



APA CENTENNIAL  
FEATURE

## A Glance Back at a Quarter Century of Social Psychology

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This article, an expansion of remarks originally made on the occasion of the 25th anniversary of the Society of Experimental Social Psychology in October of 1990, presents a selective and informal retrospective on the rapid growth of social psychology from a discipline uncertain of its identity and future 25 years ago to the robust field it is today. The entry of women into the field, the cumulative nature of the growth of social psychological theory and research, as well as the expansion of social psychology's basic and applied research boundaries are highlighted. It is concluded that the potential promised by social psychology just a quarter of a century ago has been realized despite a harsh funding environment. Hope is expressed for more adequate funding in the near future for basic social psychological research and theory development.

Twenty-five years ago, social psychology was experiencing growing pains. Hollander (1968) related that the size of APA's Division 8, now the Society of Personality and Social Psychology, had become so large that the convention program had reached "intimidating dimensions" and that "personal contact and communication [had become] unwieldy." Hence, the establishment of the Society of Experimental Social Psychology (SESP) in 1965, which the organizers envisioned as including "a relatively small number of social psychologists whose interests were primarily research-oriented," believing that "the common focus and smaller size of such a group [beginning with about 50 members and growing slowly to a limit of 100] would allow more flexible organization and would permit the group to engage in more intimate and informal dialogue than is possible at the conventions of the larger associations" (p. 280). Those who have attended an SESP conference recently and have had to shoehorn themselves into one of the meeting rooms (the "intimate and informal dialogue," where all attendees sat in one room and talked about a common problem of interest, having

disappeared years ago) will conclude that the original vision for the society failed miserably in the execution. In retrospect, however, it is clear that, rather than creating a cozy atmosphere in which social psychologists could interact, it was the idea of bringing together social psychologists with a "common focus," specifically, a *research* focus, that was to play an important role in transforming social psychology from a gangly adolescent afflicted with growing pains and facing an uncertain future to the robust and mature discipline it is today. But I am getting ahead of myself.

The remarkable evolution social psychology has undergone over the past quarter century encompasses so many changes in the field's form and content, in the number and nature of its contributors, as well as in the context in which it is embedded, both in psychology and in society, that which of these changes can be singled out for notice in a brief, informal retrospective is necessarily a very personal and impressionistic matter. The comments that follow, which highlight only three of the many changes the field has undergone in the past few decades, meet none of the historian's claims. I hope only to give the flavor of social psychology's evolution over the past 25 years, a period which is clearly demarcated for me, for it was in 1965 that I received my doctorate in social psychology from the University of Minnesota, where I have remained ever since.

What happened to me after I received my degree at Minnesota, and what likely would happen to me today, illustrates with a single brushstroke many of the changes to which social psychologists in my age cohort have been witness. Like many female graduate students of the day, I had expected to retire from academic life after receiving my degree; women were rarely admitted into the academy and none of the three institutions at

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The original occasion for these remarks, expanded here, was the plenary session of the 25th anniversary conference of the Society of Experimental Social Psychology (Buffalo, New York, October 13, 1990) entitled "Experimental Social Psychology: A Twenty-Five Year Perspective." The helpful comments of my colleagues, Gene Borgida and Mark Snyder, University of Minnesota, Elaine Hatfield, University of Hawaii, and Harry Reis, University of Rochester, are gratefully acknowledged.

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which I had received my undergraduate and graduate training (including the University of Nevada where I earned a master's degree with Paul Secord) had one female professor on their psychology faculty, although there were a few women "adjunct" professors. The Minnesota psychology department, I was told, once had a woman faculty member but relation of that fact was often followed by a somber pause and then, in sotto voce, the phrase "but it just didn't work out." Unexpectedly, however, my retirement plans were shelved when I was offered a temporary but open-ended, nontenure track assistant professorship in the Department of Marketing of Minnesota's business school. There, I quickly became known as "the lady professor" (as in "Good morning, lady professor!"), being the only female professor in the business school. The student body, both graduate and undergraduate, was overwhelmingly if not exclusively male (I did not set eyes on, and never taught, a single female student there), and, befitting my place in the general scheme of things, these young men called me either Mrs. Berscheid or "Blondie" (as in "Hey, Blondie, what'd I get on the test?"). I look back on my business school job with nostalgia, for it was my good fortune, no doubt the result of my being a temporary alien, that I was left almost entirely alone. I was not invited to attend department meetings, for example, or to serve on college and university committees, or asked to do any of those things for discipline, God, and country that young professors are required to do today, sapping vast quantities of their time and their energy.

