. In contrast, white loser prize fights do not trigger significant increases in black male homicides.

Black homicides rise significantly on the fourth and fifth days after black loser fights by a total of 4.96 (= 2.68 + 2.28) per fight.[16] The total impact of the black loser prize fight exceeds the impact of almost all seasonal variables. Only New

[16]The analysis of Table 10.4 is parallel to that of Table 10.3. Once again, statistical theory and the covariance analysis justify treating as independent the 11 *t*-statistics for BL(0), . . . BL(10). We observe two BL(X) coefficients significant at .032 or better. The probability of finding two or more BL(X) coefficients significant at this level in 11 independent trials is .0465. Hence, young, black male homicide deaths increase significantly just after "Black Loser" prize fights.

Year's Day and Thanksgiving trigger larger increases in homicides (the coefficients for these holidays being 8.88 and 8.00, respectively). Evidently, a black loser prize fight has a significant, substantive effect on young, black male homicides.

Precipitation hypothesis. The above evidence is consistent with the notion that prize fights sometimes serve as aggressive models and trigger some U.S. homicides. But perhaps the prize fight merely precipitates a murder that would have occurred anyway, even in the absence of the prize fight.

If a prize fight merely "moves up" a murder so that it occurs a little sooner than it otherwise would have, then the peak in homicides after a prize fight should be followed by a *dip* in homicides soon after. An examination of the three-week period following the prize fight reveals no significant dip in homicides. None of the negative coefficients for PFIGHT(l), PFIGHT(2), . . . , PFIGHT(21) is significant, even at the .10 level. Hence, the precipitation hypothesis seems to be implausible.

Gambling hypothesis. Perhaps the prize fight provokes no aggressive modeling whatsoever. It merely triggers an increase in gambling, which in turn provokes anger, fighting, and murder. If this explanation is correct, then homicides should rise not only after prize fights but also after other occasions that provoke a great deal of gambling. In the United States, the Super Bowl probably provokes more gambling than any other single event. Yet homicides do *not* rise significantly after these occasions.

One can construct a variable, SUPERBOWL(X), to assess the impact of the Super Bowl on homicides X days later, and one can include this variable in the regression model specified in Table 10.1. The coefficients for SUPERBOWL(X) are listed in Table 10.5. There is some weak evidence that homicides actually *decrease* on the day of the Super Bowl and one day later, and then rise above the expected rate on the third day. Even if one considers these coefficients to be statistically significant (which they are not), it is evident that the Super Bowl is associated with a net drop in homicides rather than a rise. This is not what one would expect if the gambling hypothesis were correct. This hypothesis is also rendered implausible by some of the other evidence presented above: If the gambling hypothesis were true, then it is difficult to see why the traits of the homicide victims should be similar to the traits of the losing boxer.[17]

[17]The evidence presented does not support the notion that the gambling hypothesis is a necessary and sufficient explanation for the rise in homicides after a prize fight. But it remains possible that gambling *in combination with* aggressive modeling is helping to provoke the increase in homicides. One way to test this hypothesis is to examine police case histories of murders occurring three and four days after a prize fight. These case histories would have to be compared with case histories taken from control periods.

TABLE 10.5
Impact of the Superbowl on U.S. homicides, controlling for the effect of seasonal and prize fight variables, U.S., 1973–1977

Regressor	Regression coefficient	*t*-statistic
SUPERBOWL(−1)	2.78	.68
SUPERBOWL(0)	−5.03	−1.22
SUPERBOWL(1)	−6.36	−1.55
SUPERBOWL(2)	2.26	.55
SUPERBOWL(3)	6.00	1.46
SUPERBOWL(4)	−1.41	−.34
SUPERBOWL(5)	1.10	.27
SUPERBOWL(6)	−1.67	−.41
SUPERBOWL(7)	−2.24	−.54
SUPERBOWL(8)	3.61	.87
SUPERBOWL(9)	−4.82	−1.17
SUPERBOWL(10)	2.16	.52

Note: The coefficients for the other regressor variables (i.e., prize fight and seasonal variables) are not displayed.

In sum, we have now assessed four possible explanations for the rise in homicides after a heavyweight prize fight. At present, the best available explanation is that the prize fight provokes some imitative, aggressive behavior, which results in an increase in homicides. The size of this increase will be considered in the next section.

Size of the increase in homicides after prize fights. Column 3 of Table 10.3 gives the amount by which homicides increase on the third day after each prize fight. The sum of the numbers in this column is 125.64, indicating that U.S. homicides rose by this amount on the third day after championship heavyweight prize fights, 1973–1978. The sum of the numbers in column 2 gives the total number of homicides expected on the third day—1008.36. Dividing 125.64 by 1008.36 gives the percentage increase in homicides on the third day—12.46 percent. Thus, whether one considers the percentage increase or the absolute increase, it appears that homicides rise by a nontrivial amount on the third day after a championship heavyweight prize fight.

The rise in homicides on the fourth day is smaller but still not negligible. Employing calculations similar to those in Table 10.2, one can determine that homicides increase by 67.97 on the fourth day. The percentage increase is 6.58 percent (= 67.97/1033.03). For the third and fourth days combined, homicides increase by a little less than 200 (193.61 = 125.64 + 67.97). The percentage increase for the two-day period is 9.48 percent (= 193.61/[1008.36 + 1033.03]).

This paper has presented evidence which suggests that heavyweight prize fights provoke a brief, sharp increase in homicides. Some implications of this evidence will be briefly considered in the final section of this paper.

SUMMARY

Many researchers have claimed that one cannot generalize with confidence from the impact of mass media violence *in the laboratory* to the impact of mass media violence *in the real world.*[18] These critics point out that laboratory experiments have been set in highly artificial contexts. Typically, the sorts of aggression studied in a laboratory (like hitting plastic dolls or inflicting electric shocks) have not been representative of serious, real-life violence, such as murder or rape. In almost all studies, the laboratory subjects have been nursery school children or college students and thus not representative of the U.S. television audience. Typically, the laboratory subject is presented with a brief, violent excerpt of a television program. In contrast, the "real-life" viewer may watch several hours of television at a sitting, and the violence may be interspersed with humor, commercials, and trips to the bathroom. In contrast to the laboratory subject, who watches television alone, the real-life viewer may well be surrounded by family or friends. Their comments may distract from the television or shape the perception of its many messages. For these reasons, it is inappropriate to generalize from the laboratory to the real world.

The above argument appears to be seriously challenged by the evidence provided in this paper. The data presented in this paper indicate that mass media violence *does* provoke aggression in the real world as well as in the laboratory. In contrast to laboratory studies, the present investigation assesses the effect of mass media violence in a natural context. Unlike laboratory studies, the present study examines a type of violence which is of serious concern to policy makers. Finally, the present investigation does not focus exclusively on a mass media audience consisting of college students and nursery school children. The laboratory study, with its great potential for rigor, has always been capable of establishing the internal validity of findings. The present study has helped to establish that these findings have external validity as well.

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[18]Comstock (1975:30–40) provides a valuable summary of the debate on this topic. In addition, see Phillips ("Behavioral impact," 1982), who also indicates why it is difficult to generalize from the few field experiments that exist.

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IV

SOCIAL COGNITION

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11

Contrast Effects and Judgments of Physical Attractiveness: When Beauty Becomes a Social Problem

Douglas T. Kenrick and Sara E. Gutierres

Three studies were conducted to test the hypothesis that judgments of average females' attractiveness or dating desirability will be adversely affected by exposing judges to extremely attractive prior stimuli (i.e., judgments will show a "contrast effect"). Study 1 was a field study in which male dormitory residents watching a popular television show, whose main characters are three strikingly attractive females, were asked to rate a photo of an average female (described as a potential blind date for another dorm resident). These subjects rated the target female as significantly less attractive than did a comparable control group. Two other studies demonstrated analogous effects in a more controlled laboratory setting. In addition, the third study indicated a direct effect of informational social influence on physical attractiveness judgments. Implications are discussed, with particular attention to mass media impact.

Within the past several years, social psychologists have gathered a wealth of data attesting to the central importance of physical attractiveness in interpersonal interaction (see Berscheid and Walster, 1974, for a review). This variable has been found to have a particularly profound effect in dating situations (e.g., Berscheid, Dion, Walster, and Walster, 1971; Brislin and Lewis, 1968; Byrne, Ervin, and Lamberth, 1970; Stroebe, Insko, Thompson, and Layton, 1971; Walster, Aronson, Abrahams, and Rottman, 1966). Brislin and Lewis (1968), for instance, found a correlation of .89 between perceived physical attractiveness and the "desire to date."

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It has been noted that physical attractiveness has generally been used as an independent variable in social psychological research (Berscheid and Walster, 1974; Gross and Crofton, 1977). As Gross and Crofton (1977) put it, "beauty has been conceptualized as an invariant 'cause' in previous studies" (p. 86). Nevertheless, a number of studies have shown that judgments of attractiveness can be influenced by other information likely to affect interpersonal attraction in general, such as knowledge of a target person's attitudinal similarity (Walster, cited in Berscheid and Walster, 1974), information that she/he possesses positively valued traits (Gross and Crofton, 1977), actual acquaintance with the target person (Cavior, 1970), or association of the target person with a highly attractive other (Meiners and Sheposh, 1977). In addition to these situational variations, judgments of physical attractiveness have been found to vary across cultural and racial groups (e.g., Cross and Cross, 1971; Marshall and Suggs, 1971), although Berscheid and Walster (1974) suggest that modern mass media may soon obscure any such differences in favor of Western standards. These authors also suggest that socialization of romantic preference is accomplished through the mass media and that "few advertisements or popular movies and novels depict mundane levels of physical attractiveness" (p. 167). If the media do influence one's standards of attractiveness, while at the same time suggesting that only highly beautiful or handsome others are appropriate as love objects, one might expect an inverse relationship between exposure to mass media and the extent to which an individual's standards for the attractiveness of a romantic partner are "realistic."

In fact, if one can extrapolate the findings from research into other areas of perceptual judgment, there is reason to be concerned about even the short-term impact of mass media on our judgments of the attractiveness of the more mundane potential romantic partners around us. One consistently reported finding in perceptual judgments is a "contrast" effect; that is, judgments of moderate stimuli in a series are found to be displaced away from extreme or distant stimuli. This effect has been found for judgments of physical dimensions such as weight (e.g., Heintz, 1950; Sherif, Taub, and Hovland, 1958), length of lines (e.g., Krantz and Campbell, 1961); and shape (Helson and Kozaki, 1968); as well as social stimuli such as attitudes (Hovland, Harvey, and Sherif, 1957), pleasantness of facial expressions (Manis, 1971), criminal acts (Pepitone and DiNubile, 1976), and personality impressions (Simpson and Ostrom, 1976). If such effects can be presumed to generalize to judgments of physical attractiveness, prior exposure to highly unattractive individuals would result in an enhanced perception of the attractiveness of an "average" person, with the reverse being true of exposure to very attractive persons. Some additional implications would follow from the existence of such an effect in this realm. In our mass-media–oriented culture, where we are bombarded with highly attractive females, such exposure should produce a rather high "adaptation level" (Helson, 1964), resulting in lowered assessments of the beauty of average "real world" females. Given the particularly high relationship between such judgments and dating desirability, such exposure might also lead to an analogous decrement along this latter dimension as well.

It should be noted that in this case, there are at least two bodies of literature that might lead one to expect that such contrast effects would *not* hold in this realm. First, there is evidence that perceptual contrast will not be induced by stimuli that, although sufficiently distant from the stimulus along the relevant dimension, possess other characteristics that lead subjects to consider them to belong to different "universes of discourse" (Helson, 1971). Brown (1953), for instance, had subjects lift a tray between judgments of a series of weights. Although a similarly heavy anchor weight produced a contrast effect, lifting the tray had no effect on judgments. Similarly, Bevan and Pritchard (1963) found that shape judgments were not affected by grossly deviant or oversized stimuli. If media females are not considered to belong to the same category or "universe of discourse" as real-life females, their beauty might be discounted and fail to influence judgments of nonmedia females. Second, since physical attractiveness has been shown to have the qualities of a reinforcer (e.g., Byrne, Ervin, and Lamberth, 1970; Dion, 1977), and since contextual association with reinforcing events has been shown to enhance the attractiveness of a target person (Clore and Byrne, 1974; Lott and Lott, 1974), exposure to an average female in a context of highly attractive females might be expected to lead to a classical conditioning effect such that the average female would actually come to be judged more positively. Similarly, in a context of highly unattractive females, she should come to be judged more negatively if such a process applied here. Three studies were therefore performed to test the hypothesis that exposure to extreme attractiveness stimuli would produce contrasted judgments of a target person of average attractiveness.

Study 1 was a field study in which subjects were asked to judge a potential blind date for a fellow dormitory resident. One group of subjects was students who were watching the television program "Charlie's Angels" (whose main characters are three beautiful women), whereas controls consisted of residents of the same dormitory (sampled during the same night) who were not watching this show at the time.

STUDY 1

Method

Subjects. Subjects were 81 male dormitory residents at Montana State University. They participated in groups of 1 to 6 on the evening of February 1, 1977.

Procedure. Two male confederates entered a dormitory room during one of two time periods (during the hour preceding "Charlie's Angels," or during the hour at which "Charlie's Angels" was aired).[1] They explained to the students in the room:

[1]All subjects who were watching television during this time slot were tuned to this program, which was quite popular at the time of this study (sixth in the Nielsen ratings).

> **Confederate A:** Listen, could I just interrupt you guys for 30 seconds? We're having a major philosophical dispute here and we need to do an informal survey to resolve the question. You see, we have a friend coming to town this week and we want to fix him up with a date, but we can't decide whether to fix him up with her or not, so we decided to conduct a survey.
> **Confederate B:** You see, I don't think she looks very good.
> **Confederate A:** But I think she looks pretty good. At any rate, we want you to give us your vote on how attractive you think she is. (Confederate A begins to hold up the picture, but it is faced away from subjects so they can't see it).
> **Confederate B:** Right, on a scale of 1 to 7, with 1 being very unattractive, 4 being exactly average, and 7 being beautiful.
> **Confederate A:** (turns over photo) Now, nobody say anything until everyone makes up his own mind, and be honest—give your honest opinion.
> **Confederate B:** Remember, 1 is very unattractive, 4 is right in the middle, and 7 is very attractive.

Confederate A held up the picture, allowed each subject to make a silent judgment, and then had the subjects give their responses. They thanked the subjects, left the room, and immediately recorded the time, condition, and each subject's response independently. Agreement between the two confederates was 100 percent in all instances.

Stimulus photo. The stimulus photo was an 8 cm $\times$ 5 cm. black-and-white yearbook snapshot taken from a series of slides presented in earlier research (Kenrick and Gutierres, Note 1). A group of 11 undergraduate males from the population used in the present research had given this photo a mean rating of 4.11 on a scale analogous to that used by the subjects in our field study.

Results

Subjects' data were broken down into four groups. Those watching "Charlie's Angels" constituted the "experimental" group, whereas those watching another television program earlier in the night were designated as control subjects. To control for the possibility that ratings by experimental subjects may have been due simply to their having been made at a later hour, two additional control groups were included. Subjects in these groups were not watching television but were sampled either during the "Charlie's Angels" time slot or during the preceding hour. The data from the group watching "Charlie's Angels" were plotted against the data from all control groups, using a planned orthogonal contrast. It was expected that the "Charlie's Angels" group would show the lowest mean ratings of the target's attractiveness. Although subjects were cooperative in making silent independent judgments, the group mean was used as the unit of analysis in the contrast presented below.

In line with predictions, results indicated relatively lowest ratings by viewers of "Charlie's Angels" (M = 3.43, versus 4.00 combined M for the controls),[2] F (1, 24) = 5.03, $p < .03$. This contrast accounted for 84 percent of the between-groups variance. A similar test using the individual subject as the unit of analysis resulted in F (1, 77) = 7.39, $p < .01$.

Discussion

Results of Study 1 were directly in line with predictions, indicating relatively lowest ratings of an average female by subjects who were observing highly attractive media females. Nevertheless, since subjects were not randomly assigned to conditions, our results are open to several interpretations. First, "Charlie's Angels" viewers may have been more negative in their rating of the target female because of the immediate influence of the beautiful media stimuli. Second, the effect may have been due to the fact that these viewers were more negative because of a chronic tendency to expose themselves to highly attractive females depicted in the media. Both of these possibilities would be consistent with our hypotheses. A third possibility, however, is that some other difference may have existed to make "Charlie's Angels" viewers more generally negative in their judgments of females. Although we can offer no intuitively compelling reasons to assume this to be the case, it seems best to consider our hypothesis confirmation in Study 1 as suggestive evidence only at this point. Study 2 was designed to offer a more direct test of our hypothesis by manipulating exposure to media beauty in subjects randomly assigned to conditions.

STUDY 2

Method

Subjects. Subjects were 48 male undergraduates enrolled in introductory psychology at Montana State University, who participated in small groups of 3 to 5.

Procedure. Subjects arrived for an experiment entitled "personality" and were told

> This is a study of first impression formation. We are interested in determining how much we can tell about a person from only a brief encounter or glance. Many people assume that we can tell quite a bit from a person's face alone. It is assumed that we can tell whether someone is honest or dishonest, sociable or unsociable, et cetera

[2]Five groups of subjects (19 total) were watching "Charlie's Angels." Respective means and number of groups were 4.31 (n = 3) for nontelevision watchers sampled during the "Charlie's Angels" time slot and 4.08 (n = 8) and 3.82 (n = 12) for television watchers and nontelevision watchers, respectively, who were sampled during the previous time slot. A test of the residual effects was not significant.

> from their eyes or mouth, for instance. Advertisements, books, and magazines often include a certain type of face in an attempt to present a certain image to the public—of an intelligent scientist, an overworked housewife, a dedicated businessman, a vivacious and happy young model, and so on. As part of the present investigation we've given intensive psychological tests and interviews to a group of students and interviewed several of their friends and acquaintances as well. We've developed a personality profile for each of these persons. You will be seeing only a yearbook photo of one of these people, and your task will be to simply give your honest first impression of what that person is really like. You may not feel that you have enough information to respond to each dimension, but simply take your best guess, since accuracy or inaccuracy of impression formation is what we're interested in.

Following this, subjects were instructed not to communicate with one another verbally or nonverbally, since the experiment necessitated completely independent judgments. Seating was arranged so that observation of other subjects' written responses was not possible. For experimental subjects, a black-and-white slide of an attractive female in a magazine advertisement[3] was turned on during the verbal instructions, and the experimenter pointed to it (as if to give an example) as he mentioned the "young model." Control subjects heard the same instructions but were not exposed to the magazine ad. Following the instructions, all subjects were shown a slide of a female of average attractiveness (the same one used in the first study) and were given a "personality rating" sheet to fill out on her. The rating form contained several bipolar scales (likable–unlikable, reasonable–unreasonable, courteous–rude, selfish–unselfish, warm–cold, sincere–insincere, responsible–irresponsible, beautiful–ugly, kind–cruel). The rating of the target person along the dimension beautiful–ugly constituted the main dependent variable. It was predicted that she would be rated as significantly less beautiful following exposure to the attractive female advertisement.

Results

In line with predictions, ratings of the target person indicated that she was seen as significantly less beautiful by subjects exposed to the advertisement, F (1, 46) = 7.10, $p < .01$. Mean ratings were 4.41 for the experimental group and 3.52 for the controls (higher ratings indicate that the target person was seen as relatively *less* beautiful).[4] These results parallel those of the first study and cannot be explained as due to self-selection of subjects.

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[3]This slide was an advertisement for Wella Balsam, depicting the popular model Farrah Fawcett-Majors. In pretesting, a group of 21 undergraduates (11 males, 10 females) from the same population used in the study proper gave this slide a mean rating of 6.67 on the 7-point scale analogous to that used in Study 1.

[4]None of the other adjectives showed any effect of the manipulation except "responsible–irresponsible." Subjects exposed to the advertisement saw the target as significantly more responsible. Although these results were not predicted, they may fit with Stephan, Berscheid, and Walster's (1971) finding that under some circumstances, males who judge a female as sexually attractive may also see her as relatively "careless" and "uninhibited."

STUDY 3

The first two studies were concerned with the indirect influence of contextual stimuli on judgments of attractiveness. They demonstrated that exposure to beautiful media females could result in lowered assessments of a female of average attractiveness. A third study was conducted to provide an additional test of the question addressed in the first two studies while also examining the more direct impact of informational social influence (Deutsch and Gerard, 1955) on judgments of attractiveness. We are often exposed to our peers' assessments of members of the opposite sex, and a number of classical social psychological findings suggest that peers' judgments can influence assessments even of "objective" reality (e.g., Asch, 1951; Crutchfield, 1955; Sherif, 1935). Given the interpersonal importance of attractiveness judgments, it is of some interest to determine the applicability of this classical effect in the present realm.

Also of some interest was the question of whether exposure to peer evaluations would influence judgments of persons not directly commented on. That is, would exposure to peer evaluations produce an alteration of *standards* that would generalize to judgments of other persons (not directly evaluated)?

Finally, the third study included female subjects as well as males. Since physical attractiveness judgments have been found to influence interpersonal behaviors between same-sex as well as opposite-sex persons (see Berscheid and Walster, 1974, for a review), it is of interest to determine the applicability of our findings to same-sex assessments.

Method

Subjects. Subjects were 98 undergraduates (49 males, 49 females) enrolled in introductory psychology at Montana State University who participated in groups of 2 to 7. One subject was deleted because of suspicion regarding the male confederates.

Procedure. Subjects arrived for an experiment entitled "pretesting stimuli" and were led to a small room containing several chairs and a slide projector. In half the conditions, two male confederates posed as subjects and always sat together in the back row of two rows of five chairs. The experimenter explained

> As the sign-up sheets indicated, we're interested in having you help us pretest some stimuli for an experiment we'll be running at the start of next semester. What that experiment will involve is seeing how well people judge personality from only a small amount of information. We'll be showing people a photograph including only a person's face and asking them to judge the person's overall personality. We've found in the past, however, that these judgments are often influenced by other irrelevant factors, so we'll be asking you to make an objective judgment of several photographs so we can control for these irrelevant factors next semester. All right, for the first 6 photographs you'll be rating the physical attractiveness of each face.

Prior stimuli. Subjects were then given a sheet containing six 9-point scales labeled 1 (extremely unattractive) and 9 (extremely attractive). They were further instructed to observe each slide carefully for 40 seconds, at which point a blank screen would appear that would signal them to make their judgment. For all conditions Slides 2, 3, and 5 were held constant. These slides showed black-and-white yearbook photographs of females previously judged to be "average" in attractiveness (Kenrick and Gutierres, Note 1). Half the subjects saw highly attractive slides in Slots 1, 4, and 6, whereas the other half saw unattractive slides in these positions. Attractive and unattractive slides were also yearbook photos selected in the same manner as the "average" photos.

Confederate comments. Half of each group heard the confederates make comments about the third and fifth (average) slides. These comments were negative in the groups exposed to the high-attractive series (e.g., "What a dog," nonverbal utterances of displeasure) and positive in the groups exposed to the low-attractive series (e.g., "You can set me up with her," nonverbal utterances of attraction). After Slide 5 had been rated, the experimenter mentioned that any comments might influence the others in the room and asked that the subjects refrain from giving any public responses to the stimuli. (For controls, this request was made before Slide 1 was shown.)

Target person. After Slide 6 had been rated, subjects were told that the next slide would be "evaluated on a completely different dimension" and were handed a sheet that asked them to check one of seven sentences ranging from "I would find this person extremely desirable as a date" to "I would find this person extremely undesirable as a date." Female subjects were instructed to evaluate the female as a potential date for a male friend. This final photo was selected in the same manner as the other "average" slides (2, 3, and 5) and was, like them, held constant for all subjects.[5]

Subjects were then fully debriefed and were probed for suspicion. All reported having clearly heard the confederates' comments (when they were made), and most generally reported in informal discussion that they found them obnoxious and did not feel they were influenced. Nevertheless, as might be expected, other males had in several cases spontaneously joined in verbal agreement with the confederates' comments during the experimental session. Data were therefore treated using the group as the unit of analysis.

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[5]Although this photo was not rated on physical attractiveness within the context of the present study (as the other average photos used were) we did have it rated on a 7-point scale like that used in the first two studies (1 = extremely unattractive, 4 = average, 7 = extremely attractive) by 66 undergraduates (34 males, 32 females) from the same subject pool during the following academic year. The mean rating obtained was 4.42 for this group. Note that the polarity here is reversed from that of the 7-point "dating desirability" scale used in Study 3, and this should be read as 3.58 if one wishes to make direct comparison. We would suggest that the reader who chooses to do so should keep in mind that there may well be some slippage in making such a conversion.

Predictions

1. It was expected that exposure to attractive prior stimuli would lead to relatively decreased ratings of the target person's dating desirability.

2. It was further expected that subjects' ratings of the physical attractiveness of Slides 3 and 5 (the "average" slides for which comments were made) would be affected in the direction of the confederates' comments. Since this would have resulted in decreased ratings of these two slides in the attractive condition (since negative comments were made here) and increased ratings in the unattractive condition (since positive comments were made here), this would have shown up as an interaction between the confederate comment factor and the attractiveness factor.

3. In addition, we were interested in seeing whether confederates' comments would indirectly enhance the influence of the prior stimuli on ratings of the final target person (by further heightening standards in the attractive condition, and vice versa). This would also have shown up as an interaction effect, as indicated in Prediction 2 above.

Results

Manipulation check. Comparisons between attractive and unattractive slides in Positions 1, 4, and 6 yielded differences significant beyond the .001 level in each instance. Mean ratings of the three attractive slides were 7.00, 7.74, and 7.82 for the attractive and 4.24, 3.26 and 2.36 for the unattractive series, respectively. Discounting the effects of the independent variables (discussed below), the overall mean ratings for Slides 2, 3, and 5 (constant slides in series) were 5.18, 5.00, and 5.51, respectively.

Sex of subject. Prior to the analysis using the whole group mean as the unit of analysis, an analysis dividing each group into male and female subjects was performed. This analysis indicated that sex of subject yielded no main effects or interactions on any of the dependent variables, except for ratings of Slide 5, for which a main effect of subject sex was obtained, F (1, 29) = 8.26, $p < .01$. Females rated this slide as more attractive (M = 5.91) than did males (M = 5.19).

Main analyses. Ratings of the target person's "dating desirability" showed the predicted main effect of prior stimuli (P), F (1, 14) = 15.01, $p < .002$. As expected from our earlier results, subjects exposed to the attractive prior slides gave significantly lowered ratings of the target person (see Table 11.1). As indicated in the Predictions section above, a significant interaction of prior stimuli and confederates' comments (C) would have indicated that the comments further enhanced the standards set by the prior stimuli. The F for the interaction term was less than 1, thus failing to support this suggestion. The C "main effect" was also nonsignificant, F (1, 14) = 1.51.

Prior stimuli and confederate comments. On the slides for which confederates made direct comments, ratings were, as predicted, lowered in the attractive

TABLE 11.1
Influence of stimulus attractiveness and confederate comments on ratings of prior stimuli and target person, Study 3

	Attractiveness of prior stimuli			
	Attractive		Unattractive	
Item rated	Confederate comment	Control	Confederate comment	Control
Slide 3[a]	4.63	5.08	6.04	5.00
Slide 5[a]	4.30	5.02	6.96	6.08
Target person[b]	4.33	4.16	3.34	2.78

[a]Higher ratings indicate *more* positive ratings of female's beauty on a 9-point scale.
[b]Higher ratings indicate *less* desirability as a date on a 7-point scale.

condition (when negative comments were made) and enhanced in the unattractive condition (when positive comments were made). This showed up as a significant P $\times$ C interaction on Slide 5, F (1, 14) = 4.66, $p < .05$, and a marginally significant effect on Slide 3, F (1, 14) = 4.41, $p < .06$ (Table 11.1). In addition, there was a significant main effect of prior stimuli on Slide 5, F (1, 14) = 25.50, $p < .001$, as well as a similar trend on Slide 3, F (1, 14) = 3.91, $p < .07$, indicating relatively lower ratings in the context of highly attractive stimuli.[6] Since confederates' comments should have canceled out for the C effect, *F*s were, not surprisingly, less than 1 for ratings of both Slide 3 and Slide 5.

GENERAL DISCUSSION

The results of Study 3 were consistent with those of the first two studies in supporting the existence of a contrast effect phenomenon for judgments of physical attractiveness. This effect occurred despite the fact that the attractive stimuli in the first two studies were drawn from a different "universe of discourse" than that from which the target person was drawn. To the extent that the beauty of media females may have been "discounted" due to this factor, it was not sufficient to remove the adverse contrast effect. Similarly, the contrary prediction based on a simple application of classical conditioning principles (i.e., that reinforcement value of attractive photos would generalize to an average target photo in the same series) was not borne out. This result is consistent with other findings suggesting that simple generalization of affective reactions to attraction objects is often overruled by other (e.g., cognitive) factors (Kenrick and Johnson, 1979).

[6]Ratings of Slide 2 were also significantly more negative when it followed an attractive slide in Position 1, F (1, 14) = 9.23, $p < .01$.

Possible Implications

Media impact (the "Farrah factor").[7] The present results support the suggestion that our initial impressions of potential romantic partners will be adversely affected if we happen to have been recently exposed to posters, magazines, television, or movies showing highly attractive individuals (or if such stimuli are concurrently present). Kenrick and Gutierres (Note 1) found analogous results to those obtained here, using stimuli randomly chosen from advertisements in best-selling magazines. Their results indicated, not surprisingly, that media females are indeed selected from a highly skewed distribution with regard to physical attractiveness.

Whether our obtained effects are long lasting cannot be determined from the present series of studies. Even if such effects are very short-lived, however, they could still be of some consequence, influencing the desirability of females who happen to meet a male immediately following or during exposure to such media. Of some interest in this regard is a recent study by Snyder, Tanke, and Berscheid (1977), which suggests that initial judgments of a target's attractiveness may function as a self-fulfilling prophecy. In the Snyder et al. study, targets who were perceived to be unattractive actually came to behave in a less friendly and likable manner than targets who were regarded as attractive. Further, there is other research showing that the judged physical attractiveness of a computer date actually determines the likelihood of seeking further interaction with that person (Walster et al., 1966).

Thus, let us imagine a scenario involving a college-age male who, like the subjects in our first study, is engrossed in an episode of a television show containing unusually beautiful females in the central roles (the examples are not hard to come by, especially given the recent conscious and concerted effort of TV network producers to place very highly attractive women in starring roles). He is briefly introduced to a neighbor who happens to be a female of average physical attractiveness. Our data suggest that his immediate assessment of her attractiveness and dating desirability will be lower than might otherwise be the case. Based on the findings of Dion (1977), he might be expected to subsequently reduce his visual attention to her (thus retarding any return of his "adaptation level" to mundane levels). He might also act in such a manner as to inhibit demonstrations of friendliness on her part, following Snyder et al. (1977), and also be less likely to seek to interact with her in the future, in line with the findings of Walster et al. (1966).

Individual differences in history of exposure. Research on perceptual judgment in other areas suggests that the judgment of a stimulus is determined by both the immediate stimulus context and by a "pooled" estimate of the judge's past experience with stimuli in the same realm of discourse (Helson, 1964, 1971).

[7]This term was coined by the editors of *Human Behavior* magazine (February 1979 issue) for the effect we have been investigating in this series of studies.

Given the male college student's vast history of exposure to female facial stimuli, the manipulations in Studies 2 and 3 could only be seen as having a transitory impact, likely to be erased by relatively short exposure to real-world females. Nevertheless, given a tendency to selectively attend to and actively seek visual exposure to highly beautiful females (Dion, 1977), our results suggest the possibility that "chronic" standards for physical attractiveness may be somewhat inflated, particularly among individuals who are exposed to relatively more mass media (whose pooled estimate of a facial stimulus is based on a highly skewed and "nonrepresentative" sample). Note in this regard that there is evidence that the average adolescent in this society has spent more time watching television than in school (Gerbner and Gross, 1976).

We have focused our discussion thus far on the effects of media beauty, but it should also be pointed out that our results have potential implications for other realms as well. For instance, some individuals may be chronically exposed to unusually high levels of attractiveness by virtue of their occupation (e.g., airline pilots, bartenders in Playboy clubs, professors at UCLA, and so on). If these individuals are themselves unattractive, the effects of such exposure may be particularly adverse, leading to the adoption of unrealistically high standards and consequent dissatisfaction with those females actually available to them and likely to be interested in them (at least according to the "matching" hypothesis; Berscheid and Walster, 1974).[8]

Cognitive influences. Social psychologists have recently shown renewed interest in studying subjects' phenomenological reconstructions of the social situations they are faced with. Social behavior seems to be influenced not simply by "objective" environmental stimuli but also by the subjects' tendency to interpret these stimuli, selectively attend to them, and selectively recall them (e.g., Berkowitz, 1978; Snyder and Uranowitz, 1978). When the present research is considered in the light of such findings, some additional researchable implications unfold. Given the rewarding nature of attractiveness, individuals may well selectively notice the atypically attractive persons in their environments and selectively recall them (perhaps even actively generating images of such persons in their absence). Thus, an individual whose everyday activities expose him or her to a "representative" sample of opposite-sex persons may nevertheless *construct* a "biased" adaptation level.

It should be noted at this point that we have not elucidated the cognitive mediators underlying our obtained effect. Although such a question is not relevant for the social implications we have touched on, it would be of some theoretical interest to investigate the cognitive processes responsible for our effect. It seems unlikely, for instance, that contrast effects obtained in this realm (and in other realms of social judgment) are due to "receptor fatigue," as the analogous effects obtained with purely sensory phenomena might be (Helson, 1964), unless one posits a relatively

[8]Our thanks to an anonymous reviewer for this suggestion.

central and higher order mechanism for such judgments. On the other hand, an explanation in terms of "scale usage" effects (e.g., Anderson, 1975; Parducci, 1965) cannot deal with the results of Studies 1 and 2 and must be stretched a great deal to account for the results of Study 3.

Influence of Peer Evaluations on Attractiveness Judgments

In addition to demonstrating indirect contextual influences on judgments of attractiveness, the results of the third study indicated that information regarding peer judgments will influence evaluations of physical beauty. Subjects' judgments tended to conform to evaluative comments expressed by confederates in this study. Since subjects' judgments were private in this case, these results would seem to be an instance of what Deutsch and Gerard (1955) have referred to as "informational" as opposed to "normative" social influence. This latter effect would also seem to have clear "real-life" analogs. It has been noted that initial preinteraction encounters are strongly influenced by visual characteristics (Levinger, 1974), and very often such first visual encounters are accompanied by friends' explicit evaluations of the target individual's attractiveness. In fact, students can often be observed actively seeking peer evaluations of the attractiveness of potential romantic partners with whom relationships have progressed to what Levinger (1974) has termed the level of "surface contact."

The results of Study 3 showed only a direct effect of confederates' comments on physical attractiveness judgments, whereas general "standards" for dating desirability were not influenced by these comments. It is possible that long-term exposure to peers with either very high or very low "standards" would result in a more general effect, but the present methodology (in which only two females were evaluated by the peer models) does not allow for any such determination.

Influence of Media Depictions of Males on Females' Judgment

The present series of studies used only female target persons. Although physical attractiveness has generally been found to be more important for females than for males (e.g., Berscheid et al., 1971; Efran, 1974; Stroebe et al., 1971; Walster et al., 1966), attractiveness has not been found to be insignificant for men, by any means. These same studies, for instance, have shown physical attractiveness to be significant in importance for males as well, and Berscheid and Walster (1974) point out that although females consistently *report* physical attractiveness to be less important in their judgments of males, the findings with regard to *behavioral* measures are sometimes contradictory (e.g., Byrne et al., 1970). It seems likely that the present findings would have similar implications for judgments of males, although this remains to be empirically verified.

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12

The Effect of Attitude on the Recall of Personal Histories

Michael Ross, Cathy McFarland, and Garth J. O. Fletcher

Two studies were conducted to examine the relation between attitudes and the recall of past behaviors. The same paradigm was utilized in both experiments. First, subjects' attitudes on an issue were manipulated. Subjects were then asked to recall past behaviors relevant to the newly formed attitude. The results indicated that attitudes can exert a directive influence on recall of personal histories. The implications of the results for investigations of the relations between attitudes, behavior, and recall are discussed.

The concept of attitude has had a variable status in social psychology. In one famous quote, attitude was described as the "primary building stone in the edifice of social psychology" (Allport, 1968, p. 63). In recent years, however, some social psychologists have been more inclined to agree with Wicker's (1971) suggestion that "it may be desirable to abandon the attitude concept" (p. 29). This disenchantment stems from the evidence that attitudes fail to predict behavior in a variety of circumstances (Calder and Ross, 1973; Wicker, 1969). Although this view is overly pessimistic (Ajzen and Fishbein, 1977; Fazio and Zanna, 1981), it seems clear that attitudes play a limited role in the etiology of many behaviors.

The importance of attitudes is not circumscribed by their role in predicting behavior, however. The function of attitudes examined in the present research was suggested by Bartlett (1932) in the context of his work on recall of pictures and prose passages. Bartlett proposed that attitudes influence memory: "Recall is . . . a

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construction, made largely on the basis of an attitude, and its general effect is that of a justification of the attitude" (p. 207). Bartlett deduced this from the content of subjects' recall and their self-reports of how they prodded their memories.

Following Bartlett, there has been a considerable amount of research relating cognitive schemata to recall that suggests, albeit indirectly, that attitudes might be expected to influence memory. This research demonstrates that recall can be biased by evaluations and impressions. For example, in a study testing Heider's (1958) balance theory, Zajonc and Burnstein (1965) found that errors in recall of hypothetical relationships tended to be in the direction of producing cognitively balanced rather than imbalanced structures. Also, memory of the characteristics of hypothetical individuals has been shown to be influenced by impressions of the target people formed after the characteristics have been described (Lingle, Geva, Ostrom, Leippe, and Baumgardner, 1979; Snyder and Uranowitz, 1978).

Research on the selective learning hypothesis is more directly relevant to the present concerns. This research has provided less compelling evidence for a relation between attitudes and recall. Levine and Murphy (1943) described the selective learning hypothesis as follows: An individual "notes and remembers material which supports his social attitudes better than material which conflicts with these attitudes" (p. 515). The paradigm utilized by Levine and Murphy and subsequent researchers consists of exposing subjects to material that is consistent or inconsistent with their attitudes and asking them to reproduce the material at a later time. Although early studies provided support for the hypothesis (e.g., Jones and Kohler, 1958; Levine and Murphy, 1943), subsequent research has often yielded inconsistent or negative results (e.g., Greenwald and Sakumura, 1967). There is some recent evidence that the selective learning hypothesis may hold only for certain kinds of people (Olson, 1980).

It is reasonable to expect that attitudes will at times affect the recall of prose passages or opinion statements. Anyone who has examined the complicated and esoteric passages employed by Bartlett, for example, will not be surprised to learn that subjects forgot the details quickly and reconstructed them using vague residual feelings or attitudes as guides.

For the social psychologist, however, a more interesting question concerns whether attitudes guide or shape the recall of events that occur in everyday life. Accordingly, in the present research we examined whether attitudes affect the recall of personal histories. We have been unable to locate any previous research that attempts to show that people's attitudes shape their memories of their own past behaviors. A possible cause of this neglect is that there are a number of reasons to believe that people may not base their recall on their attitudes. For example, many of our actions are well practiced, even routinized. As a result, we may be able to recall them without difficulty, not necessarily by remembering each instance but simply by remembering that the activities are performed habitually. Second, actions occur in social and environmental contexts that limit severely the range of potential behaviors. Thus, remembering the context may serve to evoke the behavior enacted. Third, many attitudes are related in a loose and multifaceted fashion to a network of behaviors rather than to one specific behavior (Fishbein and Ajzen,

1974). Moreover, a number of different attitudes may relate to the same behavior, and, alternatively, some behaviors may not be connected to any salient attitudes. In sum, attitudes often may appear to be indirect and unnecessary vehicles for recalling past actions.

With these concerns in mind, the following question was examined in the present research: Can attitudes be shown to exert a directive influence on individuals' recall of their past behaviors when the conditions are optimal for demonstrating such an effect? To answer this question, we changed subjects' attitudes on an issue. We then measured recall of past behaviors relevant to the newly formed attitudes.

There were a number of considerations that guided our selection of attitude issues and experimental design. First, for the hypothesis to receive a fair test, it is necessary to produce quite sizable shifts in attitude. It would hardly be surprising if recall of past behaviors were unaffected by a minor change in attitude. McGuire (1964) has shown that beliefs that are widely shared and rarely questioned ("cultural truisms") are vulnerable to attack. For this reason, we chose to manipulate attitudes on two issues of this nature. Second, we selected issues for which we could identify unambiguously relevant and specifiable behaviors. Third, we incorporated procedures that were intended to reduce the possible experimental demands inherent in asking subjects to recall actions that may be related to their attitudes. Thus, we manipulated but did not assess the attitudes of subjects who answered the behavior recall questions. This procedure was designed to decrease the likelihood that subjects would view the experiment as a study of the relation between attitudes and recall. Also, the behavior recall items were imbedded in a lengthy questionnaire that was administered in a "second experiment." This second study was allegedly unrelated to the first one, in which attitudes were manipulated. Finally, to verify that the attitude manipulation was indeed effective, we ran an additional group of subjects whose attitudes were measured in the absence of behavior recall.

In summary, the purpose of the present study was to examine the effects of attitudes on the recall of personal histories. Would recall be biased by subjects' attitudes?

EXPERIMENT 1

Method

Subjects. The subjects were 54 males and 47 females recruited from the voluntary subject pool at the University of Waterloo. Thirty-five of the subjects completed the questionnaire that assessed attitudes, and 66 of the subjects completed the questionnaire that assessed behavior recall. The data from 4 subjects were eliminated from the analysis. Three of these subjects were suspicious of the link between the two studies, and the other subject did not follow the instructions correctly in filling out the questionnaire. Of the 3 suspicious subjects, 1 was in the negative toothbrushing condition, attitude assessment; 1 was in the negative

toothbrushing condition, recall assessment; and 1 was in the positive bathing condition, recall assessment.

Procedure. A pretest was conducted to select the attitude issues for the study. Twenty-eight students were stopped on campus and asked for their opinions on a series of health-related issues; opinions were assessed on a 7-point scale (1 = strongly disagree; 7 = strongly agree). On the basis of this pretest, two items that showed reasonably high agreement were selected for the experiment: "It is important for health reasons to shower or bathe at least once a day" (M = 5.41, SD = 1.07), and "It is important for proper dental care to brush one's teeth after every meal" (M = 5.83, SD = 1.34). Toothbrushing was one of the cultural truisms employed by McGuire (1964).

In the experiment proper, some subjects were exposed to a communication that was either favorable or unfavorable toward toothbrushing; the remaining subjects were exposed to a communication that was either favorable or unfavorable toward bathing frequently. In the context of a second experiment, subjects were then asked to indicate either their attitudes on the relevant issues (toothbrushing and bathing) or to indicate how often they had brushed their teeth and bathed in the preceding 2 weeks.

Subjects were contacted for the experiment by telephone. The caller identified herself as a secretary for the psychology department. Subjects were informed that they would participate in two brief studies that were to be conducted one after the other due to scheduling ease. Subjects were told that the first study examined factors influencing the effectiveness of communications and that the second study concerned the relation of personality type to life-style.

Upon arrival at the first experiment, subjects were taken to a room in groups of 2 to 5 and told that they would be listening to tape recordings of a university radio program dealing with current health problems. They were informed that this show typically consisted of a brief interview with a medical or social scientist.

The subjects were told that they would listen to two of these interviews and write down the most important points. They were informed that the experimenter wanted to determine if there was agreement among nonexperts as to which were the key points. They would also be asked to generate an argument of their own that was consistent with the material being presented. They were told that their judgments and arguments would be used in the construction of health appeals that would eventually be presented to the Canadian public. They were asked to not put their names on their answer sheets; their responses were to be anonymous.

Subjects were taken to individual rooms to listen to the tape recordings through headphones. All subjects listened first to a filler tape arguing for higher seat belt usage in automobiles and then to either the positive or negative toothbrushing tape or the positive or negative bathing tape.

The interviews on the issues of toothbrushing and bathing were allegedly with officials of the Canadian Dental Association and the Canadian Medical Association, respectively. In the positive toothbrushing condition, the official described

how frequent brushing strengthened tooth enamel and protected the teeth and gums against disease. In the negative toothbrushing condition, the same official argued the dangers of frequent brushing: Abrasive toothpastes cause erosion of the enamel, and frequent brushing may harm the gums, leading to infection and tooth loss. Further, it was emphasized that flossing was much superior to toothbrushing.

In the positive bathing condition, the official suggested that infrequent bathing can create skin complaints and that body odor can only be eliminated by frequent use of soap and water. In the negative bathing condition, he stated that frequent bathing removes a layer of skin that protects one against infection and that it also causes increased oil production in the skin, which can cause skin complaints.

The seat belt interview was allegedly with a representative of the Ontario Government Safety Division who argued for increased seat belt usage. We presented the seat belt tape to obscure the connection between the two studies. In the second study, there were no items on the questionnaire that pertained to the seat belt issue.

After listening to the tapes and making their evaluations, subjects were thanked and dismissed. They returned to the waiting area and were contacted by the experimenter for the second study. This experimenter took subjects to a large "survey research" room. They were told that they would anonymously complete a questionnaire designed to assess the relation between personality type and lifestyle. They were informed that previous studies relating personality to life-style had focused on the abnormal personality and the everyday activities he or she engaged in. The present study would be concerned with this relationship in normal individuals.

The behavior recall groups responded to a version of the questionnaire that contained questions asking them to estimate the frequency with which they had engaged in various activities during the past 2 weeks. The target questions, which were imbedded among many other health-related items, were "How many times have you brushed your teeth in the past two weeks? (a) after breakfast, (b) after lunch, (c) after dinner," and "How many baths (or showers) have you taken in the past 2 weeks? (a) in the morning, (b) between 10 A.M. and 6 P.M., (c) in the evening."

The attitude assessment groups responded to a questionnaire that asked them to give their opinions of the same everyday activities that appeared in the recall version of the questionnaire. Three 9-point semantic differential scales were used to assess attitudes on the two target issues. The items were worded as follows: "Brushing your teeth after every meal is: important–unimportant, healthy–unhealthy, beneficial–harmful," and "Taking showers and baths often is: healthy–unhealthy, hygienic–unhygienic, beneficial–harmful."

After answering either the recall or opinion questions, all subjects filled out the personality section of the questionnaire. The personality test was Snyder's (1974) self-monitoring scale. Finally, subjects were probed for suspiciousness. They anonymously completed a questionnaire that required them to describe the purposes of the study and to discuss any confusing or odd aspects of the procedures. The experimenter also asked subjects directly if they had noticed a connection be-

tween the two studies. Subjects were informed of the purposes of the experiment and fully debriefed following the suspiciousness probe.

Results

Attitude change. The three semantic differentials for each issue were summed, with higher scores reflecting more negative attitudes. All of the subjects responded to both the toothbrushing and bathing items on their attitude questionnaires. Accordingly, subjects who listened to the persuasive message on toothbrushing served as controls for subjects who heard the bathing message, and vice versa. The mean attitude in each condition is shown in Table 12.1. Subjects who listened to the positive messages were substantially more favorable toward toothbrushing and bathing than were subjects who heard the negative messages: For toothbrushing, $t(7.36) = 3.46$, $p < .002$; for bathing, $t(8.03) = 5.23$, $p < .001$.[1]

The attitudes of control subjects were similar to those of the pretest group, which were very positive. Not surprisingly, therefore, it was the negative message that exerted the stronger influence on attitudes. Subjects who heard the messages derogating toothbrushing and bathing held more negative attitudes toward these activities than did control subjects: For toothbrushing, $t(7.45) = 3.25$, $p < .01$; for bathing, $t(22) = 3.34$, $p < .003$. The tendency for subjects who heard the positive message to hold more favorable attitudes than control subjects approached significance on the bathing message only, $t(19.48) = 2.07$, $p < .052$.

TABLE 12.1
Mean attitude and behavior recall scores for Experiment 1

	Toothbrushing			Bathing		
	Positive message	Control	Negative message	Positive message	Control	Negative message
Attitude						
M	3.62	4.17	12.75	3.70	5.87	11.63
n	8	18	8	10	16	8
Behavior recall						
M	35.85	32.8	28.62	11.06	11.35	10.89
n	13	36[a]	13	18	26	19

Note: The higher the attitude score, the more negative the attitude. The behavior recall scores indicate the number of times subjects reported brushing their teeth or bathing in the preceding 2 weeks.

[a]The *n* in this condition is 36 rather than 37, because one of the subjects failed to answer the relevant control question.

[1]The degrees of freedom used were fractional, because the variances were heterogeneous and a separate variance *t* value was calculated. This procedure is described in Myers (1979).

Behavior recall. Subjects who heard a message derogating toothbrushing reported brushing their teeth significantly fewer times in the preceding 2 weeks than did subjects who heard the positive message, $t(24) = 2.16$, $p < .05$ (means are shown in Table 12.1). However, the difference between the negative and positive bathing groups was slight and nonsignificant ($t < 1$).[2]

Once again, subjects who listened to the persuasive message on one of the target issues could serve as controls for subjects who heard the persuasive message on the other issue. The control mean was intermediate to and nonsignificantly different from the mean behavior recall scores of subjects who heard the positive, $t(36.4) = 1.20$, $p < .24$, and negative, $t(47) = 1.20$, $p < .24$, toothbrushing messages. The control mean was virtually identical to the means obtained from subjects who heard the positive and negative bathing messages (ts < 1).

Discussion

Relatively large shifts in attitudes toward toothbrushing and bathing were obtained. Behavior recall appeared to be biased by attitudes only on the toothbrushing issue, however.

There are a number of differences between the two types of events that may account for the discrepant behavior recall results. Bathing takes longer than toothbrushing and constitutes a more complex series of behaviors. Bathing is also usually carried out no more than once per day. Perhaps for these reasons, bathing is a more vivid and memorable event than toothbrushing and therefore less prone to memory distortion over relatively short periods.

On this basis, it appears plausible to suggest that a longer period than 2 weeks may be necessary for changes in attitudes toward bathing to significantly influence behavior recall. To test this hypothesis, the bathing recall condition in the first experiment was replicated in Experiment 2, with subjects being asked to recall the number of times they had bathed or showered in the previous month rather than in the preceding 2 weeks.

EXPERIMENT 2

Method

Subjects. The subjects were 12 females and 30 males obtained from the voluntary subject pool at the University of Waterloo. Four subjects (3 in the negative bathing condition and 1 in the positive bathing condition) were suspicious of the link between the two experiments; their data were therefore excluded from the analysis.

[2]The results are not altered when the data from the suspicious subjects are included in the analysis.

Procedure. The positive and negative bathing tapes were identical to those used in the previous experiment. The bathing recall question was altered; subjects were asked how many baths or showers they had taken over the last month. The filler items in the questionnaire were also changed to assess recall over a 1-month period.

Results

Subjects who were exposed to the negative bathing message reported bathing fewer times in the preceding month ($M = 17.15$) than did subjects who were exposed to the positive message ($M = 25.33$), $t(36) = 2.51$, $p < .02$. Thus, the shift from a 2-week to a 1-month recall period produced the hypothesized effect (see Footnote 2). Because only one issue was used in this study, no control comparisons were derived.

SELF-MONITORING AND THE RELATION BETWEEN ATTITUDES AND RECALL

The self-monitoring scale was included largely to provide subjects with a rationale for distinguishing the second part of each study from the first part. Based on Snyder's (1979) characterization of low and high self-monitors, however, one might expect low self-monitors in particular to show the assimilation of behavior recall to current attitudes. As the behaviors of low self-monitors generally correspond to their attitudes, their attitudes may be a useful cue by which to recall their behaviors. The attitudes of high self-monitors should not serve this function to the same degree, because their behavior tends to be guided by the situation rather than by their attitudes (Snyder, 1979).

A median split on the self-monitoring dimension produced some support for this reasoning in both experiments. In the first experiment, low self-monitors who received the negative toothbrushing message ($M = 26.43$) reported brushing their teeth significantly fewer times than low self-monitors who received the positive toothbrushing message ($M = 38.00$), $t(22) = 2.29$, $p < .05$. This contrast was not significant among high self-monitors (for negative toothbrushing, $M = 31.1$; for positive toothbrushing, $M = 34.5$; $t < 1$). Similarly, in the second experiment, low self-monitors who received the negative bathing message ($M = 14.20$) reported bathing fewer times than low self-monitors who received the positive bathing message ($M = 27.40$), $t(27) = 2.28$, $p < .05$. High self-monitors who received the negative bathing message ($M = 19.20$) did not differ significantly in their recall from high self-monitors who received the positive bathing message ($M = 25.50$; $t = 1.16$, $p < .30$).[3]

[3]Note that there was no relation between self-monitoring and amount of attitude change (for bathing, $F = 1.69$, *ns;* for toothbrushing, $F < 1$) and that results from the control conditions revealed that high and low self-monitors did not differ in the frequency with which they reported brushing their teeth or bathing in the absence of persuasive messages ($ts < 1$).

These data should be considered with caution, however. The two-way interaction between self-monitoring and the direction of the persuasive message was not statistically significant in either experiment, $F(1, 22) = 1.44$, $p < .25$ (Experiment 1), and $F(1, 27) < 1$ (Experiment 2). Also, the cell *n*s were low, ranging from 5 to 10 subjects. The pattern is intriguing, though, and replication with higher *n*s seems warranted.

GENERAL DISCUSSION

The results indicate that attitudes can exert a directive influence on recall of personal histories under carefully controlled and specifiable conditions. Presumably, the attitude serves as a retrieval cue. Individuals either reconstruct their actions in light of the attitude, or they focus on the subset of their behaviors that is consistent with the attitude. The present research does not reveal whether this effect on retrieval can be explained purely in information processing terms (different cues, i.e., attitudes, prompting different retrieval) or whether motivational concerns need be posited. (Attitude-consistent material arouses positive affect, and attitude-inconsistent material evokes negative affect, resulting in selective attention to attitude-consistent material.) The quote from Bartlett presented earlier allows for both possibilities, which seems to be the most reasonable hypothesis at the moment.

The data also reveal that attitudes do not always affect the recall of past behaviors, even within the limits of our experimental paradigm. The results for the recall of bathing behavior in Experiment 1 suggest that attitudes may not influence reconstructive memory when the behavior is vivid or readily recalled. Note that this lack of effect argues against simple demand and self-presentation interpretations of the significant results. If subjects had formulated the experimental hypothesis and were responding to confirm it, or if subjects were responding on the behavior recall assessment to present themselves in a positive way, they were certainly free to do so on the 2-week bathing measure used in Experiment 1. Yet they did not.

There are two other reasons for disputing a self-presentation interpretation of the data. First, the anonymity of subjects' responses and the separation of the attitude and behavior recall portions of the study should have attenuated concerns for self-presentation. Second, Snyder's (1979) characterization of the self-monitoring dimension suggests that high self-monitors are more concerned about self-presentation than are low self-monitors. Thus, the impact of attitude on behavior should be greater with high self-monitors, assuming that the effect is mediated by a concern for self-presentation. If anything, though, the effect was stronger among low self-monitors, a result that is congruent with the hypothesis that attitudes rather than concerns for self-presentation guide recall.

The current research has shown that attitudes affect recall, whereas much of the research on selective learning has failed to demonstrate such selectivity (e.g., Greenwald and Sakumura, 1967). There are differences between the paradigms that could contribute to the discrepant results. In the typical selective learning

study, subjects are provided with arguments on both sides of an issue. Although it might be expected that subjects will remember arguments consistent with their attitudes better than arguments inconsistent with their attitudes, the procedure introduces a number of factors that may influence memory and obscure the relation between attitudes and recall. For example, the relative familiarity or novelty of the arguments may be related to the subject's own position on the issue. For many subjects, the arguments that contradict their attitudes may be more novel and elicit greater attention than the arguments that support their attitudes (Greenwald and Sakumura, 1967). Because both attention and familiarity should enhance memory, there may not be a difference in recall between the familiar supporting arguments and the novel opposing arguments.

The current paradigm sidesteps this difficulty. The source of recall is the subject's personal history rather than material containing novel arguments provided by the experimenter. Furthermore, an inconsistency between attitudes and own behaviors may arouse more negative affect and, hence, greater motivation to reduce the disparity than an inconsistency between attitudes and information provided by an experimenter.

A series of studies by Feather (1969a, 1969b, 1969c) and a recent experiment by Weldon and Malpass (1980) offer some support for these contentions. Feather simply asked subjects to list the arguments pertinent to an issue. Subjects were able to report more arguments that supported their position than arguments that contradicted their position. Thus, selectivity is evidenced when recall is based on personal experience rather than on experimentally contrived materials. In addition, Weldon and Malpass demonstrated in their study that attitudes are related to recall in a standard selective learning paradigm when factors such as familiarity with the arguments and intellectual skills are controlled for.

The current results complement those of past research on attitudes, behaviors, and recall of behaviors. Previous studies have shown that (a) attitudes can, at times, direct behavior (Ajzen and Fishbein, 1977; Fazio and Zanna, 1978), (b) behavior can affect attitudes (Bem, 1972), (c) current behaviors can affect the recall of past attitudes (Bem and McConnell, 1970), and (d) current attitudes are affected by the selective recall of past behaviors (Salancik, 1974). The present study adds another link. Attitudes can influence the recall of past behaviors.

It is intriguing to speculate about the possible associations among these causal sequences. For example, the results of the present experiment have methodological implications for the study of attitude–behavior relations. In some investigations of the effects of attitudes on behavior, subjects' recall of actions relevant to the attitude being investigated serves as the measure of behavior (e.g., Fishbein and Ajzen, 1974; Kahle and Berman, 1979; Ostrom, 1969). Our research suggests that behavioral recall may be biased by attitudes; as a result, the obtained degree of attitude–behavior consistency in such studies may be inflated spuriously (i.e., attitudes may exert a stronger influence over behavior recall than over behavior). The possibility of such a confound provides one more reason why actual behavior should be examined in studies of this kind, though we appreciate how difficult an investigative imperative this can be.

In addition, it is plausible to postulate a feedback loop between behavior recall and attitude. The present research indicates that attitudes may bias the recall of past behaviors. It follows that a review of previous behavior may lead individuals to conclude (perhaps inappropriately) that their past actions are consistent with their current attitudes and, hence, increase the confidence with which current attitudes are held.

The ramifications may not stop at this point, however. Fazio and Zanna (1981) have shown that the more confident individuals are of their attitudes, the better these attitudes predict future behavior. Thus, much like a self-fulfilling prophecy, the process of behavior review may increase the control that attitude exerts over future behavior and produce a genuine increase in attitude–behavior consistency.

In a more general vein, the current data support Greenwald's (1980) characterization of the self or ego as analogous to a totalitarian political regime in which history is revised and fabricated to suit present concerns. Individuals appear to be revisionist historians with respect to their personal memories.

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13

The Peculiar Longevity of Things Not So Bad

Daniel T. Gilbert, Matthew D. Lieberman, Carey K. Morewedge, and Timothy D. Wilson

Intense hedonic states trigger psychological processes that are designed to attenuate them, and thus intense states may abate more quickly than mild states. Because people are unaware of these psychological processes, they may mistakenly expect intense states to last longer than mild ones. In Study 1, participants predicted that the more they initially disliked a transgressor, the longer their dislike would last. In Study 2, participants predicted that their dislike for a transgressor who hurt them a lot would last longer than their dislike for a transgressor who hurt them a little, but precisely the opposite was the case. In Study 3, participants predicted that their dislike for a transgressor who hurt them a lot would last longer than their dislike for a transgressor who hurt someone else a lot, but precisely the opposite was the case. These errors of prediction are discussed as instances of a more general phenomenon known as the region-β paradox.

She will be more hurt by it, for Robert always was her favourite. She will be more hurt by it, and on the same principle, will forgive him much sooner.

—Jane Austen, *Sense and Sensibility* (1811/1996, p. 35)

In just less than 3 million years, the human brain nearly tripled in size, in large part because of the dramatic growth of the frontal lobe and its prefrontal cortex. This structure endowed human beings with unprecedented cognitive capacities, none of which was more important than the ability to travel mentally forward in

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time and preview the future (Banyas, 1999; Melges, 1982; Wheeler, Stuss, and Tulving, 1997). The ability to peer deeply into their own tomorrows meant that people could discover the consequences of an event without actually having to experience it. Modern human beings can envision different futures, envision their hedonic impacts, and then act to bring about those futures they deem most desirable and avoid those they deem dangerous, unprofitable, or upsetting. Whereas other animals learn by making mistakes or watching others make them, human beings use their powers of affective forecasting to avoid mistakes that no one has ever made before.

Of course, even the most powerful adaptations have their limits, and research suggests that people often err where attempting to forecast their hedonic reactions to future events ranging from romantic breakups to serious illnesses (for reviews, see Frederick and Loewenstein, 1999; Gilbert, Driver-Linn, and Wilson, 2002; Lowenstein and Schkade, 1999; Wilson and Gilbert, 2003). Moreover, people make these errors even when they accurately anticipate the time, place, and manner in which an event will unfold. How can people be right about what will happen but wrong about how much they will like it when it does? One explanation is that intense hedonic states trigger a variety of processes that are designed to attenuate them (Taylor, 1991; Wilson, Gilbert and Centerbar 2002), ranging from the homeostatic processes that diminish a state's physiological impact (Sandvik, Diener, and Larsen, 1985; Solomon, 1980) to the defensive processes that diminish a state's physiological impact (Freud, 1937; Gross, 1999; Lazarus and Alfert, 1964; Taylor, 1991; Taylor and Brown, 1988). People tend to overestimate the power of these attenuating processes (Kahneman and Snell, 1992; Lieberman, Ochsner, Gilbert, and Schacter, 2001; Snell, Gibbs, and Varey, 1995), and thus they tend to overestimate the duration of their hedonic states (Gilbert, Pinel, Wilson, Blumberg, and Wheatley, 1998). This oversight can have a variety of paradoxical consequences, one of which we explore in this article.

The Region-β Paradox

When a spoon falls off a table, any first-year physics student can calculate how long it will take to hit the floor. Because no invisible processes inside the spoon are actively working to speed it up or slow it down, the duration of its descent depends entirely on its initial position. The higher the table from which the spoon falls, the longer it takes to hit the floor—and it can never take longer for a spoon to fall from a low table than from a high one. In other words, for objects that do not actively respond to their circumstances, the relation between time and distance is strictly monotonic. In contrast, for objects that do actively respond to their circumstances, the relation between time and distance can become briefly nonmonotonic. For instance, people tend to use faster modes of transportation for longer distances: An urban commuter may adopt the habit of walking to destinations

within a mile of her origin and bicycling to more distant destinations. The paradoxical result of actively changing one's mode of travel when the journey exceeds a critical distance is that one will occasionally arrive at a distant destination more quickly than a near destination, briefly reversing the normal relation between time and distance.

Figure 13.1 illustrates this paradox. Notice that although time and distance are monotonically related within each of the three regions labeled α, β, and γ, they are nonmonotonically related across regions. That is, it always takes the commuter more time to go to a distant point than to a close point in region β than it does to reach the farthest point in region α. Anyone who has flown from New York to London more quickly than he or she could have driven from New York to Boston has experienced this *region-β paradox.*

This paradox applies not only to time and distance, but also to the intensity and duration of various states. For instance, one might expect the intensity of a physical injury to determine the duration of one's recovery. But just as travelers take active steps to speed their journeys when their destinations are especially distant, injured people take active steps to speed their recoveries when their injuries are especially severe. Injured people swallow medicines, consult physicians, solicit therapies, and undergo surgeries, but because each of these remedies has its costs, people are much more likely to seek them when they have a heart attack than when they have a hangnail. When people actively adapt their strategies for recovery to

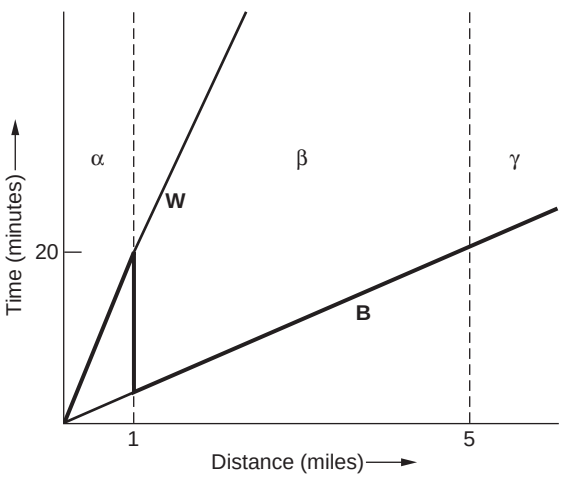

FIGURE 13.1
The region-β paradox. This example shows the relation between time and distance for a person who walks at 3 miles per hour (line W), a person who bicycles at 15 miles per hour (line B), and a person who walks at 3 miles per hour to destinations that are less than a mile away and bicycles at 15 miles per hour to destinations that are more than a mile away (bold line). For the latter person, it takes less time to reach any point in region β than to reach the shorter distances in region α.

the severity of their injuries, they may paradoxically recover more quickly from severe injuries than from mild ones. A trick knee hurts longer than a shattered patella because the latter injury exceeds the critical threshold for pain and thereby triggers the very processes that attenuate it.

The psychological processes that attenuate distress can also have costs (Lazarus, 1985; Pennebaker, 1989; Richards and Gross, 2000; Wegner, Erber, and Zanakos, 1993), and thus they tend to be triggered only when distress passes a critical threshold. People rationalize divorces, demotions, and diseases but not slow elevators and uninspired burgundies. The paradoxical consequence is that people may sometimes recover more quickly from truly distressing experiences than from slightly distressing ones (Aronson and Mills, 1958; Gerard and Mathewson, 1966; Zimbardo, 1966). A wife may do the costly cognitive work necessary to rationalize her husband's infidelity ("I guess men need to try this sort of thing once to get out of their systems") but not his annoying habits ("I guess men need to experiment with leaving their dirty dishes in the sink") and thus the wife's anger about her husband's disorderliness may outlive her anger about his philandering.

The Present Research

If people do not realize that the psychological processes that attenuate distress are triggered only when the severity of that distress passes a critical threshold, then they may mistakenly expect the longevity of distress to be a monotonic function of its initial intensity. In a sense, people may think of themselves as spoons. We sought to demonstrate that people do indeed expect intense distress to last longer than mild distress (Study 1), but that because of the region-β paradox, this expectation can be precisely wrong (Studies 2 and 3).

STUDY 1: PREDICTING DURATION FROM INTENSITY

Method

Fifty-seven male and 41 female students completed a questionnaire that asked them to imagine (a) that they asked someone for a date and were politely turned down, (b) that a person whom they had recently met failed to recognize them later, (c) that their roommate borrowed their boots without permission, (d) that their classmate failed to show up for a scheduled study session, (e) that an old friend of theirs joined a neo-Nazi group, (f) that they were denied use of a restaurant's restroom because they were not a customer, (g) that they caught someone trying to break into their gym locker, (h) that their best friend had a romantic encounter with a former girlfriend or boyfriend, and (i) that a careless driver dented their car in a parking lot and then sped away. Participants estimated the intensity of their initial

TABLE 13.1
Intensity and forecasted duration of disliking of transgressor in Study 1

Transgression	Immediate disliking (intensity)	Disliking 1 week later (duration)
Rejected	1.07 (1.37)	0.14 (1.28)
Not Recognized	1.10 (1.37)	0.35 (1.21)
Borrowed Boots	1.78 (1.64)	0.62 (1.93)
Stood up	1.94 (1.26)	0.07 (1.29)
Neo-Nazi	2.05 (1.68)	1.88 (1.75)
Restroom	2.46 (1.51)	1.13 (1.72)
Gym locker	2.97 (1.42)	2.13 (1.69)
Romantic betrayal	3.22 (1.47)	2.24 (2.25)
Dented car	3.47 (1.10)	2.51 (1.81)

Note: Standard deviations are in parentheses. The original scale values were recoded so that larger values indicate greater disliking of the transgressor.

reactions to the transgression by indicating how they thought they would feel about the transgressor "at the moment this happened," using a scale whose end points were labeled *dislike very much* (−4) and *like very much* (4). Participants estimated the duration of each reaction by indicating on the same scale how they thought they would feel about the transgressor "1 week later."[1]

Results

Table 13.1 shows the mean intensity and duration estimates for each transgression. The correlation between intensity and duration estimates across participants was significant, r = .88, $p < .01$, and there was no evidence of a curvilinear relationship between these variables (i.e., adding the square of intensity to a regression equation resulted in a trivial and nonsignificant increase in the amount of variance explained). We also correlated each participant's intensity estimates with that participant's duration estimates, thereby producing a correlation for each participant. (One participant who made the same numerical estimate for all nine duration questions was excluded because a correlation could not be computed.) The distribution of within-participants correlations had a strikingly negative skew (–1.34), with a mean of .65 (SD = .30) and a median of .75. In short, participants clearly expected their feelings at the time of the transgression to be a powerful predictor of their feelings a week later.

[1]There are many ways to measure the predicted duration of any event (such as an affective reaction). One way is simply to ask people how long they expect the event to endure, but research suggests that people have considerable difficulty making estimates in temporal units (Zakay and Block, 1997). Another way is to ask people to predict whether the event will or will not still be occurring at some point in the future, which is what we did. This method is conservative inasmuch as it risks Type II errors (e.g., two events may have different predicted durations and yet may both have abated by the particular time about which the experimenter has inquired).

STUDY 2: PARTNERS AND NONPARTNERS

People expect that the more intense their hedonic reactions are, the longer those reactions will last. But if the psychological mechanisms that attenuate such reactions are triggered only at critical levels of intensity, then the region-β paradox suggests that there should be instances in which more intense reactions abate more quickly than mild reactions. In Study 2, participants were insulted by a person with whom they expected to interact (a prospective partner) or with whom they expected not to interact (a prospective nonpartner). Because people trust their interaction partners to make special efforts to be polite (Brown and Levinson, 1987; Fraser, 1990), they should feel worse when insulted by a partner than by a nonpartner and thus should expect to dislike an insulting partner more than an insulting nonpartner over time. Yet, just as a severe injury triggers the actions that will attenuate it, the intense distress caused by a partner's insult should trigger the psychological processes that attenuate it (Darley and Berscheid, 1967; Finkel, Rusbult, Kumashiro, and Hannon, 2002). Therefore, we predicted that (a) people would initially feel more distressed when insulted by a partner than by a nonpartner, (b) they would therefore expect that a few minutes after receiving the insult they would dislike a partner more than a nonpartner, but (c) because intense distress triggers the processes that attenuate it, people would actually dislike an insulting nonpartner more than an insulting partner a few minutes after receiving the insult. We tested the first of these predictions in Study 2a and the second and third predictions in Study 2b.

Study 2a

Method. Twenty-one female and 5 male students completed a baseline measure of their hedonic state ("How are you feeling right now?") by drawing a slash at the appropriate locations on continuous 125-mm lines that corresponded to different emotions. Seven lines corresponded to specific emotions: excited, good, happy, hostile, insulted, proud, and upset. The end points of these lines were labeled with the phrases *not at all* and *extremely.* In addition, the end points of an eighth line were labeled with the phrases *extremely negative* and *extremely positive.*

Participants were then told that another participant (the rater) was already seated in an adjacent room, that they and the rater would each write an autobiographical story that the other would read, that on the basis of those stories they would assess each other's personalities, and that each would then learn how he or she had been assessed. Participants in the *partner* condition were told that they would ultimately meet the rater and discuss their experiences in the experiment, whereas participants in the *nonpartner* condition were told that they would never meet the rater. In fact, there was no rater.

After participants wrote their autobiographical stories, the experimenter gave them a handwritten story that had ostensibly been written by the rater and a written description of three personality types (adapted from Gilbert et al., 1998) that differed in their positivity. Participants indicated which of the personality types best described the rater and reported their confidence in that assessment on a 125-mm continuous line whose end points were labeled with the phrases *not at all* and *extremely.* Next, participants estimated the extent to which their assessment had been influenced by several factors (e.g., their current mood, the rater's handwriting) and estimated which of the personality profiles best described them.

Next, participants were given a handwritten sheet indicating that the rater had assessed them with relatively high confidence as the worst of the three personality types. After placing the sheet on the desk, the experimenter asked participants to complete the same scales they had completed at baseline.

Results.[2] We expected participants to feel more distressed when insulted by a partner than by a nonpartner. We averaged separately the eight measures taken at baseline ($\alpha = .82$) and the eight measures taken after participants received the insult ($\alpha = .81$) and then subtracted one average from the other to examine changes in the participants' hedonic states. As predicted, participants experienced a greater change in their hedonic state when insulted by a partner ($M = -13.5$), $t(24) = 2.075$, $p < .05$. In Study 2b, we examined how participants thought they would feel about the rater and actually felt about the rater 5 minutes after being insulted.

Study 2b

Method. Twelve male and 17 female students were randomly assigned to play the role of an *experiencer* or a *forecaster* who was insulted by a partner or a nonpartner.

The procedure for experiencers was identical to the procedure used in Study 2a with two exceptions. First, participants made no ratings of their hedonic states. Second, after receiving the insult, experiencers were left alone in their cubicles while the experimenter ostensibly went to make a photocopy. The experimenter returned 5 minutes later and asked experiencers to report how they felt about the rater on a scale whose end points were labeled with the words *negatively* (1) and *positively* (7).

The procedure for forecasters was identical to the procedure for experiencers with one exception. Instead of actually receiving an insult, forecasters were asked to estimate how they would feel about the rater 5 minutes after learning that the rater had assessed them with relatively high confidence as the worst of the three

[2]Participants' assessments of themselves and the rater were entirely unremarkable and showed no interactions in this and the subsequent studies; thus, for the sake of brevity, we do not discuss these data.

TABLE 13.2
Liking of rater in Study 2b

Participant's role and statistic	Rater's Role		Difference[a]
	Partner	Nonpartner	
Forecaster			
Mean liking	2.83	4.57	−1.74*
SD	1.17	1.51	
n	7	7	
Contrast weight	−1	1	
Experiencer			
Mean liking	4.57	3.33	1.24*
SD	0.78	1.30	
n	7	8	
Contrast weight	1	−1	

Note: Higher values indicate greater liking
[a]An asterisk (*) indicates that the difference between cells is different from zero, $p < .05$

personality types. These ratings were made on a scale whose end points were labeled with the words *negatively* (1) and *positively* (7).[3]

Results. We expected the forecasters would predict that they would like a partner less than a nonpartner 5 minutes after being insulted, but that experiencers would report feeling precisely the opposite. A weighted contrast analysis (as recommended by Rosenthal and Rosnow, 1985) confirmed this prediction, $t(27) = 3.17$, $p < .005$. As Table 13.2 shows, the rater's role had opposite effects on forecasters' predictions and experiencers' reports. Although participants expected to dislike a partner more than a nonpartner 5 minutes after being insulted, they actually disliked the partner less than the nonpartner.

STUDY 3: VICTIMS AND BYSTANDERS

Participants experienced more intense distress when insulted by a partner than when insulted by a nonpartner (Study 2a), and yet, contrary to their own predictions, they ended up liking an insulting partner more than an insulting nonpartner (Study 2b). Presumably this happened because intense distress triggered the psychological processes that attenuated it. This reasoning makes a counterintuitive prediction. If victims of insults experience more intense distress than do bystanders, then the psychological processes that attenuate distress may be more likely to be triggered for victims than for bystanders. Therefore, after a few minutes, victims may actually like a person who insults them more than bystanders do!

[3]To avoid creating suspicion, we then asked forecasters to predict how they would feel about the rater if the rater assessed them as the other two personality types.

If victims are unaware of the psychological processes that will attenuate their distress, then they should expect precisely the opposite. Study 3 tested this prediction.

Method

Sixteen male and 42 female students were randomly assigned to play the roles of victim or bystander and of experiencer or forecaster.

Victims. Victims were randomly assigned to play the role of experiencer or forecaster. The procedure for victims was identical to the procedure used for experiencers and forecasters in the partner condition of Study 2b.

Bystanders. Bystanders were told (a) that the rater and the victim were reading each other's stories and would soon be assessing each other's personalities, (b) that the victim and the rater would later interact and that both of them knew this, and (c) that the bystanders themselves would later interact with the victim but not the rater. Bystanders read the three personality profiles used in Study 2 and were then given the handwritten story by the victim. Bystanders were also given a handwritten story that had ostensibly been written by the rater but that had actually been written by a participant in the partner condition of Study 2b. (A different story was randomly selected for each bystander.) Next, bystanders were shown the victim's assessment of the rater, as well as the victim's self-assessment. Thus, bystanders had precisely the same information that victims had.

Each bystander in the *experiencer* condition was given a handwritten sheet indicating that the rater had assessed the victim with relatively high confidence as the worst of the three personality types. After placing the sheet on the desk, the experimenter explained that he needed to print a copy of the next questionnaire and left the bystander alone in the cubicle. The experimenter returned 5 minutes later and asked the bystander to report how he or she felt about the rater by marking a scale whose end points were labeled with the words *negatively* (1) and *positively* (7).

Bystanders in the *forecaster* condition were told that before they learned how the rater had assessed the victim, they should estimate how they would feel about the rater 5 minutes after learning that the rater had assessed the victim with relatively high confidence as the worst of the three personality types. Forecasters made this rating on a scale whose end points were labeled with the words *negatively* (1) and *positively* (7).

Results

We expected that forecasters would predict that they would dislike an insulting partner more when they were the victim of the insult than when they were a bystander to it, but that experiencers would report feeling precisely the opposite. A

TABLE 13.3
Liking of rater in Study 3

Participant's role and statistic	Participant's status		
	Victim	Bystander	Difference[a]
Forecaster			
Mean liking	3.15	3.83	−0.68*
SD	0.88	1.27	
n	20	12	
Contrast weight	−1	1	
Experiencer			
Mean liking	4.33	3.36	0.97**
SD	1.05	0.92	
n	15	11	
Contrast weight	1	−1	

Note: Higher values indicate greater liking
[a]Asterisks indicate that the difference between cells is different from zero: *$p < .10$; **$p < .05$.

weighted contrast analysis confirmed this prediction, $t(56) = 3.24$, $p < .01$. As Table 13.3 shows, the participant's status had precisely opposite effects on forecasters' predictions and experiencers' reports. Although participants expected to dislike an insulting partner more when they were victims than when they were bystanders, they actually disliked the partner less when they were victims than when they were bystanders.

GENERAL DISCUSSION

When Ovid wrote two millennia ago that "small things affect small minds," he was apparently unaware that when small things fail to trigger one's defenses, they may attain a peculiar longevity that even great minds do not anticipate. Contrary to their own predictions, participants in our studies disliked least who hurt them most. This paradox arises because intense hedonic states are especially likely to trigger the psychological processes that attenuate them. Because people are unaware of these processes, they mistakenly expect more intense states to last longer than less intense states.

It is easy to imagine how such errors of prediction could become errors of action. An employee who is told that he must either relinquish a file cabinet or move to a smaller office may be correct in believing that the minor inconvenience of losing some furniture will be less distressing than the major inconvenience of moving to smaller quarters. What he may fail to realize, however, is that the major inconvenience may be so distressing that it will trigger the psychological processes that attenuate it ("Now that I'm downstairs, I realize how much I like being close to the coffee machine"), whereas the minor inconvenience may not be quite distressing enough to trigger such processes ("Now that I have to stack my files

on the floor, I realize how much I hate my boss"). Therefore, the employee may choose the option that is initially less distressing but that is ultimately less satisfying. The present studies join others (e.g., Gilbert and Ebert, 2002) in suggesting that when people make decisions without regard for the psychological processes that different outcomes will trigger, they may do so at the expense of their ultimate satisfaction.

The region-β paradox provides a simple, unifying framework within which this and a host of otherwise disparate phenomena may be similarly understood. For instance, Trope and Fishbach (2002) have shown that people who are scheduled to undergo medical procedures are more concerned about "chickening out" when they expect the procedure to be extremely painful than when they expect it to be slightly painful. Therefore, they tend to use counteractive self-control strategies (e.g., making social commitments, agreeing to pay large cancellation fees) only for extremely painful procedures. The ironic consequence is that people are ultimately more likely to chicken out of slightly painful than extremely painful procedures. This interesting phenomenon exemplifies the region-β paradox. Because self-control strategies are triggered by critical levels of anticipated pain, the normally monotonic relation between anticipated pain and the likelihood of chickening out is reversed, and a more painful procedure can actually induce greater compliance than a less painful one.

Many other phenomena take the same form and produce the same sort of paradoxical consequences. For instance, people may buy small rather than large amounts of forbidden foods because they believe that the amount they consume will be a monotonic function of the amount they keep in their kitchen cabinets. And yet, when one opens the cabinet, a full-sized Hershey's bar may trigger concerns about health and diet that a single Hershey's Kiss does not. The paradoxical consequence is that people may actually eat more chocolate when the kitchen cabinet contains one Kiss than when it contains a full-sized bar—a violation of the normally monotonic relation between availability and consumption. Or consider the fact that drivers may avoid long trips because they believe that the odds of being involved in an accident are monotonically related to the time they spend on the road. If a trip to another state triggers the decision to wear a seat belt and a trip around the block does not, the paradoxical consequence is that people may be more likely to sustain injuries in automobile accidents when they are taking short rather than long trips. Similarly, partygoers may limit their alcohol consumption because they believe that their blood alcohol levels are monotonically related to the likelihood that they will cause an automobile accident on the way home. If becoming thoroughly inebriated triggers prophylactic interventions ("We hid your car keys and called you a cab") and becoming slightly tipsy does not, the paradoxical consequence is that partygoers may be safer when they consume a pitcher of martinis than when they consume two glasses of chardonnay. The ease with which examples such as these are generated highlights the ubiquity of the region-β paradox in everyday life, as well as the potential dangers of ignoring it.

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14

Strangers to Ourselves: Discovering the Adaptive Unconscious

Timothy D. Wilson

Of all studies, the one he would rather have avoided was that of his own mind. He knew no tragedy so heartrending as introspection.

—Henry Adams, *The Education of Henry Adams* (1918)

There is a lot about ourselves that is difficult to know, such as our nonconscious preferences, personality traits, goals, and feelings. How might people gain insight into the hidden corners of their minds? What better place to start than with introspection, which many of us assume opens an inner path that, if followed carefully, leads to important self-insights. Introspection can be quite useful, but not always in the way that most people think.

EVERYDAY INTROSPECTION

A few years ago some friends of mine, both research psychologists, moved to a new city and began looking for a house. They took a rather unusual approach to their house hunt. First, they made a list of all the attributes of a house they cared about, such as the neighborhood, school district, number of rooms, layout of the kitchen, and so on. The list was quite exhaustive, taking up several pages. Then,

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when they visited houses with their real estate agent, they took out a copy of the list and rated each house on every attribute. They used the familiar tool of the social psychologist, the 7-point scale. Is the kitchen in this house a 5 or a 6 on the scale? What about the broom closet? After seeing several houses, my friends figured, they would have a good way of quantifying and remembering how they had felt about each one. They could simply compute the average rating of each house and know which one to buy.

Contrast this to the way my real estate agent determines the kind of house her clients want. When she meets with her clients for the first time, she listens patiently as they describe their preferences, nodding her head sympathetically. Many people, like my psychologist friends, go into exhaustive detail. Then, my agent ignores everything the clients just said. She takes them to a wide variety of houses—some modern, some old; some with large yards, some with small; some in town, some in the country—even if the clients have just told her that they would never consider houses in some of these categories.

On the initial visits, the agent pays close attention to her clients' emotional reactions as they walk through the houses, trying to *deduce* what they are really looking for. Often, she says, she determines that people like something quite different from what they have described. One couple said they had to have an older house with charm and would not even consider a newer house. My agent noticed, however, that the couple perked up and seemed happiest when she took them to modern houses. The couple eventually bought a house in a new development outside of town, rather than the older house in the city they said they had always wanted. My agent's wisdom is shared by other real estate professionals, so much so that there is a common saying in the business: "Buyers Lie."

Buyers, of course, do not deliberately misrepresent what they want. Rather, they may not be fully aware of their preferences or have difficulty articulating them. One reason my real estate agent is so successful is that she is quite skilled at inferring what her clients want and often knows their preferences better than the clients themselves do.

Is there a way that people can introspect more carefully about these nonconscious states in order to figure them out? A lot of time would be saved if people could articulate their preferences exactly. Real estate agents would not have to drive clients around to different kinds of houses and figure out what they really wanted.

Perhaps my psychologist friends are onto something. If people approached their preferences more carefully and analytically, using 7-point scales to rate every attribute of a new house or car or potential mate, maybe they could determine better what they really liked. This strategy has been recommended by many very smart people, such as Benjamin Franklin, in a letter to the scientist Joseph Priestly:

> My way is to divide half a sheet of paper by a line into two columns, writing over the one Pro, and over the other Con. Then, during three or four days of consideration, I

> put down under the different heads short hints of the different motives, that at different times occur to me, for or against each measure . . . . When each [reason] is thus considered, separately and comparatively, and the whole lies before me, I think I can judge better, and am less likely to make a rash step.

Other people have suggested that the analytic "pluses and minuses" approach is not very useful. Even worse, as the writer Mario Vargas Llosa discovered when he was a judge at the Berlin film festival, it might actually obscure how one really feels:

> I went to every screening with a fresh pack of notecards that I would dutifully cover with my impressions of each and every film. The result, of course, was that the movies ceased to be fun and turned into problems, a struggle against time, darkness and my own esthetic emotions, which these autopsies confused. I was so worried about evaluating every aspect of every film that my entire system of values went into shock, and I quickly realized that I could no longer easily tell what I liked or didn't or why.

A well-known social psychologist had a similar experience when trying to decide whether to accept a job offer from another university. It was a difficult decision, because there were many attractive features of both her current position and the new one—as well as some minuses. One of her colleagues, Irving Janis, had written a book advising people to complete detailed "balance sheets," listing the pros and cons of each alternative (much as Benjamin Franklin recommended), so she decided to give it a try. Here is her report of what happened: "I get half way through my Irv Janis balance sheet and say, 'Oh hell, it's not coming out right! Have to find a way to get some pluses over on the other side.' "

And finally, I should report on what happened to my psychologist friends who carried their exhaustive list of 7-point scales to every house they visited. After dutifully filling out the scales for a few houses, they found that they were even more confused than before about which houses they liked and why. "We finally threw away the list," they said, "and went with our gut feelings about which house we liked the best." They bought a lovely house in which they have been living happily for the past fifteen years. Perhaps introspection is not always fruitful, and may even mislead people about how they feel. As the poet Theodore Roethke put it, "Self-contemplation is a curse / That makes an old confusion worse."

Does this mean that introspection is a useless exercise that is best avoided? Should we advise against all navel-gazing, tell insight therapists to take down their shingles, and recommend that people focus on anything but themselves? It would be odd for a psychologist to tell people never to think about themselves, and this is not my message. The key is to understand that introspection does not open magic doors to the unconscious, but is a process of construction and inference. Once this is understood, the question becomes when this process of construction is likely to be helpful and when it is not.

Ours Is Not to Reason Why

Consider what happens when people engage in the Franklinesque type of introspection, whereby they analyze the reasons for their preferences. Sometimes people do this formally, as Franklin suggested, by making lists of the pluses and minuses of the alternatives. At other times they do it less formally, such as when they think, "Why do I feel the way I do about this person I'm dating, anyway?" My colleagues and I have investigated what happens when people introspect in this manner. We typically ask people to spend about ten minutes writing down their reasons for a particular feeling. We tell them that the purpose of this exercise is to organize their thoughts and that no one will read what they write, and then see what effect this introspection has on their subsequent attitudes.

We have asked people to analyze a wide range of attitudes, including their feelings toward someone they have just met, romantic partners, political candidates, social issues, consumer products, works of art, and college courses. We have been struck by the fact that people have no difficulty in coming up with a list of reasons for their feelings. Almost never has anyone said, "Sorry, I just don't know why I feel the way I do." Instead, people freely and readily write quite detailed reasons for their feelings.

The accuracy of people's reasons, however, is suspect. People are not always wrong—if they say they love their romantic partner because he is extremely kind, or because he has a great sense of humor, they might be right. People do not have access to all the determinants of their feelings, however, and their reasons are often a function of cultural or personal theories that can be wrong or, at best, incomplete. As Immanuel Kant put it, "We can never, even by the strictest examination, get completely behind the secret springs of action."

If people recognized that their explanations were sometimes inaccurate, there would be no danger in making a list of the reasons why they felt the way they did. "I'll do the best I can," they might say, "but keep in mind that my list is undoubtedly incomplete and that some of the things I put down are probably wrong. Hey, I took psychology in college, Doc." There is an illusion of authenticity, however, such that the reasons people give feel more accurate than they are.

Because people have too much faith in their explanations, they come to believe that their feelings match the reasons they list. If they generate several reasons why their dating partner is pretty unexciting ("He has really nice taste in upholstery"), they infer that they are not all that in love—even if they were in love before. In other words, they construct a story about how they feel that is based on reasons that are not entirely trustworthy. The story has the ring of truth to people, but because they have used faulty information (reasons that happened to be on their minds), it often misrepresents how they really feel.

We have found evidence of just this sequence of events. For example, Dolores Kraft and I asked college students involved in dating relationships to write down,

privately and anonymously, why their relationship was going the way it was, and then to rate how happy they were with their relationship. Compared with people in a control condition who did not analyze reasons, these students tended to change their attitudes toward their relationship. Some became happier with it, some less happy.

Why? First, we assumed that people did not know exactly why they felt the way they did. It is not as if people can say with any accuracy, "Okay, here are my reasons: Her basic integrity and kindness account for 43 percent of my love, her sense of humor 16 percent, her political views 12 percent, that endearing way she brushes the hair out of her eyes 2 percent, and the rest is pheromones." Instead, people brought to mind reasons that conformed to their cultural and personal theories about why people love others and that happened to be on their minds ("I was just looking at the paisley pattern on his couch and thinking about what a great decorator he is"). Because there is a certain arbitrariness to these reasons, they often do not match people's prior feelings perfectly. In fact the reasons people gave bore almost no relationship to how happy they said they were with their relationship a few weeks earlier. But because people do not recognize this fact, they assume that their reasons are an accurate reflection of their feelings, leading to attitude change. In short, people construct a new story about their feelings based on the reasons that happen to come to mind.

This is what seems to have happened to Proust's Marcel in *Remembrance of Things Past.* As seen in Chapter 1, Marcel becomes convinced that he no longer loves Albertine, after analyzing and introspecting about his feelings: "As I compared the mediocrity of the pleasures that Albertine afforded me with the richness of the desires which she prevented me from realizing . . . [I] concluded that I did not wish to see her again, that I no longer loved her."

I should point out that analyzing reasons does not always lead to attitude change in a negative direction. In our study of dating couples, not all people who listed reasons became more negative toward their relationship. Rather, the direction of the attitude change depended on the nature of the reasons that happened to come to each person's mind. People who found it easiest to think of positive reasons ("He's a great friend and easy to talk to") changed their attitude in a positive direction, whereas those who thought of lukewarm or negative reasons ("He has a fine fashion sense, though it would be nice if he didn't wear that pink shirt quite so often") changed in a negative direction. Marcel found it easiest to think of negative aspects of his relationship with Albertine, and thus concluded that he no longer loved her.

If Benjamin Franklin picked up a psychology journal and read about these findings, he might respond, "Just as I thought—when people step back and think about the pros and cons, they come up with a better-informed, more reasoned point of view. After people analyze reasons, their attitude is superior to the quick, possibly rash judgments they would have otherwise made."

The story people construct on the basis of their reasons analysis, however, can misrepresent how they really feel. Such was the case with Marcel, who discovers, only after learning that Albertine has left him, how wrong he was about his over-analyzed feelings. We have found that the feelings people report after analyzing reasons are often incorrect, in the sense that they lead to decisions that people later regret, do not predict their later behavior very well, and correspond poorly with the opinion of experts.

For example, in another study we compared people who were asked to list reasons about why their relationship was going the way it was with people who did not list reasons. Whose feelings did the best job of predicting the longevity of the relationship? It was the latter group, who did not analyze reasons. This is consistent with the notion that when people analyzed reasons, they constructed stories based on faulty data, such as which aspects of the relationship were easiest to put into words, were on their minds, or were consistent with their theories about how they should feel, leading to attitudes that were less well informed than those of people in the control group, who just gave their unanalyzed, gut feelings. As Goethe put it, "He who deliberates lengthily will not always choose the best."

A study of people's attitudes toward works of art tested Goethe's hunch. Some people analyzed exactly why they liked or disliked five art posters and some did not. Then, all participants chose one of the posters to take home. Two weeks later, we called people up and asked them how happy they were with the poster they had chosen. Benjamin Franklin might predict that the people who analyzed their reasons would make the best choices, by carefully laying out the pros and cons of each option. We found the opposite: The people who did not list reasons, and presumably based their choices on their unanalyzed gut feelings, were happier with their posters than were the people who had listed reasons. Like Mario Vargas Llosa, who found it difficult to tell how he felt about the films when he analyzed each one, the students in the reasons analysis group seemed to lose sight of which poster they really liked the best.

A few years ago I was interviewed by a reporter about this line of research. After we chatted for a while the reporter said she had one final question: "So, Dr. Wilson, I gather you are saying that people should never think about why they feel the way they do and should simply act on their first impulses?" I was horrified and had images of people following the reporter's conclusions about my research, leading to increases in teen pregnancy, drug relapses, and fistfights.

It is important to distinguish between informed and uninformed gut feelings. We should gather as much information as possible, to allow our adaptive unconscious to make a stable, informed evaluation rather than an ill-informed one. Most of us would agree that it would not be wise to marry the first person we are attracted to. If we spend a lot of time with someone and get to know him or her very well, and still have a very positive gut feeling, that is a good sign.

The trick is to gather enough information to develop an informed gut feeling and then not analyze that feeling too much. There is a great deal of information we

need in order to know whether someone would make a good partner, much of it processed by our adaptive unconscious. The point is that we should not analyze the information in an overly deliberate, conscious manner, constantly making explicit lists of pluses and minuses. We should let our adaptive unconscious do the job of forming reliable feelings and then trust those feelings, even if we cannot explain them entirely.

Is It Always So Bad to Think About Reasons?

Another thing I told the reporter is that there are some exceptions to the danger of analyzing reasons, which follow from our explanation of why it can be harmful. As we have seen, people often change their minds about how they feel because the reasons they think of do not match their prior feelings very well. There is a group of people for whom this is not true, namely people who are quite knowledgeable about the topic they are analyzing. In the study with the art posters, for example, people who knew a lot about art—those who had taken high school and college art courses—tended to list reasons that matched their prior feelings well. Consequently, the act of listing reasons did not lead to any attitude change in this group. It was the unknowledgeable people who were most likely to bring to mind reasons that conflicted with their initial feelings, causing them to revise their stories about how they felt. Contrary to Benjamin Franklin's advice, the knowledgeable people in our studies do not seem to gain anything by analyzing reasons. The art experts did not like the posters they chose more than unknowledgeable people, but neither did they like them more.

But surely, you might argue, we have not done a fair test of the kind of introspection Franklin recommended. He suggested that people write down pros and cons "during three or four days consideration," whereas in our studies people typically write about reasons only once for ten minutes or so. Might people better decipher their feelings with a longer self-analysis? To find out, Dolores Kraft and I asked the people in our dating couples study to come back to our lab and analyze reasons again, once a week for four weeks. We found that a fair amount of attitude change occurred the first time people analyzed reasons (as discussed earlier), and then people tended to stick to this new attitude when they came back and analyzed reasons again. There did not seem to be any advantage to analyzing reasons more than once; rather, people brought to mind reasons that conflicted with their initial attitude, changed their attitudes to match those reasons, and then stuck to that new attitude.

It is possible, of course, that people would have benefited from an even longer reasons analysis or from one that was not spread out over so much time. My hunch, though, is that if people are not very knowledgeable about the topic they are analyzing, it is an exercise best avoided—at least in the way we have studied it, whereby people sit down by themselves and think about why they feel the way they do.

Recognizing Gut Feelings

Suppose you take my advice and let your adaptive unconscious develop feelings about somebody or something, and avoid the kind of introspection in which you try to put into words exactly why you feel the way you do. What if you are still not certain how you feel? Sometimes people have mistaken beliefs about the nature of their feelings, particularly when their feelings conflict with cultural feeling rules ("people love their ponies," "my wedding day will be the happiest time of my life"), personal standards ("I am not prejudiced at all toward African Americans"), or conscious theories ("I must love him because he conforms to my idea of Mr. Right"). Is there a kind of introspection by which you can gain access to feelings that are hidden in this manner?

Introspection should not be viewed as a process whereby people open the door to a hidden room, giving them direct access to something they could not see before. The trick is to allow the feelings to surface and to see them through the haze of one's theories and expectations.

A recent study by Oliver Schultheiss and Joachim Brunstein suggests one way people might accomplish this. They measured people's implicit motives, using the Thematic Apperception Test technique, whereby people make up stories about a set of standard pictures and these stories are coded for how people express motives such as the need for affiliation or power. They then told participants that they would play the role of a therapist who would use directive techniques to counsel a client. Because people were instructed to be directive and keep control of the situation, and to focus on ways of helping the client, those who were high in both the need for power and the need for affiliation were expected to react especially positively.

The question is, did people know that this was a situation that was well suited or not well suited to their implicit motives? The answer was no when the researchers simply described the counseling situation to participants, and then asked them how they would feel. Consistent with many studies that find that people are not very aware of their implicit motives, people who were high in the need for affiliation and power did not anticipate that the counseling session would make them any happier or feel more engaged than other participants.

In another condition, however, people first underwent a goal imagery procedure whereby they listened to a detailed, tape recorded description of the counseling session and imagined how they would be likely to feel in that situation. Under these circumstances people high in the need for affiliation and power were more likely to recognize that the situation would be one that they would enjoy, and they reported that they would be much happier and more engaged in that situation than other participants did.

Thus, hearing a detailed, image-laden description of the situation was sufficient to trigger feelings generated by people's implicit motives, and people were able to pay attention to these feelings and use them to predict how they would feel in the

real situation. I would not call this "introspection" as normally defined, because people were not opening doors to hidden rooms to see feelings of which they were unaware. Instead, they were able to imagine a future situation well enough that the feelings it would invoke were actually experienced, and were able to avoid the kinds of introspection we have studied (analyzing reasons) that might obscure how they would really feel.

It remains to be seen how well people can use this technique in everyday life. The suggestion, at least, is that if people took the time to imagine future situations in great detail (e.g., "How would I feel if my housekeeper rushed in with the news that Albertine has left me?"), they might be better able to recognize the feelings generated by their adaptive unconscious, and to see through the smoke screen created by analyzing reasons or by the adoption of cultural feeling rules and conscious theories. They would have better data on which to base their narrative about their feelings and reactions.

INTROSPECTING ABOUT PERSONAL PROBLEMS

Although some of the studies on introspection considered so far dealt with topics that were very important to people, such as why a romantic relationship is going the way it is, they are generally not topics that are causing people distress (most of the participants in our studies were reasonably happy with their relationships). Perhaps people are more adept at introspecting about things that have gone wrong in their lives. There are many ways to introspect about one's source of distress, however, some of which are more helpful than others.

Ruminating When Distressed

One way is to ruminate about a problem, which Susan Nolen-Hoeksema defines as thinking about one's feelings and their causes repetitively without taking action to improve one's situation. In numerous studies, she has found that rumination leads to a negative, self-defeating pattern of thought that makes matters worse, especially when people are depressed or in bad moods to start with. Ruminators are worse at solving problems related to their distress, focus more on negative aspects of their past, explain their behavior in more self-defeating ways, and predict a more negative future for themselves.

In one study, for example, the participants were college students who were either moderately depressed or nondepressed. In the rumination condition, the students were asked to spend eight minutes thinking about their emotions and traits—that is, to try to understand their feelings, why they felt that way, their character, why they turned out the way they did, and who they strived to be. In

a distraction condition, the students spent eight minutes thinking about mundane topics unrelated to themselves, such as "clouds forming in the sky" and "the shiny surface of a trumpet." People's moods were measured before and after they completed the rumination or distraction task. Rumination caused depressed participants to become even more depressed, whereas the distraction task made them less depressed. Rumination had little effect on people who were not depressed.

When the depressed students ruminated they focused on the negative side of things, as if their dysphoria was a filter that kept out any positive thoughts. Compared with the other groups—such as the nondepressed students who ruminated and the depressed students who did not ruminate—they brought to mind more negative memories from their pasts (e.g., "Everyone passed the test except me") and felt that negative events in their current lives, such as getting into arguments with their friends, were more common. In another study, people who reported that they often ruminated when they felt depressed were more likely to be depressed a year later, even after their initial levels of depression were controlled for. In short, unhappiness and ruminating about your unhappiness is a bad combination that leads to more depression.

Finding Meaning Through Introspection

Imagine that you received these instructions:

> For the next three days, I would like for you to write about your very deepest thoughts and feelings about an extremely important emotional issue that has affected you and your life. In your writing, I'd like you to really let go and explore your very deepest emotions and thoughts. You might tie your topic to your relationships with others, including parents, lovers, friends, or relatives; to your past, your present, or your future; or to who you have been, who you would like to be, or who you are now.

Jamie Pennebaker and his colleagues have given these instructions to hundreds of people, including college students, community members, maximum-security prisoners, people laid off from their jobs, and new mothers. Most people take it quite seriously and write about personal, often deeply troubling incidents, such as the death of a loved one, the end of a relationship, or sexual and physical abuse. Not surprisingly, people find it upsetting to write about such events and, right after doing so, report more distress than do control participants who write about superficial topics (such as their plans for their day).

As time goes by, however, people show remarkable benefits from the writing exercise. Compared with people in the control condition, those who write about emotional experiences report better moods, get better grades in college, miss fewer days of work, show improved immune system functioning, and are less likely to visit physicians. Writing about emotional experiences is distressing in the short run but has quite positive long-term effects.

Why does writing about emotional experiences—often very painful ones—have more beneficial effects than the other kinds of introspection we have discussed? One possibility is that people tend to hide or suppress their negative emotional experiences, and that the stress caused by constant inhibition takes its toll on their mental and physical health. Having the opportunity to express traumatic events might have a cathartic effect, improving people's well-being by removing the stress caused by inhibition. Although inhibition may well cause stress and contribute to health problems, there is no evidence that Pennebaker's writing exercise works by lowering inhibition. For example, people who write about events that they have already discussed with others do as well as people who write about events they have kept secret.

Rather, writing seems to work by helping people make sense of a negative event by constructing a meaningful narrative that explains it. Pennebaker has analyzed the hundreds of pages of writing his participants provided, and found that the people who improved the most were those who began with rather incoherent, disorganized descriptions of their problem and ended with coherent, organized stories that explained the event and gave it meaning.

Why is rumination harmful whereas Pennebaker's writing exercise is beneficial? One key is that people often ruminate when they are depressed, and the depression focuses their attention on negative thoughts and memories, making it difficult to construct a meaningful, adaptive narrative about the problems. Rumination is a repetitive, spiraling kind of thought whereby people can't stop thinking about things in a negative light, like Mr. Dimmesdale in *The Scarlet Letter:* "He kept vigils, likewise, night after night, sometimes in utter darkness; sometimes with a glimmering lamp; and sometimes, viewing his own face in a looking-glass, by the most powerful light which he could throw upon it. He thus typified the constant introspection wherewith he tortured, but could not purify, himself." In contrast, Pennebaker's participants, who are typically not depressed, are able to take a more objective look at their problems and to construct a narrative that helps explain it in a more adaptive manner. In fact, Pennebaker's technique does not work as well right after a severe trauma, when people are too upset to examine their situation objectively.

Constructing a meaningful narrative can also keep people from trying to suppress their thoughts about a distressing topic. If an event has no coherent explanation it is likely to keep coming to mind, leading to further rumination, or possibly to an attempt to push the thoughts away. A deliberate attempt at thought suppression is a losing exercise, as Daniel Wegner and his colleagues found. People may be able to succeed in not thinking about something for a short time, but often thoughts about the unwanted topic come flooding back. Under some circumstances, such as when people are tired or preoccupied, thought suppression backfires, leading to even more thought about the unwanted topic. An event that has been explained and assimilated into one's life story is less likely to keep coming to mind, triggering attempts to suppress it.

The narrative metaphor helps explain all the examples of everyday introspection we have considered. Analyzing reasons focuses people on bad "data," information that is easy to verbalize but may have little to do with true feelings. Consequently, people construct stories about their feelings from faulty information. Rumination and thought suppression can be harmful in at least two ways: they can make it difficult to engage in the construction of a new narrative, because people are preoccupied with uncontrollable, unwanted thoughts; and, to the extent that people do construct new narratives, they can focus people's attention on negative, pejorative thoughts. Pennebaker's writing exercise is the only kind of introspection we have seen so far in which people are able to construct meaningful stories that have beneficial effects.

15

Self-Theories of Intelligence

Carol S. Dweck

Why do some very bright individuals do poorly in school and end up achieving little in life? Why do other, seemingly less bright people, rise to the challenges and accomplish far more than anyone ever expected? Much of my career has been devoted to answering these questions, and that's where social psychology comes in.

One of the most important things social psychology has done is to show us how profoundly people's beliefs affect their behavior. This has been shown very clearly in the realm of motivation and achievement. Do people believe their intelligence is a fixed trait or an expandable quality? Do they believe their failures are due to a lack of effort or a lack of ability? Do they believe they are doing a task to learn something new or to show how smart they are? These beliefs are key components of individuals' eagerness to learn, their love of challenge, and their ability to persist and thrive in the face of difficulty. This is why they are key factors in what people achieve—quite apart from their intellectual ability.

The most exciting thing about this is that beliefs can be changed. So, even more important than showing that beliefs matter for people's motivation and achievement, is showing that when you change their beliefs, you change their motivation and achievement. It is sometimes amazing to people who are not social psychologists that what looks like minor belief-changing interventions—teaching students a different view of intelligence, teaching them a different interpretation for failure,

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or orienting them toward different reasons for achieving—end up having real effects on their school engagement and achievement. I have seen researchers from other fields be completely baffled by these results because they are used to seeing hugely expensive, large-scale, long-term, multifaceted interventions that yield only small effects. Yet, social psychologists understand the power of a carefully targeted intervention that changes a key belief and refocuses people's motivation in highly productive ways.

THE ROLE OF MOTIVATION IN ACHIEVEMENT

What role does motivation play in achievement? There are many researchers who argue that motivation is the key ingredient not simply in outstanding achievement, but also in extraordinary achievement. Their work suggests that creative genius itself grows out of the ability to sustain intense commitment for very long periods in the face of obstacles (Runco, Nemiro and Walberg, 1998; see also Hayes, 1989; Nickerson, 1999; Perkins, 1994; Weisberg, 1986; 1999). They tell us, much to our surprise, that many well-known geniuses were pretty much ordinary bright children who then became obsessed with something and, because of that obsession, ended up making enormous contributions (Howe, 1999; Simonton, 1999). This is true in science: Darwin's father was deeply disappointed in how ordinary his son seemed as a child (Simonton, 1999). It is true in philosophy: John Stuart Mill's father, in fact, was tickled to show that his ordinary child could be trained to be a world-famous philosopher (Howe, 1999). Tolstoy and William James were also seen by some as unexceptional children (Howe, 1999).

Even Mozart, whom we think of as composing in infancy, did not produce really original and noteworthy works until after more than 10 years of nonstop composing (Bloom, 1985; Hayes, 1989; Weisberg, 1999). Most of his early compositions were amateurish hodgepodges of other people's compositions. The same principle applies in athletics as well. We all know the story of how Michael Jordan was cut from his high school basketball team—which only increased his commitment to relentless practice until he became one of the greatest athletes of all time.

Yet, much of society is stubbornly wedded to the idea that accomplishment, especially outstanding accomplishment, is about endowment. After Mozart, Darwin, Michael Jordan, and Tiger Woods practice their skills feverishly and single-mindedly for years and years, we ignore this and instead believe that they were simply born with one-in-a-million ability. When Thomas Edison claimed that genius was 99 percent perspiration, and only 1 percent inspiration, we think he is just being modest. And when we hear that the ringlike structure of benzene came to Kekule, the great chemist, in a dream or that the brilliant poem "Kubla Khan" came to Samuel Taylor Coleridge in an opium-induced delirium, we think "Ah yes, that's genius," forgetting the years of training, commitment, and perseverance that led up to these episodes.

SELF-THEORIES OF INTELLIGENCE

In discussing beliefs that play a key role in motivation, I will focus on one particular kind of belief, namely, people's "theories" about their intelligence. I will begin by describing the two theories of intelligence that people hold: The belief that intelligence is a fixed trait that cannot be developed (which we have called an "entity" theory) versus the idea that intelligence is a malleable quality, a potential that can be cultivated (which we have called an "incremental" theory). I will then show how these beliefs affect the tasks students will take on, the effort they are willing to exert, their ability to cope with setbacks, and—ultimately—their academic performance.

As you will see, the belief in fixed intelligence can lead even the most able students to worry about how smart they are, to think they're dumb when they fail, to dislike and avoid effort, and to show impaired performance when they face academic difficulty (which even top students can experience when they enter a new school). The belief that intelligence can be developed, in contrast, makes students want to do just that: It leads them to value learning over looking smart, to enjoy effort and challenges, and to thrive in the face of difficulty (see Dweck, 1999).

Which is the correct view of intelligence? Is it fixed or is it something that can be developed? Psychologists have always taken and still take both views. Alfred Binet (1909/1973), the inventor of the IQ test, however, was a radical proponent of the idea that intelligence can be developed. He believed that children's most basic capacity to learn could be transformed through education, and he devoted much of his career to designing educational programs that might do that. Today, more and more psychologists are taking the view that intelligence or important components of it can be developed through motivation and learning (Brown and Campione, 1996; Perkins, 1995; Resnick, 1983; Sternberg, 1985). My work does not directly address the question of the true nature of intelligence, but it does show the critical importance of what people believe about intelligence. Let's now look at the motivational worlds created by the two different beliefs.

THE BELIEF IN FIXED INTELLIGENCE

What Is the Student's Goal?

When students believe that their intelligence is fixed what is it they want most to accomplish through their academic work? The answer is they want to look smart. Since intelligence is such a deeply valued commodity and they only have a fixed amount of it, they want to feel as though they have the right amount. In many studies with grade school, junior high school, and college students, we give students a choice between a challenging task where they can learn important new things and a "safer" task where they can look smart. Most students with the fixed view take the task that will make them look smart (Dweck and Leggett, 1988; Stone and Dweck,

1998). This means that rather than risk making errors, they will sacrifice valuable learning opportunities.

A recent study shows exactly how self-defeating this can be. At the University of Hong Kong (the premier institution of higher learning in Hong Kong), all classes, class assignments, and exams are in English. But not all students come to the university knowing much English. In this study (Hong, Chiu, Dweck, Lin and Wan, 1999), we assessed new students' theories about their intelligence and we obtained their scores on their English proficiency exams. We then asked them if they would be willing to take a remedial English course if the faculty offered it. Students who had low English proficiency and believed in malleable intelligence said yes, but students with low English proficiency and a fixed view of intelligence did not. They were not willing to expose their ignorance or risk errors—even though, by not doing so, they were putting their academic career in jeopardy.

What Does Failure or Difficulty Mean?

You might think that students who believe in fixed intelligence would form an opinion of their intelligence and stick to it, but this isn't the case. Why not? Because, although they believe intelligence is fixed, intelligence is an invisible, internal thing that they can't really observe directly—so they just have to guess its level from their performance. This means that one day students may think their fixed ability is high because they've done well, but the next day think it's low because they haven't.

We have found over and over that students who believe in fixed intelligence see academic setbacks as meaning something very negative about their intellectual abilities (Dweck and Sorich, 1999; Henderson and Dweck, 1990; Mueller and Dweck, 1998; Stone and Dweck, 1998; cf. Grant-Pillow and Dweck, 2003). This is even true of bright students at top schools.

