Psychological Inquiry
2005, Vol. 16, No. 4, 185–193

The Message Is the Method: Celebrating and Exporting the Experimental Approach

Timothy D. Wilson
University of Virginia

I am grateful for the opportunity to comment on the wisdom that social psychology has to offer to society at large. It is a challenging task, given the vast amount of knowledge the field has accumulated in its 60 or so years as an experimental science. As noted by Ross and Nisbett (1991), social psychology is unique in changing the lives of people who study it, because so much of what we investigate has implications for how we interact with others and get to know ourselves (Wilson, 2002).

When I teach introductory social psychology I try to convey these practical lessons. On the 1st day of class I tell the students that my goal is ambitious: to change the way in which they view the world and live their lives. I am sure I do not succeed with every student, but given the implications of social psychological research findings for everyday living, I hope I do with at least a few. On the last day of class I present a list of the 15 top practical lessons offered by social psychology, grouped into three categories: dealing with and understanding others (e.g., “have high expectations for others,” based on research on the self-fulfilling prophecy); managing our own lives (e.g., “act how you want to be,” based on research on self-perception theory), and child rearing practices (e.g., “don’t overdo punishments and rewards, use minimally sufficient justifications,” based on research on dissonance theory and attribution theory).

I have thought at some length about which of these lessons I want to expand on here. In the end I decided not to write about a specific body of knowledge, in part because it was so hard to narrow down the list. Instead I focus on a larger lesson that I try to convey when teaching social psychology, namely that the reason the field has so much to offer is because of its powerful methodology, in particular the experimental method. It is easy to forget what a huge innovation it was to conduct experiments on social influence and how much of our progress has depended on this approach. I discuss the origins of experimental social psychology to make the point that a major lesson we have to offer is how best to investigate socially relevant questions.

I then discuss ways in which we might take this innovation even further by returning to Lewin’s (1946) notion of action research, in which the experimental method is used to address social problems in applied settings. It is, perhaps, presumptuous of me to spend most of an article that is supposed to celebrate social psychology’s accomplishments on ways we can progress even further. It is because the field has done so well, however, and contributed so much, that we are in such a wonderful position to stretch our boundaries further.

The Audacious Use of Experiments to Test Social Psychological Questions

I am fortunate to own a copy of the first, 1947 edition of *Readings in Social Psychology*, edited by Theodore Newcomb and Eugene Hartley (my father was assigned the book in a college course soon after it was published and he kindly passed it on to me.) The 75 chapters offer an intriguing snapshot of the state of the field 60 years ago. There are an impressive number of classic articles, including Clark and Clark’s (1947) influential work on African American children’s preferences for dolls of different colors; a summary of Newcomb’s (1943) Bennington College study on social influence; an excerpt from Sherif’s (1936) book describing his studies of social influence using the autokinetic effect; Allport and Postman’s (1945) studies of rumor transmission; the Katz and Braly (1933) survey of college students’ stereotypes; and a chapter by Lewin (1947) reporting his studies of persuasion in group settings, in which homemakers were successfully convinced to use more organ meats during World War II. Many of these articles continue to be cited in today’s social psychology textbooks.

On the other hand, we see a discipline that was quite different from modern social psychology, especially in the diffuseness its topic matter and methods. In addition to the aforementioned articles, the Newcomb and Hartley book contains reprinted articles by Margaret Mead, Jean Piaget, Abraham Maslow, Erich Fromm, and Bruno Bettelheim, as well as chapters on child development, adolescence, social class, and linguistics. The nascent field of social psychology was moving toward Lewin’s (1947) goal of integrating “cultural anthropology, psychology, and sociology into one science” (p. 330), but it was not as distinct from the other social sciences or subareas of psychology as it would soon become.

An important reason for this diffuseness is that the field had not yet settled on a common methodology. The authors of the Newcomb and Hartley chapters made brilliant use of many kinds of data, including informal observations, participant observation, and laboratory studies in which people were given surveys or observed under various circumstances. What was missing was the tightly controlled experiment that has become the bread-and-butter method of the field. In my (admittedly quick) perusal of the 75 chapters, I was able to find only a handful that reported anything like modern-day experiments, in which a small number of independent variables were manipulated in an experimental design.

How quickly this was about to change! In the 1930s and 1940s Lewin and his students had made a critical discovery: the audacious idea that the experimental method could be used to study complex social psychological problems. We can see the beginnings of this revolution in the Newcomb and Hartley book. True, the initial uses of the experimental method were crude by today's standards. The Lewin studies of group influence, for example, and studies of leadership style by Lewin, Lippitt, and White (1939), included experimental treatments that were broad-brush and multifaceted, such that it was not entirely clear what produced the critical effects. But viewing these studies through the lens of present methodological standards misses the point of how pioneering they were–a bit like criticizing James Naismith, the inventor of basketball, for not using sophisticated zone defenses in his initial games.

