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# Personality Psychology's Comeback and Its Emerging Symbiosis With Social Psychology

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*Psychology's early allegiance to behaviorism and experimental methods led many to disparage personality approaches throughout much of last century. Doubts about personality psychology's viability culminated in Mischel's assertion that measures of personality account for modest amounts of variance in behavior. In the years immediately following this critique, interest in personality research waned and many psychology departments dropped their training programs in personality. Throughout the past two decades, however, personality psychology has enjoyed a resurgence. The authors discuss several possible explanations for personality's comeback and then describe the emergence of a promising symbiosis between personality psychology and its sister discipline, social psychology. The article concludes by noting that although this emerging symbiosis is likely to continue bearing considerable theoretical fruit, the traditional distinction between personal, situational, and interactional determinants of behavior continues to be useful within appropriate contexts.*

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Several years ago, an undergraduate student wandered into the first author's office wearing a mystified frown on her face. The student explained that after much soul searching, she had decided that she wanted to pursue a graduate degree in personality psychology. When she looked for potential grad programs, however, she was disappointed to learn that relatively few training programs in personality psychology existed. Her first response was to develop second thoughts about the viability of her chosen specialization. Later, however, these doubts changed to concerns about the perspicacity of the field of psychology: "Psychologists recognize how fundamental personality is, right? If so, then why has the field of psychology banished personality psychology while related areas like social psychology have continued to thrive?"

The first author responded by attempting to place the fate of personality psychology in historical perspective. He began by considering the factors that caused personality psychology to lose force within psychology and showed how these factors laid the groundwork for a later critique of personality by Walter Mischel (1968). He noted that this critique was something of a bitter pill for personality psychologists; although it diminished interest in personality psychology in the short run, it inspired a core of dedicated psychologists to press on and make important discoveries. Specifically, these researchers unraveled a host of subtle and complex issues that qualified and, in the eyes of many, effectively refuted the notion that personality was unimportant. Their efforts, together with changes within the social psychological community, brought personality psychology back into the psychological mainstream.

This article represents an elaboration of the conversation between the student and first author. After discussing the history of skepticism toward personality psychology, we document its fall and subsequent rise by examining trends in three decades of published research articles, graduate training programs, and dissertation research. We then consider two distinct explanations of personality psychology's comeback. First, psychologists may have been persuaded by thoughtful rebuttals to

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Mischel's critique and refinements to personality research. Second, a process of self-recrimination may have persuaded social psychologists to conclude that had Mischel's (1968) alter ego drawn a series of conceptually analogous arrows from his quiver and aimed them at social psychology, he would just as easily have found his mark. Both of these sets of considerations may have enhanced the relative attractiveness of personality psychology. In the concluding section, we suggest that personality psychology's comeback is a positive development in that it has set the stage for a symbiosis with social psychology that will enrich both subdisciplines. We begin by identifying some key movements that shaped psychology in the last century, particularly behaviorism and related themes.

#### THE LEGACY OF BEHAVIORISM

The behaviorist John Watson (1925) once boasted that he could fashion people into whatever he desired—regardless of their unique qualities and personalities. A decade or so later Kurt Lewin (widely regarded as the father of experimental social psychology) challenged this one-sided view of psychology. In his field theory, Lewin argued that behavior was a function of both persons and situations (Lewin, 1946). Nevertheless, Lewin's interactionist message was largely lost on his extremely influential student, Leon Festinger. For Festinger, the Person in Lewin's widely heralded Behavior =  $f$  (Person  $\times$  Situation) deserved scrutiny only *in the service of* understanding the influence of the Situation; personality variables were merely "error variance" and of relatively little intrinsic interest. His viewpoints helped shape the perspectives of an entire generation of social psychologists, particularly those trained in the 1950s.

Although behaviorism gradually yielded to more cognitive approaches in the 1960s, it was still a force to be reckoned with when Mischel (1968) wrote his critique of personality psychology. Indeed, Mischel himself advocated one of behaviorism's intellectual children—social learning theory—as an alternative to traditional personality approaches. After doing so, he went on to evaluate the predictive power of extant measures of personality,

In sum, the data reviewed on the utility of psychometrically measured traits . . . show that responses have not served very usefully as indirect signs of states and traits. . . . With the possible exception of intelligence, highly generalized behavioral consistencies have not been demonstrated and the concept of personality traits as broad response predispositions is thus untenable. . . . The initial assumptions of trait-state theory were logical, inherently plausible, and also consistent with common sense and intuitive impressions about personality. Their

real limitation turned out to be empirical—they simply have not been supported adequately. (pp. 145-147)

At first blush, Mischel's (1968) conclusions hardly seem to be the stuff of which knockout punches are made. Yet his reference to conventional personality approaches in the past tense seemed to some to imply that it was time to consign them to the fate of phlogiston, ether, and the four humors.

#### THE IMPACT OF MISCHEL'S CRITIQUE ON PERSONALITY PSYCHOLOGY

The publication of Mischel's (1968) book was followed by a marked decline in the number of research studies, graduate training programs, and dissertations devoted to personality psychology. We consider each of these consequences in turn.