Unfortunately, the beginning of the end of my halcyon days at the business school came about a year later when, waiting for the elevator one day, I casually surveyed the business school's employment bulletin board and noticed an advertisement for a "market research analyst" at the company where I briefly had held such a position. I was dismayed to read that the only "qualification" for the job, entered on the appropriate blank on the business school's standard form, was "male." I removed the ad from the board and wrote a letter to the company pleading (politely, I thought) for reconsideration of the gender qualification, copying the business school's employment bureau. Moreover, I wrote letters to my Senators, at that time Eugene McCarthy and Hubert Humphrey, telling them that it had become apparent to me that women did not have the same opportunities as men did in the workplace and asking what, if anything, they were doing about it. I then forgot about the whole episode, having discharged my duties as I saw them, and so I was truly surprised when, a week or so later, I received a curt summons to appear "immediately" in the office of the associate dean. There, I found an agitated man who, venting his anger with the coarsest of language, told me that my actions had jeopardized scholarships and other monies donated to the school by the company in question and that he intended to bring charges against me. When I asked him what the charge was going to be, he replied, "It is against the law to mutilate University property, and by removing that ad, you mutilated our employment bulletin board."

Fortunately, several things happened to allow me to leave the business school quietly, and each event reflected the times: First, it turned out that the reason the associate dean, rather than the dean, had handled the matter was because the dean had been in Chicago that week attending a national conference addressed to the topic "The Place of Women in Business." That

was heartening because it at least assumed that women had a place, although it apparently was taking a great deal of deliberation to figure out exactly where that place might be. Second, several weeks later, a group of black students who were short-cutting through the building lobby (there were no black students at the school either) spied an ad on the employment bulletin board that, under qualifications, listed "Caucasian." Their response was to douse the board with gasoline and set it afire, thereby leaving a rather large black hole where the employment bulletin board used to be for the remainder of the academic year. Third, the senior business school faculty rallied in my support and called a special meeting where impassioned speeches were made decrying the vulgar language and ungentlemanly behavior of the associate dean toward their lady professor and demanding that he apologize. No one mentioned the advertisement. Although an apology was subsequently made, when Elaine Hatfield left her research position with the Student Activities Bureau in the office of the Dean of Students to take a "real" psychology professorship at the University of Rochester, she arranged for me to be offered her job in the Bureau and I accepted it, believing it to be a safer foxhole than the business school. (For one thing, the Dean was a personal friend of Leon Festinger's, who had been Elaine Hatfield's doctoral adviser as well as Elliot Aronson's, who had been my own adviser.) A year or so later, I, too, was able to join a psychology department, when John G. Darley, the powerful and wily chair of the Minnesota department, called a faculty meeting for a Saturday morning after a violent snow storm during spring break and made a motion that I be hired; when he later informed me that I was going to be transferred to the psychology department, Darley professed himself to have been "terribly pleased" that his motion had received the "unanimous" support of all who had attended the faculty meeting.

As all this suggests, in 1965 few people, including myself, and few institutions were aware that the word *sex* had been inserted at the last minute into Title VII of the 1964 Civil Rights Act. Indeed, it may have been the case that even some members of Congress were yet to become aware of what they had voted for, as neither of the replies I received from Senators McCarthy and Humphrey made reference to that piece of legislation, although each man personally assured me that he was laboring mightily to improve working conditions for women. (Because one of the letters was addressed to "Dr. Allen Berscheid" and began "Dear Sir," I wasn't entirely convinced). The word *race* had been noticed, of course, being the focus of the legislation, but as the business school's employment bulletin board reflected, most employers were still conducting business as usual. Martin Luther King's freedom marches, however, were keeping the issue in the headlines and on our minds. In fact, Elaine Hatfield had the wit to send Rev. King copies of our studies (e.g., "When Does a Harm-Doer Compensate a Victim?" [Berscheid & Walster, 1967]), in the hope that he could make some use of them. To our surprise, he wrote back to express his appreciation for the information, commenting that he was sure that there were many other studies in the social science literature that would be helpful to him if only he knew where they were. In 1965, that was wishful thinking on his part.

But that was then. Now, and just as I was preparing these remarks, my colleague, Gene Borgida, dropped on my desk a

preprint of an article (that has since appeared in the *American Psychologist*; Fiske, Bersoff, Borgida, Deaux, & Heilman, 1991) entitled "Social Science Research on Trial: The Use of Sex Stereotyping Research in *Price Waterhouse v. Hopkins*." As I picked it up and read how social psychological research on the antecedent conditions, indicators, consequences, and remedies of stereotyping played a crucial role at each stage of Hopkins's litigation, a lengthy judicial process that included a Supreme Court decision and ended with Hopkins winning her case of sex discrimination, I unabashedly admit that my eyes dampened with pride in social psychology. Twenty-five years ago, few of us could have imagined that there soon would be an army of social psychologists who would give the problem of stereotypes a full-court press in theory and research and that well within our lifetime, the fruits of our own discipline would be used to strike a landmark legal blow against sex discrimination.

### Women in Social Psychology

In my personal view, then, one of the biggest changes that has taken place in the past 25 years has been the increase in the number of women social psychologists and the dramatic improvement in our working conditions (improvement but not yet equality, according to Brush, 1991). In fact, my guess is that the proportional increase of women into research positions in social psychology was greater than in any other subarea of psychology. (Unfortunately, the APA does not have the appropriate statistics, identifying researchers and nonresearchers, that would allow documentation of this point.)