These classic experimental studies and others, notably Sherif's (1936) investigations of social influence and Hovland, Janis, and Kelley's (1953) studies of persuasion, are best viewed as harbingers of a coming explosion of experimental research. Social psychologists today sometimes forget, I think, what a radical departure it was for our discipline to harness the experimental method to study questions of social influence. Although Lewin's contributions to social psychology were many, perhaps his greatest was the novel idea that social influence could be studied scientifically with the experimental method (Festinger, 1980). Lewin began the revolution, and after his premature death it was carried on by those he trained, notably Leon Festinger and the first crop of students at the Research Center for Group Dynamics, including Stanley Schachter and Harold Kelley. The social psychological experiment was a powerful tool that led to the development of influential new theories (e.g., dissonance theory, attribution theory) and the explosion of research that continues today.

I do not mean to minimize the importance of other methodological approaches; psychology needs a large toolbox of methods, including correlational and longitudinal designs. Experiments are what separate us from related disciplines, however, and without them we would be a much more amorphous field that would differ little from, say, sociology or anthropology. We have, thankfully, avoided much of the angst these fields are experiencing, as their methods of inquiry are challenged by postmodernists and other antiempiricists (save for a minor skirmish in the 1970s that was easily fended off; Gergen, 1973; Schlenker, 1974). Social psychology has remained a vibrant science, unlike its ailing cousins in the social sciences.

Have We Become Too Narrow in Our Methods and Topics of Study?

It might be argued that social psychology went down too narrow a path in its obsession with the experimental method, particularly the laboratory experiment. Reading the chapters in the Newcomb and Hartley book (compared, say, to an issue of one of our recent journals) is quite refreshing; as mentioned, the topics and methods are broader than what we typically study today, including participant observation studies of rumor transmission, discussions of war and peace, and case studies of polygynous Mormon families and a professional thief. Have we become too narrow?

I think the answer is no, in terms of our methods, but yes, in terms of our topics of study. It was a necessary step for the field to find its identity by harnessing the experimental method and finding creative ways of using it to develop social psychological theories. But now that we have accomplished this, it may be time to become more ambitious in our topics of study. We have found our identity and should not be shy about stretching the boundaries of the field and applying the experimental method to a wider scope of issues. In one sense such a broadening is well under way, as evidenced by the renewed interest in topics such as culture and evolution. Indeed, it is clear that the most interesting work on culture is occurring in psychology and not anthropology—in large part because of the application of rigorous methodological techniques, chiefly the laboratory experiment (e.g., Markus & Kitayama, 1991; Nisbett, 2003). I would like to focus on a different type of expansion that I think has been neglected: exporting the experimental method out of the laboratory into applied settings.

Such an exportation would return to an important mission of the founders of our discipline, namely Lewinian action research. Although this term was never precisely defined by Lewin and his colleagues (Festinger, 1980), the gist was to understand basic social processes with the use of the experimental method and to use this knowledge to understand and solve social problems. In Lewin's (1946) words, "Progress will depend largely on the rate with which basic research in social sciences can develop deeper insight into the laws which govern social life. This 'basic social research' …

will have to include laboratory and field experiments in social change" (pp. 35–36). Most of the early research efforts by Lewin and his students were experimental investigations of applied problems, such as the study on increasing use of organ meats, the attempt to study leadership styles in an experiment, and studies by Lewin's students on how best to introduce changes in production techniques in factories (Coch & French, 1948). In Festinger's (1980) words, a research topic was important to Lewin and his students "if it made a difference with respect to actual problems in the world, real events and processes" (p. 239).

I believe we are in an even better position now to conduct such action research. The field has done an excellent job on the basic research side, and I fervently hope it continues to do so. But, we can do a better job of combining basic and applied research in the manner advocated by Lewin. By so doing we will make real progress in solving social problems and will gain credibility with policy makers and granting agencies. The key, I believe, is taking what we do best—conducting experiments—and extending this method to more applied domains. We have not done as much as we can to show others the importance of conducting experiments to determine causality, whether the question is about basic social processes or the effectiveness of social interventions.

