

their role of stabilizing conflict in society. As soon as one generalization that is somewhat more intraceptive in content appears to be influential, it is replaced by another that may appear to encompass a less intraceptive view (e.g., AIDS and sexual life-style; new wave of anti-Semitic doctrine; effects of concept of legal doctrine that dictates diversity in employment on employee morale in society-at-large and even in higher education).

In the final analysis, one would hope that the fine scholarship reflected in Simonton's target article is viewed as more than producing knowledge for its own sake. Exploring the issue of the impact of behavioral generalizations may not only make a contribution in epistemological terms but can present an important challenge to the behavioral sciences. Which of these

generalizations that have evolved over the years has truly been accepted by a skeptical society to the point that such generalizations are taken seriously enough to affect public policy, society, and the individual in a manner that contributes to general societal health? What mechanisms can be developed toward this end? If the behavioral sciences do not encompass such an "activist" stance, and the question of impact is left to other agencies, as is most often the case at present, is this ultimately acceptable?

Note

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Psychology of Science and History of Psychology: Putting Behavioral Generalizations to the Test

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The psychology of science is the youngest sibling in the metascience¹ family and is trying to prove that it has as much to offer as its older siblings—philosophy of science (born circa 1900), history of science (born circa 1920), and sociology of science (born circa 1940). But, as is true for many youngest children, the psychology of science has developed a rather definite inferiority complex. Being the youngest and most vulnerable, psychology of science has indeed been bullied and pushed around by its older siblings, in particular the oldest sibling, philosophy (see discussions by Houts, 1989; Shadish, Houts, Gholson, & Neimeyer, 1989). The elder spokesperson of the philosophy of science, Karl Popper, early on developed a somewhat smug and arrogant view toward the younger metasciences. He even went so far as to accuse all metasciences other than philosophy of being crazy, referring to them as "spurious sciences" and "the sociological (or psychological or historical) lunatic fringe" (Popper, 1970, pp. 57–58). Rather harsh words coming from an older brother!

Very briefly, the psychology of science is the scientific study of the behaviors and thoughts of

scientists (Gholson, Shadish, Neimeyer, & Houts, 1989; Simonton, 1988b). That is, what are the personality, developmental, cognitive, and social psychological processes behind the creation and verification of scientific ideas? It is difficult to pinpoint a date of birth for the psychology of science, but the early 1960s saw a dramatic increase in psychological research on scientists, especially the creative aspects of science (Barron, 1963; Chambers, 1964; Eiduson, 1962; Taylor & Barron, 1963). And, in 1966, Maslow even published a book entitled *The Psychology of Science*. The 1970s, however, saw a decline in such work, and, yet, by the early 1980s, a very clear and growing subset of psychologists began to identify themselves either primarily or secondarily as psychologists of science (Shadish et al., 1989; Simonton, 1988b). The following are just some of the topics examined historically by psychologists of science—creativity, cognitive processes, personality, developmental influences, genius, mentorships, motivation, and social/political influences (see Albert, 1992; Barron, 1963; Busse & Mansfield, 1984; Chambers, 1964; Eiduson, 1962; Feist, 1991, 1993; Gorman, 1992; Roe, 1952; Simonton, 1976b, 1980; Taylor & Barron, 1963; Tweney, Doherty, & Mynatt, 1981).

Because of this recent increase in activity, there are now signs that the more mature metascientific disciplines are taking notice of the psychology of science and even wanting to cooperate on problems rather than

¹Metasciences are disciplines that study science. They are referred to as *meta* because they are generally sciences studying science. Technically speaking, however, they need not be a science (i.e., empirical) but rather can be any systematic study of the nature of science.

hold fast to their “disciplinocentrism”—the belief in the superiority of one’s own discipline and the uselessness of others. For example, the epistemological question of what is scientific knowledge stands to gain much from the recent developments in cognitive science and artificial intelligence (Gorman, 1992; Miller, 1989; Simon, Langley, & Bradshaw, 1981; Tweney, 1989; Tweney et al., 1981). Indeed, a few contemporary philosophers are developing the discipline of “natural epistemology” (Fuller, 1989, 1990; Heyes, 1989), and they very directly acknowledge the value of psychology in addressing metascientific questions. Even though this is the case, there still is the question whether the psychology of science is an established subdiscipline within psychology and the other metasciences (see Gorman, in press). At the 1992 annual convention of the American Psychological Association in Washington, DC, Simonton, Fuller, Gorman, Shadish, and Feist discussed this issue, and the general conclusion was that, although still somewhat marginal, the psychology of science is showing signs of becoming a viable, healthy, independent subdiscipline.