##### *Research After Mischel's Critique*

To determine if the landscape of published personality and social psychological research changed following Mischel's criticisms, we surveyed the past 35 years of articles in one of the flagship journals of personality and social psychology: the *Journal of Personality and Social Psychology* (*JPSP*). We chose the *JPSP* for several reasons. First, of the journals that publish research on both social and personality psychology, it is arguably the most representative; indeed, empirical evidence (Sherman, Buddie, Dragan, End, & Finney, 1999) indicates that publication trends in other influential journals (e.g., *Personality and Social Psychology Bulletin*) resemble trends in *JPSP*. Second, it is unique among the most prominent journals in that its publication history extends back to 1965, thus permitting us to establish baseline publication rates prior to the appearance of Mischel's book. Finally, *JPSP* is consistently ranked as having the highest impact score of the journals in social-personality psychology, making it both a reflection of and an influence on the most important research in the field (Reis & Stiller, 1992).

Several interesting findings emerged from our analysis.<sup>1</sup> Figure 1 shows the average percentage of articles in each issue of *JPSP* containing any reference to individual differences (i.e., individual difference, Experiment  $\times$  Individual Difference, or experiment with incidental measure of individual differences) versus those with no reference to individual differences. In 1966, 50% of the articles included at least one individual difference measure, but this figure subsequently drops precipitously.<sup>2</sup> By 1977, studies of individual differences have dipped to their lowest point, significantly lower than any year except 1972 ( $p < .001$ ).<sup>3</sup> The data thus suggest that the publication of Mischel's book was followed by a rise in

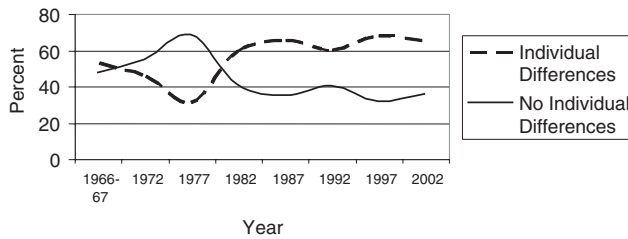


Figure 1 Percentage of articles including individual differences.

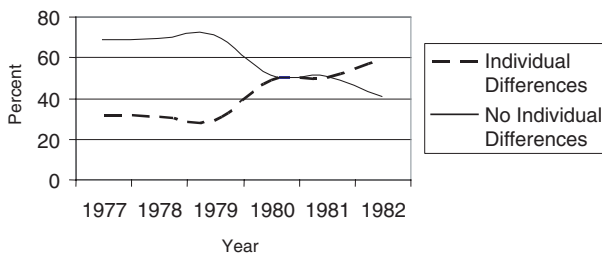


Figure 2 Change in individual difference research before and after *Journal of Personality and Social Psychology* (JPSP) reorganization.

experimentation at the expense of studies of individual differences. After 1977, however, researchers began returning to personality psychology almost as quickly as they had abandoned it.

Two distinct mechanisms may have underlied personality psychology's recovery: (a) researchers themselves decided that it was time to return to the study of personality or (b) the creation of a separate section of *JPSP* ("Personality and Individual Differences") in 1980 facilitated the publication of personality articles. To assess the relative viability of these two possibilities, our judges examined in more detail the years surrounding the reorganization of *JPSP* into its current three-section format. Figure 2 shows the percentage of studies including individual differences between 1977 and 1982. The data suggest that the Publication Board's decision to reorganize *JPSP* into the three-section format had a dramatic impact, nearly doubling the number of individual difference articles. Nevertheless, the fact that the number of articles devoted to personality psychology or individual differences research continues to increase after 1980 (a significant upward trend,  $p < .001$ ,  $\beta = .459$ ) indicates that interest in personality psychology continued to surge after *JPSP* was divided into three sections.

Whatever its cause, less than 15 years after the publication of Mischel's (1968) remarks the study of individual

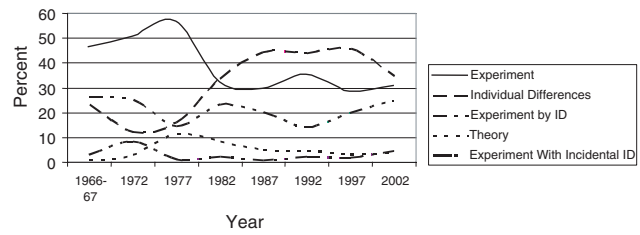


Figure 3 Percentage of articles by type.

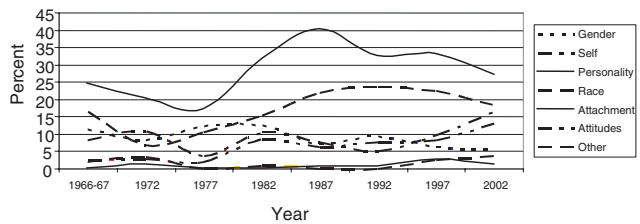


Figure 4 Percentage of articles by individual difference studied.

differences had rebounded to levels slightly above the 1966-1967 baseline. A linear regression with orthogonal polynomial components revealed a significant cubic component ( $p < .005$ ) qualifying a significant linear relationship ( $p = .001$ ), suggesting a dip-and-rebound pattern plus an overall increase over time (see also West, Newsom, & Fenaughty, 1992).