I fear that by making such a plea I will appear critical of basic theoretical research in social psychology, which is not my intention at all. Another of Lewin's major contributions, in addition to championing the experimental method, was the emphasis placed on theory-based research. It was the brilliance of Lewin's students in researching basic theoretical questions (e.g., the contributions of Festinger, Kelley, and Schachter) that shaped the field and continues to take center stage—rightly so, in my opinion. My point is not to question basic research, but to suggest that because of it we are in an excellent position to conduct more action research. We know a great deal about stereotyping, prejudice, persuasion, social influence, relationships, self-regulation, and group behavior—should not some of us, at least, see what can be done with this knowledge to solve problems in the real world?

Curiously, research on applied issues by the early Lewinians did not amount to much (other than inventing consciousness-raising sensitivity groups that became de rigueur in the 1960s; Ross & Nisbett, 1991). It is because of our success at developing powerful methodologies and theories that we are in a much better position now than in the 1940s to adopt the Lewinian action research approach. It is time for more researchers to at least think about what we have to contribute to important social problems and perhaps make efforts to export what we know.

Again, I believe the most important thing to export is not a specific body of knowledge but a way of evaluating interventions and social policies—the experimental method. We have a great deal of knowledge to apply, of course, but it is often hard to predict exactly how effects studied under controlled laboratory conditions will play out in the real world. Given the complexities of social life, it is always best to begin with small-scale interventions that are tested experimentally, rather than a massive intervention with no means to test its effects (Ross & Nisbett, 1991).

The Power of the Experimental Method: Let Us Export It

Although the power of the experimental method may seem obvious to social psychology insiders, its potential is less obvious to practitioners, policy makers, and the general public. A few examples might help make this point.

Many organizations, including schools, universities, and businesses, are mandating diversity education for their members. The initiatives include workshops, written materials, online exercises, videos, and small group meetings, and the goals are to educate people about cultural differences, improve communication, and increase tolerance. As admirable as these goals are, the question arises as to how to design optimal interventions and assess their effectiveness.

To people untrained in social psychology, common sense dictates that such assessment is easy and straightforward: ask people what they thought about the intervention. Keller, Young, and Riley (1996), for example, offer four ways of assessing the effectiveness of diversity training programs, including the assessment of participants' reactions, learning, transfer of what people learned to the workplace, and organizational change. None of these assessments involve control groups; at best, the authors suggest a pre–post design, in which people's knowledge is assessed before and after the exercise.

Measures such as feedback from participants are, of course important. If participants complain that the intervention was a waste of time or worse, insulting and demeaning, the designers of that intervention should obviously take note. But, it is a near truism in social psychology that participants are not always the best authority to assess how much their attitudes and beliefs have changed (Bem & McConnell, 1970; Goethals & Reckman, 1973; Wilson, Houston, & Meyers, 1998). Further, because an intervention made people feel uncomfortable or bored does not necessarily mean it failed to change their attitudes and beliefs. One of the major attitude change techniques in social psychology, procedures based on cognitive dissonance theory, *requires* that people experience negative affect for attitude change to take place (Elliot & Devine, 1994; Zanna & Cooper, 1974). Also, just because participants believe that they learned a lot does not mean they did (M. Ross, 1989).

Suppose we convinced administrators that to assess the impact of diversity education exercises, we must measure people's actual attitudes, beliefs, and behaviors. Fine, they might say, follow people over time and see if there is any change in these measures after people take part in the exercise. If people's attitudes toward out-groups become more favorable after participating in diversity education, and incidences of harassment and conflict decrease, then clearly the intervention worked. Right? Wrong, as any beginning student in a methodology class knows. Without a randomly assigned control group, there is no way of telling whether the intervention or some other factor, such as a highly publicized public incident or a change in some other organizational policy, produced the result.

I recently experienced the failure to appreciate these methodological niceties firsthand. A university with which I am familiar decided that all incoming students should be exposed to some form of diversity education. One option under consideration was an online exercise that would make students aware of issues surrounding diversity on campus and appreciate alternative points of view and lifestyles.

Who would design the online exercise and how would it be evaluated? To any experimental social psychologist, the answer to these questions seems obvious. We, as a field, know a great deal about stereotyping, prejudice, persuasion, and attitude change, and we know how to design such an intervention so that we could evaluate its impact. In fact, many of us would salivate over the opportunity to test interventions with a sample of several thousand students, with the ability to randomly assign the students to treatment and control conditions.