Simonton’s body of work in particular has been instrumental in the development of the psychology of science. As one of the most productive and prolific writers in the field, Simonton has contributed to the topics of scientific eminence, creativity, and genius (Simonton, 1988b); the influence of age and birth order (1988a, 1988b); multiple discoveries and zeitgeist (1979, 1986, 1987); role models (1976a, 1976b); the relation between quantity and quality of publications (1984, 1985, 1988b, 1992); and the influence of war and political upheaval on scientific discovery (1976b, 1980). In addition, he has put forth one of the few comprehensive theories of scientific productivity—the chance-configuration theory (Simonton, 1988b, 1989). With his target article, Simonton continues this tradition. Although he discusses topics such as the origins and historiographical uses of behavioral laws, I focus my comments on the last two sections of his article—namely, the scientific status and prospective status of behavioral generalizations.

Scientific Status of Behavioral Laws: Relation Between Theory and Research

Simonton makes a rather cogent and detailed argument for empirical psychologists of science and other behavioral scientists to take a more active role in gathering behavioral generalizations and putting them to the test. He writes: “Indeed, histories of psychology are a source of important hypotheses that deserve the empirical scrutiny of behavioral scientists in many disciplines besides psychology.” By focusing on behavioral gener-

alizations made by historians and psychologists, Simonton has brought to our attention a very wide and deep pool of testable psychological hypotheses. No longer should there be a paucity of testable hypotheses for the beginning or even experienced researcher of psychology of science. To quote Simonton: “Some of these inductive hypotheses could possibly fuel lifelong programs of metascientific research.”

However, much empirical work that bears directly on assumptions and nomothetic statements made by historians has been ignored. As stated very simply by Simonton, “It is perhaps remarkable that those who write histories of psychology seldom cite empirical findings that bear upon a particular abstract claim.” It may be remarkable, but it is not necessarily all that surprising, for it is just one instance of the ubiquitous disunity between the more theoretical and the more empirical domains of psychology (Staats, 1991). The history of psychology has shown that theory and research have at best an uneasy relation despite the common prescription that they should be closely linked.

There are many reasons for the contentious relation between theory and research in psychology, and I now discuss some of these to draw attention to the fact that, although very sensible and admirable, Simonton’s call for a more mutual relation between historians and empiricists may either fall on deaf ears or even meet with active resistance. The two enterprises are well entrenched in their separate worldviews. However, in the spirit of Simonton’s request, I argue that, until theory and research in psychology become more interactive, our discipline will remain in its undeveloped and somewhat “prescientific” state.

First, there can be little doubt that most American PhD graduate programs in psychology emphasize empirical rigor over, and even at the expense of, theoretical sophistication. For instance, the cognitive psychologist of science, Michael Gorman (1992), described a reviewer’s criticism of a submitted manuscript. One of the chief criticisms was its lack of theoretical perspective. “The referee had a point. My graduate program in psychology had emphasized methodological rigor, somewhat at the expense of theory” (Gorman, 1992, p. 38). My own experience in graduate school at the University of California at Berkeley was similar. For instance, my major thesis advisor (Mendelsohn, 1983) wrote the following in a review of theory-of-personality textbooks: “Personality is at present a lively and evolving field. Very little of what makes it so is embodied in theories that, with few exceptions, are primarily of historical interest and that never had clear-cut empirical roots or consequences” (p. 437). Although I agree to some extent with Mendelsohn’s point, I use it primarily as an example of an attitude I believe is rather pervasive at most major research universities: Empirical rigor is emphasized over theoretical sophistication.

Another reason for the separation of theory and research may lie in the fundamental individual differences between those attracted to quantitative, mechanistic explanations in science compared to those attracted to more qualitative, conceptual explanations (Atwood & Tomkins, 1976; Johnson, Germer, Efran, & Overton, 1988; Stolorow & Atwood, 1979). Johnson et al. (1988) argued that mechanistic worldviews predispose individuals toward “a reductionistic analysis of parts and their functional relations” (p. 828), and, hence, these individuals have more quantitative and empirical orientations compared to those with organismic worldviews. Those with organismic worldviews, on the other hand, are predisposed toward “a synthetic understanding of organized complexity” (Johnson et al., 1988, p. 828) and are therefore likely to be attracted to holistic theoretical explanations. Although a source of uneasiness in the relation between theory and research, the influence of personality on theoretical orientation is another opportunity for psychologists of science to make an important contribution. Simonton’s recommendation that generalizations be examined empirically could be applied very fruitfully to the claims that personality influences theoretical orientation.

One could also argue that a final reason for the uneasy relation between theory and research in psychology may be the relative immaturity of psychology as a discipline. For example, it could be argued that, because physics is such a mature and developed science, theory and experiment can and do mutually benefit from each other. Indeed, one is either an “experimental” physicist or a “theoretical” physicist, and each builds upon the work of the other. After psychology has developed to the point of physics, then it too will have a similar close relation between theory and research.