Figure 3 displays changes in each of the major categories of articles over the past three decades. Between 1967 and 1977 there was a sudden dip in studies of "pure" individual differences (those which looked solely at nonmanipulated individual differences, with no experimental manipulation) and an equally sudden rebound between 1977 and 1987 (likely also influenced by the reorganization of *JPSP*). Studies of the interaction between individual differences and experimental manipulations also dipped in the 1967-1977 period, although the dip-then-rebound pattern was less dramatic than that associated with pure individual differences.

Although Mischel's (1968) critique focused on personality variables alone, the results plotted in Figure 4 hint that it also diminished interest in other individual difference approaches. That is, in the 1970s, there was a decline in articles devoted to several distinct types of individual difference variables, such as nonmanipulated attitudes and "other" individual differences.<sup>4</sup>

Overall, these data show that there was a dramatic shift toward experimentation in the early 1970s followed

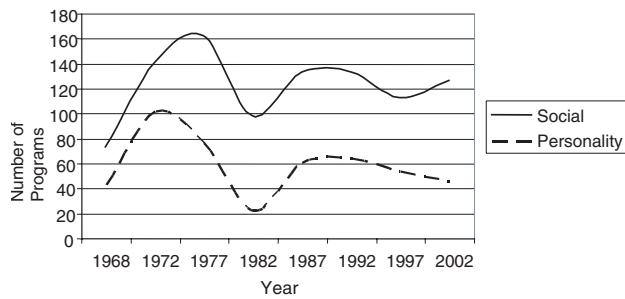


Figure 5 Number of programs in personality and social psychology.

by an equally dramatic shift back toward personality approaches. The rebound of interest in the study of personality differences was sufficiently strong that today, interest in individual differences is somewhat stronger than it was in 1968.

#### *Graduate Training Programs in Personality in the Wake of Mischel's Critique*

In the years following the publication of Mischel's book, several premier personality programs were terminated (e.g., Harvard, UCLA), others were substantially reduced in size or changed in orientation (e.g., Michigan, Illinois), and many others were assimilated into social programs (e.g., Berkeley, Texas). To document this hypothesized shift in graduate education programs, we consulted the American Psychological Association's (APA's) *Manual for Graduate Study in Psychology*. Figure 5 displays the number of programs describing themselves as having a "strong focus" in personality versus social psychology. Both personality and social programs increased markedly between 1968 and 1972. After 1972, the patterns for the two subdisciplines diverge somewhat. Personality underwent a precipitous decline after 1972, followed by a resurgence that began a decade later and then leveled off in the 1990s. Social psychology continued to grow until the late 1970s, then experienced a decline until 1982 (perhaps triggered by the "crisis" discussed below), followed by a resurgence and then a short-lived dip in the mid-1990s. Comparing 1968 levels with 2002, social psychology has enjoyed a substantial increase, whereas personality psychology is roughly where it began (although there have been shifts in the location of the most prominent programs).<sup>5</sup>

#### *Dissertations Focusing on Personality Versus Social Psychology*

To determine the impact of Mischel's critique on the number of dissertations conducted in personality and

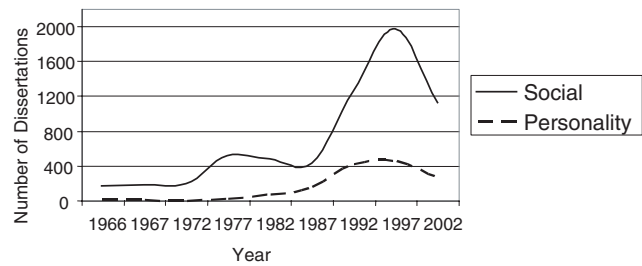


Figure 6 Number of dissertations returned for keywords "personality" and "social psychology."

social psychology, we examined the *Dissertation Abstracts International* online catalogue. Figure 6 shows the number of dissertations in the personality and social psychology subject areas that were returned for keyword searches of "personality" and "social psychology." Although scarcely visible in the figure, the number of dissertations focusing on personality underwent a similar pattern of drop-and-rebound as seen in publications in *JPSP*. That is, dissertations classified as focusing on personality dropped from 14 (7% of dissertations) in 1966 to only 3 (1.5%) in 1972. There is a rebound, however, such that the study of personality exceeded baseline levels by the 1980s. By 1984, the editors of *Dissertation Abstracts International* recognized the growth of personality psychology by granting personality its own section of the volume.

In contrast, the number of dissertations focusing on social psychology appears to have enjoyed a steady increase over the past three decades (there has been a recent drop in the number of dissertations devoted to social psychology, but we suspect that this is attributable to the remarkable spike in social dissertations in 1997). Comparison of the relative standing of social and personality in 1966 and 2002 reveals that social has gained more than personality.