In one study (Dweck and Sorich, 1999), we asked junior high school students to imagine that they really liked a new class they were taking in school. They studied for the first test, but did really poorly. What would they feel and what would they think? They told us they would feel stupid and would think, "I'm just not good at this subject" and "I wasn't smart enough." One test had the power to define them.

What would they do? Work harder? Guess again.

What Do They Believe About Effort?

Other students believe that effort can compensate for lower ability, that you can get to the same place by working that much harder—but not these students. They tell us that if you don't have ability, forget it. Specifically, they agree that: "If you're not good at a subject, working hard won't make you good at it" and "It doesn't matter how hard you work—if you're smart you'll do well; if you're not smart, you won't."

So what *would* they do after doing poorly on that test? Compared with students with the malleable view, the students with a fixed view of intelligence agree more with the following: "I would spend *less* time on the subject from now on," and "I would try to cheat on the next test." If effort doesn't work, these are, sadly, the options they see for themselves.

I believe there is no more damaging view for students than the belief that effort is unnecessary (if you're smart) and ineffective (if you're not).

What Happens to Performance?

The need to prove your intelligence can be highly motivating when things are going well, but it does not serve students well when the going gets tougher. To examine this, we have done several large studies of students making the transition to junior high school (Dweck and Sorich, 1999; Henderson and Dweck, 1990). This is when students leave the cocoon of grade school in which teachers have time to be more nurturant, the work is more individualized, and the grading tends to be more generous. Suddenly, students find themselves in a new, more impersonal world with harder work and more stringent grading. This is a particular threat to students with the fixed view, whose intelligence is now on line.

In each of our studies, we have found that this is a time when their grades and achievement test scores suffer compared with those of their classmates who hold the more malleable view. Regardless of their past achievement, students with the fixed view are more likely to show lower academic performance. Some students who were among the highest achievers before are now in trouble.

Now let's look now at the alternative.

THE BELIEF IN MALLEABLE INTELLIGENCE

What Is Their Goal?

When students believe their intelligence is a potential that they can develop, they focus—not so much on looking smart—but on challenges and learning. When we offer *them* a choice of tasks, they reject the task that would simply make them look smart, in favor of the task that allows them to learn something new, even with a risk of errors.

They agree with statements like these: "It is much more important for me to learn things in my classes than it is to get the best grades," and "I like schoolwork I can learn from even if I make a lot of mistakes." For these students, the name of the game is not the quick fix of outdoing others, but rather personal mastery over time: "I feel successful when I improve in school even if other students get a higher score than me."

What Does Failure or Difficulty Mean?

It sends a message about your effort or strategy, not your fixed ability. Mistakes are simply a natural part of learning, and something that give you information about what to do next. Failure, while never a welcome event, is also signal to do something. When these students get a disappointing grade, they tell us they'd get information about what went wrong, engage in remedial work, and study more the next time (Dweck and Sorich, 1999; Grant-Pillow and Dweck, 2003; Hong et al., 1999). In other words, they take the bull by the horns and work toward improvement in the future.

What Does Effort Mean?

Everything. For these students, effort is what powers their ability and allows them to use it to the fullest. Even geniuses, they believe, have to work hard for their accomplishments. In this, they are in sync with Thomas Edison and his claim of 99 percent perspiration. They also believe that even if you're not so good at something, effort will certainly help you achieve, for effort is the way to overcome setbacks. In short, they agree: "The harder your work at something, the better you will be at it" (Dweck and Sorich, 1999).

What Happens to Performance?

In line with their emphasis on challenge, effort, and learning, these students are able to do well as they make difficult educational transitions. As I've reported, they outshine their classmates with the fixed view in grades and achievement test scores as they confront the challenges of a new school (Dweck and Sorich, 1999; Henderson and Dweck, 1990). Even some students who had not done that well before in school blossom at this time (Henderson and Dweck, 1990).

CAN THEORIES OF INTELLIGENCE BE CHANGED?

Yes, they can. Even though when people's theories are left alone, they tend to be fairly stable, people can be taught a different view.

In a number of experiments, psychologists have tested the effects of orienting students toward a malleable view of ability (by telling them that the ability required by the task could be learned over time with practice or effort) versus a fixed view of ability (by telling them that the ability is inborn, or is one that you either have or don't have) (Aronson, 1998; Jourden, Bandura and Banfield, 1991; Tabernero and Wood, 1999; Wood and Bandura, 1989). These studies have found that when indi-

viduals are given the malleable view, they choose more challenging goals for themselves, they maintain confidence and high effective persistence in the face of setbacks, and they end up outperforming those who are given the fixed view. What's also interesting is that the students given the malleable view maintain their interest in the activity even when they're experiencing difficulty.

In a fascinating study designed to test the effects of a self-theory intervention, Joshua Aronson, Carrie Fried, and Catherine Good (2002) taught college students a malleable view of their intelligence and looked at the impact of this on their actual engagement and achievement in school. Students who were taught this theory, especially the minority students, showed greater engagement with their studies and higher grade-point averages than groups of comparable students who were not taught this theory. This study shows what a profound effect a short belief-changing experience can have on a student's academic life.

Aronson and Good (2002) also performed a self-theory intervention with minority students in junior high school as part of computer skills class. For the students in the experimental group, the mentors conveyed the view that intelligence is expandable, and helped each student design a Web page that advocated this view. At the end of the year, they compared the performance of the experimental and control groups on statewide standardized achievement tests. The results showed that the students in the malleable theory intervention earned higher achievement test scores in both math and reading than did students in the control group. Although the malleable manipulation helped all students, it was particularly beneficial to females in math. In the malleable condition, the gender gap in math, evident in the control group, disappeared.

Recently, we completed a theory-changing intervention with at-risk junior high school students who had been showing sharp declines in achievement (Blackwell, Dweck and Trzesniewski, 2003). As in the Aronson and Good study, students in the experimental group (who received instruction in the theory of malleable ability) and students in the control group both received training in important academic skills and knowledge. However, only students in the experimental group were taught the idea that learning changes the brain and makes you smarter. By the end of the semester, students in the experimental group earned significantly higher math grades than those in the control group and, according to their teachers (who did not know which group students were in), showed marked changes in academic motivation. The malleable theory appeared to energize students to use the new skills they had learned.

Most academic interventions simply try to strengthen the skills that vulnerable students are thought to lack, for example, by teaching them study skills or by tutoring them in the subjects that present difficulty. Yet this does not appear to be enough. In all of the three studies just described, the students in the control group either already had equivalent skills or were given all of the same skill-boosting programs as the experimental group. In each case, only those who experienced the belief-changing intervention showed changes in motivation, put their skills to work for them, and displayed gains in achievement.

Aside from direct interventions, there are also some common practices that can—inadvertently—alter students' theories about their intelligence.

THE EFFECTS OF PRAISE

A few years ago, Melissa Kamins, Claudia Mueller, and I began to wonder if some very common and very well-meant practices might be having unwanted effects on students. We started thinking about how students with the fixed view of intelligence were so fixated on their intelligence and so concerned with how their performance measures them. Well, we thought, when you praise students' intelligence after a job well done, isn't that telling them that intelligence is what it's all about and isn't it telling them that their intelligence can be measured directly from their performance?

Now, of course, this isn't what people mean to do when they praise intelligence. They mean to boost students' confidence and motivate them to do well in the future. In fact, in a survey of parents that we conducted, more than 80 percent of parents agreed that it was *necessary* to praise children's ability to make them feel good about themselves. However, to see what intelligence praise *really* does, we conducted a series of studies. To foreshadow our findings, I will tell you that the power of intelligence praise was astounding to us.

In six studies (Mueller and Dweck, 1998; see also Kamins and Dweck, 1999, for studies with younger children), we gave 5th-grade children interesting puzzlelike problems to solve. These problems were part of a popular nonverbal IQ test. The first set of problems they worked on were challenging but well within their range, and virtually all children did really well. After this success, children received one of three forms of praise. One-third of the children were given intelligence praise: "You must be smart at these problems." One-third were given effort praise: "You must have worked hard at these problems." And one-third of the children were simply told that they had done really well, but were not praised for their ability or their effort. Remember, all three groups of students were really the same when they came into the study. The only thing that differed was one sentence of the praise feedback they received. Next, they all received far more difficult problems, on which they did more poorly (so we could assess the impact of this difficulty on their subsequent motivation and performance). Finally, all children received another set of the easier (but challenging) problems.

First, we found that students who were given intelligence praise actually now bought into the fixed view of intelligence more than the students who were given the effort praise. Thus, the intelligence praise per se seemed to imply that the adult was judging some deep-seated ability that dwelt within them, and the effort praise seemed to imply that skills are more acquirable and can be expanded through effort.

What goals did students want to pursue following the initial praise? The majority of the intelligence-praised students chose a task that would allow them to look

smart and rejected a challenging task that would allow them to learn a lot but wouldn't necessarily make them look good. The effort-praised students did completely the opposite: as many as 80 to 90 percent of them preferred the task that would allow them to learn a lot and rejected the task that would just make them look smart. So, intelligence praise, rather than making students eager for challenge and learning, did just the opposite. It was effort praise that made students eager to jump right in and learn.

What did failure mean to them? If praising students' intelligence did in fact teach them to measure themselves from their performance, then they should measure themselves from their poorer performance on the second set of problems—and that's what they did. When asked to rate different explanations for why they had trouble with those problems, intelligence-praised students agreed strongly with: "I'm not good enough at the problems," and "I'm not smart enough." What the intelligence praise gave them, one experience with difficulty took away. The effort-praised students hardly blamed their ability at all. Instead they agreed strongly with: "I didn't work hard enough," something they could easily remedy.

How did the difficulty affect children's enjoyment of the task? We assessed this by asking students how much fun they thought the problems were and how much they liked working on them. When students answered these questions right after the praised success, students from all groups thought the task was great fun. And when the effort-praised students were asked these questions after the hard problems, impressively, the difficulty did not change their enjoyment one bit; they really seemed engaged and challenged. But the enjoyment of the intelligence-praised students declined significantly after the difficulty (as did their desire to take the problems home to practice). In short, effort praise, but not intelligence praise, allowed students to maintain their interest and commitment.

What happened to performance? In four studies, we tracked students' performance over the three sets of problems, and looked at what happened from the first set (where all three groups had done equally well) to the third set (that was equivalent to the first set in difficulty, but came after the hard second set). The effort-praised students did the best of the three groups on the final set, and they improved significantly from the first set to the third. This means that their continued involvement and effort paid off. They actually became better ("smarter") at the problems.

In contrast, the intelligence-praised students did the worst of the three groups on the last set of problems, and *declined* significantly from the first to the third set. So, not only does this mind-set lead to self-denigration and loss of enjoyment in the face of difficulty, but it also leads to impaired performance.

This is not to say that students didn't enjoy the intelligence praise. To the contrary. When students received the intelligence praise, they often displayed a proud, satisfied smile that they didn't show in the other conditions. However, that smile of satisfaction was short-lived, as a host of concerns soon overwhelmed them. In

contrast, the effort praise, although perhaps not as thrilling initially, had a host of beneficial effects. By the way, we meant the effort praise to stand for a category of praise—process praise—where the emphasis is on what students put into their work to achieve an admirable result. The praise could as easily refer to their strategies and the like, not just sheer effort.

FEEDBACK IN THE REAL WORLD

How might these trait-versus-process messages be communicated to kids in the real world? Chauncy Lennon and I (Dweck and Lennon, 2001) began thinking seriously about this issue when he called to my attention a very strange finding from a large-scale study of thousands of students across the country: The more time parents spent with their children on homework, the less well the students did! Now, of course, it might be the case that parents felt they had to spend more time with poorer students. But it also could be that spending a lot of time with students on their homework sends them the message that they're not capable. So, we started thinking about all the good things parents do that can send their kids a trait message—or that can send their kids a process message—and we developed a questionnaire to ask beginning junior high school students about this.

We developed a series of scenarios in which we asked students to imagine that their parents did something. Here is an example: "Imagine that your parents offer to help you with your schoolwork. Why would they do this?" We then offered them a series of four reasons to rate. Each reason represented a trait message ("They are worried that I'm not smart enough to figure it out on my own") or a process message ("They want to make sure I learn as much as I can from my schoolwork"). The trait messages focused on judgments of smartness-dumbness or goodness-badness, whereas the process messages focused on learning, challenge, effort, strategies, and understanding things. Here's another example.

"Suppose that your parents are very excited when you bring home some good grades. Why would they be this way?" Trait message: "They think it means I'm smart in school." Process message: "They think I worked hard to get good grades." We included positive trait messages like this so that it wouldn't just be that the trait-focused messages were ones that were more negative.

We then looked at what these trait-versus-process messages predicted. First, the trait message predicted a more fixed theory of intelligence, whereas the process message predicted a more malleable theory. The messages also predicted students' effort beliefs, their explanations for failure, and their grades across their transition to junior high school.

Of course, we don't know from these findings whether there are real differences in parents' messages that are causing these differences or whether students who already have a fixed theory just interpret their parents' practices differently from students who have a malleable theory. Our findings from the studies on intelli-

gence-versus-effort praise show that adults' practices *can* have a direct effect on children's beliefs and motivation. However, both possibilities would be interesting and we are looking at this question in our current studies (Pomerantz and Dweck, 2003).

(MORE) IMPLICATIONS

Our findings on trait-versus-process feedback tell a consistent story. They show that practices focusing students on judgments of their intelligence can lead to a belief in fixed intelligence with all of its vulnerabilities. On the other hand, practices that focus students on such things as effort, challenge, or strategies can lead to a belief in malleable intelligence with all of its benefits. Let's look at some of the implications of these findings.

One thing that excites me about these findings is that the process message is pretty easy to incorporate into existing practices. A teacher, for example, may be very structured or unstructured, very autocratic or democratic, very warm or formal—it doesn't matter. A process message can fit into any of these styles.

The same thing is true for parents. In some cultures, the parents rule the roost and children must simply comply. In other cultures, there is a higher premium on understanding the child's point of view and in having a more equal give-and-take between parent and child. Still, a process message can be integrated into either style without asking parents to develop a whole new philosophy of child-rearing that may or may not fit with their culture.

Learning Is Nice, but Aren't Performance Goals Also Important?

I've talked about how students with the malleable theory of intelligence value learning something new (a learning goal) over validating their intelligence through their performance (a performance goal). But given our society, are performance goals also important? Yes, they are.

To my mind, it's the balance that counts—keeping a balance between valuing learning and performance. Let's face it: Grades often matter a lot, and many students who want to go on to top schools need good grades. Problems arise when students come to care so much about their performance that they sacrifice important learning opportunities and limit their intellectual growth—as they do with a fixed theory of intelligence.

Problems also arise when students equate their grades with their intelligence or their worth. This can be very damaging, for when they hit difficulty, they may quickly feel inadequate, become discouraged, and lose their ability or their desire to perform well in that area.

For me, the best mix is a combination of a) valuing learning and challenge, and b) valuing grades but seeing them as merely an index of your current performance, not a sign of your intelligence or worth (see Grant-Pillow and Dweck, 2003).

Students who are taught that their performance simply measures their current skills can still relish learning and challenges, for mistakes and setbacks would not be undermining them. By the way, this stance characterizes many top athletes. They are *very* outcome-oriented during a game or match, but they do not see a negative outcome as reflecting their underlying skills or potential to learn. Moreover, between games they are very learning-oriented. They review tapes of their past game, they try to learn from their mistakes, they talk to their coaches about how to improve, and they work ceaselessly on new skills.

What About "Gifted" Labels and Programs?

Calling a student gifted can be just like intelligence praise. This means that labeling kids as gifted can sometimes do more harm than good by feeding into a fixed theory of intelligence. The label "gifted" implies that students have received some magical quality (the gift) that makes them special and more worthy than others. Some students are in danger of getting hung up on this label. They may become so concerned with deserving the label and so worried about losing it—that is, they may become so concerned with bolstering their fixed, superior intelligence—that they may lose their love of challenge and learning. They may begin to prefer only things they can do easily and perfectly, thus limiting their intellectual growth.

In line with what I discussed earlier, psychologists who study creative geniuses point out that the single most important factor in creative achievement is willingness to put in tremendous amounts of effort and to sustain this effort in the face of obstacles. It would be a pity if by labeling students as gifted, we limited their creative contributions.

However, we can prevent this by making clear to students that "gifted" simply means that if they work hard and keep on learning and stretching themselves, they will be capable of noteworthy accomplishments. Of course, that is true of many people, not just those considered gifted.

What About Girls?

Girls, especially bright girls, have traditionally underestimated themselves and shied away from challenges. Don't they *need* a message that they're smart?

As I have shown, the way to motivate students and to give them more lasting motivation and confidence is not by telling them they're smart, but by focusing them on the processes that create achievement. In fact, my great fear is that in our zeal to bolster the confidence of girls (and minority students as well), we have lav-

ished intelligence praise upon them, thereby making them perhaps even more vulnerable than they were before.

PYGMALION AND THE MALLEABLE THEORY

The idea of beliefs affecting behavior was dramatically demonstrated in the original "Pygmalion in the Classroom" studies by Rosenthal and Jacobson (1968). It was *teachers'* beliefs and *teachers'* behavior that were influenced, but the impact was on students' achievement.

At the beginning of a school year, teachers were told that some students in their classes had the potential to blossom intellectually. These students had in fact been randomly selected, but by the end of the year, especially in the younger grades, many of these students actually showed marked gains in achievement.

This study is often interpreted to mean that when teachers think students are smart, they treat them differently. But teachers were not told that the students were simply smart. They were given a malleable theory of those students' intelligence, and it may well be the malleable theory of intelligence that produced the results. This would mean that it is not only students' theories of intelligence that matter, but teachers', too (see Dweck, 2001).

CONCLUSION

I have shown that self-theories about intelligence can play a central role in motivation and achievement by orienting individuals toward different goals, different interpretations of outcomes, different views of effort, and different ways of coping with difficulty. I have also shown how these theories can be shaped by experience, and how interventions that teach a malleable theory of intelligence can boost motivation and achievement. Just as social psychologists have long known, changing a key belief such as this can have a surprisingly large impact in a surprisingly short time. The task ahead is to identify other key beliefs that affect motivation and effective functioning in other areas of people's lives.

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SELF-JUSTIFICATION

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16

Dissonance, Hypocrisy, and the Self-Concept

Elliot Aronson

Since its appearance some 40 years ago, Leon Festinger's theory of cognitive dissonance has enjoyed a prodigious and often controversial history. Its impact on the field of social psychology has been enormous—both in its capacity to account for a broad range of apparently disparate phenomena and in the innovative, high-impact methodology developed to test its propositions.

The theory is also very important to me, personally, because it played a pivotal role in my own professional development. As a first-year graduate student at Stanford, I had zero interest in social psychology—what little I knew about that discipline seemed both boring and pedestrian. At the heart of social psychology was the issue of social influence—certainly an important topic—but, in the mid 1950s, the existing knowledge of social influence seemed fairly cut and dried and rather obvious. What did social psychologists know for sure about social influence at that time?

1. If you want people to go along with your position, offer tangible rewards for compliance and clear punishments for noncompliance.
2. Present an audience with a reasonable communication, attributing it to a highly credible communicator.
3. Present the individual with the illusion that everyone else in sight agrees with one another.

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4. If a member of your discussion group disagrees with you, you will send him more messages (in an attempt to get him to see the light) than if he agrees with you. If he persists in being stubborn, you will try to eject him from the group.

In those days, the overwhelming trend in all of American empirical psychology was, "Let's find the reinforcer." If a person (or a rat) does something, there must be a reason, and that reason had to be the gaining of an identifiable reward such as food, money, or praise, or the removing of a noxious state of affairs such as pain, fear, or anxiety. If food will induce a hungry rat to press the lever of a Skinner box or turn left in a Y-maze, surely conceptually similar rewards can induce a person to adopt a given opinion. The classic experiment that seemed to epitomize experimental social psychology in the mid 1950s is the still classic Asch (1951) experiment in which a unanimous majority apparently disagrees with an individual on a simple, unambiguous perceptual judgment. Why do most people conform to this kind of group pressure? Perhaps it makes them anxious to be alone against a unanimous majority; they fear being considered crazy, being held in low esteem, and so on. It's comforting to be in agreement with others. That's the reward for conformity.

Or take the equally classic experiment done by Hovland and Weiss (1951). Why do people tend to believe a statement attributed to a credible source (like J. Robert Oppenheimer) rather than a noncredible one (like Pravda)? Perhaps it increases the probability of being right—and being right reduces anxiety and makes us feel good, smart, and esteemed. That's the reward for changing one's belief.

These data are true enough—but hardly worth getting excited about. My old "bobbeh" (grandmother), a fountainhead of folk wisdom, could have told me those things without having done an elaborate experiment to demonstrate the obvious. Then, in 1957, Leon Festinger invented the theory of cognitive dissonance—deftly combining cognition and motivation—and produced a revolution that revitalized social psychology—and changed it forever. I first read Festinger's book in the form of a prepublication carbon copy that the author thrust into my hands (rather disdainfully!) after I told him I was trying to decide whether to enroll in his graduate seminar. Reading that manuscript was something of an epiphany for me. It was (and still is) the single most exciting book I have ever read in all of psychology.

The core proposition of the theory is a very simple one: If a person were to hold two cognitions that are psychologically inconsistent, he would experience dissonance. Because dissonance is an unpleasant drive state (like hunger, thirst, or pain), the person will attempt to reduce it—much like he would try to reduce hunger, thirst, or pain. Viewed more broadly, cognitive dissonance theory is essentially a theory about sense-making—how people try to make sense out of their environment and their behavior—and thus, try to lead lives that are (at least in their own minds) sensible and meaningful.

As I implied above, one of the theory's most important aspects was in the challenge it presented to the long-standing dominance of reinforcement theory as an all-purpose explanation for social-psychological phenomena. To illustrate this challenge (as well as its importance) consider the following scenario: A young man

performs a monotonous, tedious task as part of an industrial relations experiment. After completing it, he is informed that his participation as a subject is over. The experimenter then appeals to him for help. He states that his research assistant was unable to be there and asks the subject if he would help run the experiment. Specifically, the experimenter explains that he is investigating the effect of people's preconceptions about their performance of a task—specifically, he wants to see if a person's performance is influenced by whether he's told either positive things about the task (in advance), negative things about the task (in advance), or nothing at all about the task. The next participant, who is about to arrive, is assigned to be in the "favorable information condition." The experimenter asks the subject if he would tell the incoming participant that he had just completed the task (which is true) and that he found it to be an exceedingly enjoyable one (which is not true, according to the subject's own experience). The subject is offered either $1 or $20 for telling this lie and for remaining on call in case the regular assistant cannot show up in the future.

The astute reader will recognize this as the scenario of the classic experiment by Leon Festinger and J. Merrill Carlsmith (1959). I regard this experiment, because of the enormous impact it had on the field, the single most important study ever done in social psychology. The results were striking. The subjects who said that they found the task enjoyable in order to earn the paltry payment of $1 came to believe that it actually *was* enjoyable to a far greater extent than those who said it to earn the princely payment of $20. The experiment was a direct derivation from the theory of cognitive dissonance. Needless to say, reinforcement theory would suggest that, if you reward individuals for saying something, they might become infatuated with that statement (through secondary reinforcement). But dissonance theory makes precisely the opposite prediction. If I were a subject in the Festinger-Carlsmith experiment, my cognition that the task I performed was boring is dissonant with the fact that I informed another person that it was enjoyable. If I were paid $20 for making that statement, this cognition would provide ample external justification for my action—thus reducing the dissonance. However, if I were paid only $1, I would lack sufficient external justification for having made the statement—I would be experiencing the discomfort of dissonance—and would be motivated to reduce it. In this situation, the most convenient way to reduce dissonance would be for me to try to convince myself that the task was somewhat more interesting than it seemed at first. In effect, in the process of persuading myself that the task was actually interesting, I would convince myself that my statement to the other student was not a great lie.

Similarly, in another early experiment aimed at testing dissonance theory, Aronson and Mills (1959) demonstrated that people who go through a severe initiation to gain admission to a group, come to like that group better than people who go through a mild initiation to get into the same group. Reinforcement theory would suggest that we like people and groups that are associated with reward; dissonance theory led Aronson and Mills to the prediction that we come to like things for which we suffer. All cognitions having to do with the negative aspects of the group

are dissonant with the cognition that we suffered in order to be admitted to the group—and therefore, they get distorted in a positive direction, effectively reducing the dissonance.

Even in the early years, it was crystal clear that dissonance-generated attitude change was not limited to such trivial judgments as the dullness of a boring task or the attractiveness of a discussion group. The early researchers extended the theory to much more important opinions and attitudes, such as a striking reassessment of the dangers of smoking marijuana among students at the University of Texas (Nel, Helmreich, and Aronson, 1969), and the softening of Yale students' negative attitudes toward the alleged anti-student brutality of the New Haven police (Cohen, 1962).

IMPACT ON THE FIELD

It is hard to convey the impact these early experiments had on the social psychological community at the time of their publication. The findings startled a great many social psychologists largely because they challenged the general orientation accepted either tacitly or explicitly by the field. These results also generated enthusiasm among most social psychologists because, at the time, they represented a striking and convincing act of liberation from the dominance of a general reward-reinforcement theory. The findings of these early experiments demonstrated dramatically that, at least under certain conditions, reward theory was inadequate. In doing so, dissonance research sounded a clarion call to cognitively oriented social psychologists, proclaiming in the most striking manner that human beings *think;* they do not always behave in a mechanistic manner. It demonstrated that human beings engage in all kinds of cognitive gymnastics aimed at justifying their own behavior.

Perhaps most important, dissonance theory inspired an enormous number and variety of hypotheses that were specific to the theory and could be tested in the laboratory. The wide array of research that dissonance theory has produced is truly astonishing. Dissonance research runs the gamut from decision-making to color preferences; from the socialization of children to curing people's snake phobias; from interpersonal attraction to antecedents of hunger and thirst; from the proselytizing behavior of religious zealots to the behavior of gamblers at racetracks; from inducing people to conserve water by taking showers to selective informational exposure; from helping people curb their temptation to cheat at a game of cards to inducing people to practice safer sex.

The impact of dissonance theory went even beyond the generation of new and exciting knowledge. Given the nature of the hypotheses we were testing, dissonance researchers were forced to develop a new experimental methodology; a powerful, high-impact set of procedures that allowed us to ask truly important questions in a very precise manner. As we all know, the laboratory tends to be an artificial environment. But dissonance research made it necessary to overcome that

artificiality by developing a methodology that would enmesh subjects in a set of events—a drama, if you will—which made it impossible for them to avoid taking these events seriously.

In my writing on research methods (Aronson and Carlsmith, 1968; Aronson, Ellsworth, Carlsmith, and Gonzales, 1990) I've called this tactic *experimental reality* where, within the admittedly phony confines of the lab, real things are happening to real people. Because of the nature of our hypotheses, we could not afford the luxury—so common in contemporary research—of having subjects passively look at a videotape of events happening to someone else and then make judgments about them. Rather, our research questions required the construction of an elaborate scenario in which subjects became immersed. Thus, what dissonance research brought into focus more clearly than any other body of work is the fact that the social psychological laboratory, with all of its contrivances and complex scenarios, can produce clear, powerful effects which are conceptually replicable in both the laboratory and in the real world.

DISSONANCE AND THE SELF: THE POWER OF SELF-PERSUASION

Research has shown that the persuasive effects in the above experiments are more powerful and more persistent than those resulting from persuasion techniques based on rewards, punishments, or source credibility (e.g., see Freedman, 1965). The major reason is that the arousal of dissonance always entails relatively high levels of *personal involvement,* and therefore the reduction of dissonance requires some form of *self-justification.* Let me elaborate. From the very outset, some of us who were working closely with the theory felt that, at its core, it led to clear and unambiguous predictions—but around the edges it was a little too vague. Several situations arose where it wasn't entirely clear what dissonance theory would predict, or indeed whether dissonance theory even made a prediction. Around 1958, the standing joke among Leon's research assistants was, "If you really want to be sure whether A is dissonant with B, ask Leon!" Although this was said with our tongues firmly planted in our cheeks, it reflected the fact that we argued a lot about whether dissonance theory applied in a wide variety of situations.

What comes to mind, most specifically, are two strenuous running arguments that Leon and I had about two of his classic examples. The first involved a person stepping out of doors in a rainstorm and not getting wet. Festinger was convinced this would arouse a great deal of dissonance, while I had considerable difficulty seeing it. My disagreement went something like this: "What's that got to do with *him*? It's a strange phenomenon, all right—but unless he feared he was losing his mind, I don't see the dissonance."

The second was Festinger's classic example of a situation where dissonance theory *didn't* apply. This was the case of a man driving, late at night, on a lonely country road and getting a flat tire (Festinger, 1957, pp. 277–278). Lo and behold, when he opened the trunk of his car, he discovered he didn't have a jack. Leon maintained that, although the person would experience frustration, disappointment, perhaps even fear—there are no dissonant cognitions in that situation. My argument was succinct: "Of course there is dissonance! What kind of idiot would go driving late at night on a lonely country road without a jack in his car?" "But," Leon countered, "where are the dissonant cognitions?"

It took me a couple of years, but it gradually dawned on me that what was at the heart of my argument in both of those situations was the self-concept. That is, when I said above that dissonance theory made clear predictions *at its core,* what I implicitly meant by "at its core" were situations where the individual's self-concept was at issue. Thus, in the raindrop situation, as far as I could judge, the self was not involved. In the flat tire situation, the self-concept *was* involved; what was dissonant was (A) the driver's cognition about his idiotic behavior with (B) his self-concept of being a reasonably smart guy. Accordingly, I wrote a monograph (Aronson, 1960), in which I argued that dissonance theory makes its strongest predictions when an important element of the self-concept is threatened, typically when an individual performs a behavior that is inconsistent with his or her sense of self. Initially, I did not intend this to be a major modification of the theory—but only an attempt to tighten the predictions a bit. That is, in my opinion, this "tightening" retained the core notion of inconsistency but shifted the emphasis to the self-concept—thus clarifying more precisely when the theory did or did not apply. I believe that this minor modification of dissonance theory turned out to have important ramifications inasmuch as it increased the predictive power of the theory without seriously limiting its scope.

In addition, this modification uncovered a hidden assumption contained in the original theory. Festinger's original statement—and all of the early experiments—rested on the implicit assumption that individuals have a reasonably positive self-concept. But, if an individual considered himself to be a "schnook," he might expect himself to do schnooky things—like go through a severe initiation to get into a group, or say things that he didn't quite believe. For such individuals, dissonance would not be aroused under the same conditions as for persons with a favorable view of themselves. Rather, dissonance would occur when negative self-expectancies were violated—that is, when the person with a poor self-concept engages in a behavior that reflects positively on the self.

To test this assumption, Merrill Carlsmith and I conducted a simple little experiment which demonstrated that, under certain conditions, college students would be made uncomfortable with success; that they would prefer to be accurate in predicting their own behavior, even if it meant setting themselves up for failure. Specifically, we found that students who had developed negative self-expectancies regarding their performance on a task showed evidence of dissonance arousal when

faced with success on that task. That is, after repeated failure at the task, subjects who later achieved a successful performance actually changed their responses from accurate to inaccurate ones, in order to preserve a consistent, though negative, self-concept (Aronson and Carlsmith, 1962). [Swann and his students later confirmed this basic finding in a number of experiments and quasi-experiments (Swann, 1984; Swann, 1991; Swann and Pelham, 1988; Swann and Read, 1981).]

A few years later, I carried this reasoning a step further (Aronson, 1968; Aronson, Chase, Helmreich, and Ruhnke, 1974), elaborating on the centrality of the self-concept in dissonance processes and suggesting that, in this regard, people generally strive to maintain a sense of self that is both consistent *and* positive. That is, because most people have relatively favorable views of themselves, they want to see themselves as:

(1) competent
(2) moral
(3) able to predict their own behavior.

In sum, efforts to reduce dissonance involve a process of self-justification because, in most instances, people experience dissonance after engaging in an action that leaves them feeling stupid, immoral, or confused (see Aronson, Chase, Helmreich, and Ruhnke, 1974). Moreover, the greater the personal commitment or self-involvement implied by the action and the smaller the *external* justification for that action, the greater the dissonance and, therefore, the more powerful the need for self-justification. Thus, in the Festinger-Carlsmith experiment, the act of deceiving another person would make one feel immoral or guilty. To reduce that dissonance, one must convince oneself that little or no deception was involved—in other words, that the task *was*, in fact, a rather interesting activity. By justifying one's actions in this fashion, one is able to restore a sense of self as morally good. In the Aronson and Mills experiment, going through hell and high water to gain admission to a boring discussion group is dissonant with one's self-concept as a smart and reasonable person who makes smart and reasonable decisions.

SCOPE VS. TIGHTNESS IN THEORY-BUILDING

All theories are lies. That is, all theories are only approximations of the empirical domain they are trying to describe. Accordingly, it is inevitable that theories will evolve and change to more accurately reflect new data that are being generated. Indeed, it is the *duty* of theorists to modify their theory in the face of new data and new ideas. Festinger understood this better than most scientists. At the same time, understandably, he was deeply enamored of both the elegant simplicity and the breadth of his original theoretical statement. Accordingly, when I first came out with the self-concept notion of dissonance, Festinger was not pleased. He felt that,

although my revision had led to some interesting research, he also felt that, conceptually, I was limiting the scope of the theory far too much. I agreed that the scope was a bit smaller, but I believed that the increased accuracy of prediction (the added tightness) was worth the slightly more limited scope. In 1987, while serving as the discussant at an APA symposium on cognitive dissonance, Festinger acknowledged the dilemma of the theorist who has a hard time seeing his theory change—yet, who knows that change it must. (One might say that this is a situation bound to produce considerable dissonance in the theorist!) In typical fashion, Leon poked fun at himself for trying to cling to the original conceptualization even though he knew better:

> No theory is going to be inviolate. Let me put it clearly. The only kind of theory that can be proposed and ever will be proposed that absolutely will remain inviolate for decades, certainly centuries, is a theory that is not testable. If a theory is at all testable, it will not remain unchanged. It has to change. All theories are wrong. One doesn't ask about theories, can I show that they are wrong or can I show that they are right, but rather one asks, how much of the empirical realm can it handle and how must it be modified and changed as it matures.
>
> As a lot of people know, I ended up leaving social psychology, meaning dissonance theory, and I want to clarify that. Lack of activity is not the same as lack of interest. Lack of activity is not desertion. I left and stopped doing research on the theory of dissonance because I was in a total rut. The only thing I could think about was how correct the original statement had been—how every word in that book was perfect. So to me I did a good thing for cognitive dissonance by leaving it. I think if I had stayed in it I might have retarded progress for cognitive dissonance for at least a decade.

In theory building, there is always a tension between scope and precision; generally speaking, the more precise one usually gains precision at the price of scope. In my own (admittedly biased) opinion, the self-concept notion strikes exactly the right balance between scope and precision.

THE SELF-CONCEPT AND THE INDUCTION OF HYPOCRISY

In recent years, the self-concept notion of dissonance has led us into areas of investigation that would not have been feasible under the rubric of Festinger's initial formulation. One of these involves the induction of feelings of hypocrisy. This discovery came about quite by accident. At the time, I was not even thinking about theory development but was struggling to find an effective way to convince sexually active college students to use condoms in the era of AIDS. The problem is not an easy one to solve—because it transcends the simple conveying of information to rational people. College students already have the requisite information; that is, virtually all sexually active college students know that condoms are an effective

way to prevent AIDS. The problem is that the vast majority are not using condoms—because they consider them to be a nuisance, unromantic and unspontaneous. In my research, I had run into a stone wall; I had tried several of the traditional, direct persuasive techniques (powerful videos, aimed at arousing fear or at eroticizing the condom) with very limited success. Whatever impact my videos did have was of very short duration; our subjects would try condoms once or twice and then stop using them.

Eventually, I thought about using the counter-attitudinal attitude paradigm. That is, why not try to get people to argue against their own attitudes—as in the Festinger-Carlsmith experiment? On the surface, it seemed like a great idea. After all, we had found that this strategy was powerful and, when judiciously applied, had long-term effects on attitudes and behavior—precisely what was needed in this societal situation. But wait a minute: In the condom-use situation, there were no counter-attitudinal attitudes to address. That is, our surveys and interviews had demonstrated that sexually active young adults already were in favor of people using condoms to prevent AIDS. *They simply weren't using them.* They seemed to be in a state of denial—denying that the dangers of unprotected sex applied to them—in the same way they applied to everyone else. How could we invoke the counter-attitudinal attitude paradigm if there was no counter-attitude to invoke?

It occurred to me that the solution had to come from the self-concept—because being in denial is not an attractive thing to be doing. The challenge was to find a way to place individuals in situations where the act of denial would be unfeasible because it would conflict, in some way, with their positive images of themselves. And then it struck me. Suppose you are a sexually active college student and, like most, (a) you do not use condoms regularly and (b) being in denial, you have managed to blind yourself to the dangers inherent in having unprotected sex. Suppose, on going home for Christmas vacation, you find that Charlie, your 16-year-old kid brother has just discovered sex and is in the process of boasting to you about his many and varied sexual encounters. What do you say to him? Chances are, as a caring, responsible older sibling, you will dampen his enthusiasm a bit by warning him about the dangers of AIDS and other sexually transmitted diseases, and urge him to, at least, take proper precautions—by using condoms.

Suppose that I am a friend of the family who was invited to dinner and who happened to overhear this exchange between you and your kid brother. What if I were to pull you aside and say, "That was very good advice you gave Charlie—I am very proud of you for being so responsible; by the way, how frequently do *you* use condoms?" In other words, by getting you to think about that, I am confronting you with your own hypocrisy. According to the self-concept version of the theory, this would produce dissonance because you are not practicing what you are preaching. That is, for most people, their self-concept does not include behaving like a hypocrite.

My students and I then proceeded to design and conduct a simple little experiment following the scenario outlined above (Aronson, Fried, and Stone, 1990). In a 2×2 factorial design, in one condition, college students were induced to make a

videotape in which they urged their audience to use condoms; they were told that the video would be shown to high school students. In the other major condition, the college students simply rehearsed the arguments without making the video. Cutting across these conditions was the "mindfulness" manipulation: In one set of conditions, our subjects were made mindful of the fact that they themselves were not practicing what they were preaching; to accomplish this we asked them to think about all those situations where they found it particularly difficult or impossible to use condoms in the recent past. In the other set of conditions, we did nothing to make the students mindful of their past failures to use condoms.

The one cell where we expected to produce dissonance was the one high in hypocrisy—i.e., where subjects made the video and were given the opportunity to dredge up memories of situations where they failed to use condoms. Again, how did we expect them to reduce dissonance? By increasing the strength of their intention to use condoms in the future. And that is precisely what we got. Those subjects who were in the high dissonance (hypocrisy) condition showed the greatest intention to increase their use of condoms. Moreover, two months later, there was a tendency for the subjects in the high dissonance cell to report using condoms a higher percentage of the time than in any of the other three cells.

In a follow-up experiment (Stone, Aronson, Crain, Winslow and Fried, 1994) we strengthened the manipulations of the initial experiment and used a "behavioroid" measure of the dependent variable. Specifically, in each of the conditions described above, subjects were subsequently provided with an opportunity to purchase condoms at a very substantial discount (10 cents each). The results were unequivocally as predicted. Fully 83% of the subjects in the hypocrisy condition purchased condoms; this was a significantly greater percentage than in each of the other three conditions, none of which were reliably different from each other. The effect was a powerful and long lasting one: Three months after the induction of hypocrisy, a telephone survey indicated that 92% of the subjects in the hypocrisy condition were still using condoms regularly—a figure that was significantly different from the control conditions.

Subsequently, we increased our confidence in the efficacy of the "induction of hypocrisy" paradigm, testing the paradigm in a different situation, a situation where we could get a direct behavioral measure of the dependent variable. We found one in the shower room of our campus field house. As you may know, central California has a chronic water shortage. On our campus, the administration is constantly trying to find ways to induce students to conserve water. So we decided to test our hypothesis by using dissonance theory and the induction of hypocrisy to convince students to take shorter showers. What we discovered is that although it is impossible, within the bounds of propriety, to follow people into their bedrooms and observe their condom-using behavior, it was easily possible to follow people into the shower rooms and time their shower-taking behavior.

In this experiment (Dickerson, Thibodeau, Aronson and Miller, 1992), we went to the university field house and intercepted college women who had just finished

swimming in a highly chlorinated pool and were on their way to take a shower. Just like in the condom experiment, it was a 2 × 2 design in which we varied commitment and mindfulness. In the commitment conditions, each student was asked if she would be willing to sign a flyer encouraging people to conserve water at the field house. The students were told that the flyers would be displayed on posters; each was shown a sample poster—a large, colorful, very public display. The flyer read: "Take shorter showers. Turn off water while soaping up. If I can do it, so can you!" After she signed the flyer, we thanked her for her time, and she proceeded to the shower room, where our undergraduate research assistant (blind to condition) was unobtrusively waiting (with hidden waterproof stopwatch) to time the student's shower.

In the mindful conditions we also asked the students to respond to a water conservation "survey," which consisted of items designed to make them aware of their proconservation attitudes and the fact that their typical showering behavior was sometimes wasteful.

The results are consistent with those in the condom experiment: We found dissonance effects only in the cell where the subjects were preaching what they were not always practicing. That is, in the condition where the students were induced to advocate short showers and were made mindful of their own past behavior, they took very short showers. To be specific, in the high dissonance cell, the length of the average shower (which, because of the chlorine in the swimming pool, included a shampoo and conditioner) averaged just over three and a half minutes (that's short!) and was significantly shorter than in the control condition.

How can we be certain that dissonance is involved in these experiments? Although the data are consistent with the self-concept formulation of dissonance theory, there is another plausible interpretation. It is conceivable that the effects of the hypocrisy manipulation may have been due to the effects of priming. The combination of proattitudinal advocacy and the salience of past behavior may have served, in an additive fashion, to make subjects' positive attitudes toward condom use or water conservation highly accessible, thus fostering a stronger correspondence between their attitudes and behavior (e.g., Fazio, 1986). What is needed to pin it down was evidence that the hypocrisy effect involves physiological arousal, thereby indicating the presence of dissonance rather than the mere influence of attitude salience.

An experiment by Fried and Aronson (1995) provide exactly this sort of evidence. Within the context of the hypocrisy paradigm, this experiment employed a "misattribution of arousal" manipulation, a strategy brilliantly developed in earlier research to document the existence of dissonance as an uncomfortable state of arousal by Zanna and Cooper (1974). Zanna and Cooper found that, when subjects are given an opportunity to misattribute their arousal to a source other than their dissonance-arousing behavior—for example, to an overheated room, a placebo, or glaring fluorescent lights—the attitude change typically associated with dissonance reduction no longer occurs.

Using a modified version of the earlier condom experiments, Fried and Aronson's study required subjects to compose and deliver proattitudinal, videotaped speeches advocating the importance of recycling. These speeches were ostensibly to be shown to various groups as part of a campaign to increase participation in recycling programs on campus and in the larger community. Hypocrisy was induced in half the subjects by asking them to list recent examples of times when they had failed to recycle; the other half simply wrote and delivered the speech, without being reminded of their wasteful behavior. In addition, half the subjects in each condition were given an opportunity to misattribute arousal to various environmental factors within the laboratory setting. Specifically, subjects were asked to answer questions regarding the room's lighting, temperature, and noise level, including how these ambient factors might have affected them. (This was accomplished under the guise of asking subjects to rate the room's suitability for use as laboratory space—a request that was made to appear unrelated to the activities in which subjects were participating.) To summarize, proattitudinal advocacy was held constant, and the salience of past behavior and the opportunity for misattribution were manipulated, yielding a 2×2 factorial design with the following conditions: (1) hypocrisy (high salience); (2) hypocrisy with misattribution; (3) no hypocrisy (low salience); and (4) no hypocrisy with misattribution. Dissonance reduction was measured by asking subjects to volunteer to help a local recycling organization by making phone calls soliciting support for recycling.

The results of this experiment revealed that arousal was indeed present within the hypocrisy conditions. Hypocrisy subjects who were not afforded the opportunity to misattribute the source of their arousal volunteered significantly more often, and for longer blocks of time, than subjects in the other experimental conditions. Moreover, volunteer behavior for hypocrisy subjects who were allowed to misattribute their arousal was no greater than for subjects who were not exposed to the hypocrisy manipulation.

HYPOCRISY: DISSONANCE IN THE ABSENCE OF AVERSIVE CONSEQUENCES

As I stated above, the initial reason for the development of the hypocrisy paradigm was couched in my attempt to apply dissonance theory to the solution of a societal problem. The hidden bonus was that it also sheds some light on an interesting theoretical controversy among dissonance theorists. I should say, at the outset, that dissonance research tends to be a family business—thus, the controversies are invariably friendly arguments—around the dinner table as it were. One such controversy involves the "new look" theory developed several years ago by Cooper and Fazio (1984). In examining the early forced compliance experiments, like the Festinger-Carlsmith experiment and the Nel, Helmreich, and Aronson experiment,

Cooper and Fazio made an interesting discovery: In these experiments, not only was inconsistency present, but aversive consequences were also always present; that is, lying to another person is usually aversive because it does the recipient harm. Their next step was a bold one: Cooper and Fazio asserted that dissonance is not due merely to inconsistent cognitions at all—but rather, is aroused only when an individual feels personally responsible for bringing about an aversive or unwanted event. Or, to put it in my terms, dissonance is caused solely by harming another person—which is a threat to the person's self-concept as a morally good human being.

Although I always appreciated the boldness implicit in Cooper and Fazio's theorizing, I could never bring myself to buy into the notion that aversive consequences are essential for the existence of dissonance. Moreover, in terms of our earlier discussion of "scope vs. tightness," it seems to me that Cooper and Fazio's conception is limiting the scope of the theory enormously, while gaining nothing in tightness that wasn't already present in the self-concept notion.

The ultimate solution to a theoretical dispute is to do an experiment. How do you test this difference empirically? Several years ago, I was at a loss as to how to produce inconsistency in the Festinger-Carlsmith type of experiment without also producing aversive consequences for the recipient of one's message. After all, if you are misleading another person, by telling him something you believe is false, then you are always bringing about aversive consequences, aren't you? But, without quite realizing it, my students and I seem to have stumbled onto the solution with the hypocrisy experiments. In this procedure, the subjects are preaching what they are not practicing (and are therefore experiencing dissonance), but where are the aversive consequences for the audience in the condom experiment? There are none. Indeed, to the extent that the "hypocrites" succeed in being persuasive, far from producing aversive consequences for the recipients they may well be saving their lives. And still, it is clear from the data that our subjects were experiencing dissonance. For a fuller discussion of this theoretical controversy see Thibodeau and Aronson (1992).

SELF-JUSTIFICATION, SELF-ESTEEM, AND SELF-COMPASSION

From the very beginning I found dissonance theory to be a powerful explanation for a wide swath of human behavior. The scientist in me was always delighted by the exciting, nonobvious predictions generated by the theory, as well as the creative experimentation used to test these predictions. But, at the same time, the humanist in me was always a bit troubled by the rather bleak, rather unappetizing picture the theory painted of the human condition—forever striving to justify our actions after the fact. Over the past few years, my reasoning about dissonance and

the self-concept has led me to speculate about how a person's self-esteem might interact with the experiencing and reduction of dissonance. These speculations might suggest a more complete picture of human nature. Let me begin this speculation by calling your attention to two intriguing experiments in the dissonance literature. First, let us consider an experiment I did a great many years ago with David Mettee (Aronson and Mettee, 1968), in which we demonstrated that, if we temporarily raised an individual's self-esteem, it would serve to insulate him from performing an immoral act like cheating. We found that the higher self-esteem served to make the anticipation of doing something immoral more dissonant than it would have been otherwise. Thus, when our subjects were put in a situation in which they had an opportunity to win money by unobtrusively cheating at a game of cards, they were able to say to themselves, in effect, "Wonderful people like me don't cheat!" And they succeeded in resisting the temptation to cheat to a greater extent than those in the control condition.

Now, let us consider an experiment performed by David Glass (1964). In this study, people were put in a situation where they were induced to deliver a series of electric shocks to other people. They then had an opportunity to evaluate their victims. Dissonance theory predicts that if individuals are feeling awful about having hurt someone, one way to reduce the dissonance is to convince themselves that their victim is a dreadful person who deserved to suffer the pain of electric shock. What Glass found was that it was precisely those individuals who had the highest self-esteem who derogated their victims the most. Consider the irony: It is precisely because I think I am such a nice person that, if I do something that causes you pain, I must convince myself you are a rat. In other words, because nice guys like me don't go around hurting innocent people, you must have deserved every nasty thing I did to you. On the other hand, if I already consider myself to be something of a scoundrel, then causing others to suffer does not introduce as much dissonance; therefore, I have less of a need to convince myself that you deserved your fate. The ultimate tragedy, of course, is that, once I succeed in convincing myself that you are a dreadful person, it lowers my inhibitions against doing you further damage.

Taking the two experiments together, Aronson and Mettee showed that high self-esteem can serve as a buffer against immoral behavior, while Glass showed that once a person commits an immoral action, high self-esteem leads him into a situation where he might commit further mischief. In pondering these two experiments, I have come to the conclusion that it is far too simplistic to think of self-esteem as a one-dimensional phenomenon—either high or low. I would like to suggest that self-esteem can be high or low *and* either fragile or well-grounded. "Well-grounded" in this context means that a positive self-image has been developed and held during a great deal of past behavior, whereas "fragile" suggests that a positive self-image has never been securely developed. People with high/well-grounded self-esteem need not be concerned with developing or verifying their self-images, and can enter situations with the confident knowledge that they are competent, moral people. On the other hand, because of their lack of a secure self-image, peo-

ple with high/fragile self-esteem are overly concerned with trying to preserve images of themselves as being competent and moral at all costs.

Typically, people with high/fragile self-esteem in their zeal to maintain a belief in their terrificness, often boast about their achievements—trying desperately to convince themselves and others that they are terrific. But their boasting behavior, their misjudgments, their errors, the wrong turns they make because they are thinking more about themselves than the situation they are in—all this tends to shatter the fragile images they are trying to defend. As a result, they frequently feel like imposters, and are forever trying to prove that they aren't. Thus, they are always trying to win every possible argument, pushing themselves to believe they are always right, avoiding and explaining away failures and mistakes instead of attending to them long enough to learn from them, justifying their behavior to themselves at every turn.

In contrast, people with high/well-grounded self-esteem are not invested in winning arguments for winning's sake, don't need to believe they're always right, don't need to explain away failures and mistakes, and don't need to engage in the almost frantic self-justification in which high/fragile self-esteem people constantly engage. Instead, when they fail or make mistakes, people with high/well-grounded self-esteem can look at their failures and mistakes and learn from them. For example, a person with high, well-grounded self-esteem can look at his or her errors, and say, in effect, "Oh, my god, I screwed up—I did a stupid (or hurtful, or immoral) thing. But just because I did a stupid (or hurtful, or immoral) thing this time, this doesn't make me a stupid (or hurtful, or immoral) person. Let me look at it. How did it come about? How can I make it better? What can I learn from this situation so that I might decrease the possibility that I'll screw up in a similar way again?"

I would speculate that the majority of Glass's high self-esteem participants would have self-esteem that fits with my notion of high/fragile. This is why they rushed to derogate their victim. But if a person had high self-esteem that was well-grounded—that is, if she or he had a very secure, positive self-image—they would not use derogation of the victim as a way to reduce their dissonance caused by the psychological inconsistency between the cognitions "I just did a cruel thing" and "I am a warm, generous person."

Continuing my speculation, I would argue that the key to developing high/well-grounded self-esteem lies in the ability to look at one's shortcomings and mistakes and failures with full attention instead of turning away and trying to ignore them or explain them away. To be able to do this requires a good measure of constructive self-compassion. One must be able to look at one's errors with understanding and sympathy and have the ability to forgive oneself for not being perfect before picking up, learning from those errors, and going on. Self-compassion is not complacency. Rather, self-compassion implies the ability to examine one's imperfect behavior, feel truly bad about any harm one has done or any stupidities one has committed—and not rush to justify the behavior. People who have constructive self-compassion will examine their stupidities and failures, consider ways to atone

for the hurt they have caused others, and eventually pick themselves up, brush themselves off, learn from their mistakes, and move on. In other words, people with constructive self-compassion avoid three kinds of mistakes: trying to write off all errors as the other guy's fault; glibly forgiving themselves for everything, and moving on without learning anything; and neurotic self-flagellation.

I believe that self-compassion can be taught. That is, I believe it is possible to teach people to become more self-compassionate by helping them see mistakes not as terrible things to be denied or justified, but rather, as inevitable at worst and as golden opportunities for learning at best. It is an uphill struggle—because our culture is a judgmental one that does not take kindly to human error. As I have written elsewhere (Fried and Aronson, 1995), I find it ironic that perhaps the most prominent place where the American culture recognizes error as something that is inevitable is in our national pastime, baseball, where the very box score consists of runs, hits, and errors. In short, in the box score, errors, although certainly not desirable, are seen as part of the game—a part of the game of baseball—and perhaps a part of the game of life, as well. As I write this, the news is full of General Joseph Ralston, who was President Clinton's top choice to become Chairman of the Joint Chiefs of Staff. He has just withdrawn his name from consideration after having been pilloried by congress and the media because apparently he had a love affair some 13 years ago while separated from his wife. From the outcry, it would appear that most Americans consider his actions immoral. But are they unforgivable? Do his actions disqualify him from holding the position as our nation's top military officer? If so, in what way? These are questions that must be addressed seriously and with some compassion. It is at least conceivable that many of the people who are judging General Ralston most harshly are not, themselves, beyond reproach. How do we account for that hypocrisy?

Needless to say, it is risky for a writer to be referring to a very current event in an essay that he intends to be around for a few years; there is nothing as stale or forgettable as yesterday's scandal. But I wanted to mention the Ralston case because it is not an isolated incident; rather, I think it illustrates the kind of hypocritical witch-hunting that has become all too common in contemporary American life. I have a strong guess that if we can learn to become more adept at facing up to our own mistakes in order to learn from them, perhaps we can develop the kind of well-grounded self-esteem that will enable us to resist rushing to reduce dissonance in situations that result in harsh judgment and a high degree of cognitive distortion. At the same time, it may render us a bit more tolerant of the mistakes of others and perhaps a bit less hypocritical in our orientation to the less-than-perfect behavior of our public figures.

Admittedly, at this point, my reasoning in this entire section is purely speculative. At the same time, it is comforting to know that I am not alone in these kinds of speculations. Meg Rohan (1996), a young Australian social psychologist, has independently begun a research project on various aspects of self-esteem that seems

headed in the same direction—attesting, among other things, to the continuing vitality of Festinger's revolutionary little theory.

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17

Compliance Without Pressure: The Foot-in-the-Door Technique

Jonathan L. Freedman and Scott C. Fraser

Two experiments were conducted to test the proposition that once someone has agreed to a small request he is more likely to comply with a larger request. The first study demonstrated this effect when the same person made both requests. The second study extended this to the situation in which different people made the two requests. Several experimental groups were run in an effort to explain these results, and possible explanations are discussed.

How can a person be induced to do something he would rather not do? This question is relevant to practically every phase of social life, from stopping at a traffic light to stopping smoking, from buying Brand X to buying savings bonds, from supporting the March of Dimes to supporting the Civil Rights Act.

One common way of attacking the problem is to exert as much pressure as possible on the reluctant individual in an effort to force him to comply. This technique has been the focus of a considerable amount of experimental research. Work on attitude change, conformity, imitation, and obedience has all tended to stress the importance of the degree of external pressure. The prestige of the communicator (Kelman and Hovland, 1953), degree of discrepancy of the communication (Hovland and Pritzker, 1957), size of the group disagreeing with the subject (Asch, 1951), and perceived power of the model (Bandura, Ross, and Ross, 1963) are the kinds of variables that have been studied. This impressive body of work, added to

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the research on rewards and punishments in learning, has produced convincing evidence that greater external pressure generally leads to greater compliance with the wishes of the experimenter. The one exception appears to be situations involving the arousal of cognitive dissonance in which, once discrepant behavior has been elicited from the subject, the greater the pressure that was used to elicit the behavior, the less subsequent change occurs (Festinger and Carlsmith, 1959). But even in this situation one critical element is the amount of external pressure exerted.

Clearly, then, under most circumstances the more pressure that can be applied, the more likely it is that the individual will comply. There are, however, many times when for ethical, moral, or practical reasons it is difficult to apply much pressure when the goal is to produce compliance with a minimum of apparent pressure, as in the forced-compliance studies involving dissonance arousal. And even when a great deal of pressure is possible, it is still important to maximize the compliance it produces. Thus, factors other than external pressure are often quite critical in determining degree of compliance. What are these factors?

Although rigorous research on the problem is rather sparse, the fields of advertising, propaganda, politics, etc., are by no means devoid of techniques designed to produce compliance in the absence of external pressure (or to maximize the effectiveness of the pressure that is used, which is really the same problem). One assumption about compliance that has often been made either explicitly or implicitly is that once a person has been induced to comply with a small request he is more likely to comply with a larger demand. This is the principle that is commonly referred to as the foot-in-the-door or gradation technique and is reflected in the saying that if you "give them an inch, they'll take a mile." It was, for example, supposed to be one of the basic techniques upon which the Korean brainwashing tactics were based (Schein, Schneier, and Barker, 1961), and, in a somewhat different sense, one basis for Nazi propaganda during 1940 (Bruner, 1941). It also appears to be implicit in many advertising campaigns which attempt to induce the consumer to do anything relating to the product involved, even sending back a card saying he does not want the product.

The most relevant piece of experimental evidence comes from a study of conformity done by Deutsch and Gerard (1955). Some subjects were faced with incorrect group judgments first in a series in which the stimuli were not present during the actual judging and then in a series in which they were present, while the order of the memory and visual series was reversed for other subjects. For both groups the memory series produced more conformity, and when the memory series came first there was more total conformity to the group judgments. It seems likely that this order effect occurred because, as the authors suggest, once conformity is elicited at all it is more likely to occur in the future. Although this kind of conformity is probably somewhat different from compliance as described above, this finding certainly lends some support to the foot-in-the-door idea. The present research attempted to provide a rigorous, more direct test of this notion as it applies to compliance and to provide data relevant to several alternative ways of explaining the effect.

EXPERIMENT I

The basic paradigm was to ask some subjects (Performance condition) to comply first with a small request and then three days later with a larger, related request. Other subjects (One-Contact condition) were asked to comply only with the large request. The hypothesis was that more subjects in the Performance condition than in the One-Contact condition would comply with the larger request.

Two additional conditions were included in an attempt to specify the essential difference between these two major conditions. The Performance subjects were asked to perform a small favor, and, if they agreed, they did it. The question arises whether the act of agreeing itself is critical or whether actually carrying it out was necessary. To assess this a third group of subjects (Agree-Only) was asked the first request, but, even if they agreed, they did not carry it out. Thus, they were identical to the Performance group except that they were not given the opportunity of performing the request.

Another difference between the two main conditions was that at the time of the larger request the subjects in the Performance condition were more familiar with the experimenter than were the other subjects. The Performance subjects had been contacted twice, heard his voice more, discovered that the questions were not dangerous, and so on. It is possible that this increased familiarity would serve to decrease the fear and suspicion of a strange voice on the phone and might accordingly increase the likelihood of the subjects agreeing to the larger request. To control for this a fourth condition was run (Familiarization) which attempted to give the subjects as much familiarity with the experimenter as in the Performance and Agree-Only conditions with the only difference being that no request was made.

The major prediction was that more subjects in the Performance condition would agree to the large request than in any of the other conditions, and that the One-Contact condition would produce the least compliance. Since the importance of agreement and familiarity was essentially unknown, the expectation was that the Agree-Only and Familiarization conditions would produce intermediate amounts of compliance.

Method

The prediction stated above was tested in a field experiment in which housewives were asked to allow a survey team of five or six men to come into their homes for two hours to classify the household products they used. This large request was made under four different conditions: after an initial contact in which the subject had been asked to answer a few questions about the kinds of soaps she used, and the questions were actually asked (Performance condition); after an

identical contact in which the questions were not actually asked (Agree-Only condition); after an initial contact in which no request was made (Familiarization condition); or after no initial contact (One-Contact condition). The dependent measure was simply whether the subject agreed to the large request.

Procedure. The subjects were 156 Palo Alto, California, housewives, 36 in each condition, who were selected at random from the telephone directory. An additional 12 subjects distributed about equally among the three two-contact conditions could not be reached for the second contact and are not included in the data analysis. Subjects were assigned randomly to the various conditions, except that the Familiarization condition was added to the design after the other three conditions had been completed. All contacts were by telephone by the same experimenter who identified himself as the same person each time. Calls were made only in the morning. For the three groups that were contacted twice, the first call was made on either Monday or Tuesday and the second always three days later. All large requests were made on either Thursday or Friday.

At the first contact, the experimenter introduced himself by name and said that he was from the California Consumers' Group. In the Performance condition he then proceeded:

> We are calling you this morning to ask if you would answer a number of questions about what household products you use so that we could have this information for our public service publication, "The Guide." Would you be willing to give us this information for our survey?

If the subject agreed, she was asked a series of eight innocuous questions dealing with household soaps (e.g., "What brand of soap do you use in your kitchen sink?") She was then thanked for her cooperation, and the contact terminated.

Another condition (Agree-Only) was run to assess the importance of actually carrying out the request as opposed to merely agreeing to it. The only difference between this and the Performance condition was that, if the subject agreed to answer the questions, the experimenter thanked her, but said that he was just lining up respondents for the survey and would contact her if needed.

A third condition was included to check on the importance of the subject's greater familiarity with the experimenter in the two-contact conditions. In this condition the experimenter introduced himself, described the organization he worked for and the survey it was conducting, listed the questions he was asking and then said that he was calling merely to acquaint the subject with the existence of his organization. In other words, these subjects were contacted, spent as much time on the phone with the experimenter as the Performance subjects did, heard all the questions, but neither agreed to answer them nor answered them.

In all of these two-contact conditions some subjects did not agree to the requests or even hung up before the requests were made. Every subject who answered the phone was included in the analysis of the results and was contacted for the second request regardless of her extent of cooperativeness during the first contact. In other

words, no subject who could be contacted the appropriate number of times was discarded from any of the four conditions.

The large request was essentially identical for all subjects. The experimenter called, identified himself, and said either that his group was expanding its survey (in the case of the two-contact conditions) or that it was conducting a survey (in the One-Contact condition). In all four conditions he then continued:

> The survey will involve five or six men from our staff coming into your home some morning for about two hours to enumerate and classify all the household products that you have. They will have to have full freedom in your house to go through the cupboards and storage places. Then all this information will be used in the writing of the reports for our public service publication, "The Guide."

If the subject agreed to the request, she was thanked and told that at the present time the experimenter was merely collecting names of people who were willing to take part and that she would be contacted if it were decided to use her in the survey. If she did not agree, she was thanked for her time. This terminated the experiment.

Results

Apparently even the small request was not considered trivial by some of the subjects. Only about two-thirds of the subjects in the Performance and Agree-Only conditions agreed to answer the questions about household soaps. It might be noted that none of those who refused the first request later agreed to the large request, although as stated previously all subjects who were contacted for the small request are included in the data for those groups.

Our major prediction was that subjects who had agreed to and carried out a small request (Performance condition) would subsequently be more likely to comply with a larger request than would subjects who were asked only the larger request (One-Contact condition). As may be seen in Table 17.1, the results support the prediction. More than 50 percent of the subjects in the Performance condition agreed to the larger request, while less than 25 percent of the One-Contact condition

TABLE 17.1
Percentage of subjects complying with large request in Experiment I

Condition	%
Performance	52.8
Agree-only	33.3
Familiarization	27.8*
One-contact	22.2**

Note: $N = 36$ for each group. Significance levels represent differences frorn the Performance condition.

*$p < .07$; **$p < .02$.

agreed to it. Thus it appears that obtaining compliance with a small request does tend to increase subsequent compliance. The question is what aspect of the initial contact produces this effect.

One possibility is that the effect was produced merely by increased familiarity with the experimenter. The Familiarization control was included to assess the effect on compliance of two contacts with the same person. The group had as much contact with the experimenter as the Performance group, but no request was made during the first contact. As the table indicates, the Familiarization group did not differ appreciably in amount of compliance from the One-Contact group, but was different from the Performance group ($\chi^2 = 3.70$, $p < .07$). Thus, although increased familiarity may well lead to increased compliance, in the present situation the differences in amount of familiarity apparently were not great enough to produce any such increase; the effect that was obtained seems not to be due to this factor.

Another possibility is that the critical factor producing increased compliance is simply agreeing to the small request (i.e., carrying it out may not be necessary). The Agree-Only condition was identical to the Performance condition except that in the former the subjects were not asked the questions. The amount of compliance in this Agree-Only condition fell between the Performance and One-Contact conditions and was not significantly different from either of them. This leaves the effect of merely agreeing somewhat ambiguous, but it suggests that the agreement alone may produce part of the effect.

Unfortunately, it must be admitted that neither of these control conditions is an entirely adequate test of the possibility it was designed to assess. Both conditions are in some way quite peculiar and may have made a very different and extraneous impression on the subject than did the Performance condition. In one case, a housewife is asked to answer some questions and then is not asked them; in the other, some man calls to tell her about some organization she has never heard of. Now, by themselves neither of these events might produce very much suspicion. But, several days later, the same man calls and asks a very large favor. At this point it is not at all unlikely that many subjects think they are being manipulated, or in any case that something strange is going on. Any such reaction on the part of the subjects would naturally tend to reduce the amount of compliance in these conditions.

Thus, although this first study demonstrates that an initial contact in which a request is made and carried out increases compliance with a second request, the question of why and how the initial request produces this effect remains unanswered. In an attempt to begin answering this question and to extend the results of the first study, a second experiment was conducted.

There seemed to be several quite plausible ways in which the increase in compliance might have been produced. The first was simply some kind of commitment to or involvement with the particular person making the request. This might work, for example, as follows: The subject has agreed to the first request and perceives that the experimenter therefore expects him also to agree to the second request. The subject thus feels obligated and does not want to disappoint the experimenter; he

also feels that he needs a good reason for saying "no"—a better reason than he would need if he had never said "yes." This is just one line of causality—the particular process by which involvement with the experimenter operates might be quite different, but the basic idea would be similar. The commitment is to the particular person. This implies that the increase in compliance due to the first contact should occur primarily when both requests are made by the same person.

Another explanation in terms of involvement centers around the particular issue with which the requests are concerned. Once the subject has taken some action in connection with an area of concern, be it surveys, political activity, or highway safety, there is probably a tendency to become somewhat more concerned with the area. The subject begins thinking about it, considering its importance and relevance to him, and so on. This tends to make him more likely to agree to take further action in the same area when he is later asked to. To the extent that this is the critical factor the initial contact should increase compliance only when both requests are related to the same issue or area of concern.

Another way of looking at the situation is that the subject needs a reason to say "no." In our society it is somewhat difficult to refuse a reasonable request, particularly when it is made by an organization that is not trying to make money. To refuse, many people feel that they need a reason—simply not wanting to do it is often not in itself sufficient. The person can say to the requester or simply to himself that he does not believe in giving to charities or tipping or working for political parties or answering questions or posting signs, or whatever he is asked to do. Once he has performed a particular task, however, this excuse is no longer valid for not agreeing to perform a similar task. Even if the first thing he did was trivial compared with the present request, he cannot say he never does this sort of thing, and thus one good reason for refusing is removed. This line of reasoning suggests that the similarity of the first and second requests in terms of the type of action required is an important factor. The more similar they are, the more the "matter of principle" argument is eliminated by agreeing to the first request, and the greater should be the increase in compliance.

There are probably many other mechanisms by which the initial request might produce an increase in compliance. The second experiment was designed in part to test the notions described above, but its major purpose was to demonstrate the effect unequivocally. To this latter end it eliminated one of the important problems with the first study which was that when the experimenter made the second request he was not blind as to which condition the subjects were in. In this study the second request was always made by someone other than the person who made the first request, and the second experimenter was blind as to what condition the subject was in. This eliminates the possibility that the experimenter exerted systematically different amounts of pressure in different experimental conditions. If the effect of the first study were replicated, it would also rule out the relatively uninteresting possibility that the effect is due primarily to greater familiarity or involvement with the particular person making the first request.

EXPERIMENT II

The basic paradigm was quite similar to that of the first study. Experimental subjects were asked to comply with a small request and were later asked a considerably larger request, while controls were asked only the larger request. The first request varied along two dimensions. Subjects were asked either to put up a small sign or to sign a petition, and the issue was either safe driving or keeping California beautiful. Thus, there were four first requests: a small sign for safe driving or for beauty, and a petition for the two issues. The second request for all subjects was to install in their front lawn a very large sign which said "Drive Carefully." The four experimental conditions may be defined in terms of the similarity of the small and large requests along the dimensions of issue and task. The two requests were similar in both issue and task for the small-sign, safe-driving group, similar only in issue for the safe-driving-petition group, similar only in task for the small "Keep California Beautiful" sign group, and similar in neither issue nor task for the "Keep California Beautiful" petition group.

The major expectation was that the three groups for which either the task or the issue was similar would show more compliance than the controls, and it was also felt that when both were similar there would probably be the most compliance. The fourth condition (Different Issue-Different Task) was included primarily to assess the effect simply of the initial contact which, although it was not identical to the second one on either issue or task, was in many ways quite similar (e.g., a young student asking for cooperation on a noncontroversial issue). There were no clear expectations as to how this condition would compare with the controls.

Method

The subjects were 114 women and 13 men living in Palo Alto, California. Of these, 9 women and 6 men could not be contacted for the second request and are not included in the data analysis. The remaining 112 subjects were divided about equally among the five conditions. All subjects were contacted between 1:30 and 4:30 on weekday afternoons.

Two experimenters, one male and one female, were employed, and a different one always made the second contact. Unlike the first study, the experimenters actually went to the homes of the subjects and interviewed them on a face-to-face basis. An effort was made to select subjects from blocks and neighborhoods that were as homogeneous as possible. On each block every third or fourth house was approached, and all subjects on that block were in one experimental condition. This was necessary because of the likelihood that neighbors would talk to each other about the contact. In addition, for every four subjects contacted, a fifth house was

chosen as a control but was, of course, not contacted. Throughout this phase of the experiment, and in fact throughout the whole experiment, the two experimenters did not communicate to each other what conditions had been run on a given block nor what condition a particular house was in.

The small-sign, safe-driving group was told that the experimenter was from the Community Committee for Traffic Safety, that he was visiting a number of homes in an attempt to make the citizens more aware of the need to drive carefully all the time, and that he would like the subject to take a small sign and put it in a window or in the car so that it would serve as a reminder of the need to drive carefully. The sign was three inches square, said "Be a safe driver," was on thin paper without a gummed backing, and in general looked rather amateurish and unattractive. If the subject agreed, he was given the sign and thanked; if he disagreed, he was simply thanked for his time.

The three other experimental conditions were quite similar with appropriate changes. The other organization was identified as the Keep California Beautiful Committee and its sign said, appropriately enough, "Keep California Beautiful." Both signs were simply black block letters on a white background. The two petition groups were asked to sign a petition which was being sent to California's United States Senators. The petition advocated support for any legislation which would promote either safer driving or keeping California beautiful. The subject was shown a petition, typed on heavy bond paper, with at least twenty signatures already affixed. If she agreed, she signed and was thanked. If she did not agree, she was merely thanked.

The second contact was made about 2 weeks after the initial one. Each experimenter was armed with a list of houses which had been compiled by the other experimenter. This list contained all four experimental conditions and the controls, and, of course, there was no way for the second experimenter to know which condition the subject had been in. At this second contact, all subjects were asked the same thing: Would they put a large sign concerning safe driving in their front yard? The experimenter identified himself as being from the Citizens for Safe Driving, a different group from the original safe-driving group (although it is likely that most subjects who had been in the safe-driving conditions did not notice the difference). The subject was shown a picture of a very large sign reading "Drive Carefully" placed in front of an attractive house. The picture was taken so that the sign obscured much of the front of the house and completely concealed the doorway. It was rather poorly lettered. The subject was told that: "Our men will come out and install it and later come and remove it. It makes just a small hole in your lawn, but if this is unacceptable to you we have a special mount which will make no hole." She was asked to put the sign up for a week or a week and a half. If the subject agreed, she was told that more names than necessary were being gathered and if her home were to be used she would be contacted in a few weeks. The experimenter recorded the subject's response and this ended the experiment.

Results

First, it should be noted that there were no large differences among the experimental conditions in the percentages of subjects agreeing to the first request. Although somewhat more subjects agreed to post the "Keep California Beautiful" sign and somewhat fewer to sign the beauty petition, none of these differences approach significance.

The important figures are the number of subjects in each group who agreed to the large request. These are presented in Table 17.2. The figures for the four experimental groups include all subjects who were approached the first time, regardless of whether they agreed to the small request. As noted above, a few subjects were lost because they could not be reached for the second request, and, of course these are not included in the table.

It is immediately apparent that the first request tended to increase the degree of compliance with the second request. Whereas fewer than 20 percent of the controls agreed to put the large sign on their lawn, more than 55 percent of the experimental subjects agreed, with more than 45 percent being the lowest degree of compliance for any experimental condition. As expected, those conditions in which the two requests were similar in terms of either issue or task produced significantly more compliance than did the controls (χ^2s range from 3.67, $p < .07$ to 15.01, $p < .001$). A somewhat unexpected result is that the fourth condition, in which the first request had relatively little in common with the second request, also produced more compliance than the controls ($\chi^2 = 3.40$, $p < .08$). In other words, regardless of whether or not the two requests are similar in either issue or task, simply having the first request tends to increase the likelihood that the subject will comply with a subsequent, larger request. And this holds even when the two requests are made by different people several weeks apart.

A second point of interest is a comparison among the four experimental conditions. As expected, the Same Issue-Same Task condition produced more compliance than any of the other two-contact conditions, but the difference is not signifi-

TABLE 17.2
Percentage of subjects complying with large request in Experiment II

	Task[a]			
Issue[a]	Similar	*N*	Different	*N*
Similar	76.0**	25	47.8*	23
Different	47.6*	21	47.4*	19
One-contact 16.7 ($N = 24$)				

Note: Significance levels represent differences from the One-contact condition.

[a]Denotes relationship between first and second requests.

*$p < .08$; **$p < .01$.

cant (χ^2s range from 2.7 to 2.9). If only those subjects who agreed to the first request are considered, the same pattern holds.

DISCUSSION

To summarize the results, the first study indicated that carrying out a small request increased the likelihood that the subject would agree to a similar larger request made by the same person. The second study showed that this effect was quite strong even when a different person made the larger request, and the two requests were quite dissimilar. How may these results be explained?

Two possibilities were outlined previously. The matter-of-principle idea which centered on the particular type of action was not supported by the data, since the similarity of the tasks did not make an appreciable difference in degree of compliance. The notion of involvement, as described previously, also had difficulty accounting for some of the findings. The basic idea was that once someone has agreed to any action, no matter how small, he tends to feel more involved than he did before. This involvement may center around the particular person making the first request or the particular issue. This is quite consistent with the results of the first study (with the exception of the two control groups which as discussed previously were rather ambiguous) and with the Similar-Issue groups in the second experiment. This idea of involvement does not, however, explain the increase in compliance found in the two groups in which the first and second request did not deal with the same issue.

It is possible that in addition to or instead of this process a more general and diffuse mechanism underlies the increase in compliance. What may occur is a change in the person's feelings about getting involved or about taking action. Once he has agreed to a request, his attitude may change. He may become, in his own eyes, the kind of person who does this sort of thing, who agrees to requests made by strangers, who takes action on things he believes in, who cooperates with good causes. The change in attitude could be toward any aspect of the situation or toward the whole business of saying "yes." The basic idea is that the change in attitude need not be toward any particular issue or person or activity, but may be toward activity or compliance in general. This would imply that an increase in compliance would not depend upon the two contacts being made by the same person, or concerning the same issue or involving the same kind of action. The similarity could be much more general, such as both concerning good causes, or requiring a similar kind of action, or being made by pleasant, attractive individuals.

It is not being suggested that this is the only mechanism operating here. The idea of involvement continues to be extremely plausible, and there are probably a number of other possibilities. Unfortunately, the present studies offer no additional data with which to support or refute any of the possible explanations of the effect. These explanations thus remain simply descriptions of mechanisms that might

produce an increase in compliance after agreement with a first request. Hopefully, additional research will test these ideas more fully and perhaps also specify other manipulations that produce an increase in compliance without an increase in external pressure.

It should be pointed out that the present studies employed what is perhaps a very special type of situation. In all cases the requests were made by presumably nonprofit service organizations. The issues in the second study were deliberately noncontroversial, and it may be assumed that virtually all subjects initially sympathized with the objectives of safe driving and a beautiful California. This is in strong contrast to campaigns which are designed to sell a particular product, political candidate, or dogma. Whether the technique employed in this study would be successful in these other situations remains to be shown.

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18

Reducing Weight by Reducing Dissonance: The Role of Effort Justification in Inducing Weight Loss

Danny Axsom and Joel Cooper

The role of effort justification in psychotherapy was examined. It was hypothesized that the effort involved in therapy, plus the conscious decision to undergo that effort, leads to positive therapeutic changes through the reduction of cognitive dissonance. An experiment was conducted in which overweight subjects attempted to lose weight through one of two forms of "Effort Therapy." These therapies were bogus in that they were based solely on the expenditure of effort on a series of cognitive tasks that were unrelated to any existing techniques or theory addressing weight loss. One of the therapies called for a high degree of effort, while the degree of effort in the second therapy was low. Decision freedom to enter into and continue with the study was also varied. It was predicted that weight loss would occur only when both effort and decision freedom were high. Results supported these predictions, although some ambiguity arose from the failure to clearly manipulate decision freedom. Over an initial three-week period, High Effort subjects lost slightly more weight than Low Effort subjects. A six-month follow-up revealed that the effects of effort on weight loss had increased and were highly significant. Internal analyses indicated a further influence of the decision freedom variable. Possible mechanisms mediating the dissonance effect were discussed, as were several alternative explanations.

Several theorists have noted that psychotherapy is potentially a fertile arena for the application of social psychological principles (Frank, 1961; Goldstein, Heller, and Sechrest, 1966; Brehm, 1976; Strong, 1978). Frank (1961), for example, has characterized therapy as a relationship between a sufferer and a socially sanctioned authority who attempts to produce certain changes in the emotions, attitudes, and behaviors of the sufferer. Clearly this implies the importance of social psychological processes dealing with attitude change and social influence. These would seem to have an important bearing on the interpersonal influence setting we call psychotherapy.

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One approach that has been applied to the study of attitude change is the concept of effort justification derived from the theory of cognitive dissonance (Festinger, 1957). We believe that the notion of effort justification is also potentially important in understanding both the process and outcome of psychotherapy. Effort justification concerns the consequences of engaging in an effortful activity in order to obtain some goal. The fact that one has engaged in an effortful event is discrepant with the notion that one does not usually engage in such effort. And for what purpose? In the typical effort justification sequence, either the goal or the means for achieving that goal is not attractive at the outset of the sequence. Consider, for example, the classic experiment by Aronson and Mills (1959). Subjects in a High Effort condition were made to undergo an event that was difficult and embarrassing. Their goal was to join a sexual discussion group that was, in reality, dull, boring, and a general waste of time. Yet those subjects who underwent the highly effortful procedure came to indicate that the group and its members were generally interesting and enjoyable.

Why did such changes occur? The reason given by Aronson and Mills was based upon the tension state of dissonance that was created by the voluntary expenditure of effort. "Why did I undergo such embarrassment and effort?" a subject may have asked. "Because I really did like the discussion group," might be the reply. In other words, the goal was elevated in attractiveness as a way of justifying the expenditure of effort.

Cooper (1980) argues that the effort justification sequence might lie at the basis of many psychotherapeutic systems. Typically, psychotherapy involves a patient volunteering for an effortful and sometimes emotionally draining process. A client may have a fear of certain objects, may find relationships with others unpleasant, or may find it noxious to behave in certain socially adaptable ways. Yet in any of the myriad procedures generically called psychotherapy, clients often make changes in their attitudes, emotions, and behavior. The goal states—be they phobic objects, interpersonal relations, or particular behaviors—become more acceptable or attractive. At least part of this change may result from an attempt to justify the expenditure of effort, just as in Aronson and Mills's study the discussion group became more attractive.

Which systems of therapy are more effective in producing change? The answer does not appear to rest decisively with any school. Despite the fact that some systems of therapy rely upon the production of behavior, others on the arousal of anxiety, and others on the discussion of emotionally traumatic events, none seems to be clearly superior in producing effective changes (e.g., Sloane et al., 1975). Frank (1961) has aptly noted that the commonalities in psychotherapeutic systems may be more germaine than their differences. And one of the common factors that would seem to underlie virtually all effective therapy systems is the expenditure of effort. Psychoanalytic schools rely upon emotional catharses, desensitization approaches evoke degrees of anxiety, and virtually all therapies rely upon the use of a considerable amount of time and often expense. To the degree that participation in the therapies is voluntary, they are conceptually similar to Aronson and Mills's study of the effort justification sequence.

Cooper (1980) conducted a pair of experiments to test the role of effort justification in psychotherapy. He reasoned that if successful therapy outcomes were due to effort justification, the particular form of effort should not matter, as long as that effort is seen as related to the goal in question. In one experiment, college students who were afraid of snakes were recruited. They participated in one of two experimental therapies. One of the therapeutic procedures was a modified form of implosive therapy (Stampfl and Levis, 1976) that is based upon the learning theory notion of extinction. This therapy involves a high degree of effortful participation on the part of the participant. The other therapy was based solely on the expenditure of physical effort and was not based upon any existing theory of therapy. Subjects were asked to jump rope, run in place, and perform other physical exercises. Subjects in each therapy condition first attempted to come as close as they could to a six-foot boa constrictor. Half the subjects in each condition then made an informed choice about participating in the effortful therapy; the other half participated without such a choice. According to dissonance theory, the effort justification sequence should be invoked only under conditions of an informed choice (e.g., Linder, Cooper, and Wicklund, 1968). Therefore it was predicted that, on the basis of dissonance theory, effort justification would be invoked by either therapy since both implosive therapy and the newly created physical exercise therapy involved high degrees of effort—but this would occur only under conditions of high decision freedom. It was found that subjects in either form of therapy made significant improvement in the degree to which they could approach the boa constrictor—if and only if they participated under conditions of free choice. Neither the exercise therapy nor implosive therapy was effective in the absence of choice.

The basic approach was repeated in a conceptual replication. Participants were university students who were nonassertive. Cooper (1980) assigned half of them to a physical exercise therapy and half to a standard therapy for lack of assertiveness (i.e., behavior rehearsal; see Salter, 1949). Once again, it was found that either form of therapy was effective in increasing participants' assertiveness, as long as it was engaged in voluntarily.

The conclusion from Cooper's experiments is that the voluntary expenditure of effort is at least one of the effective ingredients in psychotherapy, regardless of whether that effort forms part of a traditional therapy or whether it is improvised in a series of physical exercises. Exercise therapies or traditional therapies may be effective in promoting change, as long as they are engaged in voluntarily. However, several important questions remain unanswered. First, the notion that effort leads to positive changes in psychotherapy has yet to receive a direct test. This is because neither of Cooper's experiments used *variations* in effort as an independent variable. To the extent that effort justification is involved in psychotherapy, it should be shown that variations in the degree of effort will lead to variations in the degree of change that ensues.

Equally important is the question of duration of change that occurs as a result of effort justification. Both of Cooper's experiments involved single-session therapies with change measured immediately after the session. While this is a typical procedure in laboratory experiments involving attitude change, it is not parallel to the

desired outcome of psychotherapy. Change as a result of therapy is anticipated to be more long lasting, and an assessment of the effort justification procedure should be based not so much on an immediate assessment as on its long-term consequence. There are only a few reports in the dissonance literature of cognitive changes lasting well beyond the experimental session (Freedman, 1965). So the question of duration of consequences takes on enormous significance in assessing the appropriateness of the conceptual analysis that is based upon the psychology of effort justification.

The focus of the present experiment was on weight loss. This problem offers many advantages for an experimental study of effort justification procedures. First, there are objective and nonreactive measurements available for body weight that can be easily and repeatedly sampled. Second, the dependent measure thoroughly defines the criterion of a successful therapy, since weight loss represents the specific goal of treatment (Wollersheim, 1970). In addition, the number of different approaches to weight loss that exist in society attests to the fact that there is, at present, no agreed upon effective treatment for the loss of weight. The present experiment uses the concept of effort justification to effect such a treatment.

METHOD

Subjects

Subjects were recruited via newspaper ads for an "Experiment concerning possible methods of weight reduction." They were contacted by phone for scheduling and were assured that the procedure would be safe and would not include any medication. The following restrictions applied: First, only those 18 years of age and older were solicited. Second, men were excluded from the final subject pool (this decision was made after the initial response to the ads was overwhelmingly female). A third step, which was also taken to increase homogeneity, restricted allowable weight deviations to include only those women who were 10 to 20 percent above "desirable body weight" (according to the Metropolitan Life Insurance Company statistics [1959]). The lower limit was similar to that used by Schachter and his colleagues in their studies of obesity (Schachter, 1971); the upper limit decreased the probability of there being a physiological etiology and complication associated with overweightness (Olson, 1964). Fourth, no one receiving therapy or medication for their weight was included. Fifth, potential subjects who participated in athletic activities that would greatly increase weight by increasing muscle size rather than body fat were excluded (Schachter, Goldman, and Gordon, 1968). Finally, only subjects who lived within 20 minutes' traveling time of the laboratory were included, since extraordinary distances could affect the manipulation of effort.

Sixty-eight subjects who fulfilled the above requirements began the experiment. Each was paid at the rate of one dollar per session. Fifteen subjects were lost through attrition. Chi-square analyses failed to reveal any relationship between experimental conditions and the decision to terminate.[1] In addition, data from one subject were omitted when she arrived at two consecutive sessions too late to complete the full procedure. Fifty-two subjects comprised the final sample.

Procedure

General overview. A 2 (level of effort) × 2 (level of choice) between-subjects design with an external control group was utilized. Subjects attended five sessions over a three-week period. They attended two sessions during each of the first two weeks and one during the final week. To minimize extraneous influences on weight change, sessions for each subject were scheduled at the same time of day and the same days of the week throughout. Six months after the experimental sessions were completed, a final weighing session was conducted.

The choice variable. Subjects in the four experimental conditions were first met by a female experimenter who measured and recorded their weight.[2] She then administered the choice variable. Subjects in the High Choice conditions were told: "I have been instructed to advise you that although the procedures you will follow are perfectly safe and harmless, they may also be effortful and anxiety producing. If you like, you can stop the experiment now and you will be paid for this session. Would you like to continue?"

A Princeton University Informed Consent form was then given to be read and signed. This stated, in part, "I may withdraw my consent and discontinue participation in the project at any time." As the form was being read, the weigher emphasized, "As you notice on the form, you may withdraw your consent and stop at any time . . . you still have the prerogative to stop participation later . . . ."

Subjects in the Low Choice conditions were not asked whether they wanted to continue. Subjects were merely warned about the potential effort and anxiety and then told, "We'll go ahead and begin."

The second experimenter introduced himself and administered a "Life Pattern Questionnaire" concerning everyday eating and exercise patterns and other activities that might be useful in interpreting weight change data. It was also administered to increase the perceived legitimacy of the procedure. All subjects were then given a small booklet in which to monitor their eating over the three-week period. This too was partly to increase the perceived legitimacy of the procedure.

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[1]Reported reasons for dropping out included loss of interest, outside interference such as sudden job changes, and unexpected transportation problems, which prevented participation.

[2]Subjects were weighed in indoor clothing and without shoes.

The second experimenter explained the rationale for the study by noting that psychologists have frequently found strong correlations between heightened neurophysiological arousal and increased emotional sensitivity. The present researchers, he added, had been able to take advantage of this by presenting subjects with various tasks designed specifically to increase this neurophysiological arousal and thereby enhance emotional sensitivity in a way that helped lead to weight reduction. He then explained that although the procedure had been very successful in the preliminary investigations, the precise reasons for the weight loss obtained were still uncertain, and the present study aimed to make the process clearer.

The tasks were described as requiring much concentration and consequently being neurophysiologically arousing and sometimes stressful. The subject was assured that this arousal and stress would be brief and not last beyond any session. In keeping with the cover story, all subjects were attached to a galvanic skin response (GSR) apparatus (ostensibly to measure their level of arousal) while performing the tasks described below.

The effort variable. Effort in the present experiment involved the degree of difficulty of a variety of cognitive tasks. Effort was manipulated by varying task difficulty and duration, the parameters chosen being established through pretesting. Subjects in the High Effort conditions worked for 20 minutes at a 3-channel tachistiscope (T-scope). Their task was to discriminate which of several near-vertical lines presented sequentially was most vertical. Each line was visible for only 350 milliseconds. Those in the Low Effort condition worked for 3 minutes and were given 1 second to view each line.

Subjects then moved to a delayed-auditory feedback (DAF) apparatus. High Effort subjects were given 30 minutes of recitation, in which they attempted to recite nursery rhymes, read a short story, and recite the U.S. Pledge of Allegiance with their own voice reflected back to them via earphones at a delay of 316 milliseconds. The delay was similar to that used by Zimbardo (1965) in his manipulation of high effort. In addition, the voice of a woman attempting similar tasks during pretesting was overlayed onto the recorder so that the subject not only had to contend with the delay, but also with yet another voice. Low Effort subjects worked for only 10 minutes and the auditory delay was cut in half (158 milliseconds). This reduced delay, accomplished by increasing tape speed, also rendered the voice distraction incomprehensible and therefore less disruptive (Mackworth, 1970).

To avoid the potential confounding of session length with level of effort, subjects in the Low Effort condition, after completing the T-scope task, returned to the waiting room to relax for 40 minutes before finishing the session. This procedure, which is similar to that used by Wicklund, Cooper, and Linder (1967), was explained to Low Effort subjects as being necessary to allow the arousal due to the T-scope task to dissipate before beginning the next task.

Upon finishing the final task, the subject completed a brief questionnaire concerning her impression of the session. Most important, she was asked, “In general,

how effortful would you describe the hour as a whole?" This was followed by a 7-point response scale labeled "Very little" and "Very much" at the end points. The subject then returned to the weighing room for the final choice manipulation. For High Choice subjects, the first experimenter stated, "I'll remind you again that you can still stop the experiment and be paid for what you've done so far. Would you like to continue?" When the subject acknowledged that she wished to continue, she was given an appointment card containing the time and date of the next session. Low Choice subjects were merely given the appointment card without any mention of a new decision.

Sessions two through four. The following three sessions were similar to the first. The Life Pattern Questionnaire was not administered during these sessions and the cover story was not repeated. The sessions contained T-scope and DAF tasks that were of the same durations as those of Session One, although the content of the visual discriminations and DAF reading tasks was altered to relieve possible boredom.

The final experimental session. Session Five contained the final assessment of weight change during the experimental period and was conducted by the second experimenter. Unaware of the subject's choice condition, he weighed the subject and then informed her that her participation in the study was completed. He asked the subject to fill out a questionnaire that covered various weight-related topics and the subject's perceptions of the study. Crucial were two final manipulation checks: "How effortful would you describe the experiment as a whole?" (1 = very little; 9 = very much); and "How free did you feel *not* to continue with the experiment at any time?" (1 = not free to choose; 9 = very free to choose). Next the Life Patterns Questionnaire was readministered. Finally, the subject was carefully questioned about any suspicions she may have had about the purpose of the study, fully debriefed, and paid for her participation. The need for deception and the importance and possible implications of the study were discussed at length.

Control group. To provide a baseline indication of normal weight fluctuation among subjects in our sample, ten subjects from those who responded to the advertisement were randomly assigned to a control condition. When contacted to begin, they were told that they would be unable to participate as originally planned because of a change in the procedure that meant using less participants. They were then asked to engage in a project "to determine normal female weight fluctuation over time." Control subjects also participated in five sessions, scheduled in a similar fashion as the experimental subjects. They were simply greeted and weighed during each session. The Life Patterns Questionnaire was administered at the first and fifth sessions. A shortened version of the final questionnaire was also administered at the fifth session. It was emphasized that this was not a weight reduction

study and that subjects should therefore simply carry on their normal daily activities. Dieting was left to their discretion.

The final assessment. Six months after the fifth session was completed, participants were contacted for a follow-up weighing. The subjects were unaware that they would be re-contacted. Forty-two of the 52 subjects were able to return; nine had since moved from the area or were unable to be reached; one had become pregnant. At this follow-up, subjects were given a copy of the results from the original experiment.

RESULTS

Checks on the Manipulations

The amount of effort involved in the subjects' participation was assessed in two ways. First, at the end of the four experimental sessions, subjects were asked to rate the effortfulness of the preceding hour (1 = very little; 7 = very much). A 2 (choice) × 2 (effort) × 4 (sessions) repeated measures analysis of variance showed a significant main effect for Effort ($\bar{\chi}_{high} = 4.31$ vs. $\bar{\chi}_{low} = 3.14$), F (1, 37) = 10.21, $p < .005$. In addition, during the final session all subjects, including the control group, rated the effortfulness of the experiment as a whole (1 = very little; 9 = very much). Planned comparisons again confirmed that High Effort subjects rated the study as more effortful, F (1, 45) = 4.62, $p < .05$. Dunnett comparisons showed that only the High Effort–Low Choice groups differed significantly from the effortfulness ratings of the Control group ($p < .08$ and $p < .05$, respectively).

Results from the perceived choice manipulation checks revealed that the manipulation of this variable was not entirely effective. At the final session, the subjects were asked how free they felt not to continue with the experiment at any time. This primary measure of decision freedom was supplemented by three additional questions that related to perceived freedom (all scaled 1 = not free to choose; 9 = very free to choose). A multivariate analysis of variance on these questions revealed a marginal main effect for Choice, Wilkes Likelihood Ratio Exact F (4, 34) = 2.19, $p < .10$. Why this effect was only marginal can be seen by examining the primary choice measure. Of the 41 subjects in the experimental groups who responded to this question, 56 percent marked the highest level of choice. In fact, even 40 percent of the Low Choice subjects responded this way. This apparent ceiling effect was evident on all four questions. Although a univariate analysis of variance performed on the data from the primary choice measure showed that High Choice subjects felt significantly more freedom to discontinue the experiment than Low Choice subjects, F (1, 45) = 5.06, $p < .05$, the ceiling effect caused both the normality and homogeneity assumptions underlying the analysis of variance to be violated ($F_{max} = 13.04$, $p < .01$), which results in a positive bias in the test (Winer,

1971). The overwhelming proportion of extreme responses also rendered nonparametric analyses of little use, since the number of tied ranks was unacceptably high. We are thus left uncertain as to the statistical and psychological significance of the choice manipulation.

Weight loss: data from session five. The amount of weight change at the immediate conclusion of the experimental sessions was measured in pounds.[3,4] As can be seen from Table 18.1, the effort justification hypothesis that there would be more weight loss in the High Effort–High Choice condition was not found. Planned comparisons revealed only a tendency toward a main effect for effort, F (1, 47) = 2.28, $p < .14$. Given the questionable effectiveness of the choice manipulation, these results are more understandable. If choice is considered high in all cells, then the effort justification prediction must be that subjects in the High Effort conditions would lose more weight than those in the Low Effort conditions. Although the evidence for this is marginal, additional data should be considered: Seven of the nine subjects who lost the most weight were from the High Effort conditions, while five of the six who gained the most weight were from the Low Effort

TABLE 18.1
Mean weight change over the initial five sessions (in pounds)

Group	Unadjusted	Internal analysis
High Effort–High Choice	−1.75	−2.25
High Effort–Low Choice	−1.77	−1.20
Low Effort–High Choice	−1.16	−1.33
Low Effort–Low Choice	− .45	0
Control	+ .17	—

Note: For the internal analysis, subjects were grouped according to High vs. Low *assigned* Effort and High vs. Low *reported* Choice (obtained by a median split of responses to the choice manipulation check).

[3]For clarity of presentation, the weight change analyses reported are based on difference scores. The crucial assumptions behind the use of difference scores, namely, that within-group regression coefficients relating initial to final weight are homogeneous and equal to 1.00, are both satisfied (*b*s for High Effort–High Choice; High Effort–Low Choice; Low Effort–High Choice; Low Effort–Low Choice; and the control were, respectively, 1.01, .95, 1.01, .99, and 1.02).

[4]Since the duration of this portion of the experiment was short and weight changes were expected to be small, data on each subject's menstrual cycle were also collected (during the fifth session). This was to ensure against any spurious weight change effects resulting from water retention (congestive dysmenorrhea) coinciding with menstruation. For example, if a subject began the experiment at or near menstruation, perhaps any weight loss obtained later would simply reflect a return to normal weight; likewise, legitimate losses could be concealed if the final session ended at menstruation. However, analyses of menstrual data showed that only a small number of subjects had either started or finished their sessions around menstruation and that these subjects were equally distributed across conditions.

conditions. In addition, *t* tests, adjusted for the number of possible comparisons, revealed that only in the combined High Effort conditions was weight deviation significantly different from zero, $t\,(20) = 4.11$, $p < .02$.

The equivocal success of the choice manipulation makes interpretations involving that variable difficult. To better understand the role of choice, an internal analysis was performed on the data. Specifically, the analysis used the subjects' report of how much freedom they experienced as the choice variable. Table 18.1 shows the weight lost by the subjects as a function of their *assigned* degree of effort and their *reported* experience of freedom, the latter variable obtained by a median split on the primary choice measure. A planned contrast shows a marginally significant difference between the High Effort–reported High Choice condition and the remaining three conditions, $F\,(1, 37) = 3.39$, $p < .08$. In addition, only the High Effort–reported High Choice condition showed weight deviations that were significantly different from zero, $t\,(9) = 4.26$, $p < .06$.

Weight loss: the final measure. Encouraged by the direction if not the magnitude of the results, we proceeded with the final dependent measure: the six-month follow-up. As Table 18.2 indicates, dramatic differences had developed over the six-month period. High Effort subjects had lost an average of 6.5 additional pounds. Low Effort subjects, on the other hand, showed a slight gain in weight. The difference was highly significant, whether measured as change from the fifth session, $F\,(1, 30) = 20.30$, $p < .001$, or the beginning of the experiment, $F\,(1, 30) = 20.45$, $p < .001$. Of the 16 High Effort subjects who reported for the final measure, 15 had lost weight, whereas only 7 of the 18 Low Effort subjects and 4 of the 8 Control subjects lost weight. Once again, an internal analysis using a median split on the choice measure was performed. These results showed that subjects in the High Effort–reported High Choice group lost the most weight, although this

TABLE 18.2
Mean weight change with first follow-up (in pounds)

			Internal analysis	
Group	Over follow-up period	Overall	Over follow-up period	Overall
High Effort–High Choice	-6.46_a	-7.93_a	-7.21_a	-9.75_a
High Effort–Low Choice	-6.61_a	-9.03_a	-5.03_a	-6.61_a
Low Effort–High Choice	$-\ .14_b$	-1.47_b	$+1.10_b$	$-\ .48_b$
Low Effort–Low Choice	$+1.75_b$	$+1.33_b$	$+\ .44_b$	$+\ .44_b$
Control	$+\ .94_b$	$+\ .94_b$	—	—

Note: "Over follow-up period" indicates weight change from the conclusion of the initial sessions to the follow-up; "overall" indicates weight change from the beginning of the experiment to the follow-up. Within each column, means with different subscripts differ significantly from one another ($p < .05$ by Newman-Keuls).

group did not differ significantly from those in the High Effort–reported Low Choice condition.

Weight loss: one more time. One year from the date of the initial experimental session, subjects were contacted once again. By this time, many had either moved or otherwise changed situations so that they could no longer return in person for the weighing. These subjects weighed themselves at home and reported their weight by phone. As a result, data were obtained for 50 of the 52 original subjects. Analyses using only those subjects who returned to be weighed were similar to findings based on the full subject pool, so only the latter are reported. Once again, the main effect for effort was highly reliable, F (1, 45) = 7.79, $p < .01$. The average weight loss for high effort subjects one year from their participation in the experimental session was 6.7 pounds whereas low effort subjects lost only .34 pounds (Control $\bar{\chi} = -1.86$ pounds). Ninety percent of the High Effort participants were below their initial weight whereas only 48 percent of the Low Effort and 56 percent of the Control subjects were below their initial weight. Once again, an internal analysis revealed that the High Effort–reported High Choice group had lost the most weight. An *a priori* contrast showed this group to be significantly different from the other three, F (1, 36) = 5.26, $p < .05$.

DISCUSSION

The weight loss observed in the High Effort cells was both substantial and consistent. Since subjects were initially an average of 17 pounds overweight, High Effort subjects—who lost an average of 8.55 pounds by the first follow-up—were able to achieve a 50 percent reduction in excess weight. The chief issue to be addressed, then, is not whether positive therapeutic change occurred but rather whether effort justification processes can account for this change.

The results offer clear support for the role of effort in instigating the weight reduction. But the weakness of the choice manipulation makes a final interpretation less clear than would be desired. The notion of effort justification predicts weight loss for participants who engaged in a high degree of effort and who perceived their freedom to participate to be high. Since an overwhelming proportion of subjects perceived their freedom to be high, the main effects found for the effort variable are consistent with the effort justification approach.

In retrospect, the problem with the manipulation of choice could have been expected. Subjects came to the university from various locations in the community. They returned for several sessions. Attempting to convince Low Choice subjects that they were not at all free to discontinue the experiment would have strained ethical considerations, as well as credibility. Rather, we remained silent about choice considerations to Low Choice subjects and instead emphasized the high degree of decision freedom to those subjects assigned to the High Choice conditions.

Since freedom was apparently already assumed to be high by the participants, our manipulation did not have much differential impact. Because of this strong ceiling effect, even internal analyses could offer only slight assistance by showing tendencies for the High Effort–reported High Choice group to lose the greatest amount of weight.

What of those subjects who actually reported feeling low decision freedom? There were eight such subjects who checked points lower than the mid-point of the primary choice scale. For them, it would be predicted that, regardless of effort, weight loss would not occur. Indeed, six of the eight actually gained weight over the initial five sessions. The mean weight change for all eight was +.56 pounds. Seven returned for the first follow-up. Although their mean weight change was −1.75 pounds, this was largely due to one highly aberrant subject who had lost 13 pounds. The remaining six were virtually unchanged ($\bar{\chi}$ = +.13 pounds). By the second follow-up, overall weight change for the eight subjects reporting low choice was almost zero ($\bar{\chi}$ = −.06 pounds).

The dramatic amount of weight change that occurred during the six-month period following the experimental session stands in marked contrast to the marginal degree of weight change that occurred immediately after the experiment. Of course, weight change takes time and it is not surprising that only a few pounds could be changed during the three weeks of the sessions. But this weight loss was not only maintained over the half-year period, it was markedly increased by those subjects in the High Effort conditions. Why should this have happened? Long-term changes as a function of laboratory intervention are not commonplace in the literature.

Our answer, based upon the concept of effort justification, must still be in the form of speculation. It would appear that effort justification worked to increase the attractiveness of the *goal* of weight loss. There is no evidence that subjects in the High Effort conditions found that they could be successful at losing weight during the experimental sessions and continued their method over the long period prior to the follow-up measure. The data show little relationship between weight lost during the initial experimental sessions and weight lost by the first follow-up. Our initial statement of effort justification principles is consistent with this lack of relationship, for it is the goal state that effort renders more attractive. Just as Aronson and Mills's (1959) subjects who underwent effort viewed the goal of belonging to the discussion group with greater attractiveness, so too did our subjects come to view the goal of losing weight with more zeal and fervor. As a result, subjects in the High Effort conditions pursued that goal *regardless* of how successful they might have been during the five experimental sessions. Subjects in the Low Effort conditions, on the other hand, did not view the goal as being more attractive. Regardless of whether they had shown any weight loss during the five experimental sessions, they showed no consistent pattern of weight loss during the succeeding six months. Thus, the data are consistent with the notion that the goal of weight loss became more attractive to High Effort subjects who then pursued that goal more vigorously during the intervening six months.

Alternative explanations for the data may still arise because of the equivocal nature of the choice manipulation. One such explanation is that High Effort subjects may have formed differentially higher expectations about the therapy's potential outcome. These high expectations may have led to stricter adherence to some self-prescribed regimen that resulted in weight loss. Those who initially lost weight might have formed what Ross, Lepper, and Hubbard (1975) have referred to as antecedent—consequent explanations for why they were losing weight. However, it is difficult for an expectation-based hypothesis to explain why those initially showing little or no weight loss would later lose. As we have already mentioned, the data reveal that High Effort subjects lost weight during the six-month period regardless of whether they had initially lost or gained. This factor is more consistent with the effort justification hypothesis for it appears to be the goal of weight loss that becomes more attractive.

But this speculation should only emphasize the need for more work in the area. In particular, the specific processes producing the changes need more attention. Perhaps an attributional alternative—a self-perception process, for example—could account for our findings.

When combined with the two experiments reported by Cooper (1980), the present experiment adds to the confidence with which the concept of effort justification can be applied to psychotherapy. In the present experiment, the concept of effort was expanded to include cognitive mental tasks, and the data showed that variations in this type of effort led to systematic differences in the amount of weight that participants lost. Although the findings were of a small magnitude at the conclusion of the five experimental sessions, the amount of weight lost after a six-month period by subjects who had undergone a highly effortful procedure was considerable. The extension over time adds important new dimensions to research on effort justification as well as to research on psychotherapeutic outcomes.

The application of effort justification principles to psychotherapy would seem to call for serious consideration on the basis of the present data. Some therapists may feel uneasy applying effort justification because the principles have become associated with the use of deception (Brehm, 1976). Such deception is not endemic to effort justification. In fact, effort justification could be relied upon by initially advising clients very truthfully of the effort and unpleasantness that lie ahead. As Freud (1929) stated, "when we take a neurotic patient into psychoanalytic treatment . . . we point out the difficulties of the method to him, the long duration, the efforts and sacrifices it calls for . . ." (p. 15). Effort justification also stresses the client's personal responsibility for choosing to participate and remain in therapy. When viewed in this light, perhaps the effort justification principle emanating from the social psychological laboratory will become more palatable to therapists. We are not saying that effort justification is the only influence on therapy outcomes; nor are we necessarily advocating an "effort therapy." What we do believe is that effort justification influences outcomes. To the extent that therapists take advantage of this process they may enhance the efficacy of their therapies.

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19

Dishonest Behavior as a Function of Differential Levels of Induced Self-Esteem

Elliot Aronson and David R. Mettee

After taking a personality test, Ss were given false feedback aimed at temporarily inducing either an increase in self-esteem, a decrease in self-esteem, or no change in their self-esteem. They were then allowed to participate in a game of cards, in the course of which they were provided with opportunities to cheat under circumstances which made it appear impossible to be detected. Significantly more people cheated in the low self-esteem condition than in the high self-esteem condition. A chi-square analysis evaluating cheater frequency among the high self-esteem, the no information (no change in self-esteem), and the low self-esteem conditions was significant at the .05 level. The results are discussed in terms of cognitive consistency theory.

Recent theorizing and experimentation have suggested that a person's expectancies may be an important determinant of his behavior. Working within the framework of the theory of cognitive dissonance, Aronson and Carlsmith (1962) conducted an experiment in which subjects were led to develop an expectancy of poor performance on a "social sensitivity" test. The subjects then proceeded to perform beautifully. Aronson and Carlsmith found that these subjects subsequently changed their superior performance to an inferior one when retested over the same material. Similarly, Wilson (1965) found that subjects were significantly more attracted to a negative evaluator than to a positive evaluator if the negative evaluations were in accord with a strong performance expectancy that had led the subjects to withdraw from a competitive event. Consistency theory thus has received some support in specific expectancies and performance directly related to these expectancies.

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But what about more pervasive expectancies such as those about the self? Bramel (1962) showed some evidence for the impact of self-esteem on subsequent behavior. In his study he temporarily raised or lowered the subjects' self-esteem by providing them with positive or negative information about their personalities. He then allowed them to discover irrefutable negative information about themselves. The individuals who held low self-concepts were more willing to accept this information; that is, they were not as prone as people who had been induced toward high self-esteem to project this specific negative attribute onto others. These results are consistent with the work of Rogers (1951), who argued that negative or maladaptive responses occur as the result of being consistent with a negative self-concept, and that such responses can be altered only by first changing the self-concept in a direction consistent with adaptive responses.

The prediction being tested in the present experiment is in accord with the experiments cited above. In addition, it carries our interest in the self-concept one step further in the direction Rogers has taken—toward greater generalization. What Aronson and Carlsmith showed is that people try to behave in a highly specific manner that will coincide with a highly specific self-expectancy; that is, people who believe that they are poor in a "social sensitivity" test will take action aimed at performing poorly on the test. But does this generalize? If we feel low and worthless on one or two dimensions do we behave generally in low and worthless ways—even if the behavior is not directly and specifically related to the low aspects of the self-concept? For example, if a person is jilted by his girlfriend (and thus feels unloved), is he more apt to go out and rob a bank, kick a dog, or wear mismatched pajamas?

In the present experimental situation we are predicting just that. Concretely, individuals who are provided with self-relevant information which temporarily causes them to lower their self-esteem (but does *not* specifically make them feel immoral or dishonest) are more apt to cheat than those who are made to raise their self-esteem—or those who are given no self-relevant information at all (control condition). Similarly, people who are induced to raise their self-esteem will be less likely to cheat than the controls. This hypothesis is based upon the assumption that high self-esteem acts as a barrier against dishonest behavior because such behavior is inconsistent. In short, if a person is tempted to cheat, it will be easier for him to yield to this temptation if his self-esteem is low than if it is high. Cheating is not inconsistent with generally low self-esteem; it *is* inconsistent with generally high self-esteem.

METHOD

General Procedure

The subjects were led to believe that they were participating in a study concerned with the correlation between personality test scores and extra-sensory per-

ception (ESP). They were told that their personalities would be evaluated with the self-esteem scales of the California Personality Inventory (CPI) and that their ESP ability would be ascertained with the aid of a modified game of blackjack. Before participating in the blackjack game, subjects took the personality test and received false feedback (either positive, negative, or neutral) about their personalities. During the blackjack game subjects were faced with the dilemma of either cheating and winning or not cheating and losing in a situation in which they were led to believe (erroneously) that cheating was impossible to detect. The opportunity to cheat occurred when the subjects were "accidentally" dealt two cards at once instead of one. The rightful card put the subject over 21 and ensured defeat, whereas the mistakenly dealt extra card, if kept, provided the subject with a point total that virtually assured victory. The card which the subject kept constituted the dependent variable.

Subjects

The subjects were 45 females taken from introductory psychology classes at the University of Texas, who were randomly assigned to one of three self-esteem conditions: high, low, and neutral. In actuality, 50 subjects were run; the result of five subjects were discarded because of suspicion. Three of these were in the low self-esteem condition; two were in the high self-esteem condition. The criteria for elimination were determined a priori and were followed rigidly throughout the experiment. It was made explicit that the experimenter had no preconceptions as to how personality traits might be related to ESP ability, but simply wanted to determine, for example, whether people who are easily angered have more ESP than calm people.

Personality Test

All subjects came to the first session together and were given the self-esteem scales of the CPI. The CPI was administered by a person who introduced himself as a member of the University Counseling Center staff. Subjects were told at this session that the experiment was concerned with ESP and personality characteristics. They were informed that this session was to determine the personality traits of the subjects, with ESP ability to be measured in the second session.

A shortened version of the CPI was used to evaluate the personalities of the subjects. This version contained only the six scales related to self-esteem and, for our purposes, constituted a measure of the subjects' chronic self-esteem. However, the *primary* experimental purpose of this text was merely to provide the opportunity and rationale for situationally manipulating the subjects' self-esteem via preprogrammed feedback regarding subjects' personality test results.