None of these possibilities, however, occurred to the university administrators in charge of the program. On the question of who should design the online intervention, the administrators' first idea was to hire a consulting firm that specializes in diversity education (primarily for businesses). Many such firms have sprung up, although few, to my knowledge, have hired social psychologists. On the question of how the intervention should be evaluated, well, this issue seems never to have occurred to the administrators, other than including some questions asking participants what they thought about the exercise. The idea of using an experimental design is a foreign idea to nonscientists. When I attempted to explain the rationale for conducting an experiment to a faculty committee charged with diversity education, a brilliant humanities professor listened politely to my suggestion that there be a randomly assigned control condition that did not complete the exercise (at least not right away), so that we could compare the attitudes of people who completed it with those who did not. He was genuinely perplexed and asked how we could tell whether the exercise had any impact on people's attitudes, when we had not measured their attitudes before they completed it. (In fairness, other faculty and administrators quickly caught on to the value of an experimental design, once it was explained to them.)

Educating people about the value of an experimental intervention is just the first step. There are other obstacles to implementing such interventions, both practical and political. On the political side, consider the motivations of businesses whose job it is to sell programs such as diversity education initiatives (of which there are many). One might think that they would be motivated to evaluate the effectiveness of their interventions by including randomly assigned control groups, so that they could gain an edge over their competitors. For all I know, there are businesses that have adopted this strategy. But most have not. Part of this is ignorance of the power of the experimental method. Another reason, however, may be that there is nothing to gain and much to lose for these businesses by carefully evaluating their programs.

Imagine that a consultant develops a diversity training program for a university in return for a large fee. The consultant administers an online exercise that includes a few questions at the end asking students to evaluate the program. Most students report that the exercise was interesting and useful. The consultant is happy, the university administrators are happy, and the students who completed the exercise are happy.

But did the exercise have any impact on students' attitudes, beliefs, and behaviors relevant to diversity? Suppose, like a good scientist, the consultant used an experimental design in which one randomly assigned group of students completed the exercise and another did not, after which all students completed a battery of measures of their attitudes, beliefs, and behaviors.

Although we social scientists can see the advantage of such an experiment, the consultant has little to gain from conducting it. If the experiment reveals that the group that got the intervention had more positive attitudes, beliefs, and behaviors than the control group, this would only confirm what the administrators and students already believed. If the experiment disconfirmed these impressions—showing no effects of the intervention, or worse, a negative effect on people's attitudes, beliefs, or behaviors—the consultant has a lot to lose.

I do not mean to malign the motives of the designers of interventions. Many practitioners have devoted their lives to sincere efforts to help others—more so, certainly, than people like me who have focused mostly on basic research. It may be this very devotion to a particular program, however, that produces queasiness about seeing it put under the harsh spotlight of the experimental method. All of us who do laboratory research are familiar with that awful, sinking, feeling that results from an experiment that does not work; there are few worse things than seeing months of efforts come to naught. Imagine how it must feel when the stakes are higher and an investigator learns that an intervention that cost thousands of dollars had no impact on people's lives—or worse, had a negative impact.

Another problem with using experimental designs is that it necessitates that some people will be in a control group and therefore not receive a treatment believed to be beneficial. Some may view the decision to use a control group as ethically questionable. I have found it helpful in such cases to compare an intervention to drug testing, asking people whether they would favor the mass administration of a new drug that had not been tested in experimental trials with randomly assigned control groups. Although useful, this analogy sometimes falls flat because people see little danger in administering an untested social intervention ("What's the harm? It can't hurt …"), whereas untested drugs can cause lasting harm or even death.

Unfortunately, untested social interventions can also be harmful. One example is the Cambridge–Somerville youth study, in which a massive intervention was used to try to help boys who were at risk for delinquency. Over an average of 5 years the boys received a tremendous amount of attention, including tutoring, psychotherapy, family interventions, and social and recreational activities (Powers & Whitmer, 1951). What were the effects of the program? As detailed by Ross and Nisbett (1991), if we went by people's reports, we would conclude that the program was a success. Many of the boys and their caseworkers reported positive impressions of the program. But, this program was highly unusual, in that it included a randomly assigned control group of boys who did not receive any of the treatments. Assessments of the boys in both the experimental and control groups revealed a much different story: Not only was there no evidence that the intervention helped the boys, there was some evidence that it harmed them. For example, the boys in the treatment group were significantly more likely to have multiple criminal offenses in the years after the program than were boys in the control group (see Ross & Nisbett, 1991, for an insightful discussion of why the program might have failed).

Another example is the use of Critical Incident Stress Debriefing (CISD) to treat posttraumatic stress syndrome. CISD is a technique developed by Mitchell (1983) to treat groups of individuals who have experienced a traumatic event, such as emergency service workers who have witnessed multiple deaths in a natural disaster or plane crash. The idea is to bring people together as soon as possible after the event for a 3 to 4 hr session in which participants describe their experiences in detail and discuss their emotional reactions to the events. This cathartic experience is purported to prevent later psychiatric symptoms.