*Summary of findings.* Our analyses of empirical research, graduate programs, and dissertations display similar patterns. In all three cases, a sharp decrease in personality psychology in the early 1970s was followed by a gradual return to levels at, or slightly exceeding, baseline levels. Today, more than 50% of the research published in *JPSP* includes some measure of personality. Also, examination of the programs and dissertations devoted to personality psychology indicates that personality psychology has regained the ground it lost in the early 1970s. In what follows, we consider factors that may have precipitated personality's comeback.



WHY PERSONALITY PSYCHOLOGY  
REGAINED THE GROUND IT HAD LOST

The resurgence of personality psychology is a large-scale sociological phenomenon. Like any such phenomenon, it is apt to have many causes that are difficult to document and impossible to quantify. Nevertheless, we have identified two distinct classes of hypotheses that might explain the resurgence of personality psychology: (a) increased awareness of personality's strengths resulting from a series of vigorous rebuttals of Mischel's critique coupled with scrupulous attention to methodological issues and (b) increased awareness of experimentation's limitations brought about by the "crisis in social psychology." We discuss each of these below.

*Increased Awareness of Personality Psychology's  
Strengths Relative to Its Shortcomings*

Although Mischel (1968) was surely the most influential critic of personality psychology, he was by no means the first (e.g., Ichheiser, 1943) or the last (e.g., Nisbett, 1980). These critics have raised two distinct issues: (a) relative to situational influences, personality accounts for minimal variance in behavior and (b) correlational approaches are methodologically weak. In what follows, we discuss how personality psychologists have responded to each of these concerns.

*Rebuttals of the "Personality Accounts for  
Minimal Variance in Behavior" Argument*

Perhaps the most positive contribution of Mischel's book was that it inspired psychologists to carefully and creatively dissect the assertion that personality variables account for relatively little variance in behavior relative to situations. Although there were many important contributors to this effort, David Funder and his colleagues led the charge (see especially, Funder, 1999, 2001, 2002; Funder & Colvin, 1991; Funder & Ozer, 1983; Kenrick & Funder, 1988). Together, they identified five key qualifiers to Mischel's critique.<sup>6</sup>

1. The "if personality effects are weak then situational effects are strong" syllogism is flawed. Evidence indicating that personality measures seldom correlated with behavior at levels that exceeded .30 does not imply that situations must account for the remainder of the variance because the unexplained variance could be due to trait-situation interactions, unmeasured traits, or error. In fact, when Bowers (1973) reviewed several representative studies and compared the magnitude of the effects associated with situations, persons, and their interaction, he reported that interactions generally "won." A few years after his original book, Mischel (1973, 1984) modified his approach in favor of a more interactionist approach.

2. The pool of studies Mischel reviewed and the particular studies he selected for consideration may have been biased. For a sample of experiments to offer valid estimates of the relative effects of persons and situations, the reviewer who scrutinizes the sample must be unbiased in selecting representative studies and the pool of studies must itself be unbiased. Regarding the unbiased selection of studies requirement, some (e.g., Block, 1977; Hogan, DeSoto, & Solano, 1977) have argued that Mischel selected studies that favored the influence of situations over personality variables. Regarding the unbiased pool of studies requirement, the pool of relevant studies may be biased in favor of situational effects because (a) social psychologists are more apt to design and conduct experiments than personality psychologists and (b) most social psychologists are well versed in designing situational manipulations and unschooled in the science of test construction, so they may therefore choose manipulations of situations that are exceptionally strong and measures of traits that are exceptionally weak.
3. Due to differences in the way situational versus personality influences are assessed statistically, evidence that situations influence behavior may have no bearing on the magnitude of the effects of personality. Whereas situationists generally examine the impact of variations in the situation on behavior, personality psychologists often examine the relation between people's scores on traits and behavior. The results generated by these two approaches are conceptually and statistically independent (barring ceiling or floor effects, cf. Funder, 2001). For example, although it is certainly true that the events of 9/11 increased the average level of anxiety among Americans (a tendency for a situation to influence people's average responses), it is surely also true that dispositionally anxious persons were more fearful than their relatively easygoing neighbors both before and after 9/11 (a tendency for rank order on a personality dimension to covary with the responses of individuals). Thus, the fact that situational pressures can systematically produce large changes in average behaviors does not negate the influence of personality variables (cf. Mischel, 1984).
4. When situations and persons are entered into a "horse-race" in which the goal is to predict behavior, situations do no better than persons in predicting behavior. When Funder and Ozer (1983) transformed the effect sizes of several landmark experimental studies (e.g., Darley & Batson, 1973; Darley & Latané, 1968; Festinger & Carlsmith, 1959; Milgram, 1965) into  $r$  values, they discovered that they ranged from .34 to .42—the same range within which Nisbett (1980) placed personality effects. Also, Kenny, Mohr, and Levesque (2001) used Kenny's (1994) Social Relations Model (SRM) technique to compare directly the magnitude of the person, situation, and interaction effects and reported that person (i.e., trait) effects were more substantial than situational or interaction effects. Finally, in a recent meta-analysis of 25,000 studies, Richard, Bond, and Stokes-Zoota (2003) reported virtual equivalence in the effect sizes associated with situation effects ( $\bar{r} = .22$ ) versus person effects ( $\bar{r} = .19$ ) (for similar findings, see Sarason, Smith, & Diener, 1975). Apparently, situations account for no more variance than persons.