To separate the experimenter as much as possible from the personality evaluation aspects of the experiment, subjects were told by the experimenter that Miss Jacobs,[1] a member of the University of Texas Counseling Center staff, had kindly consented to administer the personality tests. It was emphasized that she would score the personality inventories and that the experimenter's access to their scores would not be on a name basis but via a complicated coding process. It was indicated that, as a matter of convenience, subjects would be given feedback regarding their personality tests when they came for the second session of the experiment. Following this, the CPIs were distributed, subjects completed them, and before leaving were assigned a time to return for the second session with three subjects assigned to each specific time slot.

Personality Score

In the second session subjects were tested in groups of three. Upon arrival for the second session, subjects were greeted by Miss Jacobs and told to be seated in an outer office that provided access to three adjoining offices. After all three subjects had arrived, Miss Jacobs handed each subject a manila envelope bearing appropriate Counseling Center insignia and assigned each subject to a different office where she was to go to read the results of her personality test. In addition, Miss Jacobs told the subjects that she had been given a sheet of instructions to deliver to them. The sheet of instructions was handed to the subjects along with the manila envelope.

The personality test results consisted of three standard feedbacks unrelated to subjects' performance on the CPI. Each of the three subjects present at any one specific time was randomly assigned feedback of either high self-esteem (HSE = positive), no self-esteem (NSE = neutral), or low self-esteem (LSE = negative) in content.

The HSE and LSE personality reports were parallel in content except, of course, for the nature of the evaluation. For example, a portion of the HSE report stated:

> The subject's profile indicates she has a stable personality and is not given to pronounced mood fluctuations of excitement or depression. Her stableness does not seem to reflect compulsive tendencies, but rather an ability to remain calm and level-headed in almost any circumstance. Her profile does suggest she might be rather impulsive concerning small details and unimportant decisions. This impulsive tendency is probably reflected in a lack of concern with material things. In addition, it appears that material things are important to the subject only insofar as they enable her to express her generosity, good nature, and zest for living. . . . [she] is intellectually very mature for her age.

The corresponding portion of the LSE report stated:

> The subject's profile indicates that she has a rather unstable personality and is given to pronounced mood fluctuations of excitement or depression. Her instability seems

[1]The authors would like to express their appreciation to Sylvia Jacobs for her assistance in running the study.

> to reflect compulsive tendencies and relative inability to remain calm and level-headed in circumstances which involve tension and pressure. Her profile does suggest she might be rather meticulous and careful concerning small details and when making unimportant decisions. In addition, it appears that material things are very important to the subject as an end in themselves. She appears to be a very selfish person who clings to material things as a source of personal gratification and as an emotional crutch.

In the NSE condition the subject was told that her report had not yet been evaluated because of a heavy backlog of work at the Counseling Center. Instead, she was presented with a sample profile which was described as a typical or average CPI profile such as one might find in a psychological textbook. The comments in this NSE report paralleled those of the HSE and LSE reports (e.g., ". . . fairly stable personality . . . occasionally experiences mood fluctuations . . ."). The contents of the rest of the reports was designed to be global in nature and contained comments concerning the person's ability to make friends, general impact of personality, and depth of thought. Since the dependent variable involved honesty, special care was taken to refrain from mentioning anything directly involving honesty. Similarly, nothing whatever was mentioned, either explicitly or implicitly about the person's moral behavior or "goodness" of conduct.

Warm-up Instructions

After reading their personality test results, subjects turned to their page of instructions. The instructions stated:

> The purpose of this experiment is to correlate extrasensory perception ability with personality characteristics. In order to get a true measure of a person's ESP ability, it is necessary that one's mind be primed for thinking. In order to accomplish this, I am having you engage in a period of cerebral warm-up. It's not important what you think about—anything will do—but the crucial point is that you are to use your mind, warm it up by thinking. You need not concentrate intensely, just keep your mind active and filled with thought. A few minutes of this will suffice to prepare you for the ESP experiment.

The purpose of this "warm-up" period was to provide an opportunity for the impact of our manipulations to sink in. Since the subjects had just received an evaluation of their personalities we were quite confident that they would be thinking about this material.

Following the warm-up period, the subjects were sent, one at a time, at intervals of approximately 30 seconds, from the second floor of the psychology building to a room on the fourth floor. Here they were met by the experimenter who was unaware of which subject had received which type of personality feedback. The subjects were placed in one of three cubicles that isolated them from each other. Thus it was impossible for subjects to converse with one another between the time they received their personality results and the time the dependent measure was collected.

Apparatus

The apparatus consisted of four booths or cubicles; the front panels of three of the cubicles bounded an area 1 foot square. Access to this area was available via the fourth cubicle which had no front panel. The experimenter, when sitting in the fourth cubicle, was thus able to receive and dispense playing cards to the subjects in the other three cubicles via slots in their front panels. Subjects in their cubicles had no means of communicating with their fellow subjects nor could they see the experimenter because of plywood panels on both sides and at the top of their booths.

Inside each booth there were two slots and two toggle switches. One switch was designated to be turned on to indicate a "no" answer and the other to indicate a "yes" answer. One of the slots was horizontal with the base of the booth, and was situated in the middle of the front panel one-half inch above the cubicle base. The other slot was vertical and was the slot through which subjects returned cards to the experimenter.

The experimenter's cubicle contained three inclined sheet-metal slides leading down to the horizontal slots of the subjects' booths, three vertical slots from each subject's booth which were shielded so that subjects could not see into the experimenter's compartment, three scoreboards (one for each subject), six small light bulbs, each connected to one of the subjects' "yes" and "no" switches. The experimenter's booth also contained a small electric motor. The motor was functionless except for sound effects, and was activated periodically during the experimental session.

Experimental Materials

The experimenter had two decks of cards. One deck was used for the general game of cards to be played; the other deck was divided into three stacks of cards (one stack for each subject) with a prearranged sequence. These were the crucial hands which a subject would be dealt on those occasions when she would be given the opportunity to cheat. The prearranged stacks consisted of four hands of blackjack with four cards in each hand. The first two cards in all hands totaled approximately 11–13 points with the third card sending the point total over 21. The fourth card, if substituted for the third, always brought the point total to between 19 and 21 points.

Each subject's cubicle contained, in addition to the two toggle switches, a 15-watt light bulb and 10 fifty-cent pieces (or $5). The half-dollars were used as "chips" and potential reward in the experimental task.

Experimental Instructions

After the subjects were seated, the experimenter recited the instructions. He described the results of some previous experiments concerning ESP. Rhine's conclu-

sion that some persons do indeed possess ESP was presented; however, *no* indication was given that certain types of persons possess ESP while others do not. To make the cover story appear more credible, subjects were asked to read a recent newspaper article taped to a side panel of their cubicles, which told of how a young girl had been winning an astonishing number of raffle contests which, according to a research institute at Duke University, was perhaps due to her ESP powers. This article indicated that having ESP could be materially valuable, but no mention was made either in the article or in the instructions that having ESP was an intrinsically positive or valued trait to possess. Following this, subjects were told that their ESP ability was going to be evaluated in the context of a modified game of blackjack. It was clearly explained that the game was to be played among the three subjects and that the experimenter was not participating in the game as a contestant. The subjects were then informed of the presence and necessity of a "card-dealing machine." The experimenter said that the cards were to be dealt by a machine "in order to insure against possible interference with your ESP due to another person handling the cards." In actuality, the machine was used to provide an opportunity for the subjects to cheat, and to make it easier for subjects to cheat since the machine apparently removed the experimenter from the situation; this will be described below. According to the experimenter's description, the machine automatically dealt them a card on each round. To "stand pat," subjects had to switch on their "no" light so that the experimenter could divert the dealing machine from giving them a card on the next round. The use of light signals rather than verbal ones to inform the experimenter that the subject wanted to stand pat was justified by "the necessity of keeping talking at a minimum in order not to interfere with your ESP." The modifications and rules of the blackjack game were as follows:

Each subject was provided with $5 by the psychology department as a stake with which to play the game. Each subject was to "bet 50¢, no more or no less," on the outcome of each hand. Subjects were informed that at the conclusion of the experiment they would be allowed to keep all winnings over $5, but that subjects having less than $5 at the game's conclusion *would not* be required or obligated to make up the deficit out of their own pockets; this made it impossible for any subject to "lose" any of her own money.

We presented the following cover story regarding how ESP ability would be measured: The subjects were told to "concentrate for approximately 5 to 10 seconds before each hand;" that is, to think about whether they would win the upcoming hand. If they thought or felt they were going to win, they were to push the "yes" switch in front of them to the "on" position for 5 seconds, whereas if subjects felt they were not going to win the upcoming hand, they were to push the "no" switch to the "on" position for 5 seconds. The number of correct guesses *above chance* supposedly constituted a subject's ESP score. Prior to each guess, all subjects deposited a 50-cent piece in the vertical slot, and it was explained to the subjects that on every hand they were trying to win a kitty with $1.50 in it.

A thorough explanation of the objectives of blackjack was given in order to equalize differential card game experience between the subjects. It was emphasized that point totals of 14–15 made the choice of whether to take another card especially difficult because of the high probability of going over 21 if another card was taken, and the low probability of winning by standing pat. The point totals of the various cards were also emphasized with the Ace counting only 11 points rather than 1 or 11 points, to simplify the experiment; also, all face cards were worth 10 points, and all numbered cards were worth their face values.

It was explained to subjects that:

> The card-dealing machine is a "home-made" affair. It has not as yet been perfected and at times makes mistakes. So far we've been able to iron out most of the mistakes, but occasionally the machine will deal two cards at a time instead of one. The machine is set up so that it deals from the top of the deck, as in a normal game of cards; when a person receives two cards, the top card is actually the card he should have gotten and the bottom card is the mistaken card. Now, the machine is fairly good, but at times will make this mistake. If it ever happens that you are dealt two cards, pick up the two cards and immediately return the bottom card to me by slipping it through the vertical slot. Remember, the top card is your card, the bottom card should be returned to me through the vertical slot.

Subjects were told that when all "no" switches were turned on, the experimenter would say "Game," which was a signal to subjects that they were to push all their cards through the vertical slot to the experimenter. Again, no talking was allowed under the pretext of not interfering with ESP. The experimenter then examined each player's hand, determined the winner, collected the three 50-cent pieces and slid them down the slide of the winner. When the experimenter said "Begin," indicating that the next game was to start, the two subjects not having received any money were thus informed of their losing status.

Resumé of Procedural Instructions to the Subjects

The word "begin" was a signal to subjects that the previous hand was finished and that a new hand was to commence immediately. Following the word "begin," subjects deposited their 50-cent pieces through the vertical slot, concentrated for 5 seconds, made a choice of the "yes" or "no" switches regarding their outcome expectancy on the upcoming hand, and the game began. The hand continued until all subjects had indicated they no longer wanted any more cards, at which time the experimenter said "game" and subjects then pushed all their cards through the vertical slot to the experimenter. This same procedure was repeated 35 times for all subjects. However, subjects were not aware as to exactly how many hands had been played at any given point in the experiment, nor did they know precisely when the game would end.

Dependent Measure

The dependent measure was the number of times during the experimental session that subjects kept the card they should have returned to the experimenter, thus enabling them to win the hand. The experiment was designed so as to present each subject with four opportunities to cheat during a session; thus, as far as each subject was concerned, the machine had mistakenly dealt two cards at once only four times in 35 hands. In reality, the experimenter dealt all cards but synchronized his dealing with the onset and termination of the machine-generated sound effects.

The method for providing subjects with a cheating opportunity was quite simple. However, complications arose in determining precisely *when* this opportunity should present itself. We decided to present a subject with an opportunity to cheat when the following conditions could be satisfied: *(a)* If she (the subject) had guessed "yes"; *(b)* when she possessed the same amount of money as the other subjects; *(c)* if she had not previously received more opportunities to cheat than the other subjects; *(d)* when her ESP hit rate was near chance level.

The separate stacks of "cheating cards" were prearranged so that the first two cards totaled 11–14 points, the third put the total over 21, and the fourth, if substituted for the third, brought the total to 19–21 points. Thus, when subjects received two cards at once they were faced with a dilemma: if they did not cheat, they would lose the hand; if they did cheat, they would almost certainly win. Cheating, therefore, was made relatively safe from exposure by having the cards dealt by a machine and also enabled subjects to net $1. The behavioral measure of cheating was whether subjects returned to the experimenter the card that was actually theirs and kept the bottom card which enabled them to win. The remaining two subjects in such a game were dealt cards from the general deck. If the subject had cheated, she was always declared the winner on that hand and was given the three 50-cent pieces. This subject, of course, lost if she did not cheat. The other two subjects, in this hand, were always dealt a hand less than 21 or at times one of them was dealt a hand exceeding 21. If a subject did *not* cheat, the other subject with the score closest to and under 21 was declared the winner. The experimenter used scoreboards to keep track of how often a subject had won a hand, how often each had been given a cheating opportunity, and whether a subject had cheated. The scoreboard also provided for an evaluation of trial effects.

Following the card game, all subjects were asked to be seated at a table in the same room. They then filled out a questionnaire consisting of a check on the experimental manipulations and several filler items. Subjects were then debriefed completely. The purpose of the experiment was explained and subjects were assured that the results of the personality test were preprogrammed and had no relationship to their actual test scores.

RESULTS AND DISCUSSION

Analyses of variance on continuous data were all nonsignificant, although the mean differences were of the order hypothesized (mean cheats: LSE = 1.87, NSE = 1.54, HSE = 1.07). Frequency analyses, however, produced significant chi-squares. Subjects were divided according to whether they never cheated or cheated on at least one occasion. Table 19.1 shows a 2 × 3 contingency table chi-square with *df* = 2. Note that 13 people who were given negative feedback cheated at least once, while there were only 6 cheaters among the positive-feedback subjects. The chi-square proved to be significant at beyond the .05 level ($\chi^2 = 7.00$, $p < .05$). Another chi-square was computed to evaluate the cheating differences between just the HSE and LSE groups. This chi-square with *df* = 1 also proved to be significant after the correction for continuity had been made ($\chi^2 = 5.17$, $p < .03$).

Taken as a whole, the data indicate that whether an individual cheats is influenced by the nature of the self-relevant feedback he received. People who learned uncomplimentary information about themselves showed a far greater tendency to cheat (on at least one occasion) than individuals who received positive information about themselves. This suggests that individuals with LSE are more prone to commit immoral acts than individuals with HSE, at least when the immoral act is instrumental in producing immediate material gain. Moreover, the number of people cheating in each of the above groups fell on either side of the neutral condition. Although neither experimental condition was significantly different from the control, it is important to note that in the HSE condition there was a greater trend toward honest behavior than in the control, whereas in the LSE there was a greater trend toward cheating than in the control.

Our interpretation of the results hinges upon our contention that the manipulation employed in the experiment made subjects feel good about themselves or bad about themselves. In short, we contend that the feedback the subjects received had some impact (however temporary) upon their level of self-esteem. This interpretation is bolstered by our check on the manipulation which indicated that LSE subjects felt worse about themselves than either NSE or HSE subjects. However, since the manipulation check occurred subsequent to the card

TABLE 19.1
Number of people cheating at least once as a function of self-esteem

Condition	Cheat	Never cheat
LSE	13	2
NSE	9	6
HSE	6	9

Note: $\chi^2_{LHN} = 7.00$, $df = 2$, $p < .05$; $\chi^2_{LH} = 5.17$, $df = 1$, $p < .03$

game, this difference might be due to the cheating behavior rather than the self-esteem manipulation.

There is some additional evidence which is also consistent with our interpretation. This involves the cheating behavior of subjects in terms of their *chronic levels* of self-esteem. Although it was not our intention to measure chronic self-esteem, one can extract a rough measure by looking at the self-concept scores on the CPI, which all subjects filled out as part of the cover story of the experiment. According to these measures, people of high, medium, and low self-esteem had been almost equally distributed among experimental conditions. The following data emerge: In the no feedback (NSE) condition, slightly more "low chronics" cheat than "high chronics." Also, as one might expect, the greatest percentage of cheaters falls among the low chronics who were given negative feedback; the smallest percentage of cheaters falls among the high chronics who were given positive feedback. The small number of subjects in each cell makes statistical analysis of these data unfeasible. The most one can say about these results is that they are consistent with our interpretation of the overall data on the basis of the experimental treatments themselves.

It should be emphasized that one of the unique aspects of this study is the nonspecific nature of the self-concept manipulation. The subjects were not told anything about themselves which would lead them to infer that they were moral-honest or immoral-dishonest people. Rather, they were told things designed to reduce their self-esteem in general. The social implications of these findings may be of some importance. Our results suggest that people who have a high opinion of themselves are less prone to perform any activities which are generally dissonant with their opinion. Similarly, it may be easier for a person with a low self-concept to commit acts of a criminal nature. Moreover, it may be that a common thread running through the complex variables involved in successful socialization (Sears, Maccoby, and Levin, 1957) is that of different development of self-esteem. Granted that most children become aware of what behavior is approved (moral) or disapproved (immoral), the development of HSE in the individual may be crucial in his choosing a moral rather than an immoral mode of behaving.

This discussion is highly speculative to say the least. Further experimentation is necessary before the validity of our reasoning can be determined. One reason for the note of caution is the fact that it is difficult to perform an experiment involving complex human cognitions, emotions, and behavior which leaves us with a single, untarnished explanation for the results. This experiment is no exception. One conceivable alternative interpretation concerns aggressiveness: the subjects in the LSE condition, because they received a negative evaluation, may have been angry at the evaluator and, consequently, may have cheated as a way of punishing him. We attempted to eliminate this possibility in two ways: (a) We separated the experimenter who ran the ESP experiment from the evaluator (a member of the Counseling Center). Toward this end, the experimenter appeared ignorant of and uninterested in these evaluations; it was made to seem to be strictly between the subject

and the Counseling Center. (b) The subjects were playing against other subjects rather than against "the house." Thus, when an individual cheated, she was clearly not hurting the experimenter; rather, she was unjustly taking money from a fellow college student.

Perhaps a more compelling explanation involves compensation. The subjects in the LSE situation may, in effect, be saying "Well, I may not have done well on that personality test, but at least I'm going to see to it that I win some money." This explanation is quite different from the one that holds that it is easier to cheat because such behavior is consistent with feelings of LSE. Note, however, that the "compensation" explanation applies only to the subjects in the LSE condition. Thus, this alternative explanation would have been weakened if the subjects in the HSE condition had cheated less than those in the control condition. But as the reader will recall, although these data were in a direction favoring consistency theory, they were not statistically significant. Thus, until further research is performed on this problem, compensation remains a possible explanation.

A final piece of unclarity should be mentioned. We predicted that people in the LSE condition would *cheat* and people in the HSE condition would *not cheat* because we felt that such actions would reflect a consistency between self-esteem and behavior. But *cheating* is merely one of many ways in which a person's behavior could show consistency with LSE or HSE. For example, in the present situation a LSE subject could try to *lose;* being a loser might be considered as consistent with LSE. It should be noted that the experimenters took special pains to assure the subjects that they could not lose money. In this situation it seems reasonable to assume that being a loser simply means having bad luck—not being a bad person or even a poor person (financially). We selected cheating as our dependent variable because we felt that it is an unambiguously unethical piece of behavior. Although losing at cards is not pleasant, it does not seem bad, especially since a loss of money is not involved. The data indicate that our reasoning was correct—that if a sizable number of LSE subjects had sought to lose, our data would have failed to reach significance.

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20

Using Cognitive Dissonance to Encourage Water Conservation

Chris Ann Dickerson, Ruth Thibodeau, Elliot Aronson, and Dayna Miller

In a field experiment on water conservation, we aroused dissonance in patrons of the campus recreation facility by making them feel hypocritical about their showering habits. Using a 2 × 2 factorial design, we manipulated subjects' "mindfulness" that they had sometimes wasted water while showering, and then varied whether they made a "public commitment" urging other people to take shorter showers. The "hypocrisy" condition—in which subjects made the public commitment after being reminded of their past behavior—was expected to be dissonance-arousing, thereby motivating subjects to increase their efforts to conserve water. The results were consistent with this reasoning. Compared with controls, subjects in the hypocrisy condition took significantly shorter showers. Subjects who were merely reminded that they had wasted water, or who only made the public commitment, did not take shorter showers than control subjects. The findings have implications for using cognitive dissonance as means of changing behavior in applied settings, especially those in which people already support the desired goal, but their behavior is not consistent with those beliefs.

Policy makers frequently attempt to modify behavior in a community often by instituting information-based persuasive campaigns. These appear in various forms including broadcast announcements, newspaper advertisements, signs, mailings, and flyers. In recent years, drought has prompted administrators at the University of California at Santa Cruz (UCSC) to launch a major campaign of just this sort. Campus newspapers contained advertisements from the Water Conservation Office; flyers were posted on public bulletin boards and appeared in mailboxes. Specifically, the UCSC program encouraged people to think of water as a valuable resource and to adopt conservation-oriented behaviors, such as flushing toilets less

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often, stopping the flow whenever possible while brushing teeth or washing dishes, and taking shorter, more efficient showers.

The effectiveness of these types of persuasive messages and information campaigns is not certain. One experiment (Aronson and O'Leary, 1983) found that prominent signs asking people to take shorter showers produced mixed results. Indeed, if the message is too heavy-handed, it can even create a backlash. For example, although some subjects in the Aronson and O'Leary study reduced their water use, others showed their annoyance by sabotaging the signs and taking inordinately *long* showers. Moreover, even if people are initially persuaded by signs or flyers that conservation is worthwhile, there is controversy regarding the potency of such straightforward, and sometime "coercive," appeals. They can produce attitude change, but the effects are frequently short-lived (Aronson, 1980).

Similarly, even when messages praising the value of water conservation are successful in changing people's attitudes, there is no guarantee that new attitudes will translate into new behaviors. Social psychologists have long been aware that the link between attitudes and behavior is problematic (e.g., Wicker, 1969). Thus, simply persuading people that conservation is beneficial might not result in reduced consumption. For example, Bickman (1972) interviewed 500 people about their attitudes concerning responsibility for removing litter. Although 94% of the subjects expressed favorable attitudes toward removing litter, only 2% actually picked up litter that had been intentionally left outside of the experimental setting by the experimenter.

We reasoned that a more effective means of promoting water conservation on campus might involve dissonance-generated self-persuasion, rather than informational or coercive appeals to save water. The motivating influence of cognitive dissonance has been shown to promote changes in attitudes as well as behavior (Aronson, 1969, 1980; Brehm and Wicklund, 1976; Freedman, 1965). Dissonance-related techniques have been utilized successfully in a number of applied situations: for example, to improve weight loss (Axsom and Cooper, 1981), reduce snake phobia (Cooper, 1980; Cooper and Axsom, 1982), and as a component of programs designed to promote energy conservation (e.g., Gonzales, Aronson, and Costanzo, 1988). Moreover, Pallak and his colleagues have demonstrated that dissonance-related interventions can produce enduring behavior change. Longitudinal studies have shown that a public commitment manipulation can cause people to reduce their energy consumption for six months or more (Pallak, Cook, and Sullivan, 1980; Pallak and Cummings, 1976; Pallak, Sullivan, and Cook, 1976).

As formulated by Festinger (1957), dissonance theory proposes that when a person holds two cognitions that are psychologically inconsistent, the person will experience cognitive dissonance, an unpleasant drive state akin to hunger or thirst. Once dissonance is aroused, an individual is motivated to reduce it, primarily through attitudinal or behavioral changes designed to reestablish consistency. Soon after Festinger's initial conceptualization, Aronson (1960, 1968) proposed that dissonance theory makes its clearest predictions when expectancies about the self are involved—that is, when people have done something that violates their

self-concepts. Most of us share certain general beliefs about ourselves: for example, that we are good, moral, competent individuals. Therefore, choosing to engage in a behavior that is at odds with these important beliefs about the self should produce dissonance.

Given the central role of the self-concept in dissonance arousal, Aronson (1980) has argued that dissonance-related persuasion is likely to be much more effective than straightforward persuasive appeals. In a typical persuasion situation, such as those involving informational campaigns, people change their opinions because they have been convinced by an external source to do so. An unfortunate feature of this type of attitude change is that it is often impermanent. For example, if I change my attitude because I hear a persuasive argument supporting one stance, I am likely to change it again if I hear a better argument supporting another position. There is very little of myself invested in the attitude.

In contrast, dissonance-generated persuasion is highly involving because it entails a challenge to a person's self-concept. Dissonance would occur, for example, if I believed I was a moral person, and then found myself in the uncomfortable position of having done something I considered immoral. To reduce this dissonance, I would need to rethink, or "justify," my actions to make them more consistent with my self-concept—typically through changes in relevant attitudes or behaviors. This subtle form of self-persuasion is powerful because the individual's self-concept is directly engaged in the process of attitudinal or behavioral change (Aronson, 1980).

Perhaps the most dramatic demonstration of dissonance-related persuasion is evidenced in the counter-attitudinal advocacy paradigm (e.g., Cohen, 1962; Festinger and Carlsmith, 1959; Nel, Helmreich, and Aronson, 1969). In this procedure, subjects are induced, under conditions of high choice or low incentive, to persuade others to believe something that they themselves do not believe. These subjects subsequently come to believe their own rhetoric; that is, they reduce dissonance by persuading *themselves* that their counter-attitudinal statements were, in fact, a reflection of their true beliefs.

The counter-attitudinal advocacy paradigm, by definition, requires that experimenters induce people to defend a position that they were initially against. This requirement posed a problem for our effort to harness the power of the technique to promote water conservation. Conservation is an example of an "apple pie and motherhood" issue that everybody already believes in, even though not everyone practices. Recently, however, Aronson and his colleagues (see Aronson, 1992) have developed a modified version of the procedure so that it can be used in pro-attitudinal situations. The new technique involves creating feelings of hypocrisy. This is accomplished by inducing subjects to encourage other people to perform certain worthwhile behaviors. Subjects are then reminded that, on occasion, their own behavior has not been consistent with those goals. Essentially, subjects are confronted with the realization that they do not always practice what they preach. This realization is expected to generate dissonance because being a hypocrite would be inconsistent with most people's self-concepts as persons of integrity. As

a result, subjects should be motivated to reduce dissonance by behaving in a manner more consistent with their espoused attitudes.

In an experiment on AIDS prevention, Aronson, Fried, and Stone (1991) explored the dissonance-arousing properties of this new procedure. Using a 2×2 factorial design, they induced feelings of hypocrisy regarding condom use. All subjects wrote pro-attitudinal speeches advocating condom use during all sexual encounters. Then, half the subjects simply rehearsed the arguments of the speech. The rest videotaped their prepared speeches, which they believed were going to be shown to high school students as part of an AIDS prevention program. Before taping their speeches, however, half the subjects were also reminded of the occasions when they had failed to use condoms in the past. Thus, all subjects believed that condom use was important, and all had composed a speech arguing that point. However, only those who both made a videotape and were reminded that they had engaged in unsafe sexual behavior were expected to feel hypocritical. These subjects were expected to reduce dissonance by strengthening their intentions to use condoms in the future. The results from Aronson et al. (1991) were consistent with this reasoning. Compared with subjects in the other conditions, those who received the hypocrisy manipulation expressed significantly greater intentions to increase their use of condoms, relative to their past behavior.

The results of this experiment are provocative in suggesting that a "hypocrisy" manipulation can arouse dissonance. Moreover, a follow-up experiment was conducted (Stone, Aronson, Crain, Winslow, and Fried, 1992), using a behavioral measure rather than self-reported behavioral intentions. Specifically, in each of the above conditions, subjects were subsequently given an opportunity to purchase condoms at a huge discount. Fully 83% of the subjects in the hypocrisy condition purchased condoms; this was a significantly greater percentage than in each of the other three conditions.

Did subjects, in fact, increase their condom use as a result of the hypocrisy manipulation? Obviously, it is impossible to know for sure. After all, one cannot follow people into the bedroom to observe their condom-using behavior. However, one *can* follow people into the shower-room—at least at public physical education facilities. The present experiment explores the utility of the hypocrisy-induction procedure in a field setting, using water conservation as the target behavior. In a conceptual replication of Aronson et al. (1991), public commitment endorsing water conservation was crossed with feedback intended to make subjects aware that they had wasted water in the past. After acknowledging that they supported conservation efforts, half the subjects agreed to help persuade other people to conserve water. Additionally, half the subjects were reminded that they did not live up to their own standards, and had sometimes been wasteful. The condition in which subjects both committed publicly to encourage other people to conserve and were reminded that they had wasted water was designed to make subjects feel hypocritical. These subjects were expected to reduce dissonance by reducing their water use while showering.

METHOD

Overview of the Procedure

Female swimmers were recruited as they exited the pool area, on their way to the locker room. A female experimenter (Experimenter 1), posing as a member of a campus water conservation office, approached each potential subject and asked if she could spare a few moments to help with a water conservation project. Then, depending on the experimental condition, subjects either answered some questions, signed a flyer, or both. Subjects were thanked for their participation, and their interaction with the first experimenter was terminated. However, unbeknown to subjects, a second female experimenter (Experimenter 2) was waiting in the shower room where she unobtrusively timed the length of each subject's shower and noted whether subjects turned the water flow off while soaping up.

Experimental Design

Two factors were manipulated: subjects' "mindfulness" of their sometimes wasteful showering habits, and subjects' "commitment" to pro-conservation behaviors. This 2×2 factorial design yielded the following conditions: (1) mindful-plus-commitment (hypocrisy), (2) mindful-only, (3) commitment only, and (4) unmindful/no commitment (no-treatment control). Our primary dependent variable was actual water use, as reflected in the length of the subjects' showers. As a rougher measure of subjects' intentions to conserve water, we also noted whether subjects turned the shower off while applying soap, shampoo, or conditioner.

Subjects

Participants in the study were 80 female swimmers who used the shower after exercising in the campus pool. Females were selected for this study because we intended to gather data only in the women's locker room. We used swimmers because pretesting indicated that swimmers could most reliably be expected to shower and shampoo before leaving the recreational facilities. Although the majority of swimmers used shampoo and/or soap when showering (to remove chlorine from their hair and skin), those few who simply rinsed off under the shower were excluded from the study to reduce extraneous within-group variance. This exclusion was minimal and did not occur with differential frequency in any of the treatment conditions.

Experimental Setting

The particular configuration of the shower room is crucial to the design of this field study. The swimming pool and women's locker room are part of the same complex, with direct access to the showers available from poolside. The shower room is a large open room, approximately 15′ wide by 25′ long, without separate shower stalls or curtains. There are 13 showerheads, spaced along the walls of the shower room, and there are usually a number of people showering at any given time during operating hours. Typically, at least two or three other women were using the shower room at the same time as the subject and Experimenter 2. Frequently, there were more then five other women in the shower. These circumstances made it very easy to collect the dependent measures without attracting attention or arousing suspicion. Furthermore, it ensured that the presence of Experimenter 2 was unlikely to have detectable influence on the subject's behavior.

Procedure

While en route from the pool to the shower room, subjects were approached individually by Experimenter 1, who introduced herself as a representative of the campus water conservation office. After asking the subject if she had a few moments to spare, Experimenter 1 asked the subject whether she was on her way to the shower, and whether she was in favor of water conservation. If subjects answered "yes" to these questions, Experimenter 1 consulted a randomization chart and then introduced the experimental manipulations.

In the mindful-only treatment, she asked subjects to respond verbally to a "survey" consisting of a brief set of questions, such as: (1) When showering, do you ALWAYS turn off the water while soaping up or shampooing? (2) When you take showers, do you ALWAYS make them as short as possible, or do you sometimes linger longer than necessary? (3) In your view, about how long does it take an average person to shower and shampoo, without wasting any water? (4) About how long is your average shower at the field house? These questions were designed to remind subjects that they had sometimes wasted water while showering.

In the commitment-only treatment, the subject was simply asked to help out with campus conservation efforts by printing her name with a thick black marking pen on a flyer that read: "Please conserve water. Take shorter showers. Turn showers off while soaping up. IF I CAN DO IT, SO CAN YOU!" Experimenter 1 explained that the flyer would be attached to posters that were being created for distribution around campus, and that they were intended to encourage other members of the campus community to conserve water. While making this request, Experimenter 1 drew subjects' attention to the large, colorful "sample" poster on display nearby, and mentioned that another poster was already in place outside of the women's locker room.

In the hypocrisy condition (mindful-plus-commitment), subjects first responded to the brief "mindfulness" survey, then signed the "commitment" flyer as outlined above.

The fourth condition served as a no-treatment control. In essence, the behavior of the subjects in this condition reflects a baseline response to the interventions instituted by the university in an effort to save water. Because of persisting drought conditions in California, UCSC had been quite actively promoting water conservation. Advertisements in the campus newspapers and flyers posted on public bulletin boards urged members of the campus community to reduce their water use. Most pertinent to this study, the university had posted a very large sign inside the actual shower room. The sign read: "Take Shorter Showers. Turn the Water Off While Soaping Up."

Experimenters

Both experimenters were female students. The actions of the two experimenters were carefully coordinated. Experimenter 1 stood near a large doorway leading from the pool deck into the athletic facilities complex. From this vantage point, she was able to intercept all female swimmers who were leaving the pool to enter the locker room. Experimenter 2 sat sunbathing by the pool, near the back door to the woman's locker room. This was approximately 30 feet from the position of Experimenter 1. As Experimenter 1 began her interaction with the subject, Experimenter 2 watched, and made sure she could identify the subject later to collect the dependent measures.

As Experimenter 1 approached a potential subject, she asked the subject if she was on her way to the showers, and next, whether she could spare a few moments to participate in a water conservation project. If the subject answered yes to both, Experimenter 1 casually scratched her own knee before continuing the interaction. The knee scratch was a signal to Experimenter 2, who quickly entered the back door of the locker room, and began showering while waiting for the subject. This process enabled Experimenter 2 to remain unaware regarding which manipulation Experimenter 1 had delivered to the subject.

Experimenter 2 was already in the shower room, showering, when the subject entered. Several precautions were taken to guarantee that Experimenter 2 would not influence the subject's behavior. First, as noted above, the setting was a large shower room, and there were frequently a number of women showering. This reduced the possibility that the presence of Experimenter 2 had any noticeable effect on subjects. Additionally, since Experimenter 2 was often in the shower room for 10 to 15 minutes, she always brought shampoo, conditioner, a shaving razor, and a comb into the shower. These were used as necessary to make her showering appear as natural as possible. Finally, Experimenter 2 always left the water running during her shower. This was to avoid any possibility of influencing subjects to turn their own faucet on and off.

Dependent Measures

Experimenter 2 wore a waterproof sports watch with stopwatch capacity, which she unobtrusively activated as soon as the subject turned on the shower. She also noted whether the subject turned off the shower while applying soap or shampoo. To assess water use accurately, the watch was stopped when the subject turned off the shower and was reactivated if the subject turned it back on to continue her shower.

RESULTS

Manipulation Checks

All subjects answered "yes" to the first question in the survey "Are you in favor of water conservation?"—thus indicating that their attitudes on this issue were positive. In the two conditions in which mindfulness was manipulated, subjects' answers to the brief set of questions confirmed that they were aware of their sometimes wasteful showering habits. That is, all subjects replied that: (a) they did not always take the shortest possible showers; (b) they sometimes lingered longer than necessary in the shower; and (c) they did not always turn the shower off while soaping up or shampooing.

Shower Times

A two-way ANOVA was performed on subjects' shower times, measured in seconds (Table 20.1). No main effects for commitment or mindfulness were obtained, nor was the interaction of the two factors statistically significant, model $F(1, 76) = 1.48$, $p < .26$. However, a planned comparison of mean shower times revealed a significant difference between the hypocrisy group ($M = 220.5$ sec) and the control group ($M = 301.8$ sec), $F(1, 39) = 4.23$, $p < .05$. Means for the commitment-only ($M = 247.7$) and mindfulness-only ($M = 248.3$) groups did not differ from each other, nor did either differ from the control or hypocrisy groups.

TABLE 20.1
Mean shower times (in seconds)

Condition	Mean	SD
Mindful-only	248.3	146.07
Commitment-only	247.7	104.05
Mindful/committed (hypocrisy)	220.5	100.62
Unmindful/uncommitted (control)	301.8	142.32

Turning Off the Shower

We also compared how often subjects in each condition turned off the shower while shampooing or soaping up. An overall chi-square analysis yielded a marginally significant difference among all four groups on this dichotomous measure ($\chi^2 = 7.742$, $df = 3$, $p < .052$) (Table 20.2). Next, a comparison of the hypocrisy and control groups revealed a significant difference in the expected direction, with hypocrisy subjects turning off the shower more often than control subjects ($\chi^2 = 4.912$, $df = 1$, $p < .027$).

The frequencies in the hypocrisy condition did not, however, differ from those in the mindful-only and commitment-only conditions. Indeed, the data from these three conditions were identical, with 14 of 20 subjects in each group turning off the shower, compared to only 7 of 20 in the control group ($\chi^2 = 7.742$, $df = 1$, $p < .005$).

TABLE 20.2
Frequency of turning off the shower

Condition	Yes	No
Mindful-only	14	6
Commitment-only	14	6
Mindful/committed (hypocrisy)	14	6
Unmindful/uncommitted (control)	7	13

DISCUSSION

The data from this experiment are consistent with our reasoning that higher levels of dissonance would be aroused for subjects in the hypocrisy condition, leading them to make greater efforts to conserve water than subjects in other conditions. Specifically, it was only subjects in this condition who took significantly shorter showers than subjects in the control condition. Unexpectedly, however, shower times for hypocrisy subjects were not significantly shorter than times for subjects in either the mindful or commitment conditions, both of which fell midway between times for hypocrisy subjects and controls. In addition, subjects in the mindful and commitment conditions were just as likely as those in the hypocrisy condition to turn the water off while showering. In all three conditions, this behavior occurred significantly more often than in the no-treatment condition.

Overall, this pattern of data suggests the possibility that subjects in all three groups were motivated to conserve water, although this effect was strongest for those in the hypocrisy condition. That is, rather than experiencing *no* dissonance, subjects in the mindful and commitment conditions might have experienced some feelings of hypocrisy, albeit of a milder sort than their counterparts in the hypocrisy group. Subjects in the former conditions were exposed to manipulations that

could potentially arouse some feelings of hypocrisy. For example, in the commitment condition subjects signed a flyer that stated: "Take shorter showers . . . IF I CAN DO IT, SO CAN YOU!" For subjects who had wasted water in the past, this statement might have been experienced as somewhat hypocritical, even without the mindfulness manipulation to heighten its effect. Similarly, subjects in the mindful condition first affirmed their pro-conservation attitudes in the presence of the experimenter (recall that everyone answered "yes" to the initial question: "Are you in favor of water conservation?") and then were made aware of the discrepancy between their attitudes and behavior—that is, the fact that they did not always take the shortest possible showers. This awareness could have aroused mild feelings of hypocrisy, or dissonance, for these subjects.

Why did subjects in these two conditions reduce dissonance by turning the water off, yet did not take shorter showers than controls? One possible reason is that turning the shower off is a fairly vivid and unambiguous way for subjects to demonstrate their commitment to conserve water. As such, it provides a natural "first step" for subjects who are motivated to conserve water, thus affording the clearest and most available route to dissonance reduction. Unlike hypocrisy subjects, however, subjects in the mindful and commitment groups did not take the additional step of significantly reducing the duration of their showers, relative to controls. This finding is consistent with our interpretation that hypocrisy subjects were experiencing the highest levels of dissonance and, as a result, were more motivated to act in accordance with their principles: both by turning off the shower *and* actually using less water. Finally, it should be noted that our primary dependent variable, length of shower, is a true measure of water conservation—unlike turning off the water, which is simply one method of potentially achieving that goal.

Could the effects found in the present experiment be due to some cause other than dissonance arousal? For example, could subjects have taken steps to conserve water simply because their pro-conservation attitudes were made salient by the experimental manipulations? Although our manipulations may have partly served to "prime" subjects' attitudes, we think it is unlikely that the shower-time results are due to the mere effects of attitude accessibility or salience. To begin with, subjects in all three experimental conditions were, in one way or another, reminded of their favorable attitudes toward water conservation prior to taking a shower. Yet, only subjects in the hypocrisy condition showed a significant reduction in their actual water use. In addition, data from the condom experiments discussed earlier (Aronson et al., 1991; Stone et al., 1992) do not support a "priming" interpretation of the present findings. The results of these studies, which employed similar manipulations and were conducted under more controlled laboratory conditions, reflected significantly greater dissonance arousal among hypocrisy subjects compared with all other experimental conditions.

Still it could be argued that in the present study hypocrisy subjects might have experienced a more potent priming effect, given their exposure to both the mindful and commitment manipulations. Although this alternative explanation cannot be

ruled out, a close look at the details of the procedure makes this interpretation seem less plausible. Specifically, subjects in the hypocrisy condition were treated identically to those in the mindful condition except that the former also signed a leaflet advocating others to conserve water. This leaflet contained no new information above and beyond that already presented in the mindful condition; it simply restated methods of conserving water in the showers. (Indeed, this information is also posted conspicuously in the shower room itself and in other prominent locations within the adjacent locker room.) Thus, it seems doubtful that in the hypocrisy condition this redundant information—presented briefly and only seconds after the more extensive mindfulness manipulation—could have contributed appreciably to any "priming effect" produced by either of the manipulations alone. Rather, we would argue that the impact of signing the leaflet was that it made hypocrisy subjects uncomfortably aware of having preached something they did not always practice, thereby accounting for their greater motivation to conserve water. Future research is necessary, however, to determine conclusively whether these findings are best explained by dissonance arousal or are the effect of increased accessibility of attitudes via priming. In particular, laboratory studies based on the "misattribution of arousal" paradigm in dissonance research (e.g., Zanna and Cooper, 1974) would shed needed light on this issue.

Although a "priming" interpretation cannot be entirely ruled out, taken together with the findings of Aronson et al. (1991) and Stone et al. (1992) our results suggest that feelings of hypocrisy can be dissonance-arousing, thereby motivating people to bring their behavior into closer alignment with their espoused ideals. In addition, in recent years it has been proposed that individuals must produce "foreseeable aversive consequences" to experience dissonance (Cooper and Fazio, 1984; see also Thibodeau and Aronson, 1992). Our findings cast doubt on this new formulation of dissonance theory. Any consequences resulting from complying with the experimenter's requests could only serve to promote water conservation—by encouraging other people to save water, and by helping the "Water Conservation Office" with a survey. Far from being an aversive consequence, saving water was something that all subjects in the present study already supported.

Finally, in the present experiment subjects experienced dissonance in a pro-attitudinal advocacy paradigm. This represents a new twist on the counter-attitudinal advocacy manipulation traditionally employed in dissonance research and opens up new opportunities for applying the theory in real-world settings. In particular, interventions along the lines of our hypocrisy manipulation may prove successful in motivating people to act in accordance with their already favorable attitudes toward a given issue, such as water conservation, condom use, recycling, et cetera. Clearly, using dissonance arousal as a strategy for changing behavior is somewhat more involved than simply hanging signs or posting flyers. As noted earlier, however, research suggests that changes in attitudes and behavior generated by cognitive dissonance tend to be more permanent and may also transfer to new situations, as compared with changes produced by other means of persuasion (Aronson,

1980). In the long run, then, dissonance-related persuasion may prove to be a cost-effective method for policy makers to employ in a variety of settings, especially those in which the goal is to produce higher levels of consistency between attitudes and beliefs.

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21

A Self-Affirmation Intervention to Reduce the Racial Achievement Gap

Geoffrey L. Cohen, Julio Garcia, Nancy Apfel, and Allison Master

Two randomized field experiments tested a social-psychological intervention designed to improve minority student performance and to increase our understanding of how psychological threat affects performance in chronically evaluative real-world environments. Our research was based on the premise that African-American students experience relatively higher levels of psychological threat in school because of the negative stereotype impugning the intelligence of their racial group, and that this threat, in turn, undermines their academic performance. We tested whether such psychological threat can be lessened by having students' reaffirm their sense of personal adequacy or "self-integrity" via a self-affirmation intervention. The intervention, a writing assignment done in class, significantly improved the grades of African-American students and reduced the racial achievement gap by roughly 40%. Results suggest that a major social concern in the United States, the racial achievement gap, could be ameliorated, in part, by the use of timely and targeted social-psychological interventions.

The drive for self-integrity—for seeing oneself as good, virtuous, and efficacious—is a fundamental human motivation (Steele, 1988; Taylor and Brown, 1988; see also Sherman and Cohen, 2006). One critical basis of a person's self-integrity is membership in the social groups that he or she values. That is, individuals derive a sense of personal worth and meaning from the groups to which they belong (Tajfel and Turner, 1986), such as their family, or professional, national, and racial or ethnic group. People, consequently, want their groups to be viewed positively, and are threatened when they are not (Cohen and Garcia, 2005). Negative characterizations of one's group such as those conveyed by negative stereotypes, constitute a threat to self-integrity. This is especially true when the negative characteriza-

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tion is widely known, and impugns the ability of one's group on a highly regarded social value, such as intelligence. In the research presented here, we test whether the psychological threat caused by such negative stereotypes, and the debilitating effects on intellectual performance following from it, can be lessened by providing individuals with a social-psychological intervention designed to reaffirm or bolster their sense of self-integrity.

African Americans and Latino Americans are often called upon to contend with negative stereotypes about the intellectual ability of their racial or ethnic group. Because such individuals are aware of these widely known stereotypes, they may worry that performing poorly could lend credence to the stereotype in the minds of others. Such *stereotype threat* can lead to under-performance in school and work, as this psychological situation burdens these individuals with an extra stressor not experienced by those not stereotyped (Aronson, 2002; Aronson, Fried, and Good, 2002; Steele and Aronson, 1995; Steele, Spencer, and Aronson, 2002). As previous research suggests, stress can lead people to perform far below their potential, and lead to "choking under pressure" in the "big game" or on a "high-stakes standardized test" (Aronson, 2002; Gladwell, 2005; Steele and Aronson, 1995; Steele et al., 2002; see also Sarason, 1991; Zajonc, 1980). Indeed, simply observing a fellow group member who could possibly confirm a negative stereotype about one's group can be stressful and undermine one's own performance (Cohen and Garcia, 2005).

In addition to stereotype threat, three other ideas are critical to our research. The first is the idea that academic settings are by their very nature *chronically* evaluative. That is, students' abilities are under constant evaluation such that one important assessment is followed by another. This is especially true in the current educational climate given the heightened importance afforded standardized tests, where students are subjected to testing on a yearly basis. Evaluative situations can, of course, be stressful for anyone (Aspinwall and Taylor, 1992; Brown, 1998; Covington, 1984; Crocker and Major, 1989; Sarason, 1991; Steele, 1997; Zigler, Abelson, and Seitz, 1973; Zigler and Butterfield, 1968). However, chronically evaluative situations such as school or work can be particularly stressful, as the stress associated with them can be self-perpetuating. Stress can trigger a downward spiral in performance by contributing to an initial poor performance, which magnifies stress, and this, in turn, leads to still further decrements in performance over time in a repeating recursive cycle (Storms and McCall, 1976; Wilson, Damiani, and Shelton, 2002; Wilson and Linville, 1985). Students concerned with confirming a negative stereotype about their group may prove especially at risk of suffering such spiraling deficits in performance due to the extra burden of stress they experience in contending with the negative stereotype targeted at their group.

The second idea underpinning our research is that *affirmations* of individuals' overall perception of personal virtue, agency, and efficacy—their *self-integrity*—can counteract and alleviate stress (Steele, 1988; see also Creswell et al., 2005; Sherman and Cohen, 2006). According to self-affirmation theory, self-integrity is a global perception of self-adequacy and self-worth, and affirmations of it can take

the form of reflections on important values or prized skills (Steele, 1988; see also Sherman and Cohen, 2006). Although people try to maintain specific images of themselves, say as being a good student or a valued family member, what is more critical to them is their perception of their *general* adequacy. This fact provides them with a certain amount of flexibility regarding the sources of self-integrity. As shown in numerous studies, if individuals feel relatively positive about themselves as a result of their functioning in one domain, they prove both more willing and able to tolerate a threat in another domain. Individuals are less defensively closed-minded, less likely to rationalize their behavior, and more willing to accept their own or their group's responsibility for a defeat if they have first reflected on a source of self-integrity unrelated to the domain in which they were threatened (see Steele, 1988, and Sherman and Cohen, 2006, for reviews).

More important from the perspective of the research presented here is that self-affirmation has been found to reduce the psychological threat, and the stress related to it, experienced in evaluative performance situations (Creswell et al., 2005). For instance, individuals who had self-affirmed prior to giving a public presentation in front of a sullen audience were less likely to display elevated levels of the stress hormone cortisol than those who had not self-affirmed and had given such a presentation. As a result, we thought that an affirmation should be particularly effective in reducing stress and improving performance in stereotyped minority students, as they are assumed to experience higher levels of stress in academic settings because of stereotype threat than are their peers (Martens, Johns, Greenberg, and Schimel, 2006).

The last of the critical ideas at the center of our research is the notion that an understanding of how psychologically important processes unfold in a situation over time can lead to interventions that appear to have a larger effect than their size or intensity warrants (Lewin, 1952; Aronson, Fried, and Good, 2002; Wilson et al., 2002). In the present case, the knowledge that a negative recursive process can produce worsening performance over time suggests the importance of a strategy that interrupts such a cycle, and that triggers, in its place, a positive recursive process to produce sustained or improving performance. To accomplish this objective, we used an apparently brief self-affirmation task done in class. In truth, however, this task served as the trigger for activating a powerful and prolonged affirmation process throughout the course of the school term. This process, we thought, would lead to an initial reduction in stress, which would then lead to improved or sustained performance, which would then further lessen stress, leading yet again to sustained or improving performance, in a potentially repeating cycle.

The research reported here tested the hypothesis that a self-affirmation intervention designed to lessen psychological threat would enhance the academic achievement of negatively stereotyped minority students in a real-world classroom setting. This prediction was tested in two randomized double-blind field experiments. The second, a replication study, occurred a year after the first and involved a different cohort of students.

METHOD

Participants and Design

Participants were seventh graders, from middle-class to lower-middle-class families, attending a suburban Northeastern middle school with a student body divided almost evenly between African Americans and European Americans. The two experiments involved, in total, 119 African-American students and 124 European-American students distributed roughly evenly across the two studies. Each student was randomly assigned either to a no-affirmation control condition or to an affirmation treatment condition. The two studies featured an identical experimental design—that is, a 2 (student race: African American versus European American) $\times$ 2 (experimental condition: affirmation versus no affirmation) factorial design. All the teachers who participated in the studies taught the same academic subject, which served as the *intervention-targeted* course where the intervention was administered in both studies. For each teacher, there were approximately equal numbers of students in each condition.

Procedure

In both of the experiments, the manipulation took place as close to the start of the fall term as possible, when evaluative stress was assumed to be high. Teachers were kept unaware of students' condition assignment and of the specific research hypothesis. Teachers distributed to all students in their class closed envelopes, each containing an exercise packet. The exercise was presented as a regular class assignment and took approximately 15 minutes to complete. Students opened their envelopes, removed the packet, and were guided through the procedure via written instructions in the packet. Students completed the exercise independently and in silence, providing written responses to the tasks contained in the packet.

Each student in each classroom was randomly assigned either to an affirmation condition or to a control condition. Following standard procedures, the affirmation and control exercises presented a list of values (e.g., relationships with friends or family, being good at art). In Experiment 1, treatment students were asked to indicate their most important value; control students were asked to indicate their least important value. In the replication study, treatment students were asked to indicate their 2 or 3 most important values; control students their 2 or 3 least important values.

Treatment students in both studies then wrote a brief paragraph about why their selected value(s) were important to them. For instance, one student wrote, "My friends and family are most important to me when I have a difficult situation that needs to be talked about. My friends give me companionship and courage. My family gives me love and understanding." Control students wrote about why the chosen value(s) might be important to someone else. For instance, one student wrote, "Art would be important to someone who wanted to be an artist when they grow up."

The experimental intervention was reinforced through a series of writing prompts and questions designed to encourage students to reflect on their selected values and the basis of their importance. Upon completion, each student returned the exercise packet to its envelope, then sealed and returned it. Envelopes were collected and forwarded to the researchers. Teachers immediately resumed their lesson plan. One exercise was completed during the academic term in the first study; two exercises were done in the replication study.

Performance outcomes. The primary performance outcome was grade in the intervention-targeted course, calculated using end-of-term official report cards provided by the school administration. Of additional interest was the students' performances on individual assignments during the fall term. This was determined by collecting teachers' grade books and then calculating students' average performances during each of 10 chronological blocks in the fall term. The latter performance indices represent the average percentage of total points earned on the assignments administered in a given performance block.

Racial-stereotype activation measure. We also assessed the cognitive activation of the racial stereotype some time after the experimental manipulation, a psychological factor that may play a role in the intervention's efficacy, by having students complete a measure presented as a classroom exercise. This validated measure consisted of 34 word fragments (Steele and Aronson, 1995; see also Cohen and Garcia, 2005). Seven word fragments (e.g. _ A C E) could be completed with either a stereotype-irrelevant word (e.g., F A C E) or a stereotype-relevant word (e.g., R A C E). The total number of stereotype-relevant words constituted the measure of racial-stereotype activation.

RESULTS

Academic performance. We used multiple regression to test treatment effects on performance, while controlling students' teacher assignments and pre-intervention performances. Based on their official report cards, African Americans earned higher fall-term grades in the targeted course in the affirmation condition than in the control condition. On a grade metric (where "A" = 4.0, "B" = 3.0, etc.), the treatment effect was .26 grade points in Study 1 [$B = .26$, $t(41) = 2.44$, $p < .02$] and .34 grade points in the replication [$B = .34$, $t(60) = 2.69$, $p < .01$]. The likelihood of observing two effects of this magnitude by chance is approximately 1 in 5000. No treatment effect occurred for European Americans in either experiment [all Bs < 0.16; all ts < 1.1; *NS*]. The race $\times$ experimental condition interaction was significant in both experiments [$B = 0.29$, $t(98) = 2.00$, $p < .05$; $B = 0.52$, $t(119) = 2.80$, $p < .01$, respectively].

Importantly, this treatment effect was not limited to a small number of students performing at the edges of the performance range (i.e., those performing very

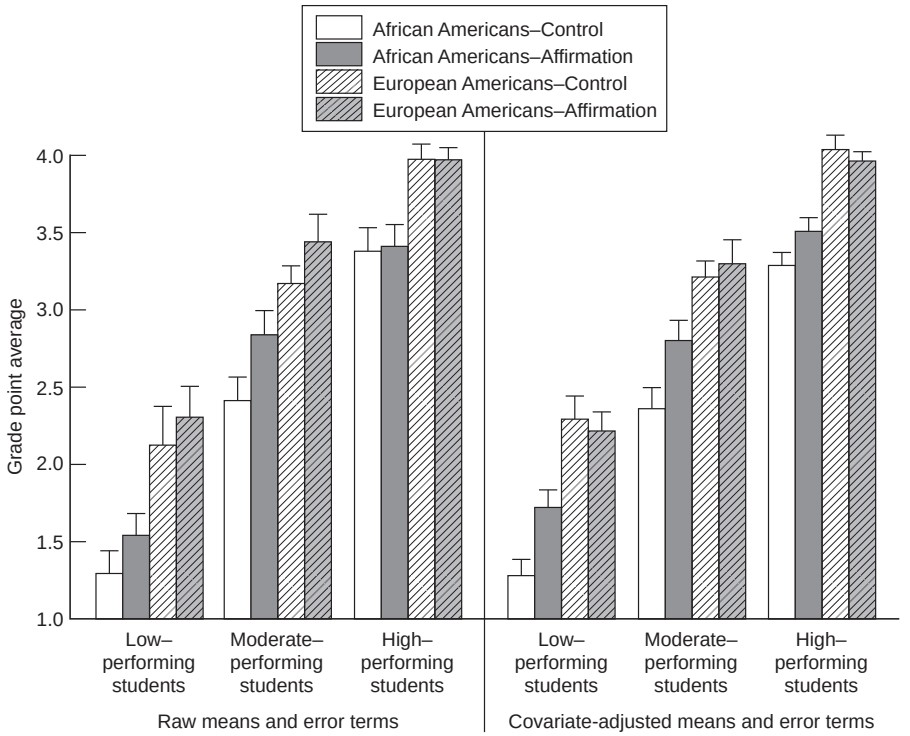

FIGURE 21.1
Mean grade point average in targeted class, as a function of student race, experimental condition, and pre-intervention level of performance (average of prior year's GPA and pre-intervention in-class performance). Students were categorized into low-, moderate-, and high-performance categories based on tertiary splits done separately within each racial group; the categorization represents students' relative standing within their race. The left panel displays raw means and error terms, the right panel displays means and error terms adjusted for baseline covariates. Scale reflects grade metric, ranging from 0 (= F) to 4.33 (= A+). Error bars represent standard errors.

poorly or well prior to the intervention). Figure 21.1, which displays the treatment effect at varying levels of pre-intervention performance (averaged over both studies), illustrates the generality of the impact of the affirmation treatment. It benefited nearly 70% of African Americans. The treatment benefit was equally strong for previously poor-performing students [$t(31) = 2.74$, $p = .01$] and for students in the moderate range [$t(30) = 2.40$, $p = .02$]. The highest performing students benefited less from the intervention than did low- or moderate-performing students, but covariate-adjusted results continued to show a marginally significant trend toward a positive effect of the intervention [$t(31) = 1.72$, $p < .10$].

The average performance gap between African Americans in the control condition and European Americans overall in the fall term of the course was .75 grade points (.68 in the first experiment, .82 in the second). The average treatment effect for African Americans was .30 points, roughly a 40% reduction in the racial achievement gap.

Combining the data of both studies provides a sufficiently large sample to permit meaningful analysis of the rate of poor performance (the percentage of students receiving a *D* or below), an index that truly brings home the practical impact of our intervention. For African-American students, this rate was 20% in the control condition but only 9% in the treatment condition, a significant difference in logistic regression [*Wald*(1) = 8.14, $\Delta\chi^2$ (1) = 11.40, $p < .01$]. European Americans did not vary by condition (6% versus 7%, respectively). The race × experimental condition interaction was significant [*Wald*(1) = 3.96, $\Delta\chi^2$ (1) = 4.39, $p < .05$]. Figure 21.2 shows that the poor performance rate of African Americans in the control condition was consistent with historical performance norms for their race. That is, control-group African Americans' performance did not differ from that of African Americans enrolled in the same course at the same school for the three years prior to our intervention [$z < 1$, *NS*]. This was also true for European Americans in both experimental conditions; their poor performance levels did not differ from historical norms for their race. By contrast, the poor performance rate of African Americans in the affirmation condition not only was significantly lower than their African-American peers in the control condition, but also was significantly lower than the historical norms for their race [$z = -2.73$, $p < .01$]. That is, the control condition showed no effect on performance, while the affirmation condition did.

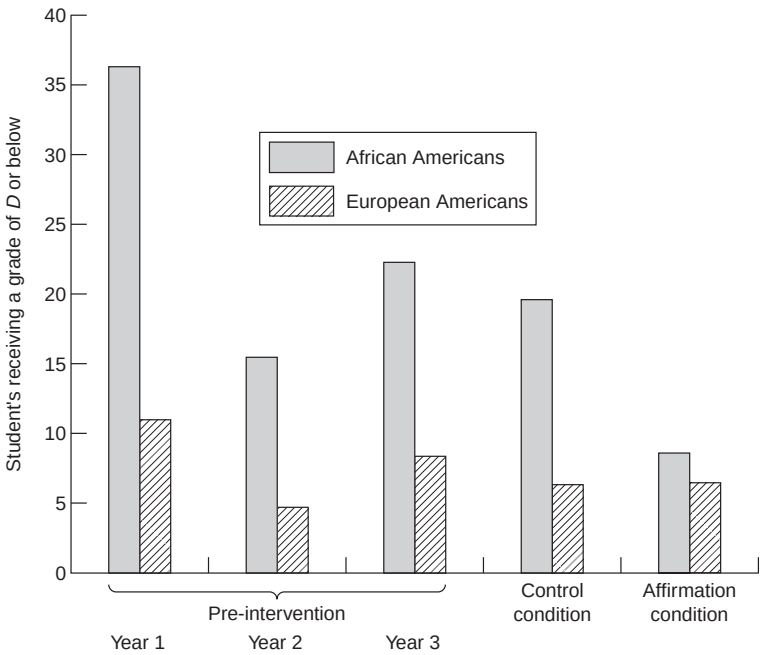

FIGURE 21.2
Poor performance rate (i.e., percentage of students receiving a grade of *D* or below) in the intervention-targeted course for four cohorts of students: African Americans and European Americans in each of the three years prior to the intervention studies, in the control condition, and in the affirmation condition.

If the affirmation reduced feelings of threat, an exploratory issue concerns whether its impact extended to other courses. African-American students tended to earn a higher grade point average (GPA) in these *nontargeted* courses in the affirmation condition than in the control condition [Experiment 1: $B = 0.31$, $t(40) = 2.63$, $p < .02$; Experiment 2: $B = 0.21$, $t(58) = 1.70$, $p < .10$ two-tailed test, $p < .05$ one-tailed test]. Pooling data from both experiments yielded a significant effect [$B = 0.23$, $t(108) = 2.51$, $p < .02$]. European Americans again displayed no condition effect in either experiment [Bs < 0.13, ts < 1, *NS*]. The race × experimental condition interaction was significant in Experiment 1 [$B = 0.45$, $t(97) = 2.75$, $p < .01$], marginal in Experiment 2 by a two-tailed test [$B = 0.30$, $t(117) = 1.74$, $p < .09$], significant by a one-tailed test [$p < .05$], and significant over both studies [$B = 0.30$, $t(228) = 2.42$, $p < .02$]. Because of these effects, the treatment effect on overall GPA was virtually as significant as it was on grades in the targeted course.

We obtained data related to how the affirmation process played out in vivo. The replication study provided data on performance over time. Figure 21.3 displays average in-class performance (proportion of total points earned) for each of 10

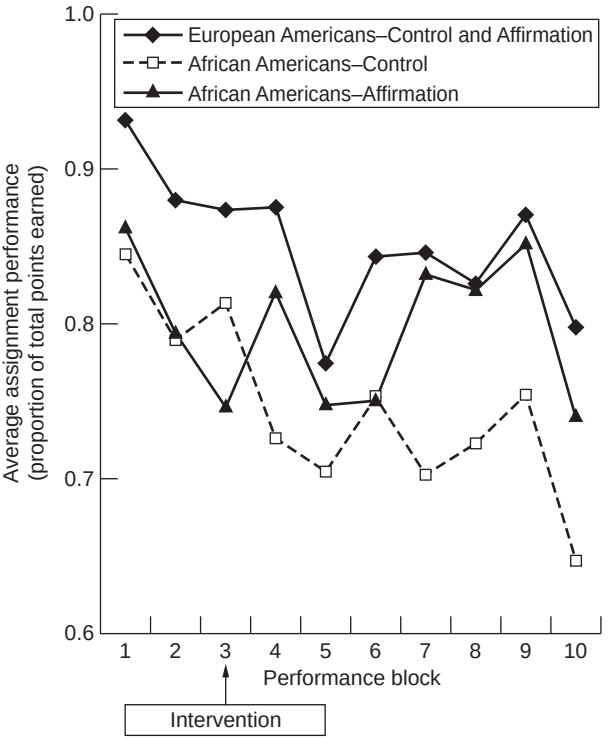

FIGURE 21.3
Mean academic performance, as a function of chronological performance block, student race, and experimental condition. Blocks 1 and 2 represent pre-intervention performance, blocks 3–10 post-intervention performance. Data of European Americans in both conditions were combined, as they did not significantly differ

chronological performance blocks during the academic term. These data suggest that the intervention buffered African-American students against the impact of an early decline in performance by interrupting a downward trend. The performance pattern of African Americans in the affirmation condition, unlike that of the remaining groups, was a "sideways S" pattern, well-fitted by a cubic function [$F(1,29) = 9.53$, $p < .01$], not a downward linear trend [$F < 1$, *NS*]. By contrast, African-American students in the control condition showed a significant downward linear trend; their performance continued to fall as the term progressed [repeated measures analysis, $F(1,33) = 10.45$, $p < 0.01$]. European Americans showed a similar—somewhat less steep—downward trend in both conditions [$F(1, 30) = 4.86$, $F(1, 25) = 7.30$, respectively; Ps $< .05$]. Repeated measures analyses (with teacher and gender controlled) indicated that the downward linear trend varied by condition for African Americans [$F(1,59) = 7.30$, $p < .01$], but not for European Americans [$F < 1$, *NS*]. The race $\times$ condition $\times$ performance block interaction was also significant [$F(1,112) = 4.57$, $p < .05$].

Racial-stereotype activation. We also obtained evidence that the intervention lessened the cognitive salience of the stereotype among African Americans. As the experiments yielded similar effects on the primary course performance outcome, and as condition effects on the activation measure did not vary by experiment [Fs < 1.7, *NS*], data were combined to increase statistical power in this ancillary analysis. African-American students generated fewer stereotype-relevant words in the affirmation condition (mean = 2.79, SD = 0.98) than in the control condition (mean = 3.25, SD = 0.91) [$t(105) = 2.56$, $p < .02$]. European-American students showed no condition effect (means = 3.07, 2.79; SDs = 1.08, 1.28, respectively) [$t < 1.3$, *NS*]. The control condition thus replicated the established pattern of African Americans' displaying higher racial-stereotype activation than European Americans in intellectually evaluative situations (Steele and Aronson, 1995) [$t(111) = 2.20$, $p = .03$]; the affirmation condition eliminated it [$t < 1.5$, *NS*]. The race $\times$ condition interaction was significant [$F(1, 218) = 6.61$, $p < .02$].

DISCUSSION

How did our intervention produce such apparently disproportionate effects? First, in normal school settings, a downward recursive performance cycle can occur where threat and poor performance feed off one another, leading to worsening performance over time. Such a downward spiral effect is indicated in the present research by (a) the steep linear decline in African Americans' performance in the control condition (see Figure 21.3); and (b) the fact that African Americans in the control condition whose performance sank early in the term, as indexed by a relatively large pre-intervention drop from block 1 to 2, performed worse later in the term by earning lower mean performance across blocks 3–10 [*partial* $r = -.52$,

$p < .01$, controlling for pre-intervention performance)]. This was not seen with European Americans in either condition [*partial* $r = +.24$, $P < 0.10$].

Second, because a recursive process depends on a continual feedback cycle, any interruption of that cycle could have long-term effects for its perpetuation (Storms and McCall, 1976; Wilson et al., 2002; Wilson and Linville, 1985). Such an interruption could occur if a small reduction in threat and its related stress took place, as this could lead to a slight improvement in subsequent performance, which, in turn, could further lessen performance-inhibiting threat and stress, leading to sustained or improving performance over time and perhaps setting off another, now positive, recursive cycle. Two of our results support this notion: (a) the intervention interrupted African Americans' downward trend and deflected it upward (see Figure 21.3); (b) an early performance drop for this group did *not* predict worse performance later [*partial* $r = 0.02$, *NS*]. The difference in correlations between the African-American conditions was significant [$z = -2.10$, $p < .05$].

Third, interventions can generate effects apart from those that occur at the time of their initial administration, and such effects can be large if they accumulate across multiple trials or tests like the multiple performance opportunities found in the classroom. As an analogy, in professional baseball, small differences in the number of successful at-bats during individual games can compound over an entire season and career and lead to one being considered an all-star rather than just another good player (Abelson, 1985). Likewise, in the classroom, a small but consistent intervention effect can compound into a large effect on final grades.

Fourth, as past research suggests, the psychological availability of mental concepts can affect the encoding and interpretation of social experience, and this can in turn affect long-term attitudes and behavior (Fiske and Taylor, 1991). Consistent with this possibility, our intervention reduced the psychological availability of the stereotype. This then could have changed the African Americans' perception of the level of bias in the environment, or the salience to them of such bias even if it was perceived, either of which could reduce their sense of stereotype threat. Such a change in perception could affect their interpretations of academic success and defeat over the long term.

Finally, our apparently disproportionate results rested on an obvious precondition. For our results to occur, adequate material, social, and psychological resources and support to permit and sustain positive academic outcomes must have existed in the school environment prior to our intervention. Students must also have had the skills to perform significantly better before its introduction. What appears to be a small or brief event in isolation was, we maintain, rather one component of a process that, when triggered, allowed other positive conditions, previously not fully realized in the situation, to manifest. The flicking of a switch, viewed in isolation, may seem a quick and minor physical movement, a movement that seems out of proportion with the effect of having a room or a city block instantaneously flooded with light.

Our findings, in conjunction with other research (Aronson et al., 2002; Good, Aronson, and Inzlicht, 2003; Blackwell, Trzesniewski, and Dweck, 2007; Wilson

and Linville, 1985), demonstrate that alleviating psychological threat can improve intellectual achievement in a real-world environment. Our intervention is among the first aimed purely at altering psychological experience to reduce the racial achievement gap, a major problem in the United States. Unlike most other interventions, it benefits the targeted students, in the present case those most at risk, reducing group-based inequality, and it does not adversely affect other students (see Ceci and Papierno, 2005). Our research highlights the importance of situational threats linked to group identity in understanding intellectual achievement in real-world, chronically evaluative settings. Our results challenge conventional and scientific wisdom by demonstrating that a psychologically targeted and well-timed intervention, even a brief one, can help reduce what many view as an intractable disparity in real-world academic outcomes.

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VI

HUMAN AGGRESSION

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22

The Effects of Observing Violence

Leonard Berkowitz

Experiments suggest that aggression depicted in television and motion picture dramas, or observed in actuality, can arouse certain members of the audience to violent action.

An ancient view of drama is that the action on the stage provides the spectators with an opportunity to release their own strong emotions harmlessly through identification with the people and events depicted in the play. This idea dates back at least as far as Aristotle, who wrote in *The Art of Poetry* that drama is "a representation . . . in the form of actions directly presented, not narrated; with incidents arousing pity and fear in such a way as to accomplish a purgation of such emotions."

Aristotle's concept of catharsis, a term derived from the Greek word for purgation, has survived in modern times. It can be heard on one side of the running debate over whether scenes of violence in motion pictures and television programs can instigate violent deeds, sooner or later, by people who observe such scenes. Eminent authorities contend that filmed violence, far from leading to real violence, can actually have beneficial results in that the viewer may purge himself of hostile impulses by watching other people behave aggressively, even if these people are merely actors appearing on a screen. On the other hand, authorities of equal stature contend that, as one psychiatrist told a Senate subcommittee, filmed violence is a "preparatory school for delinquency." In this view emotionally immature individuals can be seriously affected by fighting or brutality in films, and disturbed young people in particular can be led into the habit of expressing their aggressive energies by socially destructive actions.

Until recently neither of these arguments had the support of data obtained by controlled experimentation; they had to be regarded, therefore, as hypotheses, supported at best by unsystematic observation. Lately, however, several psychologists

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have undertaken laboratory tests of the effects of filmed aggression. The greater control obtained in these tests, some of which were done in my laboratory at the University of Wisconsin with the support of the National Science Foundation, provides a basis for some statements that have a fair probability of standing up under continued testing.

First, it is possible to suggest that the observation of aggression is more likely to induce hostile behavior than to drain off aggressive inclinations; that, in fact, motion picture or television violence can stimulate aggressive actions by normal people, as well as by those who are emotionally disturbed. I would add an important qualification: such actions by normal people will occur only under appropriate conditions. The experiments point to some of the conditions that might result in aggressive actions by people in an audience who had observed filmed violence.

Second, these findings have obvious social significance. Third, the laboratory tests provide some important information about aggressive behavior in general. I shall discuss these three statements in turn.

Catharsis appeared to have occurred in one of the first experiments, conducted by Seymour Feshbach of the University of Colorado. Feshbach deliberately angered a group of college men; then he showed part of the group a filmed prizefight and the other students a more neutral film. He found that the students who saw the prizefight exhibited less hostility than the other students on two tests of aggressiveness administered after the film showings. The findings may indicate that the students who had watched the prizefight had vented their anger vicariously.

That, of course, is not the only possible explanation of the results. The men who saw the filmed violence could have become uneasy about their own aggressive tendencies. Watching someone being hurt may have made them think that aggressive behavior was wrong; as a result they may have inhibited their hostile responses. Clearly there was scope for further experimentation, particularly studies varying the attitude of the subjects toward the filmed aggression.

Suppose the audience were put in a frame of mind to regard the film violence as justified—for instance because a villain got a beating he deserved. The concept of symbolic catharsis would predict in such a case that an angered person might enter vicariously into the scene and work off his anger by thinking of himself as the winning fighter, who was inflicting injury on the man who had provoked him. Instead of accepting this thesis, my associates and I predicted that justified film aggression would lead to stronger rather than weaker manifestations of hostility. We believed

FIGURE 22.1
Typical experiment tests reaction of angered man to filmed violence. Experiment begins with introduction of subject *(white shirt)* to a man he believes is a coworker but who actually is a confederate of the author's. In keeping with pretense that experiment is to test physiological reactions, student conducting the experiment takes blood-pressure readings. He assigns the men a task and leaves; during the task, the confederate insults the subject. Experimenter returns and shows filmed prizefight. Confederate leaves; experimenter tells subject to judge a floor plan drawn by confederate and to record opinion by giving confederate electric shocks. Shocks actually go to recording apparatus. The fight film appeared to stimulate the aggressiveness of angered men. (Photographs by Gordon Coster.)

that the rather low volume of open hostility in the Feshbach experiment was attributable to film-induced inhibitions. If this were so, an angered person who saw what appeared to be warranted aggression might well think he was justified in expressing his own hostile desires.

To test this hypothesis we conducted three experiments. Since they resulted in essentially similar findings and employed comparable procedures, I shall describe only the latest. In this experiment we brought together two male college students at a time. One of them was the subject; the other was a confederate of the experimenter and had been coached on how to act, although of course none of this was known to the subject. Sometimes we introduced the confederate to the subject as a college boxer and at other times we identified him as a speech major. After the introduction the experimenter announced that the purpose of the experiment was to study physiological reactions to various tasks. In keeping with that motif he took blood-pressure readings from each man. Then he set the pair to work on the first task: a simple intelligence test.

During this task the confederate either deliberately insulted the subject—for example, by remarks to the effect that "You're certainly taking a long time with that" and references to "cow-college students" at Wisconsin—or, in the conditions where we were not trying to anger the subject, behaved in a neutral way toward him. On the completion of the task the experimenter took more blood-pressure readings (again only to keep up the pretense that the experiment had a physiological purpose) and then informed the men that their next assignment was to watch a brief motion picture scene. He added that he would give them a synopsis of the plot so that they would have a better understanding of the scene. Actually he was equipped with two different synopses.

To half the subjects he portrayed the protagonist of the film, who was to receive a serious beating, as an unprincipled scoundrel. Our idea was that the subjects told this story would regard the beating as retribution for the protagonist's misdeeds; some tests we administered in connection with the experiment showed that the subjects indeed had little sympathy for the protagonist. We called the situation we had created with this synopsis of the seven-minute fight scene the "justified fantasy aggression."

The other subjects were given a more favorable description of the protagonist. He had behaved badly, they were told, but this was because he had been victimized when he was young; at any rate, he was now about to turn over a new leaf. Our idea was that the men in this group would feel sympathetic toward the protagonist; again tests indicated that they did. We called this situation the "less justified fantasy aggression."

Then we presented the film, which was from the movie *Champion;* the seven-minute section we used showed Kirk Douglas, as the champion, apparently losing his title. Thereafter, to measure the effects of the film, we provided the subjects with an opportunity to show aggression in circumstances where that would be a socially acceptable response. We separated each subject and accomplice and told the subject that his coworker (the confederate) was to devise a "creative" floor plan for

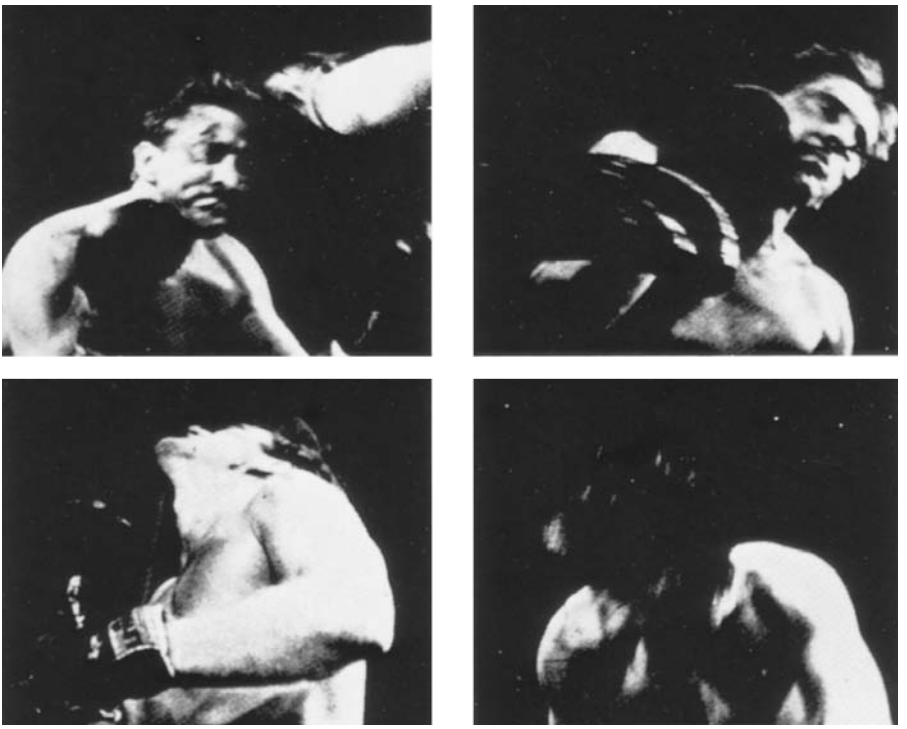


FIGURE 22.2
Filmed aggression shown in author's experiments was from the motion picture *Champion* and included these scenes in which Kirk Douglas receives a bad beating. Watchers had been variously prepared; after showing, they were tested for aggressive tendencies. (Astor Pictures, Inc.)

a dwelling, which the subject would judge. If the subject thought the floor plan was highly creative, he was to give the coworker one electric shock by depressing a telegraph key. If he thought the floor plan was poor, he was to administer more than one shock; the worse the floor plan, the greater the number of shocks. Actually each subject received the same floor plan.

The results consistently showed a greater volume of aggression directed against the anger-arousing confederate by the men who had seen the "bad guy" take a beating than by the men who had been led to feel sympathy for the protagonist in the film (see Figure 22.3). It was clear that the people who saw the justified movie violence had not discharged their anger through vicarious participation in the aggression but instead had felt freer to attack their tormenter in the next room. The motion picture scene had apparently influenced their judgment of the propriety of aggression. If it was all right for the movie villain to be injured aggressively, they seemed to think, then perhaps it was all right for them to attack the villain in their own lives—the person who had insulted them.

Another of our experiments similarly demonstrated that observed aggression has little if any effectiveness in reducing aggressive tendencies on the part of an

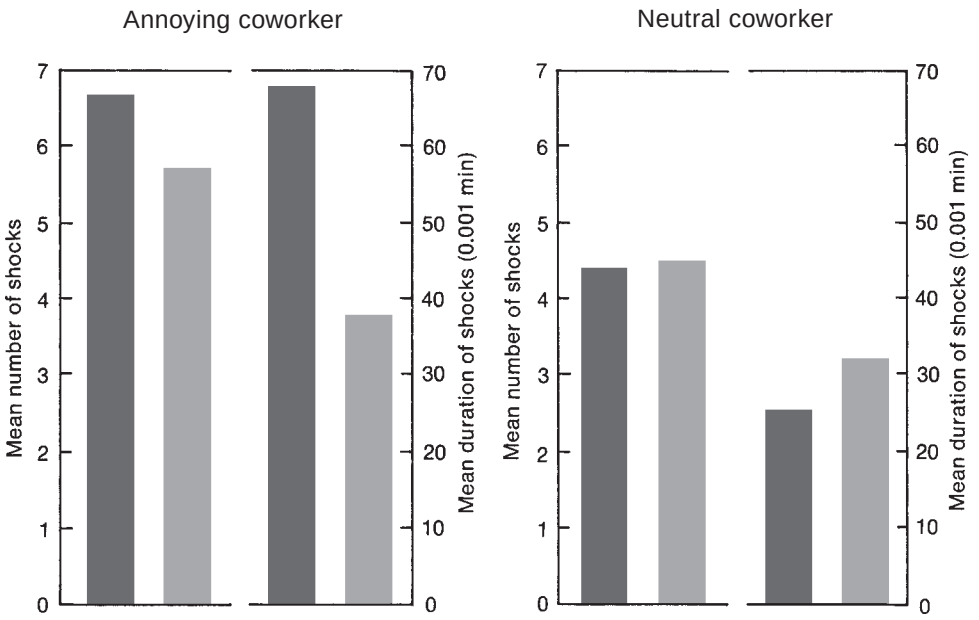


FIGURE 22.3
Responses of subjects invited to commit aggression after seeing prizefighting film varied according to synopsis they heard beforehand. One *(dark gray)* called Douglas's beating deserved; the other *(light gray)* said it was undeserved. After the film the subjects were told they could give electric shocks to an annoying or neutral coworker based on his "creativeness" in doing a task. Seeing a man receive what had been described as a well-deserved beating apparently lowered restraints against aggressive behavior.

observer. In this experiment some angered men were told by another student how many shocks they should give the person, supposedly in the next room, who had provoked them. Another group of angered men, instead of delivering the shocks themselves, watched the other student do it. Later the members of both groups had an opportunity to deliver the shocks personally. Consistently the men who had watched in the first part of the experiment now displayed stronger aggression than did the people who had been able to administer shocks earlier. Witnessed aggression appeared to have been less satisfying than self-performed aggression.

Our experiments thus cast considerable doubt on the possibility of a cathartic purge of anger through the observation of filmed violence. At the very least, the findings indicated that such a catharsis does not occur as readily as many authorities have thought.

Yet what about the undoubted fact that aggressive motion pictures and violent athletic contests provide relaxation and enjoyment for some people? A person who was tense with anger sometimes comes away from them feeling calmer. It seems to me that what happens here is quite simple: He calms down not because he has discharged his anger vicariously but because he was carried away by the events he witnessed. Not thinking of his troubles, he ceased to stir himself up and his anger

dissipated. In addition, the enjoyable motion picture or game could have cast a pleasant glow over his whole outlook, at least temporarily.

The social implications of our experiments have to do primarily with the moral usually taught by films. Supervising agencies in the motion picture and television industries generally insist that films convey the idea that "crime does not pay." If there is any consistent principle used by these agencies to regulate how punishment should be administered to the screen villain, it would seem to be the talion law: an eye for an eye, a tooth for a tooth.

Presumably the audience finds this concept of retaliation emotionally satisfying. Indeed, we based our "justified fantasy aggression" situation on the concept that people seem to approve of hurting a scoundrel who has hurt others. But however satisfying the talion principle may be, screenplays based on it can lead to socially harmful consequences. If the criminal or "bad guy" is punished aggressively, so that others do to him what he has done to them, the violence appears justified. Inherent in the likelihood that the audience will regard it as justified is the danger that some angered person in the audience will attack someone who has frustrated *him*, or perhaps even some innocent person he happens to associate with the source of his anger.

Several experiments have lent support to this hypothesis. O. Ivar Lövaas of the University of Washington found in an experiment with nursery school children that the youngsters who had been exposed to an aggressive cartoon film displayed more aggressive responses with a toy immediately afterward than a control group shown a less aggressive film did. In another study Albert Bandura and his colleagues at Stanford University noted that preschool children who witnessed the actions of an aggressive adult in a motion picture tended later, after they had been subjected to mild frustrations, to imitate the kind of hostile behavior they had seen.

This tendency of filmed violence to stimulate aggression is not limited to children. Richard H. Walters of the University of Waterloo in Ontario found experimentally that male hospital attendants who had been shown a movie of a knife fight generally administered more severe punishment to another person soon afterward than did other attendants who had seen a more innocuous movie. The men in this experiment were shown one of the two movie scenes and then participated in what was supposedly a study of the effects of punishment. They were to give an electric shock to someone else in the room with them each time the person made a mistake on a learning task. The intensity of the electric shocks could be varied. This other person, who was actually the experimenter's confederate, made a constant number of mistakes, but the people who had seen the knife fight gave him more intense punishment than the men who had witnessed the nonaggressive film. The filmed violence had apparently aroused aggressive tendencies in the men and, since the situation allowed the expression of aggression, their tendencies were readily translated into severe aggressive actions.

These experiments, taken together with our findings, suggest a change in approach to the manner in which screenplays make their moral point. Although it

may be socially desirable for a villain to receive his just deserts at the end of a motion picture, it would seem equally desirable that this retribution should not take the form of physical aggression.

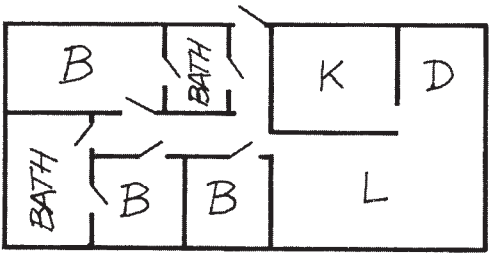
The key point to be made about aggressiveness on the basis of experimentation in this area is that a person's hostile tendencies will persist, in spite of any satisfaction he may derive from filmed violence, to the extent that his frustrations and aggressive habits persist. There is no free-floating aggressive energy that can be released through attempts to master other drives, as Freud proposed, or by observing others as they act aggressively.

In fact, there have been studies suggesting that even if the angered person performs the aggression himself, his hostile inclinations are not satisfied unless he believes he has attacked his tormentor and not someone else. J. E. Hokanson of Florida State University has shown that angered subjects permitted to commit aggression against the person who had annoyed them often display a drop in systolic blood pressure. They seem to have experienced a physiological relaxation, as if they had satisfied their aggressive urges. Systolic pressure declines less, however, when the angered people carry out the identical motor activity involved in the aggression but without believing they have attacked the source of their frustration.

I must now qualify some of the observations I have made. Many aggressive motion pictures and television programs have been presented to the public, but the number of aggressive incidents demonstrably attributable to such shows is quite low. One explanation for this is that most social situations, unlike the conditions in the experiments I have described, impose constraints on aggression. People are usually aware of the social norms prohibiting attacks on others, consequently they inhibit whatever hostile inclinations might have been aroused by the violent films they have just seen.

Another important factor is the attributes of the people encountered by a person after he has viewed filmed violence. A man who is emotionally aroused does not necessarily attack just anyone. Rather, his aggression is directed toward specific objectives. In other words, only certain people are capable of drawing aggressive responses from him. In my theoretical analyses of the sources of aggressive behavior I have suggested that the arousal of anger only creates a readiness for aggression. The theory holds that whether this predisposition is translated into actual aggression depends on the presence of appropriate cues: stimuli associated with the present or previous instigators of anger. Thus if someone has been insulted, the sight or the thought of others who have provoked him, whether then or earlier, may evoke hostile responses from him.

An experiment I conducted in conjunction with a graduate student provides some support for this train of thought. People who had been deliberately provoked by the experimenter were put to work with two other people, one a person who had angered them earlier and the other a neutral person. The subjects showed the greatest hostility, following their frustration by the experimenter, to the coworker they disliked. He, by having thwarted them previously, had acquired the stimulus quality that caused him to draw aggression from them after they had been aroused by the experimenter.



Subject	**A**	B	**C**	D	**E**	F	**G**	H
Number of shocks	**6**	3	**8**	3	**6**	7	**5**	4
Duration (0.001 min)	**46**	38	**76**	10	**120**	49	**60**	28

FIGURE 22.4
Task by annoying coworker supposedly was to draw a floor plan. Actually, each subject saw the floor plan shown here. The subject was asked to judge the creativeness of the plan and to record his opinion by pressing a telegraph key that he thought would give electric shocks to the coworker; one shock for a good job and more for poor work. Responses of eight subjects who saw prizefight film are shown; those in boldface type represent men told that Douglas deserved his beating; those in lightface type, men informed it was undeserved.

My general line of reasoning leads me to some predictions about aggressive behavior. In the absence of any strong inhibitions against aggression, people who have recently been angered and have then seen filmed aggression will be more likely to act aggressively than people who have not had those experiences. Moreover, their strongest attacks will be directed at those who are most directly connected with the provocation or at others who either have close associations with the aggressive motion picture or are disliked for any reason.

One of our experiments showed results consistent with this analysis. In this study male college students, taken separately, were first either angered or not angered by *A*, one of the two graduate students acting as experimenters. *A* had been introduced earlier either as a college boxer or as a speech major. After *A* had had his session with the subject, *B*, the second experimenter, showed the subject a motion picture: either the prizefight scene mentioned earlier or a neutral film. (One that we used was about canal boats in England; the other, about the travels of Marco Polo.)

We hypothesized that the label "college boxer" applied to *A* in some of the cases would produce a strong association in the subject's mind between *A* and the boxing film. In other words, any aggressive tendencies aroused in the subject would be more likely to be directed at *A* the college boxer than at *A* the speech major. The experiment bore out this hypothesis. Using questionnaires at the end of the session as the measures of hostility, we found that the deliberately angered subjects directed more hostility at *A*, the source of their anger, when they had seen the fight film and he had been identified as a boxer. Angered men who had seen the neutral

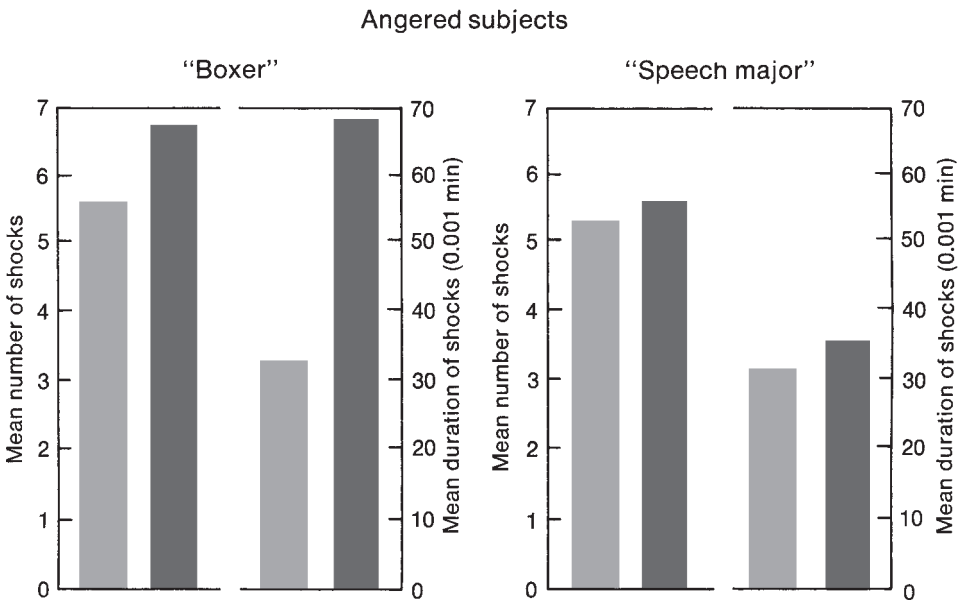


FIGURE 22.5
Coworkers introduction also produced variations in aggressiveness of subjects. Coworker was introduced as boxer or as speech major; reactions shown here are of men who were angered by coworker and then saw either a fight film *(dark gray)* or neutral film *(light gray).* Coworker received strongest attacks when subjects presumably associated with fight film.

film showed no particular hostility to *A* the boxer. In short, the insulting experimenter received the strongest verbal attacks when he was also associated with the aggressive film. It is also noteworthy that in this study the boxing film did not influence the amount of hostility shown toward *A* when he had not provoked the subjects.

A somewhat inconsistent note was introduced by our experiments. The nonangered groups, regardless of which film they saw, gave the confederate more and longer shocks when they thought he was a boxer than when they understood him to be a speech major (see Figure 22.6). To explain this finding I assume that our subjects had a negative attitude toward boxers in general. This attitude may have given the confederate playing the role of boxer the stimulus quality that caused him to draw aggression from the angered subjects. But it could only have been partially responsible, since the insulted subjects who saw the neutral film gave fewer shocks to the boxer than did the insulted subjects who saw the prizefight film.

Associations between the screen and the real world are important. People seem to be emotionally affected by a screenplay to the extent that they associate the events of the drama with their own life experiences. Probably adults are less strongly influenced than children because they are aware that the film is make-believe and so can dissociate it from their own lives. Still, it seems clear from the experiments I have described that an aggressive film can induce aggressive actions

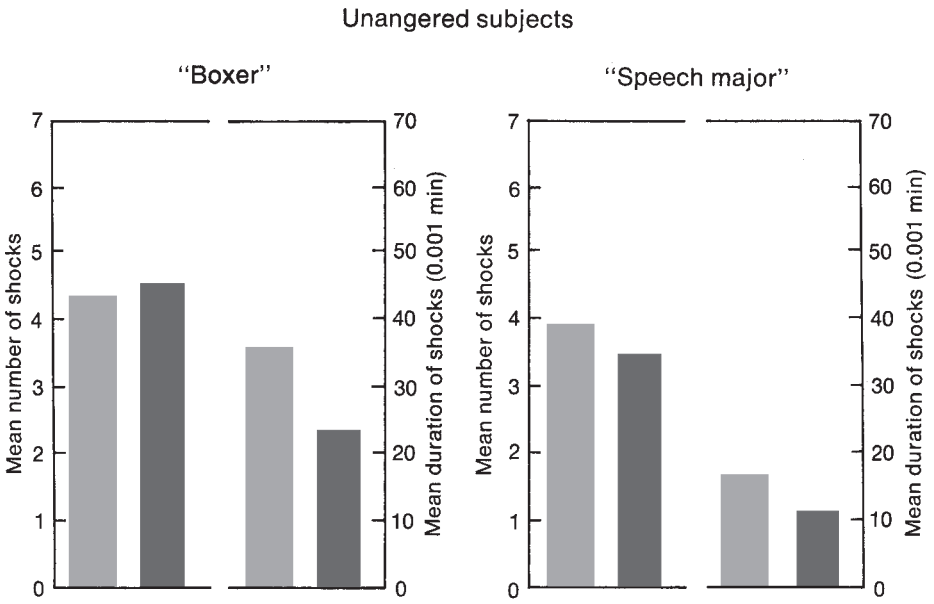


FIGURE 22.6
Similar test, varied by the fact that the coworker behaved neutrally toward the subjects and therefore presumably did not anger them, produced these reactions. The greater number of shocks given to the coworker introduced as a boxer than to the one introduced as a speech major apparently reflected a tendency to take a generally negative attitude toward persons identified as boxers.

by anyone in the audience. In most instances I would expect that effect to be short-lived. The emotional reaction produced by filmed violence probably dies away rather rapidly as the viewer enters new situations and encounters new stimuli. Subjected to different influences, he becomes less and less ready to attack other people.

Television and motion pictures, however, may also have some persistent effects. If a young child sees repeatedly that screen heroes gain their ends through aggressive actions, he may conclude that aggression is desirable behavior. Fortunately screenplays do not consistently convey that message, and in any event the child is exposed to many other cultural norms that discourage aggression.

As I see it, the major social danger inherent in filmed violence has to do with the temporary effects produced in a fairly short period immediately following the film. For that period, at least, a person—whether an adult or a child—who had just seen filmed violence might conclude that he was warranted in attacking those people in his own life who had recently frustrated him. Further, the film might activate his aggressive habits so that for the period of which I speak he would be primed to act aggressively. Should he then encounter people with appropriate stimulus qualities, people he dislikes or connects psychologically with the film, this predisposition could lead to open aggression.

What, then, of catharsis? I would not deny that it exists. Nor would I reject the argument that a frustrated person can enjoy fantasy aggression because he sees

characters doing things he wishes he could do, although in most cases his inhibitions restrain him. I believe, however, that effective catharsis occurs only when an angered person perceives that his frustrater has been aggressively injured. From this I argue that filmed violence is potentially dangerous. The motion picture aggression has increased the chance that an angry person, and possibly other people as well, will attack someone else.

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23

Does Venting Anger Feed or Extinguish the Flame? Catharsis, Rumination, Distraction, Anger, and Aggressive Responding

Brad J. Bushman

Does distraction or rumination work better to diffuse anger? Catharsis theory predicts that rumination works best, but empirical evidence is lacking. In this study, angered participants hit a punching bag and thought about the person who had angered them (rumination group) or thought about becoming physically fit (distraction group). After hitting the punching bag, they reported how angry they felt. Next, they were given the chance to administer loud blasts of noise to the person who had angered them. There also was a no punching bag control group. People in the rumination group felt angrier than did people in the distraction or control groups. People in the rumination group were also most aggressive, followed respectively by people in the distraction and control groups. Rumination increased rather than decreased anger and aggression. Doing nothing at all was more effective than venting anger. These results directly contradict catharsis theory.

The belief in the value of venting anger has become widespread in our culture. In movies, magazine articles, and even on billboards, people are encouraged to vent their anger and "blow off steam." For example, in the movie *Analyze This,* a psychiatrist (played by Billy Crystal) tells his New York gangster client (played by Robert De Niro), "You know what I do when I'm angry? I hit a pillow. Try that." The client promptly pulls out his gun, points it at the couch, and fires several bullets into the pillow. "Feel better?" asks the psychiatrist. "Yeah, I do," says the gunman. In a *Vogue* magazine article, female model Shalom concludes that boxing helps her release pent-up anger. She said,

> I found myself looking forward to the chance to pound out the frustrations of the week against Carlos's (her trainer) mitts. Let's face it: A personal boxing trainer has

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> advantages over a husband or lover. He won't look at you accusingly and say, "I don't know where this irritation is coming from." . . . Your boxing trainer knows it's in there. And he wants you to give it to him. ("Fighting Fit," 1993, p. 179)

In a *New York Times Magazine* article about hate crimes, Andrew Sullivan writes, "Some expression of prejudice serves a useful purpose. It lets off steam; it allows natural tensions to express themselves incrementally; it can siphon off conflict through words, rather than actions" (Sullivan, 1999, p. 113). A large billboard in Missouri states, "Hit a Pillow, Hit a Wall, But Don't Hit Your Kids!"

Catharsis Theory

The theory of catharsis is one popular and authoritative statement that venting one's anger will produce a positive improvement in one's psychological state. The word *catharsis* comes from the Greek word *katharsis,* which literally translated means a cleansing or purging. According to catharsis theory, acting aggressively or even viewing aggression is an effective way to purge angry and aggressive feelings.

Sigmund Freud believed that repressed negative emotions could build up inside an individual and cause psychological symptoms, such as hysteria (nervous outbursts). Breuer and Freud (1893–1895/1955) proposed that the treatment of hysteria required the discharge of the emotional state previously associated with trauma. They claimed that for interpersonal traumas, such as insults and threats to the ego, emotional expression could be obtained through direct aggression: "The reaction of an injured person to a trauma has really only a . . . 'cathartic' effect if it is expressed in an adequate reaction like revenge" (p. 5). Breuer and Freud believed that expressing anger was much better than bottling it up inside.

Freud's therapeutic ideas on emotional catharsis form the basis of the hydraulic model of anger. The hydraulic model suggests that frustrations lead to anger and that anger, in turn, builds up inside an individual, similar to hydraulic pressure inside a closed environment, until it is released in some way. If people do not let their anger out but try to keep it bottled up inside, it will eventually cause them to explode in an aggressive rage. The modern theories of catharsis are based on this model. Catharsis is seen as a way of relieving the pressure that the anger creates inside the psyche. The core idea is that it is better to let the anger out here and there in little bits as opposed to keeping it inside as it builds up to the point at which a more dangerous explosion results.

If venting really does get anger "out of your system," then venting should decrease aggression because people are less angry. Almost as soon as psychology researchers began conducting scientific tests of catharsis theory, the theory ran into trouble. In one of the first experiments on the topic (Hornberger, 1959), participants first received an insulting remark from a confederate. Next, half of the participants pounded nails for 10 minutes—an activity that resembles many of the "venting" techniques that people who believe in catharsis continue to recommend even today. The other half did not get a chance to vent their anger by pounding nails. Af-

ter this, all participants had a chance to criticize the person who had insulted them. If catharsis theory is true, the act of pounding nails should reduce subsequent aggression. The results showed the opposite effect. The people who had hammered the nails were more (rather than less) hostile toward the confederate afterward than were the ones who did not get to pound any nails.

In 1973, Albert Bandura issued a statement calling for a moratorium on catharsis theory and the use of venting in therapy. Four years later, Geen and Quanty (1977) published their influential review of catharsis theory in *Advances in Experimental Social Psychology.* After reviewing the relevant data, they concluded that venting anger does not reduce aggression. If anything, they concluded, it makes people more aggressive afterward. More recent research has come to similar conclusions (e.g., Bushman, Baumeister, and Stack, 1999). Geen and Quanty also concluded that venting anger can reduce physiological arousal but people must express their anger directly against the provocateur. People also must believe that the provocateur will not retaliate. Venting against substitute targets does not reduce arousal.

Cognitive Neoassociation Theory

According to cognitive neoassociation theory (Berkowitz, 1993), aversive events (e.g., frustrations, provocations, hot temperatures) produce negative affect. Negative affect, in turn, automatically stimulates thoughts, memories, expressive motor reactions, and physiological responses associated with both fight and flight tendencies. The fight associations give rise to rudimentary feelings of anger, whereas the flight associations give rise to rudimentary feelings of fear.

Cognitive neoassociation theory posits that aggressive thoughts are linked together in memory, thereby forming an associative network. Once an aggressive thought is processed or stimulated, activation spreads out along the network links and primes or activates associated thoughts as well. Not only are associated aggressive thoughts linked together in memory, but thoughts are also linked along the same sort of associative lines to emotional reactions and action tendencies (Bower, 1981; Lang, 1979). Thus, the activation of aggressive thoughts can engender a complex of associations consisting of aggressive ideas, emotions related to violence, and the impetus for aggressive actions.

Cognitive neoassociation theory predicts that venting should increase rather than decrease angry feelings and aggressive behaviors. Venting involves behaving aggressively, often against "safe" inanimate objects. To vent, people punch pillows, wallop punching bags, beat on couches with foam baseball hats, throw dishes on the ground, kick trash cans, scream and swear into pillows, and so forth. In essence, venting is practicing how to behave aggressively. Such aggressive activity should prime aggressive thoughts, feelings, and behavioral tendencies, especially if the people think about the source of their anger while venting. Thus, venting should keep angry feelings active in memory and also should increase the likelihood of subsequent aggressive responses.

Rumination and Distraction

Most pop psychology and self-help books implicitly assume that people are ruminating about their provocateur while venting anger. Some authors, however, are more explicit. For example, John Lee (1993) gives the following advice to angry people in his popular book *Facing the Fire: Experiencing and Expressing Anger Appropriately.*

> Punch a pillow or a punching bag. Punch with all the frenzy you can. If you are angry at a particular person, imagine his or her face on the pillow or punching bag, and vent your rage physically and verbally. You will be doing violence to a pillow or punching bag so that you can stop doing violence to yourself by holding in poisonous anger. (p. 96)

Some devices for venting anger make it easy for people to ruminate about their provocateur. For example, consider the following advertisement from a toy catalog.

> WHEN YOU NEED SOMETHING THAT WON'T HIT BACK.*Wham-It* stands 42″ tall and takes abuse from kids and adults alike. When you feel like you just have to strike out, *Wham-It* is always on call. New clear vinyl pocket lets you insert a photo or drawing.

Rumination is defined as "self-focused attention," or directing attention inward on the self, and particularly on one's negative mood (Lyubomirsky and Nolen-Hoeksma, 1995). Any process that serves to exacerbate a negative mood, such as rumination, should increase anger and aggression. In contrast, any process that distracts attention away from an angry mood should reduce anger and aggression. If provoked individuals are induced to think about how they feel, they will maintain, or exacerbate, their angry mood. If they are induced to think about something else, however, the anger will dissipate in time.

Previous research has shown that rumination increases angry feelings. In one study (Rusting and Nolen-Hoeksma, 1998), college students were angered by reading a story about a professor who treated a student unfairly and were told to imagine themselves in a similar situation. Some students ruminated by writing about emotion-focused and self-focused topics (i.e., "Why do you think the way you do"), whereas others were distracted by writing about nonemotional, irrelevant topics (i.e., "the layout of the local post office"). Participants who ruminated for 20 minutes reported being angrier than did participants who were distracted. Another study found that aggression toward an insulting confederate was decreased by having people solve distracting math problems (Konecni, 1974). Solving the math problems presumably distracted people from the source of their anger. Two other studies found that rumination increased displaced aggression after a minor triggering event (Bushman, Bonacci, Pedersen, Vasquez, and Miller, 2005). In Study 1, provoked participants focused attention on or away from their negative mood and later engaged in displaced aggression against a competent or fumbling confederate. Provoked participants who ruminated engaged in more displaced aggression against the fumbling confederate than did participants who were distracted. Study 2

replicated the findings from Study 1 using different operational definitions and a substantially longer (8-hour) rumination period.

To date, no research has examined the effects of rumination and distraction in the effects of venting activities on anger and subsequent aggression. According to cognitive neoassociation theory, ruminating while venting should prime aggressive thoughts, feelings, and behavioral tendencies.

Overview

In the present study, 600 college students (300 men, 300 women) were first angered by another participant who criticized an essay they had written. In fact, there was no other participant. Next, participants were randomly assigned to rumination, distraction, or control groups. Participants in the rumination group hit a punching bag as long, as hard, and as many times as they wanted to. While they hit the bag. they were told to think about the other participant who had criticized their essay. For a visual aid, they were shown a photo ID of a same-sex college student described as the "other participant" on a 15-inch computer monitor. Participants in the distraction group also hit a punching bag as long, as hard, and as many times as they wanted to. While they hit the bag, they were told to think about becoming physically fit. As a visual aid, they were shown a photo ID of a same-sex athlete from a health magazine on a 15-inch computer monitor. Participants in the control group did not hit the punching bag. Instead, they sat quietly for a couple minutes while the experimenter supposedly worked on the other participant's computer. No attempt was made to reduce the anger of participants in the control group. Anger was measured using a mood form. Aggression was measured by allowing participants to blast their provocateur with loud and long noises through a pair of headphones on a competitive reaction time task. Catharsis theory would predict the lowest levels of anger and aggression among participants in the rumination condition. Cognitive neoassociation theory would predict the exact opposite results.

METHOD

Participants

Participants were 602 undergraduate college students (300 men and 302 women) enrolled in introductory psychology courses.[1] Students received extra course credit in exchange for their voluntary participation. The data from 2 women were discarded

[1]According to Cohen (1988), most of the effects in the social sciences are small to moderate in size. I assumed that the effect obtained in the present study would be in this range. A power analysis (Cohen, 1988) revealed that with power = .80 and two-sided significance level = .05, 400 participants were needed in each group to detect a small effect (i.e., $d = 0.20$) and 64 participants were needed in each group to detect a moderate effect (i.e., $d = 0.50$). Thus, the present study included 200 participants in each group.

because they refused to hit the punching bag. The final sample consisted of 300 men and 300 women. There were 100 men and 100 women in each of the three experimental conditions (i.e., rumination, distraction, control).

Procedure

Participants were tested individually, but each was led to believe that he or she would be interacting with another participant of the same sex. They were told that the researchers were studying first impressions.

After giving informed consent, each participant wrote a one-paragraph essay on abortion, either prochoice or prolife (whichever the participant supported). After finishing, the participant's essay was taken away to be shown to the other participant (who was in fact nonexistent) for evaluation. Meanwhile, the participant was permitted to evaluate the partner's essay, which expressed the opposite view on abortion (e.g., if the participant's essay was prochoice, the partner's essay was prolife).

A short time later, the experimenter brought the participant's own essay back with comments ostensibly made by the other participant. All participants received bad evaluations consisting of negative ratings on organization, originality, writing style, clarity of expression, persuasiveness of arguments, and overall quality. The ratings ranged from −10 to −8 on a 21-point scale ranging from −10 *(very bad)* to +10 *(very good)*. There was also a handwritten comment stating "This is one of the worst essays I have read!" Previous research has shown that this procedure makes people quite angry (e.g., Bushman and Baumeister, 1998; Bushman, Baumeister, and Phillips, 2001; Bushman et al., 1999).

After reading the evaluation, the participants rated how much they wanted to perform each of 10 activities on a list. Included in this list of activities was "hitting a punching bag." Other activities included playing solitaire, reading a short story, watching a comedy, and playing a computer game. Ratings were made on a 10-point scale ranging from 1 *(not at all)* to 10 *(extremely)*.

The punching bag manipulation came next. Two thirds of participants received the punching bag procedure. If the participant did not rank the punching bag activity first, the experimenter asked if the participant would be willing to hit the punching bag, explaining that ratings were needed for each activity on the list and that more ratings were needed for the punching bag activity. By requesting the participant to agree, we were able to ensure that the punching bag activity was the result of choice by all participants, including those who had not originally listed it as their top choice.

Participants who received the punching bag procedure were told that because physical appearance could influence their impression of their partner, a coin would he tossed to determine whether they would know what their partner looked like. On the basis of the coin toss, participants were assigned to rumination or distraction conditions. Participants in the rumination condition were told that they would

know what their partner looked like. On a 15-inch computer monitor, participants were shown a photo ID of another Iowa State University student of the same sex. The experimenter actually rolled a die to determine which of six photo IDs to show. The names and identification numbers were removed from all IDs. The experimenter then gave the participant some boxing gloves and demonstrated how to hit the 70-pound punching bag (Everlast, Model 4820). Participants were told that they should think about their partner while hitting the bag.[2]

Participants in the distraction condition were told that they would not know what their partner looked like. Instead of thinking about their partner while hitting the bag, they were told to think about becoming physically fit. Instead of seeing a photo ID of their partner on the computer screen, they saw a photo of someone of the same sex exercising. The photos were taken from fitness magazines and the experimenter rolled a die to determine which photo to show.

Participants in both the rumination and distraction condition were told that their partner would not see them (because of the coin toss). The participant was left alone to hit the punching bag. They were told they could hit it as long and as many times as they wanted to. Because there was an intercom system in the participant's room, the experimenter was able to time how long the participant hit the bag and count the number of times the participant hit the bag. The experimenter also rated how hard the participant hit the bag on a 10-point scale ranging from 1 *(very soft)* to 10 *(very hard).* The experimenter also asked the participant how hard he or she hit the bag (using the same 10-point scale). Participants then indicated how much they enjoyed hitting the punching bag on a 10-point scale ranging from 1 *(not at all*) to 10 *(extremely).*

Participants in the control condition did not hit the punching bag. Instead, they sat quietly for 2 minutes. The justification for the delay was that the experimenter was fixing their partner's computer. No attempt was made to reduce participant's anger during the 2-minute delay. Instead, participants in the no punching bag group did nothing at all. This allowed a test of the hypothesis that angry people are better off doing nothing at all than engaging in cathartic activities.

Next, participants completed a mood form that measured anger and positive affect. The anger measure consisted of 15 adjectives (e.g., *angry, annoyed, furious)* from the hostility subscale of the revised Multiple Affect Adjective Checklist (Zuckerman and Lubin, 1985). The positive affect measure consisted of 10 adjectives (e.g., *alert, determined, enthusiastic)* from the positive affect subscale of the Positive and Negative Affect Schedule (Watson, Clarke, and Tellegen, 1988). Watson and his colleagues define positive affect as a state of "high energy, full concentration, and pleasurable engagement" (p. 1063). All adjectives were rated along a 5-point Likert-type scale. where 1 = *very slightly or not at all,* 2 = *a little,* 3 = *moderately,* 4 = *quite a bit,* and 5 = *extremely.* Participants were told to "indicate to what extent you feel this way

[2]One man in the rumination group became so angry while hitting the punching bag that he also punched a hole in the laboratory wall.

right now, that is, at the present moment." The alpha coefficients for the measures of anger and positive affect were .88 and .89, respectively.