Does CISD work? Once again, the answer depends on how it is evaluated. Judging by the reports of people who undergo the intervention, the technique gets a thumbs up—most participants describe CISD as having been helpful (McNally, Bryant, & Ehlers, 2003). The results of experiments in which people are randomly assigned to receive CISD or to a control group, however, reveal a different story. There is considerable evidence that people who receive the treatment fare no better than those who do not, and some evidence that receiving the treatment increases people's risk for later problems (McNally et al., 2003). Right after a traumatic event, when people are experiencing considerable negative emotions, may not be the best time for people to focus on the event and discuss it with others. Instead, it may be better to wait until people have some distance from the event and can process it more objectively (Pennebaker, 2001).

The point is that the null or harmful effects of these interventions would never be known if people had not used experimental designs to test them. Most social interventions are not tested in this manner, although they could be. The staff at colleges and universities who assign roommates to entering students often have firm ideas about what makes people compatible. It is costly to students if their theories are wrong, and costly to the university to have to deal with complaints and requests to change roommates. It would be relatively easy to conduct experiments in which roommates are assigned on the basis of different sets of criteria, and I bet that many social psychologists interested in interpersonal relationships would jump at the chance to help design such a study.

Hundreds of new programs are initiated each year in secondary schools, many of which are attempting to change psychological variables such as learning and memory, peer relationships, and again, stereotyping and prejudice. Experimental designs to test the effectiveness of these programs are rarely considered. Similarly, many social issues and policies could be tested experimentally, with a little Lewin-like ingenuity. An example is the Moving to Opportunity project sponsored by the United Stated Department of Housing and Urban Development, which is a rare case of an experimental test of a social program (Goering & Feins, 2003). Volunteer low-income families with children, living in high-poverty areas in five American cities, were randomly assigned to receive financial assistance to move to housing in low-poverty areas or to a control group that did not move (Kling, Ludwig, & Klatz, 2005; Leventhal & Brooks-Gunn, 2003). Surely, one might think, such an experiment is unnecessary, because it is obvious that improving the living situation of poor families would have beneficial effects. In particular, it seems obvious that adolescents who move from poor, high-crime neighborhoods to wealthier, low-crime neighborhoods would be less likely to get into trouble, because they might be less likely to encounter negative peer influences. Should we not devote all available resources to help people move, rather than conducting an experiment?

As it happened, the housing experiment uncovered some nonobvious effects. In the first 2 years after moving, as expected, adolescents in the experimental group were less likely to be arrested for violent crimes (Kling et al., 2005). After 5 years, however, unexpected differences emerged in crime rates: Although female youths in the experimental continued to be less likely to be arrested for most crimes, male youths were significantly *more* likely to be arrested for property crimes, compared to those in the control group, who did not move (Kling et al., 2005). This surprising gender difference, which would probably not have been discovered without an experimental design, is likely to stimulate novel theorizing and research into its origins. Kling et al. (2005), for example, considered a variety of explanations, including the possibility that male youths become greater targets of discrimination after moving to wealthy neighborhoods (almost all of the participants were African American or Hispanic); that girls thrust into a new neighborhood cope in more effective ways than boys do, such as by consulting with parents and other adults when they have difficulties; and that boys who were already inclined to commit property crimes had more opportunities to do so in wealthier neighborhoods.

Examples such as the effects of CISD and the Moving to Opportunity project reiterate the importance of conducting experimental investigations of social programs in spite of possible ethical objections to leaving some participants "untreated." In McNally et al.'s (2003) words,

> Investigators may object to randomly assigning trauma-exposed individuals to a no-treatment control condition. Depriving them of a potentially helpful treatment seems to raise ethical issues. Of course, this objection presupposes that the intervention is, indeed, effective. If an intervention is not known to work, there is no ethical problem in withholding it. (p. 58)

I would add that there is an ethical problem in *not* conducting experiments to test interventions that could have harmful effects. If it is indeed true that CISD has harmful effects, imagine the disservice that has been done to the thousands of emergency service workers who have been required to undergo it.

Who Should Do the Exporting?

It may seem odd for someone who has spent most of his career conducting basic research to make a plea for more research on social problems. Although I have dabbled in applied research (Wilson, Damiani, & Shelton, 2002; Wilson & Linville, 1982), and have recently become involved in other applied projects, I have not practiced much of what I am preaching. Let me explain.

First, as I mentioned earlier, I am not maligning basic research at all. I believe that it is *because* we have made so much progress in understanding basic social psychological processes, and how to investigate them, that we are in such a good position to export what we know. I certainly hope that many talented theoreticians and methodologists continue on the basic research path.