5. A tendency for an experimental manipulation to account for more of the variance in behavior than an individual difference measure does not necessarily mean that the situation was more responsible for causing the behavior because situational and dispositional explanations are often interchangeable (Gilbert, 1998). Consider the propensity for some teachers in Milgram's obedience studies to relentlessly shock learners. Although this phenomenon has routinely been attributed to the power of the situation (e.g., Ross & Nisbett, 1991), Kreuger and Funder (2004) note that it is equally valid to describe it in terms of the interplay between dispositional tendencies to obey authority and dispositional tendencies toward compassion (cf. J. C. Wright & Mischel, 1987). In addition, there is emerging evidence that the situational pressures people encounter are systematically shaped by their personalities (e.g., Buss, 1987; Snyder & Ickes, 1985) and self-views (Swann, Rentfrow, & Guinn, 2002). Through random assignment, experiments short-circuit these processes through which people choose and shape situations (Wachtel, 1973). As a result, experiments systematically underestimate the influence that individual differences have on behavior in naturally occurring situations.

In hindsight, the foregoing rebuttals of the "personality accounts for minimal variance in behavior" critique seem so compelling that it is difficult to understand how this critique could ever capsize an entire subdiscipline of psychology. We believe, however, that it is *only* in hindsight that the fragility of this argument is apparent; indeed, it took many years of thoughtful conceptual analysis and systematic research to fully grasp the many subtle and complex issues that bubbled beneath the surface of Mischel's seemingly straightforward remarks. From this vantage point, it is somewhat ironic (albeit understandable) that some personality psychologists continue to harbor misgivings about Mischel's commentary. That is, his comments played an instrumental role in inspiring personality psychologists to generate some exceptionally penetrating insights into the interplay of persons and situations (for an excellent overview, see Snyder & Ickes, 1985).

*Implementation of the Construct-Validation Fix to the "Correlational Approaches Are Methodologically Inferior" Critique*

For social psychologists, skepticism regarding the potential contribution of personality approaches ran high due to a belief that relative to experiments, correlational approaches were methodologically weak or "messy." In particular, many felt that there was no way of knowing whether personality tests were actually related to the constructs under scrutiny. Doubts about the meaningfulness of scores on personality measures have a

long history. In fact, APA's Committee on Psychological Tests (1950-1954) identified an urgent need to develop a strategy for determining whether psychological tests measured what they were purported to measure. Meehl met this need by developing a systematic procedure for validating scales, a procedure that capitalized on experimental methods to test theoretically derived propositions about the relation between scores and overt behaviors. Cronbach and Meehl (1954) fashioned these ideas into a clearly delineated procedure for establishing the construct validity of psychological tests.

If it did nothing else, Mischel's (1968) critique dramatized the importance of rigorous adherence to principles of construct validation. To a greater degree, personality psychologists came to embrace construct validation as the gold standard to which new measures of personality were held. This process enabled psychologists to attain considerable confidence, not only that the validated test did indeed measure what it was designed to measure but also that the construct under scrutiny was distinct from rival constructs. And such high levels of methodological rigor have paid off. For example, recent estimates indicate that the validity of psychological tests is comparable to that of medical tests (Meyer et al., 2001). Such increased attentiveness to the construct validation process surely increased the attractiveness of personality approaches to workers in the field.

*Increased Awareness of Experimentation's Limitations*

During the 1970s, social psychology was of two minds when it came to experimentation. As shown in Figures 1 through 3, early in the decade there was a marked increase in experimentation relative to individual differences approaches. Even as researchers turned increasingly to experimental approaches, however, the field underwent a wrenching "crisis" that was marked by considerable doubts regarding the utility of experimentation (see Jones, 1998, for a discussion). Questions arose regarding both the internal as well as external validity of experiments. Together, these questions may have diminished experimentation's appeal in much the same way that Mischel's critique sucked the wind from personality psychology's sails several years earlier.