The next part of the procedure was presented as a competitive reaction time task, based on a paradigm developed by Taylor (1967). Previous studies have established the construct validity of this paradigm (e.g., Bernstein, Richardson, and Hammock, 1987; Giancola and Zeichner, 1995). The participant was told that he or she and the partner would have to press a button as fast as possible on each trial and whoever was slower would receive a blast of noise. The participant was permitted to set in advance the intensity of the noise that the other person would receive between 60 decibels (level 1) and 105 decibels (level 10) if the other lost. A nonaggressive no-noise setting (level 0) also was offered. In addition to deciding the intensity, the winner decided the duration of the loser's suffering because the duration of the noise depended on how long the winner held the button pressed down. In effect, each participant controlled a weapon that could be used to blast the other person if the participant won the competition to react faster.

The reaction time task consisted of 25 trials. After the initial (no provocation) trial, the remaining 24 trials were divided into three blocks with eight trials in each block. Within each block of trials, the other participant set random noise levels (ranging from 65 decibels to 100 decibels) and random noise durations (ranging from 0.25 seconds to 2.5 seconds). The participant heard noise on half of the trials within each block (randomly determined). An iMac computer controlled the events in the reaction time task and recorded the noise levels and noise durations the participant set for the other person. The white noise was delivered through a pair of Telephonics TDH-39P headphones.

Half the participants completed the mood form first, followed by the competitive reaction time task. The other half of the participants completed the competitive reaction time task first, followed by the mood checklist. A full oral debriefing (with probe for suspicion) followed. Because none of the participants expressed any suspicion, all 600 were included in the data analyses.

RESULTS

Preliminary Analyses

Counterbalance Order. Counterbalance order did not significantly influence responses on any of the measures (*p*s > .05). Thus, data from the two counterbalance orders were combined for subsequent analyses.

Punching Bag Preference. It was important to determine whether participants in the three groups differed in their desire to hit the punching bag after they had been angered. Because participants were randomly assigned to conditions, no differences were expected. We also tested for sex differences in punching bag pref-

erences. Because aggressive activities are more socially acceptable among men than among women, punching bag preferences were expected to be higher among men.

Desire to hit the punching bag. Ratings of how much participants wanted to hit the punching bag were analyzed using a 3 (ntmination, distraction, control) × 2 (men, women) analysis of variance. Men wanted to hit the punching bag more than did women, M= 4.33, SD = 2.77, and AI = 3.10, SD = 2.33, F(l, 588) = 33.87, p < .0001, d = 0.48. As expected, the effects involving experimental condition were nonsignificant (ps > .05).

Punching bag selected as top activity choice. Whether participants selected hitting a punching bag as their top choice of activities was a dichotomous variable (1 = selected hitting a punching bag as top choice, 0 = selected another activity as top choice). Thus, these data were analyzed using a 3 (rumination, distraction, control) × 2 (men, women) log-linear analysis. Men were more likely to select hitting a punching bag as their top choice than were women, 6% and 1%, χ^2 (1, N = 600) = 6.58, p < .01, ϕ = .13. As expected, the effects involving experimental condition were nonsignificant (ps > .05)

Punching Bag Measures. It was important to test whether participants in the rumination group vented more than did participants in the distraction group.

How hard the punching bag was hit. The intraclass correlation between experimenter and participant ratings of how hard the bag was hit was .69 (Shrout and Fleiss, 1979). The same pattern of results also was found for the two ratings. Thus, the two ratings were averaged.

Overall, men hit the punching bag harder than did women, M = 6.69, SD = 2.05, and M = 4.73, SD = 1.88, F(1, 396) = 99.14, p < .0001, d = 1.00. No other effects were significant (ps > .05).

Number of times punching bag was hit. Participants who thought about becoming physically fit hit the punching bag more times than did participants who thought about the person who insulted them, M = 127.5, SD = 63.5. and M = 112.2, SD = 57.5, F(1, 396) = 6.31, p < .05, d = 0.25. In other words, participants in the rumination group vented less than did participants in the distraction group. No other effects were significant (ps > .05).

Time spent hitting punching bag. No significant effects were found for time spent hitting the punching bag (ps > .05).

Enjoyed hitting the punching bag. Men enjoyed hitting the punching bag more than did women, M = 6.11, SD = 2.53, and M = 4.96, SD = 2.51, F(l, 396) = 20.85, p < .0001, d = 0.46. No other effects were significant (ps > .05).

TABLE 23.1
Anger and aggression levels for participants in the control, distraction, and rumination groups

Measure	Control	Distraction	Rumination
Positive mood	29.61_{a} (7.34)	29.71_{a} (7.86)	30.11_{a} (8.23)
Anger	26.25_{b} (10.98)	27.32_{b} (10.88)	29.78_{a} (11.56)
Aggression	-0.21_{b} (1.27)	0.01_{ab} (1.39)	0.21_{a} (1.54)

Note: n = 200 in each group. Standard deviations are in parentheses. Subscripts refer to within-row comparisons. Means having the same subscript are not significantly different at the .05 significance level.

Primary Analyses

Positive Mood. There was no significant effect for experimental condition on positive mood, $F(2, 594) = 0.24$, $p > .05$ (see Table 23.1). Regardless of the condition they were in, men were in a more positive mood than were women, $M = 31.51$, $SD = 7.85$, and $M = 28.12$, $SD = 7.40$, $F(1, 594) = 29.31$, $p < .0001$, $d = 0.44$.

Anger. There was a main effect for experimental condition on anger $F(2, 594) = 5.23$, $p > .01$ (see Table 23.1). Participants in the rumination group felt more angry than did participants in the distraction and control groups, $t(594) = 2.20$, $p < .05$, $d = 0.22$, and $t(594) = 3.15$, $p < .005$, $d = 0.31$. Participants in distraction and control groups did not differ in terms of how angry they felt, $t(594) = 0.95$, $p > .05$.

Aggressive Behavior. The same pattern of results was found for the two measures of aggression—noise intensity and noise duration. Thus, the two measures were standardized and summed to form a more reliable measure of aggression. The same pattern of results also was obtained on Trial 1 and on the remaining 24 trials of the competitive reaction-time task. Thus, the responses on the 25 trials were standardized and summed.

There was a main effect for experimental condition on aggression, $F(2, 594) = 5.03$, $p < .01$ (see Table 23.1). Participants in the rumination group were more aggressive than participants in the control group, $t(594) = 3.17$, $p < .005$, $d = 0.30$. Participants in the distraction group were more aggressive than participants in the control group and were less aggressive than participants in the rumination group, although neither difference was statistically significant, ts (594) = 1.68 and 1.49, ps > .05. Men were also more aggressive than were women, $M = 0.44$, $SD = 1.62$, and $M = -0.44$, $SD = 0.99$, $F(1, 594) = 66.52$, $p < .0001$, $d = 0.33$.

DISCUSSION

Does venting anger extinguish or feed the flame? The results from the present research show that venting to reduce anger is like using gasoline to put out a fire—it

only feeds the flame. By fueling aggressive thoughts and feelings, venting also increases aggressive responding. People who walloped the punching bag while thinking about the person who had provoked them were the most angry and the most aggressive in the present experiment. Venting did not lead to a more positive mood either.

People in the distraction group were less angry than were people in the rumination group, but they were not less aggressive. Thus, performing an aggressive activity such as hitting a punching bag can increase aggression even if people are distracted while performing the activity.

In the present experiment, people were better off doing nothing at all than venting their anger. No attempt was made to reduce anger or aggressive impulses in the control group. Even so, anger and aggression levels were lowest in the control group. The results might have been more dramatic if participants in the control group actively sought to reduce their angry feelings.

Overall, the present results support cognitive neoassociation theory (Berkowitz, 1993) and directly contradict catharsis theory. Venting while ruminating about the source of provocation kept aggressive thoughts and angry feelings active in memory and only made people more angry and more aggressive. These results provide one more nail in the coffin containing catharsis theory.

Magnitude of Observed Effects

Although the effects obtained in the present study were small to moderate in size (see Cohen, 1988), they are in the opposite direction predicted by catharsis theory. In the present study, the distraction activity was an aggressive one—angered people hit a punching bag. Larger effects might have been obtained if distraction activity would have been a nonaggressive one, such as working a crossword puzzle. Similarly, larger effects might have been obtained if angered people would have engaged in a behavior incompatible with anger and aggression, such as watching a funny TV program or petting a puppy (e.g., Baron, 1976, 1983).

Can These Findings Be Due to Arousal?

One well-known finding in psychology is that arousal enhances whatever response is dominant (e.g., Cottrell and Wack, 1967: Criddle, 1971; Eysenck, 1975; Markovsky and Berger, 1983; Zajonc, Heingartner, and Herman, 1969; Zajonc and Sales, 1966). This finding is central to the drive theory of social facilitation (e.g., Geen and Bushman, 1987, 1989). Walloping a punching bag for a few minutes can certainly increase arousal levels. Because participants in the present study all were provoked, it seems likely that aggression would be a dominant response for them. Arousal cannot, however, explain the pattern of results obtained in the present

study. If the results were due to arousal, people in the distraction group should have been more aggressive than people in the rumination group because they hit the punching bag a greater number of times. The results, however, were in the opposite direction.

Is Intense Physical Activity an Effective Technique for Managing Anger?

If used as a form of distraction, intense physical activity does not necessarily increase anger, even if the activity is aggressive in nature (e.g., hitting a punching bag). Physical activity should, however, increase anger if the person is provoked after engaging in the intense physical activity. According to excitation transfer theory, the arousal from the physical activity will be misattributed to the provocation and will therefore transfer to the provocation (e.g., Zillmann, 1979). Mislabeling the arousal from the physical activity as anger would therefore increase aggressive responding (e.g., Zillmann, Katcher, and Milavsky, 1972). In the present study, participants were provoked before engaging in an intense physical activity, so excitation transfer should not occur. Although it might be good for your heart, intense physical activity is probably not an effective technique for reducing anger and aggression.

CONCLUSION

Catharsis theory predicts that venting anger shuld get rid of it and should therefore reduce subsequent aggression. The present findings, as well as previous findings, directly contradict catharsis theory (e.g., Bushman et al., 1999; Geen and Quanty, 1977). For reducing anger and aggression, the worst possible advice to give people is to tell them to imagine their provocateur's face on a pillow or punching bag as they wallop it, yet this is precisely what many pop psychologists advise people to do. If followed, such advice will only make people angrier and more aggressive.

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24

Peacetime Casualties: The Effects of War on the Violent Behavior of Noncombatants

Dane Archer and Rosemary Gartner

Violence by the State is strangely absent from most discussions of the problem of violence. Books about aggression, for example, often treat topics ranging from hormones to homicidal criminals without mentioning capital punishment, the shooting of looters, the beating of protesters, or even that most impressive form of "official" violence: war. This last omission is particularly astonishing, since the unrivaled mortality of wars is both well documented (Singer and Small, 1972) and reasonably contemporary—including the killing of at least 46,000 young American men between 1963 and 1973, in addition to the much greater death toll of other nations involved in the Vietnam War.

Why, then, is official violence nearly invisible in discussions of murder and aggression? The most obvious explanation is that wars and other forms of official violence are unique in that they wear the mantle of governmental legitimacy. When aircraft bomb a village, when the United States CIA hires assassins to kill foreign leaders, when a policeman shoots a looter, when a prison firing squad kills a convicted murderer, and when National Guardsmen use lethal weapons to break up a protest, the killings that occur are the result of orders (see Marx, 1970). These orders originate in a hierarchical organization; they are issued by appointed or elected officials and are carried out collectively by uniformed deputies who perform the actual killing. In each case the resulting homicides are described as necessary to

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accomplish some official objective—to stop the spread of an alien ideology, to stem the destruction of private property, to deter potential murderers of the future, or to control opponents of governmental policy. Official violence may also be justified as retaliatory—a response to "illegal" violence or the alleged threat of illegal violence. Official killings, therefore, differ from illegal violence in that they result from governmental orders, are usually performed by several agents acting collectively, and are justified as instrumental to some higher purpose. These differentiating features are the basis of the legitimation of official violence.

This process of legitimation is clearly successful in the sense that large numbers of citizens appear to regard government violence as acceptable and unproblematic. Evidence of a public mandate for official violence is easily found. In a 1968 survey, for example, 57 percent of a national sample agreed with the statement "Any man who insults a policeman has no complaint if he gets roughed up in return" (Gamson and McEvoy, 1972, p. 336). The public mandate for official violence includes even the extreme act of homicide. In another 1968 survey, by the Gallup organization, respondents were asked to react to the fact that the mayor of a large city had ordered police to shoot looters on sight during race riots; 61 percent of the men interviewed said that this was the "best way" to deal with the problem of looting. In 1969 roughly half the American people (48 percent) also thought that shooting was the best way to handle student protests on campus (Kahn, 1972, p. 48). This tolerance of official violence may explain the persistent public perception of riots and rioters as violent—despite the fact that the number of people killed by authorities during civil disorders has consistently exceeded the number of people killed by rioters by approximately ten to one (Couch, 1968).

Public support for official violence is so pervasive that the definition of violence is itself affected. In a 1969 survey, for example, 30 percent of a national sample said that "police beating students" was *not* an act of violence, and an astonishing 57 percent said that "police shooting looters" was *not* an act of violence (Blumenthal et al., 1972, p. 73). The semantics of the label *violence,* therefore, clearly reflect the perceived legitimacy of the actor and not merely the nature of his or her act. The same 1969 survey asked respondents what violent events were of greatest concern to them. Even though the survey occurred during the Vietnam War, only 4 percent of those interviewed mentioned war.

One consequence of the legitimation of official violence, therefore, is the creation of a reservoir of public support. For at least large numbers of Americans, it is clear that even the extreme act of homicide is not regarded as violence—if the homicide carries the stamp of government authority. Widespread deference to government violence may explain the intensity of public reaction to the concept of war crimes, including the trial of Lieutenant William Calley during the Vietnam War (Kelman and Lawrence, 1972). In view of citizen support for official killing, it is perhaps not surprising that public discussions of violence are generally limited to "criminal" violence.

The invisibility of official violence in scientific discussions of aggressions, however, is somewhat more disturbing. Since social scientists are also citizens, it is

possible that they too have been captured by the process of legitimation and that they—like many other citizens—no longer think of governmental homicides as violence. For example, wars are generally discussed in political terms in scholarly circles, and political opponents of a war are the only ones who label wartime killings as "murders." Since social scientists are trained to try to regard behavior in a value-free manner, however, it is curious that there has not been more critical discussion of those acts of homicide that happen to have government auspices. Perhaps we are all, scientists included, socialized to accept the State's monopoly on legitimate violence—and perhaps this socialization influences our curiously selective use of the label *violence.*

This tendency to see official violence as legitimate seems limited, however, to the acts of our own government. As an extreme example, wartime practices in Nazi Germany had official government auspices and seem to have enjoyed legitimacy among most contemporary German citizens—including many scientists and intellectuals—but not among citizens of other nations. Even though socialization leads us to defer to governmental violence, therefore, a kind of moral ethnocentrism may restrict this deference to only the acts of our own government. Official acts of violence may enjoy perceived legitimacy inside a government's borders but not outside them.

There may also be structural explanations for the scientific neglect of official violence, however, and this possibility is particularly worrisome. Since social scientists operate in an institutional context, governments may be exerting a disproportionate influence on research agendas for the study of violence. The structure of scientific research itself may therefore create a lamentable "blind spot" concerning governmental violence. Unhappily, case studies of this scientific blind spot are easy to find. For example, when Governor Ronald Reagan of California announced his controversial proposal to establish a Center for the Study of Violent Behavior at UCLA, Earl Brian, the state's secretary of health and welfare, held a press briefing to describe the mission of the proposed center. When asked by a reporter whether the forms of violence studied by the violence center would include war, Dr. Brian replied, "I hadn't thought about war."

Another fascinating illustration of the way official violence can be defined to preclude scientific consideration is described in James Short's (1975) excellent account of the history of the National Commission on the Causes and Prevention of Violence. Since Short served as codirector of research (with Marvin E. Wolfgang) for this important presidential commission, his narrative is as well informed as it is interesting. The national commission initially adopted a neutral definition of violence, one that concentrated on the nature of violent acts themselves: "the threat or use of force that results, or is intended to result, in the injury or forcible restraint or intimidation of persons, or the destruction or forcible seizure of property" (Short, 1975, p. 68). Early in the life of the commission, this neutral definition of violence prompted commission researchers to cast a wide net. The commission's *Progress Report,* for example, outlined the scope of the investigation: "There is no implicit value judgment in this definition. The maintenance of law and order falls within it,

for a policeman may find it necessary in the course of duty to threaten or use force, even to injure or kill an individual. Wars are included within this definition, as is some punishment of children. It also includes police brutality, the violence of the Nazis, and the physical abuse of a child" (Commission *Progress Report,* p. 3; cited in Short, 1975, p. 68). Even though official violence, such as wars and police brutality, was clearly included in the commission's initial agenda, this emphasis all but disappeared in the commission's subsequent research. For example, by the time its *Final Report* was issued, the commission was concentrating on "all illegal violence" (Short, 1975, p. 69). The insertion of the adjective *illegal* is of pivotal importance, since the actions of governments are—by definition—seldom perceived as illegal. This change of emphasis in the work of a prestigious national investigation had the effect of shifting attention to the acts of "deviant" individual criminals, members of rioting mobs, and assassins. At the same time, this rather traditional focus on illegal violence excluded the acts of governments from consideration—even though the commission conducted its investigations at the very height of the Vietnam War. In fact, Short indicates that some commission researchers had suggested creating a task force on war to go along with the seven task forces on other topics. This idea was abandoned, according to Short, because of the "potentially explosive nature of such a direct focus on war in general and on the conflict in Indochina in particular" (p. 71). Even though much of the commission's research was excellent, and even of permanent value, it is still troublesome that the commission was apparently influenced to concentrate on those acts of violence of greatest concern to government—a concern that did not, of course, include the violent acts of government itself.

For all these reasons, discussions of the problem of violence—in both popular and scientific publications—have centered almost exclusively on illegal or "deviant" violence. This prejudice is clearly reflected in language. The term *murder* is almost always reserved for the acts of individual criminals, and even the more neutral word *homicide* is almost never used to describe killings committed by government officials. During wartime, for example, governments use terms like *casualties, body counts,* or even just *losses* to refer to actions that are really—despite these pastel euphemisms—violent homicides. When officials take lives in peacetime, words like *execution* again camouflage the fact that a killing takes place. Only the opponents of a government's violence refuse to participate in this banal vocabulary. Opponents of a war, for example, accuse government officials of "murderous" policies and refer to soldiers as "killers."

We believe that the scientific neglect of official violence needs to be remedied. It is clear that "deviant" violence has in the past monopolized the attention of researchers interested in violence. We believe that this monopoly is scientifically shortsighted, for two reasons. First of all, acts of official violence may turn out to be particularly pernicious, since they are the only forms of violence that carry the prestige and authority of the State. Second, many important and exciting questions about official violence await answers: What kinds of citizens support government

violence most strongly? What kinds of government justifications of violence (such as wars or executions) are most effective in legitimating such violence? Do young children regard government violence, such as wartime killing, as "wrong"? At what developmental age are children socialized to accept government violence? Are people at various stages of "moral development" differentially supportive or critical of official violence? Do violent "criminals" support government violence more strongly than a matched group of noncriminals? How do the deputies who carry out official violence justify their behavior to themselves? What kinds of arguments are most effective with juries in death penalty cases? Are Americans more supportive or tolerant of official violence than citizens of relatively nonviolent societies like England? This research agenda is clearly both rich and relatively unexplored.

CONSEQUENCES OF VIOLENCE BY THE STATE

For the past several years, we have been interested in the consequences of violence by the State. Specifically, we wondered whether the most impressive form of official violence, war, tends to increase the level of violence in a society after the war is over. There are rather compelling theoretical reasons to suspect that wars might produce a legacy of postwar violence. For example, there is now incontrovertible evidence that social learning or "modeling" mediates many forms of aggression and violence (Bandura, 1973). Although most research in this area has used either experiments or causal regression techniques to assess the effects of watching media violence, "modeling" theory also appears to provide the best explanation of the apparently contagious patterning of specific murder methods, airplane hijackings, and terrorism (Bandura, 1973, pp. 101–107). The basic tenet of social learning theory is that acts of media violence and real violence can provide a model or script that increases the likelihood of imitative violence. In addition, research indicates that aggressive models appear to be most influential when they are seen as rewarded for their aggression.

If the violent acts of real or fictional individuals can compel imitation, it seems to us very possible that official violence like war could also provide a script for the postwar acts of individuals. Wars, after all, carry the full authority and prestige of the State, and wars also reward killing in the sense that war "heroes" are decorated and lionized in direct proportion to the number of their wartime homicides. Wars also carry, of course, objectives and rationalizations unique to each war—securing the Crimea, humbling the Boers, stopping fascism, deterring communism, and so on. But what all wars have in common—when they are stripped of their idiosyncratic circumstances—is the unmistakable moral lesson that homicide is an acceptable, or even praiseworthy, means to certain ends. It seems likely that this moral lesson will not be lost on at least some of the citizens in a warring nation. Wars, therefore, contain in particularly potent form all the ingredients necessary to

produce imitative or modeled violence: great numbers of violent homicides, official auspices and legitimation, and conspicuous praise and rewards for killing and killers.

Even though there are theoretical reasons to expect wars to provide an imitated model for postwar killing, a rigorous test of this legitimation hypothesis is fraught with complications. At first glance, there are a number of intriguing observations in support of the hypothesis. For example, during the Vietnam War the United States murder and nonnegligent manslaughter rate more than doubled—from 4.5 (per 100,000 persons) in 1963 to 9.3 in 1973 (Archer and Gartner, 1976b). This single case of an astonishing increase cannot be regarded as persuasive evidence of a violent legacy of war, of course, because there were many other social and demographic changes in the United States during this decade. A definitive test of the legitimation hypothesis requires a large number of cases, so that the changes in homicide rates in many warring societies can be assessed. In addition, it is important to disentangle the two very different questions included in the legitimation hypothesis: (1) the empirical question of whether, in general, homicide rates do increase after wars and (2) the more interpretive question of what wartime variables could actually cause such increases.

Although writers and social scientists have long suspected that war leaves a violent legacy, a general test of this suspicion has not been possible in the past. The central obstacle to such a test has been the unavailability of the necessary records on national rates of homicide over time. Without a large archive of annual homicide rates for many nations, it has not been possible to test the legitimation hypothesis. Over a period of three years, we have collected a Comparative Crime Data File (CCDF) containing annual rates of homicide and other offenses for 110 nations and 44 major international cities for the period 1900–1970 (see Archer and Gartner, 1976b; see also Archer et al., 1978; Archer and Gartner, 1977).

The creation of the CCDF makes it possible for the first time to attempt a general answer to the question of whether violence by the State increases subsequent violence by individuals. Our basic research design involves a comparison of homicide rate changes in combatant societies with the changes in a control group of noncombatant nations. This controlled comparison is designed to guard against the possibility that homicide increases might be universal over a given period. Only if wars produce consistent increases in postwar homicide rates, therefore, will the changes in combatant nations differ from those in noncombatant nations during the same years. Since wartime mobilization renders the data uninterpretable for war years themselves, we chose as the two periods of comparison the five years prior to a war and the five years after (Archer and Gartner, 1976b). Records of national participation in wars were obtained from an encyclopedic inventory published by Singer and Small (1972). The rich historical data in the CCDF made it possible to examine homicide rate changes after 14 wars and, since several nations were involved in some wars, after a total of 50 “nation-wars” (one nation in one war). These changes, and the changes in 30 control cases uninvolved in these wars, are shown in Table 24.1.

Our analysis demonstrates that warring nations were more likely to experience homicide rate increases than nations not involved in war. A majority of the combatant nations experienced homicide rate increases of at least 10 percent, while a majority of the uninvolved nations experienced homicide rate *decreases* of greater than 10 percent. Many of the homicide rate increases in warring nations were very large; in several cases the nation's prewar homicide rate more than doubled. The legitimation hypothesis is, therefore, consistent with the results in Table 24.1. Wars do produce a postwar legacy of increased homicide rates.

Even though the difference between combatant and noncombatant nations is striking, this comparison is actually a very conservative test of the legitimation hypothesis. Along with many other changes, wars produce dramatic changes in the age and sex structure of a nation's population. The numbers of young men killed in twentieth-century wars have often been staggering. At the end of World War I, for example, out of every 1000 men who were between 20 and 45 years of age at the war's outbreak, 182 died in France, 166 in Austria, 155 in Germany, 101 in Italy, and 88 in Britain (von Hentig, 1947, p. 349). Since young men are universally overrepresented in homicide offense rates (Wolfgang and Ferracuti, 1967), these wartime losses remove from a nation's population precisely those who are statistically most likely to commit homicide in the postwar years. In addition, the postwar baby booms, particularly after large wars, also tend to reduce postwar homicide rates by inflating the population denominator on which they are calculated.

The implications of these conservative artifacts can be summarized easily: If one corrects for the wartime depletion of young men and the surge of postwar babies, warring nations experienced homicide increases even greater than those shown in Table 24.1. For example, France is shown as having a negligible homicide rate increase of 4 percent after World War I and is classified as an unchanged case in Table 24.1. If one controls for France's appalling wartime losses, however, the observed postwar homicide rate can only be considered as an impressive increase. The fact that combatant nations still showed increases much more frequently than the noncombatant nations in Table 24.1, despite the conservative effect of these demographic changes, is therefore particularly impressive.

The nation-wars in Table 24.1 varied considerably in length of wartime involvement and number of men killed. The warring nations in our study, therefore, received different "doses" of war. In terms of the legitimation hypothesis, nations with the greatest wartime mortality are exposed to a larger scale of legitimate killing than other nations. Using the information provided by Singer and Small (1972), we were able to classify nation-wars according to the amount of war experienced by each. The classification we used was whether a nation had fewer or more than 500 battle deaths per million prewar population. This control procedure produces even more dramatic results and again runs counter to what one would expect from population changes alone. The nations that were heavily influenced by war-time killing suffered postwar homicide increases with particular consistency; 79 percent of these nations experienced homicide rate increases, and only 21 percent had decreases. We interpret this result as an internal validation of the overall

TABLE 24.1
Homicide Rate Changes in Combatant and Control Nations After World War I, World War II, the Vietnam War, and 11 Other Wars

	Decrease	%	Unchanged (<10%)	%	Increase	%
Combatant nations	Australia (I)	−23	England (I)	−5	Belgium (I)	24
	Canada (I)	−25	France (I)	4	Bulgaria (I)	22
	Hungary (I)	−57	S. Africa (I)	−1	Germany (I)	98
	Finland (II)	−15	Canada (II)	6	Italy (I)	52
	N. Ireland (II)	−83	Australia (VN)	7	Japan (I)	12
	U.S. (II)	−12	Korea (VN)	6	Portugal (I)	47
	India (1962 Sino-Ind)	−14	Philippines (VN)	9	Scotland (I)	50
	Israel (1956 Sinai)	−58	Egypt (1956 Sinai)	−2	U.S. (I)	13
	Italy (1896 It-Eth)	−15	Egypt (1967 6-Day)	−4	Australia (II)	32
	Italy (1935 It-Eth)	−44	France (1884 Sino-Fr)	0	Denmark (II)	169
			India (1965 2nd Kash)	6	England (II)	13
			Japan (1904 Russo-Jap)	−9	France (II)	51
			Japan (1932 Manch)	−8	Italy (II)	133
					Japan (II)	20
					Netherlands (II)	13
					New Zealand (II)	313
					Norway (II)	65
					Scotland (II)	11
					S. Africa (II)	104
					New Zealand (VN)	50
					Thailand (VN)	14
					U.S. (VN)	42
					Hungary (1956 Russo-H)	13
					Israel (1967 6-Day)	14
					Japan (1894 Sino-Jap)	15
					Jordan (1967 6-Day)	35
					Pakistan (1965 2nd Kash)	13

Control nations	Norway (I)	−37	Ceylon (I)	8	Finland (I)	124[a]
	Ceylon (II)	−19	Chile (I)	−3	Thailand (I)	112[a]
	Chile (II)	−67	Netherlands (I)	−2	Colombia (II)	34
	El Salvador (II)	−20	Thailand (1932 Manch)	7	Sweden (II)	14
	Ireland (II)	−22	Ceylon (1962 Sino-Ind)	−4	Turkey (II)	l2
	Switzerland (II)	−42			Canada (VN)	11
	Thailand (II)	−17			England (VN)	23
	Burma (VN)	−17			Taiwan (VN)	37
	Indonesia (VN)	−23			Ceylon (1965 2nd Kash)	11
	Japan (VN)	−23				
	Austria (1956 Russo-H)	−13				
	Burma (1965 2nd Kash)	−13				
	France (1896 Italo-Eth)	−13				
	Switzerland (1935 It-Eth)	−22				
	Turkey (1956 Sinai)	−33				
	Turkey (1967 6-Day)	−19				

[a]Finland and Thailand are both included as control cases for World War I because these two nations were not identified as combatants by Singer and Small (1972). However, the wisdom of classifying these nations as noncombatants during this period is far from clear. Finland underwent something of an "internal" or civil war in 1918, and Thailand actually sent troops to the Allied cause during World War I.

Source: Archer and Gartner (1976b).

finding in Table 24.1, since the nations most likely to show homicide increases are precisely those nations that experienced the largest "amounts" of war.

The answer to the first of the two questions prompted by the legitimation hypothesis is therefore unambiguous. Warring nations are more likely than nonwarring nations to experience postwar surges in individual acts of homicide. The increases occur despite massive wartime losses of young men and are particularly common among nations whose wartime involvement is unusually deadly. It is still a separate matter, of course, to test the second half of the legitimation hypothesis: What is it about wars that actually produces these increases in postwar violence? For example, there has been a long history of speculation about whether veterans of wars are more likely than other citizens to commit acts of violence (Archer and Gartner, 1976a). This fear appears to surface after each war. For example, attorney Clarence Darrow (1922, p. 218) attributed post-World War I crime increases to the returned veterans, whom he described as "inoculated with the universal madness," and Lifton (1973, p. 32) made this more recent prediction about Vietnam veterans: "Some are likely to seek continuing outlets to a pattern of violence to which they have become habituated, whether by indulging in antisocial or criminal behavior or by offering their services to the highest bidder." The basic idea behind this speculation is that the military socializes men to be both more accepting of and more proficient at killing and that this experience may increase the likelihood that veterans will use violence even after the war is over.

A number of spectacular case studies appear to lend substance to the image of the violent veteran. For example, some soldiers who rode with Quantrill's guerrillas during the American Civil War became well known for their postwar lawlessness; they were Jesse and Frank James and the Younger brothers. There are also many other examples. In 1949 a combat veteran named Howard Unruh went on a rampage in Camden, New Jersey, and killed 12 people with a souvenir pistol—Unruh had won marksman and sharpshooter ratings during World War II. During the Vietnam War, a soldier named Dwight Johnson killed about 20 enemy soldiers and was awarded the Congressional Medal of Honor; several months after his return home to Detroit, he was shot and killed while trying to rob a grocery store. Case studies prove very little by themselves, of course, and the more general question is whether the acts of violent veterans can explain the homocide increases we have observed in postwar societies. Since most nations, including the United States, do not maintain records on the military experience of persons arrested for homicide, it is difficult to determine whether veterans are overrepresented in homicide statistics. Even if the number of homicides committed by veterans were known, it is not clear what rate we would compare this with, since veterans and nonveterans differ in many ways in addition to military experience. An indirect approach to this question involves comparisons between two types of veterans, combat and noncombat; and recent research has not found unusually high rates of offenses or violence among combat veterans (Borus, 1975). In the absence of firm evidence, the image of the violent veteran may be more myth than reality. The persistence of the myth is probably due, in part, to the nature of the training that sol-

diers receive. Civilians may have deep-seated misgivings about teaching soldiers to kill, and they may fear that it is easier to unleash violence than to rein it in again.

We have found evidence that the hypothesis of the violent veteran, despite its popularity, cannot explain the postwar homicide increases observed in warring nations. The key answer to this question is our finding that postwar increases in violence have occurred among groups who could not have been combat veterans. During the Vietnam War, for example, violence increased precipitously for men and women in the United States; between 1963 and 1973 homicide arrests increased 101 percent for men and 59 percent for women. We have also obtained data from other nations on postwar offense rates for men and women, and it is clear that postwar rates of violence increase for both sexes. As a final bit of evidence, postwar homicide rates also increase among all age groups, and not just among the young veteran age cohort.

In summary, postwar increases in homicide rates are both common and pervasive. They occur after large and small wars, in victorious as well as defeated nations, in nations with improved postwar economies and nations with worsened economies, among veterans and nonveterans, among men and women, and among offenders in several age groups. We believe that these homicide rate increases cannot be explained by artifacts or by other social changes that happen to coincide with wars. Instead, we think that this finding reveals a potential linkage between the violence of governments and the violence of individuals. This linkage is mediated, we believe, by a process of legitimation in which wartime homicide becomes a high-status, rewarded model for subsequent homicides by individuals. Wars provide concrete evidence that homicide, under some conditions, is acceptable in the eyes of a nation's leaders. This wartime reversal of the customary peacetime prohibition against killing may somehow influence the threshold for using homicidal force as a means of settling conflict in everyday life. There is even some independent evidence of the mechanisms by which governmental violence can permeate the private lives of citizens in warring societies; for example, Huggins and Straus (1975) have shown that fictional violence in children's literature reaches a dramatic maximum in war years.

Even though social scientists have in the past amassed impressive experimental evidence that violence can be produced through imitation or modeling, they have in general neglected the possibility that government—with its vast authority and resources—might turn out to be the most potent model of all. This powerful influence of governments on private behavior seems to be what Justice Louis Brandeis had in mind when he wrote in 1928: "Our government is the potent, the omnipresent teacher. For good or ill, it teaches the whole people by its example. Crime is contagious. If the government becomes a lawbreaker, it breeds contempt for the law."

The astonishing neglect of official violence in the social sciences has almost certainly resulted from the curious tendency of both citizens and scientists to avoid labeling the acts of governments—including even the extreme act of homicide—as violence. This deference to the legitimacy of governments has resulted in the near

omission of wars and other forms of official homicide from discussions of violence. The new finding that wars cause surges in postwar homicide rates suggests that this omission is lamentable and that the violent acts of individuals may in part be catalyzed by the violent times in which governments cause them to live.

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25

Deindividuation and Anger-Mediated Interracial Aggression: Unmasking Regressive Racism

Ronald W. Rogers and Steven Prentice-Dunn

A factorial experiment investigated the effects of deindividuation, anger, and race-of-victim on aggression displayed by groups of whites. Deindividuating situational cues produced an internal state of deindividuation that mediated aggressive behavior. Deindividuation theories were extended by the finding that the internal state of deindividuation was composed not only of the factors Self-Awareness and Altered Experience, but also Group Cohesiveness, Responsibility, and Time Distortion. As predicted, nonangered whites were *less* aggressive toward black than white victims, but angered whites were *more* aggressive toward blacks than whites. Interracial behavior was consistent with new, egalitarian norms if anger was not aroused, but regressed to the old, historical pattern of racial discrimination if anger was aroused. This pattern of interracial behavior was interpreted in terms of a new form of racism: regressive racism.

Mob violence that has occurred since the time of the Roman republic has been attributed typically to short-term economic motives and political issues (cf. Rude, 1964). Economic and political motives, however, were inadequate to explain the torture, mutilation, and burning that frequently occurred in outbursts of interracial violence. Lynch mobs convinced social scientists that "the fundamental need was for a better understanding of the causes underlying the resort to mob violence" (Southern Commission on the Study of Lynching, 1931, p. 5). The major purpose of the present experiment was to examine interracial aggression within a group context, especially a context conducive to deindividuation.

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Deindividuation is a process in which antecedent social conditions lessen self-awareness and reduce concern with evaluation by others, thereby weakening restraints against the expression of undesirable behaviors (e.g., Diener, 1977; Zimbardo, 1970). Prentice-Dunn and Rogers (1980) provided the first confirmation of deindividuation theory's major assumption that deindividuating situational cues produce an internal state of deindividuation that mediates the display of aggressive behavior. The deindividuating cues lowered self-awareness and altered cognitive and affective experiences. This deindividuated state weakened restraints against behaving aggressively that are normally maintained by internal and external norms of social propriety. In the present study, therefore, we hypothesized that deindividuating situational cues would produce more aggression than individuating cues, and that an internal state of deindividuation would mediate the effects of deindividuating cues on antisocial behavior.

Many problematic forms of interracial conflict occur in group contexts. The major contribution to our understanding of interracial aggression has come from the Donnersteins' research program (cf. Donnerstein and Donnerstein, 1976), which has focused on situations involving one aggressor and one victim; no published studies have examined interracial aggression displayed by a *group* of whites toward a black individual. The present experiment examined interracial aggression in a group setting in which angry aggressors were deindividuated. This social situation approximates many naturalistic situations.

Studies of interracial aggression have consistently shown that the strength of aggression directed toward a different-race victim varies as a function of, for example, potential censure (Donnerstein and Donnerstein, 1973), threatened retaliation (Donnerstein, Donnerstein, Simon, and Ditrichs, 1972), and the victim's expression of suffering (Baron, 1979; Griffin and Rogers, 1977). Donnerstein and Donnerstein (1976) have reported that, in a variety of conditions, white subjects manifest less direct aggression toward black than white victims. Griffin and Rogers (1977) interpreted their white subjects' more lenient treatment of blacks than whites in terms of "reverse discrimination" (cf. Dutton, 1976): To avoid appearing prejudiced, whites treated blacks more favorably (i.e., less aggressively) than they treated whites.

Reverse discrimination is the overt manifestation of white people's viewing themselves as egalitarian and feeling threatened by the prospect of appearing prejudiced. Blacks would not be expected to display reverse discrimination, and studies of blacks' aggression have confirmed they do not (Wilson and Rogers, 1975). Both blacks' and whites' behavior, however, can be traced to the same underlying source: Both races seem to be "reacting against the older, traditional patterns for their races" (Griffin and Rogers, 1977, p. 157).

For whites, the historical pattern of appropriate behavior toward blacks was racial discrimination and inferior treatment. Although whites may have negative attitudes on several specific issues such as blacks' economic gains (Ross, Vanneman, and Pettigrew, 1976) and race riots (Davis and Fine, 1975), survey data indicate that the new norm is an egalitarian view of the races (Brigham and Wrightsman, 1982; Campbell, 1971; Taylor, Sheatsley, and Greeley, 1978). This new norm is especially prevalent

among college students. Surveys at the university where the present study was conducted confirmed that the current norm among white students is an unprejudiced, egalitarian view of the races (Rosenberg).[1] Theoretically, reverse discrimination is a product of this relatively new egalitarian view of blacks (Dutton, 1976).

For blacks, the historical pattern of appropriate interracial behavior was to inhibit aggression toward whites and to displace it to fellow blacks. The new norms favor more militancy, antiwhite attitudes, and overt hostility toward whites (Caplan, 1970; Wilson and Rogers, 1975). The new norms for blacks and whites represent dramatic departures from the deep-rooted values of the past. Both races have been found to act on these new norms if they are not emotionally aroused by a verbal insult. Thus, blacks are more aggressive toward white than black targets (Wilson and Rogers, 1975), and whites are more aggressive toward white than black targets (Griffin and Rogers, 1977).

But what happens to behavior based on these new norms if the aggressors are insulted? Baron (1979) reported a three-way interaction effect among race-of-victim, insult, and pain cues. An examination of the conditions comparable with those to be studied in the present experiment (i.e., Baron's no-pain-cues condition) indicated that when white subjects were not insulted, black victims received less aggression than white victims (i.e., reverse discrimination); if insulted, the level of aggression expressed toward blacks increased, but did not significantly differ from the level directed toward the white victims.

Since we wish to understand interracial aggression in general and not merely whites' behavior toward blacks, let us also examine blacks' aggression toward whites. To interpret the interracial aggressive behavior of blacks, Wilson and Rogers (1975) suggested that emotional arousal produced a regression to a chronologically earlier mode of responding. The data from that experiment were interpreted as evidence that blacks' behavior could be understood as a product of "the conflict between new militant norms and the residue of oppression" (p. 857). Anger-mediated aggression should not be as firmly under the cognitive control of new norms of in-group solidarity and pride. The black students had been exposed to the traditional values of in-group rejection and out-group preference for many years before the appearance of the Black Power movement. They undoubtedly retained some residual symptoms. Thus, when they became emotionally aroused, the new values, which had not been fully internalized, gave way to the older, more traditional pattern. Similarly, the young white adults in the present study had been exposed during their socialization to the older tradition of belief in black inferiority.

The foregoing considerations converge to suggest an interaction between race-of-victim and insult variables. If whites are not angered, we predicted that they would display reverse discrimination, directing weaker attacks against blacks than against whites. If angered, we hypothesized that whites would regress to the traditional pattern, displaying more aggression toward blacks than toward whites.

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[1]Rosenberg, J. *Racial attitudes of undergraduate students.* Unpublished manuscript, University of Alabama, 1980.

One class of deindividuation theories suggests that victim characteristics (e.g., different race) become less salient under conditions of deindividuation. For Festinger, Pepitone, and Newcomb (1952), the defining characteristic of deindividuation is that individuals are not paid attention to as individuals. As elaborated by Zimbardo (1970), deindividuated behavior is not under the controlling influence of usual discriminative stimuli; it is "unresponsive to features of the situation, the target, the victim" (p. 259). Based upon this theoretical position, any differential treatment of different-race victims should vanish when group members become deindividuated. On the other hand, Diener's (1980) theory of less extreme forms of deindividuation postulates that crowd members are *more* responsive to external stimuli as a result of the focus of attention shifting away from the self. It is plausible to infer that any differential treatment of different-race victims should be enhanced by deindividuation. Therefore, the present study was designed to test these two alternative predictions.

Verbal attack, or insult, is a potent and well-established antecedent of aggression in dyadic situations involving one aggressor and one victim (see review by Baron, 1977); however, the effects of anger in a group context have not been investigated. Virtually all studies of deindividuation and aggression have involved unprovoked aggression. Yet, anger adds an important theoretical and applied dimension to our understanding of mob violence. It was hypothesized that insult, or anger arousal, would facilitate the expression of aggression among members of small groups.

One class of deindividuation theories postulates that deindividuated behavior is not influenced by usual discriminative stimuli. It may be derived from these theories that prior insult would have less impact on deindividuated than individuated group members. On the other hand, it may be derived from Diener's (1980) theory that "because self-regulation is minimized or eliminated the deindividuated person is more susceptible to the influences of immediate stimuli, emotions [e.g., anger], and motivations" (p. 211).

There are several limitations to deriving predictions of interaction effects from these two classes of deindividuation theories. First, neither theory explicitly states how the variables of insult or different-race victim would interact with deindividuation. Thus, other interpretations are possible. Second, the form of the interaction effect may vary as a function of the strength of the deindividuation state. The present study is certainly not an *experimentum crucis,* but perhaps it can shed light on the interaction of deindividuation and anger-mediated interracial aggression.

METHOD

Design and Subjects

A $2 \times 2 \times 2$ factorial design was employed with three between-subjects manipulations: (a) deindividuating cues versus individuating cues, (b) white versus black

victim, and (c) no insult versus insult. Ninety-six male introductory psychology students participated in the experiment to earn extra credit. Twelve subjects were randomly assigned to each cell.

Apparatus

The shock apparatuses were modified Buss aggression machines connected to a polygraph. Each of the four aggression machines had 10 pushbutton switches that could be depressed to deliver "shocks" of progressively increasing intensity. Of course, shocks were not actually delivered. A Grason-Stadler noise generator (Model 901A) was used to produce white noise in the deindividuating cues condition.

Procedure

The procedure was highly similar to one we had used previously (Prentice-Dunn and Rogers, 1980). Subjects arrived in groups of five; four were naive participants and one was our assistant. The study was explained as a combination of two experiments. The subjects had signed up for an experiment entitled "Behavior Modification" and were to be tested together. Our assistant, ostensibly another introductory psychology student, had volunteered for a study labeled "Biofeedback." After the experimenter determined who had volunteered for each topic, the biofeedback subject was sent to another room to receive detailed instructions for the biofeedback study.

After hearing explanations of the concepts of behavior modification and biofeedback, the four white behavior modifiers were told that the response of interest in both studies was heart rate. It was indicated that the biofeedback subject would be attempting to maintain his heart rate at a predesignated, high level. Whenever his heart rate fell below the predetermined level, the behavior modifiers would administer an electric shock. The purpose of having groups of four behavior modifiers was explained as an attempt to establish a laboratory analog of a ward at the local state hospital where behavior modifiers actually worked in small groups.

We explained to all subjects that they received their extra credit points for simply showing up and that they could discontinue at any time. Each subject was asked if he had any questions about or any objections to the use of electric shock. All questions were answered and no one declined to participate. In addition, written informed consent was obtained. Two mild sample shocks were administered to the behavior modifiers (i.e., the subjects) via finger electrode. The shocks were from Switches 4 (.3 mA) and 6 (.45 mA) on the aggression machine and each lasted for 1 sec. These samples were administered to convince the subjects that the apparatus really worked and to give them some idea of the shocks they would be delivering.

The behavior modification subjects were then taken to an adjoining room, seated at aggression machines with partitions that blocked observation of others' responses (thus, responses were experimentally independent), and given instructions for operating the shock apparatus. Each time the biofeedback subject's heart rate fell below the predetermined level, a signal light would be illuminated on their panels. It was explained that the higher the level chosen and the longer the switch was depressed, the stronger the shock administered would be. The "shock" received by the biofeedback subject was alleged to be the average of the intensities and durations selected by the four behavior modifiers.

The final instruction given to the subjects was that any of the 10 shock switches would be sufficient for the purposes of the experiment. It was explained that the equipment had been designed with different shock intensities because we had not known how strong the shocks would have to be to increase heart rate. We explained we had discovered that the different shocks all had equal effects on the biofeedback subject's heart rate, so the naive subjects could choose any intensity they wished on each trial. These instructions were designed to eliminate any potential altruistic motivation, and they made clear that use of the lowest possible intensity on every trial would fulfill the requirements of the experiment. Use of any intensity greater than "1" would only result in additional pain to the biofeedback subject.

Each group was presented with 20 signal lights over the course of the experiment. The interval between the appearance of any two signals was initially chosen randomly, ranging from 20 to 50 seconds. The intervals were then held constant across subsequent trials.

The experimenter then left to bring the biofeedback subject from a waiting room to the experimental room. The doors were left open, so that subjects heard the final instructions given by the experimenter to the biofeedback subject about his role. Thus, the naive subjects would easily hear, but not see, their future victim.

Experimental Manipulations

The first manipulation attempted to differentiate maximally between deindividuating situational cues and individuating ones. In the *deindividuating* cues condition, the experimenter did not address subjects by name. They were informed that the shocks they used were of no interest to the experimenter and that he would not know which intensities and durations they selected (anonymity to the experimenter). Subjects were further informed that they would not meet or see the biofeedback subject (anonymity to the victim). The experimenter indicated that he assumed full responsibility for the biofeedback subject's well-being (no responsibility for harm-doing). Finally, white noise was played at 65 dB (SPL) in the dimly lit room under the guise of eliminating any extraneous noise from the hall or other experimental rooms (arousal). Prentice-Dunn and Rogers (1980) have shown that

such manipulations decrease the subjects' feelings of identifiability and self-awareness.

In the *individuating* cues condition, the subjects wore name tags and were addressed on a first-name basis. As in Zimbardo's (1970) study, the "unique reactions" of each subject were emphasized, and the experimenter expressed his interest in the shock intensities and durations used by the subjects. Subjects were informed that they would meet the biofeedback subject on completing the study. It was emphasized that the biofeedback subject's well-being was the responsibility of each individual behavior modifier. The room was well-lit and no white noise was broadcast.

A second independent variable, race of victim, was manipulated through the use of four experimental assistants, two whites and two blacks. Assistants were assigned to the treatment cells randomly, with the exception that they appeared an equal number of times in each treatment combination. Analyzing this "assistants" factor as an additional variable in the factorial design yielded no main or interaction effects. Thus, the data from the assistants of each race were pooled in the analyses reported below.

The third independent variable was introduced when the behavior modifiers overheard a conversation in an adjoining room between the experimenter and the biofeedback subject. This conversation took place immediately after the naive subjects received their instructions. This insult manipulation was operationalized as a series of questions and answers between the experimenter and the biofeedback subject (i.e., our assistant). Care was taken that the insulting remarks applied to all of the subjects and were devoid of any racial content or connotation. In the *insult* condition, the biofeedback subject, when asked if he objected to the behavior modifiers shocking him, responded that the equipment looked complicated and he wondered if people who appeared as dumb as the behavior modifiers did could follow instructions properly. When the experimenter reiterated the biofeedback subject's option to withdraw from the experiment, the biofeedback subject answered that he hoped the behavior modifiers were not as stupid as they appeared. Finally, when asked by the experimenter if he knew the behavior modifiers, the biofeedback subject said that he didn't know them personally, but that he knew their type; he could tell that they thought they were "hot stuff." In the *no insult* condition, the biofeedback subject simply stated that he had no objections to these particular behavior modifiers shocking him.

Postexperiment Session

Following the last shock trial, subjects completed a questionnaire containing manipulations-check items (10-point Likert rating scales) and 17 items tapping an internal state of deindividuation. A second questionnaire assessed suspicions about the experiment. Five subjects suspected that shocks were not actually delivered, two

subjects correctly guessed the race-of-victim hypothesis, and four subjects believed that the insult manipulation was staged. These subjects were deleted from the data analyses. The data in this unequal *n* design were analyzed with the complete least squares model recommended by Overall, Spiegel, and Cohen (1975) because this model meets the criterion of estimating the same parameters as those estimated in an orthogonal design. After each experimental session, each subject was thanked and given a full debriefing that was based on Mills's (1976) recommendations. Finally, a questionnaire was given to each student in a stamped envelope addressed to the Psychology Department's Committee on Ethics. These anonymous responses were returned by 58% of the subjects. Ninety-eight percent of the respondents understood why the deception had been necessary and did not resent it. One subject (2% of the sample) indicated that he did not think the deception was necessary, and another subject stated that his participation had not been voluntary. Neither of these two subjects explained their responses in the space provided on the questionnaire. Fortunately, both of these students, along with every other respondent (100% of the sample), indicated that (a) the experiment should be allowed to continue and (b) that they would be willing to participate in another similar experiment.

RESULTS

Aggression

Deindividuating cues. A multivariate analysis of variance was performed on the sums of the shock intensity scores and the shock duration scores. This analysis yielded a main effect associated with the situational cues manipulation, Wilks's lambda (Λ) = .843, $F(2, 152) = 6.79$, $p < .001$. Univariate analyses of variance disclosed main effects for the situational cues variable on both shock intensity, $F(1, 77) = 12.01$, $p < .001$, and on shock duration, $F(1, 77) = 5.37$, $p < .03$. Compared with subjects in the individuating cues condition, subjects exposed to the deindividuating cues used higher shock intensities (*Ms* = 5.3 and 6.4, respectively) for longer durations (*Ms* = 1.6 and 2.8 sec, respectively). Neither of the two predicted two-way interaction effects with deindividuating cues were significant (*Fs* < 1).

Insult and race. To determine if the insult manipulation had been successful, an analysis of variance was performed on the sum of the two items assessing anger toward the victim. The only significant effect was that the insulted groups ($M = 5.1$) expressed much more anger than the noninsulted groups ($M = 1.9$), $F(1, 77) = 29.71$, $p < .0001$. A multivariate analysis of variance indicated that the insulted groups expressed more aggression toward the victim than the noninsulted groups, $\Lambda = .904$, $F(2, 152) = 3.97$, $p < .02$. More important, the multivariate analysis of variance revealed a Race $\times$ Insult interaction effect, $\Lambda = .912$, $F(2, 152) = 3.59$, $p < .03$. The aggression centroids are shown in Figure 25.1. Univariate analyses of

variance yielded identical interaction effects for both the intensity data, F (1.77) = 4.63, $p < .04$, and the duration data, $F(1, 77) = 4.57$, $p < .04$. A Duncan's multiple-range test ($p < .05$) yielded an identical pattern of results for both measures. As may be seen in Figure 25.1, when white subjects were not insulted, they expressed less aggression toward black than white victims; however, if the white subjects were insulted, they expressed more aggression against black than white victims. There were no other significant main or interaction effects on the aggression data.

Internal State of Deindividuation

Prior research (Diener, 1979; Prentice-Dunn and Rogers, 1980) had established that a subjective state of deindividuation is composed of at least two factors, Self-Awareness and Altered Experience. The 17 items in the Prentice-Dunn and Rogers (1980) study that loaded greater than .4 on only one of these factors were included in the post-experiment questionnaire. Although the present study differed from the former one in that half the subjects had been emotionally aroused by an insult and in that half aggressed against a different-race victim, an initial principal-axes factor analysis with varimax rotation yielded two factors highly similar to those previously reported. However, five eigenvalues were greater than unity, hence five factors were rotated to a varimax solution. This solution accounted for an additional 26% of the variance. The factor loadings that are greater than or equal to .4 on only one of the factors are shown in

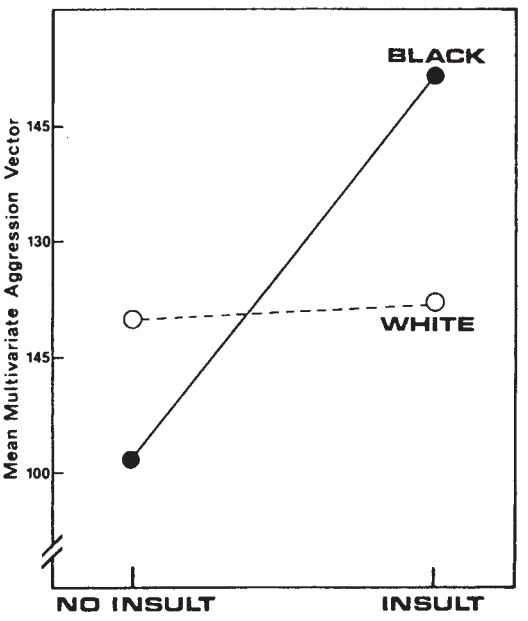

FIGURE 25.1
Aggression as a function of insult and race of victim.

Table 25.1. This factor structure, although not predicted, is readily interpretable. The first factor, which is composed of four items from the original Self-Awareness factor, is highly similar to Fenigstein, Scheier, and Buss's (1975) and Buss's (1980) concept of Public Self-Consciousness coefficient (alpha = .72). The items loading on this factor are highly similar to Fenigstein et al.'s items (e.g., "I'm concerned about what other people think of me.") The original Altered Experience factor decomposed into the second, third, and fourth factors. The second factor might still be labeled Altered Experience (alpha = .62). The third factor represents another facet of altered experiencing, but refers specifically to Time Distortion (alpha = .35). The fourth factor is composed of items traditionally used to measure cohesiveness, thus, it is labeled Group Cohesiveness (alpha = .61). The final factor might be labeled Responsibility (alpha = .69); it reflected the degree to which all group members, including the subject himself, had responsibility for harm-doing. The remaining two items loaded on more than one factor. The question assessing a feeling of togetherness among group members loaded on both the Group Cohesion and Altered Experience factors. Finally, the item measuring inhibition loaded on both the Public Self-Awareness and Altered Experience factors.

TABLE 25.1
Factor loadings on the rotated factors

Factor	Loading
Factor I (Public Self-Awareness)	
Concerned with what experimenter thought of me	.78
Concerned with what victim thought of me	.78
Concerned with what other group members thought of me	.71
Felt self-conscious	.50
Factor 2 (Altered Experience)	
Emotions were different from normal	.69
Thinking was somewhat altered	.65
Felt aroused	.59
Factor 3 (Group Cohesiveness)	
Liked other group members	.70
Session was enjoyable	.65
Willing to volunteer for another study with same group	.77
Factor 4 (Time Distortion)	
Thoughts were concentrated on the moment	.69
Time seemed to go quickly	.67
Factor 5 (Responsibility)	
I had responsibility for harm-doing	.83
Responsibility was shared	.84

Note. These five factors accounted for 13%, 14%, 13%, 9%, and 10% of the variance, respectively.

To replicate our previous study, we asked whether the original two factors could discriminate between the individuating and deindividuating cues conditions. The sums of each subject's raw scores on the Self-Awareness items and the Altered Experience items were submitted to a multivariate analysis of variance. A significant difference was obtained between the two situational cues conditions, $\Lambda = .882$, $F(2, 164) = 5.34$, $p < .006$. Subjects in the deindividuating cues condition had higher scores on the Altered Experience factor ($M = 65.8$) than subjects in the individuating cues condition ($M = 62.8$), but lower scores on the Self-Awareness factor ($Ms = 33.1$ & 38.5, respectively). A multivariate analysis of variance comparing the two situational cues conditions on the five factors also yielded a significant main effect, $\Lambda = .801$, $F(5, 79) = 3.91$, $p < .004$.

Deindividuation and Aggression

The correlation matrix among the original two factors of the internal state of deindividuation and shock intensity was decomposed into components corresponding to paths. This analysis attempted an exact replication of previous findings that there was a causal path from Altered Experience and Self-Awareness to shock intensity. The coefficients were .29 and −.20, respectively, confirming the previous structural model.

There was no strong a priori theoretical model that postulated relationships among our three independent variables, the five components of an internal state of deindividuation, and the dependent measures of shock intensity and duration. Therefore, no path analysis was performed. However, a multiple regression analysis was performed using the five factors to predict aggression (sum of shock intensity and duration). All of the relationships were in the expected direction, but the only statistically significant predictor of aggression was Group Cohesiveness (standardized partial regression coefficient = .25, $p < .05$).

DISCUSSION

Deindividuation

The results of the present experiment replicated and extended previous findings (Prentice-Dunn and Rogers, 1980) that deindividuating situational cues produce an internal state of deindividuation that mediates aggression. We had suggested previously that different configurations of stimulus conditions might produce different factor structures of the deindividuation state. Although the present data are only suggestive, the Self-Awareness and Altered Experience components may be differentiated into more precise aspects of deindividuation. First, the original Self-Awareness factor may reduce to Public Self-Awareness, which focuses on reactions of others to oneself (Buss, 1980; Fenigstein et al., 1975). The self-awareness

literature has demonstrated that reductions in self-awareness are associated with increased feelings of deindividuation (Ickes, Layden, and Barnes, 1978) and with increased aggression (Scheier, Feningstein, and Buss, 1974). Diener, Lusk, DeFour, and Flax (1980) pointed out that self-awareness is not a unitary phenomenon, but may have several facets.

Our original Altered Experience factor may be differentiated into three independent factors. One factor contained only items assessing altered thinking and altered feelings, accompanied by increased arousal. Although we shall retain the label Altered Experience for this factor, it is interesting to note its similarity to Fenigstein et al.'s (1975) and Buss's (1980) concept of private self-awareness. Private self-awareness refers to the *process* of being conscious of one's thoughts and feelings, whereas the Altered Experience factor refers to a *product* of that awareness. Another interesting facet of the original Altered Experience factor may be found in the Time Distortion factor, which reflected a concentration on the "here and now" that seemed to pass more rapidly than it actually did. Perhaps the most readily interpretable new factor to emerge was Group Cohesiveness, which was the best predictor of aggressive behavior. Finally, the data suggested that feelings of responsibility may be another component of the subjective state of deindividuation.

It is important to emphasize that these data only suggest the existence of these additional factors for several reasons: (a) the ratio of subjects to items was only 5:1; (b) there were a small number of items per factor (2–4); and (c) partially because of the small number of items, the mean reliability coefficient was a modest .60. Future research that overcomes these problems is obviously required before we can confidently accept these additional components of a deindividuated state. Taken together, the available data indicate that the internal state of deindividuation is composed of at least two factors, Self-Awareness and Altered Experience, which may be differentiated into more refined subcomponents that reliably distinguish individuating from deindividuating situations.

Neither of the two alternative forms of a Deindividuation $\times$ Race or Deindividuation $\times$ Insult interaction effect were obtained. It was surprising that deindividuation did not interact with the race variable. Donnerstein and Donnerstein (1976) found that race interacted with anonymity, potential retaliation, and censure, all of which were manipulated in the individuation–deindividuation independent variable. These interactions were produced by whites' fear of high levels of retaliation from blacks. In the present study, however, whites reported the same level of expected counteraggression from blacks (M = 6.3 on a 10-point scale) and whites (M = 6.4).[2] Thus, the state of deindividuation neither made group members hyperresponsive to immediate stimuli nor oblivious to them. A similar finding had been obtained for high- versus low-aggressive models (Prentice-Dunn and Rogers, 1980). The models had the same effect in the individuating and deindividuating conditions. One of the next steps in constructing a theory of deindividuation will

[2]The authors wish to thank Ed Donnerstein for suggesting this interpretation.

be to reexamine and refine general propositions that a state of deindividuation will render a person more (or less) responsive to environmental cues. An important conceptual and empirical task is to identify the specific classes of environmental stimuli to which a state of deindividuation would make a group member more or less responsive.

Interracial Aggression

The Race × Insult interaction effect (see Figure 25.1) indicated that nonangered white subjects aggressed more against same-race victims than against different-race victims; in contrast, angered white subjects aggressed more against different-race victims than same-race victims. The interaction effect was stronger in the present study than in Baron's (1979). A key difference between the two experiments was whether the aggressors were acting alone or in a group. Clearly, the interaction emerges more strongly within a group setting. As Miller and Dollard (1941) noted, "People in a crowd behave as they would otherwise, only more so" (p. 218).

If whites were not insulted, they engaged in reverse discrimination (cf. Dutton, 1976) and behaved consistently with current norms. Similarly, noninsulted blacks act consistently with current norms for their race (Wilson and Rogers, 1975). When emotionally aroused, however, the whites' behavior was consistent with the older, traditional pattern of discrimination against blacks. When emotionally aroused, blacks also act consistently with their traditional pattern (Wilson and Rogers, 1975). This finding is but one specific manifestation of Sargent's (1948) thesis that anger can lead to several overt responses, one of which is regression to more primitive behaviors.

When emotionally aroused, both blacks (Wilson and Rogers, 1975) and whites (present study) regress to chronologically earlier modes of appropriate racial behavior. This pattern of interracial aggression could aptly be labeled *regressive racism.* Although racism may be defined to include beliefs of inferiority of a different race, we prefer to adopt the definition of racism as simply the differential treatment of people based solely on their race (Katz, 1976). Regressive racism differs in at least two important ways from the two other major forms of individual racism. First, both the dominative and aversive racists (cf. Kovel, 1970) firmly believe in the inferiority of blacks; they never embraced the new egalitarian norms. Thus, they would not exhibit reverse discrimination. Second, the concepts of dominative and aversive racism have been applied almost exclusively to whites. Regressive racism, on the other hand, may be found in the members of any race. To a large extent, black and white Americans take a Janus-faced view of interracial encounters, one face looking forward but the other face focusing grimly on the past.

Katz and his colleagues (e.g., Katz and Glass, 1979) could interpret the Race × Insult interaction effect as evidence of ambivalence-amplification. Although the data from our program of interracial aggression may be consistent with either the

regressive racism or ambivalence-amplification interpretations, there are three major problems with the latter analysis of our data. First, ambivalence-amplification assumes that an individual cannot cope with mixed feelings: that a different-race person's behavior that threatens our positive or negative attitudes *must* be resolved by enhancing behavior that will confirm one aspect of our feelings and suppress the other. There is no direct evidence for this assumption. It is more intuitively compelling to believe that people can act on a mixture of positive and negative feelings toward other people. Second, there is no evidence to support the proposed mediating process, whether it is labeled threat to self-esteem, guilt, distress over inequity, or dissonance. Several of Katz's studies (especially Katz, Glass, Lucido, and Farber, 1979) have tested the ambivalence-amplification hypothesis that ambivalence heightens guilt and that the ensuing behavior reduces guilt. Despite the great difficulties of measuring this mediator, Katz's studies have found no supporting evidence. Furthermore, the present study included an item assessing guilt feelings in the postexperiment questionnaire. The data analyses indicated that the independent variables did not differentially affect guilt, nor did the subjects' behavior reduce guilt. Third, it is very difficult for the ambivalence-amplification hypothesis to account for blacks' aggressive behavior (Wilson and Rogers, 1975). In contrast, the concept of regressive racism can account for the behavior of both whites and blacks.

Additional support for and an extension of the concept of regressive racism may be found in the research from the Donnersteins' program. An interaction effect highly similar to the one displayed in Figure 25.1 has been reported between the race-of-victim variable and (a) anonymity-to-victim (Donnerstein et al., 1972), (b) threatened retaliation (Donnerstein et al., 1972), and (c) potential censure (Donnerstein and Donnerstein, 1973). Thus, in Figure 25.1, if the two points on the abcissa labeled "no-insult" and "insult" were to be relabeled (a) "nonanonymous" and "anonymous," (b) "retaliation" and "no retaliation," or (c) "potential censure" and "no censure," the same interaction would be present.

Crosby, Bromley, and Saxe (1980) interpreted the Donnersteins' findings as revealing that antiblack hostility is actually pervasive, but subtle and covert. The Donnersteins reported that whites were very hostile toward blacks in those conditions in which their negative behavior could not be detected or punished. These latter conditions (anonymity, no retaliation, no censure) are well-established *disinhibitors* of aggression that increase aggressive responding. The present study disclosed that a well-established *instigator* of aggression—anger arousal—produced the same effect as the disinhibitors. Perhaps regressive racism is revealed not only by emotional arousal, but also by a host of variables that disinhibit or instigate aggression. Moreover, regressive racism may be revealed whenever unprejudiced values are not fully internalized (one case of incomplete internalization occurs when the unprejudiced values are relatively new). Regressive racism may be a sufficiently inclusive concept to give coherence to a variety of interracial behavior. Interpreting the Donnersteins' data in terms of regression

racism is speculative, and perhaps this extension of the concept goes too far. The concept of regressive racism, however, accounts quite well for anger-mediated interracial aggression.

Both the blacks and whites in our studies and the whites in the Donnersteins' studies have been highly sensitive to the race of their victim. Sadly, neither whites nor blacks accorded the other race egalitarian treatment. Awaiting us, black and white together, is the fire next time; it is smoldering in the regressive racist.

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26

Predictors of Naturalistic Sexual Aggression

Neil M. Malamuth

This research integrated within a theoretical and empirical framework varied predictor factors pertaining to males' sexual aggression against women. The selection of predictors was guided by theorizing that sexual aggression is caused by the interaction among multiple factors, including those creating the motivation for the act, those reducing internal and external inhibitions, and those providing the opportunity for the act to occur. The predictor factors assessed were sexual arousal in response to aggression, dominance as a motive for sexual acts, hostility toward women, attitudes accepting of violence against women, psychoticism, and sexual experience. A measure assessing self-reported sexual aggression (primarily among acquaintances) in naturalistic settings served as the dependent measure. The subjects were 155 males. As expected, nearly all the predictor factors significantly related to sexual aggression. In addition, much better prediction of such aggression was achieved by a combination of these factors than by any one individually. It was also found that including interactions among these predictors yielded a regression equation that was more successful in relating to sexual aggression than an equation using an additive combination only. The relevance of these data to the causes and prediction of violence against women is discussed.

Within the last decade, there has been increasing research on the causes of male sexual aggression against women, particularly rape. As described later, most of this research attempted to identify individual factors that may predict such aggression. More recently, however, there has been growing recognition of the need for multifactorial models.

The theoretical guidance for the present research was provided at the general level by Bandura's social learning theory of aggression (1973, 1978) and by

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various applications of it to sexual aggression (e.g., Earls, 1983; Malamuth, 1983b; Marshall and Barbaree, 1984). Also providing theoretical guidance was a recent model of the causes of child sexual abuse (Finkelhor, 1984; Finkelhor and Araji, 1983) and its extension by Russell (1984) to sexual aggression. These theories have several features in common. Most important, they emphasize that to understand the causes of sexual aggression it is essential to consider the role of multiple factors, such as those creating the motivation to commit the act, those reducing internal and external inhibitions that might prevent it from being carried out, and those providing the opportunity for the act to occur. Some of these multifactorial models propose additive (e.g., Earls, 1983) and others propose interactive (e.g., Bandura, 1978; Finkelhor, 1984; Malamuth, 1983b) combinations of the causal factors.

The research reported here empirically compared three alternative theoretical models regarding the causes of sexual aggression: The Single Factor model suggests that sexual aggression results from a single factor (e.g., hostility). The Additive model posits multiple factors combining in an additive manner (Earls, 1983). The Interactive model asserts that multiple factors (i.e., motivation, disinhibitory, and opportunity) interact to produce sexual aggression, particularly at high levels. Although the dependent measure used here primarily assesses sexual aggression between acquaintances, as noted later, I suggest that considerable similarity may exist among the causes of such aggression and that committed against nonacquaintances.

In studying self-reported naturalistic sexual aggression, six predictors were used in the present study.[1] Three were intended to assess the motivation to commit sexual aggression. These were sexual arousal in response to aggression, the desire to be sexually dominant or powerful, and hostility toward women. In addition, two variables were included primarily to measure factors that may overcome internal and external inhibitions. These consisted of attitudes condoning sexual aggression and of antisocial personality characteristics. Finally, sexual experience was assessed because if a person did not engage in sexual acts generally, then the "opportunity" for sexual aggression would not exist. As discussed later, an additional reason for including this dimension relates to differences in the degree and nature of sexual experiences between relatively sexually aggressive and nonaggressive men. The following discussion expands on the rationale for the selection of the various predictors and describes previous research pertaining to each.

SEXUAL RESPONSIVENESS TO RAPE

The most widely studied response designed to differentiate rapists from nonrapists has been the penile tumescence rape index, a ratio of sexual arousal to rape por-

[1]The term *predictor* is used here in the statistical sense and does not necessarily imply a temporal or causal relation with the criterion (or dependent) variable.

trayals compared with arousal to consenting sex portrayals (Abel, Barlow, Blanchard, and Guild, 1977). With this index, a man whose penile tumescence to rape is similar to or greater than his tumescence to consenting depictions is considered to have some inclination to rape (see Quinsey, 1984, for a review).

DOMINANCE

The view has been widely expressed that the desire to dominate women is an important motive of sexual aggression both at the cultural (Brownmiller, 1975; Sanday, 1981) and individual (e.g., Scully and Marolla, 1985) levels. Based on clinical interviews with convicted rapists, Groth (1979) concluded that in all cases of forcible rape, three components are present: power, anger, and sexuality. Groth (1979) distinguished among several types of rapists depending on the primary element characterizing their motivation: the power rapist, the anger rapist, and the sadistic rapist. The most common type among the convicted rapists studied by Groth (i.e., 55 percent) was the power rapist. Here he suggests that the offender's desire is to conquer and sexually dominate his victim.

HOSTILITY TOWARD WOMEN

The second most frequent type of rapist, according to Groth (1979) (i.e., about 40 percent of those he studied) is the anger rapist, characterized by his hostility to women. In the present research, hostility toward women was studied primarily for its possible motivating functions. However, it may also discriminate between men who would and those who would not be inhibited by women's suffering from and resistance to sexual aggression. Research on the consequences of victims' reactions to nonsexual aggression indicates that the aggressors' hostile feelings may be a very important determinant. For those feeling relatively low hostility, the victim's suffering and resistance is likely to be unpleasant and therefore inhibit aggression (Geen, 1970; Rule and Leger, 1976). In contrast, for those with relatively high hostility, the victim's suffering may actually be reinforcing and thereby encourage further aggression in the face of resistance (Baron, 1974, 1977; Feshbach, Stiles, and Bitter, 1967; Hartmann, 1969).

ATTITUDES FACILITATING AGGRESSION

Burt (1978, 1980) theorized that certain attitudes that are widely accepted in Western culture but are particularly held by rapists and potential rapists play an important role in contributing to sexual violence by acting as "psychological releasers or

neutralizers, allowing potential rapists to turn off social prohibitions against injuring or using others" (1978, p. 282). She developed several scales to measure attitudes that directly and indirectly support aggression against women. Data consistent with Burt's theorizing indicate that male college students' levels of sexual aggression are correlated with attitudes condoning violence against women (e.g., Koss, Leonard, Beezley, and Oros, 1985). In addition, it has been found that convicted rapists have relatively high acceptance of violence against women (e.g., Scully and Marolla, 1984).

ANTISOCIAL PERSONALITY CHARACTERISTICS/PSYCHOTICISM

Rapaport and Burkhart (1984) recently suggested that, although certain factors may provide a context for coercive sexual behavior, the actual expression of aggression occurs only if the subject also has certain personality or characterological deficits. Although these investigators did not directly test this proposition, convicted rapists do sometimes show elevated scores on measures of psychopathic/antisocial characteristics (Armentrout and Hauer, 1978; Rada, 1977). Koss and Leonard (1984) point out, however, that a major problem in these studies has been the failure to control for demographic variables that could cause spurious elevation. Koss and Leonard (1984) found only very weak and/or nonsignificant relations in various studies assessing possible links between measures of psychopathy and sexual aggression among men from the general population.

SEXUAL EXPERIENCE

As noted earlier, assessment of sexual experience may be useful to include in the prediction of sexual aggression due to *opportunity* or access. If powerful factors (e.g., religious convictions) prevented a person from participating in sexual relationships generally, he might not be sexually aggressive even if he has a high proclivity for such behavior. Particularly in the case of nonstranger sexual aggression, it is likely that the willingness and opportunity to engage in sexuality per se is an important factor distinguishing those who will and those who will not express an inclination to sexually aggress in actual behavior.

In addition to the opportunity function, an assessment of sexual experience was included in the present research in light of Kanin's (1957, 1983, 1984) studies of college males who have engaged in various degrees of sexual aggression. He found that more sexually aggressive men are more likely to view sexuality as a means of establishing their self-worth and as an arena for "conquest." They were also found to be more sexually experienced at younger ages, but less likely to view these ex-

periences as satisfactory. Similar data with convicted rapists were recently reported by Langevin, Paitich, and Russon (1985).

PREDICTING LABORATORY AGGRESSION

Malamuth (1983a) assessed the extent to which two of the factors described earlier predicted males' laboratory aggression against women. Males' penile tumescence to rape portrayals as compared with mutually consenting depictions and their attitudes condoning violent acts such as rape and wife battering were assessed in one session. About a week later, subjects participated in what they believed was a totally unrelated experiment. In it they were first angered by a female confederate of the experimenter. Later, they could choose to aggress against her via the administration of aversive noise and other responses. It was found that the degree of both men's attitudes facilitating aggression and of their sexual arousal to rape predicted significantly the amount of their laboratory aggression against the woman. In assessing one aspect of these findings, Malamuth and Check (1982) successfully replicated the relation between attitudes condoning aggression against women and laboratory aggression measured several days later in an ostensibly unrelated context.

THE PRESENT RESEARCH

The present study assessed empirically the prediction of sexual aggression in naturalistic settings using the factors described earlier. Sexual aggression was measured by a self-report inventory developed by Koss and Oros (1982). This measure asks male subjects whether they have engaged in a wide range of coercive sexual activities, ranging from trying to get intercourse by "threatening" to end the relationship, to actually using physical force, such as twisting a woman's arm, to coerce her into intercourse.

It is important to note that the two types of aggression measures used in our research program complement each other well, having opposite advantages and disadvantages. The advantage of the laboratory assessment of aggression (Malamuth, 1983a, 1984b; Malamuth and Check, 1982) is that it is an "objective" measure that does not rely on subjects' self-reports. However, it assesses behavior in a setting that some argue is artificial and low in ecological validity (e.g., Kaplan, 1983). The measure of naturalistic aggression has the advantage of assessing behavior occurring in nonartificial settings. Its disadvantage is in being a self-report measure. Consequently, considerable confidence in the validity of the relations would be gained if the predictors related to both of these aggression measures. Of course, although some similarity may be expected, conceptually there are also important differences between aggression in the laboratory, which does not contain any overt sexual elements (i.e., administering aversive noise to a person in the next room), and aggression in natural settings which occurs within a sexual context.

The factors studied in the present investigation are based on theory and research conducted either with convicted rapists or with subjects from the general population, particularly college students. One of the issues that the current data pertain to is the assertion (see Russell, 1984; Scully and Marolla, 1985) that similar factors contribute to both the type of sexual aggression committed by incarcerated rapists (usually against nonacquaintances) and the type that does not receive legal attention, particularly that committed against acquaintances. If variables derived from work with convicted rapists and with sexual aggressors in the general population can be integrated within a unified empirical and theoretical framework, the findings will provide a firmer basis for understanding the causes of sexual aggression in both populations.

Based on the theory and research described, three interrelated questions were investigated in the present research:

1. Would the predictor factors relate significantly to naturalistic aggression against women?
2. If the factors related to sexual aggression against women, would they provide "redundant prediction" or would a combination of factors predict better than each alone?
3. If a combination of factors were superior, would the Additive or the Interactive models provide the best prediction of naturalistic aggression?

METHOD

Overview of Design

One hundred fifty-five males participated in the first phase of the research. In this phase subjects completed various scales, including all the predictors except for the sexual arousal measure, as well as the dependent measure of sexual aggression.

The second phase consisted of the assessment of sexual arousal in response to rape portrayals and to consenting depictions. Ninety-five of those participating in Phase 1 also participated in Phase 2.

Subjects

Subjects were recruited from various sources: Several university courses, announcements displayed on university campuses and at a city summer employment center, and via newspaper ads.

The initial descriptions of the research indicated that applicants at or over the age of 18 were needed to participate in various unrelated experiments. They were told that they may sign up for a general subject pool. Experimenters would then se-

lect subjects from this list and invite them to participate in specific experiments. Participants were paid about $5.50 per hour.

When contacted by the different experimenters conducting each phase (presented to subjects as independent experiments), potential subjects were given general descriptions of the procedures and measures used. For example, in Phase 2 they were informed that genital measures of sexual arousal would be used. It was emphasized in each phase that subjects could leave at any time and that there would be no penalty whatsoever nor would any explanation be required. Subjects were paid upon arrival at each study and were told that they could keep the money irrespective of whether they completed the experiment. As an additional safeguard, an ombudsman, who was a Professor of Law, was hired for the project. All subjects were given his name and phone number upon signing up for the subject pool. They were told that he was completely independent of the staff conducting the research and that they could voice any complaints to him. No complaints were ever made.

At the end of Phase 2 subjects were given debriefings. They included segments emphasizing the horror of rape and presented several points designed to dispel rape myths. The effectiveness of such debriefings in counteracting some potential negative effects of exposure to sexually violent stimuli has been demonstrated in several studies (Check and Malamuth, 1984; Donnerstein & Berkowitz, 1981; Linz, 1985; Malamuth and Check, 1984).

Although subjects' names were not obtained, various background information that was gathered (e.g., date of birth) enabled exact matching of responses across the two research phases. The purpose of leading subjects to believe that these were independent studies was to reduce "demand characteristics" (Orne, 1962) and/or undue self-consciousness that might affect honest responding. Similar procedures have been used successfully in other studies (e.g., Malamuth, 1983a; Malamuth and Check, 1981). The two research phases were completed within 2 months for virtually all subjects.

Obtaining background information enabled a general description of the sample. Subjects were asked about their age, marital status, whether they were students, their major, religious affiliation, and their frequency of attendance of church or other religious institution. These variables were selected on the basis of previous research (e.g., Koss et al., 1985; Schulz, Bohrnstedt, Borgatta, and Evans, 1977) showing that factors such as religiousness and age may affect college students' sexual and sexually aggressive behavior. If the regression analyses reported later are computed by first partialing out the background factors, the relations are at least as strong as those reported without such partialing.

The average age of the 155 male subjects was 23, with a range from 18 to 47 years. Sixty-six percent of the sample were between the ages of 18 to 22, 24 percent between the ages of 23 to 30, and the remaining 10 percent were above the age of 30. Eighty-seven percent of the sample were single, 8 percent were married, and the remaining 5 percent were separated or divorced. Eighty percent were university students, 20 percent were not. Of the students, 21 percent majored in the "pure" sciences, 13 percent in engineering, and 23 percent in the humanities and the social

sciences (including psychology). The remainder were distributed over a wide range of majors or as yet undeclared. Twenty percent were Catholics, 32 percent Protestants, 8 percent Jewish, and the remaining 40 percent listed no specific religious affiliation. Fifteen percent indicated that they visit a religious institution (e.g., church) at least once a week, 11 percent at least once a month, 8 percent approximately every two months, and the remaining 56 percent seldom or never.

Phase 1: Materials and Procedure

In the first phase, subjects completed a questionnaire administered by a male experimenter. While filling out this measure they were seated sufficiently apart to ensure confidentiality of responses. Embedded within other items on this questionnaire were the following measures:

Dominance as sexual motive. Part of a measure developed by Nelson (1979) assessed the function of or motivations for engaging in sexual acts. This measure asks respondents the degree to which various feelings and sensations are important to them as motives for sexual behavior. Nelson (1979) presented data concerning the reliability and validity of this scale, which yields scores on several functions of sexuality. The present study used the dominance segment (eight items) of the power function (composed of the dominance and submissiveness segments). This dominance component refers to the degree to which feelings of control over one's partner motivate sexuality (e.g., "I enjoy the feeling of having someone in my grasp"; "I enjoy the conquest"). It yielded an alpha coefficient of .78.

Hostility. The Hostility Toward Women (HTW) scale (30 items) was recently developed by Check and Malamuth, 1983 (see also Check, Malamuth, Elias, and Barton, 1985). Data concerning its reliability and validity were presented by Check (1985). Examples of items are "Women irritate me a great deal more than they are aware of," and "When I look back at what's happened to me, I don't feel at all resentful toward the women in my life." In the present study it had an alpha coefficient of .89.

Attitudes facilitating violence. The attitude measure used in this study was the Acceptance of Interpersonal Violence (AIV) against women scale developed by Burt (1980). Five of its six items measure attitudes supporting violence against women, whereas the sixth concerns revenge. An example of an item is "Sometimes the only way a man can get a cold woman turned on is to use force." This scale was selected because it measures attitudes that *directly* condone the use of force in sexual relationships. Two other scales developed by Burt (1980) assessing attitudes *indirectly* supportive of sexual aggression, the Rape Myth Acceptance and the Adversarial Sexual Beliefs scales, were also used. The findings with these measures were very similar to those with the AIV, but as expected the relations with sexual

aggression were somewhat weaker. The results presented in this article are for the AIV scale only. It had an alpha coefficient of .61, which is similar to that originally reported by Burt (1980).

Antisocial characteristics/psychoticism. The Psychoticism (P) scale of the Eysenck Personality Questionnaire (EPQ) was used (Eysenck, 1978). As Eysenck makes abundantly clear, this scale purports to reflect a variable that stretches through the normal, nonpsychiatric population.

There were three reasons for selecting this particular measure to assess antisocial tendencies. First, Eysenck (1978) hypothesized that psychoticism may be particularly associated with interest in impersonal sex and in sexual aggression. He also reports the findings of an unpublished study that sex offenders are relatively high P scorers. Second, reviews of the literature pertaining to this measure (e.g., Claridge, 1983) concluded that rather than being a measure of psychoticism in the clinical sense, this scale primarily assesses antisocial traits that may relate to aggression. Third, recent research (Barnes, Malamuth, and Check, 1984; Linz, 1985) suggests that this measure may be particularly useful in predicting some sexually aggressive responses.

The alpha coefficient obtained herein was .49. Although Eysenck (1978) had originally reported relatively high alpha coefficients, other researchers have recently reported similar relatively low levels of internal consistency as found here (e.g., McCrae and Costa, 1985). Nevertheless, this measure was retained in the current analyses with the recognition that relatively low levels of internal consistency reduce the likelihood of obtaining statistically significant relations with other variables (Cohen and Cohen, 1983).

Sexual experience. The Sexual Behavior Inventory (SBI: Bentler, 1968) was used to assess sexual experience in conventional heterosexual acts. Subjects indicated whether they had engaged in various sexual behaviors including kissing, fondling of breasts, intercourse, and oral sex. The alpha coefficient for this scale was .97.

Naturalistic sexual aggression. As noted earlier, the self-report instrument used to measure sexual aggression was developed by Koss and Oros (1982). It assesses a continuum of sexual aggression including psychological pressure, physical coercion, attempted rape, and rape. Subjects are asked to respond to a sexual experience survey consisting of nine circumstances pertaining to the use of aggression in the context of sexuality (e.g., sexual, oral, or anal intercourse).[2] An example of an item is "I have had sexual intercourse with a woman when she didn't want to because I used some degree of physical force (twisting her arm, holding her down,

[2]Although the scale used by Koss and Oros (1982) contains 10 items, 1 item judged ambiguous was not used in the analyses here. It asks whether the subject ever became so sexually aroused that he could not stop himself even though the woman did not want to. The ambiguity is in the lack of information regarding what sexual acts occurred and what type of coercion may have been used.

etc.)." Respondents reply on a *true* versus *false* scale. Koss and Oros (1982) and Koss and Gidycz (1985) presented data regarding the reliability and validity of this scale. In the present study, it had an alpha coefficient of .83.

Phase 2: Materials and Procedure

In the second research phase, sexual arousal in response to rape and to mutually consenting depictions was assessed. In keeping with the accepted methodology and the empirical data in this area (e.g., Abel et al., 1977; Earls and Marshall, 1983), the primary assessment was direct genital arousal measured by penile tumescence. Subjects were seated in a comfortable chair located in a sound attenuated and electrically shielded room equipped with an intercom. Penile tumescence was measured by a mercury-in-rubber strain gauge (Davis Inc., New York City), a device recommended in analyses of various instruments (Laws, 1977; Rosen and Keefe, 1978). Changes in penile diameter that resulted in resistance changes in the mercury column of this strain gauge were amplified through a Wheatstone Bridge and recorded on a polygraph.

For comparison of consistency with the physiological measure, self-reported sexual arousal was assessed on an 11-point scale ranging from 0% (*not at all*) to 100% (*very*) in units of 10 percent. Subjects indicated their arousal immediately after reading each story.

Upon arrival at the laboratory, the subject was greeted by a male experimenter. He then was given a sheet reiterating the information provided on the phone regarding the sexual content of some stimuli and the use of genital arousal measures. After signing a consent form, which emphasized that the subject was free to leave at any time without any penalty and without having to provide any reason to the experimenter, he was escorted to the sound attenuated room. Further instructions were taped, although an intercom was available if communication between the subject and the experimenter was necessary.

The subject was instructed to place the penile gauge on. Following a baseline period, he was told to open a numbered envelope and read the story. Arousal to the stories was monitored by the polygraph in the adjoining room. After the subject read each story and indicated his sexual arousal on a scale, a resting period was interposed to ensure that arousal returned to baseline levels before proceeding to the next story.

There were three depictions read in order. The first described a woman masturbating. Its primary purpose was to generate some initial level of sexual arousal in light of data (Kolarsky and Madlafousek, 1977) suggesting that arousal levels are better differentiated if presented following the elicitation of some sexual arousal rather than immediately following the first baseline period. Additionally, this story was intended to strengthen the credibility of the experimental instructions that the research concerned responses to various types of stimuli. The second and third

written stories depicted rape and mutually consenting sex, respectively. They were virtually identical to those used by Abel et al. (1977).

"Rape indices" were computed for each subject following Abel et al. (1977) by dividing maximum arousal to rape by maximum arousal to consenting sex for the tumescence data and for self-reported arousal.[3]

RESULTS

Volunteers Versus Nonvolunteers

Comparisons were made between the 95 volunteers for the second research phase and the 60 who did not volunteer. These comparisons used the 5 predictors assessed in the first phase and the measure of sexual aggression. No significant differences or effects approaching significance were obtained in either a multivariate analysis of variance or in univariate analyses of variance (ANOVAs).

Intercorrelations Among Predictors

Simple Pearson correlations among the predictors are presented in Table 26.1. In general, these data are highly consistent with previous findings (e.g., Malamuth, 1983a; 1984a; Malamuth and Check, 1983), as well as revealing relations not examined in earlier studies. The tumescence index of sexual arousal (i.e., arousal to rape contrasted with arousal to nonrape) was highly correlated with the similar index based on self-reported arousal. To a large degree, these two sexual arousal indices showed similar relations with the other variables: Both indices were significantly associated with the dominance motive and neither related significantly to psychoticism nor to sexual experience. Although the reported arousal index significantly correlated with hostility toward women and with AIV, the tumescence index showed a marginally significant effect with hostility and no significant correlation with AIV.

The dominance motive related significantly to all of the other predictors, except for sex experience, where a marginally significant correlation occurred. Hostility toward women correlated with AIV and revealed a marginally significant relation with psychoticism. Hostility showed a nonsignificant inverse relation with sexual experience. AIV did not relate to either psychoticism or to sexual experience, nor were these two variables related to each other.

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[3]In some previous research (e.g., Malamuth, 1983a) a difference rather than a ratio score was used. The ratio was used here in keeping with the commonly accepted procedure. The findings are very similar if the difference score is used.

TABLE 26.1
Simple correlations among the predictor variables and sexual aggression[a]

Predictors and dependent variable	1	2	3	4	5	6	7	8
Motivation predictors								
1. TUMRAPE	—	.66****	.25***	.19*	.13	.07	.06	.43****
2. REPRAPE		—	.23**	.26***	.22**	.06	.15	.33***
3. DOM			—	.37****	.36****	.19**	.15*	.38****
4. HTW				—	.37****	.16*	−.11	.30****
Disinhibiting predictors								
5. AIV					—	.08	−.08	.38***
6. PSYCH						—	−.03	.15*
Opportunity predictor								
7. SEXEXP							—	.32****
Dependent variable								
8. SEXAGG								—

Note: TUMRAPE = tumescence arousal to rape index; REPRAPE = reported arousal to rape index; DOM = dominance motive; HTW = hostility toward women scale; AIV = acceptance of interpersonal violence (against women) scale; PSYCH = psychoticism scale; SEXEXP = sexual experience measure; SEXAGG = sexual aggression.

[a]$n = 155$, except with sexual arousal measures where $n = 95$. *$p < .10$. **$p < .05$. ***$p < .02$. ****$p < .001$.

Naturalistic Aggression

The last column of Table 26.1 shows simple correlations between the predictors and self-reported naturalistic sexual aggression. All the predictors related significantly to sexual aggression, except for psychoticism which showed a marginally significant correlation.

Multiple regression analyses were conducted to address the issue of the combined success of the predictors to relate to sexual aggression. As recommended by Cohen and Cohen (1983), all the predictors were centered at their mean, a linear transformation that reduces multicollinearity that may occur with products such as interaction terms. In this analysis, I sought to compare the Single Factor, Additive, and the Interactive models. For the 155 participants in Phase 1, this regression analysis included dominance, hostility toward women, AIV, psychoticism, and sexual experience. For the 95 participants in both research phases, analyses were conducted with the addition of tumescence rape index.

The regression analyses were performed in the following way: To test the Single Factor model versus the Additive model, each predictor was "forced entered" as a main effect and its unique contribution (i.e., that not shared with any other predictors) to the dependent variable was assessed by squared semipartial correlations (Cohen and Cohen, 1983). The results clearly showed that the predictors did not, in general, provide redundant information, but that their combined prediction was

TABLE 26.2
Multiple regression analyses on sexual aggression without tumescence index (n = 155)

	Without interactions		With interactions	
Predictor	Beta[a]	sr^{2b}	Beta[a]	sr^{2b}
DOM	.153	.017*	.078	.005
HTW	.198	.030**	.147	.017**
AIV	.205	.033***	.210	.032***
PSYCH	.073	.005	.102	.010
SEXEXP	.340	.109****	.257	.058****
HTW × PSYCH	—	—	.150	.022**
HTW × AIV × SEXEXP	—	—	.334	.092****
HTW × DOM × AIV × SEXEXP	—	—	.313	.078****
Multiple				
R	.547****		.673****	
R^2	.300		.453	

Note: DOM = dominance motive; HTW = hostility toward women scale; AIV = acceptance of interpersonal violence (against women) scale; PSYCH = psychoticism scale; SEXEXP = sexual experience measure.

[a]Standardized regression coefficient.

[b]Squared semipartial correlation coefficient indicating unique contribution of predictor variable to dependent variable.

*p < .06. **p < .05. ***p < .01. ****p < .0001.

considerably greater than that achieved by any variable alone. More specifically, when the entire sample was used (n = 155) the HTW, AIV, and sex experience variables all had significant, unique contributions to the regression equation, whereas the unique contribution of dominance was marginally significant (see left columns of Table 26.2). The psychoticism variable did not make a significant, unique contribution. As indicated on the left side of Table 26.2, the Multiple *R* yielded by this equation assessing additive effects was .547.

When the regression analysis assessing additive effects was performed on the 95 participants in both research phases, the Multiple *R* was .619 (see left side of Table 26.3). As indicated in this table, the tumescence rape index, AIV, HTW and sex experience contributed significant, unique variance, whereas the dominance and psychoticism predictors did not.

Although these data show that a combination of predictor variables is superior to individual ones, an additional regression analysis compared the Interactive with the Additive models for combining predictors. Although the Interactive model contends that several factors must interact for relatively high levels of sexual aggression to occur, the use of more than one variable within a given theoretical category (e.g., both the sexual arousal in response to aggression and HTW variables were included as motivational factors) did not enable precise specification of which interaction set would best test this model. Rather than preferring a particular interaction, all

TABLE 26.3
Multiple regression analyses on sexual aggression with tumescence index (n = 95)

	Without interactions		With interactions	
Predictor	Beta[a]	sr^{2b}	Beta[a]	sr^{2b}
TUMRAPE	.329	.100***	.206	.026**
DOM	.085	.006	.170	.017*
HTW	.209	.032*	.037	.001
AIV	.207	.035*	.168	.022*
PSYCH	.027	.001	.016	.000
SEXEXP	.267	.066**	.111	.010
AIV × EXEXP	—	—	.166	.025**
TUMRAPE × DOM × AIV × PSYCH	—	—	.200	.029**
TUMRAPE × DOM × HTW × AIV	—	—	.493	.151****
TUMRAPE × DOM × HTW × AIV × SEXEXP	—	—	.445	.158****
Multiple				
R	.619****		.865****	
R^2	.383		.748	

Note: TUMRAPE = tumescence arousal to rape index; DOM = dominance motive; HTW = hostility toward women scale; AIV = acceptance of interpersonal violence (against women) scale; PSYCH = psychoticism scale; SEXEXP = sexual experience measure.

[a]Standardized regression coefficient

[b]Squared semi-partial correlation coefficient indicating unique contribution of variable.

*p < .05. **p < .005. ***p < .001. ****p < .0001.

possible interactions (i.e., the crossproducts) were allowed "free entry" in a stepwise process after the "forced entry" of the main effects.[4]

The resulting equation for the 155 subjects yielded a Multiple R of .673 (see right side of Table 26.2). Contributing significant unique variance were a two-way interaction between the HTW and psychoticism, a three-way interaction among HTW, AIV, and sex experience and a four-way interaction that contained these three variables, as well as dominance (see Table 26.2). A hierarchical comparison of the model with the interactions (R^2 = .453) versus the model with the main effects only (R^2 = .300) yielded a significant effect, $F(3, 146) = 9.99$, $p < .001$.

With the 95 subjects participating in both research phases, the regression equation including interactions yielded a Multiple R of .865 (see right side of Table 26.3). The squared semipartial correlation coefficients indicated that contributing

[4]Some statisticians might test the Interaction model by a fully hierarchical approach in which regression models with higher order interactions are assessed only in comparison with nested models including all the lower order interactions. The difference in such an approach as contrasted to that used here (i.e., allowing "free entry" to all the interactions) concerns only which interactions are most appropriate to include in the model and not whether some interactions account for additional variance.

unique variance were a two-way interaction between AIV and sex experience, a four-way interaction among the tumescence index, dominance, AIV, and psychoticism, a four-way interaction among the tumescence index, dominance, HTW, and sex experience, as well as a five-way interaction containing these four variables and AIV (see right columns of Table 26.3). A comparison of the models with and without the interactions yielded a highly significant difference, $F(4, 84) = 12.86$, $p < .001$. The results for both samples, therefore, indicate that a regression model containing interactive relations among the predictors is preferable to a model containing additive relations only.

To directly assess whether the tumescence rape index provided additional predictive information, it was necessary to compare regression models with and without this variable for the same 95 subjects. This comparison indicated that the regression model with this variable (Multiple $R = .865$) was significantly better than without it (Multiple $R = .600$), $F(3, 85) = 16.45$, $p < .001$.

To illustrate and further examine the data, the following analysis was performed: For each predictor a relatively high score was defined as above the median of its distribution. Subjects were then divided according to the number of predictors for which they scored *high* and *low.* This approach is analogous to classifying a characteristic as present or not by defining presence as a relatively high score. A person scoring above the median on all the variables would be considered as possessing all the characteristics. In keeping with the regression results, for the 155 subjects the dominance, HTW, AIV, psychoticism, and sex experience predictors were used for this classification, whereas for the 95 subjects these variables as well as the tumescence rape index were used.

Figure 26.1 shows the average level of sexual aggression according to this classification scheme, with the top graph showing the results for the entire sample of 155 subjects and the bottom graph for the 95 participants in the two research phases. In both instances, ANOVAs performed on these data yielded highly significant ($p < .0001$) effects. Comparisons among means were performed using the Scheffé test (Scheffé, 1953) for groups differing substantially in size and the Tukey test (Tukey, 1953) for those of similar size. These comparisons indicated that the highest levels in both graphs were significantly different ($p < .05$) from all others and that the second highest levels differed significantly from some of the lowest levels. Trend analyses showed that a cubic term fitted the curve within statistical error for the sample of 155 subjects, whereas a quintic term fitted the curve for the 95 participants in both research phases. ANOVAs comparing average sexual aggression within each level of this classification scheme (e.g., those scoring high on one set of four predictors vs. those scoring high on a different set of four predictors) supported the rationale of classifying subjects according to the number of predictors on which they scored relatively high: For both samples, no significant differences were found within each classification level. It should be noted, however, that the relatively small numbers in each cell reduced the likelihood of finding such differences.

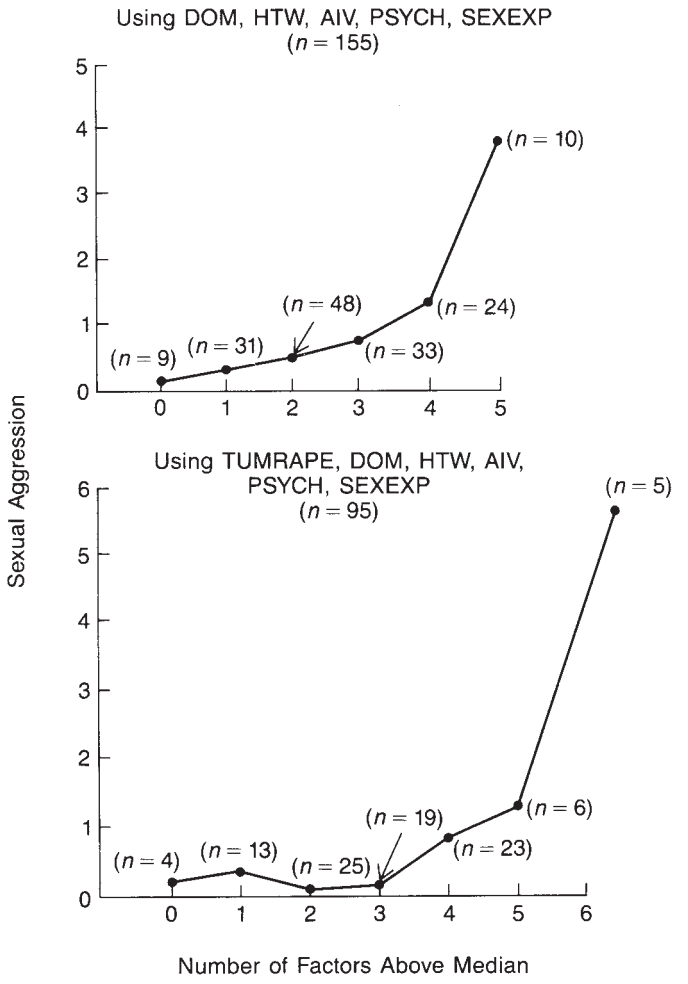

FIGURE 26.1
Mean levels of sexual aggression as a function of number of factors on which subjects scored above median.

DISCUSSION

The data provided the following answers to the three questions posed earlier: First, in the simple correlation analyses all the predictors except psychoticism were significantly related to naturalistic aggression, and psychoticism showed a marginally significant relation. Second, the predictors did not, on the whole, provide "redundant information" in that a combination of them was superior to any individual ones for predicting levels of sexual aggressiveness. Third, the data were more consistent with the Interactive than with the Additive model of combining the predictors. Regression equations containing interactive effects accounted for a significantly greater percentage of the variance (45 percent for the 155 subjects and 75

percent for the 95 subjects) than equations containing additive effects only (30 percent and 45 percent, respectively). However, it may be that a modified version of the Interactive model that also incorporates additive effects would best account for the data. Multifactorial models in other areas of research (e.g., Faraone and Tsuang, 1985) may provide useful guides in the development of such a model.

Additional analyses were conducted classifying subjects according to the number of predictors on which they scored relatively high (i.e., above the median). A curve was found indicating that with an increasing number of predictors with high scores, greater levels of sexual aggression occurred. The data pattern appeared to show a synergistic process whereby the combined action of several variables yielded considerably higher levels of sexual aggression than would be expected by the additive combination of them.

Malamuth and Check (1985b) attempted a partial replication of the research reported here. They administered to 297 males the same measures used here, except for the sexual arousal indices and for psychoticism. The results replicated very successfully the present conclusions: The predictors related significantly to sexual aggression, a combination of predictors was superior to individual ones, and an equation including interactions was preferable to an additive one only. Further, classifying subjects according to the number of predictors on which they scored relatively high showed the same general relation reported here, although the slope of the line was somewhat less steep. This appeared to be due, at least in part, to a lower proportion of subjects in that study at the very high levels of sexual aggression as compared with the present or earlier studies.

Malamuth (1984b) recently assessed the ability of a number of the predictors used in the present research (i.e., tumescence rape index, self-reported arousal to rape index, dominance, AIV, and psychoticism) to predict laboratory aggression against female and against male targets. Laboratory aggression was measured in a procedure similar to that used by Malamuth (1983a). The results showed that except for the penile tumescence measure (which was in the expected direction but not statistically significant), all the predictors significantly related to aggression against females. The data for male targets were more ambiguous, suggesting no or possibly weak relations with the predictors. On the basis of this study and earlier work (Malamuth, 1983a; Malamuth and Check, 1982), it is apparent that the same predictors found here to relate to self-reported naturalistic sexual aggression also relate in similar ways to laboratory aggression against women.[5]

The present findings suggest a high degree of similarity between some factors contributing to stranger and to acquaintance rape. Although we did not specifically

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[5]Some subjects in the research measuring laboratory aggressiveness also participated in the present research. This enabled assessing the relation between laboratory aggression and a composite of six items from the measure of naturalistic sexual aggression that concern the use of force or aggression. (The other three items refer to psychological tactics such as "saying something you don't mean.") Reported naturalistic aggression correlated significantly with both laboratory aggression against males, $r(47) = .25$, $p < .04$, one-tailed, and against female targets, $r(38) = .31$, $p < .025$, one-tailed.

ask subjects whether they knew their victims, based on earlier studies using the same measure (see Koss and Leonard, 1984), it seems very likely that the vast majority of the sexually aggressive acts reported were in acquaintance situations. Yet, some of the same factors theorized and/or found to contribute to stranger rape (e.g., sexual arousal in response to aggression, dominance motivation, hostility toward women) related to sexual aggression among the subjects studied here.

The results suggest that the presence of any predictor alone is unlikely to result in high levels of sexual aggression. This conclusion may be particularly relevant to research focusing on sexual arousal in response to aggression. Although measures assessing such arousal (i.e., the tumescence rape index) have been used in the diagnosis and treatment of rapists (Quinsey, 1984), there are considerable data showing that within the general population a substantial percentage of men show arousal patterns similar to those of known rapists (e.g., Malamuth, Check, and Briere, 1986). The present results are supportive of the view that sexual arousal in response to aggression is one of the factors that may create an inclination to aggress against women. They also indicate clearly that other factors must be present before such an arousal pattern will lead to aggressive behavior. The findings point to the types of variables that should be included in clinical and research assessments.

The data also provide important information pertaining to recent research on the effects of sexually aggressive mass media stimuli. In several studies (e.g., Linz, 1985; Malamuth and Check, 1981, 1985a) exposure to certain types of media stimuli changed men's attitudes about aggression against women, including rape. Some (e.g., Vance, 1985) have downplayed the social significance of such findings by asserting that attitudes of this type have not been shown to actually relate to aggressive behavior. The present data extend earlier laboratory findings (Malamuth, 1983a, 1984b; Malamuth and Check, 1982) in showing that the same scales used to measure the impact of media exposure on attitudes (e.g., the AIV scale) are useful predictors, in combination with other factors, of actual aggression in naturalistic and in laboratory settings. Although causal relations cannot be inferred on the basis of such correlational data alone, the findings are consistent with a theoretical model hypothesizing that media depictions contribute to changes in attitudes and that these may, under certain conditions, be one of the contributing factors affecting actual aggressive behavior (see Malamuth and Briere, 1986).

An important goal for future research is to further develop and empirically test varied multifactorial models regarding the causes of sexual aggression. These models should attempt to define the causal links among the predictor variables in addition to their influences on sexual aggression. Structural equation modeling with latent variables (e.g., Bentler and Bonett, 1980; Kenny and Judd, 1984) may be particularly suited for this purpose. As well, such models should incorporate two conceptual elements suggested by the present data in combination with earlier work (e.g., Malamuth, 1984a). First, rather than adopting an "all or none" ap-

proach, sexual aggressiveness should be conceptualized along a continuum encompassing both differing degrees of inclinations to aggress and differing levels of actual aggressive behavior. Second, in attempting to understand the causes of relatively high levels of this continuum, emphasis should be placed on analyzing crucial configurations of multiple interacting factors (i.e., motivational, disinhibitory, and opportunity) rather than on searching for a single or even the primary causal factor.

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VII

PREJUDICE

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27

The Nonverbal Mediation of Self-Fulfilling Prophecies in Interracial Interaction

Carl O. Word, Mark P. Zanna, and Joel Cooper

Two experiments were designed to demonstrate the existence of a self-fulfilling prophecy mediated by nonverbal behavior in an interracial interaction. The results of Experiment 1, which employed naive, white job interviewers and trained white and black job applicants, demonstrated that black applicants received (a) less immediacy, (b) higher rates of speech errors, and (c) shorter amounts of interview time. Experiment 2 employed naive, white applicants and trained white interviewers. In this experiment subject-applicants received behaviors that approximated those given either the black or white applicants in Experiment 1. The main results indicated that subjects treated like the blacks of Experiment 1 were judged to perform less adequately and to be more nervous in the interview situation than subjects treated like the whites. The former subjects also reciprocated with less proximate positions and rated the interviewers as being less adequate and friendly. The implications of these findings for black unemployment were discussed.

Sociologist Robert Merton (1957), by suggesting that an originally false definition of a situation can influence the believer to act in such a way as to bring about that situation, is generally credited with focusing attention on the phenomenon of the self-fulfilling prophecy. The present investigation is concerned with such a phenomenon in face-to-face, dyadic interactions. In this context it is hypothesized that one person's attitudes and expectations about the other person may influence the believer's actions, which in turn, may induce the other person to behave in a way that confirms the original false definition. Interpersonally, this phenomenon has been documented in schools, with teachers' expectations influencing students' performances, and in psychology laboratories, with experimenters' expectations influencing subjects' responses (cf. Rosenthal, 1971).

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In the present study attention will be directed toward (1) possible nonverbal mediators of this effect, and (2) the reciprocal performances of the interactants. The focus, in addition, will be on the interaction of black and white Americans with a view toward examining the employment outcomes of black job applicants interviewed by whites.

ATTITUDES AND IMMEDIACY

Mehrabian (1968) has recently reported a series of studies linking attitudes toward a target person and the concomitant nonverbal behavior directed toward that person. The results of these studies have consistently found that closer interpersonal distances, more eye contact, more direct shoulder orientation, and more forward lean are a consequence of more positive attitudes toward an addressee. Mehrabian (1969) has considered such nonverbal behaviors in terms of "immediacy" and has defined immediacy "as the extent to which communication behaviors enhance closeness to and nonverbal interaction with another . . . greater immediacy is due to increasing degrees of physical proximity and/or increasing perceptual availability of the communicator to the addressee" (p. 203).

A related series of studies has been conducted by Kleck and his associates (Kleck, 1968; Kleck, Buck, Goller, London, Pfeiffer, and Vukcevic, 1968; Kleck, Ono and Hastorf, 1966) pursuing Goffman's (1963) observation that normals tend to avoid stigmatized persons. They have begun to document what might be called a nonverbal stigma effect. For example, normal interactants were found to terminate interviews sooner (Kleck et al., 1966) and to exhibit greater motoric inhibition (Kleck, 1968) with a handicapped person (i.e., leg amputee), and to employ greater interaction distances with an epileptic stranger (Kleck et al., 1968). This set of studies, then, also suggests that those persons who possess a personal characteristic that is discrediting in the eyes of others are treated with less immediate behaviors. In addition to such discrediting characteristics as a physical disability or a criminal record, Goffman (1963) includes blackness in a white society as a stigmatizing trait.

Thus, a body of data suggests that (1) attitudes toward an individual are linked with nonverbal behavior emitted toward that individual, and (2) positive attitudes lead to more immediate nonverbal behaviors. Two questions that now arise are concerned with whether such behaviors are (1) decoded or understood by the target and (2) reciprocated.

DECODING AND RECIPROCATING IMMEDIACY

Recent studies suggest that such evaluative, nonverbal behaviors are both decoded and reciprocated. Mehrabian (1967) found friendliness ratings of an interviewer varied as a function of the physical interaction distance, and the immediacy of

head and body positions given subjects. Eye contact has been extensively investigated. Both Kleck and Nuessle (1968) and Jones and Cooper (1971) found that a high degree of eye contact produced higher evaluations of the communicator and produced more positive evaluations on the part of the subjects than did low eye contact.

Since individuals apparently are able to decode affective components of communications from variations in immediacy behavior, it seems reasonable to expect they would reciprocate such variations. This proposition also has received support. Rosenfeld (1967), for example, found that subjects treated to more smiles and positive head nods did reciprocate with more of each.

Thus individuals apparently decode less immediacy as indicating less friendly behavior and reciprocate with less friendly (i.e., less immediate) behavior of their own. Since individuals seldom are able to monitor their own nonverbal behaviors, they are more likely to attribute the reciprocated immediacy, not to their own, original nonverbal behavior, but instead to some disposition inherent in their cointeractant (cf. Jones and Nisbett, 1971). With this nonverbal reciprocation, then, a self-fulfilling prophecy is born.

WHITE-BLACK INTERACTION IN A JOB INTERVIEW SETTING

So far we have been concerned with describing possible mechanisms of interpersonal, self-fulfilling prophecies. The discussion now turns to consider such a process in black-white, dyadic interactions. It has been demonstrated time and again that white Americans have generalized, negative evaluations (e.g., stereotypes) of black Americans. This has been shown most recently in our own subject population by Darley, Lewis, and Glucksberg (1972). Such negative evaluations, of course, represent the kind of attitudes that can initiate an interpersonal, self-fulfilling prophecy. The general hypothesis that the present study sought to investigate, therefore, was that whites interacting with blacks will emit nonverbal behaviors corresponding to negative evaluations and that blacks, in turn, will reciprocate with less immediate behaviors. If the context in which the interaction occurs involves a job interview, with the white interviewing the black, such reciprocated behavior may be interpreted as less adequate performance, thus confirming, in part, the interviewer's original attitude.

These general expectations are operationalized by two subhypotheses: First, black, as compared with white, job applicants will receive less immediate nonverbal communications from white job interviewers; second, recipients of less immediate nonverbal communications, whether black or white, will reciprocate these communications and be judged to perform less adequately in the job interview situation than recipients of more positive nonverbal communications. The first hypothesis was tested in Experiment 1, which employed naive, white job interviewers

and trained white and black job applicants; the second in Experiment 2, which used naive, white job applicants and trained white job interviewers who were instructed to emit either immediate or nonimmediate cues.