I hope it becomes easier and more acceptable, however, for people to try to apply what they know—and test new hypotheses—in real-world settings. A great deal of applied work is going on, of course, in areas such as health, law, and business. As seen in the tables of contents of our major journals and the programs of our major conventions, this work is not, for the most part, in the mainstream. In general, our field places a higher value on basic, theoretical research than on applied research.

There are exceptions, and considering them is instructive. It is dangerous to single out individual researchers, because I will invariably leave out many others who are equally worthy of acclaim. I will mention a few, however, who are true Lewinians, investigating basic theoretical issues as well as doing applied work in the field. The first is Elliot Aronson, who has conducted groundbreaking empirical and theoretical work on dissonance theory (as well as other theoretical issues) and seminal applied work on cooperative education and the reduction of prejudice (as well as research on other applied issues, such as energy conservation). Another is Claude Steele, who has conducted highly influential theoretical work on the self, including self-affirmation theory and stereotype threat, as well as theoretical work on the effects of alcohol. In addition he has been involved in a great deal of applied work, chiefly in educational settings (e.g., Steele, 1997). A third is Shelley Taylor, who has conducted important theoretical work in attribution, mental illusions, stress, and social comparison, as well as becoming one of the most prominent researchers in the field on health and coping. Yet another is Jamie Pennebaker, who has conducted important laboratory research on health, coping, language, and personality, as well as influential research applying his findings in important settings. For example, Petrie, Fontanilla, Thomas, Booth, and Pennebaker (2004) recently found that patients infected with the human immunodeficiency virus (HIV) who wrote about traumatic and emotional experiences showed significantly greater improvements in immune functioning in the ensuing months than a control group—at a magnitude comparable to the benefits of anti-HIV drugs.

It is interesting that all of these researchers began their careers by conducting basic laboratory research on important theoretical issues. In fact, it would be a misnomer to label these psychologists "applied researchers." They are basic researchers who have turned their attention to applied issues, in other words, they are true Lewinian action researchers. The lesson, I believe, is that the ontogeny of the individual career benefits from mimicking the phylogeny of the field: getting a firm grounding in the nuances of the experimental method and theory and then exporting this knowledge to applied settings. This is the direction the field as a whole has taken, and it can be a valuable path for individual researchers as well. For example, I do not think we necessarily need separate training programs in basic and applied social psychology, two tracks that graduate students have to choose between. The kind of training graduate students get in our typical graduate programs, in basic research and experimental methodology, is indispensable to doing good research in applied settings. That is not to say that we could not do a better job in preparing students for such careers by including additional training in the problems of action research (and doing it ourselves). But we should not provide this at the expense of training in basic theory and methodology.

Types of Action Research

I have used the term *action research* broadly. It is worth considering different forms it might take.

Field Studies to Test Theoretical Ideas

It is notable that Lewin conducted field studies, such as his studies of persuasion in group settings and leadership in groups, rather than lab experiments. The lab has many benefits, of course, and will always hold a central place in the field's arsenal of methods. In fact, the lab is often the most appropriate place to test theoretical ideas (Mook, 1983). We should not forget to ask ourselves, however, whether our ideas can be tested in field settings, to help establish the external validity of our findings. One of my favorite field experiments is the Lepper, Greene, and Nisbett (1973) study of intrinsic motivation in a preschool, which made an important theoretical discovery (external rewards can undermine intrinsic interest) in an applied setting. Other studies have elegantly shown how processes discovered in the laboratory operate in real-life situations, such as the Knox and Inkster (1968) study of dissonance reduction at a racetrack and the Michaels, Blommel, Brocato, Linkous, and Rowe (1982) study of social facilitation in a pool hall. Studies such as these are not applied research in the sense of addressing solutions to social problems, but are vital in establishing the relevance of social psychological theories to real-world settings. And, they are likely to stimulate thinking about interventions to change important social behaviors.

Testing Applied Implications of Theoretical Ideas

There have been several attempts to test interventions that are based directly on theoretical findings from the social psychological laboratory. Examples include research on "attribution therapy," in which the principles of attribution theory were used to design interventions to help people with physical and psychological problems (e.g., Valins & Nisbett, 1972; Wilson et al., 2002); interventions to help the residents of nursing homes, based on laboratory research on perceived control (e.g., Langer & Rodin, 1976; Schulz, 1976); and interventions to help the academic achievement of minorities, based on laboratory research on stereotype threat (Steele, 1997; Steele & Aronson, 1995). I think we will see more of this kind of application in the future, for example, in the area of implicit prejudice and stereotyping.