*Difficulties in Establishing the Internal Validity of Experiments*

In theory, experiments can provide higher levels of confidence regarding the causal mechanisms underlying phenomena than correlational approaches. What the crisis forced social psychologists to realize, how-

ever, was that the results of experiments are not always as illuminating as they could be in principle. Although not among the instigators of the crisis, Aronson and Carlsmith (1968) articulated a central concern,

How can we be sure that this operation is, in fact, an empirical realization of our conceptual variable? Or conversely, how can we abstract a conceptual variable from our procedures? . . . There is no cheap solution to the problem. This is largely due to the fact that in social psychology there exist relatively few standard methods of manipulating any given conceptual variable. (pp. 14-15)

But the problem goes much deeper than this. Consider the common assumption that one advantage of experimental designs over nonexperimental designs is that, through randomization, researchers eliminate the “omitted variable” problem (i.e., the possibility that some variable other than the one that was manipulated was responsible for an effect). This is not true of social psychological experimentation or, for that matter, any other type of experimentation. Bollen (1980) notes that “in any experiment we must be aware that variables other than the intended one may be influenced by the treatment and that these other variables could be responsible for the effects found” (p. 76).

In theory, the omitted variable problem could be addressed by a procedure for validating manipulations—parallel to the construct validation process designed by Cronbach and Meehl (1954) for psychological tests. Although Brunswik and his students (e.g., 1944, 1947; for a overview, see Hammond, 1980) attempted to address this issue through the development of his “representative design” and “rich stimulus” approaches, these approaches have garnered little support by social psychologists. Perhaps the most serious effort to address the validity issue has been the use of “manipulation checks.” Nevertheless, on those infrequent occasions wherein manipulation checks are used and reported, they are themselves of unknown validity. Moreover, if the results of manipulation checks fail to converge with the primary findings of the study, investigators often assume that the manipulation check was faulty or that the effects of the manipulation were simply inaccessible to participants (e.g., Nisbett & Wilson, 1977). And even if valid manipulation checks were used routinely, they would not fully address the omitted variable problem because they are only capable of revealing what the manipulation *did* do; they cannot provide assurances that the manipulations’ effects were limited to those intended.

Although the omitted variable problem is not fatal, recognition of this and related problems did dampen

some of the enthusiasm enjoyed by advocates of experimental approaches. This problem was further compounded by the recognition of experimentation’s problems with external validity.

#### *Difficulties in Establishing the External Validity of Experiments*

Until the early 1970s, social psychologists were far more concerned with issues related to internal validity than external validity. Instructors of methods classes justified this imbalance by noting that if an experiment lacks internal validity, questions about external validity are moot. The logic underlying the priority placed on establishing the internal validity of experiments has merit, as experiments lacking in internal validity are indeed uninterpretable. Moreover, there surely are instances in which the researchers’ interests do not extend beyond what occurs within the confines of a narrowly defined laboratory setting. In such instances, “in principle” demonstrations are perfectly legitimate and acceptable outcomes of the research process (e.g., Mook, 1983).

Nevertheless, Gergen (1973) argued that the results of social psychological experiments may not generalize beyond a particular historical epoch (but see Schlenker, 1974). Others argued that an exclusive focus on the internal validity of experiments encourages researchers to focus on the reality of the contexts that they create in their laboratories to the exclusion of the contexts to which they hope to generalize (e.g., Alexander & Knight, 1971). Experimentalists may venture down a path where they can only see failed experiments as flawed designs rather than as examples of mistaken hypotheses. Embedded in their self-generated labyrinths of independent and dependent variables, researchers may be seduced into concluding that the only thing that matters is to somehow make the experiment “work,” to forge whatever connections are needed to induce participants to respond as they are “supposed” to respond. Experiments may thus cease serving as vehicles for making discoveries, becoming instead contrivances commandeered by researchers intent on finding what the “theory” knows to be true (e.g., Ickes, 2003; McGuire, 1973).

Such concerns were surely disillusioning for advocates of experimentation. This disillusionment, coupled with the fact that personality and individual difference based approaches were much stronger in the external validity department (by virtue of the fact that psychological tests could be administered to relatively large, diverse samples), may have led some researchers to shift from experimentation to personality psychology.



*The Emerging Symbiosis of  
Personality and Social Psychology*

Although our emphasis here has been on the comeback of personality and individual differences, we should note that social psychology has continued to thrive while this comeback has occurred. Witness, for example, the substantial gains that social psychology has made throughout the past three decades in number of graduate programs and dissertations. We suspect that such gains have been made, in part, because social psychology has successfully addressed shortcomings it has recognized in itself. For example, social psychologists have warded off threats to internal validity by developing tighter designs and making greater efforts to identify the precise mechanisms triggered by their manipulations. In addition, social psychologists have attempted to buttress the external validity of experiments by doing more field research and devoting more attention to individual differences both within and between cultures. Some (Gillis, 1980) have even embraced Brunswik's (1947) "rich stimulus" approach. We suspect that these trends will accelerate in the future (see also Aronson, Wilson, & Brewer, 1998; Schwarz, Groves, & Shuman, 1998). For example, several investigators have shown that it is feasible to use the Internet to administer personality tests to extremely large international samples (e.g., Gosling, Vazire, Srivastava, & John, 2003; Srivastava, John, Gosling, & Potter, 2003; Swann, Rentfrow, & Gosling, 2003; Vazire & Gosling, 2002). With a little ingenuity, this strategy could be adapted to experimental work, thus broadening the populations sampled by social psychologists.