There are several possible reasons for the influx of women into social psychology, but one that should be noted on an anniversary occasion is that the men who were influential in social psychology in 1965—and they were all men, as the identities of the founding fathers of SESP reflects—were far more egalitarian in outlook and values than those in any other area of psychology. The overlap in membership between Divisions 8 (the Society of Personality and Social Psychology) and 9 (the Society for the Psychological Study of Social Issues) of the APA, as well as the pervasive influence of Kurt Lewin, his students, and such other important early social psychologists as Gordon Allport would suggest that women and minorities could expect a warmer reception in social psychology than in other domains of psychology. And, for the most part, we did. Many of these men are still alive and active today, and their extracurricular contribution to social psychology through training their own female students and through the other professional roles they played at the time should be acknowledged. In addition to those who trained us and gave us jobs, one also immediately thinks of such people as Bill McGuire, then editor of *JPSP*, and Bob Krauss, then editor of the *Journal of Experimental Social Psychology*, who made special efforts to include women in the research review and editorial enterprise, as well as the rapidity (1967) with which SESP put women on its program. It is important to note that these efforts to encourage women to join the mainstream of social psychology were made long before such actions were regarded as socially chic, politically correct, or legally mandated. In short, the "culture" of social psychology in 1965 was well ahead of its time.

It can be argued that the relatively rapid entry of women into

social psychology had a number of salutary consequences for the development of the field. Perhaps the most important of these was to keep the caliber of talent high while social psychology was undergoing enormous growth. By allowing the other half of the human race to participate in the enterprise—a half equal to the other in what we graduate students used to call "raw *g*"—the intellectual talent devoted to social psychological problems was not diluted in quality as it expanded in quantity, as appears to have been the case in certain other subareas of psychology that shall remain nameless here but whose graduate student applicants' Graduate Record Examination scores and GPA records at Minnesota over the years tell the tale.

Second, the influx of women into social psychology influenced the approach taken to many traditional research questions in the field. In this regard, it should be noted that because women were admitted into the mainstream and thus worked on research questions central to the discipline, there has been less "ghettoization" of women in social psychology than there has been in many other disciplines. Rather than an alternative and "feminist" view of social psychology, one that offers an opposing view of the discipline's dominant knowledge domain, there has evolved, by and large, a single social psychology that has integrated, and has been enriched by, the different experiences and views that female social psychologists have brought to their work. Because examples of such enrichment abound, they perhaps are unnecessary, but one spontaneously remembers the sighs of recognition that greeted the Deaux and Emswiller (1974) article, whose subtitle, "What is Skill for the Male is Luck for the Female," said it all for many of us. The work of Alice Eagly and her colleagues also quickly comes to mind, for when I began teaching in 1965, it was a "fact," duly reported in the social psychology texts of the day, that women were more influenceable (read "gullible," "childlike," and "uninformed") than men, a finding that seemed to fit nicely into a constellation of data said to document the submissive and dependent nature of women. Those of us using those texts could only caution our students that not all women were easily influenceable (namely, their very own instructor) and once again drag out our all-purpose and over-used example of Golda Meir—the then Prime Minister of Israel and first female political leader of a major country in modern times, whom the daily news was revealing to be no docile "Mrs. Nice Guy"—as an illustration of the hazards of generalizing to the individual case. Sistrunk and McDavid (1971), of course, dealt the first empirical blow to the idea of women's innate influenceability, but it was Eagly's (e.g., Eagly, 1978) work that buried it.

That work, in fact, turned up a subsidiary finding with important implications for the development of the social and behavioral sciences, both then and now. Pursuing the question of women's special influenceability with the then-new technique of meta-analysis, Eagly and Carli (1981) found an association between the sex of the researcher and the outcome of the experiment, such that both male and female researchers were more likely to find results favorable to their own sex. A tendency to produce findings favorable to groups intimately associated with the researcher's own identity, through unintentional and as yet unidentified mechanisms, argues that diversification of the researcher population—apart from moral, legal, and human resource utilization considerations—serves an important scien-

tific goal: Diversification protects against unintended and unidentified bias in any knowledge domain that purports to be applicable to all humans.

Ironically, the concern today seems to be that the discipline of psychology is tilting again but in the other direction. According to the APA newsletter *Advancing the Public Interest* (J. M. Jones, 1991), the profession of psychology currently consists of 60% men and 40% women. That number is likely to reach parity soon, for in 1989 56% of the graduates with doctoral degrees in psychology were women. Curiously, this movement toward gender parity has been popularly termed, not the *demasculinization* nor even the *degenderization* of our discipline but, rather, the *feminization of psychology*. However, in terms of the knowledge domain that we disseminate to the world—and this surely is where it counts—it is questionable whether psychology will be feminized soon. The critical issue concerns the extent to which the current 60:40 ratio holds in the research arena where psychological knowledge is produced. Only a small fraction of those who receive doctorates ever contribute to the knowledge base of their discipline, and, according to the National Research Council's recent report (Gerstein, Luce, Smelser, & Sperlich, 1988), this figure is not only lower for the social and behavioral sciences than it is for other science and engineering doctorates, but also "even at its highest point, campus strength in behavioral and social sciences research was well short of what one might expect on the basis of the numbers of trained personnel available" (p. 205). My guess is that the percentage of men in the population of researchers in psychology at the present time is higher than 60%. Supporting that hypothesis are figures cited by Bernadine Healy (1992), the new director of the National Institutes of Health. Although women's share of research grant money from the National Institutes of Health has doubled since 1981, and women's success rates for competing research grants is now equal to men's, women submitted and received only 19% of these awards, accounting for "a mere 16% of funds for research project grants" (1992, p. B5).<sup>1</sup>