Testing Interventions in Applied Settings

There are many opportunities to conduct action research that is less theory based, by testing the effectiveness of programs and policies in natural settings. One example is diversity education programs that are being implemented in many organizations, as discussed earlier. If social psychologists can gain control over programs such as these, the interventions do not have to be theory-less, of course. In the best of all worlds, we will design theory-based interventions and test them experimentally. When we have less control over the content of interventions and social policies, as is often the case, we still have a lot to offer by designing experiments to test them. For reasons already discussed, it is not always easy to convince policy makers of the importance of such designs. We need to do a better job of educating the public, by, for example, stressing methodology more in our classes and working to include education about methodology in secondary schools. A side benefit of getting involved in evaluating social interventions should not be overlooked: Our theories will be enriched. Tightly controlled experimental tests of diversity education programs, educational initiatives, roommate matching, and economic policies promise to yield findings of considerable theoretical interest.

It is exciting to think about the progress that could be made if we were to engage in more action research using experimental designs. Not only would we learn much more about how to solve many social problems, we would gain credibility with governmental officials and granting agencies. In my fanciful view of the future, policy makers will seek out social psychologists to help solve problems before consulting economists and political scientists. The policy makers will think, "Not only do social psychologists have well grounded theories about how to address social problems, they have methods to see if they are right."

Note

Correspondence should be sent to Timothy D. Wilson, Department of Psychology, 102 Gilmer Hall, University of Virginia, Charlottesville, VA 22904–4400. E-mail: twilson@virginia.edu

References

Allport, G. W., & Postman, L. J. (1945). The basic psychology of rumor. *Transactions of the New York Academy of Sciences, Series 2, 8,* 61–81.

Bem, D. J., & McConnell, H. K. (1970). Testing the self-perception explanation of dissonance phenomena: On the salience of premanipulation attitudes. *Journal of Personality and Social Psychology, 14,* 23–31.

Clark, K. B., & Clark, M. P. (1947). Racial identification and preference in Negro children. In T. M. Newcomb & E. L. Hartley (Eds.), *Readings in social psychology* (pp. 169–178). New York: Holt.

Coch, L., & French, J. R. P., Jr. (1948). Overcoming resistance to change. *Human Relations, 1,* 512–532.

Elliot, A. J., & Devine, P. G. (1994). On the motivational nature of cognitive dissonance: Dissonance as psychological discomfort. *Journal of Personality and Social Psychology, 67,* 382–394.

Festinger, L. (1980). Looking backward. In L. Festinger (Ed.), *Retrospections on social psychology* (pp. 236–254). New York: Oxford.

Gergen, K. J. (1973). Social psychology as history. *Journal of Personality and Social Psychology, 26,* 309–320.

Goering, J. & Feins, J. D. (Eds.). (2003). *Choosing a better life?: Evaluating the moving to opportunity social experiment*. Washington, DC: Urban Institute Press.

Goethals, G. R., & Reckman, R. F. (1973). The perception of consistency in attitudes. *Journal of Experimental Social Psychology, 9,* 491–501.

Hovland, C. I., Janis, I. L., & Kelley, H. H. (1953). *Communication and persuasion: Psychological studies of opinion change*. New Haven, CT: Yale University Press.

Katz, D., & Braly, K. W. (1933). Verbal stereotypes and racial prejudice. *Journal of Abnormal and Social Psychology, 28,* 280–290.

Keller, J. M., Young, A., & Riley, M. (1996). *Evaluating diversity training: Seventeen ready-to-use tools*. San Diego, CA: Pfeiffer.

Kling, J. R., Ludwig, J., & Klatz, L. F. (2005). Neighborhood effects on crime for female and male youth: Evidence from a randomizing housing voucher experiment. *Quarterly Journal of Economics, 120,* 87–130.

Knox, R., & Inkster, J. (1968). Postdecision dissonance at post time. *Journal of Personality and Social Psychology, 8,* 319–323.

Langer, E. J., & Rodin, J. (1976). The effects of choice and enhanced personal responsibility for the aged: A field experiment. *Journal of Personality and Social Psychology, 34,* 191–198.

Lepper, M. R., Greene, D., & Nisbett, R. E. (1973). Undermining children's intrinsic interest with extrinsic reward: A test of the overjustification hypothesis. *Journal of Personality and Social Psychology, 28,* 129–137.

Leventhal, T., & Brooks-Gunn, J. (2003). Moving to opportunity: An experimental study of neighborhood effects on mental health. *American Journal of Public Health, 93,* 1576–1582.