Increased interplay between social and personality psychology also has produced significant advances. Indeed, we believe that one positive consequence of Mischel's critique was that it motivated a search for personality variables that moderate the impact of situations on behavior (for useful overviews, see Cantor & Kihlstrom, 1981; Funder & Colvin, 1991; Snyder & Ickes, 1985). Mischel and Shoda (1999) have extended this moderator approach by developing a new conceptualization of the nature of personality. They propose that personality consists of a profile of characteristic reactions to situations (if-then propositions) rather than highly general traits. From this perspective, the personality theorist's task is to determine how people will respond in particular contexts rather than knowledge of their characteristic responses across many different contexts.

In a parallel development within social psychology, Swann and his colleagues (Gill & Swann, 2004; Swann, 1984) have conceptualized accuracy in person perception in a similarly contextualized manner. Based on the

assumption that people often negotiate relationship-specific identities, Gill and Swann (2004) have shown that perceivers are particularly good at estimating the behaviors of targets in the local contexts that they share with targets. Furthermore, the accuracy of perceivers in local contexts is especially predictive of the quality of their relationships. The everyday person perceiver, like Mischel and Shoda's (1999) personality theorist, is often concerned with learning how people respond in particular, goal relevant contexts rather than across all contexts (see also Idson & Mischel, 2001).

Yet the full implementation of Mischel and Shoda's (1999) innovative approach clearly requires the development of a comprehensive taxonomy of situations—a development that has been pursued with stunningly modest success since H. Wright and Barker's (1950) early attempt. Cervone (2004) has recently addressed this limitation of the Mischel and Shoda (1999) approach by noting that cross-situationally stable patterns of behavior *do* emerge for qualities that are highly important to people. He has also proposed some specific cognitive mechanisms that theoretically underlie the processes proposed by Mischel and Shoda. We believe that Cervone's extension of the Mischel and Shoda approach represents an important development.

Another prominent theme in recent interactional approaches has been the acknowledgment that people select or create situations that are commensurate with their actual or self-perceived qualities (e.g., Ickes, Snyder, & Garcia, 1997; Magnusson, 2001; Snyder & Ickes, 1985; Swann, 1987; Wachtel, 1973). Systematic consideration of relevant personality influences thus provides insight into the idiosyncratic ways in which people organize and shape the situational influences they encounter. Insight into these processes will, in turn, offer a deeper understanding of the joint and reciprocal influences of persons and situations. Similarly, Nowak, Vallacher, and Zochowski (2002) have argued that personality is shaped by the distinctive social environments people find themselves in, while at the same time personality guides people toward differing social interactions. This reciprocal model of personality and behavior as a nonlinear dynamical system can be considered a modified form of Lewin's  $B = f(PXE)$ , one in which personality and the social environment are intertwined and recursive processes rather than separate vectors.

One can take this line of thinking even further by arguing that the distinction between the influence of persons and situations is inherently arbitrary (e.g., Gilbert, 1998). Higgins (1990), for example, has shown how dispositional and situational influences can be discussed in terms of the common language of knowledge accessibility. From this vantage point, personological variables routinely prompt people to categorize situa-



tions in idiosyncratic ways, whereas situations routinely prime people to perceive certain types of behavior as appropriate. The behaviors that people display are determined by the accessibility of information that derives from their chronic individual differences and contextual primes. He has recently expanded this argument to include other psychological principles as well (Higgins, 2000).

This line of reasoning also illuminates why, at a theoretical level, it is somewhat arbitrary to frame the results of studies such as the Milgram (1965) experiment as a demonstration of the power of the situation (Krueger & Funder, in press). Indeed, Milgram's study could just as easily be framed as a test of the relative strength of participants' tendency to obey legitimate authority, on one hand, or to feel compassion for the screaming victim, on the other. Moreover, obedience in naturally occurring settings is undoubtedly influenced by a tendency for people who have internalized norms for obeying legitimate authority to select or remain in situations in which they are compelled to be obedient. From this vantage point, the "power of the situation" derives not so much from the intrinsic properties of the situation but from the propensity of people acting in various situations to "listen" to what the situation is telling them to do. Situations influence people only when the actors themselves are receptive to such influences.

This line of reasoning also calls for some tweaking of Lewin's vision of the interplay between persons and situations. Although a biologist by training, Lewin's advocacy of the "mathematization" of psychology (e.g., Lewin, 1946) revealed his commitment to metaphors derived from geometry and physics. That is, while endorsing the interactive effects of persons and situations, he assumed that the two influenced behavior in much the same way that the drag and velocity of an airplane influence its course. We suggest instead that a biological metaphor is more appropriate, one in which particular persons are influenced by situations only insofar as persons have receptors that are "tuned" to the situational factor in question. A model that depends only on the relative strength of the dose of virus and the immune system of the infected person will not capture this process. Instead, in the biological metaphor, people acquire viruses only if they have the appropriate receptors to which the virus can attach itself. This metaphor is compatible with many new theories that argue that there is a complex interaction between enduring individual variables and the influence of situations, such as Cervone's KAPA model (Cervone, 2004), Mischel and Shoda's (1999) CAPS model, and goal-feedback models (Carver & Scheier, 2003; Dweck & Leggett, 1988).