Women, accounting as they do for half of humankind, constituted the biggest and most obvious boulder to be moved on the road to diversification, but we have become more sophisticated over the past few decades about what true diversification means. And there is yet no reason for celebration. With respect to ethnic minorities, for example, E. E. Jones (1990) recently reported that ethnic minorities account only for about 3%–4% of APA membership and only about 8% of new doctorates. When one considers that these figures include African-Americans, native Americans, Hispanics, and Asian-Americans—people who, collectively, soon will comprise one third of the population in this country—it is clear that no subarea of psychology can claim it has a diversified research arm.

### Increase in the Status and Centrality of Social Psychology Within Psychology

Rivaling in importance the increase in the number of women within social psychology over the past quarter century has been the increase in the status and centrality of social psychology within psychology. As Zimbardo (1992) recently observed, social psychology was "long relegated to a subordinate position within psychology's status hierarchy" (p. xiv), a delicate way of

saying that back in 1965, and for many years after, social psychologists were the lowest of the low. When I went off to the business school, social psychologists were having a tough time in departments of psychology. The reigning prima dons were the "experimentalists" in learning psychology, easily recognized as they flapped through the halls in their white lab coats stained with rat urine and pigeon droppings. Searching for universal laws of behavior that would span millions of years of evolutionary time, from earthworms to *Homo sapiens*, and often using precise mathematical models to represent their hypotheses and findings (many of which later turned out to be much ado about not very much of enduring interest), the experimental psychologists, one must admit, were doing a fine job of imitating their acknowledged betters in the "hard" sciences, especially their much admired colleagues in classical physics (most of whom, ironically, were already dead in 1965 or in a deep funk and paralyzed into inactivity by the epistemological conundrums posed by the new physics; see, for example, Capra, 1982).

It was out from the wings and onto this stage, where the experimental psychologists were busily performing their classical scientific ballet, that the new social psychologists came clomping in their concrete overshoes. With live humans as our subjects and complex social phenomena as our focus, some of our early attempts to join in the dance were ungraceful at best and downright ludicrous at worst. One thinks, for example, of George Homans's (1961) treatise, *Social Behavior: Its Elementary Forms*, which, while it proved valuable to social psychology for other reasons, was a textbook illustration of the popular game played in most social psychology parlors of the day: "Let's Find the Reinforcer!" (of this or that social behavior). Unfortunately, the dominant learning theories of those times had been developed primarily with animals missing an upper cortex, and their raw application to humans in social situations often had a rather surreal quality to them (e.g., one thinks back to those experiments in which bright college students, treated like rats, would look up puzzled each time they heard a penny come rattling down the chute before them as they performed their laboratory task). But we, too, wanted to be real scientists. And so, looking to our superiors for guidance, we spent time doing things we might not have done had we had more confidence in our unique mission. I recall, for example, the months we spent debating the perfect equation to represent such social phenomena as interpersonal equity (e.g., Walster, Berscheid, & Walster,

<sup>1</sup> Some data relevant to the hypothesis that men are overrepresented in the researcher population was provided by a special breakdown I requested of the APA research office on the 16,194 persons who completed work activity forms in the 1989 APA Directory Survey and 1990–1991 new member updates: Of those who indicated that they performed at least some research activities, over 66% were men. Only those who identified their major field as developmental psychology came close to gender parity, with 44% male researchers (this was also the only area in which there was a gender reversal). The figure for social psychology was 69% male. A better index may be authorship of articles in archival journals: for *JSPS* and *Personality and Social Psychology Bulletin* in the years 1989 and 1990, and eliminating the 39 authors whose gender was "hard to tell" by virtue of first name alone, 63% of the 1,263 authors appear to have been men.

1973), and none of us will ever forget the meticulous precision of the “law of interpersonal attraction” (e.g., Byrne, 1971; Byrne & Nelson, 1965).