EXPERIMENT 1

METHOD

Overview

In the context of a study on group decision-making white subjects, as representatives of a team in competition with other teams, interviewed both white and black job applicants. The applicants were trained to respond similarly in both the verbal and nonverbal channels. The interview situation itself was arranged to give the subject-interviewers the opportunity to treat their applicants differentially without the knowledge (1) that their own behavior was being monitored, or (2) that race of the applicants was the experimental variable.

Subjects (Interviewers) and Confederates (Applicants and Team Members)

Subject-interviewers were 15 white male Princeton University students recruited to participate in a study of group decision-making conducted by Career Services and the Psychology Department. They were informed that the study would last approximately one hour and a half and that they would be paid $2.00 and possibly $5.00 more. One of the subjects was eliminated when he indicated that he was aware of the purpose of the study before the debriefing period. No other subject volunteered this sort of information after intensive probing, leaving an *n* of 14.

Confederate-applicants were two black and three white high school student volunteers referred by their high school counselor. Each was told that the study was concerned with cognitive functioning and that the experimenter was interested in finding out how subjects made up their minds when forced to choose between nearly identical job applicants. All confederates in both experiments were naive with respect to the hypotheses. Intensive probing following the experiment indicated that they did not become aware. The three confederates who served as the subject's "team members" and the experimenter were male volunteers from Princeton.

Procedure

Upon arrival the subjects entered a room containing two confederate team members, who introduced themselves and acted friendly. Another confederate entered and acted friendly, as well. Then the experimenter entered, handed out written instructions and answered any questions.

The instructions informed subjects that the four people in the room constituted a team; that they were to compete with four other teams in planning a marketing campaign; and that they needed to select another member from four high school applicants. To increase incentive and concern, an additional $5.00 was promised to the team which performed best in the competition. Using a supposedly random draw, the subject was chosen to interview the applicants. He was then handed a list of 15 questions which was to serve as the interview material, told he had 45 minutes to interview all four high school students and taken to the interview room where the first confederate-applicant was already seated.

To measure the physical distance that the interviewer placed himself from the applicant, the experimenter upon entering the interview room, feigned to discover that there was no chair for the interviewer. Subjects were then asked to wheel in a chair from an adjoining room.

Subjects were led to believe that there would be four interviews so that the race variable would be less apparent to them. In addition, to eliminate any special effect that might occur in the first and last interview, an a priori decision was made not to analyze the data from the first "warm-up" interview and not to have a fourth interview. The "warm-up" job candidate was always white. Half the subjects then interviewed a black followed by a white applicant; the other half interviewed a white then a black candidate. After completion of the third interview, subjects were told that the fourth applicant had called to cancel his appointment. After the third interview, subjects were paid and debriefed.

Applicant Performance

Confederate-applicants were trained to act in a standard way to all interviewers. First, they devised answers to the 15 questions such that their answers, though not identical, would represent equally qualifying answers. Confederates then rehearsed these answers until two judges rated their performances to be equal. Confederates were also trained to seat themselves, shoulders parallel to the backs of their chairs (10° from vertical) and to make eye contact with the interviewer 50% of the time. A code was devised to signal confederates during their interviews if they deviated from the pose or began to reciprocate the gestures or head nods given them.

Dependent Measures

Immediacy behaviors. Following Mehrabian (1968, 1969), four indices of psychological immediacy were assessed: (1) Physical Distance between interviewer and interviewee, measured in inches; (2) Forward Lean, scored in 10° units, with zero representing the vertical position and positive scores representing the torso leaning toward the confederate; (3) Eye Contact, defined as the proportion of time the subject looked directly at the confederate's eyes; and (4) Shoulder Orientation, scored in units of 10°, with zero representing the subject's shoulders parallel to those of the confederate and positive scores indicating a shift in either direction. Two judges,[1] placed behind one-way mirrors, scored the immediacy behaviors.

More distance and shoulder angle represent less immediate behaviors while more forward lean and more eye contact represent more immediate behaviors. An index of total immediacy was constructed by summing the four measures, standardized, and weighted according to the regression equation beta weights established by Mehrabian (1969). Final scores of this index represent (−.6) distance + (.3) forward lean + (.3) eye contact + (−.1) shoulder orientation. Positive scores represent more immediate performances.

Related behaviors. Two related behaviors, which indicate differential evaluations of the applicants (cf. Mehrabian, 1969), were also assessed: (1) Interview length indicates the amount of time from the point the subject entered the interview room until he announced the interview was over, in minutes. This measure was taken by the experimenter. (2) Speech Error Rate, scored by two additional judges from audiotapes, represents the sum of (a) sentence changes, (b) repetitions, (c) stutters, (d) sentence incompletions, and (e) intruding, incoherent sounds divided by the length of the interview and averaged over the two judges. Higher scores represent more speech errors per minute.

RESULTS

Reliabilities and Order Effects

Reliabilities, obtained by correlating the judges' ratings, ranged from .60 to .90 (see Table 27.1). Preliminary analyses also indicated that there were no effects for the order in which confederate-applicants appeared, so that the results are based on data collapsed across this variable.

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[1]All judges employed in the present research were Princeton undergraduates. Each worked independently and was naive concerning the hypothesis under investigation. Intensive probing indicated that they did not become aware of the hypothesis.

TABLE 27.1
Mean interviewer behavior as a function of race of job applicant; Experiment I

Behavior	Reliability	Blacks	Whites	t^b	p
Total immediacy[a]	—	−.11	.38	2.79	<.02
Distance	.90	62.29 inches	58.43 inches	2.36	<.05
Forward lean	.68	−8.76 degrees	−6.12 degrees	1.09	n.s.
Eye contact	.80	62.71%	61.46%	<1	n.s.
Shoulder orientation	.60	22.46 degrees	23.08 degrees	<1	n.s.
Related behaviors					
Interview length	—	9.42 min.	12.77 min.	3.22	<.01
Speech error rate	.88	3.54 errors/min.	2.37 errors/min.	2.43	<.05

[a]See text for weighing formula, from Mehrabian (1969).
[b]*t* test for correlated samples was employed.

Immediacy Behaviors

The results, presented in Table 27.1, indicate that, overall, black job candidates received less immediate behaviors than white applicants ($t = 2.79$; $df = 13$; $p < .02$). On the average, blacks received a negative total immediacy score; whites received a positive one. This overall difference is primarily due to the fact that the white interviewers physically placed themselves further from black than white applicants ($t = 2.36$; $df = 13$; $p < .05$). None of the other indices of immediacy showed reliable differences when considered separately.

Related Behaviors

The results for interview length and speech error rate are also presented in Table 27.1. Here it can be seen that blacks also received less immediate behaviors. White interviewers spent 25% less time ($t = 3.22$; $df = 13$; $p < .01$) and had higher rates of speech errors ($t = 2.43$; $df = 13$; $p < .05$) with black job candidates than white job candidates.

The results of the first experiment provide support for the hypothesis that black, as compared with white, job applicants receive less immediate nonverbal communications from white job interviewers. Indirectly the results also provide support for the conceptualization of blackness as a stigmatizing trait. The differences in time (evidenced by 12 of 14 interviewers), in total immediacy (evidenced by 10 of 14 interviewers), and in speech error rate (evidenced by 11 of 14 interviewers) argues for an extension of the stigma effect obtained by Kleck and his associates to include black Americans.

EXPERIMENT 2

METHOD

Overview

A second experiment was conducted to ascertain what effect the differences black and white applicants received in Experiment I would have on an applicant's job interview performance. In the context of training job interviewers, subject-applicants were interviewed by confederate-interviewers under one of two conditions. In the Immediate condition, as compared with the Nonimmediate condition, interviewers (1) sat closer to the applicant, (2) made fewer speech errors per minute, and (3) actually took longer to give their interviews. The main dependent measures were concerned with the interview performance of the applicant, both in terms of its judged adequacy for obtaining the job and in terms of its reciprocation of immediacy behaviors.

Subjects (Job Applicants) and Confederates (Interviewers)

Thirty white male Princeton University students were recruited ostensibly to help Career Services train interviewers for an upcoming summer job operation. No subjects were eliminated from the study, leaving an *n* of 15 in each condition. The two confederate-interviewers were also white male Princeton students.

Procedure

Upon arrival each subject was given an instruction sheet that informed him that Career Services had contracted with the Psychology Department to train Princeton juniors and seniors in the techniques of job interviewing and that one of the techniques chosen included videotaping interviewers with job applicants for feedback purposes. The subject was then asked to simulate a job applicant, to be honest, and to really compete for the job, so as to give the interviewer real, lifelike practice. To make the simulation more meaningful, subjects were also informed that the applicant chosen from five interviewed that evening would receive an additional $1.50.

Subjects were taken to the interview room and asked to be seated in a large swivel chair, while the Experimenter turned on the camera. The confederate-interviewer then entered, and assumed either an immediate or nonimmediate position which will be described in more detail below. Exactly five minutes into the interviewing in both conditions, a guise was developed that resulted in the experimenter having to reclaim the chair in which the subject was sitting. The subject

was then asked to take a folding chair leaning against the wall and to continue the interview. The distance from the interviewer at which the subject placed his new chair was one of the study's dependent measures designed to assess reciprocated immediacy.

When the interview ended, the experimenter took the subject to another room where a second investigator, blind as to the condition of the subject, administered self-report scales and answered any questions. The subject was then paid and debriefed.

Immediacy Manipulation

As in the Kleck and Nuessle (1968) and the Jones and Cooper (1971) studies, systematic nonverbal variations were introduced by specifically training confederates. Two confederate-interviewers alternated in the two conditions. In the Immediate condition, confederates sat at a chair on the side of a table. In the Nonimmediate condition, confederates sat fully behind the table. The difference in distance from the subject's chair was about four inches, representing the mean difference in distance white interviewers gave black and white applicants in Experiment 1.[2]

In addition, the confederate-interviewers in the Immediate condition were trained to behave as precisely as possible like the subject-interviewers in Experiment 1 had acted toward white applicants. In the Nonimmediate condition, interviewers were trained to act as subject-interviewers had acted toward Blacks in Experiment 1. The factors used to simulate the immediacy behaviors found in the first experiment were speech error rate, length of interview and, as has been previously mentioned, physical distance. Eye contact, shoulder orientation and forward lean did not show significant differences in Experiment I and thus were held constant in Experiment 2 (with the levels set at 50% eye contact, 0° shoulder orientation and 20° forward lean).

Dependent Measures

Three classes of dependent variables were collected: (1) judges' ratings of interview performance; (2) judges' ratings of reciprocated immediacy behaviors; and (3) subjects' ratings of their postinterview mood state and attitudes toward the interviewer.

Applicant performance. Applicant interview performance and demeanor were rated by a panel of two judges from videotapes of the interviews. The videotapes were recorded at such an angle that judges viewed only the applicant, not the

[2]By having the interviewer sit either behind or at the side of the table, the impact of the four-inch difference in distance was intentionally maximized in terms of psychological immediacy.

confederate-interviewer. The judges were merely instructed about the type of job subjects were applying for, and were asked to rate (1) the overall adequacy of each subject's performance and (2) each subject's composure on five (0–4) point scales. High scores, averaged over the judges, represent more adequate and more calm, relaxed performances, respectively.

Reciprocated immediacy behaviors. Two additional judges, placed behind one-way mirrors as in Experiment 1, recorded subjects' forward lean, eye contact, and shoulder orientation in accordance with the procedures established by Mehrabian (1969). Distance was directly measured after each interview, and represents the distance, in inches, from the middle of the interviewer-confederate's chair to the middle of the subject's chair, after the interruption. Speech errors were scored by another panel of two judges from audiotapes of the interviews, also according to Mehrabian's (1969) procedures. High scores represent more speech errors per minute.

Applicant mood and attitude toward the interviewer. After the interview, subjects filled out a series of questionnaires designed to assess their mood state and their attitudes toward the interviewer. Following Jones and Cooper (1971), subjects' moods were expected to vary as a function of immediacy conditions. The mood scale adapted from that study was employed. It consisted of six polar adjectives (e.g., happy-sad) separated by seven-point scales. Subjects were asked to respond to each pair according to "the way you feel about yourself."

Two measures of subjects' attitudes toward the interviewer were collected. First, subjects were asked to rate the friendliness of the interviewer on an 11-point scale, with zero representing an "unfriendly" and 10 representing a "friendly" interviewer, respectively. Second, to assess subjects' attitudes concerning the adequacy of the interviewer as an individual, they were asked to check the six adjectives best describing their interviewer from a list of 16 drawn from Gough's Adjective Checklist. Final scores represent the number of positive adjectives chosen minus the number of negative adjectives checked.

RESULTS

Reliabilities and Interviewer Effects

Reliabilities, obtained by correlating judges' ratings, ranged from .66 to .86 (see Table 27.2). Preliminary analyses also indicated that there were no effects for interviewers, so that the results presented are based on data collapsed across this variable.

TABLE 27.2
Mean applicant responses under two conditions of interviewer immediacy; Experiment 2

Response	Reliability	Nonimmediate	Immediate	*F*	P
Applicant performance					
Rated performance	.66	1.44	2.22	7.96	<.01
Rated demeanor	.86	1.62	3.02	16.46	<.001
Immediacy behaviors					
Distance	—	72.73 inches	56.93 inches	9.19	<.01
Speech error rate	.74	5.01 errors/min.	3.33 errors/min.	3.40	<.10
Self reported mood and attitudes					
Mood	—	3.77	5.97	1.34	n.s.
Interviewer friendliness	—	4.33	6.60	22.91	<.001
Interviewer adequacy	—	−1.07	1.53	8.64	<.01

Applicant Performance

It was predicted from an analysis of the communicative functions of nonimmediacy that applicants would be adversely affected by the receipt of nonimmediate communications. Those effects were expected to manifest themselves in less adequate job-interview performances.

Subjects in the two conditions were rated by two judges from video-tapes. The main dependent measure, applicant adequacy for the job, showed striking differences as a function of immediacy conditions (see Table 27.2). Subjects in the Nonimmediate condition were judged significantly less adequate for the job ($F = 7.96$; $df = 1/28$; $p < .01$). Subjects in the Nonimmediate condition were also judged to be reliably less calm and composed ($F = 16.96$; $df = 1/28$; $p < .001$).

Reciprocated Immediacy Behaviors

Following Rosenfeld (1967) among others, it was expected that subjects encountering less immediate communications would reciprocate with less immediate behaviors of their own. This expectation was supported by both the measures of physical distance and speech error rate (see Table 27.2).

Subjects in the Immediate condition, on the average, placed their chairs eight inches closer to the interviewer after their initial chair was removed; subjects in the Nonimmediate conditions placed their chairs four inches farther away from their interviewer. The mean difference between the two groups was highly significant ($F = 9.19$; $df = 1/28$; $p < .01$).

As in Experiment 1 mean comparisons for the forward lean, eye contact, and shoulder orientation measures of immediacy did not reach significance. The combination of these measures, using the weighting formula devised by Mehrabian

(1969), however, was reliably different (means of −.29 and .29 in the Nonimmediate and Immediate conditions, respectively; $F = 5.44$; $df = 1/28$; $p < .05$).

The rate at which subjects made speech errors also tended to be reciprocated with subjects in the Nonimmediate condition exhibiting a higher rate than subjects in the Immediate condition ($F = 3.40$; $df = 1/28$; $p < .10$).

Applicant Mood and Attitude Toward the Interviewer

It was expected that subjects receiving less immediate (i.e., less positive) communication would (1) feel less positively after their interviews, and (2) hold less positive attitudes toward the interviewer himself. These expectations were only partially supported (see Table 27.2). Although subjects in the Nonimmediate condition reported less positive moods than subjects in the Immediate condition, this difference was not statistically reliable.

Subjects in the less immediate condition did, however, rate their interviewers to be less friendly ($F = 22.91$; $df = 1/28$; $p < .001$) and less adequate overall ($F = 8.64$; $df = 1/28$; $p < .01$) than subjects in the more immediate condition.

DISCUSSION

Results from the two experiments provide clear support for the two subhypotheses, and offer inferential evidence for the general notion that self-fulfilling prophecies can and do occur in interracial interactions.

The results of Experiment 1 indicated that black applicants were, in fact, treated to less immediacy than their white counterparts. Goffman's (1963) conception of blackness as a stigmatizing trait in Anglo-American society is, thus, given experimental support—insofar as that classification predicts avoidance behaviors in interactions with normals. These results may also be viewed as extending the stigma effect documented by Kleck and his associates with handicapped persons.

That the differential treatment black and white applicants received in Experiment 1 can influence the performance and attitudes of job candidates was clearly demonstrated in Experiment 2. In that experiment those applicants, treated similarly to the way black applicants were treated in Experiment 1, performed less well, reciprocated less immediacy, and found their interviewers to be less adequate. Taken together the two experiments provide evidence for the assertion that nonverbal, immediacy cues mediate, in part, the performance of an applicant in a job interview situation. Further, the experiments suggest that the model of a self-fulfilling prophecy, mediated by nonverbal cues, (1) is applicable to this setting, and (2) can account, in part, for the less adequate performances of black applicants (cf. Sattler, 1970).

Social scientists have often tended to focus their attention for such phenomena as unemployment in black communities on the dispositions of the disinherited. Such an approach has been termed "victim analysis" for its preoccupation with the wounds, defects and personalities of the victimized as an explanation for social problems (Ryan, 1971). The present results suggest that analyses of black-white interactions, particularly in the area of job-seeking black applicants in white society, might profit if it were assumed that the "problem" of black performance resides not entirely within the black people, but rather within the interaction setting itself.

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28

Low Numbers: Stereotypes and the Underrepresentation of Women in Math and Science

Joshua Aronson

Not long after Harvard president Lawrence Summers was publicly excoriated and ultimately removed from office for sharing his thoughts about men being naturally better scientists and mathematicians than women, I found myself invited to sit on various panels convened to "debate the scientific evidence" on gender differences in math and science, specifically to present my research on "stereotype threat," which is the psychological predicament women and other minority group members face when confronted by cultural stereotypes alleging lower intellectual ability. Nearly all of these symposia were alike in their titles, which were questions, like this:

Are Women Being Held Back in Math and Science?

Female Under-Representation in Science: Nature or Nurture?

Was Larry Summers Right about Sex Differences in Math and Science?

As provocative as such questions are, they have the unfortunate tendency of eliciting answers that are either wrong or ultimately of little use. Indeed, even the notion of a "debate" on the scientific issues presumes that the number of positions one can take on a given issue is two, and thus leads us to ignore the middle ground, the complex ways nature and nurture interact. Judging from the apparent number of changed minds at the end of these debates—approximately zero—it is reason-

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able to ask whether the way we debate science may actually slow progress toward narrowing group differences and increasing the ranks of women in the highest levels of math and science.

It doesn't help matters that such discussions nearly always are conducted in the wake of a public crucifixion of the Larry Summers variety. At this writing, the most recent notable to be brought down for racial insensitivity is the Nobel Laureate James Watson, who, in attributing Africa's problems to the lower innate intelligence of Africans, found his reputation, his book tour, and his job directing a research center all destroyed by a few poorly chosen words. One need not agree with the Summerses or Watsons of the world to experience anguish at their professional demise, to cringe at the bigotry on both sides of the nature/nurture divide, and to shudder at the thinness of the political ice that surrounds us all, just waiting for a slip of the tongue.

If these debates have convinced me of anything, it is that the most malign influence is neither "IQ fundamentalism"[1] on the one hand, nor "political correctness," on the other. It's the *overconfidence* that so often accompanies either of these extremes. Whether it is expressed by a women's rights activist's fleeing the room at the mention of "biological differences" or the sneering hostility of a *Wall Street Journal* editorial extolling them, it is the overconfidence and dig-in-the-heels-and-say-anything-to-win mindset that keeps us debating rather than learning from the science on gender and race differences.

Although neither sneering nor shrill, the recent National Academy of Sciences report, *Beyond Bias and Barriers: Fulfilling the Potential of Women in Academic Science and Engineering,* is nonetheless a bit too confident about the barriers holding women back.[2] Its central argument is that girls and women are impaired by both conscious and unconscious biases that stem from cultural notions about female inferiority in math and science, and by processes such as "stereotype threat," in which one's own performance and motivation are spoiled by worries about living down to the low expectations these cultural notions impose. Although it is clear that gender bias exists and that stereotype threat—which I will describe in detail in this chapter—can significantly impede intellectual performance and development, the confidence that they play a big role in holding women back is unwarranted. Despite the admirable intentions of the NAS report, its authors are simply too confident that biological differences are not involved and that bias is. If we are true to the scientific process, we need to be clear on the fact that we do not know to what extent bias, stereotype threat, or other social factors contribute to the low numbers of women in math and science. Nor can we be confident that these processes are not involved.

The problem is that, unlike years of education, annual yearly income, or even IQ, constructs like gender bias and stereotype threat defy easy quantification; they cannot be easily entered into multiple regression analyses to calculate the percentage of variance explained in the gender gap—not in any terribly convincing way, at least. By the same token, we cannot confidently say how much biological "femaleness"

matters in the gender gap, either. Thus, we are left knowing that biology and socialization matter, but how much each factor matters remains unclear.

This is precisely the conclusion reached by the psychologists Stephen Ceci and Wendy Williams in their recent book, *Why Aren't More Women in Science?*[3] After considering the research and viewpoints of the nation's sex-difference experts, they admit being unable to offer any firm conclusions about the relative roles played by nature and nurture in the low numbers of women in science. They further point out how the vast mosaic of findings that comprises the body of research on gender differences provides the scientific community with its own Rorschach test, affording diametrically opposed conclusions to be drawn—often from the same data—presumably depending upon one's implicit theories about nature and nurture, or upon one's political ideology. If we are rational and honest, the only reasonable response to such a mixed bag of scientific evidence like this is to get less confident, less extreme, and to become more tentative, to recognize that there are no simple answers. But studies of partisans reveal that people are often far more rationalizing than they are rational; and so mixed evidence has the paradoxical effect of further polarizing attitudes, leaving partisans ever more confident that they were right all along, and that those on the other side of the argument must be terribly biased. This is yet another reason these debates can be a waste of time.

As an applied social developmental psychologist, my read of the group-differences inkblot is greatly influenced by my goals as a researcher—which are not to focus on the question of *how much* group differences in achievement are caused by biology or socialization but, rather, on how an understanding of human psychology can help improve learning, motivation, performance, and intellectual development. This perspective renders the how-much-nature-how-much-nurture question irrelevant. Neither denying biological differences nor emphasizing them, I ask instead the practical question of whether differences among groups can be narrowed, and if so, how. Stereotype threat, a theory that I've been fortunate to help develop with my mentor, Claude Steele, and others, has been a fruitful vehicle for addressing these questions.

STEREOTYPE THREAT

A psychologist friend of mine spent a day the White House some time ago to participate in a conference on programs to help inner-city children. President George W. Bush kicked off the proceedings with some introductory remarks. To my friend's surprise, the president spoke for nearly fifteen minutes without notes or teleprompter—and was articulate, fluent, and engaging. He was also genuinely witty and, at moments, even brilliant. In other words, he was utterly different from the person we have come to know from television, whose verbal gaffes fill the pages of such books as *Bushisms* and *The Bush Dyslexicon*, and whose behavior and speech have earned him derogatory nicknames and inspired countless late-

night talk show jokes riffing on his assumed stupidity. My friend's experience with a surprisingly intelligent Bush was not an anomaly. After viewing a 1994 video of Bush's gubernatorial debate with Texas governor Ann Richards, a similarly astonished James Fallows wrote,

> This Bush was eloquent. He spoke quickly and easily. He rattled off complicated sentences and brought them to the right grammatical conclusions. He mishandled a word or two ("million" when he clearly meant "billion"; "stole" when he meant "sold"), but fewer than most people would in an hour's debate. More striking, he did not pause before forcing out big words, as he so often does now, or invent mangled new ones.[4]

Many commentators, both Bush friends and Bush foes, have said that, off camera, he is far smarter and more articulate than people realize. Bush's SAT score, it turns out, places him in the same IQ range as John F. Kennedy, and higher than that of Bill Bradley and John Kerry. Stories friends tell of Bush's college days portray him as neither intellectually curious nor engaged, but reveal him as capable of stunning feats of intelligence—for example, being able to remember the names of a roomful of fifty fraternity brothers after hearing their names announced only once, something none of the other pledges could come close to doing. None of this means, of course, that Bush is an intellectual powerhouse whenever he's not on the spot, but it does raise an important question: What accounts for the fact that people can be demonstrably smart under some circumstances and yet apparently much stupider in others? What renders a person unable to demonstrate knowledge he or she can be assumed to have—say, on a test or during a speech? And what, if anything, might this have to do with racial and gender gaps in test scores?

The stock in trade of social psychology is to demonstrate that social context can exert powerful effects on human behavior and psychological functioning, overwhelming what we consider to be essential, defining personality characteristics. Hundreds of experiments demonstrate that the same individuals can act very differently—more competitively, more aggressively, more kindly—than their personality profiles or social reputations indicate, depending often upon small details of the social situation, the relationships among the actors involved, and the interpretations the actors draw about the context and the people in it.[5] Thus, "honest" students in one context have been shown to lie, cheat, and steal in another; a girl who disobeys her parents and dominates her younger brother in the home is shy, retiring, and obedient in school; the brilliant academic, adored by his students for his wit and charm in the classroom or on television, is awkward and boring in the presence of his mother-in-law—and so on. We've all experienced this, both as spectators of others' variability and as actors noticing our own. The research on stereotype threat simply extends this logic to the domain of human intellectual performance, and suggests that the predicament that has created difficulties for George Bush—a reputation for being ignorant and inarticulate that became widespread after he botched an on-air foreign policy quiz in 1999—can also operate at a

group level, creating difficulties for individuals belonging to groups traditionally considered intellectually inferior. Human intellectual behavior is more fragile and malleable than we tend to realize, and what makes it fragile is that it is transacted within a web of social forces. These forces are powerful yet mostly invisible, and, thus, when we evaluate the performances of presidents, college students, or fourth graders, we typically do a bad job of factoring them into our judgments of how smart people are.

Claude Steele and I tested the hypothesis that black college students may experience considerable apprehension when they are called upon to demonstrate their intelligence. In such situations (taking a test, speaking up in class, discussing a difficult concept with a fellow student, and so on), they face a predicament brought on by their awareness of the stereotype of African-American intellectual inferiority and their presumption that others may use the stereotype to evaluate them. Depending on the details of the situation, they may feel especially at risk of living up to the stereotype—of conforming to the image of the stereotypical stupid black person—and thus being devalued in the eyes of those around them. We further reasoned that the black student may even experience stereotype threat when no one else is watching, because the stereotype stands ready in his subconscious to explain any experienced difficulty performing an intellectual task. Such feelings could arouse anxiety, which, on complicated intellectual tasks, is known to disrupt performance, divide attention, and otherwise create an extra layer of unpleasantness not experienced by individuals for whom the stereotype does not apply. If this is true, we reasoned, perhaps it could help explain some part of the test-score gap—the portion that consistently remains after accounting for such factors as parents' level of intelligence, education, wealth, and the other "usual suspects" to which people customarily attribute group differences.[6]

We first tested these ideas with a series of laboratory experiments, in which African-American and white college students took a difficult verbal reasoning test. Half of the test-takers were given the test under normal testing conditions, with the test presented as a measure of verbal ability. For the remaining test-takers, we altered the test-taking situation, taking pains to assure the students that our purpose in having them solve the verbal items was not to measure their intelligence but, rather, to examine the psychology of problem-solving. The test performance results were quite striking. African Americans led to believe we were not interested in measuring their intelligence performed significantly better than those in the control condition. The difference in instructions had no appreciable effect on the white test-takers' performance.

Because black students were scarce at the university, we were forced to work with small numbers, and, thus, despite a very strong effect, we needed to use a statistical correction to move from marginal to conventional statistical significance. Thus, we employed a commonly used statistical correction for test-takers' prior verbal scores on the SAT that adjusts the performances in a way that allows us to treat the students, who varied in their SAT scores, as though they had equivalent

scores, thus putting us in a better position to disentangle the effects of experimental conditions from differences in test-taking ability and preparation. This correction is standard practice in such situations. Nonetheless, it has raised questions that have had the unfortunate effect of distracting some people from the essential message of this work.

The message, again, is this: Intellectual performance is more fragile and malleable than customarily thought, and attending to the psychological context—what it feels like to be an African American taking an IQ test, a woman taking a math test, or George W. Bush making a speech with the television cameras rolling—can help us understand and improve performance and learning among individuals confronted by negative stereotypes or personal reputations. No matter how one analyzes the data, the conclusion is the same: We boosted African Americans' performance by deemphasizing an analysis of their ability, thereby making the situation psychologically safer and more conducive to intelligent thought. As one can see in Figure 28.1, correcting for SAT or not correcting for SAT does not change this conclusion.

In the wake of the publication of this study in 1995, a good number of similarly constructed experiments (some three hundred at last count) have been conducted with targets of other group stereotypes—women taking mathematics tests, Latinos taking verbal tests, students of low socioeconomic status taking verbal tests, elderly individuals performing short-term memory tasks, women taking tests of political knowledge, chess, and driving, and even African Americans taking tests of miniature golf![7] In addition to supporting the findings of the original study, some of these experiments also establish important insights about the nature of stereotype threat. Most important, perhaps, is the fact that one need not be regularly or persistently targeted by ability stereotypes to be affected by them. For example, my colleagues and I found that white males with very high math abilities can be led to choke on a test by informing them that their scores would be used to evaluate their ability relative to Asian test-takers.[8] Studies like this underscore the power of stereotypes to suppress intelligent thought and performance.

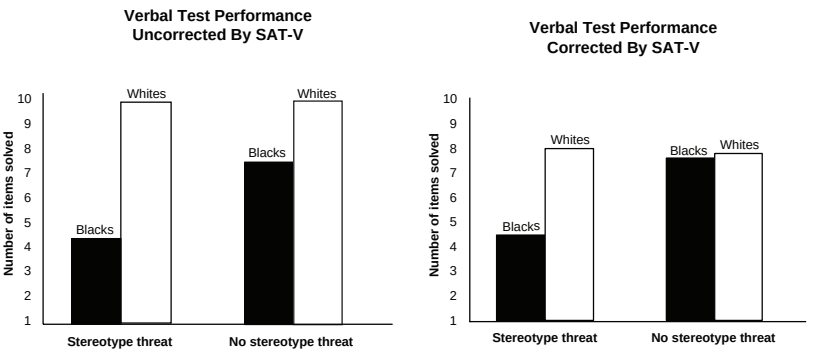

FIGURE 28.1
Verbal test performance uncorrected by SAT-V vs. verbal test performance corrected by SAT-V.
Source: Test performance results from Steele and Aronson 1995.

Reducing stereotype threat can improve performances that draw upon abilities thought to be mediated by hormones. For example, mental rotation—the ability to mentally rotate representations of two-dimensional or three-dimensional objects—has long been shown to produce robust gender gaps favoring males at all ages and has been linked in some studies to the amount of testosterone in the body.[9] This suggests a hard-wired sex difference in abilities and interests in spatial abilities with implications that range from a child's preference for playing with LEGOs over dolls, to performing well on geometry tests in high school, to success in engineering classes in college, all of which could help explain the low numbers of women in "hard" math and science careers. And yet Matthew McGlone and I found that we could significantly influence mental rotation performance on the Vandenberg Mental Rotation Test (a common measure of spatial ability) simply by influencing what test-takers thought about prior to the test. In our study, we gave male and female college students one of three mindsets. To get some of the test-takers thinking about their gender, we asked them to tell us a few things a student might like about living in a coed dorm. We compared this to a control condition in which they were asked to tell us a few things a student might like about living in the Northeast (where their college was located). As can be seen in Figure 28.2, men performed better and women worse than controls when led to think about their gender. But we almost completely eliminated the gender difference in a third condition by getting them to think about a positive achieved identity—we asked them what someone might like about being a student at a "highly selective liberal arts college." Thus, psychological mindsets can overcome even robust sex differences in performance that are often attributed to biology.

In each case, the message of these studies is the same: Reduce stereotype threat and performance improves; induce it, and performance suffers. Many studies of this sort have also examined important individual differences in what I call "stereotype vulnerability." Do students think that the stereotypes are true? Do they expect to face lots of discrimination? Do they care a great deal about academic success in

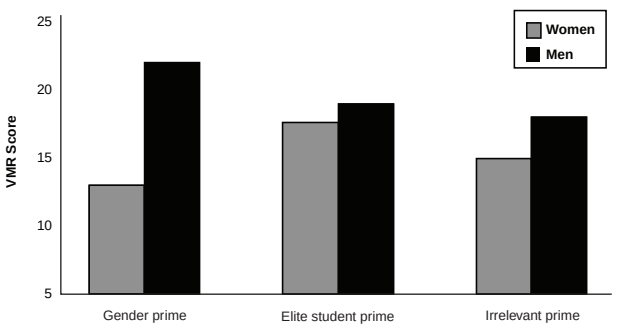

FIGURE 28.2
Identity Salience Influences Mental Rotation Performance
Source: Vandenberg Mental Rotation Scores from McGlone and Aronson 2006.

a domain? Do they feel a strong connection to their racial or gender identity? Do they believe intelligence is essentially fixed and cannot develop? Students who answer yes to these questions tend to be more vulnerable to stereotype threat.

There is a fundamental point that must be stressed about such experiments—one that any student who has taken a course in experimentation understands, but is often lost on people not trained in social science. Laboratory experiments—either one or a group of three hundred—can tell us nothing about the degree to which a phenomenon occurs in the real world, or, indeed, if it occurs at all. This is not what experiments are designed to do. They simply tell us about average responses to conditions created in the experiment—how, say, a typical person processes information when he believes his intelligence is being measured. If people respond as we predict they will, we know that our theory of human information processing is correct, at least under the conditions we have specified and with the population we have sampled. If we are wrong, we revise our theories accordingly. The experiments on stereotype threat therefore cannot tell us *how much of* the race gap or the gender gap in achievement in the real world can be attributed to stereotype threat. The laboratory experiment is simply not designed to do this, any more than a hammer is designed to saw wood, turn screws, or apply paint.

This limitation, of course, applies to any experimental evidence under consideration in sex differences research. For example, Simon Baron-Cohen and his colleagues find that male newborns are particularly drawn to spatial objects—a mobile with parts that move mechanically—whereas female newborns are particularly interested in human faces.[10] These findings clearly establish, to my mind, a critical difference that stems from biology and is relevant to spatial abilities and, thus, math performance. But how much of the gender gap in math, or the low numbers of women who will later choose a career studying "things" (physics, computer science, or engineering) versus one oriented toward "people" (biology, sociology, or politics) can be attributed to these inborn differences? The data provide no information about how much of the gap is nature or how much later socialization will enter and change the picture—or any other sort of *how much* question about the gender gap in the real world. What they *do* tell us is this: Any theory attributing differences in interest in spatial tasks uniquely to socialization is either wrong or in need of revision, because these differences show up before any socialization can occur. That's the kind of work experiments do. They test theories about the way human minds work.

Stereotype threat experiments tell us, among other things, that contexts that arouse stereotype threat can have an impact on test scores. They also reveal the psychological processes involved, such as how the cognitive activation of stereotypes accompanies impaired performance and working memory capacity, as well as the activation of brain regions associated with social and physical threats.[11] They further suggest the conditions and mind-sets that can reduce stereotype threat, such as being exposed to a role model—a female or black test administrator, for example—whose mere presence and assumed expertise can counter stereotype threat and raise the test scores of females and blacks taking the test.[12] Above all, at

a most fundamental level, these experiments tell us that any theory holding that tests measure only innate intelligence or academic preparation are either wrong or in need of revision. It is the *understanding* gained from these experimental findings—namely, that intellectual performance is subject to social influences like stereotype threat—which we apply to the real world, not the exact experimental results themselves. Our understanding tells us that IQ and effort are not the only things that matter in test performance. Psychology matters, too. We think this is a useful thing to know.

STEREOTYPE THREAT IN THE REAL WORLD

To find out if and how stereotype threat plays a role in the real-world achievement of women and minority students, research must be conducted that tests these understandings of human performance in schools, in standardized testing centers, and in college classrooms. A number of studies have done just that. To find out how much of the gap in grade point average (GPA) may be attributed to stereotype threat, one approach is to measure stereotype vulnerability in students and predict their college performance over time. For example, Douglas Massey and Mary Fischer conducted a longitudinal survey of over four thousand freshmen from different ethnic backgrounds, attending more than twenty-eight American colleges.[13] Students were surveyed at the beginning of their college careers, and their performance was monitored thereafter. Large differences in GPA were found among ethnic groups; Asians and whites outperformed blacks and Latinos, even when controlling for SAT scores, family income, and other important background factors. The degree of measured stereotype vulnerability predicted 9 to 10 percent of their variation in grades, and, when accounted for in the predictive model along with the other background factors, explained the entire performance gap. In other words, stereotype threat explained the unexplained portion of the gap that was left over once we accounted for high school grades, family background, and prior measures of ability and preparation.

A second approach to examining the role of stereotype threat outside the laboratory involves asking whether women in the pipeline to math and science careers experience it. Students at the high end of the mathematics bell curve are frequently assumed to be immune to stereotype threat. To test this, Catherine Good, Jayne Anne Harder, and I examined the performances of women and men in the highest-level calculus classes offered at the University of Texas.[14] All of the men and women in the class were informed by the professor that the test, like all of the tests in the course, was a measure of their calculus abilities and knowledge, but half were further informed that the test had never shown gender differences in the past, a framing of the test that had been shown to reduce stereotype threat in laboratory studies. In the control condition, where stereotype threat was not nullified, the women and men performed equally well, which is not surprising given that, on av-

erage, these women's grades were equal to the men's. But when stereotype threat was reduced by the statement about gender bias, the women performed significantly better than the men. Thus, stereotype threat appears to operate in high-level classes that future scientists must pass, apparently suppressing women's performances even on in-class tests.

A number of studies have gotten at the real-world applicability of the theory by conducting interventions based on the hypothesis that reducing feelings of stereotype threat in classrooms should increase grades and test scores. For example, Geoffrey Cohen and his colleagues, employing means that would cost a school system very little to implement, reduced the black–white grade gap among middle school students by 40 percent.[15] Cohen and colleagues did this by having students complete an in-class writing assignment that affirmed their most cherished values, an intervention shown in numerous laboratory studies to reduce or eliminate the effects of psychological threats by creating a sense of psychological safety. Catherine Good, Michael Inzlicht, and I conducted another theory-based intervention among middle school children and examined its effects on their statewide exit exam scores in reading and math.[16] The intervention involved having older students share their wisdom by teaching one group of younger students that human intelligence can grow with effort, and another group that most students improve their scores and grades once they get the hang of middle school. Both of these interventions, which had been shown to reduce test anxiety in the lab, significantly improved Latinos' reading scores and completely eliminated the gender gap in math test scores by improving the performance of the girls.

Considered together, the Cohen intervention and the Massey and Fisher longitudinal study of college students underscore a vital point—another that is missed by nonscientists: Even if stereotype threat accounts for, say, 9 percent of the achievement gap between two groups, interventions that reduce it can initiate a process that closes the gap to a much greater degree (for example, 40 percent). Reducing stereotype threat, in other words, may create the conditions that allow students to engage more deeply with their academic work and learn better, which, in turn engages other processes, such as encouragement and praise from parents and teachers, more adaptive responses to critical feedback,[17] peer respect, and the love of learning— processes that have recursive, spiraling effects on achievement. For this reason, insisting, as some people have, on answering the how-much-of-the-gap question before doing anything about it strikes me as a misguided obsession. Knowing that factor x causes 10 percent of a problem does not force the conclusion that intervening upon that factor can produce an improvement of only 10 percent. In the case of stereotype threat and academic achievement, the size of the cause appears not to be isomorphic with the impact of intervening on that cause. It also means that what looks like a "quick fix" of reducing threat can open the door to "slower-fix processes," like greater willingness to study, an increased sense of valuing and enjoying learning, and better responses to feedback, all of which have positive effects of their own.

Such studies make me optimistic that gender and race gaps can be considerably narrowed, and that doing so requires neither massive nor expensive interventions. Small, timely, intelligently designed interventions can be as effective as, or even more effective than, the massive but ill-conceived interventions that are so often proposed. The failure of No Child Left Behind (NCLB) to reach its goals is a testament to the fact that interventions that misunderstand or ignore the psychology of human motivation are unlikely to work well, no matter how much legislative muscle they may have behind them. Charles Murray recently interpreted the failure of NCLB as evidence that essentially nothing can be done to reduce the achievement gap.[18] I read the failure very differently—not as proof of the intractability of the gap, but as a repudiation of the higher-test-scores-or-else approach to improving learning and motivation. Some years ago I predicted NCLB would fail for just this reason,[19] and data from recent large-scale experiments with the California Academic Skills Exit Exam bears this out.[20] The study found that requiring the exam for high school graduation lowered the minority students' performance across the board and girls' performance on the math test. Similarly skilled students performed better on the same exam a year earlier, when the stakes were lower. But just because you cannot force more learning and higher test scores with threats and high pressure does not mean you cannot get them through other, more thoughtful means, and our stereotype-threat–based interventions show this.

"CONTROVERSIAL" ISSUES IN STEREOTYPE THREAT

Since I intend to play hardball in discussing the criticisms of stereotype threat, I should probably go up to bat first. There is no doubt about it: Steele and I made mistakes in our first publication on the test performance of African Americans. First, we overstated the situational side of the case, suggesting that the threat was located primarily "in the air," and not in some combination of person and social context. Our subsequent research quickly departed from this it's-all-in-the-situation stance. After all, reality is partly what people make of it, and therefore our individual personalities and mindsets—those that the culture imposes upon us, those we develop through life experiences, and those we choose—matter a great deal in how and how much we are affected by situations, such as the situations facing the targets of negative ability stereotypes. Still, many achievement situations are so powerful that a random sample of students is likely to do worse on a test when confronted with a stereotype—as in the study of male math whizzes confronted by the stereotype of Asian superiority, and the original Steele and Aronson study itself.[21] But I believe we were so excited about the power of the situation that we justifiably but insufficiently emphasized the theoretical role of individual differences in stereotype threat in that initial paper. A long list of studies illuminating these individual differences has corrected this error.[22]

A second mistake was that, in order to justify using prior SAT scores as a covariate, we made an odd argument, suggesting that stereotype threat probably did not affect students' SAT scores because the SAT was not sufficiently difficult for the Stanford University students included in the study. I no longer believe this. There is simply no theoretical reason why the SAT would be immune to stereotype threat if the test-taker experiences some difficulty with it. A recent meta-analysis of stereotype threat studies conducted in several countries and involving nearly 19,000 test-takers at all levels of ability suggests that on average, Latinos and blacks lose about 40 points on the verbal SAT due to stereotype threat, while women lose about 20 points on the math SAT.[23] In other words, because of stereotype threat, the SAT systematically underestimates the abilities of both women and non-Asian minorities.

Our third mistake, as noted earlier, is that we published our study with a graph of adjusted means that created the illusion that reducing stereotype threat eliminates the racial achievement gap, instead of presenting the unadjusted means as I have done in Figure 1. Given all the misunderstandings this graph produced, I have come to think of that error—which appeared in only the third publication of my career, submitted some fifteen years ago— the same way I think of pictures of me in the 1970s, with my bad haircut, paisley shirts, and bell-bottom jeans: *What was I thinking?* At the same time, I'm comforted by the fact that science is a self-correcting enterprise. The errors and unanswered questions in that early paper have led other scientists to raise important questions and do their own research. And this is just as it should be: One study's flaws are addressed by another's strengths. One reason for the rapid growth of stereotype-threat research is that our errors and loose ends have inspired so many independent laboratories to address the issues raised by our first publication on the subject and to do their own research. This is all for the good; it's how science progresses.

What has been most surprising about a few of the critics of stereotype-threat research is that they have tended to focus almost exclusively on the very first study—the Steele and Aronson paper—in a series of three hundred, ignoring the fact that the theory has been greatly refined and reinforced by a corpus of subsequent research. I cannot think of another case in science, medicine, or technology where an entire body of informative research by independent laboratories around the world is called into question because of loose ends in the first study in a series. It is as if a few if-man-were-meant-to-fly fundamentalists in 1945 were harping about the future of aviation because of complications at Kitty Hawk.

The absurdity of this approach makes sense only when we consider where the criticisms are coming from—namely, nonscientists with an IQ fundamentalist agenda, or scientists connected with the testing industry.[24] Two implications of stereotype threat seem to bother them. First, if scores can be pushed around so easily by changes in the context, then cognitive tests may not be such great measures of intellectual worth. Second, they appear mightily resistant to the idea that

institutions like schools can nurture intellectual development and make students smarter and more productive by attending to the psychology of achievement.

A conservative law professor named Amy Wax best exemplifies the IQ fundamentalist response to this research. Rather than conduct corrective research or sample the entire body of stereotype-threat research, she approaches scientific discussion with incuriosity and arguments more appropriate to a civil suit or an installment of Hannity and Colmes than to sober scientific discourse. In an op-ed piece she cites "grave methodological flaws" and the "lack of evidence" to support stereotype threat.[25] Don't get me wrong; she is right to raise questions about our research. The problem is that she is not interested in the answers—unless they confirm her prior beliefs, which she made clear in print before her public attacks on stereotype threat began:

> Some—perhaps many—people are not smart enough, or interested enough, to solve hard problems that demand the use of algebra and geometry. No amount of "educational reform" can change this.[26]

This is the IQ fundamentalist position on group differences: Nothing can be done to alter what is essentially set in genetic stone. The problem with this position (beyond the overconfidence and incuriosity) is simple: data.

Whether it's James Flynn's consistent finding that every succeeding generation's IQ has risen steadily in most cultures,[27] or the increasing ranks of women succeeding in mathematics and science,[28] or the fact that girls in Belgium and Japan score much higher in math and science than do boys in America,[29] or the fact that children randomly assigned to same-race,[30] same-sex,[31] or high-quality teachers[32] or to smaller classes[33] become measurably smarter and more engaged than their less fortunate peers, or the narrowing black–white test-score gap over several decades,[34] or any of our achievement-boosting interventions conducted under the aegis of stereotype threat—there is simply too much evidence that intellectual ability is shaped by culture and context for a rational social scientist to accept the notion that "no amount of school reform" can change the status quo. Indeed, as should be clear from the intervention research, even a small amount of school reform can do it; it just needs to be intelligently designed.

With IQ fundamentalism as one's working theory, one is forced in the face of contrary evidence either to revise the theory, or to twist or simply ignore the new evidence in order to preserve a sense of reasonableness. Wax has taken the latter approach in her treatment of stereotype threat.

For example, Wax bemoans the lack of evidence showing that reducing stereotype threat would improve the performance of women or minorities in the real world. Yet such evidence is abundant, as is noted above. At her request, I sent Wax articles showing that the grades and test scores of minority students and girls rise dramatically in response to simple interventions that reduce stereotype threat in schools and on statewide standardized exit tests, as well as in high-level college math courses. These were high-quality, randomized, controlled trials published in

top journals—they can be written off neither as laboratory demonstrations nor as artifacts of statistical corrections. Wax made no mention of these promising effects in her critiques of stereotype threat, and, when confronted with the data during our October 2007 debate at the American Enterprise Institute (AEI), she simply changed the subject and moved on to another item in a litany of complaints. This is not how science progresses.

More recently, Charles Murray—a serious social scientist from whom I typically learn a lot and with whom I tend to agree on a great many issues—has dismissed the implications of this body of research as "educational romanticism." But, to maintain such a position, he, too, must overlook data. He writes,

> The problem that gets in the way of this appealing story is that all of the experimental studies have explicitly induced a threat as part of the experiment's protocol. That threat consists of telling the experimental group that they are about to take a test that measures their innate ability. But tests in K–12 education are never presented that way.[35]

All of the experimental studies? In reality, studies exist (of which I made Wax and Murray aware) showing that stereotype threat is the default situation—where no mention is made about innate ability. In these experiments, test scores improve if test-takers are given instructions that reduce naturally occurring stereotype threat—for example, by presenting the test as a measure of an expandable ability, or as gender fair, or as not measuring ability at all. A particularly striking example of this kind of study was conducted by the Educational Testing Service, which found it could reduce some of the stereotype threat in an actual AP calculus test simply by removing the customary request for gender information that precedes the test. This little change significantly boosted the performance of young women taking the calculus exam, and thereby significantly increased the number of women who qualified for AP credit. Indeed, according to one published analysis of the ETS data, simply moving the demographic information to the end of the actual test would "increase the number of women receiving AP calculus credit each year by 4,700."[36]

Wax and Murray are also unmoved by the longitudinal research on thousands of college students conducted by Massey and Fischer,[37] showing that stereotype threat accounts for about 9 percent of the variance in college achievement among minority students. This last point is particularly curious, because among Wax's strongest criticisms of the theory, voiced during her AEI presentation, is the claim that nobody has done any work showing how much of the achievement gap can be attributed to stereotype threat. She said this only minutes after I presented such data.

Finally, and most depressingly, Wax[38] has repeatedly misled the public regarding the nature and meaning of Paul Sackett's theoretical musings about the original Steele and Aronson research. Sackett[39] raises two very important points. First, he found that many media accounts and textbooks and a few journal articles had spoken of our research in a shorthand that implied the entire race gap was caused by and therefore could be eliminated by reducing stereotype threat. This misunderstanding

was surely abetted by our graph showing only the SAT-corrected test scores without presenting the uncorrected scores. If true, this misperception would be unfortunate and indeed regrettable. Yet the rational, scientific approach to a myth is to look really closely at the data—all of them—and not simply generate a countermyth about our research being flawed.

Moreover, despite the alarm raised by Sackett's critique, I have never met, in years of contact with educators and psychology students, anyone who attributes the entire gap to stereotype threat. Professor Wax is the only person I've ever met who thinks that I have claimed this central role for the phenomenon, and repeatedly puts such claims in the mouths of stereotype-threat researchers.[40] So, I'll be extra clear: *Stereotype threat does not explain the entire race gap.* To think so would require ignoring years of research on important contributing factors.[41] Indeed—and this is why the critiques are so surprising—the existence of stereotype threat does not even imply that there is no genetic or biological basis to group differences in ability; it simply means that among the many factors that contribute to group differences, it is an additional factor, one that appears to be significant, and highly amenable to intervention.

Sackett's second point is that by using an analysis of test scores that controlled for students' prior SAT scores, we possibly obscured the nature of the phenomenon. Specifically, he argues, we might have been showing that by jacking up stereotype threat in the laboratory, we could scare black students into scoring at lower levels than would be reflective of their "true" abilities (that is, as indicated by their SAT scores). But, he reasons, we were not showing that reducing stereotype threat, as we argued, could improve scores relative to the prior SAT scores. I should note that of the hundreds of experiments that confirm stereotype threat, only a handful use this correction.[42]

Nonetheless, Sackett's argument about the Steele and Aronson experiments is a reasonable one. But as compelling as it is, it advances a theoretical point, not an empirical one. In other words, Sackett is speculating about our data, not analyzing it. Our actual data did not support this reasoning; again, as shown in Figure 1, when analyzed with or without the SAT correction, it is clear that black test-takers' scores were improved by assurances that we were not interested in measuring their intelligence. Still, to be certain of this requires a larger experiment than ours, one that presents the test in three ways rather than just two, so that we can determine if presenting the test as a measure of intelligence actually differs, as Sackett claims, from simply presenting it as one normally does—as a test, without adding the notion that we are examining intelligence and thus depressing scores by scaring students.

Ryan Brown and Eric Day conducted just such an experiment with students at the University of Oklahoma, and it shows that our interpretation was the correct one.[43] Brown and Day gave black and white college students the Raven's Progressive Matrices test, a culture-free, nonverbal IQ test that is prized by IQ researchers for being unbiased. One group of students was given the test with the official Raven's instructions (which do not mention intelligence). A second

group (the stereotype-threat group) was told that the test was a measure of their intelligence, and a third (the stereotype-threat-reduced group) was told that the task was simply a puzzle. The results showed identical effects in the first two conditions: Blacks scored significantly worse than whites when they thought that the test was a measure of their intelligence or when the test was given under the standard Raven's instructions. Contrary to Sackett's theory, calling the Raven's a test of intelligence did not produce lower performances than did the standard instructions. As in the Steele and Aronson studies, the framing of the test in these two conditions did not affect the performance of the white students. Moreover, the patterns of data were identical whether or not the scores were adjusted by students' prior test scores. Thus, the argument that our experiment shows only that one can make blacks or women perform worse than their prior test scores by adding an artificial, surplus fear—one that doesn't exist in the real world of standardized testing—was refuted. In the "puzzle" condition, where stereotype threat was minimized, blacks performed dramatically better. These results perfectly confirm the notion that stereotype threat is operative when people take tests under standard testing conditions, and that making the situation less psychologically threatening can improve their test scores relative to their prior tested ability. The astute reader will note that this is just the sort of experiment Charles Murray claims does not exist.

Wax has repeatedly used the Sackett argument to impugn the stereotype-threat research, despite being aware of research that demolishes its underlying premise. Worse, in her *Wall Street Journal* editorial, she pretends that Sackett and colleagues actually formulated their critique of our research with empirical data rather than speculation:

> As noted by University of Minnesota psychologist Paul Sackett and his colleagues in the January issue of *American Psychologist,* the raw, unadjusted scores of African-American and white students in the Steele/Aronson paper actually "differed to about the degree that would be expected on the basis of differences in prior SAT scores."[44]

The key words here are "the raw, unadjusted scores" and "actually," which she inserts to build a case that stereotype threat is simply a hothouse phenomenon, cooked up in the laboratory but with no meaning for the real world. The problem is that Sackett never actually examined the Steele and Aronson data. Had he requested them and analyzed them for himself, his argument would have collapsed. Raw or adjusted, the data show that reducing stereotype threat lifted African Americans' verbal test performance—just as they did with Brown and Day's Raven's scores, and just as such threat-reducing maneuvers have done in hundreds of studies with women and mathematics, both inside and outside the laboratory.

Needless to say, I find this kind of fudging deeply depressing—especially so when it is done in the name of arguing for innate and immutable group differences. Far from being a thoughtful, truth-seeking consideration of a diverse body of hundreds of published experiments, longitudinal field studies, and interventions, these

critiques amount to little more than a scattershot venting of the spleen at the notion that there may be more to the story than the intellectual inferiority of blacks and the mathematical inferiority of women. I will leave it to experts on rationalization to explain why anyone would find so inconvenient the good news that simple and inexpensive means can be used to lift students to higher levels of engagement, enjoyment of academics, and intelligence.[45] But this attitude does underscore a most depressing feature of our debates of the evidence: Some—perhaps many—people are simply not capable of modifying deeply held beliefs about group differences—and no amount of data can change this.

CONCLUSION: THE GRAND EXPERIMENT

Are women being held back in math and science? Probably, but the extent to which they are held back by nurture versus nature remains unclear. The good news is that things are changing. True, women currently earn less than 25 percent of the bachelor's degrees in physics, engineering, and computer science—and only about a third of the PhDs in these areas.[46] But it is also true that this gap shows signs of closing. And for the past three decades, women have been catching up to men, even overtaking them, in many other areas of academics. In my own field of psychology, for example, women have gone from earning a tiny minority of degrees in the 1950s and 1960s to being awarded a commanding two-thirds majority at both the bachelor's and PhD levels. A decade ago, I heard a prominent social psychologist talking about the old days, when she was one of the few women in graduate school. She was referred to openly as "Blondie" and frequently had to listen to conversations about how women were fundamentally ill-suited to scientific psychology and naturally made for nursing, teaching, and homemaking. Furthermore, these opinions were justified by theories of biological determinism—the same ones people frequently offer to explain why women will never attain the same level of proficiency in math and science as men.

Such talk about psychology would be inconceivable today. That psychology is a much richer, more interesting, and more relevant field with so many women making contributions is self-evident. Would physics, computer science, and engineering enjoy similar benefits if we increased the ranks of women? There's reason to think so. Many efforts are currently under way to encourage girls to enter science, including single-sex classrooms and schools and many programs that include methods of boosting comfort and interest in math and science classes by applying what we have learned in our psychology laboratories. It will be interesting to watch over the coming decades how the results of this grand experiment turn out, if women continue to close the gap with men in areas where they have been underrepresented.

Whatever happens with regard to the low numbers of women in science, it is clear to me that great improvement is possible. But my dream result is not equal

numbers of men and women in all fields. Rather, my dream is Charles Murray's dream, which he so eloquently shared during the conference on the nature and nurture of women in science: that every individual receives an education that leads to an enjoyable career that fits his or her unique talents and interests. I have another dream: That our educational efforts yield a population that is more scientifically literate and intellectually curious, with an interest in and respect for scientific data—so that even if we do not achieve equal numbers of men and women in science, more of us will be able to distinguish scientific arguments from ideology. That would be real progress.

Reference Notes

1. Gladwell, 2007.
2. National Academy of Sciences et al. 2007.
3. Ceci and Williams 2006.
4. Fallows 2004.
5. For reviews, see for example, Aronson and Aronson 2007; Gladwell 1999; Harris 1998.
6. Jencks and Phillips 1998.
7. For reviews, see Aronson and Steele 2005; Aronson and McGlone 2008; Steele et al. 2002.
8. Aronson et al. 1999.
9. For an excellent discussion of sex differences in spatial abilities, see Newcombe 2006, as well as chapters 1 and 2, above, and chapter 9, below.
10. Connellan et al. 2000; see also chapter 1, above.
11. Schmader and Johns 2003; Seibt and Förster 2004; Derks et al. 2008.
12. Marx and Roman 2002; Marx et al. 2005.
13. Massey and Fischer 2005; Brown and Pinel 2003.
14. Good et al. 2008.
15. Cohen et al. 2006.
16. Good et al. 2003.
17. Cohen et al. 1999.
18. Murray 2008.
19. Aronson 2004.
20. Reardon et al. 2009
21. Davis et al. 2006.
22. For reviews see Aronson and Steele 2005; Aronson and McGlone 2009.
23. Walton and Spencer, in press.
24. Stricker 2006; Sackett et al. 2004.
25. Wax 2004 and 2007.
26. Wax 2003.
27. See, for example, Flynn 2007.
28. Ceci and Williams 2006.
29. National Center for Educational Statistics 2003.
30. Dee 2005.
31. Dee 2007.
32. Pedersen et al. 1978.
33. Krueger and Whitmore 2002.
34. Jencks and Phillips 1998.
35. Murray 2008. Murray further argues that "the high-stakes tests given in elementary and secondary school are expressly described as measures of what students have

learned, not how smart they are," but presents no evidence beyond the experience of his own children. Reardon et al.'s (2009) research on the California high school exit exam should give him pause about this assumption.

36. Stricker and Ward 2004; Danaher and Crandall 2008.
37. Massey and Fischer 2005.
38. Wax 2007.
39. Sackett et al. 2004.
40. Wax 2007.
41. Jencks and Phillips 1998.
42. Wax has tended to focus primarily on this tiny minority in a sea of published studies that did not use the analysis of covariance correction. Ironically, she accuses stereotype-threat researchers of "cherry-picking" to arrive at their desired conclusions.
43. Brown and Day 2006.
44. Wax 2004.
45. A particularly thorough and entertaining explication of how and why scientists, politicians, district attorneys—and the rest of us—cook the facts to reach desired conclusions can be found in Tavris and Aronson 2007.
46. National Science Foundation 2006.

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29

Experiments in Group Conflict

Muzafer Sherif

What are the conditions which lead to harmony or friction between groups of people? Here the question is approached by means of controlled situations in a boys' summer camp.

Conflict between groups—whether between boys' gangs, social classes, races or nations—has no simple cause, nor is mankind yet in sight of a cure. It is often rooted deep in personal, social, economic, religious and historical forces. Nevertheless, it is possible to identify certain general factors that have a crucial influence on the attitude of any group toward others. Social scientists have long sought to bring these factors to light by studying what might be called the "natural history" of groups and group relations. Intergroup conflict and harmony is not a subject that lends itself easily to laboratory experiments. But in recent years there has been a beginning of attempts to investigate the problem under controlled yet lifelike conditions, and I shall report here the results of a program of experimental studies of groups which I started in 1948. Among the persons working with me were Marvin B. Sussman, Robert Huntington, O. J. Harvey, B. Jack White, William R. Hood, and Carolyn W. Sherif. The experiments were conducted in 1949, 1953 and 1954; this article gives a composite of the findings.

We wanted to conduct our study with groups of the informal type, where group organization and attitudes would evolve naturally and spontaneously, without formal direction or external pressures. For this purpose we conceived that an isolated summer camp would make a good experimental setting, and that decision led us to choose as subjects boys about 11 or 12 years old, who would find camping natural and fascinating. Since our aim was to study the development of group relations among these boys under carefully controlled conditions, with as little interference as possible from personal neuroses, background influences or prior experiences,

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we selected normal boys of homogeneous backgrounds who did not know one another before they came to the camp.

They were picked by a long and thorough procedure. We interviewed each boy's family, teachers and school officials, studied his school and medical records, obtained his scores on personality tests and observed him in his classes and at play with his schoolmates. With all this information we were able to assure ourselves that the boys chosen were of like kind and background: all were healthy, socially well-adjusted, somewhat above average in intelligence and from stable, white, Protestant, middle-class homes.

None of the boys was aware that he was part of an experiment on group relations. The investigators appeared as a regular camp staff—camp directors, counselors and so on. The boys met one another for the first time in buses that took them to the camp, and so far as they knew it was a normal summer of camping. To keep the situation as lifelike as possible, we conducted all our experiments within the framework of regular camp activities and games. We set up projects which were so interesting and attractive that the boys plunged into them enthusiastically without suspecting that they might be test situations. Unobtrusively we made records of their behavior, even using hidden cameras and microphones when feasible.

We began by observing how the boys became a coherent group. The first of our camps was conducted in the hills of northern Connecticut in the summer of 1949 (see Figure 29.1). When the boys arrived, they were all housed at first in one large bunkhouse. As was to be expected, they quickly formed particular friendships and chose buddies. We had deliberately put all the boys together in this expectation, be-

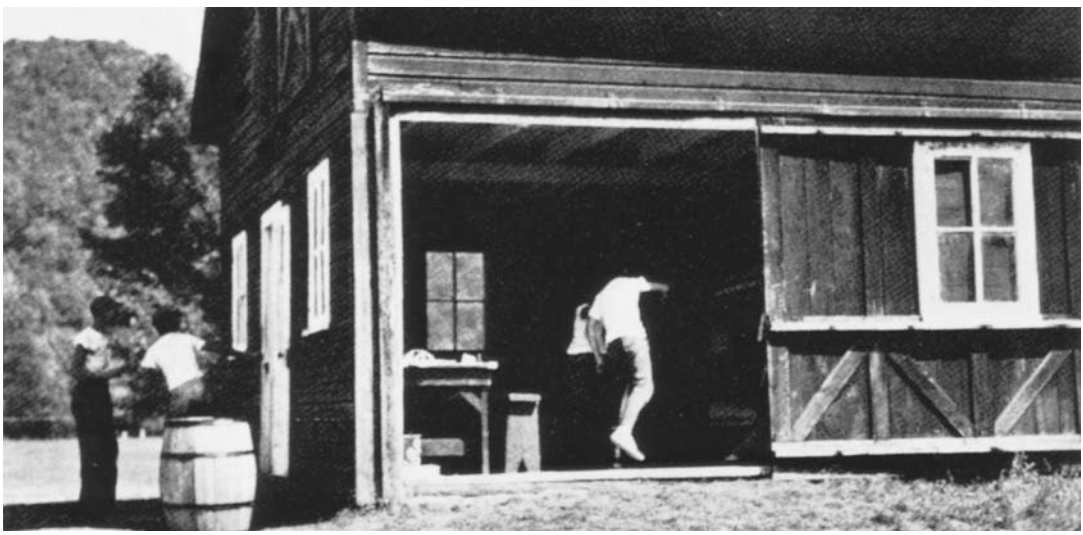


FIGURE 29.1
Members of one group of boys raid the bunkhouse of another group during the first experiment of the author and his associates, performed at a summer camp in Connecticut. The rivalry of the groups was intensified by the artificial separation of their goals. (Photograph by Muzafer Sherif.)

cause we wanted to see what would happen later after the boys were separated into different groups. Our object was to reduce the factor of personal attraction in the formation of groups. In a few days we divided the boys into two groups and put them in different cabins. Before doing so, we asked each boy informally who his best friends were, and then took pains to place the "best friends" in different groups as far as possible. (The pain of separation was assuaged by allowing each group to go at once on a hike and campout.)

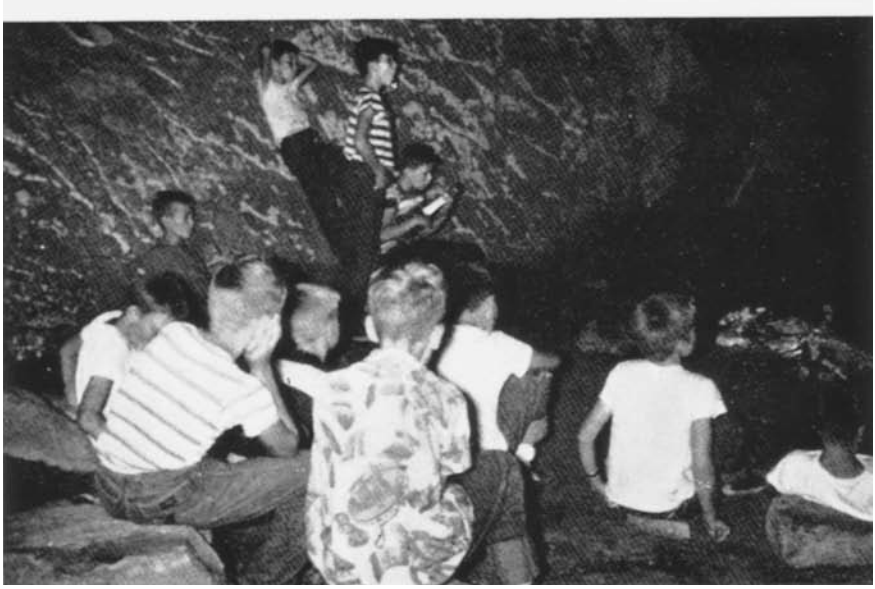
As everyone knows, a group of strangers brought together in some common activity soon acquires an informal and spontaneous kind of organization. It comes to look upon some members as leaders, divides up duties, adopts unwritten norms of behavior, develops an *esprit de corps.* Our boys followed this pattern as they shared a series of experiences. In each group the boys pooled their efforts, organized duties and divided up tasks in work and play. Different individuals assumed different responsibilities. One boy excelled in cooking. Another led in athletics. Others, though not outstanding in any one skill, could be counted on to pitch in and do their level best in anything the group attempted. One or two seemed to disrupt activities, to start teasing at the wrong moment or offer useless suggestions. A few boys consistently had good suggestions and showed ability to coordinate the efforts of others in carrying them through. Within a few days one person had proved himself more resourceful and skillful than the rest. Thus, rather quickly, a leader and lieutenants emerged. Some boys sifted toward the bottom of the heap, while others jockeyed for higher positions.

We watched these developments closely and rated the boys' relative positions in the group, not only on the basis of our own observations but also by informal sounding of the boys' opinions as to who got things started, who got things done, who could be counted on to support group activities.

As the group became an organization, the boys coined nicknames. The big, blond, hardy leader of one group was dubbed "Baby Face" by his admiring followers. A boy with a rather long head became "Lemon Head." Each group developed its own jargon, special jokes, secrets and special ways of performing tasks. One group, after killing a snake near a place where it had gone to swim, named the place "Moccasin Creek" and thereafter preferred this swimming hole to any other, though there were better ones nearby.

Wayward members who failed to do things "right" or who did not contribute their bit to the common effort found themselves receiving the silent treatment, ridicule or even threats. Each group selected symbols and a name, and they had these put on their caps and T-shirts. The 1954 camp was conducted in Oklahoma, near a famous hideaway of Jesse James called Robber's Cave (see Figure 29.2). The two groups of boys at this camp named themselves the Rattlers and the Eagles.

Our conclusions on every phase of the study were based on a variety of observations, rather than on any single method. For example, we devised a game to test the boys' evaluations of one another. Before an important baseball game, we set up a target board for the boys to throw at, on the pretense of making practice for the game more interesting. There were no marks on the front of the board for the boys



to judge objectively how close the ball came to a bull's-eye, but, unknown to them, the board was wired to flashing lights behind so that an observer could see exactly where the ball hit. We found that the boys consistently overestimated the performances by the most highly regarded members of their group and underestimated the scores of those of low social standing.

The attitudes of group members were even more dramatically illustrated during a cook-out in the woods. The staff supplied the boys with unprepared food and let them cook it themselves. One boy promptly started to build a fire, asking for help in getting wood. Another attacked the raw hamburger to make patties. Others prepared a place to put buns, relishes and the like. Two mixed soft drinks from flavoring and sugar. One boy who stood around without helping was told by others to "get to it." Shortly the fire was blazing and the cook had hamburgers sizzling. Two boys distributed them as rapidly as they became edible. Soon it was time for the watermelon. A low-ranking member of the group took a knife and started toward the melon. Some of the boys protested. The most highly regarded boy in the group took over the knife, saying, "You guys who yell the loudest get yours last."

When the two groups in the camp had developed group organization and spirit, we proceeded to the experimental studies of intergroup relations. The groups had had no previous encounters; indeed, in the 1954 camp at Robber's Cave the two groups came in separate buses and were kept apart while each acquired a group feeling.

Our working hypothesis was that when two groups have conflicting aims—i.e., when one can achieve its ends only at the expense of the other—their members will become hostile to each other even though the groups are composed of normal well-adjusted individuals. There is a corollary to this assumption which we shall consider later. To produce friction between the groups of boys we arranged a tournament of games: baseball, touch football, a tug-of-war, a treasure hunt and so on. The tournament started in a spirit of good sportsmanship. But as it progressed good feeling soon evaporated. The members of each group began to call their rivals "stinkers," "sneaks" and "cheaters." They refused to have anything more to do with individuals in the opposing group. The boys in the 1949 camp turned against buddies whom they had chosen as "best friends" when they first arrived at the camp. A large proportion of the boys in each group gave negative ratings to all the boys in the other. The rival groups made threatening posters and planned raids, collecting secret hoards of green apples for ammunition. In the Robber's Cave camp the Eagles, after a defeat in a tournament game, burned a banner left behind by the Rat-

FIGURE 29.2
Members of both groups collaborate in common enterprises during the second experiment, performed at a summer camp in Oklahoma. At the top, the boys of the two groups prepare a meal. In the middle, the two groups surround a water tank while trying to solve a water-shortage problem. At the bottom, the members of one group entertain the other. (Photographs by Muzafer Sherif.)

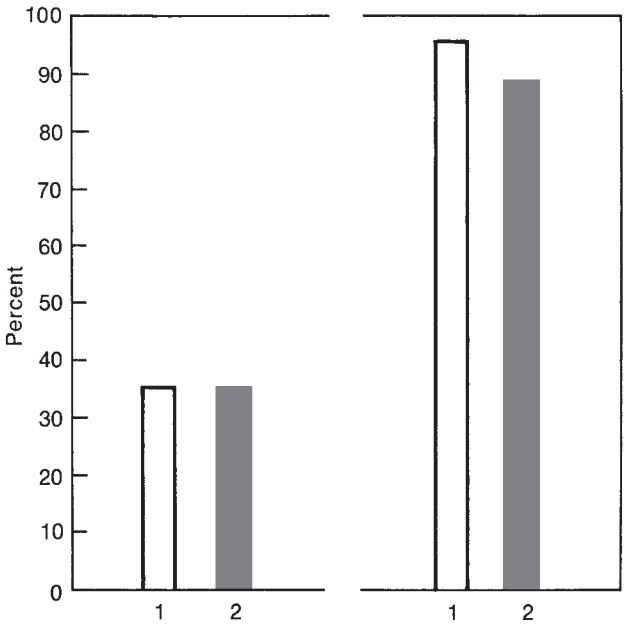


FIGURE 29.3
Friendship choices of campers for others in their own cabin are shown for Red Devils *(white)* and Bulldogs. *(gray).* At first, a low percentage of friendships were in the cabin group *(left).* After five days, most friendship choices were within the group *(right).*

tlers; the next morning the Rattlers seized the Eagles' flag when they arrived on the athletic field. From that time on name-calling scuffles and raids were the rule of the day.

Within each group, of course, solidarity increased. There were changes: one group deposed its leader because he could not "take it" in the contests with the adversary; another group overnight made something of a hero of a big boy who had previously been regarded as a bully. But morale and cooperativeness within the group became stronger. It is noteworthy that this heightening of cooperativeness and generally democratic behavior did not carry over to the group's relations with other groups.

We now turned to the other side of the problem: How can two groups in conflict be brought into harmony? We first undertook to test the theory that pleasant social contacts between members of conflicting groups will reduce friction between them. In the 1954 camp we brought the hostile Rattlers and Eagles together for social events: going to the movies, eating in the same dining room and so on, But far from reducing conflict, these situations only served as opportunities for the rival groups to berate and attack each other. In the dining-hall line they shoved each other aside, and the group that lost the contest for the head of the line shouted "Ladies first!" at the winner. They threw paper, food and vile names at each other at the tables. An Eagle bumped by a Rattler was admonished by his fellow Eagles to brush "the dirt" off his clothes.

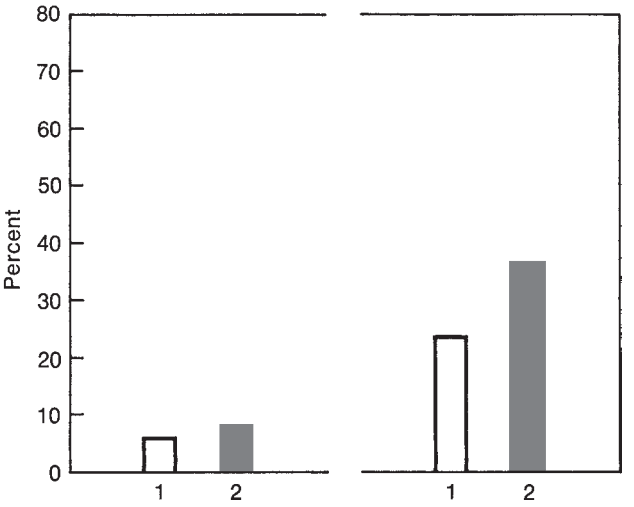


FIGURE 29.4
During conflict between the two groups in the Robber's Cave experiment, there were few friendships between cabins *(left).* After cooperation toward common goals had restored good feelings, the number of friendships between groups rose significantly *(right).*

We then returned to the corollary of our assumption about the creation of conflict. Just as competition generates friction, working in a common endeavor should promote harmony. It seemed to us, considering group relations in the everyday world, that where harmony between groups is established, the most decisive factor is the existence of "superordinate" goals which have a compelling appeal for both but which neither could achieve without the other. To test this hypothesis experimentally, we created a series of urgent, and natural, situations which challenged our boys.

One was a breakdown in the water supply. Water came to our camp in pipes from a tank about a mile away. We arranged to interrupt it and then called the boys together to inform them of the crisis. Both groups promptly volunteered to search the water line for the trouble. They worked together harmoniously, and before the end of the afternoon they had located and corrected the difficulty.

A similar opportunity offered itself when the boys requested a movie. We told them that the camp could not afford to rent one. The two groups then got together, figured out how much each group would have to contribute, chose the film by a vote and enjoyed the showing together.

One day the two groups went on an outing at a lake some distance away. A large truck was to go to town for food. But when everyone was hungry and ready to eat, it developed that the truck would not start (we had taken care of that). The boys got a rope—the same rope they had used in their acrimonious tug-of-war—and all pulled together to start the truck.

These joint efforts did not immediately dispel hostility. At first the groups returned to the old bickering and name-calling as soon as the job in hand was

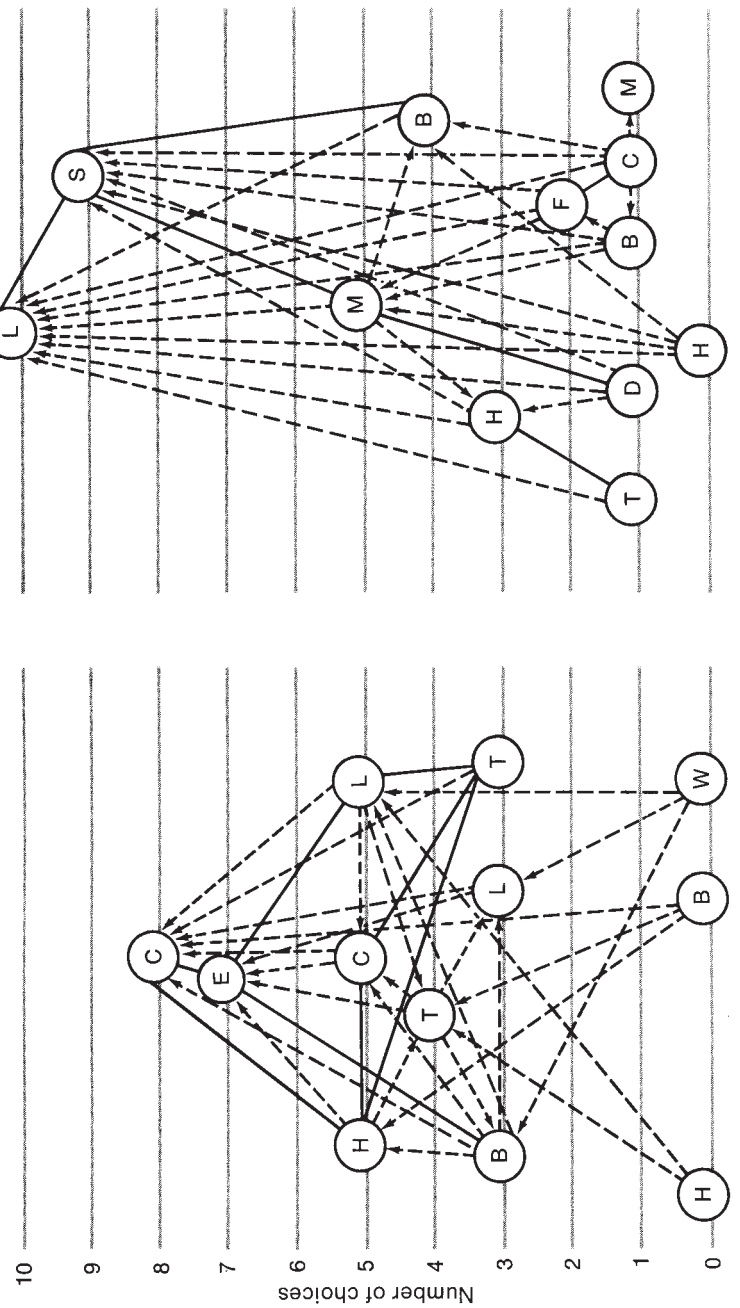


FIGURE 29.5
Sociograms represent patterns of friendship choice within the fully developed groups. One-way friendships are indicated by broken arrows; reciprocated friendships, by solid lines. Leaders were among those highest in the popularity scale. Bulldogs *(left)* had a close-knit organization with good group spirit. Low-ranking members participated less in the life of the group but were not rejected. Red Devils *(right)* lost the tournament of games between the groups. They had less group unity and were sharply stratified.

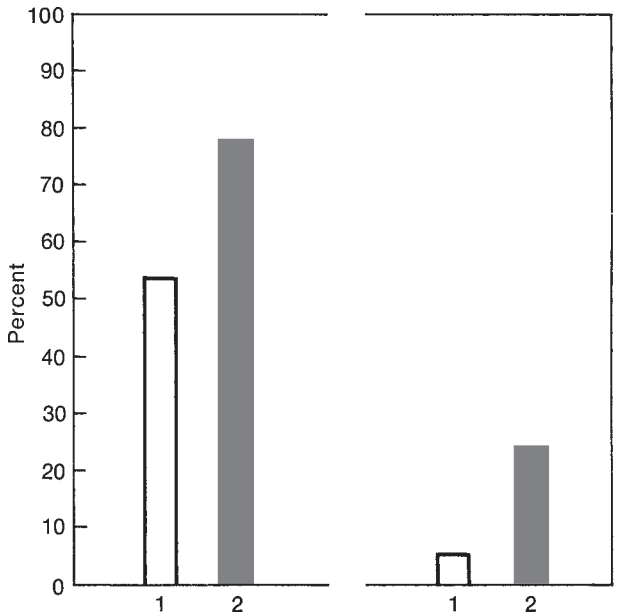


FIGURE 29.6
Negative ratings of each group by the other were common during the period of conflict *(left)* but decreased when harmony was restored *(right)*. The graphs show percentage who thought *all* (rather than *some* or *none*) of the other group were cheaters, sneaks, and so forth.

finished. But gradually the series of cooperative acts reduced friction and conflict. The members of the two groups began to feel more friendly to each other. For example, a Rattler whom the Eagles disliked for his sharp tongue and skill in defeating them became a "good egg." The boys stopped shoving in the meal line. They no longer called each other names, and sat together at the table. New friendships developed between individuals in the two groups.

In the end the groups were actively seeking opportunities to mingle, to entertain and "treat" each other. They decided to hold a joint campfire. They took turns presenting skits and songs. Members of both groups requested that they go home together on the same bus, rather than on the separate buses in which they had come. On the way the bus stopped for refreshments. One group still had five dollars which they had won as a prize in a contest. They decided to spend this sum on refreshments. On their own initiative they invited their former rivals to be their guests for malted milks.

Our interviews with the boys confirmed this change. From choosing their "best friends" almost exclusively in their own group, many of them shifted to listing boys in the other group as best friends (see Figure 29.4). They were glad to have a second chance to rate boys in the other group, some of them remarking that they had changed their minds since the first rating made after the tournament. Indeed they had. The new ratings were largely favorable (see Figure 29.6).

Efforts to reduce friction and prejudice between groups in our society have usually followed rather different methods. Much attention has been given to bringing members of hostile groups together socially, to communicating accurate and favorable information about one group to the other, and to bringing the leaders of groups together to enlist their influence. But as everyone knows, such measures sometimes reduce intergroup tensions and sometimes do not. Social contacts, as our experiments demonstrated, may only serve as occasions for intensifying conflict. Favorable information about a disliked group may be ignored or reinterpreted to fit stereotyped notions about the group. Leaders cannot act without regard for the prevailing temper in their own groups.

What our limited experiments have shown is that the possibilities for achieving harmony are greatly enhanced when groups are brought together to work toward common ends. Then favorable information about a disliked group is seen in a new light, and leaders are in a position to take bolder steps toward cooperation. In short, hostility gives way when groups pull together to achieve overriding goals that are real and compelling to all concerned.

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30

Jigsaw Groups and the Desegregated Classroom: In Pursuit of Common Goals

Elliot Aronson and Diane Bridgeman

The desegregated classroom has not produced many of the positive results initially expected by social scientists in 1954. It is argued that one of the major reasons for this failure is the overemphasis on competitiveness at the expense of interdependence in the classroom. In short, students in most classrooms very rarely cooperate with each other in pursuit of common goals. In this article, we describe a program of research in which elementary school students are "forced" to spend part of their classroom time mastering material in an interdependent structure. The results indicate that such structured interdependence increases the self-esteem, the morale, the interpersonal attraction, and the empathy of students across ethnic and racial divisions, and also improves the academic performance of minority students without hampering the performance of the ethnic majority.

There were high hopes when the Supreme Court outlawed school segregation in 1954. If black and white children could share classrooms and become friends, it was thought that perhaps they could develop relatively free of racial prejudice and some of the problems that accompany prejudice. The case that brought about the court's landmark decision was that of *Brown v. Board of Education;* the decision reversed the 1896 ruling *(Plessy v. Ferguson)* that held that it was permissible to segregate racially, as long as equal facilities were provided for both races. In the *Brown* case, the court held that psychologically there could be no such thing as "separate but equal." The mere fact of separation implied to the minority group in question that its members were inferior to those of the majority.

The Brown decision was not only a humane interpretation of the Constitution, it was also the beginning of a profound and exciting social experiment. As Stephan

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(1978) has pointed out, the testimony of social psychologists in the *Brown* case, as well as in previous similar cases in state supreme courts, suggested strongly that desegregation would not only reduce prejudice but also increase the self-esteem of minority groups and improve their academic performance. Of course the social psychologists who testified never meant to imply that such benefits would accrue automatically. Certain preconditions would have to be met. These preconditions were most articulately stated by Allport in his classic, *The Nature of Prejudice*, published the same year as the Supreme Court decision.

> Prejudice . . . may be reduced by equal status contact between majority and minority groups in the pursuit of common goals. The effect is greatly enhanced if this contact is sanctioned by institutional supports (i.e., by law, custom or local atmosphere), and provided it is of a sort that leads to the perception of common interests and common humanity between members of the two groups [Allport, 1954, p. 281].

THE EFFECTS OF DESEGREGATION

A quarter of a century after desegregation was begun, an assessment of its effectiveness is not encouraging. One of the most careful and thoroughgoing longitudinal studies of desegregation was the Riverside project conducted by Gerard and Miller (1975). They found that long after the schools were desegregated, black, white, and Mexican-American children tended not to integrate but to hang together in their own ethnic clusters. Moreover, anxiety increased and remained high long after desegregation occurred. These trends are echoed in several other studies. Indeed, the most careful, scholarly reviews of the research show few if any benefits (see St. John, 1975; Stephan, 1978). For example, according to Stephan's review, there is no single study that shows a significant increase in the self-esteem of minority children following desegregation; in fact, in fully 25 percent of the studies, desegregation is followed by a significant decrease in the self-esteem of young minority children. Moreover, Stephan reports that desegregation reduced the prejudice of whites toward blacks in only 13 percent of the school systems studied. The prejudice of blacks toward whites *increased* in about as many cases as it decreased. Similarly, studies of the effects of desegregation on the academic performance of minority children present a mixed and highly variable picture.

What went wrong? Let us return to Allport's prediction: Equal status contact in pursuit of common goals, sanctioned by authority, will produce beneficial effects. We will look at each of these three factors separately.

Sanction by Authority

In some school districts there was clear acceptance and enforcement of the ruling by responsible authority. In others the acceptance was not as clear. In still

others (especially in the early years) local authorities were in open defiance of the law. Pettigrew (1961) has shown that desegregation proceeded more smoothly and with less violence in those localities where local authorities sanctioned integration. But such variables as self-esteem and the reduction of prejudice do not necessarily change for the better even where authority clearly sanctions desegregation. While sanction by authority may be necessary, it is clearly not a sufficient condition.

Equal Status Contact

The definition of equal status is a trifle slippery. In the case of school desegregation, we would claim that there is equal status on the grounds that all children in the fifth grade (for example) have the same "occupational" status; that is, they are all fifth grade students. On the other hand, if the teacher is prejudiced against blacks, he may treat them less fairly than he treats whites, thus lowering their perceived status in the classroom (see Gerard and Miller, 1975). Moreover, if, because of an inferior education (prior to desegregation) or because of language difficulties, black or Mexican-American students perform poorly in the classroom, this could also lower their status among their peers. An interesting complication was introduced by Cohen (1972). Although Allport (1954) predicted that positive interactions will result if cooperative equal status is achieved, expectation theory, as developed by Cohen, holds that even in such an environment biased expectations by both whites and blacks may lead to sustained white dominance. Cohen reasoned that both of these groups accepted the premise that the majority group's competence results in dominance and superior achievement. She suggested that alternatives be created to reverse these often unconscious expectations. According to Cohen, at least a temporary exchange of majority and minority roles is therefore required as a prelude to equal status. In one study (Cohen and Roper, 1972), black children were instructed in building radios and in how to teach this skill to others. Then a group of white children and the newly trained black children viewed a film of themselves building the radios. This was followed by some of the black children teaching the whites how to construct radios while others taught a black administrator. Then all the children came together in small groups. Equal status interactions were found in the groups where black children had taught whites how to construct the radios. The other group, however, demonstrated the usual white dominance. We will return to this point in a moment.

In Pursuit of Common Goals

In the typical American classroom, children are almost never engaged in the pursuit of common goals. During the past several years, we and our colleagues

have systematically observed scores of elementary school classrooms and have found that, in the vast majority of these cases, the process of education is highly competitive. Children vie with one another for good grades and the respect of the teacher. This occurs not only during the quizzes and exams but also in the informal give and take of the classroom, where children typically learn to raise their hands (often frantically) in response to questions from the teacher, groan when someone else is called upon, and revel in the failure of their classmates. This pervasive competitive atmosphere unwittingly leads the children to view one another as foes to be heckled and vanquished. In a newly desegregated school, all other things being equal, this atmosphere could exacerbate whatever prejudice existed prior to desegregation.

A dramatic example of dysfunctional competition was demonstrated by Sherif et al. (1961) in the classic Robber's Cave experiment. In this field experiment, the investigators encouraged intergroup competition between two teams of boys at a summer camp; this created fertile ground for anger and hostility even in previously benign, noncompetitive circumstances—like watching a movie. Positive relations between the groups were ultimately achieved only after both groups were required to work cooperatively to solve a common problem.

It is our contention that the competitive process interacts with "equal status contact." That is to say, whatever differences in ability that existed between minority children and white children prior to desegregation are emphasized by the competitive structure of the learning environment; furthermore, since segregated school facilities are rarely equal, minority children frequently enter the newly desegregated school at a distinct disadvantage, which is made more salient by the competitive atmosphere.

It was this reasoning that led Aronson and his colleagues (1975, 1978a) to develop the hypothesis that interdependent learning environments would establish the conditions necessary for the increase in self-esteem and performance and the decrease in prejudice that were expected to occur as a function of desegregation. Toward this end they developed a highly structured method of interdependent learning and systematically tested its effects in a number of elementary school classrooms. The aim of this research program was not merely to compare the effects of cooperation and competition in a classroom setting. This had been ably demonstrated by other investigators dating as early as Deutsch's 1949 experiment. Rather, the intent was to devise a cooperative classroom structure that could be utilized easily by classroom teachers on a long-term sustained basis and to evaluate the effects of this intervention via a well-controlled series of field experiments. In short, this project is an action research program aimed at developing and evaluating a classroom atmosphere that can be sustained by the classroom teachers long after the researchers have packed up their questionnaires and returned to the more cozy environment of the social psychological laboratory.

The method is described in detail elsewhere (Aronson et al., 1978a). Briefly, students are placed in six-person learning groups. The day's lesson is divided into six

paragraphs such that each student has one segment of the written material.[1] Each student has a unique and vital part of the information, which, like the pieces of a jigsaw puzzle, must be put together before any of the students can learn the whole picture. The individual must learn his own section and teach it to the other members of the group. The reader will note that in this method each child spends part of her time in the role of expert. Thus, the method incorporates Cohen's findings (previously discussed) within the context of an equal status contact situation.

Working with this "jigsaw" technique, children gradually learn that the old competitive behavior is no longer appropriate. Rather, to learn all of the material (and thus perform well on a quiz), each child must begin to listen to the others, ask appropriate questions, and in other ways contribute to the group. The process makes it possible for children to pay attention to one another and begin to appreciate one another as potentially valuable resources. It is important to emphasize that the motivation of the students is not necessarily altruistic; rather, it is primarily self-interest, which, in this case, happens also to produce outcomes that are beneficial to others.

EXPERIMENTS IN THE CLASSROOM

Systematic research in the classroom has produced consistently positive results. The first experiment to investigate the effects of the jigsaw technique was conducted by Blaney et al. (1977). The schools in Austin, Texas, had recently been desegregated, producing a great deal of tension and even some interracial skirmishes throughout the school system. In this tense atmosphere, the jigsaw technique was introduced in 10 fifth-grade classrooms in seven elementary schools. Three classes from among the same schools were also used as controls. The control classes were taught by teachers who, while using traditional techniques, were rated very highly by their peers. The experimental classes met in jigsaw groups for about 45 minutes a day, three days a week for six weeks. The curriculum was basically the same for the experimental and control classes. Students in the jigsaw groups showed significant increases in their liking for their groupmates both within and across ethnic boundaries. Moreover, children in jigsaw groups showed a significantly greater increase in self-esteem than children in the control classrooms. This was true for Anglo children as well as for ethnic minorities. Anglos and blacks showed greater liking for school when taught in the jigsaw classrooms than when studying in traditional classrooms. (The Mexican-American students showed a tendency to like school *less* in the jigsaw classes; this will be discussed shortly.)

These results were essentially replicated by Geffner (1978) in Watsonville, California—a community consisting of approximately 50 percent Anglos and 50 percent Mexican-Americans. As a control for the possibility of a Hawthorne effect,

[1]The method works best with discrete, continuous written material (like social studies), but it has been successfully utilized with mathematics and language arts as well.

Geffner compared the behavior of children in classrooms using the jigsaw and other cooperative learning techniques with that of children in highly innovative (but not interdependent) classroom environments as well as with traditional classrooms. Geffner found consistent and significant gains within classrooms using jigsaw and other cooperative learning techniques. Specifically, children in these classes showed increases in self-esteem as well as increases in liking for school. Negative ethnic stereotypes were also diminished. That is, children increased their positive general attitudes toward their own ethnic group as well as toward members of other ethnic groups to a far greater extent than did children in traditional and innovative classrooms.

Changes in academic performance were assessed in an experiment by Lucker et al. (1977). The subjects were 303 fifth- and sixth-grade students from five elementary schools in Austin, Texas. Six classrooms were taught in the jigsaw manner, while five classrooms were taught traditionally by competent teachers. For two weeks children were taught a unit on colonial America taken from a fifth-grade textbook. All children were then given the same standardized test. The results showed that Anglo students performed just as well in jigsaw classes as they did in traditional classes ($\bar{X}$ = 66.6 and 67.3 respectively); minority children performed significantly better in jigsaw classes than in traditional classes ($\bar{X}$ = 56.6 and 49.7 respectively).[2] The difference for minority students was highly significant. Only two weeks of jigsaw activity succeeded in narrowing the performance gap between Anglos and minorities from more than 17 percentage points to about 10 percentage points. Interestingly enough, the jigsaw method apparently does *not* work a special hardship on high-ability students: Students in the highest quartile in reading ability benefited just as much as students in the lowest quartile.

UNDERLYING MECHANISMS

Increased Participation

We have seen that learning in a small interdependent group leads to greater interpersonal attraction, self-esteem, and liking for school, more positive interethnic and intraethnic perceptions, and, for ethnic minorities, an improvement in academic performance. We think that some of our findings are due to more active involvement in the learning process under conditions of reduced anxiety. In jigsaw, children are required to participate. This increase in participation should enhance interest, which would result in an improvement in performance as well as an increased liking for school—all other things being equal. But all other things are sometimes not equal. For example, in the study by Blaney et al. (1977), there was some indication from our observation of the groups that many of the Mexican-

[2]The mean scores have been converted to percentage of correct answers.

American children were experiencing some anxiety as a result of being required to participate more actively. This seemed to be due to the fact that these children had difficulty with the English language, which produced some embarrassment in working with a group dominated by Anglos. In a traditional classroom, it is relatively easy to "become invisible" by remaining quiet and refusing to volunteer. Not so in jigsaw. This observation was confirmed by the data on liking for school. Blaney et al. found that Anglos and blacks in jigsaw classrooms liked school better than those in the traditional classrooms, while for Mexican-Americans the reverse was true. This anxiety could be reduced if Mexican-American children were in a situation in which it was not embarrassing to be more articulate in Spanish than in English. Thus, Geffner (1978), working in a situation in which both the residential and school population was approximately 50 percent Spanish-speaking, found that Mexican-American children (like Anglos and blacks) increased their liking for school to a greater extent in the cooperative groups than in traditional classrooms.

Increases in Empathic Role-taking

Only a small subset of our results is attributable to increases in active participation in and of itself. We believe that people working together in an interdependent fashion increase their ability to take one another's perspective. For example, suppose that Jane and Carlos are in a jigsaw group. Carlos is reporting and Jane is having difficulty following him. She doesn't quite understand because his style of presentation is different from what she is accustomed to. Not only must she pay close attention, but, in addition, she must find a way to ask questions that Carlos will understand and that will elicit the additional information she needs. To accomplish this, she must get to know Carlos, put herself in his shoes, empathize.

Bridgeman (1977) tested this notion. She reasoned that taking one another's perspective is required and practiced in jigsaw learning. Accordingly, the more experience students have with the jigsaw process, the greater will their role-taking abilities become. In her experiment, Bridgeman administered a revised version of Chandler's (1973) role-taking cartoon series to 120 fifth-grade students. Roughly half the students spent eight weeks in a jigsaw learning environment while the others were taught in either traditional or innovative small group classrooms. Each of the cartoons in the Chandler test depicts a central character caught up in a chain of psychological cause and effect, such that the character's subsequent behavior is shaped by and fully comprehensible only in terms of the preceding events. In one of the sequences, for example, a boy who has been saddened by seeing his father off at the airport begins to cry when he later receives a gift of a toy airplane similar to the one that had carried his father away. Midway into each sequence, a second character is introduced in the role of a late-arriving bystander who witnesses the resultant behaviors of the principal character but is not privy to the causal events. Thus, the subject is in a privileged position relative to the story

character, whose role the subject is later asked to assume. The cartoon series measures the degree to which the subject is able to set aside facts known only to him or herself and adopt a perspective measurably different from his or her own. For example, while the subject knows why the child in the above sequence cries when he receives the toy airplane, the mailman who delivered the toy is not privy to this knowledge. What happens when the subject is asked to take the mailman's perspective?

After eight weeks, students in the jigsaw classrooms were better able to put themselves in the bystander's place than students in the control classrooms. For example, when the mailman delivered the toy airplane to the little boy, students in the control classrooms tended to assume that the mailman knew the boy would cry; that is, they behaved as if they believed that the mailman knew that the boy's father had recently left town on an airplane—simply because they (the subjects) had this information. On the other hand, students who had participated in a jigsaw group were much more successful at taking the mailman's role—realizing that he could not possibly understand why the boy would cry upon receiving a toy airplane.

Attributions for Success and Failure

Working together in the pursuit of common goals changes the "observer's" attributional patterns. There is some evidence to support the notion that cooperation increases the tendency for individuals to make the same kind of attributions for success and failure to their partners as they do to themselves. In an experiment by Stephan et al. (1978), it was found (as it has been in several experiments by others) that when an individual succeeds at a task, he tends to attribute his success dispositionally (e.g., skill), but when he fails he tends to make a situational attribution (e.g., luck). Stephan et al. went on to demonstrate that individuals engaged in an *interdependent* task make the same kinds of attributions to their partner's performance as they do to their own. This was not the case in competitive interactions.

Effects of Dependent Variables on One Another

It is reasonable to assume that the various consequences of interdependent learning become antecedents for one another. Just as low self-esteem can work to inhibit a child from performing well, anything that increases self-esteem is likely to produce an increase in performance among underachievers. Conversely, as Franks and Marolla (1976) have indicated, increases in performance should bring about increases in self-esteem. Similarly, being treated with increased attention and respect by one's peers (as almost inevitably happens in jigsaw groups) is another important antecedent of self-esteem, according to Franks and Marolla. There is ample

evidence for a two-way causal connection between performance and self-esteem (see Covington and Beery, 1976; Purkey, 1970).

OTHER COOPERATIVE TECHNIQUES

In recent years a few research teams utilizing rather different techniques for structuring cooperative behavior have produced an array of data consistent with those resulting from the jigsaw technique. For example, Cook and his colleagues (1978) have shown that interracial cooperative groups in the laboratory underwent a significant improvement in attitudes about people of other races. In subsequent field experiments, Cook and his colleagues found that interdependent groups produced more improved attitudes toward members of previously disliked racial groups than was present in noninterdependent groups. It should be noted, however, that no evidence for generalization was found; that is, the positive change was limited to the specific members of the interdependent group and did not extend to the racial group as a whole.

Johnson and Johnson (1975) have developed the "Learning Together" model, which is a general and varied approach to interdependent classroom learning. Basically, Johnson and Johnson have found evidence for greater cross-ethnic friendship ratings, greater self-esteem, and higher motivation in their cooperative groups than in control conditions. They have also found increases in academic performance.

In a different vein, Slavin (1978) and DeVries, Edwards, and Slavin (1978) have developed two highly structured techniques that combine within-group cooperation with across-group competition. These techniques, "Teams Games and Tournaments" (TGT) and "Student Teams Achievement Divisions" (STAD), have consistently produced beneficial results in lower class, multiracial classrooms. Basically, in TGT and STAD, children form heterogeneous five-person teams; each member of a team is given a reasonably good opportunity to do well by dint of the fact that she competes against a member of a different team with similar skills to her own. Her individual performance contributes to her team's score. The results are in the same ball park as jigsaw: Children participating in TGT and STAD groups show a greater increase in sociometric, cross-racial friendship choices and more observed cross-racial interactions than control conditions. They also show more satisfaction with school than the controls do. Similarly, TGT and STAD produce greater learning effectiveness among racial minorities than do the control groups.

It is interesting to note that the basic results of TGT and STAD are similar to those of the jigsaw technique in spite of one major difference in procedure: While the jigsaw technique makes an overt attempt to minimize competition, TGT and STAD actually promote competitiveness and utilize it across teams—within the context of intrateam cooperation. We believe that this difference is more apparent than real. In most classrooms where jigsaw has been utilized, the students are in jigsaw groups for less than two hours per day. The rest of the class time is spent in

myriad process activities, many of which are competitive in nature. Thus, what seems important in both techniques is that *some* specific time is structured around cooperativeness. Whether the beneficial results are produced *in spite* of a surrounding atmosphere of competitiveness or *because* of it is the task of future research to determine.

CONCLUSIONS

We are not suggesting that jigsaw learning or any other cooperative method constitutes the solution to our interethnic problems. What we have shown is that beneficial effects occur as a result of structuring the social psychological aspects of classroom learning so that children spend at least a portion of their time in pursuit of common goals. These effects are in accordance with predictions made by social scientists in their testimony favoring desegregating schools in 1954. It is important to emphasize the fact that the jigsaw method has proved effective even if it is employed for as little as 20 percent of a child's time in the classroom. Moreover, other techniques have produced beneficial results even when interdependent learning was purposely accompanied by competitive activities. Thus, the data do not indicate the desirability of either placing a serious limit on classroom competition or interfering with individually guided education. Interdependent learning can and does coexist easily with almost any other method used by teachers in the classroom.

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31

Branch Rickey, Jackie Robinson, and the Social Psychology of Affirmative Action

Anthony R. Pratkanis and Marlene E. Turner

April 10, 1947, was a remarkable day in the history of management. The setting was old Ebbets Field in Brooklyn, N.Y. More than 14,000 baseball fans were on hand to see the Brooklyn Dodgers play their top minor-league ballclub, the Montreal Royals, in one of the preseason's last exhibition games.

At the top of the sixth inning, Dodger announcer Red Barber read the following statement from Dodger President Branch Rickey for fans listening on the radio: "The Brooklyn Dodgers today purchased the contract of Jackie Roosevelt Robinson from the Montreal Royals. He will report immediately." Five days later Robinson donned uniform No. 42 to become the first black American since 1887 to play baseball in the all-white major leagues.

When Rickey hired Robinson, he inaugurated history's first affirmative-action program.

By affirmative action, we mean the proactive removal of discriminatory barriers and the promotion of institutions leading to integration of in- and out-groups. This definition may strike some as odd; in common parlance, affirmative action has come to mean a "quota" system. This confuses one possible tactic (among many) with what should be an overall objective of integration.

What is most remarkable about Rickey's effort is its sweeping success. He changed the face of baseball and turned his team into a winning and profitable franchise. For more than 40 years, social psychologists have studied how best to implement desegregation plans, and Rickey's desegregation of the Dodgers re-

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mains the best illustration of those principles, which still can be used to improve the effectiveness of affirmative-action programs.

THE PSYCHOLOGY OF INEVITABLE CHANGE

One factor that increases the likelihood of successful integration is the feeling that change is inevitable—that the out-group member (in this case, black Americans) will be joining the in-group members (white major-leaguers) in the very near future, and nothing can be done to prevent it. The psychology of the inevitable works because it forces the prejudiced individual to bring his attitudes in line with the new reality. By contrast, when leaders oppose or give only halfhearted support to affirmative action, opposition is encouraged.

How did Rickey use the psychology of the inevitable? During 1947 spring training, Dodger Manager Leo Durocher found out that some Dodger players—Dixie Walker, Eddie Stanky, Kirby Higbe, and Bobby Bragan—were drawing up a petition to warn that they would never play alongside Robinson. Durocher gathered the team and told them: "Well, boys, you know what you can do with that petition. You can wipe your ass with it. I hear Dixie Walker is going to send Mr. Rickey a letter asking to be traded. Just hand him the letter, Dixie, and you're gone. Gone! I don't care [what color the guy is]. I am the manager, and I say he plays."

The next night, Rickey called the dissident players to his hotel suite and lectured them on Americanism. Several felt ashamed and gave ground. At the request of Walker and Higbe, who persisted in their objections to Robinson, Rickey arranged trades; when Walker quickly changed his mind, his trade was canceled. Higbe (who, interestingly, believed as late as 1967 that he had acted correctly) was sold to the last-place Pittsburgh Pirates.

Walker, Stanky, and Bragan later became some of Robinson's strongest supporters. In 1965, Bragan sat next to Robinson at Rickey's funeral. As he recalled in Maury Allen's *Jackie Robinson: A Life Remembered:* "We shook hands warmly. I don't think either of us thought anything about it, or of the past. It was a new time. I changed. Jackie changed. The world changed."

ESTABLISHING EQUAL-STATUS CONTACT

At the time Rickey was planning to bring a black American to baseball, many believed that blacks and whites could not develop close interpersonal relationships. Social psychologists disagreed, identifying the "equal-status contact" hypothesis:

Positive intergroup relations can occur when members of the two groups: a) possess equal status, b) seek common goals, c) are cooperatively dependent on each other, and d) interact with positive support of authorities, laws, or customs.

To Rickey, who was privy to social-science thinking, the typical ballclub does or (more often) could meet those four conditions. Each playing position has equal status; teams seek a common goal (the pennant); each player is mutually dependent on teammates to reach the goal; and the owner and manager can provide the positive support of authority.

The task of Dodger management was to keep the team on track toward its superordinate goal. Dodger management reminded the white players of how close they had come the previous season to qualifying for the World Series and that the missing component might be Robinson's speed on the base paths. As Durocher repeatedly told the white players in spring training, "He'll put money in your pocket, boys."

Dixie Walker's behavior shows the results of equal-status contact. A short time into the 1947 season, Walker approached Robinson in the batting cage and spoke his first words to him: "Ah think you'd be better able to handle that curveball if you didn't stride so far." Walker later showed the rookie how to hit behind the runner. When asked why, he replied, "I saw things in this light: When you're on a team, you got to pull together to win." At the end of the 1947 season, Walker credited Robinson with putting the Dodgers in the race.

THE PEE WEE REESE PRINCIPLE

One factor that perpetuates prejudice is a norm—a rule or expectation used to guide behavior (e.g., whites don't play baseball with blacks; women can't handle top-management positions). Norms draw their power from the belief that everyone else supports the norm and that social sanctions will result if the norm is transgressed.

The most effective way to change a norm is to violate it, puncturing the reality the norm creates. By hiring Robinson, Rickey took the first step toward breaking the norms of prejudice. Others displayed support: Brooklyn fans wore "I'm for Robinson" buttons; Dodger sportswriters wrote sympathetic articles; players such as Pirate Hank Greenberg supported Robinson when Pittsburgh fans verbally abused him.

One Dodger stands out in his actions to break the norms of prejudice—eventual Hall-of-Famer Harold "Pee Wee" Reese. In spring training, Reese refused to sign the anti-Robinson petition, surprising teammates who expected a Southern boy would "know better." Reese was the first Dodger to kid Robinson and to dine with him off the field. After Reese broke the ice, others grew friendlier.

One Reese-Robinson incident has acquired legendary status. Early in the 1947 season, the Dodgers traveled to Ohio to play the Cincinnati Reds. Thousands from

Reese's boyhood home of Kentucky jammed Crosley Field to hurl racial slurs. The hatemongering continued into the bottom of the first inning, when the Dodgers took the field. Reese left his shortstop position and walked the 120 feet over to first base, where Robinson stood.

Reese smiled at Robinson and spoke a few words; Robinson smiled back. With all eyes on the pair, Reese casually placed his arm around Robinson's shoulder. The stands were silent. Reese's action punctured the norm of anti-black racism and broke another common myth as well: that all whites are racists and incapable of change.

NONVIOLENT RESISTANCE

Robinson and Rickey first met August 28, 1945, in Rickey's office, where the owner broke the news: "I've sent for you because I'm interested in you as a candidate for the Brooklyn National League club. I think you can play in the major leagues. . . . What I don't know is whether you have the guts."

For the next three hours, Rickey described the situations that Robinson would face as a black American entering an all-white league: the segregated hotels and restaurants, what his wife would hear in the stands, the abuse he would take on the field. Rickey got into Robinson's face, yelling insults and racial slurs, asking Robinson what he would do if a player spiked him during a game and said, "How do you like that, nigger boy?" Robinson asked, "Mr. Rickey, do you want a Negro who's afraid to fight back?" Rickey answered, "I want a ballplayer with guts enough not to fight back."

Why did Rickey, nicknamed "the Mahatma," place so much emphasis on nonviolence? First, Rickey feared a spiral of violence—between 1900 and 1949, 33 major race riots rocked U.S. towns, many near baseball parks.

Second, and perhaps more important for today's manager, nonviolence alters the social dynamics. If Robinson were not to fight back, then it became his Dodger teammates' responsibility to stand up for him, which they did on many occasions, most notably a run-in with the St. Louis Cardinals. After two previous spiking incidents, Enos Slaughter hit a grounder that was tossed to Robinson at first base, and deliberately spiked him on the back of the leg. The Dodgers immediately poured out of the dugout to protest, threatening the Cardinals with "dire consequences" if attacks continued.

The lesson: Don't leave it to the newcomer alone to defend his right to be in the organization; co-workers and management must share this burden.

One effective means of attitude change is to reverse perceptions and present the world through another's viewpoint. Seeing the world as another sees it provides an opportunity to receive new information and to challenge old ways of seeing. Rickey succeeded in getting the Dodgers to see the world through Robinson's eyes.

The Dodgers' baptism by fire occurred April 22, when the Philadelphia Phillies came to town for a three-game series. Phillies Manager Ben Chapman began screaming racial slurs at Robinson: "Hey, nigger, why don't you go pickin' cotton?" Soon his team joined in: "Hey, coon, do you always smell so bad?" and then they would flap their hands as if something stunk.

The abuse continued into the second game of the series. Feelings on the Dodger bench were tense. Most likely, the white Dodgers had never experienced prejudice before, and now they started to get the full treatment: "Hey, you carpet-baggers, how's your little Reconstruction period getting along?"

Toward the end of the second game, the white Dodgers exploded. Eddie Stanky was the first off the bench, demanding of the Phillies what type of man would say such things. Dixie Walker—yes, the former petitioner Dixie Walker—was next to support Robinson. Although the Phillies' abuse persisted for several years, it was much diminished.

For the record, the Dodgers swept the series, with Robinson scoring the winning run in the first and third games and adding an insurance run in the second game. Branch Rickey was beside himself with glee and could not resist the opportunity to lecture Ben Chapman. As he later told Harvey Frommer in *Rickey and Robinson: The Men Who Broke Baseball's Color Barrier* (MacMillan): "When Chapman and the others poured out that string of unconscionable abuse, he solidified and unified 30 men, not one of whom was willing to see someone kick around a man who had his hands tied behind his back."

INDIVIDUATING THE NEW GROUP MEMBER

One problem that occurs when an out-group member joins an in-group is the continued use of stereotypes—the person is not seen as Jackie Robinson but as a Negro ballplayer. The use of stereotypes promotes in-group favoritism and makes it easier to dislike the new employee since he is seen as a vague category and not a flesh-and-blood human being.