These and related ideas suggest that persons and situations should be seen as collaborators—rather than

adversarial forces—in causing behavior. This conclusion parallels a similar one that has been drawn by the protagonists in the perennial battle between advocates of nature and nurture or genetic versus environmental explanations. Despite occasional declarations of victory from one side or the other, an emerging consensus suggests that it is naïve to champion either force as offering a complete explanation for human behavior. Instead, theorists have begun to argue that truth resides in an appreciation for the manner in which these forces interact to determine behavior (e.g., Gottlieb, 2003; Ridley, 2003; Turkheimer, 1991).

In advocating an interactionist approach, we hasten to add that we are suggesting that this approach should *complement* rather than *supplant* traditional approaches to personality and social psychology. That is, although we are convinced that interactionist approaches will usher in a deeper understanding of human behavior, traditional personality and situational approaches continue to provide quite useful information. For example, researchers interested in predicting the rank ordering of individuals within a situation or class of situations will benefit from learning how those individuals score on well-validated measures of personality, particularly if the relevant trait is highly important (e.g., Cervone, 2004). Similarly, those interested in predicting the typical behavior of individuals within a given setting (e.g., guards within a prison) will benefit from learning what that situation is. From this vantage point, interactionism may allow us to transcend the distinction between persons and situations at a philosophical level, but it does not negate the usefulness of embracing this distinction when appropriate.

With such a conceptualization of persons and situations in hand, the key points of contention that have historically divided personality and social psychology melt away. What is left are two subdisciplines united by their commitment to finding answers to the same basic questions. The similarities borne of these common goals far outweigh the differences associated with their unique methods of inquiry. As members of the two subdisciplines begin asking their questions in a single voice, the answers should come more quickly.

#### NOTES

1. To gather these data, two graduate students in personality-social psychology examined the years 1966, 1967, 1972, 1977, 1982, 1987, 1992, 1997, and 2002 and classified all articles that appeared in each year (we examined the consecutive years 1966-1967 to obtain a stable baseline). Articles were first coded into seven categories generated through a preliminary examination of some representative issues of the *Journal of Personality and Social Psychology (JPSP)*: experimental, individual difference, interactional (Experimental  $\times$  Individual Differences), theoretical, meta-analysis, meta-analysis of an individual difference variable, and experiment with incidental individual differences. The latter category included those studies that were conceptualized as

experiments but included individual differences (e.g., gender) in the analysis, presumably to address external validity issues. Several subtypes of individual differences were also identified: gender, self, personality, race, attachment style, attitudes (nonmanipulated), and "other," which included demographic variables such as age or culture and also relatively rare individual difference variables such as birth order.

The coders began by analyzing 2002 and discussed ambiguous cases until consensus was reached. Coders then independently analyzed 1967 without discussing ambiguous cases and a Cohen's Kappa was generated to test reliability. The Kappa was acceptably high (.804). The remainder of the years were counterbalanced by half years and coded independently. If coders identified cases that were difficult to classify, they discussed them until consensus was reached. This was necessary only rarely (less than 3% of the cases).

2. These data are presented as percentages to control for format changes in *JPS* over the years that influenced the number of articles published per issue. However, the raw data show a similar pattern of precipitous drop in personality research in the early 1970s, followed by a steady climb in the 1980s and 1990s.

3. This statistic was generated through an analysis of the articles broken down by issue, which provides enough data points to reliably run an ANOVA by year.

4. Note that these categories are not mutually exclusive in that studies could be categorized as examining up to three major types of differences. Thus, for example, a single study could be classified as looking at age, gender, and personality.

5. We hasten to add two qualifiers to our findings. First, we were unable to identify "pure" personality or social programs because respondents indicated whether they had a focus in either subdiscipline or both but did not indicate joint programs. We suspect that most of the departments that indicated a concentration in personality in recent years also indicated a concentration in social, but we have no way of documenting this impression. Second, we cannot say whether the changes in the number of programs listing themselves as social and personality programs are due to actual reorganizations of the departments concerned or to changing editorial constraints on how programs may choose to list themselves.

6. One legitimate rebuttal to Mischel's (1968) critique that we have not listed here is this: *Most empirical assessments of the effects of personality have not been faithful to the conceptual definition of trait.* The argument is that, by definition, traits are *general* predispositions to respond, so that the most appropriate criterion is a broad sample of behaviors rather than the single criterion variables used in most studies (Epstein, 1979; Hogan, 1998). Consistent with this idea, Epstein and O'Brien (1985) showed that when measures of personality are used to predict aggregated behaviors, prediction coefficients improve dramatically (e.g., from .40 to .60 or higher). Nevertheless, on this point we believe that this argument unnecessarily devalues the usefulness of traits (see also Mischel & Shoda, 1999). That is, often the usefulness of traits is to predict behavior in particular situations and the Epstein position essentially removes traits out of the running.

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