Despite our efforts to ape our betters in the world of psychology, and no doubt sometimes because of them, social psychologists were frequently the objects of laughter and derision; we were regarded as soft headed and sloppy, an embarrassment, in fact, to “serious” psychologists. No one was immune. For example, even though the Laboratory for Research in Social Relations at Minnesota was among the first and most prestigious training grounds for social psychologists, with the likes of Leon Festinger, Stanley Schachter, Hal Kelley, and Elliot Aronson as psychology faculty in residence in its early years, when we left the lab to attend our psychology classes, we frequently heard social psychology ridiculed from the lectern. As an assistant professor, in fact, the first question the then-president of the local American Association of University Professors (AAUP) asked when we were introduced was “Why do you social psychologists take the abuse?” Embarrassed that word of our pariah status had seeped out of Elliott Hall into the wider world, I retorted, “Because tomorrow belongs to us!” The bravado of that reply owed as much to the fact that I had seen the movie *Cabaret* as it did to my faith in the future, for at that time we social psychologists were haunted by dark nights of the soul and afflicted with wrenching “crises of confidence” (e.g., McGuire, 1973). Now, from the distance of 25 years and a cool look back at the hostile context in which social psychology was developing, it seems no wonder that we were frequently driven to contemplate our collective navel and to question whether we had a place in the scientific universe.

But it wasn't just our hostile academic environment that bedeviled us. We had most of the problems any new field has. We suffered from an identity problem, for example. Though still very much apparent in 1965, that problem at least was beginning to abate. In his review of the three new social psychology texts that had just blossomed (the original Roger Brown, 1965, text; the Secord & Backman, 1964, text; and a text by Newcomb, Turner, & Converse, 1965), Brewster Smith (1966) observed that earlier social psychology texts, reflecting the marginal status of social psychology between the disciplines of psychology and sociology, had devoted much space to the competing claims of each discipline and to trying to resolve, unsuccessfully, the conflicts between them. But Smith could now report that

None of the [current] books pays attention to the earlier quarrels of sociology and psychology over the lineage and legitimacy of their offspring. Indeed, for two of them (Brown and Secord-Backman), the field that they survey or sample is to be defined only by academic and scientific convention: what social psychologists have been curious and busy about. (p. 110)

These three books broke a 13-year hiatus in social psychology texts (with only the Krech, Crutchfield, & Ballachey, 1962, revision of the 1948 Krech and Crutchfield text appearing in this time). The Brown and the Secord and Backman texts, along with the E. E. Jones and Gerard (1967) classic that was to appear shortly, were signal landmarks in social psychology for several reasons, but especially because they gave shape and direction to the field. They not only finessed questions of general-

ogy, but also their surveys of “what social psychologists have been curious and busy about” turned out to be mostly what psychological social psychologists were curious and busy about. Moreover, they systematically infused those doings with heavy doses of theory and findings from psychology proper, making useful interpretations and translations to social behavior (e.g., from object perception to social perception). In addition, by this time there were enough concrete findings to report that the empirical quality of the field was emerging clearly, putting armchair philosophizing in retreat. In this regard, and given the blizzards of findings that social psychologists today routinely attempt to assimilate, it is amusing to read Brewster Smith's plaint back in 1966:

For myself, I am proud of the very real gains in the course of the thirteen years since my last comparative review, but I am not too happy about the clogged state of our journals, filled with the products of project-supported busyness, in which fad and fashion, methodological fetishism, and what I remember Gordon Allport to have called ‘itsy-bitsy empiricism’ make it easy to lose direction and significance. (p. 117)

In bypassing questions of identity, and in moving social psychology toward its psychological and Lewinian heritage, the appearance of these texts also was a godsend to those of us who had to fend off perennial attempts to eject us (all two of us at Minnesota, until 1976) from the psychology department; around budget time, we came to expect that someone would come up with the wonderful idea of creating two new line items for the psychology department by making us the wards of the sociology department and ask us if we didn't think it was a wonderful idea, too. In writing our annual “we shall not be moved” statement, the psychological content and orientation of these texts, which we used in our social psychology classes but which the sociological social psychologists did not, were invaluable.<sup>2</sup>

Apart from who we were and where we belonged in the acad-

<sup>2</sup> In commenting on this article, Sidney Rosen, an observer of the fortunes of social psychologists within sociology over the past 25 years, noted that Herbert Simon addressed a sociological convention some years ago in which he offered a translation of Homans's (1961) propositions into a set of mathematical equations and wondered whatever happened to them, a rather telling question. Rosen then made the following fascinating points: “We might add the observation of a cultural lag distinguishing sociology and psychology. One relevant aspect of this lag was that theoretical sociology started to become attracted to formalization at about the time that such a preoccupation began to lose favor in psychology. Another relevant aspect of this lag is that sociological social psychology is still struggling to establish its identity within sociology. In this regard, Felson wrote the following, in the Winter 1992 newsletter of the social psychology section of ASA: ‘Why did the status of social psychology in sociology decline in the last 20 years: I believe that one reason is ideological: social psychology is not politically correct’ (p. 3). Suffice it to say here that he meant political correctness from the perspective of traditional sociology.” It should also be mentioned that sociological social psychologists are still represented in SESP, although their proportion to the total membership has declined radically over the past 25 years, and that sociological social psychologists as well as sociologists concerned with marriage and the family currently are core contributors to the emerging interdisciplinary science of interpersonal relationships.

emy, serious questions and allegations were frequently raised about the value of our activities. I shall mention only one of these charges and that because, first, it struck at the heart of our claim to scientific status; second, because we were relatively defenseless against it 25 years ago when the field was young in age and few in number; and third, because it retained its currency long after it should have. Although the allegation was made by many, it perhaps was stated most persuasively and succinctly by my Minnesota colleague, Paul Meehl (1978):