One solution to this problem is to encourage the treatment of the new group member as an *individual*. Introduce the new employee as a unique person with special skills, experience, and qualities that will add new depth to the organization. Don't introduce him as one manager of a major hospital-supply company did a new employee: "It is good she is a woman and even better that she is black—but it would have been best of all if she was disabled, too."

Rickey felt that through the "intimacy" of baseball and the heat of competition, the white Dodgers would come to know and like Robinson. Since baseball fans would have no opportunity to interact personally with him, Rickey called on media friends to "introduce" Robinson as an individual of accomplishment in sports, aca-

demics, and the military. In 1950 Robinson contributed greatly to the individuation effort by starring in *The Jackie Robinson Story.*

Through the psychology of the inevitable, Rickey put great pressure on the white Dodgers to change their prejudices. He coupled this with a positive means of coming to terms with racism—forgiveness and redemption. In Rickey's Methodist theology, redemption means the transformation of the person; one hates the sin but loves the sinner.

When Rickey saw signs that a white player was "coming around," he would place that player in a new role of authority—a position that would involve interacting with and serving black Americans. Social scientists call this cooptation, or providing the opponent with a new role complete with new information, role expectations, and social pressures that increase identification with the organization's goals.

A case in point is Bobby Bragan, the Dodgers' expendable second-string catcher. Rickey could easily have washed his hands of him after Bragan's participation in the petition drive, but he felt Bragan needed time to think and that someday he would learn he was wrong. In 1948, Rickey appointed Bragan manager of the Dodger farm club in Fort Worth, Texas. Monte Irvin, one of the New York Giants' first black players, described it this way: "After a year or so, Bragan realized how wrong his attitude was. Later he went out of his way to help black ballplayers."

Rickey was not above using even the bitter Chapman incident to promote his cause. The Phillies' attack on Robinson and the other Dodgers drew a public outcry. Worried about his and his team's image, Chapman issued a statement—and an insincere apology—that he was riding Robinson as he would any rookie. Rickey took the opportunity to stage a photo of Chapman with Robinson that appeared in every major newspaper's sports pages.

UNDO THE PERCEPTION OF PREFERENTIAL SELECTION

Many white Americans doubted Jackie Robinson's ability to make it in the major leagues. Star pitcher Bob Feller said, "If he were a white man, I doubt they would even consider him big-league material." When Robinson had an off-day because of a sore shoulder, a reporter commented, "Had he been white, the Royals would have dropped him immediately."

New York Yankees General Manager Larry McPhail said that the level of Robinson's ability, "were he white, would make him eligible for a trial with, let us say, the Brooklyn Dodgers' Class B farm at Newport News." McPhail also believed black Americans, in general, could not compete in the white leagues without

considerable training. "A major-league player must have something besides natural ability. He must also have a competitive attitude and discipline"—implying that blacks lacked these characteristics.

The fact that Jackie Robinson was perceived as preferentially selected for the Dodgers should alert us to one potential problem of affirmative action—it can lead to the (mis)perception that the recipient was hired just because he was black (or a woman, etc.) and can't necessarily do the job.

Of course, in Robinson's case the perception was ridiculous. In 1946, with the Montreal Royals, Robinson won the International League batting championship with a .349 average, scored 113 runs, was chosen the league's Most Valuable Player, and went on to lead the club to victory in the Little World Series. If Robinson did not qualify for a chance in the majors, then no minor-league player that year did.

Social psychologists have identified three general reasons why affirmative action can be perceived as preferential selection. The first is simple racism. As McPhail's quote indicates, some believed that black Americans lack certain essential abilities. If they lacked the ability, then why did they get the job? The answer: It must be preferential selection.

Second, some affirmative-action procedures may be seen as violating norms of universality and procedural justice or fairness. Finally, white resistance to affirmative action is higher among those who feel relatively deprived. In such cases, attitudes toward affirmative action serve a scapegoating function—the dissatisfied individual blames black Americans for his problems.

The perception of affirmative action as preferential selection can have negative consequences for the beneficiary, as well. Considerable research has shown that women who feel they were preferentially selected for a desired position give lower self-evaluations of their abilities and skills. Others in the organization can pick up on this "second-class" status, thereby creating a two-tiered organization.

But managers do not have to let the misperception of preferential selection go unchecked. Rickey and his assistants followed seven guidelines in countering this misperception: (1) Establish unambiguous, explicit, and focused qualifications criteria to be used in the selection decision (the baseball box score); (2) Be certain that selection procedures are perceived as fair (Robinson paid his dues in the minors); (3) Provide specific information testifying to the competencies of the new hire (Rickey's and Durocher's confidence in Robinson's ability; Robinson's season with Montreal); (4) Emphasize the recipient's contributions to the team (Durocher claimed Robinson would get the Dodgers in the pennant drive); (5) Develop socialization strategies that deter feelings of helplessness (the minor leagues and "Dodgertown"); (6) Reinforce the fact that affirmative action is not preferential selection (Rickey repeatedly told Robinson that he would have to make it on his own—in the box score and with his teammates—and not to depend on the owner for help); and (7) Refocus the helping effort away from the recipient by identifying and communicating the social barriers preventing integration (everyone was aware of the color bar).

Removing Institutional Barriers

Out-groups are excluded from the in-group by more than just interpersonal prejudice; certain institutional practices can also restrict the choices of an out-group member. Institutional barriers can include inferior schools in minority-populated areas, the lack of day-care for working parents, limited job opportunities in poor areas, laws restricting new housing, insistence on traditional career paths, the flow of information in "old boy" networks, and tracking out-groups into certain careers.

One institution that posed a particular problem for Rickey was many towns' segregated housing and dining facilities. Black Americans, even star ballplayers, were not allowed to stay in most hotels nor to eat in most restaurants. Rickey discovered the significance of this practice during spring training in 1946, when Robinson was isolated from his team. In 1947, Rickey attempted a temporary and costly measure: The Dodgers held spring training in Havana, Cuba—a racially desegregated country. (Ironically, Rickey paid for his Cuban spring training with the money from the sale of petition organizer Kirby Higbe.)

In 1948, Rickey opened "Dodgertown," fulfilling his dream of a "college of baseball." In Dodgertown, black and white players could room together, eat together, and train together without the pressure of local segregation laws. It is interesting to note that long after the repeal of segregation, Dodgertown and its many imitators remain in operation because such camps are useful in creating team cohesion. There is a lesson to be learned: The creative removal of institutions serving as out-group barriers can also improve the lot of the in-group.

WHY DID RICKEY DO IT?

Branch Rickey hardly fits today's stereotype of an "advocate for affirmative action"—a wide-eyed liberal, out of touch with reality. Rickey was a lifelong conservative, a Republican, a staunch Christian, and above all else, a strong believer in capitalism. He was also a shrewd business leader with an eye on the bottom line. In the 1930s, he made the St. Louis Cardinals into one of baseball's winningest and most profitable franchises through an innovative training program—the development of the "farm" system of minor-league clubs.

So why did Rickey hire Robinson? Rickey gave two answers:

First, for the good of his Dodgers. As Rickey told an aide: "The greatest untapped reservoir of raw material in the history of the game is the black race! The Negroes will make us winners for years to come, and for that I will happily bear being called a bleeding heart and a do-gooder and all that humanitarian rot." Rickey was right; the Dodgers were perennial contenders, and the gates swelled. His genius lay in recognizing the advantages of affirmative action when most other owners felt that hiring a black American would send baseball into financial ruin.

Second, his Christian faith led Rickey to conclude that affirmative action was a moral imperative. To illustrate his point, he often told the story of coaching his college team in 1904, when the team hotel refused to admit one player, Charley Thomas, because he was black. Rickey finally convinced the manager to place a cot in his room for Thomas to sleep on—as the hotel would do for any black servant. That night Thomas cried in bed, pulling at his skin, "It's my black skin, Mr. Rickey. . . . If I could pull it off I'd be like everybody else." The incident haunted Rickey for more than 40 years, and he vowed to do whatever he could to see that other Americans did not have to face the bitter humiliation heaped on Thomas.

Rickey's two reasons for affirmative action may strike some as odd; after all, you can't serve God and mammon. But Rickey saw no contradiction. For him, capitalism works best when barriers are removed and free enterprise can operate. Expanding the labor market means lower labor costs, a greater pool of talent to choose from, and ultimately a superior product. Capitalism requires a society where all can compete fairly. In this case, the moral imperative merely underscored the mission's importance.

A Final Box Score

Branch Rickey stayed with the Brooklyn Dodgers through 1950, moving to Pittsburgh and building a farm system that would help the Pirates win the 1960 World Series. After retiring in 1955, Rickey continued to speak out for integration, serving as chairman of President Eisenhower's commission to promote racial harmony (a precursor to the Equal Opportunity Employment Commission). Two years after his 1965 death, Rickey received baseball's highest honor: induction into the National Baseball Hall of Fame and Museum.

Jackie Robinson played for the Dodgers for 10 years—a period baseball writers call the golden Brooklyn decade. In that 10-year period, the Dodgers won six National League pennants, won the World Series in 1955, and took opponents to the wire in two pennant races. Robinson was named baseball's Rookie of the Year in 1947, won the league batting title and was named Most Valuable Player in 1949, and was named to the National League All-Star team six consecutive years. He batted .311 lifetime and held the fielding mark for second basemen playing 150 or more games, with a .992 percentage.

In 1962, Robinson was elected to the Hall of Fame—along with Bob Feller, who years before had publicly questioned Robinson's ability. The two were the first players in baseball history elected in their first year of eligibility, and their Cooperstown plaques are displayed side by side.

32

Prejudice as Self-Image Maintenance: Affirming the Self Through Derogating Others

Steven Fein and Steven J. Spencer

The authors argue that self-image maintenance processes play an important role in stereotyping and prejudice. Three studies demonstrated that when individuals evaluated a member of a stereotyped group, they were less likely to evaluate that person negatively if their self-images had been bolstered through a self-affirmation procedure, and they were more likely to evaluate that person stereotypically if their self-images had been threatened by negative feedback. Moreover, among those individuals whose self-image had been threatened, derogating a stereotyped target mediated an increase in their self-esteem. The authors suggest that stereotyping and prejudice may be a common means to maintain one's self-image, and they discuss the role of self-image-maintenance processes in the context of motivational, sociocultural, and cognitive approaches to stereotyping and prejudice.

A most striking testament to the social nature of the human psyche is the extent to which the self-concept—that which is the very essence of one's individuality—is integrally linked with interpersonal dynamics. Since the earliest days of the formal discipline of psychology, the significant influences of a number of social factors on the self-concept have been recognized. A central focus of sociocultural and social–cognitive approaches to psychology has concerned the ways in which individuals' self-concepts are defined and refined by the people around them. This is evident in early discussions of the social nature of individuals' self-concepts (Cooley, 1902; Mead, 1934) and of social comparison theory (Festinger, 1954), and it continues to be evident in more recent work, such as that concerning self-fulfilling prophecies (e.g., Eccles, Jacobs, and Harold, 1990; Rosenthal and Jacobson, 1968; Snyder, 1984) and cultural influences (Abrams, 1994; Cameron and Lalonde, 1994; Cohen

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and Nisbett, 1994; H. R. Markus and Kitayama, 1991, 1994; Triandis, 1989; Turner, Oakes, Haslam, and McGarty 1994).

The converse focus—the self-concept's influence on perceptions of and reactions toward others—has been recognized more fully within the last two decades, through, for example, research on self-schemas (H. Markus, 1977; H. Markus and Wurf, 1987), self-verification (Swann, Stein-Seroussi, and Giesler, 1992), self-discrepancies (Higgins, 1996; Higgins and Tykocinski, 1992), and a host of self-serving biases in individuals' perceptions, judgments, and memories involving the self (e.g., Ditto and Lopez, 1992; Greenwald, 1980; Klein and Kunda 1992, 1993; Nisbett and Ross, 1980; Ross and Sicoly, 1979; Schlenker, Weigold, and Hallam, 1990).

Particularly within the past decade, research has converged on the role of self-image- and self-esteem-maintenance processes in people's perceptions and reactions regarding others. These approaches, whose roots can be seen in the earlier work of James, Festinger, Heider, Sherif, Tajfel, and others, include research on downward social comparison (Brown, Collins, and Schmidt, 1988; Brown and Gallagher, 1992; Gibbons and Gerrard, 1991; Gibbons and McCoy, 1991; Taylor and Lobel, 1989; Wills, 1981, 1991; Wood and Taylor, 1991), self-evaluation maintenance (Tesser, 1988; Tesser and Cornell, 1991), social identity (Abrams and Hogg, 1988; Brewer, 1993; Crocker, Thompson, McGraw, and Ingerman, 1987; Hogg and Abrams, 1988; Smith, 1993; Turner, 1982), terror management (Greenberg et al., 1992), and self-affirmation (Liu and Steel, 1986; Steele, 1988; Steel and Liu, 1983).

This article examines the role of self-image-maintenance processes in a particular set of reactions and perceptions: those concerning prejudice and negative evaluations of others. More specifically, we examine the thesis that many manifestations of prejudice stem, in part, from the motivation to maintain a feeling of self-worth and self-integrity. That is, self-image threat may lead people to engage in prejudiced evaluations of others. These negative evaluations can, and often do, make people feel better about themselves. Prejudice, therefore, can be self-affirming. By using available stereotypes to justify and act on prejudices, individuals may be able to reclaim for themselves a feeling of mastery and self-worth, often saving themselves from having to confront the real sources of self-image threat.

Several self-image-maintenance processes are described or implied in the existing literature, but the research reported in this article focuses on one in particular: self-affirmation. Steele and his colleagues (e.g., Steele, Spencer, and Lynch, 1993) have argued that people seek to maintain "an image of self-integrity, that is, overall moral and adaptive adequacy" (p. 885). If an individual experiences a threat to this image, he or she attempts to restore this image by reevaluating and reinterpreting experiences and events in ways that reaffirm the self's integrity and value. Supported by research on self-affirmation effects in cognitive dissonance, Steele et al. (1993) argued that when facing a potential threat, even an important one, people have "the option of leaving the threat unrationalized—that is, accepting the threat without countering it or its implications—and affirming some other important aspect of the self that reinforces one's overall self-adequacy" (p. 885).

We argue that prejudice often serves a self-affirming function for individuals, and providing people with other means of self-affirmation should reduce their desire to make prejudiced evaluations. The link between self-image threats and the use of prejudice should be weakened by providing people with the opportunity to self-affirm; that is, by providing them with information that restores their positive sense of self-integrity. This approach is distinct from many of the classic approaches to stereotyping and prejudice, such as frustration–aggression theory and scape-goating (Dollard, Doob, Miller, Mowrer, and Sears, 1939; Miller and Bugelski, 1948), social identity theory (Tajfel, 1982), and downward social comparison theory (Wills, 1981). We argue that this process of self-affirmation should reduce the desire to make prejudiced evaluations even though it does not release pent-up anger or aggression, as frustration–aggression theory would require; enhance social identity, as social identity theory would require; make self–other comparisons, as downward social comparison theory would require; or confront the threat itself in any way. Only a self-affirmational perspective suggests that restoring a positive sense of self-integrity in this way would result in the decrease of prejudiced evaluations. Of course, this thesis shares many assumptions with these other theoretical positions. Our approach, however, can be seen as extending previous approaches by examining self-image maintenance as both cause and effect of prejudiced evaluations and by integrating these approaches with contemporary views of the self.

Taken together, the studies reported in this article examined both sides of this process; the roles of self-affirmation and self-image threat in influencing the likelihood that individuals will use stereotypes or prejudice and the role of prejudice in helping individuals restore a positive sense of self.

STUDY 1

In Study 1, we examined the hypothesis that self-affirmation should make participants less likely to evaluate another individual in ways that reflect their prejudice toward the individual's group. Participants in this study were asked to evaluate a target person who apparently was a member of a group for which there was a readily available negative stereotype or a member of some other outgroup for which there was not a strong available stereotype. Before being exposed to this target person, participants were either self-affirmed or not affirmed. That is, half of the participants completed a task designed to affirm and make salient an important aspect of their self-concepts, and the other half completed a task designed not to affirm any important aspects of their self-concepts.

We believe that many stereotypes and prejudices are such readily available and cognitively justifiable means of self-enhancement that individuals often use their stereotypes and prejudices to self-enhance in the face of everyday vulnerabilities and frustrations (e.g., see Wood and Taylor, 1991). That is, unless other motives are

activated, such as a goal of accurate perception (Darley, Fleming, Hilton, and Swann, 1988; Neuberg and Fiske, 1987), accountability (Tetlock, 1983), or social desirability or egalitarian motives (Dovidio and Gaertner, 1991; Monteith, 1993), people may find stereotyping and prejudice to be a reliable and effective way to protect their self-esteem in a frequently threatening world. To the extent, then, that the use of stereotypes and prejudice stems in part from self-image maintenance needs, self-affirmation should make individuals less likely to resort to this use. Study 1 was designed to test this hypothesis.

Method

Participants. Seventy-two introductory psychology students from the University of Michigan participated in this experiment as partial fulfillment of a course requirement.[1]

Procedure. The participants were told that they would participate in two experiments in this session. The first experiment was portrayed as a study of values. The second experiment was portrayed as an investigation of how employees evaluate candidates in the hiring process.

Manipulation of self-affirmation. Half the participants completed a self-affirmation procedure, and half did not. This procedure was a modified version of that used by Steele and Liu (1983; see also Steele, 1988; Tesser and Cornell, 1991) to affirm and make salient an important part of individuals' self concepts. Participants were given a list of several values (adapted from values characterized by the Allport–Vernon Study of Values), including *business/economics, art/music/theater, social life/relationships, and science/pursuit of knowledge.* Participants in the self-affirmation condition were asked to circle the value that was most important to them personally and then to write a few paragraphs explaining why this value was important to them.[2] In contrast, participants in the no-affirmation condition were asked to circle the value that was least important to them personally and then to write a paragraph explaining why this value might be important to someone else. Steele and his colleagues (e.g, Spencer and Steele, 1990; Steele, 1988) have found that causing participants to think about a value that is personally very important to them is an effective means of producing self-affirmation and that, in the absence of self-image threat, it does not affect participants' state self-esteem.

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[1]Although 72 people participated in the experiment, 18 were excluded because they were Jewish, for reasons that are described in the *Manipulation of target's ethnicity* section. Thus, the data from 54 participants were included in all analyses.

[2]None of our participants wrote paragraphs concerning prejudice or tolerance. Moreover, the effects of the manipulation were not related to which value—business/economics, art/music/theater, social life/relationships, or science/pursuit of knowledge—the participants chose.

Evaluation task. For what we portrayed as the second experiment, participants were placed in individual cubicles and were told that their task was to evaluate an individual who has applied for a job as a personnel manager at a particular organization. The participants were given general information about the responsibilities of a personnel manager at this hypothetical organization and were encouraged to try to make an accurate assessment of the candidate's suitability for the job.

All participants next examined information about a fictitious job candidate who was about to graduate from their university. Participants were given the candidate's completed job application to examine. The application contains questions about the candidate's previous work experience, academic and extracurricular skills and interests, and other résumé-type information. The completed application was constructed to suggest that the applicant was fairly well qualified for the position but was not necessarily a stellar candidate. Attached to the application was a photograph of the candidate. All of the participants saw virtually the same application and photograph; the variations are noted in the section below. After examining this material, participants watched an 8-minute videotape presented as excerpts from the candidate's job interview. All participants saw the same videotape, which featured a fairly neutral performance by the candidate—that is, her responses tended to be adequate but not extremely positive or negative. After watching the excerpts, participants completed a questionnaire about the candidate and her qualifications.

Manipulation of target's ethnicity. Although all participants saw the same job interview excerpts, saw the same woman in the photograph attached to the job application, and read the same information about her work experiences, academic record, and other job-relevant information, we included two minor variations in the photograph and three in the application to suggest either that the candidate was Jewish or that she was not Jewish (and probably was Italian).

We used this distinction for several reasons. At the time and place in which this study was conducted, there was a very well known and relatively freely discussed stereotype concerning the "Jewish American princess" (JAP). There was a fairly sizable and salient minority of students at this campus who were Jewish women from New York City and Long Island, New York, and these women were the targets of a number of JAP jokes that spread across campus. In contrast to stereotypes about African Americans, gay men and lesbians, and many other groups, the JAP stereotype was one that many students were willing to discuss quite candidly, with many of them openly endorsing it.[3]

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[3]One of the reasons for this may be that the stereotype is diffused across two types of prejudice: anti-Semitism and sexism. That is, those who endorse the stereotype are protected against being considered anti-Semitic because they are not implicating Jewish men in their derogatory comments or beliefs, and they are protected against being considered sexist because they are not implicating most women. A second reason may be that the targeted group is perceived as being relatively privileged, and thus, disparaging them may not seem as harmful.

Another factor that played a role in our decision to examine this form of prejudice was that we were able to select a stimulus person who could be considered representative of the JAP and yet, with a few subtle manipulations, could just as easily be considered representative of a non-Jewish group—one that also was an outgroup to most participants but about which there was no strong negative stereotype or prejudice on this campus. This alternative categorization was of an Italian American woman. Although also a minority on campus, this group was not nearly as salient on campus, and as pilot testing confirmed, there was no strong, consensual stereotype or prejudice on campus concerning this group.

To manipulate the target's ethnic background, we varied the following elements of her application: her name (Julie Goldberg vs. Maria D'Agostino), an extracurricular activity (volunteering for a Jewish or Catholic organization), and her sorority (either of two sororities that shared similar reputations in terms of status, but one of which consisted predominantly of Jewish women and one of which consisted predominantly of non-Jewish women of European, but not Hispanic, descent). All the other information on the application, including all of the job-relevant information, was identical.

In both conditions, the photograph attached to the job application was of the same woman (who was also featured in the videotape). We had chosen a female undergraduate, unknown to the participants, who could be seen either as fitting the prototypic image of a JAP or as non-Jewish (and probably Italian). The photograph varied slightly, however, so that "Julie" was wearing a necklace featuring the Star of David and had her hair clipped up in back (in a clip that some pilot test students referred to as a JAP clip), whereas "Maria" was wearing a cross and had her hair down. Pilot testing suggested that our manipulation was successful.

This woman appeared in the video wearing a sweater that covered her necklace, and her hair was down but brushed in such a way that its length seemed somewhere in between the styles depicted in the two photographs. As indicated above, all participants saw the same 8-minute video.

Dependent measures. Participants rated the candidate in terms of her overall personality and her qualifications for the job. Her personality was assessed by the extent to which participants agreed (on a 7-point scale) that each of the following traits described her: intelligent, insensitive, trustworthy, arrogant, sincere, inconsiderate, friendly, self-centered, down-to-earth, rude, creative, materialistic, motivated, cliquish, ambitious, conceited, happy, vain, warm, superficial. Negative traits were reverse scored. Her job qualifications were assessed by the extent to which participants agreed (on a 7-point scale) with the following statements: "I feel this person would make an excellent candidate for the position in question," "I would guess that this person is in the top 20% of people interviewed," and "I felt favorably toward this person." Both scales showed good internal reliability (Cron-

bach's alphas of .93 and .91, respectively). Finally, participants were asked to indicate their own and the target's ethnicity and religion.

Results

Recall that our prediction was that when participants were not self-affirmed, they would evaluate the target more negatively when she was portrayed as Jewish than when she was portrayed as Italian, whereas when participants were self-affirmed, this difference would be reduced or eliminated.

The critical measure in this study was participants' ratings of the target's personality across a variety of dimensions. These ratings were subjected to a two-way analysis of variance (ANOVA). The ANOVA revealed that there was no significant main effect for the manipulation of affirmation, $F(1, 50) = 1.8$, $p > .15$, but that there was a significant main effect for the manipulation of the apparent ethnicity of the target, as the target was rated more positively when she appeared to be Italian than when she appeared to be Jewish, $F(1, 50) = 4.9$, $p < .05$. Most important, this main effect was qualified by a significant interaction, $F(1, 50) = 8.5$, $p < .01$. As can be seen in Figure 32.1 and consistent with our predictions, not affirmed participants who evaluated the Jewish target were significantly more negative in their evaluations of the target's personality than were participants in

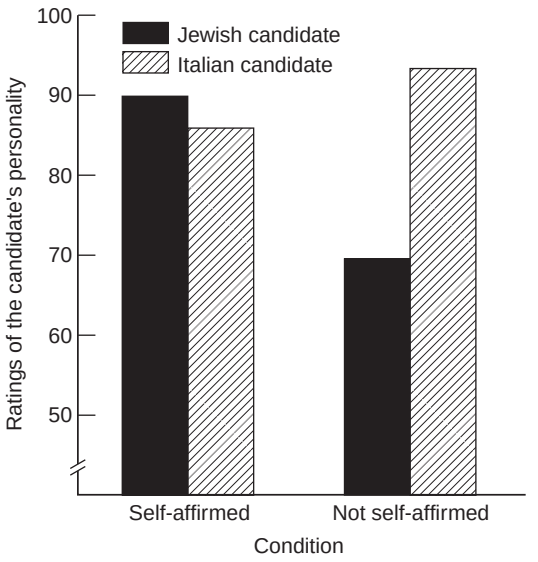

FIGURE 32.1
Rating of candidate's personality as a function of self-affirmation condition and ethnicity of the candidate. Higher numbers indicate more favorable evaluations.

all other conditions, $t(50) = 3.7$, $p < .001$. None of the other conditions differed significantly from each other.[4]

Ratings of how qualified the target was for the job in question also were consistent with our predictions. A two-way ANOVA revealed that participants who had not been affirmed tended to rate the target more negatively than did participants who had been affirmed, $F(1, 50) = 4.6$, $p < .05$. The ANOVA revealed further that participants rated the candidate more positively when she was depicted as Italian than when she was depicted as Jewish, $F(1, 50) = 6.3$, $p < .05$. These main effects were qualified, however, by a marginally significant interaction between the two independent variables, $F(1, 50) = 3.0$, $p < .10$. Consistent with our predictions, not affirmed participants evaluated the qualifications of the candidate more negatively when she was portrayed as Jewish ($M = 14.9$) rather than Italian ($M = 20.6$), whereas affirmed participants did not make this discrimination (Ms = 20.2 and 21.2, respectively). The planned comparison indicated that not affirmed participants who evaluated the Jewish target were significantly more negative in their evaluations of the target's job qualifications than were participants in all other conditions, $t(50) = 3.7$, $p < .001$. None of the other conditions differed significantly from each other.

Discussion

The results of this study demonstrate that self-affirmation can reduce the likelihood that individuals will derogate members of stereotyped groups. In the absence of self-affirmation, participants' evaluations of the job candidate were biased as a function of her apparent ethnicity. That is, these participants evaluated the target more negatively if she was a member of a stereotyped group than if she was not. Among participants who had been self-affirmed, however, this difference was eliminated.

These results, therefore, highlight the significant role played by the self-concept in prejudice. More specifically, they support the idea that thinking about a self-relevant value, even one completely unrelated to prejudice, can reduce the expression of prejudice. Thinking about a self-relevant value has this effect even though it need not release pent-up anger or aggression, enhance social identity, or involve self–other comparisons, as frustration–aggression theory, social identity theory, and downward social-comparison theory would require.

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[4]We used this planned comparison for each of the dependent measures in this study, as well as a comparable planned comparison for each of the dependent measures in Studies 2 and 3, because it was the most direct test of our theoretically derived hypotheses (see, e.g., Hays, 1981; Keppel, 1973; Rosenthal and Rosnow, 1991; Winer, 1971). For each of these measures, we also conducted the more conservative Newman–Keuls post hoc comparisons. In each case, the Newman–Keuls comparisons indicated the difference tested in the planned comparison to be significant and revealed further that none of the other conditions differed significantly from each other.

In a replication of this study, we also examined the potential mediating role of participants' mood in this paradigm with an independent sample of 71 participants. We measure participants' mood using the Mehrabian and Russell (1974) mood scale after the manipulation of self-affirmation but before the participants evaluated the target. The mood scale consists of three subscales, each consisting of six sets of bipolar adjectives. These subscales, measure pleasure (e.g., happy–unhappy, pleased–annoyed), arousal (e.g., stimulated–relaxed, excited–calm), and dominance (e.g., controlled–controlling, influential–influenced). Consistent with the findings of Liu and Steele (1986), the manipulation of self-affirmation had no significant effects on any one or any combination of these subscales (all Fs < 1). Moreover, participants' mood was unrelated to their evaluation of the target's personality, $r(69) = -.120$ *ns*, or of her qualifications for the job $r(69) = .04$, *ns*. Replicating the results of Study 1, not affirmed participants who evaluated the Jewish target rated the target's personality significantly more negatively than did participants in all other cells, $t(67) = 2.4$, $p < .01$. Similarly, not affirmed participants who evaluated the Jewish target tended to rate the target's job qualifications more negatively than did participants in all other cells, $t(67) = 1.8$, $p < .05$.

The results of these studies suggest that at least part of the negative evaluation of people who are stereotyped may result from people trying to affirm their self-image. To the extent that people's self-images have been buffered by other means of self-affirmation, they should be less drawn to such a strategy. In the absence of such self-affirmation, however, stereotyping and prejudice may provide a mechanism by which people protect or bolster their self-esteem. Stereotyping and prejudice may be reinforced, therefore, because they can make people feel better about themselves.

STUDY 2

The results of Study 1 suggest that self-affirmation can play an important role in reducing the effects of stereotyping or prejudice on individuals' evaluations of a member of a stereotyped group. In Study 2, we focused on the other side of this self-image maintenance coin by examining whether a self-image threat would exacerbate the effects of stereotyping or prejudice on individuals' evaluations of a member of a stereotyped group.

Study 2 differed from Study 1 in two other important ways, thereby providing a better test of the generalizability of our hypotheses. First, rather than varying the target's apparent ethnicity, in Study 2 we manipulated the target's apparent sexual orientation. Thus, whereas the stereotyped group in Study 1 was contrasted with a nonstereotyped group that was also a distinct minority, the stereotyped group in Study 2 was contrasted with the nonstereotyped majority. Second, rather than measuring participants' general derogation of a target as a function of her membership in a stereotyped group, Study 2 measured participants' stereotyping of an individual as a function of his membership in a stereotyped group.

More specifically, some participants in this study received self-image-threatening information in the form of bogus negative feedback on an intelligence test; the other participants received no such threat. Later, all participants evaluated a target on a series of trait dimensions relevant to popular stereotypes of gay men. The biographical information about the target was manipulated so as to suggest to some of the participants that the target may have been gay and to suggest to the other participants that he was straight (heterosexual). The hypothesis tested in Study 2 was that participants should be more likely to exhibit stereotyping of the (apparently) gay target if they had previously received negative feedback on the intelligence test than if they had not.

Method

Participants. Sixty-one male undergraduates from Williams College participated in this experiment either for extra credit for their introductory psychology course or for the change to win money in a random drawing.

Procedure. Participants reported to the laboratory individually and completed the tasks in individual rooms containing a desk and a Macintosh computer. Participants first read a sheet of paper containing the cover story, which stated that the study involved a series of different cognitive and social judgment tasks. The first part of the study involved the manipulation of self-relevant feedback (described below). After some filler tasks (e.g., a simple word-stem completion task) designed to preserve the integrity of the cover story, participants completed the social judgment task, in which the participants read information about a male target. The information was designed to suggest either that the target was gay or that he was straight. After rating the target on a series of dimensions, the participants were probed for any suspicions, debriefed thoroughly, and thanked for their participation.

Manipulation of feedback. Half the participants were assigned randomly to the negative feedback condition, and the other half were assigned to the neutral condition. To the former half, the experimenter introduced the first set of tasks as "a new form of intelligence test that is given on the computer. It measures both verbal and reasoning abilities." To the latter half, the experimenter explained that they had been assigned to a control condition in which they were simply to read the materials contained in a bogus test of intelligence. The experimenter revealed to these participants that the participants in the treatment condition of the study would be told that the test was a real, valid measure of intelligence. In other words, the experimenter told the neutral condition participants the truth. These participants were instructed to refrain from trying hard to answer the questions on the bogus test because many of the questions had no correct answer and because the time limits were unrealistically quick. The experimenter also told them that the computer

would present them with bogus scores at the conclusion of the test. To assure the participants that these scores were indeed bogus, the experimenter told them what these scores would be. The experimenter explained that the participants in the treatment condition would be led to believe that the scores were real. The purpose of having the participants in the neutral condition learn this cover story and go through the test was so that they would be exposed to the same test and specific items as the participants in the negative feedback condition, but that the test would have no relevance to their self-image.[5]

All subsequent instructions for the test were presented on the computer. The instructions were presented in a professional-looking design that introduced the intelligence test as "The Reasoning and Verbal Acuity Battery." The instructions explained that the test had been validated in numerous studies throughout the United States and Canada. The test consisted of five parts, each tapping different sets of intellectual skills. The first four parts consisted of analogies, antonyms, sentence completions, and syllogisms. The fifth part was called a "verbal–nonverbal matching test" and involved matching difficult vocabulary words to various pictures; this was a modified version of the Ammons and Ammons (1962) Quick Test of Intelligence. The instructions to this battery of tests explained that research had shown that this combination of tasks was the ideal, most valid method to measure individuals' general intelligence.

To emphasize the relevance of these intellectual skills, each test within the battery was introduced with an explanation of what it measured. Many of the specific items in these test were taken from advanced tests used for admission to graduate school or law school. To make the tests seem even more challenging (and thus to help to justify the bogus feedback for the participants in the negative feedback condition), we modified several of the items so that there was no correct answer among the options given. Moreover, the time limits for each item were very short (ranging from 10 to 20 seconds, depending on the test), and a clock showing the seconds ticking away appeared on the screen for each item.

At the conclusion of this battery of tests, the computer program indicated that it was calculating the scores. After 7 seconds a new screen appeared that indicated the participant's percentile rankings (relative to other college students tested in the United States and Canada) for each test. Each participant received an identical set of scores: 51st percentile for the analogies test, 54th for the antonyms, 56th for the sentence completions, 33rd for the syllogisms, and 38th for the verbal–nonverbal matching. Given the prestige of the college in which this study was conducted and the students' previous scores on tests such as the Scholastic Achievement Test,

[5]Consistent with the intent of the manipulation, pilot testing of 36 other participants from the same population revealed that the state self-esteem (as measured by Heatherton and Polivy's [1991] state self-esteem scale) of participants in the neutral condition was not significantly lower than that of participants who were not exposed to the test or cover story ($F < 1$). In addition, the state self-esteem of these participants (in either condition) was significantly higher than that of pilot test participants who were led to believe the test was real (Fs > 6).

these scores are extremely disappointing to the students from this population. (See Footnote 5.)

Manipulation of target's apparent sexual orientation. After administering a series of brief cognitive tasks designed to enhance the integrity of the cover story, the experimenter introduced the "social judgment tasks" by informing the participants that they would read some information about an individual and make some judgments about him or her.

All participants read about a target named Greg, a 31-year old struggling actor living in the East Village in New York City. The information summarized Greg's ambitions and career struggles and listed some of the many odd jobs that Greg had taken to pay the rent while he pursued his dream. The information continued by detailing a recent event in Greg's life concerning landing "a fairly large part in a serious and rather controversial play directed by a young director." Participants read that Greg was excited about the play and, in particular, about working with this young director. The director's name was not mentioned, but gender pronouns indicated that the director was a man. The participants read that after the first week of rehearsals, Greg approached the director and asked him whether he wanted to get "a drink or something" with him after that night's rehearsal so that they could talk about his role in some more depth. The story continued for a few paragraphs, summarizing the play's opening and reviews, and it concluded with the information that while continuing to act in the play, Greg was writing his own play and had already gotten a commitment from the director to help him with it.

The information about Greg was identical across conditions with the following exceptions. In the first sentence, the participants in the straight-implied condition read that Greg "has been living with his girlfriend, Anne, in a small apartment" for several years. Anne's name was mentioned three more times in subsequent parts of the story about Greg, and there was one additional reference to his "girlfriend." For the gay-implied condition, in the first sentence we replaced the word "girlfriend" with "partner" and dropped reference to Anne. Neither the partner's name nor the partner's gender was specified, and there were no subsequent references to this partner.

Many of the details of the story about Greg (e.g., his living in the East Village, his caring "for a very close and very ill friend for the last 2 months of his friend's life," and his relationship with the director) were included to support the implication in the gay-implied condition that Greg was gay. Because each piece of information by itself very plausibly could describe a straight actor's life, however, we believed that the participants who were introduced immediately to references to Greg's girlfriend would not entertain the idea that Greg was gay.[6]

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[6]An obvious question is why we did not simply state that Greg was gay. Pilot testing of students from this campus revealed quite strongly that many of the participants became suspicious of the purpose of the study if they read that the target was gay. More than half of the participants told the experimenter that they suspected that the study concerned their stereotypes about gay men. When we eliminated any explicit reference to Greg's sexuality, our pilot test participants did not raise these suspicions, although most of them did spontaneously entertain the thought that Greg was gay.

Dependent measures. Participants used an 11-point scale ranging from 0 (*not at all*) to 10 (*extremely*) to rate Greg's personality on each of 10 dimensions. Three of these (intelligent, funny, and boring) were included as stereotype-irrelevant fillers. The stereotype relevant traits included sensitive, assertive/aggressive, considerate, feminine, strong, creative, and passive (see Fein, Cross, and Spencer, 1995; Kite and Deaux, 1987). Assertive/aggressive and strong were reverse-coded so that for each item, higher ratings indicated greater stereotyping. An index of this set of seven traits showed moderate internal reliability (Cronbach's α = .77). It may be worth noting that these traits, when taken out of a stereotyped context, are not necessarily negative and may indeed be rather positive. But to the extent that participants perceived these traits as more descriptive of a target if they thought that the target was gay than if they thought he was straight, this would indicate stereotyping, and the valence of these traits would be debatable.

In addition, participants used the same 11-point scale to indicate the degree to which they would like Greg as a friend and the degree to which their own personality was similar to Greg's. These measures, of course, were less ambiguous in terms of valence: Lower ratings on these two measures clearly indicated more negative feelings toward the target.

Results

Recall that we predicted that if participants read information about a target that implied that he was gay, they would be more likely to evaluate this target consistently with the gay stereotype if they had received threatening, negative feedback about their performance on the intelligence test than if they had not received any threatening feedback. If the information about the target indicated that he was straight, however, the manipulation of feedback should not have had a strong effect on participants' evaluation of the target. The results supported these predictions.

Stereotyping. A two-way ANOVA on the ratings of the target on the set of seven stereotype-relevant trait dimensions revealed a significant main effect for the manipulation of feedback, $F(1, 57) = 11.3$, $p < .001$, indicating that participants who had received negative feedback on the intelligence test rated the target more stereotypically (i.e., gave higher ratings on the stereotype-consistent items) than did participants who had not received any feedback. In addition, the ANOVA revealed a significant effect for the manipulation of the target's apparent sexual orientation, $F(1, 57) = 5.3$, $p < .03$, indicating that participants who read information that implied that the target was gay rated him more stereotypically than if they read information suggesting that he was straight. Most important, the ANOVA revealed a significant interaction, $F(1, 57) = 4.4$, $p < .05$. As can be seen in Figure 32.2, and consistent with our predictions, participants who had received negative feedback and read information implying that the target was gay rated the target much more stereotypically than did participants in all other conditions,

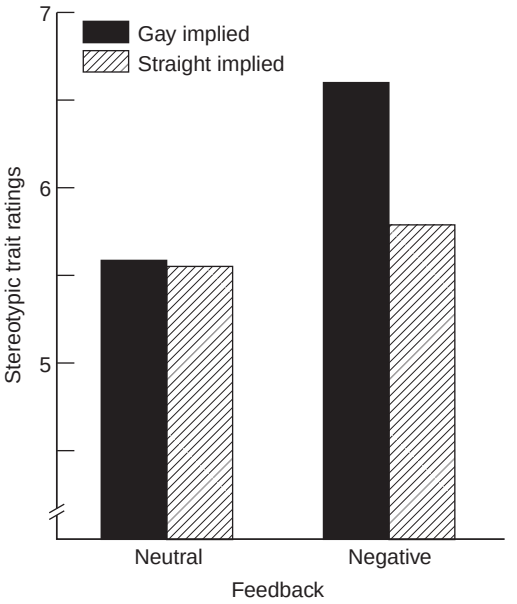

FIGURE 32.2
Rating of target on stereotype-relevant traits as a function of feedback and implied sexual orientation of the target. Higher numbers indicate greater stereotyping.

$t(57) = 4.1$, $p < .001$. None of the other conditions differed significantly from each other (see Footnote 4).

Although the stereotype-irrelevant traits were used as filler to make the participants less likely to be suspicious of the intent of our questions, we did conduct an ANOVA on the ratings concerning those traits. The independent variable did not have any significant effects on participants' ratings of the target on any or all of these traits (all Fs < 1).

Liking and Similarity. The measure of stereotyping yielded results consistent with our predictions. But would self-esteem threat also make participants less willing to indicate that they would like the target as a friend or that their own personality was similar to the target's? To address this question, we conducted an ANOVA on each of these measures.

The ANOVA on participants' ratings of the degree to which they would like the target as a friend revealed a significant main effect for the manipulation of feedback, $F(1, 57) = 5.7$, $p < .03$, indicating that participants who had received negative feedback on the intelligence test rated themselves as less inclined to like the target ($M = 5.81$) than did participants in the neutral condition ($M = 6.87$). The main effect for the manipulation of the information about the target's apparent sexual orientation did not approach significance ($F < 1$), but the interaction between the two variables was significant, $F(1, 57) = 4.1$, $p < .05$. Participants who had received

negative feedback on the intelligence test were significantly less inclined to like the target than were those who had not received the feedback, whether or not the target information suggested he was gay, but the interaction reflects the tendency for this difference to be greater in the gay-implied condition (*M*s = 5.48 vs. 6.98) than in the straight-implied condition (*M*s = 6.11 vs. 6.75).

The ANOVA on participants' ratings of how similar their own personality was to the target's revealed a significant main effect for the manipulation of feedback, $F(1, 57) = 5.3$, $p < .03$, reflecting the tendency for participants to rate their personality as less similar to the target's if they had received negative feedback on the intelligence test ($M = 4.16$) than if they had received no feedback ($M = 5.33$). The manipulation of information about the target's sexual orientation did not have a significant effect ($F < 1$). More important, the independent variable produced a significant interaction, $F(1, 57) = 4.1$, $p < .05$. Consistent with our predictions, participants were particularly unlikely to rate their personality as similar to the target's if they had received negative feedback and read information implying that the target was gay ($M = 3.94$), $t(57) = 2.3$, $p < .03$. None of the other conditions differed significantly from each other.

Discussion

Consistent with our predictions, participants showed more stereotyping in their evaluations of the target if they had previously received negative feedback about their own performance on an intelligence test. In addition to resulting in greater stereotyping, the negative feedback led participants to psychologically distance themselves from the target if they had reason to suspect that he was gay, by rating themselves as less likely to be friends with or be similar in personality to the target. If the information about the target suggested he was straight, however, the negative feedback had less effect on these measures.

These results support the hypothesis that self-esteem threat can increase individuals' likelihood of exhibiting stereotyping or prejudice toward members of stereotyped groups. Using a different stereotype, a different stereotype comparison condition (i.e., a majority rather than alternative minority group condition), and different dependent measures from those used in Study 1, Study 2 yielded results consistent with the hypothesis that self-image-maintenance processes can play an important moderating role in stereotyping or prejudice.

But does stereotyping or prejudice in response to self-image threat restore an individual's self esteem? This question was addressed in Study 3.

STUDY 3

Our view suggests that one motivation for stereotype- or prejudice-based evaluations is that these sorts of evaluations can restore a threatened self-image. Study 3

provides the first complete test of this hypothesis by examining both sides of this process: the role of a threatened self-image in causing participants to derogate a member of a stereotyped group and the role of this derogation in restoring participants' threatened self-image. Thus, an important goal of Study 3 was to provide the first evidence that negative evaluation of a stereotyped target in response to self-image threat mediates increase in self-esteem.

Participants in Study 3 took what they thought was an intelligence test. Unlike in Study 2, all participants in Study 3 were led to believe that the test was real. They received bogus positive or negative feedback.[7] After the feedback, all participants completed a questionnaire that measured their state self-esteem. In an ostensibly unrelated experiment that followed, participants evaluated a woman portrayed as Jewish or Italian, as in Study 1. Following this evaluation, participants again completed the state self-esteem questionnaire so that we could monitor changes in their self-esteem.

We predicted that (a) participants who received negative feedback would have lower state self-esteem than participants who received positive feedback, (b) participants who received negative feedback and evaluated the Jewish target would rate the target more negatively than would the participants in the other conditions, (c) participants who received negative feedback and evaluated the Jewish target would exhibit a greater increase in state self-esteem than would participants in the other conditions, and (d) this increase in state self-esteem would be mediated by their evaluations of the target.

Method

Participants. One hundred twenty-six introductory psychology students from the University of Michigan participated in this experiment for partial fulfillment of a course requirement.[8]

Procedure

Overview. Participants reported to the laboratory in pairs and were told that they would be participating in two experiments: an intelligence test and a social in-

[7]We believed that it would be difficult or impossible to provide performance feedback that would be neutral for most participants, unless as in Study 2, we did not lead the participants to believe that the test was real. An average score was quite threatening to our participants, and determining how much above average would be neutral for all participants seemed impossible.

[8]Although 126 participants participated in the experiment, 17 were excluded because they were Jewish, 7 because they were foreign students and, consequently, would have been less likely to be familiar with the stereotype about Jewish American women, 4 because they misidentified the target's ethnicity, and 2 because they did not believe the false feedback about their performance on the intelligence test. Thus, the data from 96 participants were included in the analyses.

teraction. Participants first were given an intelligence test and were given bogus feedback about their performance. They next completed a measure of their state self-esteem (Heatherton and Polivy, 1991) and were asked to indicate their score on the intelligence test, after which they were thanked for their participation, dismissed, and sent to the "social evaluation" experiment, where they was met by a different experimenter. The social evaluation experiment involved the same procedure as that used in Study 1. That is, participants received information about a job candidate who was depicted as either Jewish or Italian. After evaluating this candidate's personality and job qualifications by using the same measures as those used in Study 1, participants again completed the Heatherton and Polivy measure of state self-esteem, after which they were asked to indicate their own and the target's race and ethnicity. Finally, they were probed for any suspicions, debriefed thoroughly, and thanked for their participation.

Manipulation of feedback. When participants arrived for what was portrayed as the first experiment, they were told that the study was concerned with a new, improved form of intelligence test. The rationale and instructions were similar to but briefer than those given to the participants in Study 2. The intelligence test used in this study consisted of a longer but less difficult version of one of the tests from the battery of tests used in Study 2; the verbal–nonverbal matching test in which participants tried to match difficult vocabulary words to various pictures. This test was purported to be a very valid test of verbal and nonverbal skills. The experimenter began by giving each participant a pencil and a form commonly used for exams featuring multiple-choice questions that are graded via a computer. The test consisted of three sets of 10 words each.

The test was designed to be difficult and ambiguous enough for students to believe either positive or negative performance feedback. Some of the words were difficult or obscure for the average student (e.g., *capacious, celerity*), and some were easier (*forlorn, imminent*), but all had the feel of the kinds of vocabulary items that are included in college entrance exams and many were such that participants felt as if they may have known what they meant but could not be sure. Moreover, the match between words and pictures often was not obvious, particularly given the fast pace of the test. Pretests and postexperiment interviews confirmed that participants tended to be unsure of how they were doing during the test and to believe the feedback that was given them.

At the completion of the test, the experimenter took the participants' answers and went into an adjacent room. The door to this room was left open, and the participants could hear what sounded like a Scantron machine grading the tests. The experimenter returned each participant's answer form to him or her. The experimenter explained that a red mark appeared next to each incorrect answer, that the first number on the bottom of the form indicated the number of correct answers, and that the second number indicated the participant's percentile ranking relative to all the other students who had taken the test thus far.

The feedback was, of course, bogus. Half the participants received positive false feedback about their test performance (i.e., a high score that ostensibly put them in the 93rd percentile for the university), whereas the other half received negative fake feedback (i.e., a low score that ostensibly put them in the 47th percentile). Although the 47th percentile is close to the median, pretesting had indicated that participants uniformly found this to be very negative feedback (see also Stein, 1994).

Results and Discussion

Recall that we predicted that if participants had received threatening, negative feedback about their performance on the intelligence test, they would be more likely to derogate the target as a function of her apparent ethnicity than if they had received positive feedback about their performance. We also predicted that derogating the stereotyped target would help restore threatened participants' self-esteem. The results were consistent with these predictions.

Evaluations of the Target. Participants' ratings of the target's personality were subjected to a two-way ANOVA, which revealed strong support for our predictions. Two significant main effects emerged: Participants who had received negative feedback about their performance on the intelligence test rated the target's personality more negatively than did participants who had received positive feedback, $F(1, 92) = 9.1$, $p < .05$, and participants who were led to believe that the woman was Jewish rated her qualifications more negatively than did participants who were led to believe that the woman was Italian, $F(1, 92) = 5.2$, $p < .01$. More important, these main effects were qualified by a significant interaction between the manipulations of feedback and ethnicity, $F(1, 92) = 7.1$, $p < .01$. As can be seen in Figure 32.3, participants who had received positive feedback did not evaluate the personality of the target as a function of her apparent ethnicity, whereas participants who had received negative feedback evaluated the qualifications of the target much more negatively if she was portrayed as Jewish than if she was portrayed as Italian. The planned comparison indicated that participants who had received negative feedback and evaluated the Jewish target were significantly more negative in their evaluations of the target's personality than were participants in all other conditions, $t(92) = 4.5$, $p < .001$. None of the other conditions differed significantly from each other (see Footnote 4).

The ANOVA of the ratings of the target's job qualifications yielded a similar pattern of results. The two main effects were again significant: Participants who had received negative feedback about their performance on the intelligence test rated the target's qualifications more negatively than did participants who had received positive feedback, $F(1, 92) = 3.7$, $p = .05$, and participants who were led to believe that the woman was Jewish rated her qualifications more negatively than did participants

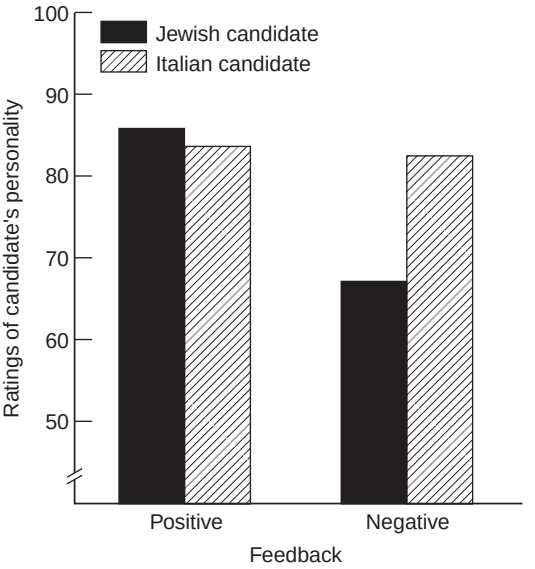

FIGURE 32.3
Rating of candidate's personality as a function of feedback and ethnicity of the candidate. Higher numbers indicate more favorable evaluations.

who were led to believe that the woman was Italian, $F(1, 92) = 6.3$, $p < .05$. Although the interaction was not significant for this measure, $F(1, 92) = 2.3$, $p < .12$, the pattern of cell means was consistent with our predictions. Participants who had received positive feedback did not evaluate the target very differently as a function of her apparent ethnicity ($M_{\text{Jewish}} = 18.8$ vs. $M_{\text{Italian}} = 19.7$), but participants who had received negative feedback evaluated the qualifications of the target much more negatively if she was portrayed as Jewish ($M = 15.3$) than if she was portrayed as Italian ($M = 19.3$). The planned comparison indicated that participants who had received negative feedback and evaluated the Jewish target were significantly more negative in their evaluations of the target's qualifications than were participants in all other conditions, $t(92) = 3.4$, $p < .001$. None of the other conditions differed significantly from each other.

These results, therefore, provide a conceptual replication of those found in Study 2 and support the generalizability of the findings by demonstrating them in the context of a different stereotype, a different kind of nonstereotyped group, and different dependent measures.

Self-Esteem. In Study 3 we measured participants' state self-esteem at two points: after the feedback manipulation and after they rated the target. The theoretical range for this scale is 20 to 100, with higher numbers indicating higher state self-esteem. As expected, feedback had a significant effect on participants' state self-esteem. Participants who received the positive feedback felt better about themselves

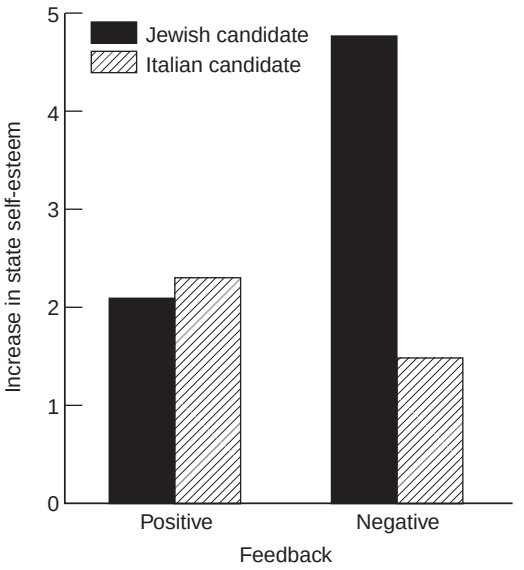

FIGURE 32.4
Change in state self-esteem as a function of feedback and ethnicity of the job candidate. Higher numbers indicate greater increase in state self-esteem.

(M = 77.5) than did those who received the negative feedback (M = 72.9), $F(1, 94) = 4.4$, $p < .05$.

The change in state self-esteem from this first measure to the measure taken after participants evaluated the target was also consistent with predictions. The ANOVA revealed a marginally significant interaction between feedback and ethnicity, $F(1, 92) = 2.7$, $p = .10$. As can be seen in Figure 32.4, and consistent with our predictions, participants who received negative feedback and evaluated the Jewish target had a significantly greater increase in state self-esteem than did participants in the other conditions, $t(1, 92)$ 2.3, $p < .05$. None of the other conditions differed significantly from each other on this measure.

These results suggest that the participants who received negative feedback and rated the Jewish woman restored their self-esteem by engaging in negative evaluation of the stereotyped target. We conducted a path analysis to test this reasoning (Baron and Kenny, 1986). Figure 32.5 depicts the results of this analysis. We allowed the planned interaction contrast to predict change in participants' self-esteem. This direct effect was significant, $\beta = .23$, $t(92) = 2.3$, $p < .05$. Next we allowed the planned interaction contrast to predict participants' ratings of the target's personality. This path was significant as well, $\beta = .42$, $t(92) = 4.6$, $p < .01$. Finally, we allowed the planned interaction contrast and participants' ratings of the target's personality to predict participants' change in state self-esteem. The path from participants' ratings of the target was significant, $\beta = .37$, $t(92) = 3.5$, $p < .01$, but the direct effect of the planned interaction contrast on participants' change in self-esteem was no longer significant,

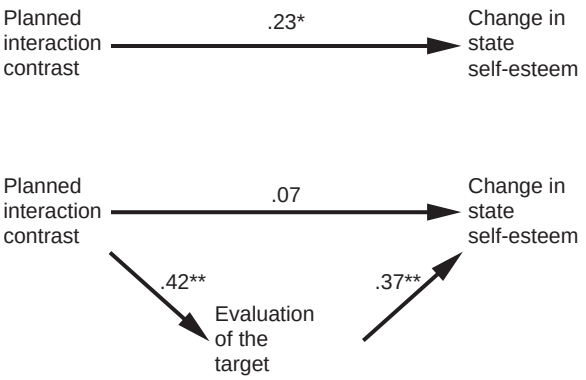

FIGURE 32.5
Change in state self-esteem as mediated by negative evaluations of the job candidate's personality. *$p < .05$. **$p < .01$

$\beta = .07$, $t(92) = 0.7$, $p > .40$. Thus, this path analysis suggests that the direct effect of the manipulations in this experiment on participants' change in state self-esteem was mediated by their evaluations of the stereotyped target's personality. These analyses suggest that the negative feedback led to increased derogation of the Jewish target, which in turn led to increased state self-esteem, rather than suggesting that positive feedback led to a reduced derogation of the Jewish target.

Taken together, these results provide the first demonstration that self-image threats, such as negative feedback, can lead to negative evaluations of a stereotyped target and that these negative evaluations, in turn, can restore people's threatened self-images. Moreover, these findings support our hypothesis that derogating a stereotyped target in response to self-image threat mediates increase in self-esteem. These results, therefore, strongly corroborate the idea that negative evaluations of a stereotyped target may often result from an effort to affirm a threatened self-image.

GENERAL DISCUSSION

This set of three studies examined evaluations of a member of a stereotyped group. Study 1 found that participants evaluated an individual target person more negatively if they thought she was a member of a stereotyped group than if they thought she was a member of a nonstereotyped group, but this effect did not occur if the participants' self-images had been bolstered through an affirmation procedure. Study 2 found that receiving self-image-threatening information led participants to evaluate an individual more stereotypically if he appeared to be a member of a stereotyped group. Study 3 demonstrated that receiving self-image-threatening information led participants to negatively evaluate an individual if she appeared to be a member of a stereotyped group, and these negative evaluations in turn were particularly effective in restoring participants' self-esteem. Moreover, the degree to which these participants made negative evaluations of the stereotyped target mediated the restoration

of their self-esteem. Taken together, this research suggests that a threat to one's positive self-image or a self-affirmation that provides a buffer against self-image threats can moderate negative evaluations of a member of a stereotyped group and that these biased evaluations can in turn affect one's sense of self-worth.

Self-Affirmation and Negative Evaluations of Others

This set of studies highlights the role of self processes in the perceptions of others. Information that threatens perceivers' sense of self-worth leads to the need to restore a positive self-image. Research by Steele and others (Steele, 1988; Steele and Liu, 1983; Steele et al., 1993) has shown that people can restore a threatened self-image in a number of ways, including by drawing on their own self-concept resources or by taking advantage of affirmational opportunities available in the situation. Steele et al. (1993) have suggested, however, that it may be difficult for people to spontaneously draw upon their self-concept resources to affirm their self-image. Therefore, people will often look to the situation to find opportunities to affirm their self-image. The studies presented here demonstrate that stereotyping or derogation of a member of a stereotyped group can provide such situational opportunities to restore a threatened self-image. Because it is likely that people often will encounter others in situations where it is personally and socially acceptable to evaluate them negatively, stereotyping and prejudice may be common reactions to self-image threat. However, when perceivers encounter someone who is a member of a group for which they do not have strong, accessible negative stereotypes, such as the woman in Studies 1 and 3 who was Italian or the man in Study 2 who apparently was straight, stereotyping or derogation is unlikely to be used as a self-affirmational strategy.

These studies also suggest that self-affirmation processes may affect a wide range of phenomena. Most of the research on self-affirmation theory has examined how self-affirmation affects cognitive dissonance processes (Steele, 1988; Steele and Liu, 1983; Steele et al., 1993). but some research has suggested that self-affirmation can also influence self-evaluation maintenance (Tesser and Cornell, 1991), learned helplessness reactions (Liu and Steele, 1986), and the academic performance of women and minorities (Steele and Aronson, 1995). The present research, in which self-affirmation affected stereotyping and prejudice, provides further evidence that self-affirmation and self-image maintenance processes have broad applicability to a wide range of important phenomena.

Relations to Other Theories

Our approach emphasizes that stereotyping others is one of several possible self-image-maintenance strategies (Steele, 1988; Tesser and Cornell, 1991). We argue that negatively evaluating others has the potential to restore a positive self-image.

Because these evaluations are part of a larger self-system that seeks to maintain an overall image of the self as morally and adaptively adequate, the state of the self-image—specifically, the extent to which it is threatened or affirmed—will influence when people will engage in stereotyping and when that stereotyping will restore a positive self-image. This approach clearly is related to other theories of stereotyping and prejudice, such as frustration–aggression, social identity, and downward social comparison. However, there are distinct theoretical differences between our approach and these approaches. In addition, the findings of the current studies support our approach and would not be predicted by these other theories.

In contrast to frustration–aggression theory, which argues that people may displace aggression by derogating others in response to blocked goals and frustrations in their life, our approach emphasizes that threats to the self-image in particular, rather than any source of frustration, lead to derogation of others. The results of Study 1 highlight this difference. Consistent with our predictions, we found that self-affirmation reduced participants' tendency to derogate a stereotyped target. It is unclear from frustration–aggression theory how a self-affirmation procedure such as that used in Study 1 would reduce frustration, unless frustration is defined more broadly than it has been in the past.

Social identity theory suggests that people favor their own groups over other groups in an effort to boost their group's status, which in turn boosts their own self-esteem. Although our approach would suggest that favoring one's own group over another group can restore one's self-image, we argue that negatively evaluating a stereotyped target can restore one's self-image even if group evaluations and in-group–out-group comparisons are not made. In the current studies there is no evidence that people are making in-group–out-group evaluations or comparisons. Both the threats and the affirmation were directed at the self, rather than at the group, and the evaluations were always of a single individual. Given that the self-affirmation manipulation in Study 1 was irrelevant to participants' group identity or status, it is unclear how social identity theory could account for the results of this study. Moreover, from a perspective that emphasizes in-group–out-group differences, one might predict that the negative feedback in Study 3 should have caused participants to derogate the Italian candidate because the Italian candidate could be considered an out-group member for most of the participants. In addition, such derogation should have been associated with a greater increase in self-esteem. The results do not support this account.

Downward social comparison theory argues that people make negative evaluations of others to bolster their self-esteem. A more precisely defined conception of downward social comparison, however, might require that social comparisons involve self–other distinctions. Our approach suggests that such self–other distinctions might indeed restore one's self-image, but negative evaluations of stereotyped others that do not involve self–other comparisons should also restore one's self-image. In the current studies there is no evidence that our participants made self–other comparisons when evaluating the targets. Moreover, even if participants

made self–other distinctions, downward social comparison theory would predict that the self-image threats should have led to derogation of all other targets, whether or not they appeared to be members of a stereotyped group. The results of our studies do not support such a prediction.

At a theoretical level, therefore, our approach is consistent in many ways with other theories, such as frustration–aggression, social identity, and downward social comparison theory, although there are some important differences. In addition, only our account can explain the set of results found in the current studies.

At an empirical level, several studies have shown that self-image threat can lead to negative evaluations of others (Brown and Gallagher, 1992; Crocker et al., 1987; Gibbons and Gerrard, 1991), and other studies have demonstrated that negative evaluations of others can lead to increased self-esteem (Brickman and Bulman, 1977; Taylor and Lobel, 1989; Wills, 1991; Wood and Taylor, 1991). Our studies differ from these previous studies by demonstrating that when people experience self-image threats, their negative evaluations of stereotyped others can mediate an increase in self-esteem. Furthermore, the current studies are the first to show that thinking about a self-relevant value unrelated to prejudice can lead to a reduction in stereotyping. Thus, the findings of the current studies support our contention that stereotypic evaluations of others can serve a self-image-maintenance function.

In our view, any negative evaluation of others—through downward social comparisons, intergroup discrimination, or stereotyping and prejudice—has the potential to serve a self-image-maintenance function. Because of the prevalence, consensual nature, and potential subtlety of negative stereotypes in particular, stereotyping and prejudice may be an especially common and effective means of self-affirmation.

The Role of Motivation in Stereotyping and Prejudice

Major reviews of the stereotyping and prejudice literature (e.g., Ashmore and Del Boca, 1981; Brewer and Kramer, 1985; Hamilton and Trolier 1986; Hilton and von Hippel, 1996; Snyder and Miene, 1994; Stroebe and Insko, 1989) acknowledge the role of motivational factors (which may be paired with or subsumed under a personality or psychodynamic approach) as one of the principal perspectives or approaches to the study of stereotyping and prejudice, along with the sociocultural and cognitive approaches. Typically, however, relatively little empirical evidence beyond research concerning psychodynamic-based constructs and theories from the 1940s and 1950s or intergroup relations and related phenomena (e.g., realistic group conflict and social identity theory) is cited in support of this perspective. The present research, along with recent examinations of the roles of affect and emotion (Esses, Haddock, and Zanna, 1994; Forgas, 1995; Islam and Hewstone, 1993; Mackie and Hamilton, 1993) and inhibition in stereotyping and prejudice (Bodenhausen and Macrae, in press; Devine, 1989; Devine, Monteith, Zuwerink, and Elliot, 1991; Monteith, 1993), examinations of the functions of stereotyping and prej-

udice (Snyder and Miene, 1994), examinations of the influence of desired beliefs on person perception (Klein and Kunda, 1992), and examinations of the roles of self esteem and collective self-esteem in intergroup perceptions and discrimination (Branscombe and Wann, 1994; Crocker et al., 1987), reflects a burgeoning interest in processes that are relevant to this underdeveloped motivational perspective.

The results of the studies reported in this article suggest that prejudiced perceptions of members of stereotyped groups can, under the appropriate conditions, help perceivers restore a positive self-image. Engaging in stereotyping and prejudice, therefore, can be an attractive way for many individuals to feel better about themselves in the absence of more readily available means of alleviating self-image threats or of affirming oneself. Given the same sociocultural context, and given the same cues and information and information-processing conditions, perceivers who are motivated to restore a feeling of overall self-worth should be more likely than other perceivers to seek out or take advantage of stereotypes.

This is not to suggest, however, that sociocultural and cognitive factors are not also critically important in the processes examined in our studies. Rather, these studies reflect an interplay of each of these factors. This is reflected in the interaction between ethnicity or sexual orientation of the target and the manipulation of self-affirmation (Study 1) or self-esteem threat (Studies 2 and 3). If the need to restore a positive overall sense of self-worth influenced prejudice independently of social–cognitive factors, then the manipulations of self-affirmation and self-threat should have resulted simply in more positive (when self-affirmed) or more negative (when the self was threatened and not affirmed) evaluations of the target individual. Rather, the manipulations of self-affirmation and self-threat significantly influenced participants' evaluations of the target only when they thought she or he was a member of a group for which there was a strong and negative stereotype, but not when they thought the target was not a member of such a group. Furthermore, evaluating the target negatively was associated with greater self-esteem boost in the former but not in the latter condition. Thus, the presence of the stereotype, stemming from sociocultural and cognitive factors, facilitated the process of derogating the target person and restoring self-esteem.

Only after recognizing the interplay among sociocultural, cognitive, and motivational factors can one adequately address the question of why derogating *any* target would not make participants feel better about themselves. In other words, if a threat to perceivers' self-esteem makes them want to restore their self-esteem, why not derogate an Italian woman if she is more available than a Jewish woman? Cognitive and sociocultural factors provide an answer to this question. Within the culture in which Studies 1 and 3 were conducted, there was a strong negative stereotype of Jewish American women but not of Italian American women. The JAP stereotype provided participants with the cognitive basis for perceiving the individual in a negative light. Similarly, the gay man stereotype provided participants in Study 2 with the cognitive basis for perceiving the individual in a stereotypical and negative light. Derogation would seem less justifiable in the absence of the

stereotype because participants' judgments would not have been biased by the stereotype. Rather than feel better about themselves, most individuals likely would feel worse if they realized that they had disparaged another person to restore their own sense of self-worth (e.g., Devine et al., 1991). Stereotypes, through social-cognitive processes such as assimilation, illusory correlations, and schematic processing, can therefore facilitate self-image maintenance, particularly to the extent that perceivers are not aware of this influence.

The Nature of Stereotyping

Most of the stereotypes that we can think of are predominantly negative. Although they are very different from each other, stereotypes about African Americans, people with disabilities, Latinos, women, Native Americans, older people, gay men, lesbians, and those low in social economic status are similar in that they are primarily negative. The current analysis provides a possible explanation for the predominantly negative character of these stereotypes. Although there are undoubtedly other mechanisms that create and perpetuate negative stereotypes (e.g., illusory correlations, out-group homogeneity, in-group bias, and social roles), our analysis suggests that stereotypes may often take on a negative character because the negativity can help restore people's self-images. When people form stereotypes about a group, they may be more likely to characterize the group in negative terms because such characterizations allow evaluations of the group that can be used for later self-affirmation. Similarly, these stereotypes may be particularly resistant to change because they can make perceivers feel better about themselves.

This analysis emphasizes the important role that motivation can play in stereotyping and prejudice. People may be more likely to stereotype others or engage in prejudicial evaluations to the extent that they are motivated to restore or enhance their self-images. Thus, understanding people's motivations may be critical in determining whether they will stereotype others, how they will stereotype others, and what form these stereotypes will take. Stereotyping and prejudice are clearly an important problem in our society. Our analysis suggests that a complete understanding of these processes and ways of mitigating them, requires an understanding of the role of the self in people's perceptions of others.

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33

The Positive Feedback Bias

Kent D. Harber

A white teacher gives feedback on a writing assignment to a black student. Will this feedback be fair? The sad legacy of racial discrimination in America, and an abundance of social psychological research demonstrating anti-minority prejudice and stereotyping (Fiske, 1998), might lead one to expect the teacher to be negatively biased—to mete out more criticism and less praise to the black student than would be given to a white student who performed similarly. However, research in my laboratory shows that the exact opposite occurs. Whites give more lenient feedback to blacks than to whites for work of equal merit, a phenomenon we call "the positive feedback bias."

The positive bias is quite reliable; it has been displayed in written and in face-to-face communications, by male and female college undergraduates, teacher trainees, and public school teachers, in feedback supposedly directed to middle school, high school, and college students, and it has been demonstrated on both American coasts and in the Midwest, (Crosby & Monin, 2007; Harber, 1998; Harber, 2004; Harber, Stafford, & Kennedy, 2010; Harber, Gorman, Gengaro, Butisingh, Ouellette, & Tsang, under review).

There are a number of competing explanations for the positive bias. One is that whites expect minorities to perform less well than fellow whites, and therefore evaluate minorities on less exacting scales (e.g., Biernat & Manis, 1994). Another is that whites feel sympathy for those who have been chronically disadvantaged, and therefore apply a "norm to be kind" (Hastorf, Northcraft, & Piucciotto, 1979) when evaluating minorities. Or, similarly, whites experience guilt when considering the harsh treatment of minorities by fellow whites, or their own anti-minority tendencies (Jones et al., 1984), and reduce this guilt through evaluative lenience.

Although such expectations and concerns certainly exist, they do not appear to cause the positive feedback bias—at least from whites to blacks. The most reliable explanation of the positive feedback bias is the whites' efforts to see *themselves* positively. Most people are highly motivated to maintain positive self-images (Tesser, 1988), which include, among other attributes, being morally good. For many whites (as well as others), egalitarianism has become a core value of their self-definition. Indeed, over the past several decades outright bigotry has itself become stigmatizing, the mark of what Goffman (1963) described as "a spoiled identity."

The positive feedback bias, we believe, is aroused by the discomfort created when the demands of feedback (to be direct, candid, and authoritative) and those of egalitarianism (to be friendly, deferential,

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and nonhierarchical) appear to conflict. Indeed, providing critical feedback to minorities can evoke the hallmarks of prejudice—asserting status and expressing negative judgments—and therefore challenge the egalitarianism many whites have embraced. The positive bias may therefore serve as a compromise between the pedagogical demands of feedback delivery and internal motives to maintain an egalitarian self-image. As a result of this compromise, whites enact the role of feedback supplier while diluting the feedback they supply.

Although the egalitarian motives driving the positive feedback bias may seem benign, the likely consequences of biased feedback to minorities are not. Feedback is one of the most important elements of instruction (Butler & Winne, 1995). It informs learners of their strengths and weaknesses, helps them understand and internalize performance standards, challenges them to produce their best work, and provides evidence of growth and improvement. However, to achieve these outcomes, feedback must be candid and accurate. If it is not, then students are misled about whether, where, and to what degree they need to focus their efforts. The positive feedback bias, if chronically displayed, may therefore contribute to the diminished challenge that profoundly undermines minority students' academic advancement (Steele, 1995).

The two articles, presented in abridged version here, describe the nature and cause of the positive feedback bias. *Feedback to Minorities: Evidence of a Positive Bias* (Harber, 1998) introduces the positive bias and provides the first empirical account of it. It also tests whether the bias is more likely aroused under conditions in which the risk of appearing prejudiced is heightened. *The Positive Feedback Bias as a Response to Self-Image Threat* (Harber, Stafford, & Kennedy, 2010), reports a direct test of whether the positive bias reflects whites' efforts to see themselves as egalitarian and nonprejudiced. The two articles also discuss the implications of the positive feedback bias for minority students and for interracial communication in general.

FEEDBACK TO MINORITIES: EVIDENCE OF A POSITIVE BIAS

Kent D. Harber

BACKGROUND

The literature on intergroup evaluation has exhaustively investigated the nature and causes of biased assessments of minority groups by whites (see Gaertner & Dovidio, 1986; and Devine, 1989, for reviews). Much of this research shows a propensity among whites to judge minority targets negatively (e.g., Henderson-King & Nisbett, 1997; Lambert, Cronen, Chasteen, & Chanowitz, 1996), a pattern consistent with abundant demonstrations of negative stereotypes and unsympathetic attitudes toward minorities (Fiske, 1998). However, in nearly all studies of interracial evaluation, white reviewers supply their judgments of minority persons to research staff or to some other third party. Situations of interracial feedback, in which whites evaluate the performance of a minority person and then communicate their assessments directly back to this person, have received scant empirical review.

Unlike the third-party evaluations that dominate the research on out-group prejudice, evaluations communicated in a feedback encounter may be biased in the op-

posite direction, such that whites are selectively lenient when criticizing minorities. This is because of social motives that, in kind or in degree, distinguish feedback delivery from other evaluation modes. These motives include whites' wishes to display egalitarian values to others (Carver, Glass, & Katz, 1978) or to themselves (Devine, Monteith, Zuwerink, & Elliot, 1991; Dutton & Lake, 1973). Interracial feedback may also be informed by a "norm to be kind" (Hastorf, Northcraft, & Piucciotto, 1979) or by sympathy motives (Jones et al., 1984) which dissuade the nonstigmatized from directly criticizing members of disadvantaged groups. Finally, feedback encounters with minorities may arouse in whites feelings of generalized awkwardness (Stephan & Stephan, 1985) or discomforting ambivalence (Katz, 1981). In such situations whites may mask underlying unease with overtly positive communications (Jones et al., 1984).

There are also automatic processes that might interfere with feedback delivery. For example, whites' negative stereotypes of minority persons' capabilities may cause whites to shift their evaluative standards down (Biernat & Manis, 1994) when supplying interracial feedback. Similarly, high standards applied to fellow whites may lead to expectancy effects (Bettencourt, Dill, Greathouse, Charlton, & Mulholland, 1997), such that whites judge members of their own group more harshly than members of other groups for work considered substandard.

Collectively, these various social and social-cognitive influences may cause whites to place a positive bias on their feedback to blacks. The present research attempted to demonstrate that such a bias exists. In two experiments, white participants were asked to review a poorly written essay supposedly composed by either a black or a white fellow student. Participants were led to believe that their comments would be returned directly to the writer. Feedback directed toward a "black" writer was predicted to be more positive than feedback directed toward a "white" writer.

In addition to demonstrating the presence of a positive feedback bias, this research also attempted to identify limiting conditions that determine when the bias occurs. It did so by examining differences in feedback directed toward subjective and objective areas of evaluation. In regards to writing, subjective evaluation applies largely to the *content* of composition (e.g., strength of argument and originality of ideas). Objective aspects of writing address the *mechanics* of composition (e.g., grammar, spelling, and usage).

Because content lacks established standards of evaluation, while also presenting the risk of implying ad hominem attacks, it may be less "socially judgable" (cf. Yzerbyt, Schadron, Leyens, & Rocher, 1994) than mechanics. That is, it may lack the standards and norms that insulate judges from untoward appearances. The research on whites' interracial concerns, cited above, suggests that whites are likely to exercise particular caution when supplying interracial feedback in a less socially judgable domain. For these reasons the positive bias was expected to be selectively robust in content-related feedback to black essay writers.

OBJECTIVES

The primary purpose of this study was to demonstrate that feedback from whites to blacks is positively biased. A poorly composed writing sample was therefore predicted to be more favorably evaluated when its author was identified as black rather than white. Additionally, this study was designed to detect an interaction between writer race (black vs. white) and evaluative domain (content vs. mechanics), revealing selective lenience in content-related feedback to a black writer.

METHODS

Participants

Ninety-two white undergraduate psychology students (44 men and 48 women) participated in this study. Participants were seen individually in hour-long sessions.

Stimulus Materials

Substandard Essay. Participants reviewed one of two editorial-style essays that were, by design, filled with grammatical errors and content flaws. Two separate essay topics (television violence; interest in the environment) were developed to control for essay theme.

Writer Demographics Sheet. This biographical survey, supposedly completed by the essay writer, was used to unobtrusively introduce the writer's race. There were two versions of this form, which were identical in all respects except an entry concerning campus affiliations. In the *black writer* condition, the entry was "Black Students' Union," and in the *white writer* condition, "None."

Procedure

Cover Story. Participants were told that the research was designed to explore peer tutoring, and that they would critique an essay written by a fellow student. Participants were informed that the reviewed essay, along with their copyedits and comments, would then be returned to the essay writer who was expecting to receive the participants' feedback.

Writer Race Manipulation. Before participants began their essay critiques they were supplied either the *black writer* or the *white writer* version of the writer demographics sheet. The experimenter then left the room with final instructions that the participants should take as much time as needed to conduct the essay review, and to notify the experimenter when the critiquing was done.

Dependent Measures. Participants' copyedits constituted the first outcome measures of this study. After supplying their essay critiques, the participants were next asked to fill out a brief Writer's Evaluation Form. This form asked participants to rate how much added work the writer should do to improve the content, mechanics and overall quality of the essay. Participants sealed the completed form in an envelope addressed to "Writer 46," and believed it would go directly to the writer and to no one else. These steps were intended to reduce social desirability concerns.

RESULTS

This study attempted to reveal an interaction between writer race and evaluative domain (i.e., content vs. mechanics). This interaction was predicted to occur both in the Writer's Evaluation Form ratings that participants believed were being transmitted directly from them to the essay writer, and in the participants' copyedits. The interaction was assessed by using a mixed design repeated measures ANOVA, with evaluative domain (content vs. mechanics) as the within-participants factor, and writer race as the between-participants factor.

Preliminary Analyses

There were no differences due to participant sex or essay topic, and data were collapsed across these variables. Two participants failed to correctly identify writer race, and their data were omitted.

Suspicion did not significantly differ by experimental condition, F (1, 88) = 1.27(p = .26) or by participant sex, F (1, 88) = 1.51 (p = .23).

Main Analyses

Writer's Evaluation Form. The Writer's Evaluation Form consisted of three rating scales on which the participants indicated how much additional work the writer should dedicate to essay mechanics, essay content, and overall quality. Data from this measure verified the predicted race by evaluative domain interaction, F (1, 88) = 14.62 (p = .004) (see Figure 33.1). Simple effects tests show that essay content was rated more favorably when the supposed author was black rather than white, F (1, 88) = 9.59 (p = .003). In contrast, ratings of essay mechanics did not significantly differ as a function of writer race F (1, 88) = 0.53 (p = .46). Ratings regarding the overall quality of the essay did not differ as a function of writer race, F (1, 88) = 1.00 (p = .31).

Essay Comments. Review of participants' copyedits also demonstrated the predicted interaction between writer race and evaluative domain (content vs

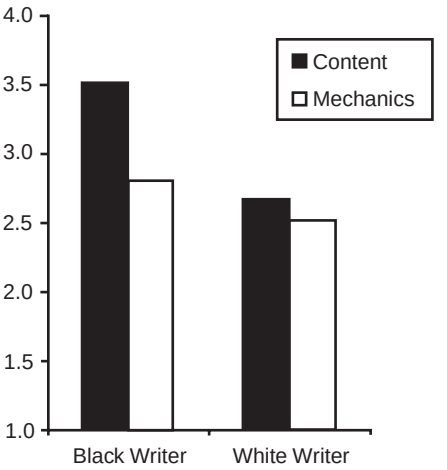

FIGURE 33.1.
The favorableness of gradelike ratings transmitted to the supposed essay writer, as a function of the supposed writer's race and evaluative domain (content vs mechanics). Note that ratings have been reverse-coded for purposes of interpretability.

mechanics), F (1, 88) = 5.70 ($p < .03$). This interaction is displayed in Figure 33.2. Simple effects tests show that participants in the *black writer* condition rated essay content more favorably ($M = .85$) than did participants in the *white writer* condition ($M = .14$); t (89) = 3.67 ($p < .001$). In contrast to feedback on content, comments regarding *black writer* mechanics ($M = .26$) did not significantly differ from those regarding *white writer* mechanics ($M = .22$); t (89) = .15 ($p = .87$).

The separate contributions of positive and negative content comments were jointly explored. This revealed an interaction between writer race and the valence of criticism, F (1, 88) = 8.40 ($p = .006$). One-way ANOVAs show that participants in the *black writer* condition supplied both more positive content comments (M = 2.09 vs M 1.04), F (1, 87) = 4.74 ($p < .04$) and fewer negative content comments ($M = 1.04$ vs $M = 2.26$), $F = 5.65$ ($p < .03$), than did participants in the *white writer* condition. The bias therefore appears to reflect both efforts to embellish praise, as well as to avoid criticism.

Summary Comments. Nearly half of the participants (46%) concluded their essay critiques with informal notes to the writer, even though there was no instruction for them to do so. Comments to a black writer were more supportive in tone than were those to a white writer, F (1, 53) = 6.46 ($p = .02$). Participants in the *black writer* condition tended to say how much they enjoyed reading the essay or how much potential they saw in it. Participants in the *white writer* condition, in contrast, could be quite harsh, such as the one who wrote,

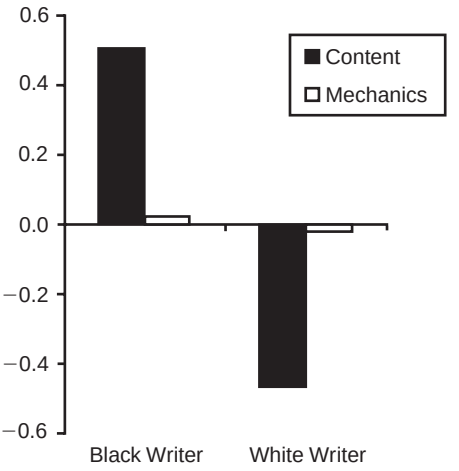

FIGURE 33.2.
The favorableness of copyedits as a function of the supposed writer's race and evaluative domain (content vs mechanics). Note that favorableness is a composite score composed of positive comments minus negative comments.

"When I read college work this bad I just want to lay my head down on the table and cry." Such baldly negative comments were never made by participants in the *black writer* condition.

Cumulatively, these results confirm that whites supply more positive feedback to blacks than to fellow whites, and that this feedback bias is selective for essay content.

A second study (Study 2), which closely followed the design and procedures of this study (Study 1), was conducted to establish the reliability of the positive bias. Results of Study 2 were consistent with those of the original study.

DISCUSSION

This research tested the prediction that whites supply more lenient feedback to blacks than to fellow whites for work of equal quality. Results confirmed that the positive feedback bias exists. In two separate experiments, white undergraduate students gave more favorable feedback on poorly written essays when the author was described as black rather than white.

These studies also attempted to identify the conditions under which the bias is expressed. As expected, the feedback bias affected the evaluation of a subjective evaluative domain (i.e., essay content), but had no influence on an objective evaluative domain (i.e., essay mechanics). Content-related feedback to a black writer received more praise and less criticism than did content-related feedback to a white

writer. The interaction between writer race and evaluative domain was evident in participants' copyedits and in their grade-like ratings to the writer.

The positive bias was predicted to selectively affect content, and be insensitive to mechanics, for two complementary reasons (cf. Shaughnessy, 1976). Content criticisms, which address writers' quality of reasoning and coherence of beliefs, can suggest personalized attacks. Mechanics criticisms, which deal with the more neutral topics of spelling and grammar, are less likely to raise such ad hominem implications. Second, there are few objective rules or standard referents that justify content criticisms, while such guidelines do exist for evaluating mechanics (e.g., dictionaries and style books). The liabilities of criticizing content may arouse whites' intergroup concerns (e.g., of appearing racist to others, or of violating their own egalitarian values), leading whites to temper feedback to blacks.

Alternative Explanations for the Feedback Bias

Social-cognitive approaches have also identified circumstances in which whites will display evaluative lenience toward blacks. According to "shifting standards" research (Biernat & Manis, 1994) evaluations of stereotyped groups are calibrated to group-based stereotypes. Thus, whites' negative stereotypes about blacks' verbal skills should cause whites to employ less exacting standards when reviewing blacks' verbal performances; they lower the bar for what constitutes good writing.

A key aspect of the shifting standards approach is that its expression is limited to subjective metrics (e.g., very poor, very good), which permit within-group comparisons (e.g., a target black in relation to all other blacks), but not objective scales (e.g., 70% correct, 90% correct), which force raters to evaluate in-group and out-group members using a common measurement scale. The present research also distinguished between subjective and objective evaluations. However, there is a critical difference between the feedback studies and the shifting standards research in how subjective and objective criteria are defined. In the feedback studies, the terms "subjective" and "objective" refer to the performance being evaluated (e.g., reasoning vs grammar), while in the shifting standards research these terms refer to metrics used to judge performance (e.g., qualitative ratings vs quantitative ratings). Thus, the evaluation of essay content (a subjective evaluation domain) was positively biased on both subjective ratings (e.g., the Writer's Evaluation Form ratings) and objective ratings (e.g., the number of positive copyedits). In contrast, evaluation of essay mechanics (an objective evaluation domain) did not show the bias on either subjective ratings or on objective tallies.

Research conducted to test the "out-group polarization theory" (Linville & Jones, 1980) has shown that people accentuate their evaluations of out-group members, exaggerating both the outsider's successes and failures. White participants in the feedback studies exaggerated their ratings of blacks' written work, but not in the direction out-group polarization would predict. Substandard essays

supposedly authored by a black writer were consistently reviewed more *favorably* than were essays supposedly written by a white person. According to polarization theory, the essays' poor quality should have led to increased criticism, rather than elevated praise, of black writers' performances.

Expectancy violation theory (Bettencourt et al., 1997) would predict whites to more harshly criticize fellow whites than blacks, especially when evaluating subpar work such as the essays used in this study. However, expectancy violation theory and its demonstrations are limited to affective and global judgments (e.g., how one feels about the person being evaluated); they fail to account for judgments relating to specific skills or qualifications (Bettencourt et al., 1997). In contrast, the feedback studies focused exclusively on judgments of actual performance. Additionally, expectancy violation does not explain why the feedback bias selectively applies to subjective evaluations (i.e., essay content).

Why do results in the feedback studies contrast with these other approaches? A likely explanation is that the feedback studies introduced a level of social consequence that these more cognitive paradigms did not present. Feedback study participants believed that they were communicating their essay criticisms directly to the essay authors rather than to an emotionally disinterested experimenter. This may have made salient one or more of the intergroup concerns that cause whites to approach blacks with extra caution and conciliation, such as appearing less egalitarian to others or even to just themselves.

Educational Costs of Biased Feedback

The positive feedback bias may present serious costs for minorities. Excessive praise and insufficient criticism may send minority students down a primrose path of inflated expectations or dissuade them from trying to redeem substandard efforts (see Massey, Scott, & Dornbusch, 1975, for elaboration). Biased feedback may also deprive minority students of the mental challenge that educators (e.g., Sommers, 1982) cite as critical for intellectual growth. Steele (1992, 1995) notes that black college students, in particular, are subject to low expectations and insufficient challenge, and that they suffer both academically and psychologically as a result.

Distrust of positive feedback, even when the feedback is valid, presents a corollary cost of a positive feedback bias. Crocker, Voelkl, Testa, & Major (1991) found that blacks may be wary of whites' praise, so much so that the receipt of positive evaluations from whites can depress their self-esteem. Significantly, the depressing effects that praise from whites had on Crocker et al.'s black participants occurred only when these participants received feedback from a white person who was supposedly aware of their racial identity. Blacks who received feedback from a white in a race-blind condition showed elevated self-esteem. Crocker et al. believe that black participants in the "race-aware" condition regarded praise as a sign of the

white feedback supplier's racial concerns, rather than as reliable testimony to the quality of their own efforts.

This catalogue of potential liabilities suggests that a positive feedback bias may lead to negative consequences for minorities. Although the present demonstration was conducted in an educational context, the bias may also arise in work settings, social gatherings, or any other circumstances in which intergroup evaluations occur. For these reasons it is important to learn more about the bias and to determine how it can be addressed.

THE POSITIVE FEEDBACK BIAS AS A RESPONSE TO SELF-IMAGE THREAT

Kent D. Harber, Reshma Stafford, and Kathleen A. Kennedy

BACKGROUND

As we have seen, feedback from whites to blacks can be positively biased, such that whites sometimes give more praise and less criticism to blacks than to whites for work of equal merit. The initial positive feedback studies provided strong evidence that the bias does indeed exist, and indicated some of its limiting conditions. For example, the positive bias was limited to subjective judgments (i.e., of writing content) where the risk of being seen as prejudiced was high, but it had no effect on objective judgments (i.e., of writing mechanics) where this risk was low (Harber, 1998; 2004). The positive bias occurred in face-to-face interactions (Harber, 2004), but only among whites who encountered an unfriendly minority recipient (whose unfriendliness might signal an interracial offense). Feedback encounters with a friendly minority recipient, or a white recipient, regardless of friendliness, did not produce the positive bias. These aspects of the positive feedback bias provide clues to the cause of the bias, but do not fully identify its cause. The present research aims to do so.

Determining the cause of the positive feedback bias will address a paradox that is gaining increased attention in intergroup relations research. This is the disruptive anxiety that whites often experience when interacting with minorities (Stephan & Stephan, 1985), and the heightened deference to minorities (i.e., exaggerated friendliness, agreement, and solicitousness) that often coincides with this anxiety. For example, whites display a classic physiological threat response to black but not to white interaction partners, but then report greater levels of liking for and agreement with black partners than white partners (Mendes, Blascovich, Lickel, &

Hunter, 2002). Furthermore, the more emotional discomfort whites experience when engaging with a minority person, the more positively they rate this person (Littleford, Wright, & Sayoc-Pariel, 2005).

One explanation for this out-group deference is that it arises from whites' motives to see themselves as socially tolerant, and their aversion to seeing themselves as prejudiced. In this case, whites' heightened deference to minorities would advertise egalitarian values inwardly, and thereby affirm to whites their own egalitarian self-images (Dutton & Lake, 1973; Gaertner & Dovidio, 1984). If such self-image maintenance concerns (e.g., Tesser, 1988) motivate out-group deference, then whites' interactions with minorities will be determined, at least partially, by whites' confidence in their own egalitarianism. Thus, those less confident should be more likely to show the positive feedback bias, and those more confident should be less likely to show the bias. The present study tested whether egalitarian self-image concerns drive the positive feedback bias, and if they do so even in the absence of external social-desirability pressures.

OBJECTIVES

To demonstrate that egalitarian self-image motives cause out-group deference in general, and the positive feedback bias in particular, it is necessary that the audience to which the whites perform is exclusively themselves. The present research attempted to produce these conditions within the context of instructional feedback. White teacher trainees were first induced to express attitudes that were sympathetic, hostile, or unrelated toward minorities, and believed that they did so anonymously. Trainees then gave anonymous feedback, they believed, to either a black or a white student. If self-image motives drive the positive bias, then feedback to a black student should be positively biased among trainees who privately impugned their own egalitarianism (by expressing negative attitudes toward minorities). However, there should be little or no bias among trainees who privately affirmed their own egalitarianism (by expressing positive attitudes toward minorities).

The study also used a design that vigorously reduced social desirability concerns and left self-judgments as the principle motivator for the positive bias. It did so by leading participants to believe that the attitudes toward minorities they were induced to express (whether pro-, neutral, or anti-minority) were anonymous, that their feedback was anonymous, and that only they, the participants, could connect their minority-related attitudes to their subsequent feedback.

We recruited students from the Rutgers University School of Education, a leading teacher training program. These trainees often get jobs at schools serving minority students. If the teacher trainees show the bias, and do so in order to protect their egalitarian self-images, then this would have important implications for minority students and for teacher training.

METHODS

Participants

A total of 108 white teachers-in-training (83.2% women; age $M = 21.32$; $SD =$ 04.68) participated in this study. Nearly half the trainees (44.4%) had previous teaching experience, averaging approximately 2.2 years. Teaching students ("trainees") completed the study individually in one-hour sessions and received $15.00.

Procedure

Trainees were told that undergraduates in a writing workshop had volunteered writing samples to get the trainees' feedback. Trainees received a poorly written two-page essay, supposedly drafted by their assigned student but actually prepared by experimental staff. Trainees were told that their job was to make comments on the essay draft, which would be returned to the writer.

Egalitarianism manipulation. After delivering the cover story, the experimenter rifled through his/her clipboard, looked chagrined, and asked the trainee if he or she had completed the *Social Issues Survey.* After the trainee responded negatively, the experimenter produced a one-page *Social Issues Survey.* The experimenter explained that the survey, which was being developed by an unrelated research team, should have been completed upon arrival. The trainee was asked if he or she would be willing to complete it now. The trainee was told not to write his or her name on the survey, and to deposit the completed survey through a slot in a locked collection box that the other researchers would retrieve later. Thus, trainees thought they were completing a measure that was totally anonymous, unrelated to the feedback study, and inaccessible to the feedback study staff.

The *Social Issues Survey* manipulated trainees' concerns about their egalitarianism, and partially followed a design employed by Monin and Miller (2001). The three versions of this survey all contained nine opinion-rating questions. The egalitarian boost version asked questions such as whether "Government offices should be closed on Martin Luther King Day" and whether "The Confederate flag should not fly over government buildings." Positive responses to these pro-minority questions, which most trainees were likely to supply, would cumulatively reinforce these trainees' egalitarian self-images. The egalitarian threat survey, designed to elicit opinions unfavorable to minorities, asked questions such as whether "It should be legal for businesses to be open on Martin Luther King Day," and whether "People should be allowed to fly the Confederate flag on their own front lawns." The civil libertarian framing of these questions (i.e.,

"should be legal," "should be allowed") pushed the egalitarian threat trainees to endorse items that might be palatable individually but that collectively had an anti-minority thrust. The neutral condition survey, which focused on shopping, was irrelevant to egalitarian self-image. It included items such as "My supermarket sells a wide variety of canned goods."

An additional question asked trainees in the egalitarian boost and the egalitarian threat conditions to list, from categories that varied by condition, five African American exemplars. In the boost condition, this item read "Please list five famous African Americans in music, poetry, literature, government, or sports." Trainees were expected to easily identify five African Americans in these categories and infer from their ease of recall (per Winkielman & Schwarz, 2001) their appreciation of minority achievement. Threat condition trainees were asked to list five famous African Americans in the visual arts, sociology, psychology, life sciences, physical sciences, or math. Most people would be pressed to name exemplars of any ethnic group within these fields. However, difficulty naming five African Americans was expected to challenge egalitarianism. Trainees in the neutral condition listed five places where they shop.

Essay Review. After completing the *Social Issues Survey,* trainees engaged in the essay feedback task. The essays (on either television violence or environmentalism) were filled with mechanical and content errors, and were of equal length and style.

Race Cue. Before reviewing their assigned essays, trainees examined a Writers' Demographics Sheet (adapted from Study 1).This form, supposedly completed by the essay writer, subtly indicated the writer's race by identifying her as Natisha K. (black) or Heidi K. (white), and by indicating membership in the "African American Heritage Organizing Committee" in the *black writer* condition.

Writer's Evaluation Form. After copyediting their assigned essays, trainees were given the Writer's Evaluation Form which was described as a direct, confidential, and anonymous communication between themselves and the writer. The Writer's Evaluation Form included ratings on essay content, mechanics, and how many hours per week the writer should devote to developing writing skills. Trainees sealed their unsigned Writer's Evaluation Form in an envelope addressed to "Writer 26."

Selective Bias for Essay Content. In accord with previous positive feedback bias studies (Harber, 1998, 2004), we predicted that the positive bias would be found in ratings of essay content (ideas, beliefs, reasoning), but not in ratings of mechanics (grammar, spelling, punctuation). Content criticisms are more subjective and less easily justified than mechanics criticisms (which draw upon objective standards), and are more interpersonally consequential (people typically value

their ideas more than their spelling). These features make content criticism more subjectively risky, and therefore more susceptible to self-image concerns that are predicted to drive the bias.

RESULTS

We predicted that feedback positivity would be jointly influenced by recipient race (black or white writer) and trainees' egalitarian self-image (boosted, unchanged, or threatened). This predicted pattern was tested using a planned contrast (Rosenthal & Rosnow, 1985). The *black/boost, white/boost, white/neutral,* and *white/threat* conditions, which were expected to supply similarly critical feedback, were assigned weights of −1. The *black/neutral* condition, which was expected to show some evidence of the positive bias, was assigned a weight of +1, and the *black/threat* condition—in which the positive bias was expected to be distinctly strong—was assigned a weight of +3. This formula was applied to all outcomes.

Writer's Evaluation Form. Trainees indicated on this form how much additional work the essay required in regard to writing content and mechanics. Results showed, as expected, that the joint effect of writer race and egalitarian self-image on content ratings was significant, and in the predicted manner, $t\ (97) = -3.67$ ($p < .001$) (see Figure 33.3). Tukey post-hoc tests show that trainees in the *black/threat* condition rated essay content as requiring less revision than did trainees in the *black/boost* condition, the *white/neutral* condition, and (marginally) the *white/-*

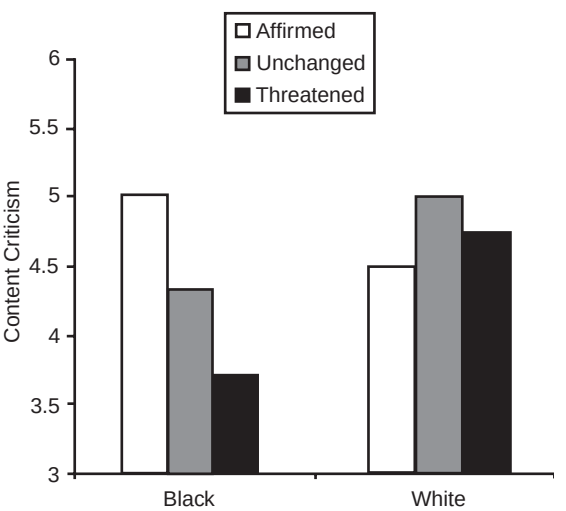

FIGURE 33.3.
Criticism of essay content, as a function of writer race and egalitarian self-image.

threat condition. The joint effect of writer race and egalitarian self-image condition on mechanics ratings was, as predicted, negligible, $t\,(100) = -0.28$ ($p = .783$).

Trainees in the *black/threat* condition also advised the writer to spend fewer hours developing writing skills than did other trainees, $t\,(97) = -2.70$ ($p = .008$) (see Figure 33.4). Furthermore, the amount of time recommended by the *black/threat* trainees ($M = 2.82$; $SD = 1.57$) was roughly one third less than that recommended by trainees in the three white writer conditions, together ($M = 4.22$; $SD = 1.97$; $p = .032$).

Copyedits. The index of comment positivity showed that copyedits from trainees in the *Black/threat* condition were more favorable than those supplied by other trainees, $t\,(98) = 3.13$ ($p = .002$). Copyedits from trainees in the *Black/threat* condition ($M = 1.34$, $SD = 2.47$) were more favorable than those from trainees in the three white writer conditions, analyzed as a single group ($M = -0.50$; $SD = 2.15$; $p = .049$).

The proportion of buffering comments (which mitigate criticism through apology or qualification) was also selectively high in the *black/threat* condition, $t\,(100) = 3.19$ ($p = .002$). The most marked disparity in buffers was between trainees in the *black/threat* condition and those in the *black/boost* condition ($p = .077$). *black/threat* trainees also produced twice the rate of buffers ($M = .077$; $SD = .081$) than did trainees in the three white writer conditions, analyzed as a single group ($M = .039$; $SD = .034$; $p = .024$).

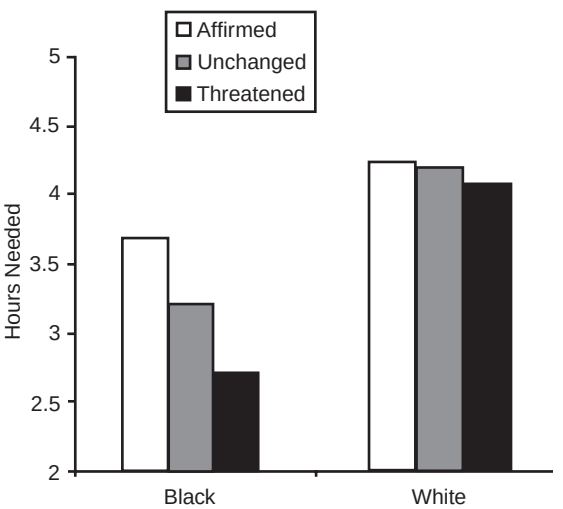

FIGURE 33.4.
The number of hours per week the study participants think the writer should devote to writing skills, as a function of writer race and egalitarian self-image.

DISCUSSION

Ideally, feedback suppliers concentrate their attention on learners; on their strengths and weaknesses, and on their anxieties and their enthusiasms. Instructors monitor themselves, if at all, to convey feedback more effectively (Lepper, 1988). The present research tested whether this necessary focus on learners is subverted when feedback suppliers are white and feedback recipients are black. It predicted that rather than attending solely to the needs of minority learners, white feedback suppliers may be distracted by their own self-image needs, causing them to positively bias their feedback to minorities.

This prediction was confirmed. White teacher trainees whose egalitarian self-images were threatened, and who believed that their feedback recipient was black, gave selectively more favorable essay content ratings, recommended less time for developing writing skills, supplied more positive copyedits, and laced their comments with more equivocating buffers. In contrast, trainees in the *black writer* condition who had an opportunity to first affirm their egalitarianism did not display the positive bias on any of these measures; their feedback was virtually indistinguishable from feedback supplied by trainees in *white writer* conditions, who themselves were unaffected by the self-image manipulation. In effect, the trainees' positive feedback bias to blacks was "turned on" when their self-image concerns were aroused, and "turned off" when self-image concerns were allayed.

Connecting the positive bias to internal self-image motives is difficult because the conditions that arouse these internal motives can also introduce external social desirability concerns (MacDonald & Nail, 2005). To address this problem, we led trainees to believe that (1) the *Social Issues Survey* (that manipulated egalitarian self-images) was anonymous, was unrelated to the feedback study, and was not viewed by feedback study staff; and (2) that trainees' feedback was an anonymous, exclusive communication between just themselves and their student. Yet the seemingly private self-knowledge of having either compromised or confirmed their egalitarian scruples was sufficient to positively bias anonymous feedback to minorities, or to prevent such bias, respectively.

Implications for Minority Schooling

Chronic expression of the positive bias may dilute the academic challenge that is of special importance to minority advancement (Steele, 1995). It might also mislead minority students about their levels of mastery, and about how much effort mastery requires (Massey, Scott, & Dornbusch, 1975). The white teacher trainees (most of whom are now teachers) in the Harber et al. (2010) study are probably similar to many if not most new teachers. Their susceptibility to self-image con-

cerns suggests that the positive feedback bias occurs in public schools. In fact, Harber et al. (under review) shows that the actual teachers express the positive bias to both blacks and to Latinos. Thus, many minority students may contend with the obscured information, diminished challenge, and propensity to be demoralized, which the positive feedback bias is likely to create.

Does the positive feedback bias actually cause harm? No research has yet directly tied the positive feedback bias to minority students' performances. However, related research suggests that the positive bias is injurious to minority students. Black college students frequently display a lack of self-knowledge about their strengths and weaknesses. They often overestimate their performance on academic tasks (Steele & Aronson, 1995), have unrealistic expectations of academic and career successes (Rowser, 1997), and can oscillate greatly in their academic self-efficacy—riding high on successes and plummeting after failures (Aronson & Inzlicht, 2004). Aronson and Inzlicht (2004) associate these liabilities to minorities' ambivalent experiences with feedback, which is augmented by stereotype threat (i.e., reactivity to and personalization of negative stereotypes about one's group).

Stereotype threat, say Aronson and Inzlicht, can exacerbate minorities' distrust and consequent discounting of feedback from whites (per Crocker & Major, 1998). This discounting can protect minorities' self-worth in the short term, but at the cost of depriving them of valid performance information. At the same time, stereotype threat makes minorities' academic self-efficacy more variable (more reactive to both successes and failures; Aronson & Inzlicht, 2004), leading minorities to be overly influenced by the feedback they do accept, be it positive or negative.

Minority students' difficulty in correctly calibrating their efforts and outcomes (Aronson & Inzlicht, 2004) and white instructors' difficulty in supplying reliable responses to minorities, might lead to a "perfect storm" of failed feedback. Those most in need of valid, candid feedback either discount it or are overly sensitive to it, and those who deliver feedback to minorities often do so in a positively biased manner. Ironically, this situation is apparently fueled by complementary anxieties; minorities' concerns about being devalued because of negative stereotypes, and whites' concerns about subscribing to such stereotypes.

By identifying self-image concerns as a cause of the positive bias, this research may provide an important first step in remedying it. Self-image concerns are fungible (Steele, 1988); if depressed in one domain (e.g., likeableness), self-images can be supplemented in others (e.g., professionalism). This suggests that shoring up teachers' feelings of self-worth in ways unrelated to their egalitarianism may inure them to the self-image concerns that produce the positive bias. Indeed, classroom teachers with more social support from fellow teachers and from school administrators are less likely to show the positive feedback bias than are teachers lacking such support (Harber et al., under review).

Egalitarian Self-Image Motives and Intergroup Relations

Whites' motives to maintain egalitarian self-images, as displayed in this research, might contribute to the anxiety whites display during encounters with minorities in general (Mendes et al., 2002; Trawalter & Richeson, 2008), and to the expressions of heightened out-group friendliness that this increased anxiety appears to produce (Littleford, Wright, & Sayoc-Parial, 2005). One result of this anxiety is that it may deter whites from engaging with minorities altogether (Jones et al., 1984). When interracial engagement does occur, egalitarian self-image concerns might inhibit whites from the candid disclosures that foster lasting social bonds (Derlega & Berg, 1987), producing instead polite and mannered encounters that fortify rather than relax social barriers.

The out-group deference arising from self-image concerns can place minorities in perplexing social dilemmas. How do they know whether whites' friendliness is in response to them, as individuals, or to the self-image threat that their minority status arouses in whites? How do they directly address this problem without exacerbating it? Whites, too, face dilemmas—for example, how do they simultaneously remain conscious of their own biasing motives and attitudes, while at the same time not behaving self-consciously?

CONCLUSIONS

This research shows how a preoccupation with unbiased self-images can lead to biased interracial feedback. However, these findings do not discount the importance of genuine egalitarian motives and beliefs. Instead, they suggest that for whites, as for minorities, intergroup contact can present self-image risks that deter the authentic, non–self-conscious communications through which true egalitarianism is displayed. Rather than focusing on compromised dispositions, it might be more productive to cultivate social situations in which both minorities and whites are taken at face value.

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VIII

LIKING, LOVING, AND INTERPERSONAL SENSITIVITY

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34

Lending a Hand: Social Regulation of the Neural Response to Threat

James A. Coan, Hillary S. Schaefer, and Richard J. Davidson

Social contact promotes enhanced health and well-being, likely as a function of the social regulation of emotional responding in the face of various life stressors. For this functional magnetic resonance imaging (fMRI) study, 16 married women were subjected to the threat of electric shock while holding their husband's hand, the hand of an anonymous male experimenter, or no hand at all. Results indicated a pervasive attenuation of activation in the neural systems supporting emotional and behavioral threat responses when the women held their husband's hand. A more limited attenuation of activation in these systems occurred when they held the hand of a stranger. Most strikingly, the effects of spousal hand-holding on neural threat responses varied as a function of marital quality, with higher marital quality predicting less threat-related neural activation in the right anterior insula, superior frontal gyrus, and hypothalamus during spousal, but not stranger, hand-holding.

Social bonding and soothing behaviors mitigate the destructive effects of negative environmental events and promote enhanced health and well-being (Berscheid, 2003). Indeed, social isolation is now considered a major health risk (House, Landis, & Umberson, 1988). Moreover, married people tend on average to be happier and healthier than unmarried people (Wood, Rhodes, & Whelan, 1989), and among married individuals, higher marital quality is associated with decreased risk of infection, faster recovery from injury, and a lower rate of mortality following a diagnosis of life-threatening illness (Coyne et al., 2001; Robles & Kiecolt-Glaser, 2003).

The likely mechanism underlying these effects is the social regulation of emotional responding (Diamond, 2001; Hofer, 1984). Theorists have long argued that relationships serve security-provision and distress-alleviation regulatory functions that influence negative affect and arousal (Bowlby, 1969/1982; Mikulincer, Shaver, & Pereg, 2003). Supportive social behaviors are known to attenuate stress-related activity in the autonomic nervous system (ANS) and hypothalamic-pituitary-adrenal (HPA) axis (DeVries, Glasper, & Detillion, 2003). Maternal grooming behaviors

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even affect glucocorticoid-receptor gene expression underlying hippocampal and HPA-axis stress reactivity in rat pups (Weaver, Diorio, Seckl, Szyf, & Meaney, 2004). It is becoming increasingly clear that the neural systems supporting social affiliation are implicated in more general emotional responding. For example, the neuropeptides oxytocin and arginine vasopressin have emerged as important mediators of social affiliation (Kosfeld, Heinrichs, Zak, Fischbacher, & Fehr, 2005; Young & Wang, 2004), and receptors for both are found in a network of emotion-related cortical and subcortical structures in monogamous nonhuman mammals (Insel, 1997).

Recent human functional neuroimaging studies of maternal affection and romantic attachment have implicated structures associated with reward seeking, including caudate-putamen and ventral tegmentum, as well as portions of the dorsolateral and ventrolateral prefrontal cortex (Aron et al., 2005; Bartels & Zeki, 2004). Interestingly, deactivations in structures associated with the regulation of negative emotion, such as the medial prefrontal and ventral paracingulate cortex, have also been observed in some of these studies (Bartels & Zeki, 2004). Although interesting, research of this sort has focused on putative neural responses to higher-order constructs (e.g., love, friendship) that are in fact difficult or impossible to capture directly using most neuroimaging technology (cf. Cacioppo et al., 2003). By contrast, simple threat cues possess discrete stimulus properties that are well suited to neuroimaging. Despite this advantage, no work to date has identified the distress-alleviating effects of romantic relationships on the neural circuitry supporting threat responding.

In this functional magnetic resonance imaging (fMRI) experiment, hand-holding and threat of electric shock were used to investigate the social regulation of neural systems underlying response to threat (cf. Dalton, Kalin, Grist, & Davidson, 2005). Because most married people in the United States identify their spouse as their central adult relationship partner (Lugaila, 1998), we asked married women in highly satisfactory marital relationships to view images indicating either safety or threat under three counterbalanced conditions while brain images were collected. In one condition, women held their husband's hand. In the other two, they held either the hand of an anonymous male experimenter or no hand at all. Hand-holding was selected as a supportive social behavior because it (a) is a common nonverbal mode of expressing social support and affection, (b) has been observed in nonhuman primates during periods of dyadic reconciliation and soothing (de Waal, 2000), (c) has been shown to reduce autonomic arousal and reports of anxiety under stressful conditions (Jung-Soon & Kyung-Sook, 2001), and (d) offered a method that was easily implemented in the fMRI environment.

We sought to test three major hypotheses. First, we hypothesized simply that both spouse and stranger hand-holding would attenuate threat-responsive neural activity. Second, we hypothesized that attenuation of the neural threat response would be maximized during spousal hand-holding. Finally, we hypothesized that attenuation of the neural threat response would be a partial function of marital quality, with higher marital quality predicting greater attenuation.