Lewin, K. (1946). Action research and minority problems. *Journal of Social Issues, 2,* 34–46.

Lewin, K. (1947). Group decision and social change. In T. M. Newcomb & E. L. Hartley (Eds.), *Readings in social psychology* (pp. 330–343). New York: Holt.

Lewin, K., Lippitt, R., & White, R. K. (1939). Patterns of aggressive behavior in experimentally manipulated "social climates." *Journal of Social Psychology,10,* 271–299.

Markus, H. R., & Kitayama, S. (1991). Culture and the self: Implications for cognition, emotion, and motivation. *Psychological Review, 98,* 224–253.

McNally, R. J., Bryant, R. A., & Ehlers, A. (2003). Does early psychological intervention promote recovery from posttraumatic stress? *Psychological Science in the Public Interest, 4,* 45–79.

Michaels, J. W., Blommel, J. M., Brocato, R. M., Linkous, R. A., & Rowe, J. S. (1982). Social facilitation and inhibition in a natural setting. *Replications in Social Psychology, 2,* 21–24.

Mitchell, J. T. (1983). When disaster strikes … The Critical Incident Stress Debriefing process. *Journal of Emergency Medical Services, 8,* 36–39.

Mook, D. G. (1983). In defense of external invalidity. *American Psychologist, 38,* 379–387.

Newcomb, T. M. (1943). *Personality and social change*. New York: Dryden.

Newcomb, T. M., & Hartley, E. L. (Eds.). (1947). *Readings in social psychology.* New York: Holt.

Nisbett, R. E. (2003). *The geography of thought: How Asians and Westerners think differently … and why.* New York: Free Press.

Pennebaker, J. W. (2001). Dealing with a traumatic experience immediately after it occurs. *Advances in Mind-Body Medicine, 17,* 160–162.

Petrie, K. J., Fontanilla, I., Thomas, M. G., Booth, R. J., & Pennebaker, J. W. (2004). Effect of written emotional expression on immune function in patients with human immunodeficiency virus infection: A randomized trial. *Psychosomatic Medicine, 66,* 272–275.

Powers, E., & Whitmer, H. L. (1951). *An experiment in the prevention of delinquency: The Cambridge-Sommerville youth study.* New York: Columbia University Press.

Ross, L., & Nisbett, R. E. (1991). *The person and the situation: Perspectives of social psychology.* New York: McGraw-Hill.

Ross, M. (1989). Relation of implicit theories to the construction of personal histories. *Psychological Review, 96,* 341–357.

Schlenker, B. R. (1974). Social psychology and science. *Journal of Personality and Social Psychology, 29,* 1–15.

Schulz, R. (1976). Effects of control and predictability on the physical and psychological well-being of the institutionalized aged. *Journal of Personality and Social Psychology, 33,* 563–573.

Sherif, M. (1936). *The psychology of social norms*. New York: Harper & Brothers.

Steele, C. M. (1997). Race and the schooling of Black Americans. In L. A. Peplau & S. E. Taylor (Eds.), *Sociocultural perspectives in social psychology: Current readings* (pp. 359–371). Upper Saddle River, NJ: Prentice Hall.

Steele, C. M., & Aronson, J. (1995). Stereotype threat and the intellectual test performance of African-Americans. *Journal of Personality and Social Psychology, 69,* 797–811.

Valins, S., & Nisbett, R. E. (1972). Attribution processes in the development and treatment of emotional disorders. In E. E. Jones, D. E. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, & B. Weiner (Eds.), *Attribution: Perceiving the causes of behavior* (pp. 137–150). Hillsdale, NJ: Lawrence Erlbaum Associates, Inc.

Wilson, T. D. (2002). *Strangers to ourselves: Discovering the adaptive unconscious*. Cambridge, MA: Harvard University Press.

Wilson, T. D., Damiani, M., & Shelton, N. (2002). Improving the academic performance of college students with brief attributional interventions. In J. Aronson (Ed.), *Improving academic achievement: Impact of psychological factors on education* (pp. 88–108). San Diego, CA: Academic.

Wilson, T. D., Houston, C. E., & Meyers, J. M. (1998). Choose your poison: Effects of lay beliefs about mental processes on attitude change. *Social Cognition, 16,* 114–132.

Wilson, T. D., & Linville, P. W. (1982). Improving the academic performance of college freshmen: Attribution therapy revisited. *Journal of Personality and Social Psychology, 42,* 367–376.

Zanna, M. P., & Cooper, J. (1974). Dissonance and the pill: An attribution approach to studying the arousal properties of dissonance. *Journal of Personality and Social Psychology, 29,* 703–709.