I consider it unnecessary to persuade you that most so-called "theories" in the soft areas of psychology (clinical, counseling, social, personality, community, and school psychology) are scientifically unimpressive and technologically worthless. . . . In the developed sciences, theories tend either to become widely accepted and built into the larger edifice of well-tested human knowledge or else they suffer destruction in the face of recalcitrant facts and are abandoned. . . . But in fields like personology and social psychology, this seems not to happen. There is a period of enthusiasm about a new theory, a period of attempted application to several fact domains, a period of disillusionment as the negative data come in, a growing bafflement about inconsistent and unexplicable empirical results, multiple resort to ad hoc excuses, and then finally people just sort of lose interest in the thing and pursue other endeavors. . . . It is simply a sad fact that in soft psychology theories rise and decline, come and go, more as a function of baffled boredom than anything else; and the enterprise shows a disturbing absence of that *cumulative* character that is so impressive in disciplines like astronomy, molecular biology, and genetics. (pp. 806–807)

Today, the charge of noncumulativeness against social psychology has a musty odor to it. In his article, "How Hard is Hard Science, How Soft is Soft Science? The Empirical Cumulativeness of Research," Hedges (1987) distinguished between *theoretical cumulativeness* and *empirical cumulativeness*; the latter being defined as the "degree of agreement among replicated experiments or the degree to which related experimental results fit into a simple pattern that makes conceptual sense" (p. 443). Comparing the consistency of research results in physics and in psychology using a sample of reviews of empirical findings from each domain, Hedges found that the results of physical experiments were not strikingly more consistent than those of social or behavioral experiments and, thus, that "the 'obvious' conclusion that the results of physical science experiments are more cumulative than those of social science experiments does not have much empirical support" (p. 443). For social psychology in particular, there is specific evidence of the empirical cumulativeness of the field. In their analysis of "Publication Trends in *JSP*: A Three-Decade Review," Reis and Stiller (1992) reported that, since 1968, published articles have become progressively longer, they present more procedural information and tables, they cite more prior literature, they report research based on more studies, they use more subjects per study, and they use more complex statistical methods. These changes, the authors persuasively argued, reflect social psychologists' focus on increasingly complex theoretical issues as well as the field's demand for higher and higher standards of evidence as it has matured.

As Hedges (1987) observed, the assessment of empirical cumulativeness, although possessing the virtue of some objectivity, is also a narrower index of the cumulativeness of a discipline than is its *theoretical cumulativeness*, or the degree to which the

field's "empirical laws and theoretical structures build on one another so that later developments extend and unify earlier work" (p. 443). Subjective though such an assessment must be, the evidence that can be mustered for the theoretical cumulativeness of social psychology in the past quarter century could easily pass the eyeball test of social psychology's severest critic.

In 1965 social psychology was already theory rich, but it remained to be seen whether these theories would provide the muscle and sinew the field needed to develop. Festinger (1954, 1957) had offered his theory of social comparison processes a decade earlier (in 1954), and his theory of cognitive dissonance (in 1957) was already turning the field's attention away from "groupy" phenomena (see Steiner, 1974) to matters that today would fall under the general rubric of "social cognition." Heider (1958) had already published *The Psychology of Interpersonal Relations*, which elaborated his balance theory (sketched over a decade earlier in his hard-to-read 1946 article "Attitudes and Cognitive Organization" that had purportedly influenced Festinger's concept of cognitive dissonance, although Asch, 1946, also had started people thinking about consistency as a principle of cognitive organization). In this seminal work, Heider also discussed his observation that people often try to attribute causes to events, and E. E. Jones and Davis (1965) had already begun to flesh out attribution theory. Thibaut and Kelley (1959) already had published the first version of their theory of interdependence; Homans (1961) had presented his idea of "distributive justice" in social relationships, which shortly was to be elaborated in the equity theories; Newcomb (1956) already had drawn attention to problems in the prediction of interpersonal attraction in his APA presidential address and had recently published his study of *The Acquaintance Process* (Newcomb, 1961); and Schachter (1959) had presented both *The Psychology of Affiliation* and his article "The Interaction of Cognitive and Physiological Determinants of Emotional State" (Schachter, 1964). Moreover, Asch's (1946) empirical studies of conformity phenomena and of social perception were well-known, and the "Yale school's" work on attitude change (e.g., Hovland, Janis, & Kelley, 1953) had been around for a decade, with its "incentive motivation" view currently dueling with dissonance theory on the pages of the journals. There was much more, of course, but suffice it to say that, today, the names and the content of these theories will not strike even an undergraduate in social psychology as unfamiliar. Although some of these theories and the findings they produced may have been baffling from time to time, they were never boring, and they were never wholly abandoned. All were to prove to be vital building blocks for later theorists and investigators. And all have remained alive in the sense that they have been revised frequently in response to new findings—or they have been incorporated into other theories—or the findings they spawned have remained important in themselves or have played important roles in further theory development.