METHOD

PARTICIPANTS

Sixteen highly satisfied married couples were selected to participate; mean ages of the husbands and wives were 33 ($SD = 5$) and 31 ($SD = 5$), respectively. Fifteen couples identified themselves as Caucasian, and one identified themselves as Asian. Participants were recruited from the greater Madison, WI, area via newspaper advertisements, and respondents were excluded if they had current or past psychopathology, were pregnant, or exhibited any risk for incident in the magnetic environment of the fMRI scanner.

Because previous research suggests that highly satisfactory relationships should have the strongest distress-attenuating effects (Coyne et al., 2001), both wives and husbands rated their marital quality using the Satisfaction subscale of the Dyadic Adjustment Scale (DAS; Spanier, 1976). The DAS is a widely used measure of relationship quality comprising four correlated subscales and one overall composite score (the DAS score). Higher DAS scores indicate relationships of putatively higher quality. In the initial telephone screening, the Satisfaction subscale of the DAS was used to rapidly screen out couples who were dissatisfied with their marriage. Scores on this subscale range from 0 to 50, with 50 representing the highest level of satisfaction. Husbands and wives scoring lower than 40 on this subscale were excluded from the study. Later, total DAS scores were recorded from both husbands and wives. The total DAS score has a theoretical range extending from 0 to 151, with scores lower than 100 thought to indicate distressed marriages. Mean total DAS scores were 126 ($SD = 10$) and 127 ($SD = 6$) for husbands and wives, respectively, indicating a generally high level of marital quality among the couples in this sample. The Pearson correlation between husbands' and wives' DAS scores was .20, n.s. Total DAS scores were used for the analyses reported here.

Only wives were tested in the scanner. Husbands completed questionnaires and provided hand-holding. All participants gave written informed consent in agreement with the Human Subjects Committee of the University of Wisconsin medical school and were paid for participation.

PROCEDURE

Interested participants were screened via telephone. Eligible participants were told they were participating in a study of hand-holding, and scheduled for two visits to the laboratory. During the first visit, participants completed a battery of questionnaires selected to assess marital quality and various aspects of personality (results not reported here) before undergoing an imaging trial run in the laboratory's mock fMRI scanner. Mock scanning familiarizes participants with the

scanning environment, allows them to express any discomfort with that environment, and gives them practice using experimental devices (e.g., button box). Although all couples were fully informed about the electric shocks that would be involved in the second visit, no sample shocks were delivered during the first visit.

The second visit, which occurred approximately 1 week later, consisted of the experimental brain-imaging procedure. Couples were brought to a waiting room, where they completed an additional fMRI safety assessment as two Ag-AgCl shock electrodes were applied to the wife's right or left ankle (counterbalanced across participants). The wife was then led to the fMRI chamber, where high-resolution anatomical scans were collected before the beginning of the experiment.

For the experiment, the wife observed 12 threat and 12 safety cues, in random order, within each of three counterbalanced blocks, for a total of 24 cue trials (see Figure 34.1). Trials were randomized within subjects, and block order was counterbalanced between subjects. During one block, the wife held her husband's hand. During another, she held the hand of an unseen, anonymous male experimenter. (Wives were not introduced to the anonymous male hand-holder until after the experiment was completed.) For the remaining block, no hand-holding was provided. Subjects' right hands were used for all hand-holding; left hands were used for providing ratings of subjective experience. With the exception of 3 participants, all participants held the hand of the same male experimenter. Two other male volunteers served as the stranger on the occasions the standard stranger was unavailable. Threat cues (a red ''X'' on a black background) indicated a 20% likelihood of receiving an electric shock to the ankle. Safety cues (a blue ''0'' against a black background) indicated no chance of shock. Electric shocks were delivered using an isolated physiological stimulator (Coulbourn Instruments, Allentown, PA) with 20-ms duration at 4 mA. All subjects received two shocks per block.

Each trial began with a threat or safety cue that lasted 1 s and was followed by an anticipation period that varied between 4 and 10 s. Subjects were instructed to focus their attention on a fixation cross during the anticipation period. Shocks were delivered only at the end of the anticipation period. The end of the trial was

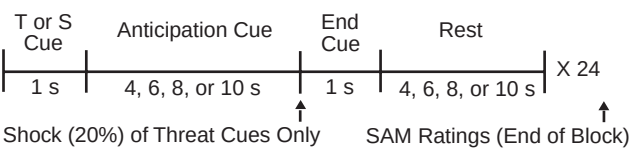

FIGURE 34.1.
Experimental procedure. Trials consisted of a 1-s threat (T) or safety (S) cue, a 4-to 10-s anticipation period, a 1-s end cue, and a 4- to 10-s resting period. At the end of each hand-holding condition, subjects completed ratings of unpleasantness and arousal using the Self-Assessment Manikin (SAM) scales.

indicated with a small circle, after which subjects were instructed to rest until the next trial began. The resting period, during which a black screen was presented, also varied between 4 and 10 s. At the end of each block, subjects rated their subjective feelings of unpleasantness (valence) and agitation (arousal) on the Self-Assessment Manikin (SAM) scales (Bradley & Lang, 1994). Using these 5-point nonverbal pictorial instruments, subjects provided one unpleasantness rating and one arousal rating for each hand-holding condition, entering their scores with a button box placed in their left hands.

IMAGE ACQUISITION AND DATA ANALYSIS

Functional magnetic images were acquired using a General Electric (Fairfield, CT) Signa 3.0-T high-speed magnetic imaging device, with a quadrature head coil. Two hundred fifteen functional images were collected per block, in volumes of thirty 4-mm sagittal echo-planar slices (1-mm slice gap) covering the whole brain. A repetition time of 2 s was used, with an echo time of 30 ms, a 60° flip, and a field of view of 240 × 240 mm, with a 64 × 64 matrix, resulting in a voxel size of 3.75 × .75 × 5 mm. Prior to collection of functional images, a T1-weighted spoiled-gradient-recalled anatomical scan consisting of one hundred twenty-four 1.2-mm slices was acquired to assist with localization of function.

Using Analysis of Functional Neural Images (AFNI) software (Version 2.52; Cox, 1996), we reconstructed raw data off-line with a 1-voxel in-plane full-width/half-maximum Fermi window, six-parameter rigid body-motion correction, high-pass filtering of 1/60 s (to remove signal unrelated to stimulus presentation), and removal of ghost and skull artifacts. Trials during which participants actually received shocks were excluded from analysis in order to minimize movement artifacts. With a least squares general linear model, time series were fit to an ideal hemodynamic response; the motion parameters were entered as covariates. The resultant beta weights were converted to percentage signal change, and the maps transformed into standardized Talairach space (Talairach, 1988).

STATISTICAL REGIONS OF INTEREST (ROIS)

An intermediate data-reduction step involved determining the normative neural threat response by contrasting activation to threat cues and activation to safety cues (threat minus safety) within the no-hand-holding condition. Multisubject ROIs were identified via voxel-wise *t* tests that indicated areas of greater activation in threat than safety-cue trials ($p < .005$ corrected, with corrections estimated from Monte Carlo simulations). As expected, this procedure revealed activation in a network of regions that numerous studies have shown to be associated with

neural response to threat, negative affect, or anticipation of pain, such as the ventral anterior cingulate cortex (vACC), right dorsolateral prefrontal cortex (right DLPFC), right inferior frontal gyrus, left superior frontal gyrus, right anterior insula, caudate–nucleus accumbens (NAcc), putamen, hypothalamus, right postcentral gyrus, superior colliculus, posterior cingulate, and left supramarginal gyrus (Davidson & Irwin, 1999; Ploghaus et al., 1999; Salomons, Johnstone, Backonja, & Davidson, 2004; Wager et al., 2004). Table 34.1 lists all the ROIs. These ROIs

TABLE 34.1
Statistical Regions of Interest and Their Effects Across Hand-Holding Conditions

Region	Centroid coordinates			*t* score	Size (mm^3)	Condition effect	Spouse effect	Stranger effect
	x	*y*	*z*					
Frontal and anterior cingulate regions								
Supplementary motor cortex	4	6	46	3.63	4,043			
Superior frontal gyrus	−10	−8	59	3.82	907			
	9	−9	64	3.89	435			
Ventral ACC	−12	39	−1	3.55	358	✓	✓	✓
	3	44	2	3.81	296			
DLPFC	32	34	30	3.78	350	✓	✓[a]	
Precentral gyrus	−39	−4	37	3.73	336			
Ventromedial PFC	12	45	−6	3.77	275			
Inferior frontal gyrus	−36	35	21	4.05	572			
Insular and subcortical regions								
Anterior insula	37	16	3	4.33	6,213			
	−28	20	3	3.92	4,937			
Caudate	8	7	8	3.89	2,092			
	−10	−3	21	3.75	491			
Caudate–NAcc	−8	4	2	3.71	1,390	✓	✓	
Putamen	28	4	−3	3.72	192			
Anterior thalamic nucleus	−11	−14	11	3.63	418			
Hypothalamus	1	−13	−5	3.72	1,441	✓	✓	
Superior colliculus	3	−28	−2	3.77	1,316			
Parietal and posterior cingulate regions								
Posterior cingulate	9	−55	19	3.65	645			
	−9	−28	38	3.93	381	✓	✓	✓
	14	−33	38	3.53	249	✓	✓	✓
Postcentral gyrus	30	−50	63	3.73	390	✓	✓	✓
Supramarginal gyrus	−53	−29	20	3.54	298	✓	✓	✓
	50	−28	17	3.73	231			

Note: Regions of interest were identified as clusters that showed significantly greater activation on threat trials than on safety trials in the no-hand condition ($p < .005$, $p_{rep} \geq .97$, corrected). ACC = anterior cingulate cortex; DLPFC = dorsolateral prefrontal cortex; NAcc = nucleus accumbens; PFC = prefrontal cortex; spouse effect = threat activity in the spouse condition < threat activity in the no-hand condition; stranger effect = threat activity in the stranger condition < threat activity in the no-hand condition.

[a]In this comparison, threat-related neural activation was greater in the stranger condition than in the spouse condition. No instances of the opposite pattern were observed.

were used in subsequent comparisons of hand-holding conditions and tests of covariation with marital quality.

To examine the effects of hand-holding on threat-related ROI activation, we employed three general data-analytic steps. First, the repeated measures general linear model was used to test for effects of hand-holding condition in all ROIs. Second, following identification of ROIs showing main effects of condition, planned comparisons were conducted to determine whether specific condition contrasts (spouse vs. stranger vs. no hand) were statistically significant. Third, in testing relationships between threat-related neural activation and marital quality (DAS scores), we used SPSS's linear mixed-model module to examine differences in slopes as a function of hand-holding condition.

RESULTS

HAND-HOLDING REDUCES SUBJECTIVE UNPLEASANTNESS AND AROUSAL

Tracking reports of subjective experience provided an important check on the efficacy of the experimental manipulation. Repeated measures analyses of variance revealed main effects of hand-holding condition on SAM ratings of both valence, $F(2, 14) = 8.30$, $p = .004$, $p_{rep} = .97$, $\eta_p^2 = .54$, and arousal, $F(2, 14) = 3.62$, $p = .05$, $p_{rep} = .88$, $\eta_p^2 = .34$. Planned comparisons revealed that unpleasantness ratings were significantly lower in the spousal-hand-holding condition than in both the stranger-hand-holding condition, $F(1, 15) = 4.77$, $p = .05$, $p_{rep} = .88$, $\eta_p^2 = .24$, and the no-hand condition, $F(1, 15) = 16.30$, $p = .001$, $p_{rep} = .99$, $\eta_p^2 = .52$. By contrast, planned comparisons of *p* arousal ratings across hand-holding conditions revealed that although the spouse and stranger conditions were both less arousing than the no-hand condition, these comparisons only approached statistical significance, $F(1, 15) = 3.85$, $p = .07$, $p_{rep} = .85$, $\eta_p^2 = .20$, and $F(1, 15) = 3.46$, $p = .08$, $p_{rep} = .83$, $\eta_p^2 = .19$, respectively (see Figure 34.2).

HAND-HOLDING ATTENUATES NEURAL THREAT RESPONSES

Table 34.1 provides a guide to the main effects and planned comparisons, with centroid coordinates and cluster size for each ROI. Significant main effects of hand-holding condition were found in vACC, right DLPFC, left caudate, superior colliculus, two regions of the posterior cingulate, left supramarginal gyrus, and

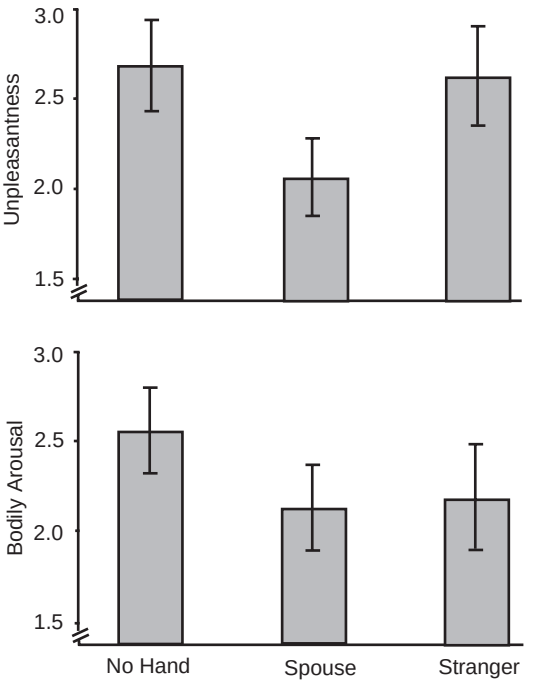

FIGURE 34.2.
Main effects of hand-holding condition on unpleasantness and arousal ratings.

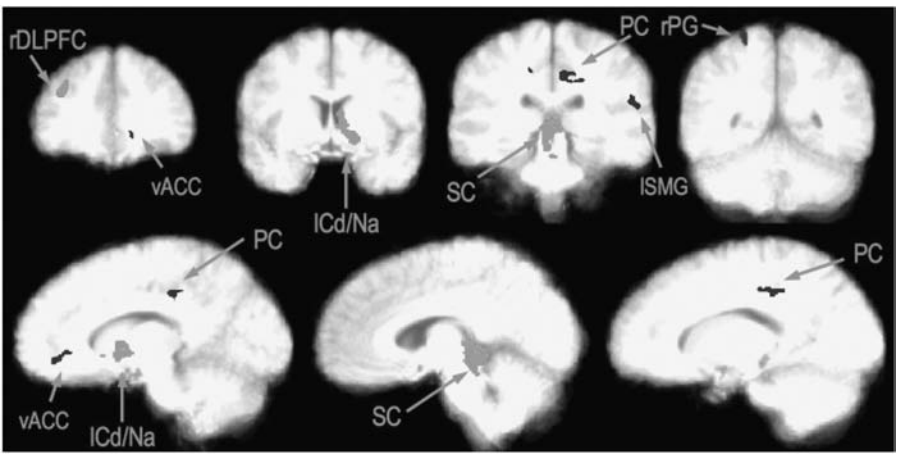

FIGURE 34.3.
Threat-responsive regions of interest affected by hand-holding condition. Light gray clusters highlighting right dorsolateral prefrontal cortex (rDLPFC), left caudate–nucleus accumbens (lCd/Na), and superior colliculus (SC) indicate spouse-related attenuation. Dark gray clusters highlighting the ventral anterior cingulate cortex (vACC), posterior cingulate (PC), right postcentral gyrus (rPG), and left supramarginal gyrus (lSMG) indicate attenuation associated with both spouse and stranger hand-holding. Section plane coordinates are as follows (from left to right): $y = +34$ mm, $+3$ mm, -29 mm, and -49 mm for the top row and $x = -10$ mm, $+2$ mm, and $+14$ mm for the bottom row.

right postcentral gyrus, all Fs(2, 14) ≥ 3.62, ps ≤ .05, p_{rep}s ≥.88, η_p^2 s ≥ .20 (see Figure 34.3).

Planned comparisons revealed that neural activation to threat (threat minus safety) was significantly lower in the spouse condition than in the no-hand condition for the following regions: vACC, left caudate, superior colliculus, posterior cingulate, left supramarginal gyrus, and right postcentral gyrus, all Fs(1, 15) ≥ 4.52, ps ≤ .05, p_{rep}s ≥ .88, η_p^2s ≥ .23. Neural activation to threat was also significantly lower in the spouse condition than in the stranger condition in the right DLPFC, $F(1, 15) = 6.89$, $p = .02$, $p_{rep} = .93$, $\eta_p^2 = .32$, though attenuation in this region p for the comparison between the spouse and no-hand conditions only approached significance, $F(1, 15) = 3.54$, $p = .08$, p_{rep} 5 .84, $\eta_p^2 = .19$

Neural activation to threat was significantly lower in both the spouse and the stranger conditions than in the no-hand condition in the vACC, posterior cingulate, left supramarginal gyrus, and right postcentral gyrus, all Fs(1, 15) ≥ 5.76, ps ≤ .03, p_{rep}s ≥ .90, η_p^2s ≥ .28.

DAS SCORES AND NEURAL RESPONSE TO THREAT

We next sought to predict threat-related neural activation using DAS scores. First, a repeated measures analysis of covariance revealed an interaction effect between hand-holding condition and wife's DAS score (WDAS) in predicting valence ratings, $F(2, 13) = 5.16$, $p = .02$, $p_{rep} = .92$, $\eta_p^2 = .44$. Pearson correlations between WDAS and valence ratings were −.46, n.s., for the no-hand condition; −.28, n.s., for the spouse condition; and −.82, $p < .001$, $p_{rep} = .99$, for the stranger condition. Husband's DAS score (HDAS) did not show any similar effects, nor were WDAS and HDAS associated with arousal ratings. Thus, it was necessary to determine whether DAS scores were capable of predicting threat-related neural activation independently of valence ratings.

To accomplish this, we conducted linear mixed models containing valence ratings (a changing covariate), HDAS, and WDAS, as well as their interactions with hand-holding condition. Neither HDAS nor the HDAS-by-condition interaction was significant. As shown in Figure 34.4, however, there were significant WDAS-by-condition interaction effects in the left superior frontal gyrus, $F(2, 26) = 4.84$, $p = .02$, $p_{rep} = .93$; right anterior insula, $F(2, 23) = 4.33$, $p = .03$, $p_{rep} = .90$; and hypothalamus, $F(2, 27) = 4.31$, $p = .02$, $p_{rep} = .93$. Inspection of separate regressions (one for each brain area and condition) revealed these interaction effects to be due to negative correlations between WDAS and threat-related neural activation in the spouse-hand-holding condition. These correlations were −.59, $p = .02$, $p_{rep} = .95$ for the left superior frontal gyrus; −.47, $p = .07$, $p_{rep} = .86$ for the right anterior insula; and −.46, $p = .08$, $p_{rep} = .83$ for the hypothalamus. In the stranger and no-hand conditions, correlations between WDAS and threat-related

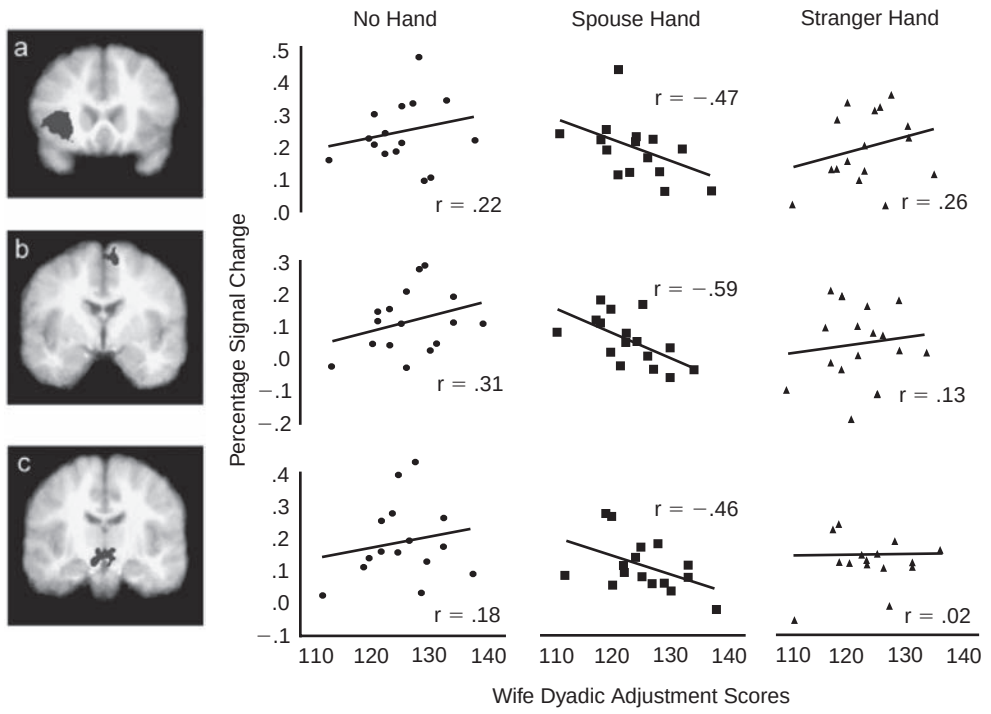

FIGURE 34.4.
Interactive effect of wife's score on the Dyadic Adjustment Scale (DAS) and hand-holding condition on neural response to threat. Percentage signal change is graphed as a function of DAS score (with correlation coefficients included) and condition for the three brain regions showing a significant interaction: (a) right anterior insula ($y = +19$ mm), (b) left superior frontal gyrus ($y = -4$ mm), and (c) hypothalamus ($y = -13$ mm).

ROI activation were either slightly positive (e.g., $r = .31$ in the no-hand condition) or near zero. Interestingly, main effects of valence were also observed in both the right anterior insula, $F\ (1, 27) = 6.02$, $p = .02$, $p_{rep} = .92$, and the hypothalamus, $F(1, 32) = 10.23$, $p = .003$, $p_{rep} = .98$, for which the average correlations were .46, $p = .08$, $p_{rep} = .89$, and .65, $p = .01$, $p_{rep} = .95$, respectively. These effects indicate that greater activation in both the anterior insula and the hypothalamus corresponded with greater levels of subjective unpleasantness, regardless of hand-holding condition.

DISCUSSION

As hypothesized, both spouse and stranger hand-holding attenuated neural response to threat to some degree, but spousal hand-holding was particularly powerful. Moreover, even within this sample of highly satisfied married couples, the

benefits of spousal hand-holding under threat were maximized in those couples with relationships of the very highest quality.

Close inspection of the regions implicated in the main effects of hand-holding suggests the following:

- Both spouse and stranger hand-holding conferred a basic level of regulatory influence on the neural response to threat cues, especially with regard to structures implicated in the modulation of affect-related action and bodily arousal, such as the vACC (Allman, Hakeem, Erwin, Nimchinsky, & Hof, 2001), and visceral and musculoskeletal responses, such as the posterior cingulate, supramarginal gyrus, and postcentral gyrus (Fulbright, Troche, Skudlarski, Gore, & Wexler, 2001; Liddel et al., 2005; Rushworth, Krams, & Passingham, 2001).
- Spousal hand-holding conferred these benefits and more, further attenuating threat-related neural activation in areas implicated in the regulation of emotion (right DLPFC, caudate) and emotion-related homeostatic functions (superior colliculus; Damasio et al., 2000; Davidson & Irwin, 1999; Liddel et al., 2005). It is striking how this pattern of neural effects was echoed in subjective reports of experience: Although both spouse and stranger hand-holding resulted in lower reports of bodily arousal, only spousal hand-holding provided the additional benefit of lowering subjective reports of task-related unpleasantness.
- Threat-related activation in the right anterior insula, superior frontal gyrus, and hypothalamus was sensitive to marital quality. This suggests that individuals in higher-quality relationships benefit from greater regulatory effects on the neural systems supporting the brain's stress response, including the affective component of pain processing (e.g., in right anterior insula; cf. Ploghaus et al., 1999; Salomons et al., 2004; Wager et al., 2004).

Indeed, regulation of the hypothalamus suggests that these benefits may be pervasive, as the hypothalamus influences a cascade of neurochemical regulatory processes, such as the release of corticotropin-releasing hormone, which in turn stimulates the release of cortisol into the bloodstream—a process widely understood to hold implications for immune function and memory (Kemeny, 2003).

It is particularly noteworthy that the effects of marital quality were specific to spousal hand-holding. This finding is consistent with conceptualizations of attachment relationships as hidden regulators—''regulators'' because of the emotion-regulatory benefits attachment relationships confer, and ''hidden'' because those regulatory benefits are frequently apparent only when the attachment system, or one of the partners within that system, is under threat (Hofer, 1984, 1995).

It is already well known that social isolation is a major health risk, and that high-quality attachment relationships mitigate the effects of stress, injury, and infection (Berscheid, 2003; Coyne et al., 2001; Hofer, 1984, 1995; House et al.,

1988; Mikulincer et al., 2003; Robles & Kiecolt-Glaser, 2003; Wood et al., 1989). The current results provide new insights into how these effects occur. At one level, hand-holding appears to produce a general regulatory effect on neural threat responses related to bodily attention and the coordination of motor responses; this suggests that such processes may represent the most immediate or lowest-level benefit of social soothing and support. At another level, structures associated with more evaluative, attentional, and affective components of the threat response were attenuated more specifically by spousal hand-holding, which suggests that attachment figures act as emotion regulators in ways that strangers do not. Put another way, both stranger and spousal hand-holding appear capable of decreasing the need for a coordinated bodily response to threatening stimuli, but only spousal hand-holding confers the additional benefit of decreasing the need for vigilance, evaluation, and self-regulation of affect.

Finally, the correspondence between the magnitude of threat-related neural responses and marital quality is consistent with known associations among measures of marital quality and health, and even points the way toward the neural mediators of those effects. Particularly promising in this regard is the observed effect of marital quality on the hypothalamus, as links between the HPA axis and various health-related processes (e.g., immune function) suggest a bridge between findings reported here and general associations between marital quality and health reported elsewhere (Robles & Kiecolt-Glaser, 2003). Other links are possible as well. For example, oxytocin has been proposed as one of the mechanisms through which the positive benefits of social support are realized (Uvnaes-Moberg, 1998), and it is plausible that oxytocin activity served as a mediator of the attenuation of threat-related neural activity reported here. Exogenous injection of oxytocin attenuates a variety of centrally mediated stress responses in rats (Izzo et al., 1999), and physical contact alone has been associated with oxytocin release from the paraventricular nuclei of the hypothalamus (Uvnaes-Moberg, 1998), which may in turn increase endogenous opioid activity (Uvnaes-Moberg, 1998) and target dopamine receptors related to inhibitory motor control throughout the basal ganglia (Gimpl & Fahrenholz, 2001).

Of course, it is important to note that these findings may not generalize to attachment relationships that are characterized by discord or that are otherwise unsatisfactory to one or the other partner. Indeed, the fact that threat-related neural activation was sensitive to marital quality even within highly satisfactory marriages suggests that many of these effects should not generalize to relationships of poorer quality. Moreover, it is well known that threat responses in the context of attachment relationships also vary as a partial function of attachment-related personality characteristics—individual differences in styles of relating to others while under stress (Bowlby, 1969/1982; Mikulincer & Shaver, 2005; Mikulincer et al., 2003). Indeed, such differences may have influenced the pattern of correlations observed between WDAS and subjective unpleasantness ratings across the different hand-holding conditions. These and other questions await further evaluation. In the

meantime, results presented here provide evidence of the neural systems and processes through which the distress-alleviating and health-enhancing effects of social soothing in general, and high-quality attachment relationships in particular, are realized.

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35

The Search for a Romantic Partner: The Effects of Self-Esteem and Physical Attractiveness on Romantic Behavior

Sara B. Kiesler and Roberta L. Baral

What do people look for in a romantic partner? Observations of everyday life suggest one "common sense" assumption: that people prefer romantic partners who are attractive and socially desirable. Yet if the person himself were truly objective, he would consider what is most practical. That is, what is the probability of a successful outcome for attempts to win various alternative partners? While attainment of a very attractive partner might be very rewarding, the chances of actually "catching" such a person are likely to be somewhat lower than are the chances of winning a less attractive partner. Hence, if one is only moderately or low attractive himself, his most realistic initial choice would be based on both the wish to maximize attractiveness of a partner and the likelihood of positive outcome. This hypothesis, of course, is not new. For example, "exchange theories" (Homans, 1961; Thibaut and Kelley, 1959) assume that people estimate potential "costs" and "rewards" when choosing others.

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For the average sort of person, then, realistic behavior should entail his choosing a person who is not extremely attractive. Yet "average" and "below average" people do not always choose those who are actually moderate or low in favorability. A study by Walster and colleagues (1966) provides an example. At a college dance, dates were randomly matched to study the relationship of physical attractiveness to romantic behavior. In general, the investigators found that the more attractive the female date, the better liked she was and the more the man said he would like to date her, *regardless* of his *own* physical attractiveness. Furthermore, the same finding held for women. The more attractive the male date, the more he was liked and wanted as a date again, regardless of the woman's own attractiveness.

The question arises, however, why the moderately and low attractive person in the study by Walser and colleagues tended to choose the most attractive partner (instead of a more "realistic" partner). One possibility is that subjects overevaluated their own attractiveness. First, the subjects may have assumed that the computer has paired them on the basis of similarity, thus making it easy to believe that they were fairly equal in attractiveness to the very attractive partners. Second, all the subjects were dressed up and prepared to present their best face. In such a context, it may have been easy to distort personal attributes and end up with relatively high "self-esteem." Even if subjects did not misperceive their own physical attractiveness, they could have felt that their intelligence or personality made up for any deficiency in physical attractiveness. If they did, indeed, exaggerate their own positive attributes, then their behavior was realistic in that both chances of success and maximization of attractiveness were considered. Each one merely ended up choosing someone of *objectively* higher attractiveness.

That people do overevaluate themselves in many situations has long been known (see Crowne and Marlowe, 1964). For example, a prior success experience should raise a person's self-esteem and feelings of worthiness (e.g., Aronson and Carlsmith, 1962). In the romantic-behavior situation, then, certain situational conditions that raise self-esteem may be of some importance in affecting what the romantic behavior will be. We have assumed that one effect will be on the person's perceptions of a potential partner. Thus, if self-esteem is high, he will perceive a highly attractive person as attainable and will choose that person.

Let us now consider what happens when self-esteem is lowered. For instance, if a person is presented with evidence that he is not overintelligent, then it is harder for him to distort his self-image in a favorable direction. Unlike the person with high self-esteem, the person with low self-esteem should perceive the very attractive partner as relatively dissimilar and "hard to get." He should then choose someone not so desirable.

We propose, then, that the self-esteem variable will influence who seems to be a realistic choice. In the present study, our hypothesis was: The *lower* the self-esteem, the lower would be the attractiveness of a chosen romantic partner. Thus we predicted that moderately attractive males with high self-esteem would tend to choose and like very attractive females, but with low self-esteem they would tend to choose only moderately attractive females.

To test our hypothesized interaction between self-esteem and attractiveness of the partner, an experiment was designed in which two levels of self-esteem, high and low, and two levels of physical attractiveness of a female, high and moderate, were orthogonally varied. Male subjects were first led to believe that they were doing very well or rather poorly on an intelligence test. We assumed that the former condition would lead to temporarily high self-esteem and encourage overevaluation of self, and that the latter condition would lead to a temporary lowering of self-esteem and would discourage overevaluation of self. The subjects were then exposed to a female of very high or only moderate physical attractiveness. Subsequently, observations were made of the subjects' romantic behavior.

METHOD

Subjects

Subjects were 43 male volunteers ranging in age from 19 to 37. They were recruited from institutions in the New Haven, Connecticut, area, including Southern Connecticut State College, Yale Medical School, Yale College, New Haven College, and local libraries. The subjects were mostly undergraduate and graduate students, but included among them were also a few men in various occupations (e.g., high school teacher). Of the 43 subjects, 6 were excluded from the final analysis: 1 for suspicion, 1 for knowing one of the confederates previously, 2 because they were married, and 2 because they were engaged. The subjects were paid $1.50 for their participation.

Procedure

The subjects were recruited for a one-hour study on "intelligence testing." As each subject arrived he was ushered into an ordinary faculty office at Yale University by a male experimenter. The experimenter told the subject that he was perfecting and establishing norms for a new intelligence test, which had already been successfully used on "hundreds" of students. He emphasized that the test was already very accurate and reliable, and that it predicted "success in life." He then asked the subject to take the test orally, explaining that it consisted of five parts, which would be presented sequentially.

Self-Esteem Manipulation

The subjects were randomly assigned to the low or high self-esteem condition. In both conditions the "test" was the same. Subjects were first required to repeat from memory long lists of numbers; in a second part, they were asked to de-

fine words. To reduce initial variance in performance, the test was made extremely difficult. For instance, some of the words to be defined were fictitious (e.g., "sympanic").

The manipulation of high self-esteem was accomplished by conveying to the subject the impression that his performance was better than that of most subjects. At preprogrammed intervals the experimenter nodded; at others he told the subject that other subjects had had much more trouble with the questions. In addition, the number task was made somewhat easier by reading the numbers with some rhythm. Thus performance was actually a little better in the high-self-esteem condition than in the low-self-esteem condition.

In the low-self-esteem condition, the experimenter attempted to convey the impression that the subject's performance was inferior. At intervals he frowned and looked away or mentioned that other subjects had performed better. At other intervals he asked the subject if he felt "relaxed enough."

After the "second part" of the test, the experimenter suggested a break. He then gave the subject a short questionnaire, saying that it was a "Psychology Department questionnaire administered to all subjects in studies at Yale." This questionnaire was designed to check on the self-esteem manipulation.

When the subject had filled out the questionnaire, the experimenter stood up and said he "hadn't eaten all day and really needed a cup of coffee." Low-self-esteem subjects were told that "maybe a break will help you." High-self-esteem subjects were told that "since you are doing so well we have plenty of time." The experimenter asked the subject to come with him to get a cup of coffee. Just before they left the room, the experimenter "noticed" a telephone message on his desk and remarked that "he'd better take care of it soon." This sequence was performed to prepare the subject for the experimenter's eventual departure.

Meeting the Potential Romantic Partner

The experimenter took the subject to a small canteen in the same building and bought the coffee. As he turned to sit down, he appeared to recognize a girl seated at one of the tables and approached her. This girl was actually one of two confederates used in the study. The experimenter greeted the confederate and asked her if she was working in the building during the summer. She was then introduced to the subject as a coed at a nearby college who was doing summer work for a psychologist, and a preprogrammed conversation began.

After a minute, the experimenter excused himself, telling the subject that "now would be a good time to make that phone call." He was gone for ten minutes, during which the confederate engaged the subject in further conversation (asking him what he did, what his likes and dislikes were, etc.). The experimenter then returned and, acting displeased, told the subject that since his "fiancée really needs the car" he would have to break off the experiment. He apologized, paid the subject, and

left. The confederate then continued talking with the subject (on prearranged topics), either for a half hour or until the subject attempted to leave or asked her for a date, whichever came first.

Physical Attractiveness Manipulation

The female confederate, one of two chosen because they were highly attractive, appeared in one of two attractiveness conditions, "high" or "moderate." Both confederates appeared in both conditions in random sequence. In the high-attractiveness condition, each confederate wore becoming make-up and fashionable clothing designed to enhance her initial attractiveness. In the moderate-attractiveness condition, an attempt was made to reduce somewhat the confederate's initial attractiveness. She wore heavy glasses and no makeup, and her hair was pulled back with a rubber band. Her skirt and blouse clashed and were arranged sloppily.

During the interaction the confederates attempted to act exactly the same in all encounters. They were friendly, accepting, and interested throughout. To reduce bias, they were not informed of the self-esteem condition the subjects were in.

The Dependent Variable: Romantic Behavior

After the experimenter "concluded" the experiment and during the following half hour, the confederate recorded the frequency of behavior falling into certain prescribed categories of "romantic behavior." The categories included asking for a date, asking for information that presumably would lead to a date (e.g., asking for the confederate's phone number), offering to buy a snack or coffee for her, offering her a cigarette or mint already on hand, complimenting her, and, finally ignoring her when, at the end of the prescribed time, she said she should "get back to work" (called "ignoring the first cue to leave"). At first, it was intended also to measure the amount of time the subject stayed with the confederate, but this measure turned out to be unreliable since the confederate had to stop the experiment if the subject asked her for a date. It should be noted, however, that the majority (80 percent) did not attempt to leave before the time was up.

At the end of the experimental session, the confederate completely debriefed the subject and in particular emphasized the true nature of the intelligence test. After the debriefing, the subject was asked to rate the confederate on her attractiveness, using a 100-point scale.[1]

[1]Although this attempt to check on the attractiveness manipulation has very limited validity, giving it during the experiment would have incurred suspicion. Informal pretest observations by a group of five persons not in the experiment supports our contention that the manipulation was effective. In fact, two judges described the "moderate" confederate as "low" in attractiveness.

RESULTS

Effectiveness of the Manipulations

Subjects were asked two questions to check on the self-esteem manipulation. These were: "How would you describe your present emotional feelings?" (on a scale from "very happy" to "very sad"); and "How good do you personally feel your performance is in the task required in the present study?" (on a scale from "excellent" to "poor"). Both scales were arbitrarily scored from 1 to 20, the higher the score, the lower the self-esteem. According to analyses of variance performed on these data, subjects in the high-self-esteem condition tended to feel better than did low-self-esteem subjects ($F = 3.89$, $df = 1, 33$, $p < .10$), and they felt their performance was better ($F = 13.17$, $df = 1, 33$, $p < .01$). Thus, while the generalized mood response to the manipulation was somewhat weaker than the more direct measure of self-esteem (felt adequacy of performance), the manipulation seemed to have been effective overall.

The manipulation of physical attractiveness of the confederate was also effective at the 0.01 level (although one must have reservations about our measure).

There were no systematic differences among conditions of subject attractiveness, according to ratings made by the experimenter and confederates.

To check on possible differences between the two confederates, all data on romantic behavior (described in detail below) were subjected first to three-way analyses of variance (self-esteem $\times$ attractiveness $\times$ confederate). These analyses revealed significant tendencies for one confederate to elicit more romantic behavior than the other. However, there were no interactions of confederate with any independent variable. Thus the data for both confederates were combined for the analyses presented below.

Comparisons were also made between the first half of the experiment and the last half to check on possible confederate bias as a result of learning to recognize cues distinguishing high-self-esteem subjects from low-self-esteem subjects. However, the analysis indicated that there was no difference between phases of the experiment. In addition, a check of confederate descriptions of subjects indicated that they were unable to guess above chance levels the subjects' experimental condition.

Romantic Behavior

We predicted an interaction such that high-self-esteem subjects would display more romantic behavior toward the highly attractive confederate, whereas low-self-esteem subjects would display more romantic behavior toward the moderately attractive confederate. To test this prediction, we first summed the frequency of romantic behavior in each behavioral category for each subject. Any subject could get a score of 0 or 1 in each category except the "asked for date" category, in which

he could obtain a score of 2 if he asked for two dates. Thus a subject's romantic behavior "score" could range from 0 to 7. The mean scores for each condition are graphically presented in Figure 35.1. Because the data were distributed in skewed fashion, the scores were transformed to produce a more normal distribution [X = log (X + 2)]. These data were then subjected to an analysis of variance. According to the analysis of variance, the hypothesized interaction between self-esteem and attractiveness was supported ($F = 5.34$; $df = 1, 33$; $p < .05$). Thus high-self-esteem subjects tended to choose the highly attractive girl, while low-self-esteem subjects tended to prefer the moderately attractive girl, as predicted.

Table 35.1 presents a breakdown of the data for each romantic-behavior category. For purposes of simplification, the percentage of subjects in each condition who displayed each type of romantic behavior (i.e., the percentage who got a score of 1 in each category) is presented. These data are exactly equivalent to the frequency data, except in the "asked for date" category, where percentages for number of subjects asking for at least one date and percentages for number of dates over total possible (two per subject) are presented separately.

The data in Table 35.1 indicate that of the 14 appropriate comparisons, 12 are in the predicted direction. Within the high-self-esteem conditions, romantic behavior is greater with the highly attractive confederate than with the moderately attractive confederate for all categories but one (asking for information). Within the low-self-esteem conditions, romantic behavior tends to be greater with the moderately attractive confederate than with the highly attractive confederate for all categories but one (offering to buy). These single comparisons are not significantly different

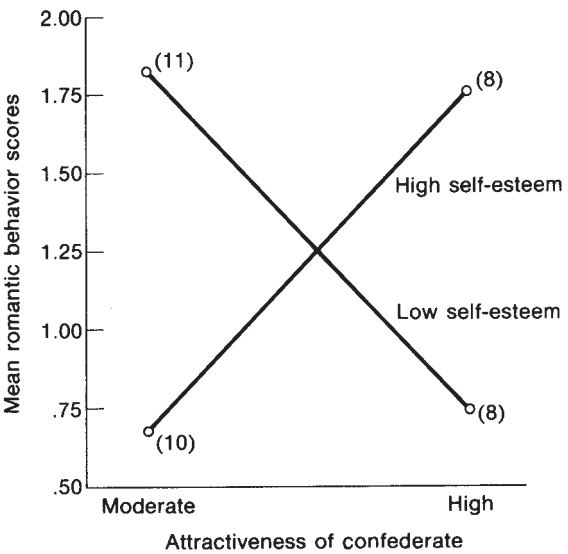

FIGURE 35.1
Romantic behavior toward another as function of self-esteem.

TABLE 35.1
Percentage of subjects displaying each type of romantic behavior

	Experimental condition			
	High self-esteem		Low self-esteem	
Romantic Behavior	Moderately attractive confederate (N = 10)	Highly attractive confederate (N = 8)	Moderately attractive confederate (N = 11)	Highly attractive confederate (N = 8)
Asked for date (at least one)	20	25	36	25
Asked for date (% dates of total possible)	10	19	27	13
Offered to buy coffee, etc.	0	25	18	25
Offered cigarette, gum, mint	10	25	27	0
Expressed compliments	0	25	18	0
Ignored first cue to leave	10	25	45	12
Asked for information—phone number, address, etc.	30	25	18	12

by themselves, but the overall consistency of the results for different romantic behaviors seems to provide some additional confidence in our hypothesis.

DISCUSSION

Our results indicate that self-esteem will affect which romantic partners are chosen: Low self-esteem will lead to choice of a partner lower in attractiveness than will high self-esteem. We interpret these results as indicating *not* that high-self-esteem persons are less "realistic" or "practical," but rather that the possession of high self-esteem changes what the person believes is a realistic or practical choice. Thus our interpretation of the effect of self-esteem on romantic behavior leans heavily on the assumption that changes in perceptions of the self affect perceptions of the chances of success or failure (i.e., probable payoff) when making a romantic choice. If a highly attractive person is perceived as much more attractive than oneself, as when one has low self-esteem, then the chances of success with that person will be perceived as relatively low. If a highly attractive person is perceived as similar in attractiveness to oneself, as when one has high self-esteem, then the chances of success will be seen as relatively high.

What if, in some way, we could have made subjects believe that choosing the confederate (in all conditions) would not entail any risk of rejection or failure? First, we should find subjects choosing the highly attractive confederate more than the moderately attractive confederate—maximizing attractiveness without having to consider possible failure. However, a recent study by Walster (1965) suggests a further difference: Low-self-esteem persons may have a higher need for affection

than high-self-esteem persons and, moreover, may be less demanding of perfection (will rate someone as more attractive than will high-self-esteem persons). Thus their attempts to secure acceptance—given no chance of failure—should be more intense than those of high-self-esteem persons. The Walster study bears out these predictions. Female subjects who had already been asked out by a male confederate liked him more when they had low self-esteem than when they had high self-esteem. In sum then, we conclude that our results will hold only when the chances of failure are unknown or known to be relatively high.

One further point should be considered. It is possible that, when giving the intelligence test, the experimenter manipulated acceptance of the subject, as well as perceived intellectual worthiness. If so, our low-self-esteem subjects may have felt socially rejected, as well as less intellectually capable. This would support another explanation of our results, that is, that the low-self-esteem subject liked the highly attractive girl less because she disconfirmed his expectation of himself as an undesirable person. We attempted to avoid this possibility by having the experimenter act friendly and accepting to all subjects, but only with another experiment, in which various manipulations of self-esteem are used, can this explanation be totally rejected.

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36

Social Perception and Interpersonal Behavior: On the Self-Fulfilling Nature of Social Stereotypes

Mark Snyder, Elizabeth Decker Tanke, and Ellen Berscheid

This research concerns the self-fulfilling influences of social stereotypes on dyadic social interaction. Conceptual analysis of the cognitive and behavioral consequences of stereotyping suggests that a perceiver's actions based upon stereotype-generated attributions about a specific target individual may cause the behavior of that individual to confirm the perceiver's initially erroneous attributions. A paradigmatic investigation of the behavioral confirmation of stereotypes involving physical attractiveness (e.g., "beautiful people are good people") is presented. Male "perceivers" interacted with female "targets" whom they believed (as a result of an experimental manipulation) to be physically attractive or physically unattractive. Tape recordings of each participant's conversational behavior were analyzed by naive observer judges for evidence of behavioral confirmation. These analyses revealed that targets who were perceived (unknown to them) to be physically attractive came to behave in a friendly, likeable, and sociable manner in comparison with targets whose perceivers regarded them as unattractive. It is suggested that theories in cognitive social psychology attend to the ways in which perceivers create the information that they process in addition to the ways that they process that information.

Thoughts are but dreams
Till their effects be tried

—William Shakespeare, *The Rape of Lucrece*

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Cognitive social psychology is concerned with the processes by which individuals gain knowledge about behavior and events that they encounter in social interaction, and how they use this knowledge to guide their actions. From this perspective, people are "constructive thinkers" searching for the causes of behavior, drawing inferences about people and their circumstances, and acting upon this knowledge.

Most empirical work in this domain—largely stimulated and guided by the attribution theories (e.g., Heider, 1958; Jones and Davis, 1965; Kelley, 1973)—has focused on the processing of information, the "machinery" of social cognition. Some outcomes of this research have been the specification of how individuals identify the causes of an actor's behavior, how individuals make inferences about the traits and dispositions of the actor, and how individuals make predictions about the actor's future behavior (for reviews, see Harvey, Ickes, and Kidd, 1976; Jones et al., 1972; Ross, 1977).

It is noteworthy that comparatively little theoretical and empirical attention has been directed to the other fundamental question within the cognitive social psychologist's mandate: What are the cognitive and behavioral consequences of our impressions of other people? From our vantage point, current-day attribution theorists leave the individual "lost in thought," with no machinery that links thought to action. It is to this concern that we address ourselves, both theoretically and empirically, in the context of social stereotypes.

Social stereotypes are a special case of interpersonal perception. Stereotypes are usually simple, overgeneralized, and widely accepted (e.g., Karlins, Coffman, and Walters, 1969). But stereotypes are often inaccurate. It is simply not true that all Germans are industrious or that all women are dependent and conforming. Nonetheless, many social stereotypes concern highly visible and distinctive personal characteristics (e.g., sex and race). These pieces of information are usually the first to be noticed in social interaction and can gain high priority for channeling subsequent information processing and even social interaction. Social stereotypes are thus an ideal testing ground for considering the cognitive and behavioral consequences of person perception.

Numerous factors may help sustain our stereotypes and prevent disconfirmation of "erroneous" stereotype-based initial impressions of specific others. First, social stereotypes may influence information processing in ways that serve to bolster and strengthen these stereotypes.

Cognitive Bolstering of Social Stereotypes

As information processors, humans readily fall victim to the cognitive process described centuries ago by Francis Bacon.

> The human understanding, when any proposition has been once laid down . . . forces everything else to add fresh support and confirmation. . . . it is the peculiar and per-

> petual error of the human understanding to be more moved and excited by affirmatives than negatives [pp. 23–24].

Empirical research has demonstrated several such biases in information processing. We may overestimate the frequency of occurence of confirming or paradigmatic examples of our stereotypes simply because such instances are more easily noticed, more easily brought to mind, and more easily retrieved from memory (see Hamilton and Gifford, 1976; Rothbart et al., unpublished). Evidence that confirms our stereotyped intuitions about human nature may be, in a word, more cognitively "available" (Tversky and Kahneman, 1973) than nonconfirming evidence.

Moreover, we may fill in the gaps in our evidence base with information consistent with our preconceived notions of what evidence should support our beliefs. For example, Chapman and Chapman (1967, 1969) have demonstrated that both college students and professional clinicians perceive positive associations between particular Rorschach responses and homosexuality in males, even though these associations are demonstrably absent in real life. These "signs" are simply those that comprise common cultural stereotypes of gay males.

Furthermore, once a stereotype has been adopted, a wide variety of evidence can be interpreted readily as supportive of that stereotype, including events that could support equally well an opposite interpretation. As Merton (1948) has suggested, in-group virtues ("We are thrifty") may become out-group vices ("They are cheap") in our attempts to maintain negative stereotypes about disliked out groups. (For empirical demonstrations of this bias, see Regan, Straus, and Fazio, 1974; Rosenhan, 1973; Zadny and Gerard, 1974).

Finally, selective recall and reinterpretation of information from an individual's past history may be exploited to support a current stereotype-based inference (see Loftus and Palmer, 1974). Thus, having decided that Jim is stingy (as are all members of his group), it may be all too easy to remember a variety of behaviors and incidents that are insufficient one at a time to support an attribution of stinginess, but that taken together do warrant and support such an inference.

Behavioral Confirmation of Social Stereotypes

The cognitive bolstering processes discussed above may provide the perceiver with an "evidence base" that gives compelling cognitive reality to any traits that he may have erroneously attributed to a target individual initially. This reality is, of course, entirely cognitive: It is in the eye and mind of the beholder. But stereotype-based attributions may serve as grounds for predictions about the target's future behavior and may guide and influence the perceiver's interactions with the target. This process itself may generate behaviors on the part of the target that erroneously confirm the predictions and validate the attributions of the perceiver. How others treat us is, in large measure, a reflection of our treatment of them (see Bandura,

1977; Mischel, 1968; Raush, 1965). Thus, when we use our social perceptions as guides for regulating our interactions with others, we may constrain their behavioral options (see Kelley and Stahelski, 1970).

Consider this hypothetical, but illustrative, scenario: Michael tells Jim that Chris is a cool and aloof person. Jim meets Chris and notices expressions of coolness and aloofness. Jim proceeds to overestimate the extent to which Chris's self-presentation reflects a cool and aloof disposition and underestimates the extent to which this posture was engendered by his own cool and aloof behavior toward Chris, that had in turn been generated by his own prior beliefs about Chris. Little does Jim know that Tom, who had heard that Chris was warm and friendly, found that his impressions of Chris were confirmed during their interaction. In each case, the end result of the process of "interaction guided by perceptions" has been the target person's *behavioral confirmation* of the perceiver's initial impressions of him.

This scenario makes salient key aspects of the process of behavioral confirmation in social interaction. The perceiver (either Jim or Tom) is not aware that his original perception of the target individual (Chris) is inaccurate. Nor is the perceiver aware of the causal role that his own behavior (here, the enactment of a cool or warm expressive style) plays in generating the behavioral evidence that erroneously confirms his expectations. Unbeknownst to the perceiver, the reality that he confidently perceives to exist in the social world has, in fact, been actively constructed by his own transactions with and operations upon the social world.

In our empirical research, we proposed to demonstrate that stereotypes may create their own social reality by channeling social interaction in ways that cause the stereotyped individual to behaviorally confirm the perceiver's stereotype. Moreover, we sought to demonstrate behavioral confirmation in a social interaction context designed to mirror as faithfully as possible the spontaneous generation of impressions in everyday social interaction and the subsequent channeling influences of these perceptions on dyadic interaction.

One widely held stereotype in this culture involves physical attractiveness. Considerable evidence suggests that attractive persons are assumed to possess more socially desirable personality traits and are expected to lead better lives than their unattractive counterparts (Berscheid and Walster, 1974). Attractive persons are perceived to have virtually every character trait that is socially desirable to the perceiver: "Physically attractive people, for example, were perceived to be more sexually warm and responsive, sensitive, kind, interesting, strong, poised, modest, sociable, and outgoing than persons of lesser physical attractiveness" (Berscheid and Walster, 1974, p. 169). This powerful stereotype holds for male and female perceivers and for male and female stimulus persons.

What of the validity of the physical attractiveness stereotype? Are the physically attractive actually more likable, friendly, and confident than the unattractive? Physically attractive young adults are more often and more eagerly sought out for social dates (Dermer, 1973; Krebs and Adinolphi, 1975; and Walster, et al., 1966).

Even as early as nursery school age, physical attractiveness appears to channel social interaction: The physically attractive are chosen and the unattractive are rejected in sociometric choices (Dion and Berscheid, 1974; Kleck, Richardson and Ronald, 1974).

Differential amount of interaction with the attractive and unattractive clearly helps the stereotype persevere, for it limits the chances for learning whether the two types of individuals differ in the traits associated with the stereotype. But the point we wish to focus upon here is that the stereotype may also channel interaction so that it behaviorally confirms itself. Individuals may have different styles of interaction for those whom they perceive to be physically attractive and for those whom they consider unattractive. These differences in interaction style may in turn elicit and nurture behaviors from the target person that are in accord with the stereotype. That is, the physically attractive may actually come to behave in a friendly, likable, sociable manner—not because they necessarily possess these dispositions, but because the behavior of others elicits and maintains behaviors taken to be manifestations of such traits.

Accordingly, we sought to demonstrate the behavioral confirmation of the physical attractiveness stereotype in dyadic social interaction. To do so, pairs of previously unacquainted individuals (designated, for our purposes, as a perceiver and a target) interacted in a getting-acquainted situation that had been constructed to allow us to control the information that one member of the dyad (the male perceiver) received about the physical attractiveness of the other individual (the female target). To measure the extent to which the actual behavior of the target matched the perceiver's stereotype, naive observer judges, who were unaware of the actual or perceived physical attractiveness of either participant, listened to and evaluated tape recordings of the interaction.

METHOD

Participants

Fifty-one male and 51 female undergraduates at the University of Minnesota participated, for extra course credit, in a study of "the processes by which people become acquainted with each other." Participants were scheduled in pairs of previously unacquainted males and females.

The Interaction Between Perceiver and Target

To insure that participants would not see each other before their interactions, they arrived at separate experimental rooms on separate corridors. The experimenter informed each participant that she was studying acquaintance processes in social

relationships. Specifically, she was investigating the differences between those initial interactions that involve nonverbal communication and those, such as telephone conversations, that do not. Thus, she explained, the participant would engage in a telephone conversation with another student in introductory psychology.

Before the conversation began, each participant provided written permission for it to be tape recorded. In addition, both dyad members completed brief questionnaires concerning such information as academic major in college and high school of graduation. These questionnaires, it was explained, would provide the partners with some information about each other with which to start the conversation.

Activating the perceiver's stereotype. The getting-acquainted interaction permitted control of the information that each male perceiver received about the physical attractiveness of his female target. When male perceivers learned about the biographical information questionnaires, they also learned that each person would receive a snapshot of the other member of the dyad, because "other people in the experiment have told us they feel more comfortable when they have a mental picture of the person they're talking to." The experimenter then used a Polaroid camera to photograph the male. No mention of any snapshots was made to female participants.

When each male perceiver received his partner's biographical information form, it arrived in a folder containing a Polaroid snapshot, ostensibly of his partner. Although the biographical information had indeed been provided by his partner, the photograph was not. It was one of eight photographs that had been prepared in advance.

Twenty female students from several local colleges assisted (in return for $5) in the preparation of stimulus materials by allowing us to take Polaroid snapshots of them. Each photographic subject wore casual dress, each was smiling, and each agreed (in writing) to allow us to use her photograph. Twenty college-age men then rated the attractiveness of each picture on a 10-point scale.[1] We then chose the four pictures that had received the highest attractiveness ratings ($M = 8.10$) and the four photos that had received the lowest ratings ($M = 2.56$). There was virtually no overlap in ratings of the two sets of pictures.

Male perceivers were assigned randomly to one of two conditions of perceived physical attractiveness of their targets. Males in the attractive target condition received folders containing their partners' biographical information form and one of the four attractive photographs. Males in the unattractive target condition received folders containing their partners' biographical information form and one of the four unattractive photographs. Female targets knew nothing of the photographs possessed by their male interaction partners, nor did they receive snapshots of their partners.

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[1]The interrater correlations of these ratings of attractiveness ranged from .45 to .92, with an average interrater correlation of .74.

The perceiver's stereotype-based attributions. Before initiating his getting-acquainted conversation, each male perceiver rated his initial impressions of his partner on an Impression Formation Questionnaire. The questionnaire was constructed by supplementing the 27 trait adjectives used by Dion, Berscheid, and Walster (1972) in their original investigation of the physical attractiveness stereotype with the following items: intelligence, physical attractiveness, social adeptness, friendliness, enthusiasm, trustworthiness, and successfulness. We were thus able to assess the extent to which perceivers' initial impressions of their partners reflected general stereotypes linking physical attractiveness and personality characteristics.

The getting-acquainted conversation. Each dyad then engaged in a 10-minute unstructured conversation by means of microphones and headphones connected through a Sony TC-570 stereophonic tape recorder that recorded each participant's voice on a separate channel of the tape.

After the conversation, male perceivers completed the Impression Formation Questionnaires to record final impressions of their partners. Female targets expressed self-perceptions in terms of the items of the Impression Formation Questionnaire. Each female target also indicated, on 10-point scales, how much she had enjoyed the conversation, how comfortable she had felt while talking to her partner, how accurate a picture of herself she felt that her partner had formed as a result of the conversation, how typical her partner's behavior had been of the way she usually was treated by men, her perception of her own physical attractiveness, and her estimate of her partner's perception of her physical attractiveness. All participants were then thoroughly and carefully debriefed and thanked for their contribution to the study.

Assessing Behavioral Confirmation

To assess the extent to which the actions of the target women provided behavioral confirmation for the stereotypes of the men perceivers, eight male and four female introductory psychology students rated the tape recordings of the getting-acquainted conversations. These observer judges were unaware of the experimental hypotheses and knew nothing of the actual or perceived physical attractiveness of the individuals on the tapes. They listened, in random order, to two 4-minute segments (one each from the beginning and end) of each conversation. They heard *only* the track of the tapes containing the target women's voices and rated each woman on the 34 bipolar scales of the Impression Formation Questionnaire, as well as on 14 additional 10-point scales; for example, "How animated and enthusiastic is this person?," "How intimate or personal is this person's conversation?," and "How much is she enjoying herself?" Another group of observer judges (three

males and six females) performed a similar assessment of the male perceivers' behavior based upon only the track of the tapes that contained the males' voices.[2]

RESULTS

To chart the process of behavioral confirmation of social stereotypes in dyadic social interaction, we examined the effects of our manipulation of the target women's apparent physical attractiveness on (a) the male perceivers' initial impressions of them, and (b) the women's behavioral self-presentation during the interaction, as measured by the observer judges' ratings of the tape recordings.

The Perceivers' Stereotype

Did our male perceivers form initial impressions of their specific target women on the basis of general stereotypes that associate physical attractiveness and desirable personalities? To answer this question, we examined the male perceivers' initial ratings on the Impression Formation Questionnaire. Recall that these impressions were recorded *after* the perceivers had seen their partners' photographs, but *before* the getting-acquainted conversation.[3] Indeed, it appears that our male per-

[2]We assessed the reliability of our raters by means of intraclass correlations (Ebel, 1951), a technique that employs analysis-of-variance procedures to determine the proportion of the total variance in ratings due to variance in the persons being rated. The intraclass correlation is the measure of reliability most commonly used with interval data and ordinal scales that assume interval properties. Because the measure of interest was the mean rating of judges on each variable, the between-rater variance was not included in the error term in calculating the intraclass correlation. (For a discussion, see Tinsley and Weiss, 1975, p. 363.) Reliability coefficients for the coders' ratings of the females for all dependent measures ranged from .35 to .91 with a median of .755. For each dependent variable, a single score was constructed for each participant by calculating the mean of the raters' scores on that measure. Analyses of variance, including the time of the tape segment (early vs. late in the conversation) as a factor, revealed no more main effects of time or interactions between time and perceived attractiveness than would have been expected by chance. Thus, scores for the two tape segments were summed to yield a single score for each dependent variable. The same procedure was followed for ratings of male perceivers' behavior. In this case, the reliability coefficients ranged from .18 to .83 with a median of .61.

[3]These and all subsequent analyses are based upon a total of 38 observations, 19 in each of the attractive target and unattractive target conditions. Of the original 51 dyads, a total of 48 male-female pairs completed the experiment. In each of the remaining three dyads, the male participant had made reference during the conversation to the photograph. When this happened, the experimenter interrupted the conversation and immediately debriefed the participants. Of the remaining 48 dyads who completed the experimental procedures, 10 were eliminated from the analyses for the following reasons: In 4 cases the male participant expressed strong suspicion about the photograph; in 1 case, the conversation was not tape recorded because of a mechanical problem; and in 5 cases, there was a sufficiently large age difference (ranging from 6 to 18 years) between the participants that the males in these dyads reported that they had reacted very differently to their partners than they would have reacted to an age peer. This pattern of attrition was independent of assignment to the attractive target and unattractive target experimental conditions ($\chi^2 = 1.27$, *ns*).

ceivers did fashion their initial impressions of their female partners on the basis of stereotyped beliefs about physical attractiveness, multivariate $F(34, 3) = 10.19$, $p < .04$. As dictated by the physical attractiveness stereotype, men who anticipated physically attractive partners expected to interact with comparatively sociable, poised, humorous, and socially adept women; by contrast, men faced with the prospect of getting acquainted with relatively unattractive partners fashioned images of rather unsociable, awkward, serious, and socially inept women, all Fs $(1, 36) < 5.85$, $p < .025$.

Behavioral Confirmation

Not only did our perceivers fashion their images of their discussion partners on the basis of their stereotyped intuitions about beauty and goodness of character, but these impressions initiated a chain of events that resulted in the behavioral confirmation of these initially erroneous inferences. Our analyses of the observer judges' ratings of the women's behavior were guided by our knowledge of the structure of the men's initial impressions of their target women's personality. Specifically, we expected to find evidence of behavioral confirmation only for those traits that had defined the perceivers' stereotypes. For example, male perceivers did not attribute differential amounts of sensitivity or intelligence to partners of differing apparent physical attractiveness. Accordingly, we would not expect that our observer judges would "hear" different amounts of intelligence or sensitivity in the tapes. By contrast, male perceivers did expect attractive and unattractive targets to differ in sociability. Here we would expect that observer judges would detect differences in sociability between conditions when listening to the women's contributions to the conversations, and thus we would have evidence of behavioral confirmation.

To assess the extent to which the women's behavior, as rated by the observer judges, provided behavioral confirmation for the male perceivers' stereotypes, we identified, by means of a discriminant analysis (Tatsuoka, 1971), those 21 trait items of the Impression Formation Questionnaire for which the mean initial ratings of the men in the attractive target and unattractive target conditions differed by more than 1.4 standard deviations.[4] This set of "stereotype traits" (e.g., sociable, poised, sexually warm, outgoing) defines the differing perceptions of the personality characteristics of target women in the two experimental conditions.

We then entered these 21 stereotype traits and the 14 additional dependent measures into a multivariate analysis of variance. This analysis revealed that our observer judges did indeed view women who had been assigned to the attractive target condition quite differently than women in the unattractive target condition,

[4]After the 21st trait dimension, the differences between the experimental conditions drop off sharply. For example, the next adjective pair down the line has a difference of 1.19 standard deviations, and the one after that has a difference of 1.02 standard deviations.

Fm(35, 2) = 40.0003, $p < .025$. What had initially been reality in the minds of the men had now become reality in the behavior of the women with whom they had interacted—a behavioral reality discernible even by naive observer judges, who had access *only* to tape recordings of the women's contributions to the conversations.

When a multivariate analysis of variance is performed on multiple correlated dependent measures, the null hypothesis states that the vector of means is equal across conditions. When the null hypothesis is rejected, the nature of the difference between groups must then be inferred from inspection of group differences on the individual dependent measures. In this case, the differences between the behavior of the women in the attractive target and the unattractive target conditions were in the same direction as the male perceivers' initial stereotyped impressions for fully 17 of the 21 measures of behavioral confirmation. The binomial probability that at least 17 of these adjectives would be in the predicted direction by chance alone is a scant .003. By contrast, when we examined the 13 trait pairs that our discriminant analysis had indicated did *not* define the male perceivers' stereotype, a sharply different pattern emerged. Here, we would not expect any systematic relationship between the male perceivers' stereotyped initial impressions and the female targets' actual behavior in the getting-acquainted conversations. In fact, for only 8 of these 13 measures is the difference between the behavior of the women in the attractive target condition in the same direction as the men's stereotyped initial impressions. This configuration is, of course, hardly different from the pattern expected by chance alone if there were no differences between the groups (exact binomial $p = .29$). Clearly, then, behavioral confirmation manifested itself only for those attributes that had defined the male perceivers' stereotype; that is, only in those domains where the men believed that there did exist links between physical attractiveness and personal attributes did the women come to behave differently as a consequence of the level of physical attractiveness that we had experimentally assigned to them.

Moreover, our understanding of the nature of the difference between the attractive target and the unattractive target conditions identified by our multivariate analysis of variance and our confidence in this demonstration of behavioral confirmation are bolstered by the consistent pattern of behavioral differences on the 14 additional related dependent measures. Our raters assigned to the female targets in the attractive target condition higher ratings on *every* question related to favorableness of self-presentation. Thus, for example, those who were thought by their perceivers to be physically attractive appeared to the observer judges to manifest greater confidence, greater animation, greater enjoyment of the conversation, and greater liking for their partners than those women who interacted with men who perceived them as physically unattractive.[5]

[5]We may eliminate several alternative interpretations of the behavioral confirmation effect. Women who had been assigned randomly to the attractive target condition were not in fact more physi-

In Search of Mediators of Behavioral Confirmation

We next attempted to chart the process of behavioral confirmation. Specifically, we searched for evidence of the behavioral implications of the perceivers' stereotypes. Did the male perceivers present themselves differently to target women whom they assumed to be physically attractive or unattractive? Because we had 50 dependent measures[6] of the observer judges' ratings of the males—12 more than the number of observations (male perceivers)—a multivariate analysis of variance is inappropriate. However, in 21 cases, univariate analyses of variance did indicate differences between conditions (all ps $< .05$). Men who interacted with women whom they believed to be physically attractive appeared (to the observer judges) more sociable, sexually warm, interesting, independent, sexually permissive, bold, outgoing, humorous, obvious, and socially adept than their counterparts in the unattractive target condition. Moreover, these men were seen as more attractive, more confident, and more animated in their conversation than their counterparts. Further, they were considered by the observer judges to be more comfortable, to enjoy themselves more, to like their partners more, to take the initiative more often, to use their voices more effectively, to see their women partners as more attractive, and, finally, to be seen as more attractive by their partners than men in the unattractive target condition.

It appears, then, that differences in the level of sociability manifested and expressed by the male perceivers may have been a key factor in bringing out reciprocating patterns of expression in the target women. One reason that target women who had been labeled as attractive may have reciprocated these sociable overtures is that they regarded their partners' images of them as more accurate, $F(1, 28) = 6.75$, $p < .02$, and their interaction style to be more typical of the way men generally treated them, $F\ (1, 28) = 4.79$, $p < .04$, than did women in the unattractive target

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cally attractive than those who were assigned randomly to the unattractive target condition. Ratings of the actual attractiveness of the female targets by the experimenter revealed no differences whatsoever between conditions, $t(36) = .00$. Nor, for that matter, did male perceivers differ in their own physical attractiveness as a function of experimental condition, $f(36) = .44$. In addition, actual attractiveness of male perceivers and actual attractiveness of female targets within dyads were independent of each other, $r(36) = .06$.

Of greater importance, there was no detectable difference in personality characteristics of females who had been assigned randomly to the attractive target and unattractive target conditions of the experiment. They did not differ in self-esteem as assessed by the Janis–Field–Eagly (Janis and Field, 1973) measure, $F(1, 36) < 1$. Moreover, there were no differences between experimental conditions in the female targets' self-perceptions as reported after the conversations on the Impression Formation Questionnaire ($Fm < 1$). We have thus no reason to suspect that any systematic, pre-existing differences between conditions in morphology or personality can pose plausible alternative explanations of our demonstration of behavioral confirmation.

[6]Two dependent measures were added between the time that the ratings were made of the female participants and the time that the ratings were made of the male participants. These measures were responses to the questions, "How interested is he in his partner?" and "How attractive does he think his partner is?"

condition.[7] These individuals, perhaps, rejected their partners' treatment of them as unrepresentative and defensively adopted more cool and aloof postures to cope with their situations.

DISCUSSION

Of what consequence are our social stereotypes? Our research suggests that stereotypes can and do channel dyadic interaction so as to create their own social reality. In our demonstration, pairs of individuals got acquainted with each other in a situation that allowed us to control the information that one member of the dyad (the perceiver) received about the physical attractiveness of the other person (the target). Our perceivers, in anticipation of interaction, fashioned erroneous images of their specific partners that reflected their general stereotypes about physical attractiveness. Moreover, our perceivers had very different patterns and styles of interaction for those whom they perceived to be physically attractive and unattractive. These differences in self-presentation and interaction style, in turn, elicited and nurtured behaviors of the target that were consistent with the perceivers' initial stereotypes. Targets who were perceived (unbeknownst to them) to be physically attractive actually came to behave in a friendly, likable, and sociable manner. The perceivers' attributions about their targets based upon their stereotyped intuitions about the world had initiated a process that produced behavioral confirmation of those attributions. The initially erroneous attributions of the perceivers had become real: The stereotype had truly functioned as a self-fulfilling prophecy (Merton, 1948).[8]

We regard our investigation as a particularly compelling demonstration of behavioral confirmation in social interaction. For if there is any social–psychological process that ought to exist in "stronger" form in everyday interaction than in the psychological laboratory, it is behavioral confirmation. In the context of years of social interaction in which perceivers have reacted to their actual physical attractiveness, our 10-minute getting-acquainted conversations over a telephone must seem minimal indeed. Nonetheless, the impact was sufficient to permit outside observers who had access only to one person's side of a conversation to detect manifestations of behavioral confirmation.

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[7]The degrees of freedom for these analyses are fewer than those for other analyses because they were added to the experimental procedure after four dyads had participated in each condition.

[8]Our research on behavioral confirmation in social interaction is a clear "cousin" of other demonstrations that perceivers' expectations may influence other individuals' behavior. Thus, Rosenthal (1974) and his colleagues have conducted an extensive program of laboratory and field investigation of the effects of experimenters' and teachers' expectations on the behavior of subjects in psychological laboratories and students in classrooms. Experimenters and teachers led to expect particular patterns of performance from their subjects and pupils act in ways that selectively influence or shape those performances to confirm initial expectations (e.g., Rosenthal, 1974).

Might not other important and widespread social stereotypes—particularly those concerning sex, race, social class, and ethnicity—also channel social interaction so as to create their own social reality? For example, will the common stereotype that women are more conforming and less independent than men (see Broverman et al., 1972) influence interaction so that (within a procedural paradigm similar to ours) targets believed to be female will actually conform more, be more dependent, and be more successfully manipulated than interaction partners believed to be male? At least one empirical investigation has pointed to the possible self-fulfilling nature of apparent sex differences in self-presentation (Zanna and Pack, 1975).

Any self-fulfilling influences of social stereotypes may have compelling and pervasive societal consequences. Social observers have for decades commented on the ways in which stigmatized social groups and outsiders may fall "victim" to self-fulfilling cultural stereotypes (e.g., Becker, 1963; Goffman, 1963; Merton, 1948; Myrdal, 1944; Tannenbaum, 1938). Consider Scott's (1969) observations about the blind:

> When, for example, sighted people continually insist that a blind man is helpless because he is blind, their subsequent treatment of him may preclude his even exercising the kinds of skills that would enable him to be independent. It is in this sense that stereotypic beliefs are self-actualized [p. 9].

And all too often it is the "victims" who are blamed for their own plight (see Ryan, 1971) rather than the social expectations that have constrained their behavioral options.

Of what import is the behavioral confirmation process for our theoretical understanding of the nature of social perception? Although our empirical research has focused on social stereotypes that are widely accepted and broadly generalized, our notions of behavioral confirmation may apply equally well to idiosyncratic social perceptions spontaneously formed about specific individuals in the course of every day social interaction. In this sense, social psychologists have been wise to devote intense effort to understanding the processes by which impressions of others are formed. Social perceptions are important precisely because of their impact on social interaction. Yet, at the same time, research and theory in social perception (mostly displayed under the banner of attribution theory) that have focused on the manner in which individuals process information provided them to form impressions of others may underestimate the extent to which information received in actual social interaction is a product of the perceiver's own actions toward the target individual. More careful attention must clearly be paid to the way in which perceivers *create* or *construct* the information that they process in addition to the way in which they *process* that information. Events in the social world may be as much the *effects* of our perceptions of those events as they are the *causes* of those perceptions.

From this perspective, it becomes easier to appreciate the perceiver's stubborn tendency to fashion images of others largely in trait terms (e.g., Jones and Nisbett, 1972), despite the poverty of evidence for the pervasive cross-situational consistencies in social behavior that the existence of "true" traits would demand (e.g.,

Mischel, 1968). This tendency, dubbed by Ross (1977) as the "fundamental attribution error," may be a self-erasing error. For even though any target individual's behavior may lack, overall, the trait-defining properties of cross-situational consistency, the actions of the perceiver himself may produce consistency in the samples of behavior available to that perceiver. Our impressions of others may cause those others to behave in consistent traitlike fashion for us. In that sense, our trait-based impressions of others are veridical, even though the same individual may behave or be led to behave in a fashion perfectly consistent with opposite attributions by other perceivers with quite different impressions of that individual. Such may be the power of the behavioral confirmation process.

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37

Some Evidence for Heightened Sexual Attraction Under Conditions of High Anxiety

Donald G. Dutton and Arthur P. Aron

Male passersby were contacted either on a fear-arousing suspension bridge or a non–fear-arousing bridge by an attractive female interviewer who asked them to fill out questionnaires containing Thematic Apperception Test pictures. Sexual content of stories written by subjects on the fear-arousing bridge and tendency of these subjects to attempt postexperimental contact with the interviewer were both significantly greater. No significant differences between bridges were obtained on either measure for subjects contacted by a male interviewer. A third study manipulated anticipated shock to male subjects and an attractive female confederate independently. Anticipation of own shock but not anticipation of shock to confederate increased sexual imagery scores on the Thematic Apperception Test and attraction to the confederate. Some theoretical implications of these findings are discussed.

There is a substantial body of indirect evidence suggesting that sexual attractions occur with increased frequency during states of strong emotion. For example, heterosexual love has been observed to be associated both with hate (James, 1910; Suttie, 1935) and with pain (Ellis, 1936). A connection between "aggression" and sexual attraction is supported by Tinbergen's (1954) observations of intermixed courting and aggression behaviors in various animal species, and a series of experiments conducted by Barclay have indicated the existence of a similar phenomenon in human behavior. In one study, Barclay and Haber (1965) arranged for students

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in one class to be angered by having their professor viciously berate them for having done poorly on a recent test; another class served as a control. Subsequently, both groups were tested for aggressive feelings and for sexual arousal. A manipulation check was successful, and the angered group manifested significantly more sexual arousal than did the control group ($p < .01$) as measured by explicit sexual content in stories written in response to Thematic Apperception Test (TAT)-like stimuli. Similar results were obtained in two further studies (Barclay, 1969, 1970) in which fraternity and sorority members were angered by the experimenter. The 1970 study employed a female experimenter, which demonstrated that the aggression–sexual arousal link was not specific to male aggression; the 1969 study provided additional support for the hypothesis by using a physiological measure of sexual arousal (acid phosphatase content in urine samples).

Barclay has explained his findings in terms of a special aggression–sexuality link and has cited as support for his position Freud's (1938) argument that prehistoric man had to physically dominate his potential mates and also a study by Clark (1952) in which increased sexual arousal produced by viewing slides of nudes yielded increased aggression in TAT responses. Aron (1970), on the other hand, argued that an aggression–sexuality link exists, but it is only a special case of a more general relationship between emotional arousal of all kinds and sexual attraction. To demonstrate this point, he designed a study in which instead of anger, residual emotion from intense role playing was the independent variable. In this experiment, each of 40 male subjects role played with the same attractive female confederate in either a highly emotional or a minimally emotional situation. Subjects enacting highly emotional roles included significantly more sexual imagery in stories written in response to TAT-like stimuli ($p < .01$) and indicated significantly more desire to kiss the confederate ($p < .05$) than did subjects in the control condition. One possible explanation is suggested by Schachter's theory of emotion (Schachter, 1964; Schachter and Singer, 1962). He argued that environmental cues are used, in certain circumstances, to provide emotional labels for unexplained or ambiguous states of arousal. However, it is notable that much of the above-cited research indicates that a sexual attraction–strong emotion link may occur even when the emotions are unambiguous. Accordingly, taking into account both the Schachter position and findings from sexual attraction research in general, Aron (1970) hypothesized that strong emotions are relabeled as sexual attraction whenever an acceptable object is present, and emotion-producing circumstances do not require the full attention of the individual.

The present series of experiments is designed to test the notion that an attractive female is seen as more attractive by males who encounter her while they experience a strong emotion (fear) than by males not experiencing a strong emotion. Experiment 1 is an attempt to verify this proposed emotion–sexual attraction link in a natural setting. Experiments 2 and 3 are field and laboratory studies which attempt to clarify the results of Experiment 1.

EXPERIMENT 1

METHOD

Subjects

Subjects were males visiting either of two bridge sites who fit the following criteria: (a) between 18 and 35 years old and (b) unaccompanied by a female companion. Only one member of any group of potential subjects was contacted. A total of 85 subjects were contacted by either a male or a female interviewer.

Site

The experiment was conducted on two bridges over the Capilano River in North Vancouver, British Columbia, Canada. The "experimental" bridge was the Capilano Canyon Suspension Bridge, a 5-foot-wide, 450-foot-long, bridge constructed of wooden boards attached to wire cables that ran from one side to the other of the Capilano Canyon. The bridge has many arousal-inducing features such as *(a)* a tendency to tilt, sway, and wobble, creating the impression that one is about to fall over the side; *(b)* very low handrails of wire cable which contribute to this impression; and *(c)* a 230-foot drop to rocks and shallow rapids below the bridge. The "control" bridge was a solid wood bridge further upriver. Constructed of heavy cedar, this bridge was wider and firmer than the experimental bridge, was only 10 feet above a small, shallow rivulet which ran into the main river, had high handrails, and did not tilt or sway.

Procedure

As subjects crossed either the control or experimental bridge, they were approached by the interviewer.[1]

Female interviewer. The interviewer explained that she was doing a project for her psychology class on the effects of exposure to scenic attractions on creative expression. She then asked potential subjects if they would fill out a short questionnaire. The questionnaire contained six filler items such as age, education, prior visits to bridge, etc., on the first page. On the second page, subjects were instructed to write a brief, dramatic story based upon a picture of a young woman covering her face with one hand and reaching with the other. The instructions and the picture

[1]The interviewers were not told of the experimental hypothesis to prevent unintentional differential cueing of subjects in experimental and control groups.

(TAT Item 3GF) employed were adapted from Murray's (1943) *Thematic Apperception Test Manual.* A similar measure of sexual arousal has been employed in the Barclay studies (1969, 1970; Barclay and Haber, 1965) and in other sex-related experiments (Aron, 1970; Clark, 1952; Leiman and Epstein, 1961). The particular TAT item used in the present study was selected for its lack of obvious sexual content, since projective measures of sexual arousal based on explicit sexual stimuli tend to be highly sensitive to individual differences because of sexual defensiveness (Clark and Sensibar, 1955; Eisler, 1968; Leiman and Epstein, 1961; Lubin, 1960). If the subject agreed, the questionnaire was filled out on the bridge.

Stories were later scored for manifest sexual content according to a slightly modified version of the procedure employed by Barclay and Haber (1965). Scores ranged from 1 (no sexual content) to 5 (high sexual content) according to the most sexual reference in the story. Thus, for example, a story with any mention of sexual intercourse received 5 points; but if the most sexual reference was "girl friend," it received a score of 2; "kiss" counted 3; and "lover," 4.

On completion of the questionnaire, the interviewer thanked the subject and offered to explain the experiment in more detail when she had more time. At this point, the interviewer tore the corner off a sheet of paper, wrote down her name and phone number, and invited each subject to call, if he wanted to talk further. Experimental subjects were told that the interviewer's name was Gloria and control subjects, Donna, so that they could easily be classified when they called. On the assumption that curiosity about the experiment should be equal between control and experimental groups, it was felt that differential calling rates might reflect differential attraction to the interviewer.

Male interviewer. The procedure with the male interviewer was identical to that above. Subjects were again supplied with two fictitious names so that if they phoned the interviewer, they could be classified into control or experimental groups.

RESULTS

Check on Arousal Manipulation

Probably the most compelling evidence for arousal on the experimental bridge is to observe people crossing the bridge. Forty percent of subjects observed crossing the bridge walked very slowly and carefully, clasping onto the handrail before taking each step. A questionnaire was administered to 30 males who fit the same criteria as the experimental subjects. Fifteen males on the experimental bridge were asked, "How fearful do you think the average person would be when he crossed this bridge?" The mean rating was 79 on a 100-point scale where 100 was equal to extremely fearful. Fifteen males on the control bridge gave a mean rating of 18 on

the same scale ($t = 9.7$, $df = 28$, $p < .001$, two-tailed). In response to the question "How fearful were you while crossing the bridge?" experimental-bridge males gave a rating of 65 and control-bridge males a rating of 3 ($t = 10.6$, $p < .001$, $df = 28$, two-tailed). Hence, it can be concluded that most people are quite anxious on the experimental bridge but not on the control bridge. To prevent suspicion, no checks on the arousal of experimental subjects could be made.

Thematic Apperception Test Responses

Female interviewer. On the experimental bridge, 23 of 33 males who were approached by the female interviewer agreed to fill in the questionnaire. On the control bridge, 22 of 33 agreed. Of the 45 questionnaires completed, 7 were unusable either because they were incomplete or written in a foreign language. The remaining 38 questionnaires (20 experimental and 18 control) had their TAT stories scored for sexual imagery by two scorers who were experienced with TAT scoring. (Although both were familiar with the experimental hypothesis, questionnaires had been coded so that they were blind as to whether any given questionnaire was written by a control or experimental subject.) The interrater reliability was +.87.

Subjects in the experimental group obtained a mean sexual imagery score of 2.47 and those in the control group, a score of 1.41 ($t = 3.19$, $p < .01$, $df = 36$, two-tailed). Thus, the experimental hypothesis was verified by the imagery data.

Male interviewer. Twenty-three out of 51 subjects who were approached on the experimental bridge agreed to fill in the questionnaire. On the control bridge 22 of 42 agreed. Five of these questionnaires were unusable, leaving 20 usable in both experimental and control groups. These were rated as above. Subjects in the experimental group obtained a mean sexual imagery score of .80 and those in the control group, .61 ($t = .36$, *ns*). Hence the pattern of result obtained by the female interviewer was not reproduced by the male interviewer.

Behavioral Data

Female interviewer. In the experimental group, 18 of the 23 subjects who agreed to the interview accepted the interviewer's phone number. In the control group, 16 of 22 accepted (see Table 37.1). A second measure of sexual attraction was the number of subjects who called the interviewer. In the experimental group 9 of 18 called, in the control group 2 of 16 called ($X^2 = 5.7$, $p < .02$). Taken in

TABLE 37.1
Sexual attraction under conditions of high anxiety
Behavioral responses and thematic apperception test imagery scores for each experimental group

Interviewer	No. filling in questionnaire	No. accepting phone number	No. phoning	Usable questionnaires	Sexual imagery score
Female					
Control bridge	22/23	16/22	2/16	18	1.41
Experimental bridge	23/33	18/23	9/18	20	2.47
Male					
Control bridge	22/42	6/22	1/6	20	.61
Experimental bridge	23/51	7/23	2/7	20	.80

conjunction with the sexual imagery data, this finding suggests that subjects in the experimental group were more attracted to the interviewer.

Male interviewer. In the experimental group, 7 of 23 accepted the interviewer's phone number. In the control group, 6 of 22 accepted. In the experimental group, 2 subjects called; in the control group, 1 subject called. Again, the pattern of results obtained by the female interviewer was not replicated by the male.

Although the results of this experiment provide prima facie support for an emotion–sexual attraction link, the experiment suffers from interpretative problems that often plague field experiments. The main problem with the study is the possibility of different subject populations on the two bridges. First, the well-advertised suspension bridge is a tourist attraction that may have attracted more out-of-town persons than did the nearby provincial park where the control bridge was located. This difference in subject populations may have affected the results in two ways. The experimental subjects may have been less able to phone the experimenter (if they were in town on a short-term tour) and less likely to hold out the possibility of further liaison with her. If this were the case, the resulting difference due to subject differences would have operated *against* the main hypothesis. Also, this difference in subject populations could not affect the sexual imagery scores unless one assumed the experimental bridge subjects to be more sexually deprived than controls. The results using the male interviewer yielded no significant differences in sexual imagery between experimental and control subjects; however, the possibility still exists that sexual deprivation could have interacted with the presence of the attractive female experimenter to produce the sexual imagery results obtained in this experiment.

Second, differences could exist between experimental and control populations with respect to personality variables. The experimental population might be more predisposed to thrill seeking and therefore more willing to chance phoning a strange female to effect a liaison. Also, present knowledge of personality theory does not allow us to rule out the combination of thrill seeking and greater sexual

imagery. Accordingly, a second experiment was carried out in an attempt to rule out any differential subject population explanation for the results of Experiment 1.

EXPERIMENT 2

METHOD

Subjects

Subjects were 34 males visiting the suspension bridge who fit the same criteria as in Experiment 1.

Procedure

The chief problem of Experiment 2 was choosing a site that would allow contact with aroused and nonaroused members of the same subject population. One possibility was to use as a control group suspension-bridge visitors who had not yet crossed the bridge or who had just gotten out of their cars. Unfortunately, if a substantial percentage of this group subsequently refused to cross the bridge, the self-selecting-subject problem of Experiment 1 would not be circumvented. Alternatively, males who had just crossed the bridge could be used as a control. The problem with this strategy was that this group, having just crossed the bridge, may have felt residual anxiety or elation or both, which would confound the study. To avoid this latter problem, control subjects who had just crossed the bridge and were sitting or walking in a small park were contacted at least 10 minutes after crossing the bridge. This strategy, it was hoped, would rule out residual physiological arousal as a confounding factor. Except that a different female experimenter was used in Experiment 2 and no male interviewer condition was run, all other details of the study were identical to Experiment 1.

RESULTS

Check on Arousal Manipulation

As with Experiment 1, no arousal manipulation check was given to experimental subjects in order not to arouse suspicion about the real intent of the experiment. Data for a group of nonexperimental subjects of the same age and sex as experimental subjects are reported in Experiment 1.

Thematic Apperception Test Responses

In the experimental group, 25 of 34 males who were approached agreed to fill in the questionnaire. In the control group, 25 of 35 agreed. Of the 50 questionnaires completed, 5 were unusable because they were incomplete. The remainder (23 experimental and 22 control) were scored for sexual imagery as in Experiment 1. The interrater reliability in Experiment 2 was + .79.

Subjects in the experimental group obtained a mean sexual imagery score of 2.99 and those in the control group, a score of 1.92 ($t = 3.07$, $p < .01$, $df = 36$, two-tailed). Thus the experimental hypothesis was again verified by the imagery data.

Behavioral Data

In the experimental group, 20 of the 25 subjects who agreed to the interview accepted the interviewer's phone number. In the control group, 19 of 23 accepted. In the experimental group, 13 of 20 called, while in the control group, 7 of 23 phoned ($X^2 = 5.89$, $p < .02$). Thus the behavioral result of Experiment 1 was also replicated.

Experiment 2 enables the rejection of the notion of differential subject populations as an explanation for the control–experimental-bridge differences for female interviewers in Experiment 1. However, some additional problems in the interpretation of the apparent anxiety–sexual attraction link require the superior control afforded by a laboratory setting.

First, although the female experimenter was blind to the experimental hypothesis and her behavior toward the subjects was closely monitored by the experimenter, the possibility of differential behavior toward the subjects occurring was not excluded. Distance of the interviewer from the subjects was controlled in both Experiments 1 and 2, but more stable nonverbal forms of communication (such as eye contact) could not be controlled without cueing the female interviewer to the experimental hypothesis.

Second, even if the interviewer did not behave differentially in experimental and control conditions, she may have appeared differently in the two conditions. For example, the gestalt created by the experimental situation may have made the interviewer appear more helpless or frightened, virtually a "lady in distress." Such would not be the case in the control situation.

If this different gestalt led to differences in sexual attraction, the apparent emotion–sexual arousal link might prove artifactual. Accordingly, a laboratory experiment was run in which tighter control over these factors could be obtained. This experiment involved a 2×2 factorial design, where *(a)* the male subject expected either a painful or nonpainful shock (subject's emotion was manipulated) and *(b)* the female confederate also expected either a painful or nonpainful shock (the lady-in-distress gestalt was manipulated).

EXPERIMENT 3

METHOD

Subjects

Eighty male freshmen at the University of British Columbia took part in this experiment. All subjects were volunteers.

Much of the initial phase of the procedure was patterned after that used in Schachter's (1959) anxiety and affiliation research. Subjects entered an experimental room containing an array of electrical equipment. The experimenter welcomed the subject and asked him if he had seen another person who looked like he was searching for the experimental room. The experimenter excused himself "to look for the other subject," leaving the subject some Xeroxed copies "of previous studies in the area we are investigating" to read. The articles discussed the effects of electric shock on learning and pain in general.

The experimenter reentered the room with the "other subject," who was an attractive female confederate.[2] The confederate took off her coat and sat on a chair three feet to the side and slightly in front of the subject. The experimenter explained that the study involved the effects of electric shock on learning and delivered a short discourse on the value and importance of the research. At the end of this discourse, the experimenter asked if either subject wanted out of the experiment. As expected, no subject requested to leave.

The experimenter then mentioned that two levels of shock would be used in the experiment, describing one as quite painful and the other level as a "mere tingle, in fact some subjects describe it as enjoyable," and concluded by pointing out that the allocation of subjects to shock condition had to be "completely random so that personality variables won't affect the outcome." At this point, the experimenter asked both subjects to flip a coin to determine which shock level they would receive.[3] Hence, the subject reported "heads/tails," the confederate reported "heads/tails," and the experimenter said, "Today heads receives the high shock level." The ex-

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[2]The female confederate knew that the study involved sexual attraction but did not know the experimental hypothesis. Her every action in the experimental room was carefully rehearsed to avoid any possibility of differential behavior among experimental conditions. Spacing of the confederate's chair from the subject's was carefully controlled, and the confederate was instructed to avoid any eye contact with the subject after their initial introduction. Hence, eye contact was restricted to the confederate's entering the room and returning to her chair after removing her coat. Both the confederate's and the subject's chairs faced the same direction (toward the experimenter), so that eye contact was easily avoided. In addition, the confederate's chair was somewhat closer to the experimenter than was the subject's chair, so that the subject could see the confederate while the experimenter delivered the instructions.

[3]The confederate was told to report either the same result as the subject or a different result (of the coin flip) to facilitate obtaining equal *ns* for experimental conditions as quickly as possible.

perimenter then described the way in which the shock series would take place, the method of hooking subjects into electrodes, etc.

The experimenter then asked if the subjects had any questions, answered any that arose, and then said

> It will take me a few minutes to set up this equipment. While I'm doing it, I would like to get some information on your present feelings and reactions, since these often influence performance on the learning task. I'd like you to fill out a questionnaire to furnish us with this information. We have two separate cubicles down the hall where you can do this—you will be undisturbed and private, and I can get this equipment set up.

The confederate then got up, walked in front of the subject to her coat, which was hanging on the wall, rummaged around for a pencil, and returned to her chair. The experimenter then led the subject and the confederate to the cubicles, where they proceeded to fill out the questionnaires.

RESULTS

A three-part questionnaire constituted the dependent measure of this study. Part 1 (feelings about the experiment) included a check on the anxiety manipulation, Part 2 (feelings toward your co-subject) included two attraction questions found to be most sensitive in experimental situations of this sort (Aron, 1970), and Part 3 included the TAT picture used in Experiments 1 and 2, which was again scored for sexual imagery.

Anxiety

Anxiety was measured by the question "How do you feel about being shocked?" (cf., Schachter, 1959) to which subjects could respond on a 5-point scale where scores greater than 3 indicated dislike. (The greater the score, the greater the anxiety.) Table 37.2 presents the results on this measure. In conditions where the subject anticipated receiving a strong shock, subjects reported significantly more anxiety than in conditions where the subject anticipated receiving a weak shock

TABLE 37.2
Reported anxiety in experimental conditions

Subject expects:	Female confederate to get strong shock	Female confederate to get weak shock	No female confederate
Strong shock	3.17	3.05	3.80
Weak shock	2.42	2.28	

Note: n per cell = 20.

TABLE 37.3
Attraction ratings by experimental condition

Subject expects:	Female confederate to get strong shock	Female confederate to get weak shock
Strong shock	3.7	3.4
Weak shock	2.9	2.7

Note: Strongest attraction rating is 5.

($t = 4.03$, $p < .001$, $df = 39$, one-tailed). In conditions where the subject anticipated receiving a strong shock with the female cosubject present, subjects reported significantly less anxiety than in a control condition ($n = 20$), where two male subjects were run ($t = 2.17$, $p < .025$, $df = 19$, one-tailed). No significant differences in the subject's anxiety occurred as a function of the confederate receiving a strong versus a weak shock (see Table 37.2).

Attraction to Confederate

Two questions assessed attraction to the confederate in this study: *(a)* How much would you like to ask her out for a date? and *(b)* how much would you like to kiss her? (An alternative set of questions was provided for those subjects who ostensibly had a male copartner. The experimenter instructed subjects in this condition to overlook these.) Attraction ratings were established by taking the mean rating made by subjects on these two questions. Table 37.3 shows the results, by condition, of those ratings. A 2×2 analysis of variance revealed a significant main effect for subjects anticipating strong shock to themselves on attraction ratings ($F = 22.8$, $p < .001$). Subjects' expectations of strong versus weak shock to the female confederate produced no significant increase in attraction ($F = 2.61$, *ns*). (There was no significant interaction.) Hence, the lady-in-distress effect on attraction did not seem to appear in this study.

Thematic Apperception Test Responses

Sexual imagery scores on the TAT questionnaire were obtained as in Experiments 1 and 2 and are shown in Table 37.4. In the present study, sexual imagery was higher when the subject expected strong shock but only when the female confederate also expected strong shock ($F = 4.73$, $p < .05$). When the female confederate expected weak shock, differences in sexual imagery scores as a function of strength of shock anticipated by the subject failed to achieve significance ($F = 4.22$, $p = .07$).

TABLE 37.4
Sexual imagery scores by experimental condition

Subject expects:	Female confederate to get strong shock	Female confederate to get weak shock
Strong shock	2.27	2.19
Weak shock	1.52	1.69

Note: Strongest imagery score is 5.

GENERAL DISCUSSION

The results of these studies would seem to provide a basis of support for an emotion–sexual attraction link. The Barclay studies (Barclay, 1969, 1970; Barclay and Haber, 1965) have already demonstrated such a link for aggression and sexual arousal, and the present findings seem to suggest that the link may hold for fear as well. Indeed, the present outcome would seem to be particularly satisfying in light of the very strong differences obtained from the relatively small subject populations, and because these results were obtained, in Experiments 1 and 2, outside of the laboratory in a setting in which real-world sexual attractions might be expected to occur.

The strong result of Experiment 3 supports the notion that strong emotion per se increases the subject's sexual attraction to the female confederate. Brehm, Gatz, Goethals, McCrimmon, and Ward (1967) obtained results consistent with Experiment 3 in a similar study. They also had male subjects threatened by impending electric shock in two experimental groups and not threatened in the control group. They obtained an *F* significant at the 5 percent level for differences between the threat and no-threat groups.

The theoretical implications of these results are twofold. First, they provide additional support in favor of the theoretical positions from which the original hypothesis was derived: the Schachter and Singer (1962) tradition of cognitive labeling of emotions and the Aron (1970) conceptual framework for sexual attraction processes. Second, these data seem to be inconsistent with (or at least unpredictable by) standard theories of interpersonal attraction. Both the reinforcement (Byrne, 1969) and the cognitive consistency (Festinger, 1957; Heider, 1958) points of view would seem to predict that a negative emotional state associated with the object would *decrease* her attractiveness; and neither theory would seem to be easily capable of explaining the arousal of a greater sexual emotion in the experimental condition of the present experiments.

Although the present data support the cognitive relabeling approach in general, they are consistent with more than one interpretation of the mechanics of the process. The attribution notions of Nisbett and Valins (1972), self-perception theory (Bem, 1972), and role theory (Sarbin and Allen, 1968) can all provide possible explanations for the anxiety–sexuality link. A further possible explanation is that

heightened emotion, instead of being relabeled as sexual, serves merely to disinhibit the expression of preexistent sexual feelings. It is known that inhibition and sexual defensiveness influence sexual content in TAT stories (Clark, 1952), and this alternative cannot be ruled out by the present data. Yet another alternative suggested by Barclay (personal communication, 1971), is that the aggression–sexuality and anxiety–sexuality links may be independent phenomena and not necessarily subcases of a general emotion–sexuality link.

Some evidence for the mechanics of the anxiety–sexual arousal link in the current research may be obtained from the fear ratings made by subjects in Experiment 3. When subjects anticipated receiving a strong shock and the female confederate was present during the anxiety manipulation, subjects reported significantly less fear than when no potential sexual object was present ($t = 2.17$, $df = 19$, $p < .025$). Since the questionnaires were filled out in private in both groups, it is unlikely that subjects' reporting merely reflects appropriate behavior in the presence of the opposite or same sex. One possible explanation for this result is that, having relabeled anxiety as sexual arousal, the subject is less likely to feel anxious. A more conclusive explanation of the mechanics of the anxiety–sexual arousal link must await the conclusion of present laboratory studies designed specifically to investigate this problem. However, regardless of the interpretation of the mechanics of this link, the present research presents the clearest demonstration to date of its existence.

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38

Cognitive, Interpersonal, and Behavioral Effects of Social Exclusion: How People Respond to Rejection

Roy F. Baumeister and Dianne M. Tice

Long ago, animals began to use social life as a biological strategy: They began to interact with one another to get what they needed for survival and reproduction. This biological strategy became increasingly effective as nature selected in favor of animals who were motivated to be with others. In that context, social exclusion—being rejected by others—represented a powerful and disturbing threat. Animals that depend on one another for their most basic needs are at risk when they cannot interact and work together, and their drive to be together is thwarted when they are excluded. Like other social animals, humans have a powerful need to belong and they find their prospects for survival and reproduction diminished if they are alone in the world. Even in the modern world, people who lack social ties show higher rates of mental and physical illness, among other problems (for review, see Baumeister and Leary, 1995).

In fact, the problem of social exclusion may have an added dimension for human beings. Humans are not just social animals, but also cultural animals. Culture is an advanced way of being social, and it relies heavily on information, communication, division of labor, exchange, and other features, all of which depend on the interactive social group. Many of the distinctively human mental and physical attributes can best be understood as selected by nature to facilitate culture (Baumeister, 2005). For example, the physical and mental requirements for speaking and hearing are tailored to facilitate language use, but language exists only as part of a group culture,

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and so those attributes evolved to facilitate social connection. Hence, it seems fair to say that being socially excluded represents a threat to many essential aspects of life for which human beings were designed.

This article will provide an overview of a laboratory research program that we, together with several colleagues, have pursued over the past decade. We have sought to understand the direct causal consequences of being rejected and socially excluded. The study began with behavioral effects, and then slowly we began to investigate the inner reactions and processes that help to produce the behavioral effects.

METHODS FOR STUDYING EXCLUSION

Researchers have devised multiple procedures for studying social exclusion. It is common for investigators to use several different methods in the same multistudy investigation to provide converging evidence. Such convergence strengthens conclusions. Although it does possibly gloss over potential differences in the psychological impact of the various methods, some recent work has begun to explore how some methods could yield different effects (Molden, Lucas, and Gardner, 2007). Such different reactions to different procedures might help explain the occasional discrepancies between findings from different investigations. In this brief report, we shall focus on convergences, but the exploration of differential impacts may be a promising area for future research.

One approach is to simulate a rejection experience by having participants believe that people they have just met have rejected them. In this procedure, a group has a get-acquainted conversation followed by having participants list the two people they would most like to work with, ostensibly in preparation for pairing off for the next task. By random assignment, some participants are told that no one chose to work with them (Nezlek, Kowalski, Leary, Blevins, and Holgate, 1997; Twenge, Baumeister, Tice, and Stucke, 2001).

A variation on this has two participants exchange information about themselves, ostensibly in preparation for an interactive task. Participants are then told that the interactive task has to be canceled, and by random assignment one of two explanations is offered: It is either because the partner suddenly remembered another appointment and had to leave, or because the partner reacted negatively to the participant's disclosures and chose not to interact further. In this procedure, both conditions end with the participant alone and the interaction canceled, but in one case it is a personal rejection, whereas in the other it is an impersonal and ostensibly random one (DeWall, Baumeister, and Vohs, 2006; also Bushman, Bonacci, Van Dijk, and Baumeister, 2003).

Another procedure relies on having participants recall a previous experience of rejection from their actual lives (DeWall and Baumeister 2006; Gardner, Pickett, and Brewer, 2000). Usually the reliving of the rejection is bolstered by having participants write about the experience. Having participants imagine a rejection experience (Leary, Springer, Negel, Ansell, and Evans, 1998) has the same effect as

does the procedure of exposing participants to subtle or even subliminal primes of the idea of rejection (Sommer and Baumeister, 2002).

There are reasons to suspect that the imagined or recalled rejection manipulations may differ from others. Imaginary or hypothetical responses often differ in important ways from actual ones (e.g., Twenge, Koole, DeWall, Marquez, and Baumeister, 2006), and recalled experiences may be meaningfully understood and digested in ways that freshly occurring ones are not. One solitary procedure that avoids these pitfalls relies on bogus predictions about the future. In this procedure (e.g., Twenge et al., 2001), participants first take an ostensible personality test. While receiving feedback about their scores on the test, they are told that the feedback includes projections about their future social lives. By random assignment, some are told that people with their profile typically end up alone in life. Their current friends and lovers are likely to drift away, and new relationships will gradually become less frequent and more ephemeral. This is called the "future alone" condition. It is usually contrasted with any of several control groups, such as involving predictions that you will always be surrounded by people who love and care about you, or that you will have a future marred by physical misfortunes such as injuries and accidents, or no feedback about the future.

These laboratory procedures are all ways of exposing participants to a contrived experience of social exclusion under controlled laboratory conditions. They are a valuable complement to studies that explore social isolation and loneliness in actual social life. These laboratory procedures lack the impact of actual rejections, such as losing a loved one or being excluded from a desired group, but they offer the advantages of experimental design (e.g., permitting causal inference).

Naturally, with manipulations like this, it is important to be especially sensitive to the reactions of participants. Initially we worried that some people might become very upset by these manipulations, especially those who were provided with bogus feedback about being rejected. In all cases, we used very careful and thorough debriefings, and these were given within a minutes of the manipulation, so that the participant spends only a few minutes thinking that he or she has been rejected. We explained the purpose of the study and told people what the other experimental conditions were like, so they could imagine what it would have been like to be in those groups and could compare the different possible reactions. Fortunately, it is clear by now that these types of manipulations do not create problems. Several thousand research participants have gone through these procedures without being harmed or disturbed or even lodging a complaint. Still, we think it is important to remain careful and vigilant and to express our appreciation for what the participant undergoes during the study. We make a practice of apologizing to the participant for the deceptions and try to help each participant understand the value of what is being learned.

These manipulations do not exhaust the possibilities. The most notable one used in other laboratories involves ostracizing participants. The impressive program of research conducted by Williams and his colleagues (see Williams, 2001) has devel-

oped multiple procedures for studying ostracism in the laboratory, many of which involve having confederates or simulated confederates first engage the participant in a ball-tossing game and then gradually and without explanation stop throwing the ball to the participant.

INTERPERSONAL BEHAVIORS

How do socially excluded persons behave toward others? One might readily predict and understand that persons would act negatively toward the people who rejected them, but it is far harder to predict how rejected persons will treat new others, who might represent either an opportunity (for forming new social connections) or a threat (of being rejected again).

Seemingly the most adaptive response to rejection would be to become nice, friendly, agreeable, well-behaved, and generally prosocial. After all, if one group has rejected you, then you need to make new friends to replace the lost connections. One of the surprising findings of the rejection work was how hard it was to find any such positive, prosocial behaviors in the wake of rejection.

Antisocial behavior emerged in some of the earliest studies of rejection. Twenge and colleagues (2001) published a series of five studies on this topic. In one of them, 30 students arrived at the lab in small groups of four to six, donned name tags, and carried on a get-acquainted conversation for about 15 minutes during which they shared information about themselves according to a standard set of ice-breaker questions provided by the experimenter. After that, they were moved into separate rooms, where they filled out a demographic questionnaire (asking basic questions about age, home town, nationality, race, and so forth) and then were asked to list the two people from the group whom they most liked and respected and with whom they would most like to work together during an upcoming task. The experimenter collected these forms and then had participants write a brief essay explaining their views on abortion, which is a sensitive and controversial topic and therefore useful for provoking a conflict.

The crucial manipulation of social exclusion came next, as the experimenter went around to each room again. By random assignment, she told half the participants, “I have good news for you—everybody chose you as someone they’d like to work with.” In contrast, the rest were told, “I hate to tell you this, but no one chose you as someone they wanted to work with.”

The experimenter went on to explain that this outcome had complicated the formation of work groups, and so the participant would instead go on to play a reaction time game with a new person rather than with someone from the original group. There was a pause here to fill out a mood measure (which found no differences as a function of whether people had received acceptance or rejection feedback), and then the experimenter came back with an evaluation of the participant’s essay, ostensibly written by the interaction partner who was to be the opponent in

the upcoming reaction time game. In all cases, the evaluation was negative: It consisted of low numerical ratings and a handwritten comment saying, "One of the worst essays I've read!" Participants were told they would get a chance to evaluate an essay by the partner later in the experiment. Thus, they did not know the partner's views on abortion, but only that the partner had seemingly disliked their own essay.

The reaction time game contained the aggression measure. In reality, there was no other person; the participant was playing against a computer with a preset style of play. But participants were told they were playing against someone. They were given instructions stating that upon presentation of a signal they should press a particular key on the keyboard as fast as possible, that whichever player responded more slowly would be the loser and would hear a blast of unpleasant noise transmitted via headphones, and that—crucially—each player could determine in advance how nasty the noise heard by the other would be if the other person lost the next trial. The options for setting the noise ranged from fairly mild and brief to quite long and loud.

In a sense, everyone should have used the same noise settings, because everyone had received the same insulting note from the pretended partner and everyone was playing the same game. But the exclusion manipulation produced big differences. The participants who had been told that nobody from their original group had chosen them wanted to blast their partners with much louder, more unpleasant noise than participants who had been told that everybody liked and chose them (Table 38.1). In short, rejection caused aggression. More precisely, people who were rejected by the first group behaved more aggressively toward a new interaction partner.

Further studies by Twenge and colleagues (2001) replicated this pattern with different methods and added new information. The "future alone" manipulation (described earlier) yielded results similar to those with the group rejection procedure. Rejected participants were more aggressive than accepted ones. They were also more aggressive than participants who received other kinds of bad feedback that did not include social exclusion (i.e., the "misfortune control" condition, in which participants were told that later in life they would be prone to accidents and injuries). Rejected people were also more aggressive toward a new person even if

TABLE 38.1
Social exclusion and aggression

	Intensity (loudness) of noise
Accepted	3.53
Rejected	6.60

High scores indicate higher aggression. Difference significant at $p < .001$. Data adapted from Twenge (2001).

the new person had not insulted and provoked them. Only if the new person praised them (i.e., giving their abortion essay favorable ratings and writing "A very good essay!" on the form) were they reasonably nice to the new person. And even then they were not particularly low in aggression—they simply showed no *increase* in aggression.

In sum, rejected participants appear ready to behave in hostile, aggressive ways toward a broad assortment of others. These data converge with studies of rejection outside the laboratory. In particular, a survey of school shooting incidents by Leary, Kowalski, Smith, and Phillips (2003) concluded that nearly all the youngsters who shot classmates had felt severely rejected and excluded by them.

The central role of feeling disconnected was verified by Twenge, Zhang, Catanese, Dolan-Pascoe, Lyche, and Baumeister (2007). They replicated the increase in aggression following social exclusion but also showed that it can be eliminated by positive social contact. Even recalling and writing about a good relationship with a family member or friend was sufficient to blunt the hostile, aggressive reaction. That is, being rejected by one person and then feeling connected to a second person was enough to reduce aggression toward a third.

A seemingly parallel pattern is that just as rejected people become more aggressive, they also reduce their prosocial behavior. Twenge, Baumeister, DeWall, Ciarocco, and Bartels (2007) found that exclusion (manipulated by the group rejection or the personality feedback "future alone" procedure) caused decrements in a broad assortment of prosocial behaviors, including financial donations to the Student Emergency Fund, cooperation on the Prisoner's Dilemma Game, willingness to do favors requested by the experimenter, and even just helping someone pick up pencils that had been spilled on the floor.

Increasing aggression and reducing cooperative helpfulness hardly seem like recipes for making new friends. One interpretation is that rejected people simply turn into antisocial misanthropes who shun social connection. This would be surprising, however, particularly in light of the powerful need to belong and the adaptive benefits of belonging. As a general pattern, when motivations are thwarted or blocked, the individual increases efforts to satisfy it (at least at first). Is the need to belong really different?

There have been some signs that socially excluded people seek to cultivate new possible friends. Williams, Cheung, and Choi (2000) found that ostracized people conformed more to the opinions of others, and the researchers interpreted this as a desire to gain acceptance, although passivity would be an alternative explanation for such conforming. Gardner, Pickett, and Brewer (2000) showed that thinking about rejection caused people to attend more to social connection information when reading about others, though this included both other people's connection and exclusion experiences (see also Pickett, Gardner, and Knowles, 2004). This finding shows that rejected people are keenly interested in information about social connection and exclusion, and this heightened interest may reflect a desire to form new relationships, though it may also be influenced by heightened sensitivity and

accessibility stemming from the recent exclusion and could even denote some desire to avoid future rejections.

Further evidence that exclusion motivates a quest for new social bonds (albeit a cautious and skeptical one) was provided by Maner, DeWall, Baumeister, and Schaller (2007). They found that excluded people were more willing than others to want to join a campus-wide service designed to help people meet each other (and more willing to spend money for that service). Excluded persons also rated neutral faces as friendlier and more welcoming than did nonexcluded people. They were more prone than others to choose to do a task that involved working with a partner rather than alone. Last, excluded persons assigned bigger cash rewards to future interaction partners (though not to others), as long as the rewards did not come directly from their own money and the future interaction partners were clearly differentiated from the people who had rejected them.

Taken together, these findings suggest that excluded people approach others with mixed feelings. They seem highly (and understandably) sensitive to the possibility of further rejection and wish to avoid it; so much so that they can turn aggressive quite easily. They are reluctant to make the first move or make personal sacrifices for another. However, they are interested in meeting others, perhaps especially if the others make the first move or seem welcoming. Thus, they do seem to exhibit the standard motivational pattern of wanting to find a new way to satisfy the thwarted need to belong. But they also want to avoid being rejected again or exploited in some other way, consistent with the general pattern of people's dislike for being played for a sucker and their considerable caution about letting it happen again after a first negative experience (Vohs, Baumeister, and Chin, in press).

EXCLUSION AND EMOTION

Emotions are often tied to strong motivations and operate as evaluative signals about the relevance of events to these motivations (e.g., Baumeister, Vohs, DeWall, and Zhang, in press). Hence, it seemed straightforward to predict on both theoretical and intuitive grounds that the dominant reaction to being socially excluded would be an immediate wave of emotional distress. We began our study of social exclusion with the working hypothesis that emotion would mediate between rejection and whatever behavioral effects would follow. Yet the findings have not been kind to that simple theory and have in fact pointed toward far more complex patterns (e.g., Twenge et al., 2001).