However, what applies to physics may not apply to psychology. We cannot necessarily apply the same logic to the laws of human behavior that we apply to the laws of physical behavior (Meehl, 1978). More specifically, the biologist Ernst Mayr (1976) discussed why the principles of the physical sciences may not be applicable to the laws of the biological sciences. The chief area of difference between physics and biology—and, by extension, psychology—lies in the uniqueness of biological organisms compared to the relative uniformity of classical physical behavior. Because of the inherent greater individual differences and complexity in human behavior than in physical behavior, the relation between theory and research in psychology may run a different course than it has in physics (see Meehl, 1978, for a detailed discussion of the difficulties inherent in the progress of psychology). In short, we cannot use the history of physics as an example of how the relation between theory and research in psychology

actually will be, even if we can argue that is the way it should be.

Prospective Status of Behavioral Laws: The Value of Testing Generalizations

Simonton also discusses the question of whether behavioral generalizations have any useful function or whether they should be eliminated altogether. His response is clear: Even if one wanted to rid the history of psychology of generalizations, their ubiquity and utility would make this very difficult and unlikely. Rather than “search and destroy,” Simonton argues for empirically testing and examining behavioral laws. He further makes recommendations for how this testing can be done: (a) weed out untestable laws, (b) categorize the testable claims as either everyday or extraordinary propositions, (c) test the everyday claims in traditional research settings (i.e., the laboratory using the student subject pool), and (d) test the extraordinary claims about extraordinary individuals with historiometric or cliometric methods.

Assuming that there are historians and psychologists who would be interested in undertaking such an enterprise, the question remains: Of what value would it be? What advantage is there to empirically scrutinizing claims made by historians of psychology? In order to answer such questions, I must first discuss the question of the criteria for progress and advancement in science.

First, there is general agreement among theorists and researchers that consensus is the key to advancement of any science (Atwood & Tomkins, 1976; Meehl, 1978; Staats, 1991). The only way that consensus can be achieved in the psychology of science is if psychologists begin to pay more attention to overall findings of many studies rather than rely on findings from single, albeit dramatic, studies. Along the same line, meta-analyses and trend analyses must become more common if consensus in psychology is to occur (Feist & Runco, 1993; Rosenthal & Rosnow, 1991). As many have pointed out, continuous measures of effect size rather than dichotomous *p*-values are a step in the right direction toward analyzing consensus or the lack thereof (Cohen, 1988; Folger, 1989; Lykken, 1968; Meehl, 1967; Rosenthal & Rosnow, 1991). Effects in research are best conceptualized as a continuum rather than as a dichotomous “significant–not significant” decision rule that is inherently biased by sample size.

Another cornerstone of the advancement of science—and a critical element to consensus—is replication. In psychology in general and in the psychology of science in particular, replications must become more acceptable and publishable. Again, Gorman’s (1992) personal experiences offer telling insights into the dis-

crepancy between the rhetoric and the reality of publishing scientific articles. Most psychologists readily admit to the importance of replication, but, if one were to try to publish a simple replication, the manuscript would stand a very good chance of being rejected. In Gorman's case, the editor did not even send the manuscript out to be refereed, saying, "Your study essentially only replicates past research efforts" (p. 22). I would speculate that this is not an isolated incident but rather is a common attitude among editors. Let me emphasize that I am not arguing that replication should be more heavily valued than original research. Rather, I am simply arguing that replication needs to become more accepted and more widely practiced in psychology if real progress is to be made. Particular individuals may benefit from continuously reporting "new" findings, but psychology as a whole suffers from fractionated, ahistorical and nonconsensual findings, making real progress very difficult.

Finally, interdisciplinary cross-fertilization is often a source of creative insight and major advancements in science (Barron, 1963; Guilford, 1963; Koestler, 1964; Shadish et al., 1989). In one of the few things Piaget ever wrote about creativity, he argued that cross-fertilization (i.e., knowledge of disciplines outside psychology), combined with an intentional ignorance of one's own literature, is a necessary condition for creative thought (Piaget, 1981; see Shadish, 1989, for similar advice to young scientists). Cross-fertilization and interdisciplinary work must become more common in psychology and the history of psychology if progress is to be made, and the psychology of science is ideally situated to cross-fertilize many of the social sciences and metasciences.

Now I can answer the question of what value would testing behavioral generalizations have. In short, it would provide an opportunity for the psychology of science and psychology in general to advance and progress as a science. More specifically, Simonton's recommendation potentially fulfills each of the criteria for scientific advancement discussed earlier. As long as replication is involved, testing of behavioral laws would ultimately lead to consensus by weeding out those "laws" that are invalid and useless. Furthermore, because the questions involved in behavioral laws come from psychology, history, philosophy, and sociology, interdisciplinary cross-fertilization would be inherent to the enterprise. In sum, the empirical investigation of behavioral laws provides the psychology of science with an opportunity for real advancement as a science. If this occurs, then there will be no question of the status of the psychology of science as an established and codified metascience and sub-discipline within psychology. Science (and its history) is indeed a rich and fertile field in which to test psychological theory, and psychology has much to offer the

study of science that other metasciences do not. If the "youngest sibling" does establish itself, it will be in no small part due to Simonton's continued efforts.

Note

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