Perhaps the most impressive example of the cumulative nature of social psychology lies in the attribution area. From the theoretical outlines originally sketched by Heider (1958) to E. E. Jones and Davis's (1965) formulation to Kelley's (1967) rendering of "Attribution Theory in Social Psychology" two years later, the attributionists have patiently and systematically pursued their phenomena along a very long and winding road.

A powerful chronicle of attribution theory and research over the past 25 years is presented in E. E. Jones's (1990) book, *Person Perception*. If social psychology is ever again required to defend itself against the charge of noncumulativeness, submission into evidence of this book alone would get the prosecution laughed out of court. And the attributionists aren't done yet; in fact, the best may be just around the bend, for two new and highly integrative theories of person perception recently have been offered, one by Susan Fiske and her colleagues (e.g., Fiske & Neuberg, 1990) and the other by Marilynn Brewer (1988).

Not only have social psychologists not been faddish about their theories, they haven't been flighty in their selection of the social behaviors they've sought to understand. In addition to the previously mentioned work on stereotypes and prejudice, begun in social psychology's infancy with the work of Gordon Allport and Kurt Lewin, and which now constitutes a theoretically impressive and practically useful body of knowledge (e.g., see Hamilton, 1981) that continues to be the subject of much current research (e.g., Swim, Borgida, Maruyama, & Myers, 1989), social comparison is still an active research area (e.g., Wheeler & Miyake, 1992), with its fruits extended over the years to illuminate other social phenomena of interest (e.g., Tesser, Millar, & Moore, 1988). Interpersonal attraction, that rockpile of theory and research on which some of us labored as graduate students, now helps form the core of the burgeoning interpersonal relationships wing of social psychology, where the cumulative and interdisciplinary nature of the work performed in this area over the past 3 decades was traced recently by George Levinger (1990) in his address to the International Society for the Study of Personal Relationships at Oxford University. Progress in this area can be illustrated by the fact that at the University of Minnesota we have graduated from Dr. Gregor Zilstein's (a.k.a. Stanley Schachter) frightening coeds with the prospect of electric shock, from Elaine Hatfield's designing computer dances for the Student Activities Bureau's freshman orientation week, and from Elliot Aronson engineering pratfalls for other-wise competent people, to the construction of a free-standing doctoral minor in Interpersonal Relationships Research. This new program will join the forces of scholars in psychology, the Institute of Child Development, family social science, sociology, that old business school (now spiffily named the Carlson School of Management), several colleges and departments in the health sciences, and more to train graduate students and facilitate research on interpersonal relationships. As this illustrates, social psychologists not only have burrowed ever more deeply into the social phenomena that were of interest 25 years ago, but another quality of our discipline has been revealed as it's matured: its boundary-spanning nature.

Social psychologists have expanded their knowledge domain in virtually every direction. Surveying the thousands of books submitted to *Contemporary Psychology* for review consideration these past several years, for example, we were continually surprised by the number of areas in which social psychologists are currently contributing theory and research. Reflecting this state of affairs, social psychology now often finds itself hyphenated to reflect its alliances with other subareas of psychology: social-developmental, social-clinical, social-personality, social psychology and law, social-health, social-organizational, social-educational, social-environmental, and social-community, for

examples. Few subareas of psychology interface with and inform so many other scholarly endeavors within psychology as well as in those disciplines located on psychology's perimeter.

It has become apparent, in fact, that social psychology has emerged as a central pivot for much of contemporary psychology. In this regard, it is interesting to note that even those prognosticators of the future of psychology who see it vanishing as a discipline, with many of its current internal domains being absorbed by other disciplines, do not foresee such a fate for social psychology. Scott (1991), for example, who subscribes to the notion that psychology as we know it will disintegrate, predicted that

Social psychology will continue to expand its strong experimental base, and will increasingly fulfill its promise to address society's most vexing problems. The solutions that emerge from social psychology laboratories will inform gender and racial issues and permeate the workplace, the inner city, and the home. Social psychology will become more practice oriented, affiliating with or creating its own professional schools . . . (p. 976)

If Scott is correct, it may fall to social psychologists to carry psychology's banner into the 21st century.

In sum, contemporary social psychology, with its dynamic, ever-changing and -expanding, character, is an exemplar of all the social and behavioral sciences as they have been characterized by the National Research Council:

Taking into consideration the dynamics of specialization, the development of data, theoretical shifts, and interdisciplinary activity—and the interactions of all of these with one another—the behavioral and social sciences resemble not so much a map as a kaleidoscope, with continuous growth, shifting boundaries, and new emphases and highlights (Adams, Smelser, & Treiman, 1982, p. 26).

Dynamic. And cumulative. Could anyone have asked more of social psychology 25 years ago?