There was ample reason to suppose that social exclusion would cause emotional distress. Leary and colleagues (1998; also Leary and Springer, 2000) showed that people associate social rejection with the familiar experience of having one's feelings hurt. Baumeister and Tice (1990) surveyed the literature on anxiety and concluded that being rejected or excluded was the most common and best established cause of anxiety. Williams's (2001) interviews and surveys found that ostracized

people reported considerable distress in connection with being ostracized, and he noted that he himself had such unpleasant emotional reactions that he could scarcely bring himself to conduct the interviews or even observe his own laboratory studies of exclusion.

Despite those promising signs, emotion often failed to show up in the laboratory studies of rejection. In multiple studies using various manipulations of social exclusion and many different measures of emotion, excluded participants failed to differ from accepted participants (e.g., Gardner et al., 2000; Twenge et al., 2001, 2002, 2003; Twenge and Campbell, 2003; Zadro, Williams, and Richardson, 2004). On the rare cases when emotional differences were found, they typically were brought about by positive emotions among the accepted participants rather than negative ones reported by the excluded participants (who typically rated themselves as feeling neutral or nothing). Moreover, and crucial for the theory, even when differences in mood and emotion were obtained, they failed to mediate between the rejection and the behavioral effects (e.g., Buckley, Winkel, and Leary, 2004; Williams et al., 2000). The large and consistent changes in behavior presented a sharp contrast with the small and inconsistent effects on emotion. Clearly, the theory that rejection causes emotion, which in turn causes behavior, needed a major overhaul.

Instead of acute upset or emotion, the standard response to exclusion by laboratory participants seemed to be one of numbness. After more than a dozen studies had yielded this pattern, some of us began to wonder whether numbness might indeed be an important but unanticipated effect of rejection. Around this time, a literature review by MacDonald and Leary (2005) concluded that in many social animals, being excluded from the group or family caused numbness to physical pain. Could there be a link between excluded animals' lack of pain sensitivity and human research participants' reports of emotional numbness after rejection?

A series of studies by DeWall and Baumeister (2006) found, first, that the laboratory manipulations of social exclusion made human participants lose their sensitivity to pain: Both pain thresholds and pain tolerance went up dramatically. Crucially, the insensitivity to pain was linked to the emotional insensitivity. DeWall and Baumeister went on to show that the reduced sensitivity to pain was correlated with breakdowns in other patterns of emotional response, such as affective forecasting (in this case, predicting how one would react to a win or loss in next month's big football game) and empathic reactions toward others who were suffering.

The link between physical and emotional numbness sheds useful light on the fundamentally social nature of the human psyche. Panksepp and his colleagues (Herman and Panksepp, 1978; Panksepp, Herman, Conner, Bishop, and Scott, 1978; Panksepp, Vilberg, Bean, Coy, and Kastin, 1978) proposed that when social animals began to evolve, they needed inner mechanisms to help them react to social events, and that instead of creating entirely new neural or hormonal pathways, evolution piggybacked the social emotions onto the pre-existing system for responding to physical harm. Hence, when people describe rejection experiences in

physical terms (e.g., "hurt" feelings; Leary and Springer, 2000), they are not just using an analogy or metaphor. Consistent with Panksepp's position, Eisenberger, Lieberman, and Williams (2003) adapted the cyberball ostracism procedure with brain scanning to show that the brain regions associated with physical pain also respond to social rejection.

The first reaction to social exclusion may therefore also be a kind of numbness. A physical injury causes the release of opioids that enable the animal to continue functioning without being overcome by pain (Panksepp et al., 1978). This was probably often vital for survival, enabling an injured animal to escape from a crisis situation or battle. Apparently, the social equivalent of injury, namely being rejected or excluded, produces a similar reaction.

What happens later? Most likely, the numbness wears off, and the person or animal will start to hurt. The delayed reaction does however afford some opportunity for a coping process to begin. Twenge, Koole, DeWall, Marquez, and Baumeister (2006) found an unexpected pattern that suggests how this coping process works. They started out still looking for emotional distress but failed repeatedly to find any signs of conscious emotion, and so they began to look for nonconscious responses. To their initial surprise, they found that excluded people exhibited patterns of enhanced *positive* emotionality. For example, exclusion caused participants to complete more ambiguous word stems with positive emotion words and to group words together based on positive emotional valence, than did accepted or neutral participants. Parallel measures for negative emotion words yielded nothing. These effects replicated with multiple procedures, thus lending confidence that they were no fluke or artifact. This so-called tuning toward emotional positivity may reflect a more general aspect of nonconscious coping. DeWall and Baumeister (in press) found similar patterns of implicit affective positivity in reactions to thinking about one's death.

Although it took us some time to come around to the right solution, we are perhaps not alone in having expected the truth to be different. The idea that rejection should cause an immediate wave of distress is plausible and indeed widely accepted. Twenge and colleagues (2006) asked some participants to predict how they would react to a laboratory manipulation of exclusion, whereas others actually underwent the same manipulation. Intuitive predictions were wrong. People predicted intuitively that they would have strong conscious distress, but participants who actually experienced the rejection had no conscious emotional reaction. Meanwhile, participants intuitively predicted no effect on the implicit response measures, but those who actually experienced rejection exhibited a strong change (toward positivity!) on the implicit measures.

Thus, we seem at last to be converging on a correct understanding of the emotional impact of rejection. Social exclusion causes an immediate reaction of numbness, including a loss of sensitivity to physical pain and a lack of emotion. The nonconscious processing systems of rejected people begin a search for happy thoughts, possibly to help mute the distress that will arise once the numbness wears off. They

do not realize that they have gone numb, and so when they use their emotional systems to deal with others—such as in empathy, which is based on internally simulating another person's experience and observing one's incipient emotional response—they feel nothing and, therefore, fail to have much sympathy or empathy. In a sense, they shrug off the other's problems, as if to say, "If that happened to me, I would not be bothered, so I don't need to feel sorry for that person."

COGNITION AND SELF-REGULATION

The lack of emotional response to social rejection prompted us to investigate other possible inner processes. Intelligent thought was one prominent candidate. Intelligence is an important feature of the human psyche, indeed so much so that the human species has named itself (*Homo sapiens*) in honor of its putative wisdom. (At least, humans seem to be the only species whose members can recognize how stupid one another can be, which itself seems an important sign of intelligence.) Intelligence is no doubt an important adaptation to facilitate the basic biological tasks of survival and reproduction. In a sense, people survive by their social life and by their wits. If the social strategy is blocked, which social exclusion entails, then the rejected person would presumably have to rely more on intelligence, and so one might hope that intellectual functioning would be improved among excluded persons.

Alas, the data did not support the view that sharper thinking compensates for the loss of social belonging. Instead, a series of studies found that social exclusion caused a sharp drop in intelligent thought (Baumeister, Twenge, and Nuss, 2002), as measured by various IQ and reasoning tests.

Consider one of the studies. Eighty-two undergraduate students participated one at a time. Upon arrival, they filled out the Eysenck Personality Questionnaire (Eysenck and Eysenck, 1975) and then were given feedback about their responses. The questionnaire measures introversion and extraversion, and everyone was given accurate feedback about those results, but then by random assignment everyone heard some additional (bogus) information. One third of them were told that people with their profile tended to end up alone in life. The experimenter said that they might have friends now, because they were in a people-rich environment, but once they moved out of the life stage where people are constantly making new friends, their current ones would drift away and not be replaced. They might marry once or twice, but relationships would not tend to last, and they would end up spending more and more time alone. This was the exclusion (future alone) condition.

Another one third of participants were told that they would always be surrounded by people who would love and appreciate them (future belonging). A final one third were told that their futures would likely contain a series of accidents and misfortunes. They would be prone to injuries and would end up spending a fair amount of time in hospitals and emergency rooms. This last (misfortune control) condition was added because it is bad news but unrelated to belongingness or rejection, so we

TABLE 38.2
Social rejection and intelligent performance

Logic problems	Correct answers	Attempts (right plus wrong)
Future alone	4.67	6.94
Misfortune control	6.81	10.31
Future belonging	6.63	8.51
Significance	$p < .01$	$p < .001$

Rote memory (nonsense syllables)	Correct answers	Attempts (right plus wrong)
Future alone	5.75	6.67
Misfortune control	5.67	6.92
Future belonging	5.18	6.09
Significance	*ns*	*ns*

High scores indicate better performance; *ns* = not significant. This information was adapted from Baumeister (2002).

could separate the effects of bad news in general from the effects of social exclusion in particular.

Then participants were randomly assigned to take one of two kinds of tests. The results of these tests are shown in Table 38.2. Half the participants took a test of logical reasoning. This was drawn from the analytical section of the Graduate Record Examination (GRE).[1] Participants in the future alone condition scored significantly worse than those in the other two conditions. There was some sign that the misfortune control group also bothered participants, because they made errors at about the same rate as those in the future alone condition, but they compensated by working harder and answering more questions. The rejected participants suffered a double whammy: They attempted fewer questions and were worse at getting them right.

The other half of the participants performed a rote memorization task. They were given a list of 15 nonsense syllables (e.g., FUM, WEV, PIH) to memorize. Then they were distracted for 90 seconds by working on arithmetic problems. (The exclusion manipulation was actually given to them at this point, after they had performed the memorizing part.) Then they were instructed to write down as many of the nonsense syllables as they could for 90 seconds. On this task, there were no differences between the conditions. The implication is that automatic information processing is unaffected by rejection, whereas logical reasoning is seriously impaired.

Another study in that series by Baumeister and colleagues (2003) examined the possibility that the social exclusion feedback was so upsetting that it distracted people, making them unable to concentrate on the problems. For this study, 65 students participated individually. They filled out the same personality questionnaire and re-

[1]This subtest is no longer included in that examination.

ceived one of the same three versions of bogus feedback, by random assignment. But the timing of this feedback was varied, depending on another random assignment. It was woven into the next task, which involved a reading comprehension test taken from the verbal test on the GRE. This test requires people to read a passage and then answer questions about it. Some of the questions are fairly straightforward in that they ask the test-taker to give back information that was given explicitly in the passage. Other questions require the person to think about, reason, and extrapolate from what he or she read. Half the participants took the straightforward part; the other half took the reasoning and extrapolation part of the test.

The interweaving of the exclusion feedback with the test-taking was done as follows. Half the participants received the exclusion feedback manipulation (i.e., future alone, misfortune control, or future belonging) first, then read the passage. Then they were debriefed: They were told that the feedback had been randomly assigned and was not true. Only then did they take the test. If the impact of the exclusion feedback was distracting and prevented people from processing information into memory, the feedback should have had a strong effect here, but it did not. This sequence of procedures yielded no differences in performance. Apparently, people were perfectly able to read and understand information and store it in memory, even if they had just been told that they were likely to end up alone in life. As long as the threat was removed (by debriefing) before the test, they did fine on the test.

The other half of the participants read the passage first and only then received the feedback about the future. They then answered the questions about the passage, and they were debriefed after this. In this group, the effects of the feedback were significant, although only on the questions that required reasoning and extrapolation, and not on the straightforward questions. Participants who were told they would end up alone in life performed significantly worse than those in the other two conditions on these reasoning questions.

Thus, it appears that rejection makes you stupid, at least temporarily and in some ways. The main exceptions appeared to be automatic information processing tasks, such as simple learning and memory tasks. These were unaffected. Rejected people were able to memorize nonsense syllables and recall them just as well as other people. They were able to read and comprehend new information and store it in memory. Exclusion mainly impaired logical reasoning, extrapolation, and other mental operations that required moving from one set of information to a different conclusion.

The fact that exclusion affected the more controlled processes and not the automatic ones raised the possibility that it affects the self's executive function. Self-regulation is a large and important part of executive function and a powerful, basic process that facilitates human social life (e.g., Baumeister, 1998, 2005). Many of the effects already described, such as increased aggressiveness, as well as impulsive selfishness and self-defeating shortsightedness (see Twenge, Catanese, and Baumeister, 2002), could be explained as a failure of self-control.

The idea that social exclusion impairs self-regulation was confirmed by Baumeister, DeWall, Ciarocco, and Twenge (2005). Exclusion made participants lose

self-control on a variety of measures. One study used a standard problem of self-control from everyday life, namely eating cookies. (A pretest sample drawn from the same student population confirmed that most students regarded eating cookies as an undesirable and fattening activity that they thought best to keep to a minimum.) The 38 participants initially met in small groups, put on nametags, and talked for about 20 minutes to get to know one another, answering a standard series of questions to elicit personal information. Then they were put into separate rooms and asked to name the two people from the group they would like to work with on the next task. This procedure was explained as enabling the researchers "to form groups in which the members like and respect each other." As usual, this was the basis for telling the participants that everyone else, or no one, had expressed a desire to work with them.

The experimenter told those in the rejected condition that as a result of this unforeseen complication, the participant would do the next task alone. It involved rating cookies for taste. Each participant was given a bowl of cookies and a rating sheet. Such a procedure is commonly used in eating research. If participants are told, "Here are some cookies, and we want to measure how much you eat," people become self-conscious and often eat very little. Using the guise of a taste test conceals the issue of how much people eat, and indeed they can chow down heartily with the justification that they are merely trying to do a good job on the questionnaire.

In fact, the number of cookies eaten was the dependent variable. Rejected participants ate about twice as many cookies as accepted participants ate (8.94 vs. 4.40). Rejected participants also rated the cookies more favorably—apparently, being rejected makes the cookies taste better. However, the improved taste and increased eating were statistically unrelated, and some participants later remarked that they found themselves gobbling down the cookies even though they did not like them.

Another study by Baumeister and coworkers (2005) used a dichotic listening procedure, which is a standard laboratory measure of attention control. In this experiment, all 30 participants were chosen partly on the basis of being right-handed, to avoid complications arising from dominant versus nondominant ears and brain hemispheres. They took part individually, first completing the Eysenck Personality Questionnaire and then receiving randomly assigned feedback that they would end up alone in life, would always be surrounded by people who loved them, or would become accident and injury prone later in life.

Then participants performed the dichotic listening task. They put on headphones and heard spoken material. In the right ear was a speech about a policy issue. In the left ear they heard a voice reciting a list of words. They were told that their job was to ignore the speech in the right ear and concentrate on the left ear so as to be able to write down every word that contained the letters *M* or *P.* The list actually consisted of the thousand most frequently used words in the English language, though we cut it off after the first 255 words. Of these words, 38 contained *M* and 10 contained *P.*

TABLE 38.3
Social exclusion and attention control

	Correctly identified words
Future alone	35.60
Misfortune control	40.00
Future belonging	42.70

High scores indicate better self-regulation. Maximum possible was 48. Future alone mean was different from each of the others at $p < .01$. Data adapted from Baumeister (2005)

When we first ran the experiment, we instructed participants to write only words that started with *M* or *P.* This was apparently too easy, and most participants got them all correct. Still, the fact that they did so well, even in the future alone condition, helps rule out any alternative explanation that rejected participants refused to comply with instructions or were too busy thinking about the rejection feedback to listen to the words. With the revised procedure that called for listing words with *M* or *P* anywhere in them, however, significant differences emerged, as Table 38.3 shows. Rejected (future alone) participants performed worse than those in the other two conditions. Apparently, being excluded makes you worse at self-regulating your attention.

Are rejected people unable or merely unwilling to self-regulate? Further studies by Baumeister and colleagues (2005) suggested the latter. We were able to eliminate the detrimental effects of rejection by making people self-aware (which stimulates self-regulation) and by offering people a cash incentive to perform well. Apparently, rejected people retain the capacity to control themselves, but they do not want to bother unless they see a direct benefit for themselves.

EXPLAINING THE INTERPERSONAL

Before concluding, we want to return from the intrapsychic to the interpersonal. In our view, inner processes serve interpersonal functions, and so the inner processes we described should help to shed light on the interpersonal behavior patterns we described.

The self-regulation findings suggest a basic truth about human social life (see also Baumeister, 2005). People have selfish wants and needs, but they also want to be accepted by others, and harmonious group life requires that people sometimes subdue their selfish impulses such as by waiting one's turn, respecting the property and rights of others, and following rules. The capacity for self-control probably developed in part to enable people to stifle their selfish impulses so as to win acceptance by behaving in socially desirable ways. Self-control is not fun and indeed requires effort and sacrifice. Human social life thus has a fundamental bargain, by

which people make these efforts and sacrifices to control and subdue their impulses, then reap the considerable benefits of belonging in return. This bargain, however, can be fragile and can break down on either side. People who fail to control themselves adequately are prone to be excluded by others, whether this takes the form of divorce, employment termination, or indeed imprisonment for crime (see Gottfredson and Hirschi, 1990, for a classic statement of the link between low self-control and criminality). Conversely, the data reviewed here show that rejection causes people to lose their willingness to make the efforts and sacrifices involved in self-control. It is as if they ask, "If people aren't going to include me, why should I bother trying to be a good person?"

The impairments of self-regulation may well contribute to the increased aggression and the reduced helping that stem from social exclusion. People have aggressive impulses and normally restrain them, and so when self-regulation is impaired, aggression increases (DeWall, Stillman, Baumeister, and Gailliot, in press; Stucke and Baumeister, 2006). In the same way, helping and other prosocial behaviors are far more extensive in humans than in other species and may require an overriding of natural selfishness, and evidence suggests that when self-regulation is impaired, helping is diminished (Gailliot et al., 2007).

The emotional numbness also contributes to the behavioral consequences of rejection. Empathy is an important contributor to helping and other prosocial behavior (Batson, 1987, 1991). As we have seen, empathy is reduced among rejected persons, apparently in connection with the loss of physical and emotional sensitivity. Empathy depends on imagining what another person is experiencing and thereby generating the same emotional reaction in oneself that the other is having. Presumably, when a recently rejected person tries to imagine what someone else is feeling, he or she conducts such an imaginary simulation—but finds that not much emotional response occurs. The rejected person concludes, wrongly, that the other person's problems are not very serious. Without the emotional reaction, there is much less impetus to help the other person.

The role of empathy was demonstrated by Twenge and colleagues (2007). In their final study, they showed that rejection by one person led to reduced empathy toward a second, and this reduction in empathy statistically predicted helping a third party. Using three different interaction targets provided methodologically strong evidence that the inner numbness and its attendant impairment of empathy mediated between the rejection experience and the reduction in prosocial behavior.

Thus, ultimately and somewhat ironically, we did find that emotion plays a role in causing the behavior effects of social exclusion—just not the role we had initially thought. We had begun with the theory that rejection would cause emotional distress and that distress would directly cause emotion. When that failed to happen, we began to reconsider the role of emotion in human behavior generally and realized it is far more indirect than we had assumed (see Baumeister, Vohs, DeWall, and Zhang, in press). Instead, it appears that people use their emotions to understand other people. When rejection causes the emotion system to cease functioning

properly, people lose one of their principal means of understanding others and therefore show less optimal interactions with them.

CONCLUSIONS

As social and cultural animals, humans have a strong and deeply rooted need to belong. They are motivated to form and maintain social relationships with other people. Research on social exclusion confirms the power of that drive. It has shown that being rejected or excluded causes strong behavioral reactions, including increased aggression, reduced prosocial behavior, and increased self-defeating behavior. Ironically, none of those seems like an adaptive response that holds much promise for re-establishing new social bonds. Yet rejected people do have heightened interest in forming new relationships. They are simply distrustful of others and often would rather avoid any chance of being rejected again rather than take the chance on making further overtures that could potentially make a connection. Rather than take the first step, they tend to wait until they see signs of promise, such as when a new interaction partner appears welcoming and friendly. Until that happens, they tend to treat others in a more wary and sometimes unfriendly manner.

Illuminating the inner responses to rejection has taken some time, in part because we had been captivated by the intuitively appealing theory that emotional distress would prove to be the main direct effect of rejection and would mediate behavior. Instead, it appears that the emotion system tends to shut down briefly in response to rejection, causing a kind of numbness that in turn hampers empathy and other efforts to relate to other people. Meanwhile, self-regulation and intelligent thought appear to suffer in rejected persons. One ray of hope takes the form of a nonconscious coping process that seeks out happy thoughts during the initial and numb phase.

Sociologists have observed that excluded classes of persons in many societies exhibit various undesirable patterns of behavior, including aggression, poor intellectual or academic performance, a lack of prosocial behavior, self-destructive indulgences, and poor self-control. Our research suggests that these are not necessarily the inner traits of society's downtrodden as much as normal reactions that many people exhibit when they find themselves excluded by others.

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39

Arbitrary Social Norms Influence Sex Differences in Romantic Selectivity

Eli J. Finkel and Paul W. Eastwick

Men tend to be less selective than women when evaluating and pursuing potential romantic partners. The present experiment employed speed-dating procedures to test a novel explanation for this sex difference: The mere act of physically approaching a potential romantic partner (vs. being approached), a behavior that is more characteristic of men than of women, increases one's attraction to that partner. This hypothesis was supported in a sample of speed daters ($N = 350$) who attended a heterosexual event where either men (eight events) or women (seven events) rotated from one partner to the next while members of the other sex remained seated. Rotators were significantly less selective than were sitters, which meant that the tendency for men to be less selective than women at events where men rotated disappeared at events where women rotated. These effects were mediated by increased self-confidence among rotators relative to sitters.

In an attempt to impress the gorgeous woman sitting at the bar, Maverick finds a microphone, approaches the woman, and serenades her with an off-key rendition of the Righteous Brothers' classic hit, ''You've Lost That Loving Feeling.'' His decision to pursue her with such boldness is unusual, requiring the sort of uncommon self-confidence that causes people to earn nicknames like ''Maverick.'' We suggest, however, that a toned-down version of Maverick's romantic initiation strategy is the norm rather than the exception in Western cultures. Strip away the microphone and the singing, and what remains? A man sees an attractive woman, and he approaches her to try to initiate a potential romantic relationship. What could be more mundane?

Reversing the male and female roles, however, renders this relationship-initiation process much less ordinary. To be sure, there are women in Western cultures who

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regularly approach men to initiate romantic relationships, but such women are the exception rather than the rule. Although women frequently play an important role in initiating the courtship process (e.g., with eye contact, smiles, or hair flips; see Moore, 1985), even egalitarian men and women expect men to play the assertive, approach-oriented role when initiating romantic relationships, and women to play the more passive role, waiting to be approached (e.g., C. L. Clark, Shaver, & Abrahams, 1999; Laner & Ventrone, 1998; Rose & Frieze, 1993). In the present article, we examine whether the mere act of physically approaching potential romantic partners (vs. being approached by them)—even in the absence of any internal motivation to do so and when removed from the context of a traditional dating script—causes individuals to evaluate these partners as more desirable, to experience greater romantic chemistry with them, and to enact behaviors that increase the likelihood of a romantic relationship developing.

ROMANTIC SELECTIVITY

Heterosexual women tend to be more selective than heterosexual men in the dating realm. Indeed, a best-selling introductory psychology textbook recently summarized the relevant literature as follows: ''People *select* their reproductive and sexual partners, and perhaps the most striking fact about this selection is that women are more selective than men'' (Schacter, Gilbert, & Wegner, 2009, p. 631, italics in original). In a recent, large-scale study of on-line dating behavior, for example, men were approximately 1.5 times more likely than women to send a first-contact e-mail after viewing a given opposite-sex person's on-line dating profile—an effect that is all the more striking when considering that men browsed approximately twice as many profiles in the first place (Hitsch, Hortaçsu, & Ariely, 2009). In a well-controlled study, men were more willing than women to go on a date with an attractive opposite-sex target depicted in a photograph (Townsend & Wasserman, 1998). In addition, across three samples, men were approximately 1.2 times more likely than women (58% vs. 48%) to accept a date from an opposite-sex research confederate who approached them on campus (R. D. Clark, 1990; R. D. Clark & Hatfield, 1989).

Several additional studies have employed speed-dating procedures to demonstrate this sex difference in romantic selectivity (Fisman, Iyengar, Kamenica, & Simonson, 2006; Kurzban & Weeden, 2005; Todd, Penke, Fasolo, & Lenton, 2007). Such procedures, which are characterized by both strong internal and strong external validity (see Finkel & Eastwick, 2008), are in many ways ideally suited to testing hypotheses related to romantic selectivity because participants indicate their romantic attraction toward numerous potential partners whom they have met in person. In speed-dating, participants attend an event where they

go on a series of brief ''dates'' (approximately 4 minutes each) with members of their preferred sex (for an overview of speed-dating procedures, see Finkel, Eastwick, & Matthews, 2007). After the event, they indicate whether they would (''yes'') or would not (''no'') be interested in seeing each partner again. Men and women who *match* (mutual ''yes'' responses) are provided with the means to contact each other to arrange a follow-up meeting. Speed-dating studies reliably demonstrate that men respond ''yes'' to a larger proportion of their partners than women do (Fisman et al., 2006; Kurzban & Weeden, 2005; Todd et al., 2007).

Many scholars explain the robust sex difference in romantic selectivity in terms of the different adaptive problems facing ancestral women and men (e.g., Buss & Schmitt, 1993; C. L. Clark et al., 1999; Symons, 1979; Todd et al., 2007; Townsend & Wasserman, 1998; Trivers, 1972). According to this evolutionary perspective, human females, like other mammalian females, generally invest more resources in a given offspring than males do. Women's minimum investment is 9 months of gestation, whereas men's is a single act of sexual intercourse. Largely because of this asymmetry, the reproductive costs of an ill-advised mating decision are considerably higher for women. Evolutionary scholars have suggested that the sex difference in romantic selectivity reflects a domain-specific adaptive mechanism that evolved to manage these sex-differentiated costs and benefits among *Homo sapiens'* ancestors. Indeed, the evidence for greater female selectivity is so obviously congruent with well-established evolutionary theorizing that one speed-dating research team played down its own evidence for this effect as ''unsurprising'' (Kurzban & Weeden, 2005, p. 240).[1]

The compelling evidence for greater female selectivity notwithstanding, definitive evidence for this sex difference awaits an empirical test that rules out the pervasive confound that, in romantic contexts, men are far more likely to approach women than women are to approach men (C. L. Clark et al., 1999; Laner & Ventrone, 1998; Rose & Frieze, 1993). Published speed-dating studies of romantic selectivity provide particularly striking examples of this confound: In all of them, men always rotated from partner to partner, whereas women always remained seated. Such procedures mimic the rotational scheme employed in virtually all events hosted by professional speed-dating companies, and correspond to normative initiation dynamics in most other dating environments (i.e., men approach and women are approached).

---

[1]Many evolutionary psychologists argue that the sex difference in selectivity will be large in short-term mating contexts, such as one-night stands, but small (or even nonexistent) in long-term mating contexts, such as selecting a marital partner (R. D. Clark, 1990; R. D. Clark & Hatfield, 1989; Buss & Schmitt, 1993; Kenrick, Groth, Trost, & Sadalla, 1993). Dating, including speed dating, likely represents a middle ground between emotionally meaningless, one-time sexual encounters and long-term, committed pair bonding, so perhaps sex differences in such contexts should be of intermediate magnitude.

MIGHT APPROACHING (VS. BEING APPROACHED) MAKE INDIVIDUALS LESS SELECTIVE?

Could a confound as trivial as which sex romantically approaches the other cause (or at least contribute to) the robust sex difference in romantic selectivity? There are reasons to think that it might. One reason, which is grounded in the extensive literature demonstrating the subtle mutual influence between the body and the mind (see Barsalou, Niedenthal, Barbey, & Ruppert, 2003), is that physical, embodied approach could lead to romantic approach. In other words, physical approach could lead to a tendency to find a given romantic target appealing, or, in other words, to be unselective. Although no previous research has examined the effects of embodied approach in romantic contexts, many studies suggest that it causes individuals to experience approach-related cognitions in nonromantic contexts, including positive evaluations of target stimuli. For example, seated participants who placed their palms on the bottom of a table and pressed up (a pose associated with approach) rated neutral Chinese ideographs as more appealing than did seated participants who placed their palms on the top of the table and pressed down (a pose associated with avoidance; Cacioppo, Priester, & Berntson, 1993). In addition, non-Black participants who had been trained to pull a joystick toward themselves (approach) when a picture of a Black person appeared subliminally on the computer screen and to push the joystick away from themselves (avoidance) when a picture of a White person appeared subsequently exhibited more positive implicit attitudes toward Blacks and behaved more warmly toward them than did non-Black participants who performed either the opposite joystick task or a side-to-side (neutral) joystick task (Kawakami, Phills, Steele, & Dovidio, 2007).

Speed dating provides an efficient means of examining the effects of approaching (vs. being approached by) potential romantic partners in part because it has an embodied-approach manipulation inherently embedded in its core structure. At all heterosexual speed-dating events, one sex (virtually always the men) rotates from partner to partner while the other sex sits to await the next partner's arrival. Just as pressing one's hand upward on the bottom of the table or pulling a joystick toward oneself causes individuals to experience approach-related cognitions, we suggest that walking toward speed-dating partners (i.e., approaching them) could result in more positive evaluations than staying seated while partners approach.

We further suggest that such approach behavior should make individuals feel more self-confident on their speed dates. The logic underlying this self-confidence prediction derives from the concept of *situated conceptualization*, which suggests that general categories become meaningful when paired with particular contexts (Barsalou et al., 2003). We suggest that, just as anger becomes meaningful when in context (e.g., anger at one's spouse, the world, or oneself), approach behavior takes on a particular meaning in romantic contexts. Specifically, approach behavior is meaningfully

related to self-confidence and to the male gender role, two constructs that are highly correlated (Spence & Helmreich, 1978). Building on evidence that ''embodied states can function as cues that trigger situated conceptualizations'' (Barsalou et al., 2003, p. 84), we suggest that embodied romantic approach (a traditionally masculine behavior) promotes feelings of self-confidence, and that this bolstered self-confidence may mediate the link between embodied approach and romantic approach.

A second reason why having men approach and women be approached in romantic settings could cause (or contribute to) the robust sex difference in romantic selectivity is that being repeatedly approached could make individuals feel particularly desirable, which could in turn cause them to become selective. According to the *scarcity principle*, individuals tend to place less value on objects or opportunities that are plentiful than on those that are rare (Cialdini, 2001). An individual who is approached repeatedly by opposite-sex individuals could make the attribution that these approaching individuals like and want to be near him or her. Participants in a speed-dating study might be sufficiently preoccupied with their own behavior that they fail to adjust this attribution to account fully for situational factors (i.e., speed-dating procedures *require* that the members of one sex approach the members of the other) that could have caused them to be repeatedly approached (see Osbourne & Gilbert, 1992). These approached individuals, gaining confidence as they sense that they are desired by many potential partners, might become more romantically selective. (Indeed, speed daters who are well liked tend to experience less romantic desire for their partners than do speed daters who are not well liked; Eastwick, Finkel, Mochon, & Ariely, 2007.)

HYPOTHESES

If either the embodied-approach or the scarcity explanation is correct, then speed daters who approach their partners (*rotators*) should be less selective regarding their partners than should speed daters who are seated (*sitters*), and should evaluate these partners as more desirable, experience greater chemistry with them, and say ''yes'' to a larger percentage of them (*rotation hypothesis*). Results supporting this hypothesis would have implications for the sex differences in selectivity discussed earlier. We predicted that, consistent with the general tendency for men to be more oriented toward romantic approach than women, male participants would show greater romantic approach (romantic desire, romantic chemistry, and ''yes'' responses) toward their speed-dating partners (i.e., they would be less selective) when they approached and women sat, but that this sex difference would diminish and perhaps even disappear when women approached and men sat (*sex-moderation hypothesis*). We did not predict that the robust finding that women are more selective than men would be significantly reversed when women approached and men

sat (we thought that subtly reversing embodied-approach dynamics at a speed-dating event would probably be insufficient to reverse a lifetime of men approaching and women being approached), but rather expected that this effect would be significantly and perhaps completely diminished.

Finally, we examined whether the level of self-confidence participants experienced on their speed dates statistically mediated these sex-moderation effects. Assessing mediation by self-confidence allowed us to test whether the potential effects of rotating versus sitting were due to embodied-approach or to scarcity dynamics. The embodied-approach idea—that approaching in romantic contexts activates masculine, agentic self-perceptions—predicts the following mediational model: rotating → increased confidence → strong romantic approach (low selectivity). In contrast, the scarcity idea—that being repeatedly approached in romantic contexts makes individuals feel like they have many options—predicts the following mediational model: sitting → increased confidence → weak romantic approach (high selectivity). If the sex-moderation effects are driven by embodied-approach processes rather than by scarcity processes, then the mediational analyses should support the former mediation model rather than the latter.

METHOD

We hosted 15 heterosexual speed-dating events for 350 undergraduates (174 women and 176 men; mean age = 19.57 years, $SD = 1.10$). Participants went on 4-minute speed dates with approximately 12 opposite-sex individuals. For each event, we randomly determined whether men (eight events) or women (seven events) rotated.

Immediately after each date, participants completed three-item measures of their *romantic desire* for that partner (e.g., ''I was sexually attracted to my interaction partner''; $\alpha = .88$) and their *romantic chemistry* with that partner (e.g., ''My interaction partner and I had a real connection''; $\alpha = .91$), using scales from 1 (*strongly disagree*) to 9 (*strongly agree*). They also reported the degree to which they experienced *self-confidence* on that date (1 = *not at all*, 9 = *extremely*). Shortly after returning home from the event, participants recorded on the study's Web site whether they would (''yes'') or would not (''no'') be interested in seeing each partner again.

RESULTS

Results from multilevel modeling analyses, which accounted for the nonindependence deriving from the fact that each participant rated approximately 12 targets,

supported the rotation hypothesis. Relative to sitters (coded −0.5), rotators (coded 0.5) experienced greater romantic desire for (*M*s = 4.83 vs. 5.13), $b = 0.29$, $t(3{,}739) = 2.72$, $p = .007$, $p_{rep} = .959$, and greater romantic chemistry with (*M*s = 4.67 vs. 4.94), $b = 0.27$, $t(3{,}739) = 2.72$, $p = .007$, $p_{rep} = .959$, their speed-dating partners, and they responded ''yes'' to a significantly larger percentage of them (43.07% vs. 47.86%), $b = 0.21$, $eb = 1.23$, $t(349) = 1.96$, $p = .051$, $p_{rep} = .876$. These effects were not significantly moderated by participant's sex, $|t|$s < 1.15, ps $> .252$.

As depicted in Figure 39.1, our results also supported the sex-moderation hypothesis (sex coded: −0.= 5 men, 0.5 = women; rotation sex coded: −0.5 = men ro-

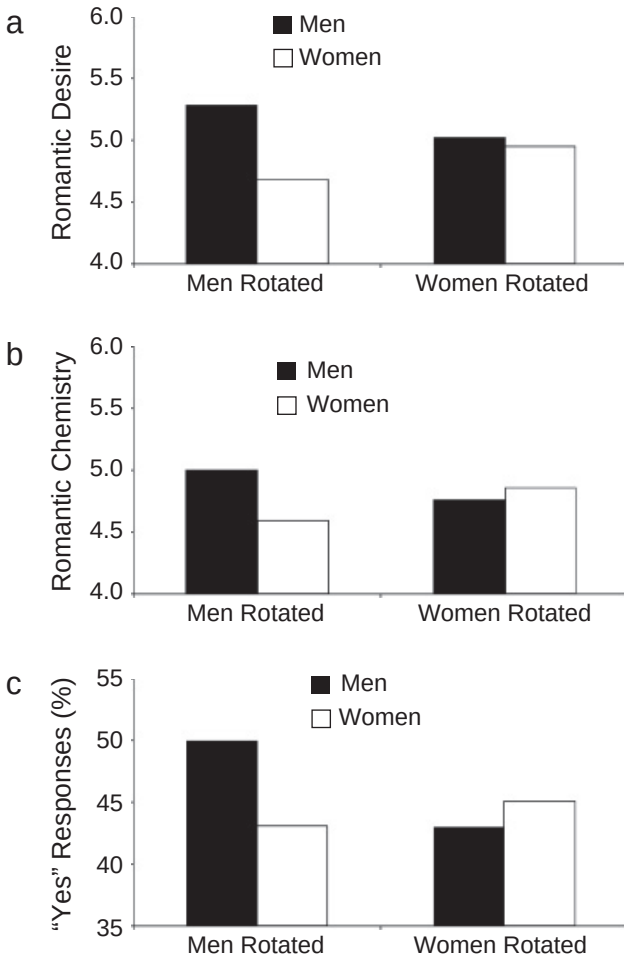

FIGURE 39.1.
Men's and women's (a) romantic desire for their speed-dating partners, (b) romantic chemistry with their partners, and (c) percentage of ''yes'' responses to their partners as a function of which sex rotated.

tate, 0.5 = women rotate). The Sex × Rotation Sex interaction was significant (or marginally significant) for all three dependent measures—romantic desire: $b = 0.51$, $t(3,739) = 2.43$, $p = .015$, $p_{rep} = .938$; romantic chemistry: $b = 0.50$, $t(3,739) = 2.55$, $p = .011$, $p_{rep} = .947$; and percentage of ''yes'' responses: $b = 0.40$, $t(349) = 1.87$, $p = .063$, × = .860. When men approached and women sat (as was the case in all previous studies), men experienced greater romantic desire for, $b = -0.59$, $t(2,091) = -3.97$, $p < .001$, $p_{rep} = .986$, and greater romantic chemistry with, $b = -0.41$, $t(2,091) = -3.09$, $p = .002$, $p_{rep} = .979$, their speed-dating partners than women did, and they responded ''yes'' to a larger percentage of these partners, $b = -0.29$, $eb = 0.75$, $t(190) = -1.91$, $p = .058$, $p_{rep} = .867$. In contrast, when women approached and men sat, none of these sex differences approached significance, $|t|$s < 0.74, ps $> .459$.

Finally, mediational analyses supported our embodied approach perspective rather than the scarcity perspective. The Sex × Rotation Sex interaction positively predicted self-confidence, $b = 0.51$, $t(3,697) = 2.39$, $p = .017$, $p_{rep} = .934$. In addition, three mediated moderation analyses (Baron & Kenny, 1986)— one each for romantic desire, romantic chemistry, and percentage of ''yes'' responses—revealed that the Sex × Rotation Sex interaction effects were significantly reduced (two to non-significance) after including self-confidence in the model, Sobel zs > 2.33, ps $< .020$, p_{rep}s $> .927$. This mediation was also relevant to the rotation hypothesis (which collapsed across participant sex): Rotators reported more self-confidence than sitters, $b = 0.23$, $t(3,697) = 2.16$, $p = .031$, $p_{rep} = .907$, and three mediational analyses (one for each dependent measure) revealed that the effect of the approach manipulation on the dependent variables was significantly reduced (one to non-significance after including self-confidence in the model, Sobel zs > 2.12, ps $< .034$, p_{rep}s $> .902$. Consistent with the embodied-approach idea, our results showed that approaching (vs. being approached) made women and men feel more confident, and this confidence seemed to promote their tendencies to experience romantic desire toward and romantic chemistry with their speed-dating partners—and to respond ''yes'' to their partners at a significantly higher rate.

DISCUSSION

Results supported the hypothesis that speed daters who approached partners (rotators), relative to those who stayed seated (sitters), would experience greater romantic desire toward and greater romantic chemistry with their speed-dating partners, and that they would respond ''yes'' to their partners at a higher rate. These results replicated the well-established finding (Fisman et al., 2006; Kurzban & Weeden, 2005; Todd et al., 2007) that women are more selective than men at speed-dating events—but this replication emerged only when men rotated.

When women rotated (a procedural feature absent from previous speed-dating studies), sex differences in romantic desire, romantic chemistry, and percentage of ''yes'' responses disappeared. The Sex × Rotation Sex interaction effects were significantly mediated through feelings of self-confidence on the speed date, with participants who rotated experiencing greater self-confidence than those who sat.

These mediational results were more consistent with the embodied-approach explanation than with the scarcity explanation. It seems that embodied approach in romantic settings (a traditionally masculine behavior) significantly boosted individuals' self-confidence, which in turn increased their tendencies toward romantic approach (i.e., made them less selective). That said, the evidence for the embodied-approach mechanism is preliminary. Indeed, an alternative explanation that, like the scarcity explanation, situates the mechanism within the sitters rather than within the rotators is also consistent with the present results. According to this alternative explanation, just as being presented with a large array of options can be demotivating and cause individuals not to make a choice (Iyengar & Lepper, 2000), perhaps being repeatedly approached by romantic partners can be overwhelming, causing individuals to experience less self-confidence and ultimately to eschew romantic approach (i.e., to be selective). Definitive conclusions about the mechanisms driving our effects await future research.[2]

Implications

The present findings have implications for the social norms surrounding the initiation of romantic relationships. Although Western civilization has become increasingly egalitarian over the past century, certain social institutions remain gendered, some in subtle, almost invisible, ways. The present research identified powerful consequences of a particularly subtle gender bias: the near-universal tendency to have men rotate and women sit at heterosexual speed-dating events. At first blush, this rotational scheme feels like an arbitrary, trivial solution to the logistical problem of ensuring that all of the women speed-date all of the men and vice versa. Executives from a popular speed-dating company confided in us that they have men rotate because (a) women often have more accessories with them at

[2]Other alternative explanations are also plausible. For example, perhaps approaching potential romantic partners (vs. being approached by them) influences how much anxiety one experiences; influences self-perceptions that one is action oriented, risk taking, or invested in making this dyadic interaction successful; and so forth. Another alternative explanation builds on the idea that individuals can misattribute their own physiological arousal to incorrect sources (Schachter & Singer, 1962). Perhaps standing up and walking several steps to the next partner caused rotators to experience increased physiological arousal (e.g., elevated heart rate), which they misattributed to romantic interest (see Foster, Witcher, Campbell, & Green, 1998). Although we cannot rule out this explanation, we find it implausible in part because our rotation manipulation would be the weakest arousal induction in the misattribution literature—far weaker, for example, than the low-arousal condition in Dutton and Aron's (1974) classic bridge study.

events (e.g., purses), (b) men never seem to mind rotating, and (c) it just seems more chivalrous that way. Speed-dating scholars have appropriately adopted many procedures from professional speed-dating companies, so it is not surprising that this gendered norm has largely persisted, even for events organized and hosted by scholars. The present results, however, present a cautionary note: Even subtle gender norms can have important consequences for romantic dynamics.

What implications do the present findings have for the extensive literature demonstrating that women are more selective than men when choosing mates (e.g., Fisman et al., 2006; Kurzban & Weeden, 2005; Symons, 1979; Todd et al., 2007; Trivers, 1972)? On the one hand, this sex difference did not significantly reverse at events where women rotated, so on average there was at least an overall trend in the present data for men to experience greater romantic approach (i.e., to be less selective) than women.[3] On the other hand, the gendered norm we manipulated in the present study is just one of a universe of possible norms that could in principle affect romantic attraction, and our participants almost certainly had a lifelong history of navigating such norms that no subtle laboratory manipulation could readily erase. Given that men are generally expected, if not required (as at professional speed-dating events), to approach in romantic contexts, perhaps this factor alone could be sufficient to explain why women tend to be more selective than men. The present results are at least partially consistent with this possibility.[4]

Conclusion

In summary, we experimentally manipulated a small component of the gender script across speed-dating events: who physically approaches whom. Having women approach and men sit caused women to behave more like men (less selective than usual) and men to behave more like women (more selective than usual), thereby eradicating the robust sex difference in romantic selectivity. It would be fascinating to examine whether changes over time in embodied-approach sex differences (who physically approaches whom) predict changes over time in romantic-selectivity sex differences. To the degree that romantic-approach sex differences disappear, perhaps romantic-selectivity sex differences will follow suit.

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[3]The main effect of participant's sex was significant for romantic desire, $p < .001$; marginally significant for romantic chemistry, $p = .059$; and nonsignificant for the percentage of ''yes'' responses, $p = .292$.

[4]Some scholars have argued that the tendencies in romantic settings for men to approach and for women to be approached reflect humans' evolutionary heritage, not socialization processes alone (e.g., Grammer, 1989). The present research was not designed to distinguish between cultural and evolutionary origin theories for human romantic approach tendencies. Rather, we sought to suggest that even a slight normative manipulation is sufficiently powerful to override the robust tendency for men to be less selective than women when evaluating potential romantic partners, regardless of the cultural or evolutionary origin of approach tendencies.

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Name Index

Abel, G. G., 359, 366, 367
Abelson, R. P., 106, 297
Abelson, W. D., 289
Abrahams, D., 149
Abrahams, M. F., 569
Abrams, D., 445, 446
Adams, H., 189
Adinolphi, A. A., 524
Adorno, T., 29
Ajzen, I., 56, 164, 165, 173
Alfert, E., 177
Ali, M., 138
Allen, M., 437
Allen, R. O., 45
Allen, V. L., 547
Allman, J. M., 507
Allport, G. W., 44, 45, 52, 164, 426, 427
Ammons, C. H., 455
Ammons, R. B., 455
Anderson, J. R., 72
Anderson, N., 161
Andison, E. S., 133
Ansell, E., 551
Araji, S., 358
Archer, D., 334, 337, 338
Arendt, H., 29
Ariely, D., 569, 572
Aristotle, 303
Arkoff, A., 101
Armentrout, J. A., 360
Aron, A., 498, 537, 539, 545, 547, 576
Aronson, E., 4, 6, 7, 90, 93, 179, 221, 222, 223, 224, 225, 227, 228, 229, 230, 231, 234, 250, 260, 263, 264, 276, 277, 278, 284, 285, 428, 513
Aronson, J., 206, 207, 289, 290, 296, 297, 394, 396, 398, 399, 400, 402, 403, 404, 405, 407, 408, 409, 411, 466, 491
Aronson, V., 149
Asch, S. E., 84, 155, 220, 237
Ashmore, R. D., 468
Aspinwall, L. G., 289
Austen, J., 176
Avis, N. E., 59
Axsom, D., 276

Bachen, C. M., 132
Backonja, M. M., 502
Bacon, F., 522

Bandura, A., 118, 129, 206, 237, 309, 317, 333, 357, 358, 523, 524
Banfield, J. T., 206
Banyas, C. A., 177
Barbaree, H. E., 358
Barclay, A. M., 536, 537, 539, 547
Bargh, J. E., 72, 74
Barker, C. H., 238
Barlow, D. H., 359
Barnes, G., 365
Barnes, R. D., 352
Barney, A. K., 571
Baron, R. A., 325, 342, 344, 353, 359
Baron, R. M., 464, 575
Baron-Cohen, S., 401
Barsalou, L. W., 571, 572
Bartels, A., 498
Bartels, J. M., 555
Bartlett, F. C., 164, 165
Barton, S., 364
Batson, C. D., 44, 45, 46, 52, 564
Baumeister, R. F., 320, 550, 551, 552, 555, 556, 557, 558, 559, 560, 561, 562, 563, 564
Baumgardner, M. H., 165
Bean, N. J., 557
Becker, H. S., 118
Becker, H. W., 533
Beery, R. G., 433
Beez, W. V., 126
Beezley, D. A., 360
Beikman, C., 87
Bem, D., 173, 547
Bentler, P. M., 365, 374
Berg, J. H., 492
Berger, S. M., 325
Berkowitz, L., 56, 57, 101, 133, 134, 138, 160, 317, 325, 363
Berkowitz, O., 57
Berman, J. J., 173
Bernheim, H., 17, 18
Bernstein, S., 322
Berntson, G. G., 571
Berscheid, E., 90, 149, 150, 154, 155, 159, 160, 161, 181, 497, 507, 524, 525, 527
Betancourt, H., 76
Bettencourt, B. A., 477, 483
Bevan, W., 151
Bickman, L., 41, 57, 58, 61, 276
Biernat, M., 475, 477, 482
Binet, A., 29, 203
Birnbaum, A., 68, 76
Bishop, P., 557
Bitter, E., 359
Blackwell, L., 207, 297
Blakey, D., 83, 84
Blanchard, E., 359
Blaney, N. T., 429, 430
Blascovich, J., 484
Blevins, T., 551
Block, R. A., 180
Bloom, B. S., 202
Blumberg, S. J., 177
Blumenthal, M. D., 330
Bodenhausen, G. V., 468
Bohrnstedt, G. W., 363
Bollen, K. A., 133, 134, 135, 136
Bonacci, A. M., 318, 551
Bonett, D. G., 374
Borgatta, E. F., 363
Borus, J. F., 338
Bower, G., 317
Bowers, K. S., 126
Bowlby, J., 497, 508
Bradley, B., 397
Bradley, M. M., 501
Bragan, B., 437, 441
Bramel, D. A., 264
Brandeis, L., 339
Branscombe, N. R., 469
Brehm, J., 89, 127, 276, 547
Brehm, S. S., 249, 261
Breitrose, H. S., 113
Breuer, J., 316
Brewer, M. B., 446, 468, 551, 555
Brian, E., 331
Brickman, P., 129, 468
Bridgeman, D. L., 431
Briere, J., 374
Brigham, J., 342
Brislin, R. W., 149
Bromley, S., 354
Broverman, I. K., 533
Brown, A. L., 203
Brown, D. R., 151
Brown, J. D., 177, 288, 289, 446, 468
Brown, L. B., 45
Brown, P., 181
Brown, R., 408, 409
Brownlee, K. A., 138
Brownmiller, S., 359
Bruner, J., 238
Brunstein, J., 196

Buck, P. L., 382
Buckley, K. E., 557
Bugelski, R., 447
Bugner, J., 139
Bulman, R. J., 468
Burkhart, B. R., 360
Burnstein, E., 165
Burt, M. R., 359, 360, 364, 365
Burton, R. V., 42
Bush, G. W., 396, 397, 399
Bushman, B. J., 317, 318, 320, 325, 326, 551
Buss, A. H., 32, 350, 351, 352
Buss, D. M., 570
Butisingh, S., 475
Butler, D. L., 476
Butterfield, E. C., 289
Byrne, D., 149, 151, 161, 547

Cacioppo, J. T., 498, 571
Cahill, T., 83, 85, 86, 88, 89, 92, 96
Calder, B. J., 164
Calley, W., 330
Cameron, J. E., 445
Campbell, A., 342
Campbell, D. T., 150
Campbell, W. K., 557, 576
Campione, J. C., 203
Camus, A., 4
Caplan, N., 343
Cardoze, S., 59
Carlsmith, J. M., 4, 6, 93, 221, 223, 224, 225, 230, 231, 238, 263, 264, 277, 513
Cartwright, S., 29
Carver, C. S., 477
Catanese, K. R.., 555, 561
Cath, S., 95
Cavior, N., 150
Ceci, S., 298, 396
Centerbar, D. B., 177
Chandler, M. J., 431
Chapman, B., 440, 441
Chapman, J., 523
Chapman, L., 523
Charcot, J. M., 17, 29
Charlton, K., 477
Chase, T., 225
Chasteen, A. L., 476
Check, J. V. P., 361, 363, 364, 365, 367, 373, 374
Cheung, C. K. T., 555
Chin, J., 556
Chiu, C., 204
Choi, W., 555
Cialdini, R. B., 57, 572
Ciarocco, N. J., 555, 561
Cicero, 86
Claridge, G., 365
Clark, C. L., 569, 570
Clark, R. A., 537, 539, 548
Clark, R. D., 569, 570
Clarke, L. A., 321
Clausen, G. T., 41
Clore, G. L., 151
Coffman, T. L., 522
Cohen, A., 222, 277
Cohen, D., 445
Cohen, E., 427, 429
Cohen, G., 288, 289, 290, 403
Cohen, J., 50, 319, 325, 348, 365, 368
Cohen, P., 365, 368
Coleridge, S. T., 202
Collins, A. M., 72
Collins, R. L., 446
Comstock, G., 132, 133, 134
Conklin, J., 134
Conner, R., 557
Conrad, J., 81
Cook, D., 276
Cook, S. W., 433
Cook, T. D., 113
Cooley, C. H., 445
Cooper, J., 229, 230, 231, 250, 251, 254, 261, 276, 285, 383, 389, 390
Coopman, J.-P., 139
Cornell, D. P., 446, 448, 466
Costa, P. T., 365
Costanzo, M., 276
Cottingham, D. R., 101
Cottrell, N. B., 325
Couch, C., 330
Covington, M. V., 289, 433
Cox, R. W., 501
Coy, D. H., 557
Coyne, J. C., 497, 499, 507
Crain, A. L., 228, 278
Crawford, Y., 83
Creswell, J. D., 289, 290
Criddle, W. D., 325
Crocker, J., 289, 446, 468, 469, 483, 491
Crofton, C., 150
Cronen, S., 476
Crosby, F., 354

Crosby, J., 475
Cross, J., 150
Cross, J. A., 457
Cross, J. F., 150
Crowne, D. P., 513
Crutchfield, R. A., 155
Cummings, W., 276

Dabbs, J. M., Jr., 105, 109
Dalton, K. M., 498
Damasio, A. R., 507
Damiani, M., 289
Daniels, L. R., 57
Darley, J., 41, 56, 76, 90, 181, 383, 448
Darrow, C., 338
Darwin, C., 202
Davidson, R. J., 498, 502, 507
Davis, E., 342
Davis, K., 93
Davis, K. E., 522
Day, E., 408, 409
de Haan, D., 133
de Waal, F. B. M., 498
Deaux, K., 457
deCharms, R., 129
DeFour, D., 352
Del Boca, F. K., 468
Derlega, V., 492
Dermer, M., 524
Detillion, C. E., 497
Deutsch, M., 57, 155, 161, 238, 428
Devine, P. G., 468, 470, 476, 477
DeVries, C. A., 497
DeVries, D. L., 433
DeWall, C. N., 551, 552, 555, 556, 557, 558, 561, 564
Diamond, L. M., 497
Dickerson, C., 228
Diener, E., 177, 342, 344, 352
Dill, K. E., 477
DiNubile, M., 150
Dion, K., 149, 151, 159, 160, 525, 527
Diorio, J., 498
Ditrichs, R., 342
Ditto, P. H., 446
Dolan-Pascoe, B., 555
Dollard, J., 353, 447
Donnerstein, E., 342, 352, 354, 363
Donnerstein, M., 342, 352, 354
Doob, L. W., 447
Dornbusch, S., 483, 490
Douglas, K., 306, 307
Dovalina, L., 59
Dovidio, J., 448, 476, 485, 571
Driver, L. E., 177
Dunn, R., 139
Durbin, J., 135
Durocher, L., 437, 438
Dutton, D. G., 342, 343, 477, 485, 576
Dweck, C. S., 203, 204, 205, 206, 207, 208, 210, 211, 212, 213, 297
Dymond, R., 122

Earls, C. M., 358, 366
Eastwick, P. W., 569, 570, 572
Ebbeson, E. B., 113
Ebel, R. L., 528
Ebert, J. E. J., 186
Eccles, J. S., 445
Edison, T., 202
Edwards, K. J., 433
Efran, M. G., 161
Eisenberger, N. I., 558
Eisler, R. M., 539
Elias, B., 364
Elliot, A. J., 468, 477
Ellis, H., 536
Ellsworth, P., 223
Enzle, M. E., 72
Epstein, S., 539
Erber, R., 179
Ervin, C. R., 149, 151
Erwin, J. M., 507
Esses, V. M., 468
Evans, K., 551
Evans, R. R., 363
Everett, P. B., 58
Eysenck, H. J., 365, 559
Eysenck, M. W., 325
Eysenck, S. B. G., 559

Fahrenholz, F., 508
Fallows, J., 397
Faraone, S. V., 373
Farber, J., 354
Fasolo, B., 569
Fazio, R., 164, 173, 174, 229, 230, 231, 285, 523
Feather, N. T., 173
Fehr, E., 498
Fein, S., 457
Feldman, N. S., 59
Feller, B., 441, 444
Fenigstein, A., 350, 351, 352

Ferguson, T. J., 59
Ferracuti, F., 335
Feshbach, S., 101, 102, 304, 359
Festinger, L., 112, 219, 220, 223, 224, 225, 226, 230, 231, 238, 250, 276, 277, 344, 445, 547
Field, P., 531
Fine, M., 342
Finkel, E. J., 181, 569, 570, 572
Finkelhor, D., 358
Finnie, W. C., 58
Fischbacher, U., 498
Fischer, M., 402, 403, 407
Fishbach, A., 186
Fishbein, M., 56, 164, 165, 173
Fiske, S., 58, 297, 448, 475, 476
Fisman, R., 569, 570, 575, 577
Flax, R., 352
Fleishman, J. A., 57
Fleiss, J. L., 323
Fleming, J. H., 448
Floyd, J., 7
Flynn, J., 406
Foreman, G., 139
Forgas, J. P., 468
Foster, C. A., 576
Frank, J. D., 29, 249, 250
Franklin, B., 190, 192, 193, 194
Franks, D. D., 432
Fraser, B., 181
Fraser, S., 92
Frazier, Joe, 138, 139
Frederick, S., 177
Freedman, J., 58, 92, 93, 223, 252, 276
Frenkel-Brunswik, E., 29
Freud, A., 177
Freud, S., 17, 44, 261, 316, 537
Fried, C., 207, 227, 228, 229, 230, 234, 278, 289, 290
Friedrich, C. J., 29
Frieze, L. H., 569, 570
Frommer, H., 440
Fulbright, R. K., 507
Funk, R. W., 43

Gaertner, S., 448, 476, 485
Gailliot, M. T., 564
Gallagher, F. M., 446, 468
Gallagher, N., 89
Gamson, W. A., 330
Ganzeboom, H. B. G., 133
Garcia, J., 288, 289
Gardner, W. L., 551, 555, 557
Garfinkel, H., 57, 76
Garry, C., 89
Gartner, R., 334, 337, 338
Gatz, M., 547
Geen, R. G., 317, 325, 326, 359
Geffner, R. A., 429, 430, 431
Geller, E. S., 58, 60
Gengaro, F., 475
Gerard, H., 4, 57, 155, 161, 179, 238, 426, 427, 523
Gerbner, G., 160
Gerrard, M., 446, 468
Geva, N., 165
Giancola, P. R., 322
Gibbons, F. X., 446, 468
Gibbs, B. J., 177
Gidycz, C. A.., 366
Giesler, R. B., 446
Gifford, R. K., 523
Gilbert, D. T., 177, 182, 186, 569, 572
Gilson, C., 106
Gimpl, G., 508
Ginnosar, Z., 59
Gladwell, M., 289
Glasper, E. R., 497
Glass, D., 353, 354, 477
Glass, D., 232
Glock, C. Y., 45
Glucksberg, S., 383
Goethals, G., 547
Goethe, J. W. von, 194
Goffman, E., 382, 392, 475, 533
Goldman, R., 252
Goldstein, A. P., 249
Goller, W. L., 382
Gonzales, M., 223, 276
Good, C., 207, 289, 290, 297, 402, 403
Gordon, A., 252
Gore, J. C., 507
Gorman, J., 475
Gottfredson, M. R., 564
Grammer, K., 577
Grant-Pillow, H., 204, 206, 212
Greathouse, S. A., 477
Greeley, A. M., 342
Green, J. D., 576
Greenberg, H., 438
Greenberg, J., 290, 446
Greenwald, A. G., 112, 165, 172, 173, 174, 446
Griffin, B. Q., 342, 343

Grist, T. M., 498
Gross, A. E., 150
Gross, J. J., 177, 179
Gross, L., 160
Groth, A. N., 359
Groth, G. E., 570
Gruder, C. L., 57
Grush, J. E., 57
Guild, D., 359
Gutierres, S. E., 156, 159

Haber, R. N., 536, 539, 547
Haddock, G., 468
Haefner, D. P., 101
Hakeem, A. A., 507
Hallam, J. R., 446
Hamilton, D. L., 468, 523
Hammock, G., 322
Hannon, P. A., 181
Harber, K. D., 475, 476, 484, 487, 490, 491
Harder, J. A., 402
Harold, R. D., 445
Hartmann, D. P., 359
Hartshorne, H., 42
Harvey, J. H., 522
Harvey, M. D., 72
Harvey, O., 150, 415
Haslam, S. A., 446
Hastorf, A. H., 382, 475, 477
Hatfield, E., 569, 570
Hauer, A. L., 360
Hayes, J. R., 202
Hays, W. L., 452
Heatherton, T. E., 455, 461
Heberlein, T. A., 58, 61
Heider, F., 118, 165, 522, 547
Heingartner, A., 325
Heinrichs, M., 498
Heintz, R. K., 150
Heller, K., 249
Helmreich, R., 222, 225, 230, 277, 572
Helson, H., 150, 151, 159, 160
Henderson, V., 204, 205, 206
Henderson-King, E. I., 476
Herman, B. H., 557
Herman, E. M., 325
Hewstone, M., 468
Higbe, K., 437, 443
Higgins, E. T., 59, 72, 74, 446
Hilton, J. L., 448, 468
Hirschi, T., 564
Hitsch, G. J., 569
Hof, P., 507
Hofer, M. A., 497, 507
Hogg, M. A., 446
Holgate, S., 551
Homans, G. C., 512
Hong, Y., 204, 206
Hood, W. R., 415
Hornberger, R. H., 316
Hortaçsu, A., 569
House, J. S., 497, 507, 508
Hovland, C., 113, 150, 220, 237
Howe, M. J., 202
Hubbard, M., 261
Huggins, M. D., 339
Hunter, S., 485
Huntington, R., 415
Hurlock, E. B., 127

Ickes, W., 352, 522
Ingerman, C., 446
Insel, T. R., 498
Insko, C. A., 101, 149, 468
Insko, V. M., 101
Inzlicht, M., 297, 403, 491
Irvin, M., 441
Irwin, W., 502, 507
Islam, M. R., 468
Iyengar, S. S., 569, 576
Izzo, A., 508

Jacobs, J. E., 445
Jacobson, L., 119, 126, 213, 445
James, F., 338
James, J., 338
James, W., 6, 202, 536
Janis, I. L., 101, 102, 109, 113, 191, 531
Javers, R., 86, 93
Jecker, J., 113
Johns, M., 290
Johnson, D. W., 433
Johnson, D., 338
Johnson, G. A., 158
Johnson, R. T., 433
Johnston, J., 134
Johnstone, T., 502
Jones, E., 93, 165, 383, 475, 477, 482, 492, 522, 533
Jones, J., 80–96
Jones, R. E., 383, 389, 390
Jones, S., 101
Jordan, M., 202

Jourden, F. J., 206
Judd, C. M., 374
Jung-Soon, M., 498

Kahle, L. R., 173
Kahn, R. L., 330
Kahneman, D., 177, 523
Kalin, N. H., 498
Kamenica, E., 569
Kamins, M., 208
Kanin, E. J., 360
Kant, I., 191
Kaplan, R., 361
Karlins, M., 522
Karlovac, M., 59
Kasindorf, J., 88
Kastin, A. J., 557
Katcher, A. H., 326
Katz, I., 118, 353, 354, 477
Katz, P., 353
Kawakami, K., 571
Kazdin, A. E., 128
Keefe, F. J., 366
Kekulé, F. A., 202
Kelley, H. H., 113, 512, 522, 524
Kelman, H. C., 113, 237, 330
Kemeny, M. E., 507
Kennedy, J. F., 397
Kennedy, K., 475, 476
Kennedy, W. A., 127
Kenny, D. A., 374, 464, 575
Kenrick, D. T., 151, 156, 158, 159, 570
Keppel, G., 452
Kerry, J., 397
Kidd, R. F., 522
Kiecolt-Glaser, J. K., 497, 508
Kilduff, M., 83, 85, 86, 87, 93
King, B. T., 113
Kippax, S., 132
Kitayama, S., 446
Kite, M., 457
Kleck, R. E., 382, 383, 387, 389, 392, 525
Klein, W. M., 446, 469
Knowles, M., 555
Kohler, R., 165
Kolarsky, A., 366
Konecni, V. J., 318
Koole, S. L., 552, 558
Korte, C., 41
Korth, B., 57
Kosfeld, M., 498
Koss, M. P., 360, 361, 363, 366, 374
Kovel, J., 353
Kowalski, R. M., 551, 555
Kozaki, T., 150
Kraft, D., 191, 195
Kramer, R. M., 468
Krams, M., 507
Krantz, D. L., 150
Krause, C., 81, 94
Krauss, R. M., 58
Kraut, R. E., 128
Krebs, D., 56, 76, 524
Kumashiro, M., 181
Kunda, Z., 446, 469
Kurzban, R., 569, 570, 575, 577
Kyung-Sook, C., 498

Lake, R. A., 477, 485
Lalonde, R. N., 445
Lambert, A. J., 476
Lamberth, J., 149, 151
Landis, K. R., 497
Laner, M. R., 569, 570
Lang, P. J., 317, 501
Langevin, R., 361
Larsen, R. J., 177
Latané, B., 41, 56, 76
Lawrence, L. H., 330
Laws, D. R., 366
Layden, M. A., 352
Layton, B. D., 149
Lazarus, R. S., 177, 179
Leary, M. R., 550, 551, 555, 556, 557, 558
Lee, J., 318
Leger, G. H., 359
Leggett, E. L., 203
Leiman, A. H., 539
Leippe, M. R., 165
Lennon, C., 210
Lenton, A. P., 569
Leonard, K. E., 360, 374
Lepper, M. R., 261, 490, 576
Leventhal, H., 101, 102, 104, 109, 110
Levin, H., 273
Levine, J. M., 165
Levinger, G. A., 161
Levinson, D. J., 29
Levinson, S. C., 181
Levis, D., 251
Lewin, K., 290
Lewis, L. D., 383
Lewis, S. A., 149
Leyens, J.-P., 477

Lickel, B., 484
Liddel, B. J., 507
Liébault, A. A., 17
Lieberman, M. D., 177, 558
Lifton, R. J., 83, 87, 89, 92, 95
Lin, D., 204
Linder, D., 8, 251, 254
Lingle, J. H., 165
Linville, P. W., 289, 297, 298, 482
Linz, D., 363, 365, 374
Lisansky, J., 76
Littleford, L. N., 485, 492
Liu, T. J., 446, 448, 453, 466
Lobel, M., 446, 468
Loewenstein, G. F., 177
Loftus, E., 72, 523
London, R. S., 382
Lopez, D. F., 446
Lott, A., 151
Lott, B., 151
Lövaas, O. I., 309
Lubin, B., 321, 539
Lucas, G. M., 551
Lucido, D., 354
Lucker, G. W., 430
Lucker, W., 4
Lugaila, T. A., 498
Lunde, D., 93
Lusk, R., 352
Lyche, L. E., 555
Lyle, R., 139
Lynch, M., 446
Lyubomirsky, S., 318

Macaulay, J., 133
Maccoby, E. E., 273
Maccoby, N., 113
MacDonald, G., 490, 557
Mackie, D. M., 468
Mackworth, J. F., 254
Macrae, C. N., 468
Madlafousek, J., 366
Major, B., 289, 483, 491
Malamuth, N. M., 358, 361, 363, 364, 365, 367, 373, 374
Malpass, R. S., 173
Mandell, W., 113
Maner, J. K., 556
Manis, M., 59, 150, 475, 477, 482
Marin, G., 76
Marini, M. M., 56, 68, 76
Markovsky, B., 325
Markus, H., 446
Markus, H. R., 446
Marlowe, D., 513
Marolla, J., 359, 360, 362, 432
Marquez, A., 552, 558
Marshall, D. S., 150
Marshall, W. L., 358, 366
Martens, A., 290
Marx, G. T., 329
Maslow, A., 11
Massey, D., 402, 403, 407
Massey, G., 483, 490
Mathewson, G. C., 179
Matthews, J., 570
May, M. A., 42
McCall, K. D., 289, 297
McClelland, J. L., 72
McConnell, H. K., 173
McCoy, S. B., 446
McCrae, R. R., 365
McCrimmon, J., 547
McEvoy, J., 330
McGarty, C., 446
McGlone, M., 400
McGraw, K. M., 446
McGuire, W. J., 127, 166, 167
McKirnan, D. J., 56
McKoon, G., 72
McPhail, L., 441
Mead, G. H., 445
Meaney, M. J., 498
Mehan, H., 57
Mehrabian, A., 382, 386, 387, 391, 453
Meichenbaum, D. H., 126
Meiners, M. L., 150
Melges, F. T., 177
Mendes, W. B., 484, 492
Merton, R. K., 523, 532, 533
Merton, R., 381
Miene, P., 468, 469
Mikulincer, M., 497, 508
Milavsky, B., 326
Milgram, S., 28, 39, 57, 84
Mill, J. S., 202
Miller, D. T., 56, 76, 486
Miller, L. E., 57
Miller, N., 4, 318, 353, 426, 427
Miller, N. E., 447
Miller, R. L., 134
Mills, Al, 85, 93–94, 95, 96
Mills, J., 90, 179, 221, 225, 250, 260, 348

Mills, J., 83, 84, 85, 87, 88, 90, 91, 92, 93, 94, 95, 96
Mills, L., 93–94
Mischel, W., 524, 534
Mochon, D., 572
Molden, D. C., 551
Monin, B., 475, 486
Monteith, M. J., 448, 468, 477
Moore, M. M., 569
Mowrer, O. H., 447
Mozart, W. A., 202
Mueller, C. M., 204, 208
Mulholland, A., 477
Murphy, G., 165
Murray, C., 404, 407, 409, 411
Murray, H. A., 539
Murray, J., 132
Myers, J. L., 169
Myrdal, G., 533

Nail, P. R., 490
Negel, L., 551
Nel, E., 222, 230, 277
Nelson, P. A., 364
Nemiro, J., 202
Nerlove, M., 135
Neuberg, S. L., 448
Newcomb, T., 344
Nezlek, J. B., 551
Nickerson, R. S., 202
Niedenthal, P. M., 571
Niles, P. A., 101, 102
Nimchinsky, E., 507
Nisbett, R., 383, 446, 476, 533, 547
Nolen-Hoeksma, S., 197, 318
Northcraft, G., 475, 477
Norton, K., 139
Nuessle, W., 383, 389
Nuss, C., 559

O'Leary, M., 276
Oakes, P. J., 446
Ochsner, K. N., 177
Olson, J. M., 165
Olson, R. E., 252
Ono, H., 382
Orne, M., 44, 363
Oros, C. J., 360, 361, 366
Osbourne, R. E., 572
Ostrom, C. W., 134
Ostrom, T. M., 150, 165, 173
Ouellette, R., 475
Overall, J. E., 348
Ovid, 185

Pack, S. J., 533
Paicheler, G., 76
Paitich, D., 361
Pallak, M., 278
Palmer, J., 523
Panksepp, J., 557, 558
Papierno, P. B., 298
Parducci, A., 161
Passingham, R. E., 507
Pedersen, W. C., 318
Pelham, B. W., 225
Penke, L., 569
Pennebaker, J. W., 179, 198, 199, 200
Pepitone, A., 56, 76, 150, 344
Pereg, D., 497
Perkins, D. N., 202, 203
Peterson, L., 76
Pettigrew, T., 342, 427
Pfeiffer, J. R., 382
Phillips, C. M., 320
Phillips, D. P., 132, 133, 134, 135, 136
Phillips, S., 555
Phills, C. E., 571
Pickett, C. L., 551, 555
Pinel, E. C., 177
Piucciotto, S., 475, 477
Ploghaus, A., 502, 507
Polivy, J., 455, 461
Pomerantz, E., 211
Popenoe, D., 76
Prather, Hugh, 82
Prentice-Dunn, S., 342, 345, 346, 351, 352
Priester, J. R., 571
Priestly, J., 190
Pritchard, J. R., 151
Pritzker, H. A., 237
Prokes, M., 96
Proust, M., 192
Pugh, E., 87

Quantrill, W., 338
Quanty, M. B., 317, 326
Quinsey, V. L., 359, 374

Rada, C. M., 360
Ralston, J., 234
Rao, P., 134
Rapaport, K., 360
Ratcliff, R., 72

Raush, H. L., 524
Read, S. J., 225
Reagan, R., 331
Reese, H. (P. W.), 438–439
Regan, D. T., 523
Reiter, S. M., 58
Reiterman, T., 85
Resnick, L. B., 203
Rhodes, N., 497
Richards, A., 397
Richards, J. M., 179
Richardson, D., 322
Richardson, R., 557
Richardson, S. A., 525
Richeson, J. A., 492
Rickey, B., 436–444
Roberts, D. F., 132
Robinson, J. R., 436–444
Robles, T. F., 497, 508
Rocher, S., 477
Roethke, T., 191
Rogers, C., 122, 264
Rogers, R. W., 342, 343, 345, 346, 351, 352, 353, 354
Rohan, M. J., 234
Rokeach, M., 29, 113
Romer, D., 57
Ronald, L., 525
Roper, S., 427
Rose, E. D., 113
Rose, S., 569, 570
Rosen, R. C., 366
Rosenberg, J., 343
Rosenberg, M. J., 44
Rosenfeld, H. M., 383, 391
Rosenhan, D. L., 523
Rosenthal, R., 119, 126, 183, 213, 381, 445, 452, 488, 532
Rosnow, R. L., 183, 452, 488
Ross, D., 237
Ross, H. L., 76
Ross, J. M., 44, 45, 52, 342
Ross, L., 261, 446, 522, 534
Ross, M., 164, 446
Ross, R. R., 126
Ross, S. A., 237
Rothbart, M., 523
Rottmann, L., 149
Rowser, J. F., 491
Ruble, D. N., 59
Rude, G., 341
Ruhnke, R., 225
Rule, B. G., 359
Rumelhart, D. E., 72
Runco, M. A., 202
Ruppert, J. A., 571
Rusbult, C. E., 181
Rushworth, M. F. S., 507
Russell, D. E. H., 358, 362
Russell, J., 453
Russon, A. E., 361
Rusting, C. L., 318
Rutkowski, G. K., 57
Ryan, L., 81, 94
Ryan, W., 393, 533

Sackett, P., 407, 408, 409
Sadalla, E. K., 570
Sagarin, E., 68, 76
Sakumura, J. S., 165, 172, 173
Salancik, J. R., 173
Sales, S. M., 325
Salinger, J. D., 7
Salomons, T. V., 502, 507
Salter, A., 251
Samuel, W., 58
Sanday, P., 359
Sandvik, E., 177
Sanford, R. N., 29
Sarason, I. G., 289
Sarbin, T. R., 547
Sargent, S. S., 353
Sattler, J., 392
Saxe, L., 354
Sayoc-Parial, M., 485, 492
Schachter, S., 252, 537, 544, 545, 547, 576
Schacter, D. L., 177, 569
Schadron, G., 477
Schaller, M., 556
Scheffé, H. A., 371
Scheier, M. F., 350, 352
Schein, E. H., 238
Schimel, J., 290
Schkade, D., 177
Schlenker, B. R., 446
Schmidt, G. W., 446
Schmitt, D. P., 570
Schneier, I., 238
Schultheiss, O., 196
Schulz, B., 363
Schur, E. M., 128
Schwartz, S. H., 41, 57, 77
Schwarz, N., 487
Scott, J. P., 557

Scott, R. A., 533
Scott, V., 483, 490
Scully, D., 359, 360, 362
Seares, R. R., 447
Sears, R. R., 273
Seaver, W. B., 126
Sechrest, L. B., 249
Seckl, J. R., 498
Seitz, V., 289
Sensibar, M. R., 539
Shaffer, L. S., 57
Shakespeare, W., 521
Shalom, 315
Shaughnessy, M., 482
Shaver, P. R., 497, 508, 569
Shavers, E., 139
Sheatsley, P. B., 342
Shelton, N., 289
Sheposh, J. P., 150
Sherif, C. W., 68, 415
Sherif, M., 56, 68, 150, 155, 415, 428
Sherman, D. K., 288, 289, 290
Short, J., 331, 332
Shrout, P. E., 323
Sicoly, F., 446
Simon, S., 342
Simonson, I., 569
Simonton, D., 202
Simpson, D. D., 150
Singer, J., 576
Singer, J. D., 329, 334, 335, 337
Singer, J. E., 537, 547
Singer, R. P., 101, 102, 109
Skudlarski, P., 507
Slaughter, E., 439
Slavin, R. E., 433
Sloane, R. B., 250
Sly, N., 88
Small, M., 329, 334, 335, 337
Smith, E. R., 446
Smith, L., 555
Snell, J., 177
Snow, C. P., 28
Snyder, M., 159, 160, 165, 168, 171, 172, 445, 468, 469
Solomon, R. L., 177
Sommer, K. L., 552
Sommers, N., 483
Sorich, L., 204, 205, 206
Spanier, G. B., 499
Spence, J. T., 572
Spencer, S. J., 289, 446, 448, 457
Spiegel, D. K., 348
Spilka, B., 45
Springer, C., 551
Springer, C. A., 556, 558
St. John, N., 426
Staats, A. W., 113, 118
Staats, C. K., 113
Stack, A. D., 317
Stafford, R., 475, 476
Stahelski, A. J., 524
Stampfl, T., 251
Stanky, E., 437, 440
Stark, R., 45
Staub, E., 56
Steele, C. A., 483, 490, 491
Steele, C. M., 288, 289, 290, 296, 396, 398, 399, 404, 405, 407, 408, 409, 446, 448, 453, 466, 476
Steele, J. R., 571
Stein, K. F., 462
Stein-Seroussi, A., 446
Stephan, C., 432, 477, 484
Stephan, W., 154, 425, 426, 477, 484
Sternberg, R. J., 203
Stiles, W. B., 359
Stillman, T., 564
Stoen, G., 85
Stoen, T., 87
Stone, J., 203, 204, 227, 228, 278, 284, 285
Storms, M. D., 289, 297
Straus, E., 523
Straus, M. A., 339
Stroebe, W., 149, 161, 468
Strong, S. R., 249
Stucke, T. S., 551, 564
Stuss, D. T., 177
Suggs, R. C., 150
Sullivan, A., 316
Sullivan, H. S., 8
Sullivan, J., 276
Summers, L., 394, 395
Sussman, M. B., 415
Suttie, I. D., 536
Swann, W. B., 225, 446, 448
Symons, D., 570, 577
Szyf, M., 498

Tabernero, C., 206
Tajfel, H., 288, 447
Talairach, J. T. P., 501
Tanke, E. D., 159
Tannenbaum, F., 533

Tarde, G., 18
Tatsuoka, M. M., 529
Taub, D., 150
Taylor, D. G., 342
Taylor, J. A., 105
Taylor, K. L., 446, 447, 468
Taylor, S. E., 58, 177, 288, 289, 297, 446, 468
Taylor, S. P., 322
Tellegen, A., 321
Terwilliger, R., 102
Tesser, A., 446, 448, 466, 475, 485
Testa, M., 483
Tetlock, P. E., 448
Thibaut, J. W., 512
Thibodeau, R., 228, 231, 285
Thomas, C., 444
Thompson, L. J., 446
Thompson, V. D., 149
Thorndike, E. L., 18
Tice, D. M., 551, 556
Tinbergen, N., 536
Tinsley, H. E. A., 528
Todd, P. M., 569, 570, 575, 577
Tolman, E. C., 54
Tolstoy, L., 89, 202
Townsend, J. M., 570
Tracy, P., 83, 85, 87
Traindis, H. C., 56
Trawalter, S., 492
Trembly, G., 101
Triandis, H. C., 76, 446
Trivers, R. L., 570, 577
Troche, C. J., 507
Trolier, T. K., 468
Trope, Y., 59, 186
Trost, M. R., 570
Trzesniewski, K., 207, 297
Tsang, W., 475
Tsuang, M. T., 373
Tukey, J. W., 371
Tulving, E., 177
Turner, J. C., 288, 446
Tuso, M. A., 58
Tversky, A., 523
Twenge, J. M., 551, 552, 553, 555, 556, 557, 558, 559, 561, 564
Tykocinski, O., 446

Umberson, D., 497
Unruh, H., 338
Uranowitz, S. W., 160, 165
Uvnaes-Moberg, K., 508

Valins, S., 547
Van Dijk, M., 551
Vance, C. S., 374
Vanneman, R. D., 342
Varey, C., 177
Vargas Llosa, M., 191, 194
Vasquez, E. A., 318
Venkatesan, M., 57
Ventrone, N. A., 569, 570
Vilberg, T., 557
Voelkl, K., 483
Vohs, K. D., 551, 556, 564
von Hentig, H., 335
von Hippel, W. H., 468
Vukcevic, D. P., 382

Wack, D. L., 325
Wadsworth, A., 113
Wager, T. D., 502, 507
Walberg, H. J., 202
Walker, D., 437, 438, 440
Wallis, K. F., 135
Walster, E., 149, 150, 154, 155, 159, 160, 161, 513, 519, 524, 527
Walster, G. W., 149
Walters, G., 522
Walters, R. H., 309
Wan, W., 204
Wang, Z., 498
Wann, D. L., 469
Ward, L., 547
Wasserman, T., 570
Watson, D., 321
Watson, J., 395
Watts, J. C., 102, 109
Wax, A., 406, 407, 409
Weaver, I. C. G., 498
Weber, M., 29
Weeden, J., 569, 570, 575, 577
Wegner, D. M., 179, 569
Wegner, D., 199
Weigold, M. F., 446
Weisberg, R. W., 202
Weiss, D. J., 528
Weiss, W., 113, 220
Weldon, P. E., 173
Wells, G. L., 59
Wepner, C., 139
Wexler, B. E., 507
Wheatley, T. P., 177

Wheeler, M. A., 177
Whelan, M., 497
Whitcup, M., 58
White, B. J., 415
Wicker, A., 164, 276
Wicklund, R., 251, 254, 276
Willcutt, H. C., 127
Willerman, B., 7
Williams, K. D., 552, 555, 556, 557, 558
Williams, W., 396
Wills, T. A., 446, 447, 468
Wilson, D. T., 263
Wilson, L., 342, 343, 353, 354
Wilson, T. D., 177, 289, 290, 297
Winer, B. J., 106, 256, 452
Winett, S., 58
Winfrey, C., 82, 85, 86, 87, 88, 92
Winkel, R. E., 557
Winkielman, P., 487
Winne, P. H., 476
Winslow, M., 228, 278
Witcher, B. S., 576
Witmer, J. F., 58
Wolfgang, M. E., 331, 335
Wollersheim, J., 252
Wood, H., 57
Wood, J. V., 446, 447, 468
Wood, R., 206
Wood, W., 497, 508
Woods, T., 202
Wright, M. O., 485, 492
Wrightsman, L., 342
Wurf, E., 446

Young, J., 139
Young, L. J., 498
Yzerbyt, V., 477

Zadny, J., 523
Zadro, L., 557
Zajonc, R. B., 165, 289, 325
Zak, P. J., 498
Zakay, D., 180
Zanakos, S., 179
Zanna, M., 164, 173, 174, 229, 285, 468, 533
Zeichner, A., 322
Zeki, S., 498
Zhang, L., 555, 556, 564
Zigler, E., 289
Zillmann, D., 326
Zimbardo, P., 91, 113, 179, 254, 342, 344, 347
Zuckerman, M., 321
Zuwerink, J. R., 468, 477

Subject Index

ability, malleable view of, 206–207
absolute power, 89
academic performance. *See also* schools; students; tests/testing
  of Asian Americans, 402
  jigsaw groups and, 11, 425–434
  positive feedback bias and, 475–492
  social exclusion and, 559–561
  stereotype threat and
    math and science achievement, 395–411
    self-affirmation intervention for, 288–298
academic setbacks, fixed intelligence and, 204
Acceptance of Interpersonal Violence (AIV) scale, 364
achievement, motivation and, 201, 202
achievement tests. *See* tests/testing
acquired immunodeficiency syndrome, 226, 278
adaptive unconscious, 189–200
adolescents. *See also* students
  condom use by, 540
Advances in Experimental Social Psychology (Geen and Quanty), 317
advocacy paradigm, counter-attitudinal, 277
aesthetic preferences, modification of, 18
affiliation behavior
  anxiety and, 544
  fMRI hand-holding study and, 496–509
  social exclusion and, 555–556
affirmative action, social psychology of, 436–444
African Americans, 4, 410. *See also* minority students; prejudice; race
  hotel and restaurant service for, 443
  intelligence self-theory and, 207
  in jigsaw groups, 425–434
  positive feedback bias and, 475–492
  stereotype threat and, 398–402, 408–409
Africans, intelligence of, 395
aggression, 307. *See also* violence
  after venting anger, 316–317, 325–326
  attitudes facilitating, 359–360

Bandura's social learning theory of, 357–358
filmed violence stimulating, 309
frustration and, 467
inclination toward, 304
interracial anger-mediated, deindividuation and, 341–355
justified fantasy, 309
mass media and, 132
predictors of, 361
sexual, 357–375, 537
social exclusion and, 553–565, 564
AIDS, 226, 278
Allport-Ross extrinsic and intrinsic scales, 45
aloofness, 524
altered experience, 352
amusement parks, littering in, 62–64
anger
fear-arousing communication and, 107–108
hydraulic theory of, 316
interracial, 341–355
repressed, 316–317
anger management, 315–326
catharsis theory of, 316–317
physical activity in, 326
anger-mediated interracial aggression, deindividuation and, 341–355
anti-Semitism, 11. *See also* prejudice
antisocial characteristics, 360, 365
anxiety, 250
sexual attraction and, 536–548
applied research, 12
arousal, 230
anger venting and, 325–326
fMRI hand-holding study and, 503
manipulation of, 319, 542
sexual, 363, 364, 537, 539
Art of Poetry (Aristotle), 303
Asch experiment (disagreement), 220
Asian Americans
grade point averages of, 402
math ability of, 399
athletes, 202. *See also* baseball
attenuation of distress, region-β paradox and, 176–186
attitude paradigm, counterattitudinal, 227
attitude(s), 17, 381, 382
behavior recall and, 166–170
change in, 102, 112–113
personal history recall and, 164–174
self-monitoring of, 171–172
toward personal responsibility, 171–172
toward toothbrushing and bathing, 168, 169
attractiveness. *See* physical attractiveness
attribution
math achievement and, 118–127
vs. persuasion, in behavior modification, 112–129
authority, 28, 37, 89, 219
autonomic nervous system, 497

babies, 401
baby booms, postwar, 335
Bandura's social learning theory of aggression, 357–358
baseball
desegregation in, 436–444
failure in, 10–13
Batson's Religious Life Inventory, 45
bathing, 167–174
beauty. *See* physical attractiveness
behavior
beliefs and, 201
changes in, 102
interpersonal
social exclusion and, 553–556
social perception and, 521–534
normative influence on, 57
social exclusion and, 550–565
behavioral compliance, fear-arousing communication and, 105
behavioral confirmation, 524, 529–530
assessment of, 527–528
mediators of, 531–532
social stereotypes of, 523–524
behavioral study of obedience, 27–40
behavior modification, 346
attribution vs. persuasion as means of, 112–129
children's littering habits and, 113–118
math achievement and, 118–129
behavior recall, effect of attitude on, 166–170
beliefs, 89
about behavior, 201
about effort, 204–205

beliefs (*continued*)
about malleable intelligence, 205–206
changed, 201
test performance and, 522
Beyond Bias and Barriers: Fulfilling the Potential of Women in Academic Science and Engineering (National Academy of Sciences), 395
Bible, 42
biofeedback, 347
black Americans. *See* African Americans
body count, 332
boxers, college, 311
boxing matches, homicide rate and, 132–144
boys' gangs, 415
boys' summer camp study, 415–424
brain, fMRI hand-holding study and, 496–509
brainwashing, Korean techniques for, 248
Brandeis University, 11
bridges, in high-anxiety sexual study, 538, 539–540
Brown v. Board of Education, 425
bubonic plague, 5
bystander intervention, 41

California Academic Skills Exit Exam, 404
California Personality Inventory (CPI) self-esteem scales, 265
campaigns, information-based persuasive, 275–281
camp study, 415–424
campus protests, 330
capitalism, 81
casualties, peacetime, 329–340
catharsis, 250, 303, 308, 313–314
catharsis theory, 316–317
Caucasians. *See also* race
overt hostility toward, 343
positive feedback bias by, 475–492
change
inevitable, psychology of, 437
in self-theories of intelligence, 206–210
charisma, 86
cheating, 232, 271, 273
Chicanos. *See* Latinos
children
African-American, 427
Latino, 4
littering by, 113–118
Mexican-American, 426, 427
minority. *See* minority students
pursuing common goals of, 427–429
sexual abuse of, 358
choice variable, in weight loss effort justification, 253–254
Christianity, 445
Civil War, lawlessness after, 338
classroom jigsaw groups, desegregated, 425–434
coercive appeals, 276
cognition, 220, 559–561
cognitive bolstering, social stereotypes of, 522–523
cognitive dissonance, 219–235, 275–281
arousal and, 277
causes of, 231
as motivating influence, 276
persuasion and, 276, 277
self and, 223–225
water conservation and, 275–276
weight loss and, 249–261
cognitive influence, on attractiveness judgments, 160–161
cognitive neoassociation theory, 317
college boxers, 311
college dances, 383
college students, 224, 343
condom use by, 226–227
dating relationships of, 192–193, 512–520. *See also* dating
littering by, 65–68
stereotype threat and, 398–399
commitment, in overcoming obstacles, 537
common endeavor, 421
common goals, pursuit of, 427–429
communication
fear-arousing, 101–110
social influence and, 219
Comparative Crime Date File, 334
competitiveness, in traditional classroom, 4
compliance, 21, 219
fear-arousing communication and, 105
without pressure, 237–248
conceptual replication, 250
condom use
by college students, 226–227
by high-school students, 228
hypocrisy about, 278
conflict, interracial, 342
conformity, 21, 82–86, 220
confusion, in social norms, 57
constructive thinkers, 522
conversations, getting-acquainted, 527

coolness, 524
cooperative learning groups, 4, 11, 425–434
cooperativeness, group, 420
coping, fear-arousing communication and, 105
counter-attitudinal advocacy paradigm, 277
creative genius, 202
criticism, 84
cults, 80–96

dances, college, 513
DAS (Dyadic Adjustment Scale), 499, 505–506, 508
dating
  in college, 192–193
  physical attraction and self-esteem and, 512–520
debriefing, helping behavior and, 49
deception, 225
deindividuation, anger-mediated interracial aggression and, 341–355
delayed auditory feedback apparatus, 254
delirium, 202
dependent variables, in jigsaw groups, 432–433
descriptive vs. injunctive norms, 57
desegregated classrooms, jigsaw groups in, 425–434
desegregation
  of baseball, 436–444
  effects of, 426–429
desensitization, 250
difficulty, meaning of, 204–205
disagreement (Asch experiment), 220
discipline, 84
dishonest behavior, induced self-esteem and, 263–274
disinhibitors, 354
disobedience, 28, 84
dissension
  in baseball desegregation, 437
  in Jonestown, 84
dissonance. *See* cognitive dissonance
distance, in interracial interaction, 387
distraction, in anger management, 318–326
distraction tasks, 198
distress
  attenuation of, region-β paradox and, 176–186
  helping in, 41
  rumination in, 197–198
  thought suppression and, 199
  writing about, 198–200
doctrinal orthodoxy scale, 45
dominance, as sexual motive, 364
dominance dimension, 359
dormitories, college, 65–68, 151–153
downward social comparison theory, 467–468
dreams, 202
Dyadic Adjustment Scale (DAS), 499, 505–506, 508

Ebbets Field, 436
economic gains, 342
education. *See* learning; schools; students
educational romanticism, 407
effort
  beliefs about, 204–205
  meaning of, 206
effort justification, in weight loss, 249–261
effort praise, 209
effort variable, in weight loss, 254–255
electrical shock, 28, 31–32
elementary schools. *See also* learning; schools; students
  cooperative learning in, 4, 11, 425–434
emergencies, bystander intervention in, 41
emotional catharses, 250
emotional experiences, writing about, 198–200
emotional response, to social exclusion, 556–559, 564
emotional-sexual attraction link, 541
emotional strain, 38
empathic role-taking, in jigsaw groups, 431–432
empathy, social exclusion and, 557–558, 564
employment interviews, interracial interactions in, 384–390
English proficiency exams, 204
entity theory, 203
equal status contact, 427, 437–438
errors, fielding, 13
esprit de corps, 417
esthetic preferences, modification of, 18
ethical implications, of achievement testing, 129
everyday events, recall of, attitudes and, 165–166
everyday introspection, 189–191
evidence base, 523–524
examinations. *See* tests/testing

exclusion. *See* social exclusion
exercise, in anger venting, 326
expectancy effects, 477
expectancy-violation theory, 483
expectations, 261, 381
experimental reality, 223
experimenter feedback, 33
external pressure, 237
extreme violence, 93
extremist dissenter, 93
extrinsic internal scale (Religious Life Inventory), 45
eye contact, 383, 386, 387

Facing the Fire: Experiencing and Expressing Anger Appropriately (Lee), 318
failure, 224
  attributions for, in jigsaw groups, 432
  in baseball, 10–13
  meaning of, 204, 206
faith
  Christian, 445
  helping behavior and, 45–46, 52–53
  leap of, 3–8
families, split, 85
fantasy aggression, justified, 309
Farrah factor, 159
fear
  fear-arousing communication, 101–110
  fMRI hand-holding study and, 496–509
fear-arousing communication, 101–110
feedback
  experimenter, 33
  in math achievement study, 119–120
  in obedience study, 32–33
  positive feedback bias, 475–492
  real-world, 210–211
females
  in compliance without pressure study, 239–244
  hostility toward, 359, 364
  intelligence praise for, 212–213
  math and science achievement of, 395–411
  sexual aggression against, 357
filmed violence, aggression and, 308–310
first impressions, attractiveness judgments and, 153–154
fixed vs. malleable intelligence, 203–205, 211, 213
fMRI hand-holding study, 496–509
foot-in-the-door technique, 237–248
forward lean, in interracial interactions, 386, 387
France, postwar homicide rate in, 335
friendliness
  anxiety and, 544
  social exclusion and, 555–556
friendly groups, 423
frustration-aggression hypothesis, 467
functional magnetic resonance imaging (fMRI), 496–509

gambling hypothesis, for homicide, 142–143
gangs, boys', 415
genius, creative, 202
Germany, Nazi regime in, 248, 331
getting acquainted conversations, 527
gifted children, labeling of, 212
girls. *See* females
goals, pursuit of, 427–429
Good Samaritan, 42
government-sanctioned violence, 329–331, 333–340
grade point averages (GPAs), 402
group decision-making, by white vs. black subjects, 384
group members
  attitudes of, 419
  individuating, 439–440
group pressure, 17, 19
  conformity and, 220
  individual resistance to, 24–25
group(s)
  conflict in, 415–424
  formation of, personal attraction and, 417
  informal, 415
  jigsaw, 4, 11, 425–434
  morale in, 420
  organization and spirit of, 419
gut feelings, 191, 194, 196–197

Handbook of Social Psychology (Lindzey and Aronson), 4
hand-holding study, 496–509
harmony, 421
Harvard University Laboratory of Social Relations, 19
hedonic states, paradoxical reactions to, 176–186
Heider's balance theory, 165

helping behavior
  situational and dispositional variables in, 41–54
  social exclusion and, 557–558, 564
high anxiety, sexual attraction and, 536–548
high-school students. *See also* students
  condom use by, 540
Hispanics. *See* Latinos
HIV infection, 226, 278
homework, parents' attitude toward, 210
homicide, 338
  alternative explanations for, 137–144
  gambling hypothesis for, 142–143
  mass media violence and, 132–144
  officially sanctioned, 330
  personal experience hypothesis for, 137
  precipitation hypothesis for, 142
  rates of, 325, 329
  ritualistic, 80–96
honesty, induced self-esteem and, 263–264
hospitals, littering in, 69–71
hostility, 304, 310, 315. *See also* aggression
  group, 419
  toward women, 364
Hostility Toward Women scale, 364
house hunting, emotional aspects of, 189–190
HPA (hypothalamic-pituitary-adrenal) axis, 497, 498, 508
human immunodeficiency virus infection, 226, 278
hurrying, helping behavior and, 47–48, 51, 53–54
hydraulic theory of anger, 316
hypnosis, 17–18
hypocrisy, 219–235, 278
hypothalamus, 502, 505, 507. *See also* HPA axis
hypothetical relationships, errors in recall of, 165

illegal violence, 330
immediacy
  decoding and reciprocation of, 382–383
  in interracial interactions, 366, 367, 369
immorality, 232
imperfection, tolerance of, 13
implosive therapy, 250
incremental requests, 244–248
incremental theory, 290
independence, 21
individuation, cues to, 347
induced self-esteem, dishonesty and, 263–264
inevitable change, psychology of, 437
inferior treatment, 342
informal groups, 415
information, exchange of, 8
informational social influences, on attractiveness judgments, 155–158
information-based persuasive campaigns, 275–281
informers, 84
in-group–out-group evaluations, in self-image maintenance, 467
initiation, 221
injunctive vs. descriptive norms, 57
in self-affirmation intervention for minority students, 288–298
inspiration, 202
institutional barriers, removal of, 443
insults
  duration of response to, region-β paradox and, 176–186
  racial aspects of, 348–349
intelligence
  changing theories of, 206–208
  fixed vs. malleable, 203–206, 211, 213
  self-theories of, 201–213
intelligence praise, 208–210
  for girls, 212–213
intelligence tests, 408–409, 514. *See also* tests/testing
interactive model, 358
interclass correlation, 528
interdependent learning environments, 428
internal state, deindividuation as, 349–351
interpersonal behavior. *See also* behavior
  social exclusion and, 553–556, 563–565
  social perception and, 521–534
interpersonal perception, social stereotypes and, 522
interpersonal violence, acceptance of, 364
interracial aggression, anger-mediated, deindividuation and, 341–355
interviews, employment, interracial interactions in, 384–390
Intrinsic scale (Religious Life Inventory), 45
introspection, 189–200
IQ fundamentalism, 395, 405–406

jigsaw groups, 4, 11, 425–434
job applicants, interracial interactions and, 383–390

Jonestown, 80–96
journals, functions of, 8
Judeo-Christian tradition, 42
judgments
  about physical attractiveness, 153–154
  confidence in, 21
junior high school students. *See also* students
  intelligence self-theory and, 207
justified fantasy aggression, 309

killing. *See* homicide
Korean brainwashing techniques, 248

labeling. *See* stereotypes
laboratory aggression, prediction of, 361
Laboratory of Social Relations (Harvard University), 19
Latinos. *See also* minority students
  academic performance of, 402, 403
  in desegregated schools, 426, 427
  intelligence self-theory and, 207
  in jigsaw groups, 425–434
  Scholastic Aptitude Test and, 410
laughter, as sign of tension, 35
leap of faith, 3–8
learning, 248. *See also* schools; students
  in cooperative groups, 4, 11, 425–434
  selective, 165
  teaching style and, 211
learning environments, interdependent, 248
learning task, in obedience study, 31
legitimization, of official violence, 330, 334–338
letters, in math achievement study, 119–120
littering
  by children, 113–118
  in public places, 56–77
looting, 330
love. *See* romantic behavior
loyalty, 81
lynch mobs, 341

mailboxes, college dormitory, littering around, 65–68
majority, influence of, 21–25
males, sexual aggression by, 357
malleable intelligence
  belief in, 205–206, 211
  Pygmalion effect and, 213
mass media
  aggressive behavior and, 132
  attractiveness judgments and, 161
  violence in, 132–144, 303, 313
math achievement
  behavior modification and, 118–129
  self-esteem and, 119
  stereotype threat and, 395–411
meaning, finding through introspection, 198–200
meaningful narrative, 199
medals, in math achievement study, 119–120
media. *See* mass media
mental rotation tests, 400
message, helping behavior and, 46–47
Mexican-Americans. *See* Latinos
mindfulness, 219, 279
minority students
  intelligence self-theory and, 207
  in jigsaw groups, 425–434
  positive feedback bias and, 475–492
  self-affirmation intervention for, 288–298
  social exclusion of, behavioral effects of, 565. *See also* social exclusion
  stereotype threat and, 398–402, 407
misattribution, of arousal, 229
mob violence, 341
modeling hypothesis, for homicide, 541
moderate dissenter, 22
morale, group, 420
motivation, 220, 288
  achievement and, 289
  for stereotyping, 470
  vs. attribution, in behavior modification, 118–129
movies. *See also* mass media
  violence in, 303, 313
murder. *See* homicide

narratives, meaningful, 199
naturalistic sexual aggression
  predictors of, 357–375
  self-reported, 358
nature–nurture debate, 394–396, 410
Nature of Prejudice (Allport), 426
Nazi Germany
  legitimated violence in, 331
  propaganda in, 248
negative affect, duration of, region-β paradox and, 176–186
negative self-expectancies, 224, 288–298
nervousness, as sign of tension, 35
neural systems, fMRI hand-holding study and, 496–509
newborns, 401

New Haven College, 514
No Child Left Behind (NCLB), 404
noncombatants, violence by, 329–340
noncompliance, 219
nonverbal mediation, of self-fulfilling prophecies in interracial interactions, 381–393
nonverbal stigma effect, 382
nonviolent resistance, in baseball desegregation, 439–440
normative conduct, focus theory of, 56–77
norms, confusion about, 57
norm to be kind, 475, 477
numbness, in social exclusion, 557–559, 564

obedience, 27–40, 82
officially sanctioned violence, 329–331, 333–340
openness, definition of, 204
opinions, social pressure and, 17–27
opinion statements, recall of, attitude and, 165
out-group polarization theory, 482–483

pain
  fear-arousing communication and, 104, 106, 109
  sensitivity to, social exclusion and, 557
paradoxical reactions, to intense hedonic states, 176–186
parents, intelligence praise by, 208–210
peacetime casualties, 329–330
peer evaluation, in attractiveness judgments, 161
penile tumescence, in sexual arousal, 366, 373
Peoples Temple, 80–96
perception, of social stereotypes, 522
performance, 205, 209, 211
  academic (*See* academic performance)
personal attraction. *See also* physical attractiveness
  group formation and, 417
personal experience hypothesis, for homicide, 137
personal history, attitude recall and, 164–174
personal involvement, cognitive dissonance and, 223
personality determinants
  fear-arousing communication and, 108
  helping behavior and, 41, 42–52, 53
personality testing, induced self-esteem and, 265–267
personality traits, physical attractiveness and, 524
personal problems, introspection about, 197–198
personal space, interracial interactions and, 386
persuasion, 86–88
  information-based, 275–281
  vs. attribution, in behavior modification, 112–129
physical activity, in anger venting, 326
physical attractiveness
  group formation and, 417
  judgments about
    cognitive influence on, 160–161
    contrast effects and, 149–162
    first impressions and, 153–154
    informational social influence and, 155–158
  romantic behavior and, 512–520, 524
  as social problem, 149–162
physical distance, interracial interactions and, 386
physical punishment, 93
Plague, The (Camus), 4–5
Plessy v. Ferguson, 425
positive feedback bias, 475–492
postwar baby booms, 335
power, absolute, 89
praise, effects of, 208–211
precipitation hypothesis, for homicide, 142
preferential selection, perception of, 142
prejudice, 11, 428
  anger-mediated interracial aggression and, 341–355
  baseball desegregation and, 436–444
  downward social identity theory and, 467–468
  frustration-aggression hypothesis and, 467
  in-group–out-group evaluations and, 467
  insults and, 348–349
  intelligence self-theory and, 207
  jigsaw groups and, 425–434
  Jonestown and, 80, 81, 87
  positive feedback bias and, 475–492
  reverse discrimination and, 342, 353
  self-affirmation function of, 445–470
  unmasking regressive racism and, 341–355
pressure, compliance without, 237–248

Princeton Theological Seminary, 44
Princeton University, 44, 384, 388
prize fights, homicide rates and, 132–144
propaganda, Nazi, 248
prosocial behavior
  situational and dispositional variables in, 41–54
  social exclusion and, 557–558, 564
psychology. *See* social psychology
psychotherapy, 249, 265
  effort justification and, 261
psychotic disorders, 360, 365
public places, littering in, 113–118
public schools. *See* schools
punching bag, in anger management, 318, 320–326
punishment, 83–84, 93, 219, 248
Pygmalion, malleable intelligence theory and, 213

race. *See also* African Americans; minority students; prejudice
  anger-mediated aggression and, 341–355
  baseball desegregation and, 436–444
  insults and, 348–349
  intelligence self-theory and, 207
  stereotype threat and, 288–298
rape, 357–358. *See also* sexual aggression
  predictors of, 357–375, 537
  self-reported, 358
  sexual responsiveness and, 358–359
rape indices, 367
Raven's Progressive Matrices test, 408–409
realistic partners, 513
recall of personal histories, attitude effects on, 164–174
reciprocated immediacy, in interracial interactions, 390–391
recycling, hypocrisy and, 230
region-β paradox, 177–186
regressive racism, unmasking of, 341–355
reinforcement, of math skills, 121–122
reinforcement theory, 199
reinterpretation, of personal history, 523
rejection. *See* social exclusion
reliability, of interracial interactions, 386, 390–391
reliability coefficients, 528
religion
  Christian, 445
  helping behavior and, 45–46, 52–53
Remembrance of Things Past (Proust), 193
repression, in catharsis theory, 316–317
requests, incremental, 244–248
resistance, nonviolent, in baseball desegregation, 439–440
reverse discrimination, 342, 353
reward-reinforcement theory, 222
rewards, in math achievement study, 119–120
Ring Book Boxing Encyclopedia, 134
ritualistic murder/suicide, 80–96
Robber's Cave experiment, 428
romantic behavior, physical attraction/self-esteem and, 512–520
rumination
  in anger management, 318–326
  self-defeating thought and, 197–200

salience procedures, 57
sanction by authority, 426–427
SAT scores. *See* Scholastic Aptitude Test scores
Scarlet Letter, The (Hawthorne), 199
Scholastic Aptitude Test (SAT) scores
  of politicians, 397
  stereotype threat and, 398–399, 405, 408
schools. *See also* learning; students
  cooperative learning in, 4, 11, 425–434
  Hebrew, 11
  littering in, 113–118
  testing in (*See* tests/testing)
scope vs. tightness, in theory building, 225–226
seatbelt interview, 168
selective learning hypothesis, 165
self-affirmation intervention, for racial achievement gap, 288–298
self-awareness, 351
self-compassion, 231–235
self-concept, 219–235
  prejudice and, 445–470. *See also* self-image maintenance
  social nature of, 557–558
self-consciousness, 7
self-esteem, 4, 221–235, 264, 273
  fear-arousing communication and, 105, 108
  induced, 263–274
  manipulation of, 514–515
  math achievement and, 119, 122
  romantic behavior and, 512–520
self-expectancies, negative, 224

self-focused attention, in anger management, 318–326
self-fulfilling prophecies, in interracial interactions, 381–393
self-image maintenance
  downward social identity theory and, 467–468
  frustration-aggression hypothesis and, 467
  prejudice as, 445–470
  social identity theory and, 467
self-image threat
  positive feedback bias and, 484–492
self-integrity, academic performance and, 288–298
self-justification, 89–94
self-persuasion, 222–225
self-regulation, social exclusion and, 561–564
self-reported sexual aggression, 358
self-theories of intelligence, 201–213
sexual abuse, 358
sexual aggression, 357, 537
  causes of, 358
  dominance and, 364
  naturalistic, 365–366
  predictors of, 357–375, 537
  rape indices and, 367
  self-reported, 358
  sexual responsiveness and, 358–359
sexual arousal, 539
  aggression and, 374, 537 (*See also* sexual aggression)
  genital measures of, 363, 366, 373
sexual arousal index, 373
sexual attraction, anxiety and, 536–548
sexual experience, 360–361, 365
sexual partnerships, 85
sexual responsiveness, sexual assault and, 358–359
Seymour: An Introduction (Salinger), 7
shifting standards research, 482
shocks, 28
  in obedience study, 31–32
shoulder orientation, in interracial interactions, 387
Single Factor model, 358
smoking cessation, fear-arousing communication in, 102
Snyder's self-monitoring scale, 168
social bonding. *See* affiliation behavior
social classes, 415
social exclusion, 550–565
  aggression and, 553–565, 564
  emotional response to, 556–559, 564
  friendliness and, 555–556
  interpersonal behaviors and, 553–556, 563–565
  physical response to, 557–558
  prosocial behavior and, 555, 564
  self-regulation and, 561–564
Social identity theory, 467
social influence, 219
  informational, on attractiveness judgments, 155–158
social norms, confusion about, 57
social perception, interpersonal behavior and, 521–534
social pressure, of opinions, 17–27
social psychology, 3–8, 10, 11, 18
  affirmation action and, 436–444
  cognitive, 522
social sensitivity test, 263
social standing, group, 419
social stereotypes, self-fulfilling nature of, 521–524
social stimuli, effect on judgment, 150
solidarity, group, 420
Southern Connecticut College, 514
spatial ability, 400, 401
speech error rate, in interracial interactions, 386, 387
spoiled identity, 475
standardized testing. *See* tests/testing
stereotypes. *See also* prejudice
  motivation for, 470
  of physical attractiveness, 524
  positive feedback bias and, 482
  self-affirmation and, 445–470
  self-fulfilling nature of, 521–524
stereotype threat, 394–414
  answers to criticisms of, 394–411
  self-affirmation intervention for, 288–298
stereotype vulnerability, 400–401, 402
stigma effect, nonverbal, 382
strangers to ourselves, 189–200
stress, stereotype threat and, 288–298
students. *See also* learning; schools
  college. *See* college students
  condom use by, 540
  cooperative learning groups for, 4, 11, 425–434
  intelligence self-theory and, 207
  littering by, 113–118

students, (*continued*)
math skills of
behavior modification and, 118–129
self-esteem and, 119
minority. *See* minority students
stereotype threat and, 398–402
testing of. *See* tests/testing
Student Teams Achievement Divisions (STAD), 433
success attributions, in jigsaw groups, 432
suggestibility, 18
suicide, 80–96, 82
summer camp study, 415–424
supporting partner, 22, 24–25
susceptibility measure, fear-arousing communication and, 105
suspension bridges, in sexual attraction study, 542
swimmers, water conservation by, 279

tasks, difficulty vs. enjoyableness of, 209
teaching style, 211
Teams Games and Tournaments (TGT), 433
teenagers. *See also* students
condom use by, 540
television, 303, 313. *See also* mass media
tension, in obedience study, 35, 37–38
tests/testing
for English proficiency, 204
personality, induced self-esteem and, 265–267
social exclusion and, 559–561
stereotype threat and, 288–298, 398–402
Thematic Apperception Test Manual, 539, 540, 543, 546
thought suppression, distress and, 199
threat, fMRI hand-holding study and, 496–509
tightness vs. scope, in theory-building, 225–226
time pressure, helping behavior and, 47–48, 51, 53–54
transgressors, dislike of, erroneous predictions about, 176–186
traumatic events
region-β paradox and, 176–186
rumination vs. writing about, 198–200
Tukey-Scheffé test, 371
tumescence rape index, 371

unambiguous perceptual judgment, 220
unconscious, adaptive, 189–200
United States homicide rate, mass media violence and, 132–144
University of British Columbia, 544
University of Minnesota, 525
University of Texas, 265
University of Waterloo, 166–169, 170–174

Vandenberg Mental Rotation Test, 400
venting, in anger management, 315–326
verbal comments, in math achievement study, 119–120
veterans, violence by, 338
victim feedback, in obedience study, 32–33
victim modeling, in homicides, 141
videotaping, condoms and, 228
Vietnam War, 329, 332, 334, 338
violence. *See also* aggression
attitudes facilitating, 364–365
filmed, 303, 308, 310
illegal, 330
interpersonal, acceptance of, 364
justified, 304
legitimation of, 330
mass media, homicide rates after, 132–144
by noncombatants, 329–340
observation of effects of, 303–314
officially sanctioned, 329–331, 333–340
by veterans, 338
visual judgment, 19

war. *See also* violence
homicide rate after, 336–337
violence by noncombatants in, 329–340
water conservation study, 229, 275–281
weight loss, effort justification in, 249–261
whites. *See also* race
overt hostility toward, 343
positive feedback bias by, 475–492
Why Aren't More Women in Science? (Ceci and Williams), 396
women. *See* females
writing, about distressing experiences, 198–200
written comments, in math achievement study, 119–120

Yale Medical School, 514
Yale University, 29–30, 103, 514