### If We're So Smart, Why Aren't We Rich?

Not everything has improved over the past 25 years. We still do not have an epistemology well suited to our endeavors (e.g., see Berscheid, 1986), which makes people uneasy when they think about it, which isn't very often anymore. Good minds have addressed the painful issues here over the years (e.g., Gergen, 1973; Harre & Secord, 1973) but with little practical effect and resolution. Fortunately, the field's momentum is such that we routinely walk on water in the faith that what we are doing is useful and important; it is only when we look down, and are reminded that the philosophy of science that supported classical physics cannot support us, do we sometimes sink and suffer yet another crisis of confidence.

There are other problems, but the most worrisome one, which brings me to the third and last change I wish to highlight, is the decrease in social psychology's funding per capita researcher. In 1965, the social and behavioral sciences were enjoying the "golden age" of research funding, and social psychology shared in the good times. Today, the amount of time talented researchers, both young and old, spend writing grant proposals only to be told that their ideas—though meritorious—must wither for lack of funds, is disheartening. A great many facts

and figures have been published documenting our inadequate research support environment, so there is no need to repeat the dreary tale here, except to say that changes in support for all of the behavioral and social sciences have been "starkly different" (as the situation is characterized in Appendix A, "Trends in Support for Research in the Behavioral and Social Sciences," of the National Research Council's 1988 report) from those of other scientific disciplines, whose federal funding increased substantially over the past 15 years.

Ever pragmatic and fleet of foot, however, social psychologists have coped. And it perhaps is not too Panglossian to say that they have done so in ways that have protected the discipline and even strengthened it along several dimensions. As Zimbardo (1992) observed, "social psychologists have become the vanguard of the movement to extend the boundaries of traditional psychology into realms vital to contributing solutions for real-world problems, the areas of health, ecology, education, law, peace and conflict resolution, and much more" (p. xiv). As funding for basic research became more and more scarce, and funding for very specific social problems—such as alcohol and drug abuse, acquired immunodeficiency syndrome (AIDS), aging, and so forth—became available, social psychologists increasingly took their theories and their methodological tools out of the laboratory and into the world of pressing societal problems. In doing so, they have broken down the wall between basic and applied research that was apparent 25 years ago, when basic and applied research were seen as parallel, rather than intertwined, research tracks. During the recent funding drought, social psychologists discovered anew the truth of Lewin's dictum that "there is nothing so practical as a good theory!" and have reunited the two faces of social psychology's research coin. We must continue to hope, however, that those who successfully use the tools and theory of social psychology in their applied enterprises never forget their way back home and the necessity for putting something back in the basic theory and research pot during the hard times of inadequate support for the discipline.

Having acknowledged some of the benefits of a harsh environment for the evolutionary development of social psychology, it must also be recognized that in addition to inadequate support for basic research and theory development, funding inadequacies have affected the field in other troubling ways. One of these is our inability to take advantage of the technological marvels that have become available over the past few decades to facilitate our research. A look at our journals today reveals that, just as it was back in 1965, social psychology is very much a pencil-and-paper enterprise. It may be even more of a pencil-and-paper enterprise today than it was then, for the use of self-report questionnaires and written treatments of independent variables seems to have increased; running subjects in large groups and handing out questionnaires is much cheaper, of course, than setting up elaborate treatment scenarios possessing high internal validity and spending one or two hours per subject to collect a single datum, and it is also much less expensive than going out into naturalistic settings to observe behavior.

One of the many unfortunate consequences of our being a pencil-and-paper field is that to those observers who still confuse technology with science—and there are many, both within the academy and without—social psychology still doesn't look

like much of a science. In this, however, we are not much different from many of the other social and behavioral sciences. As the National Research Council (Gerstein et al., 1988) observed, "There is a persisting view that behavioral and social sciences research can operate as a virtually equipment-free enterprise, a view that is completely out of date for research in many areas" (p. 214). Needless to say, social psychology is very prominently one of those areas. Even equipment that has been shown to be useful to the exploration of many different social phenomena (e.g., physiological and facial measures of affect) are not routinely available in social psychological laboratories. Moreover, unobtrusive devices to measure important social psychological variables outside of the laboratory, with extremely reactive subjects who range freely over large habitats, still appear only in our dreams. The inability of researchers in social psychology to take advantage of the many technological developments that would facilitate our empirical research and broaden our theoretical horizons currently constitutes a particularly frustrating stumbling block as other, better funded, disciplines increasingly capitalize from rapid technological advances. For now, we can only pray that the lead mines fueling our pencils don't run dry before our funding prospects improve.

In this regard, there now is reason to hope, and hope for the future is always a good place to end a look at the past. As all who keep up with the events that affect the fortunes of psychology now know, a separate directorate for the behavioral and social science disciplines is being formed at the National Science Foundation (e.g., NSF Directorate: Yes!, 1991), the agency social psychologists look to for most of our basic research support. That reorganization, according to knowledgeable observers, should increase funding for all of the social and behavioral sciences and, on that score alone, social psychology's boat should lift along with the others. But beyond that general effect, we surely have reason to be confident that even the most jaundiced observer of social psychology's development and record of contribution over the past 25 years must conclude that social psychology has been faithful to its promise to the society that supports it and that society thus has a vested interest in keeping social psychologists as busy and as curious over the next quarter century as they've been over the last.

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