

## Psychobiography and the Psychology of Science: Understanding Relations Between the Life and Work of Individual Psychologists

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This paper presents initial interpretive hypotheses about connections between the life and work of a number of eminent psychologists: Sigmund Freud, Karen Horney, Henry Murray, B. F. Skinner, and Paul Meehl. Each of these interpretations can be critically evaluated, revised and improved, leading to incrementally more adequate understanding of individual lives, interacting with advances in psychological theory and research. Psychobiographical studies of individual scientists are a valuable complement to experimental and correlational lines of research in the psychology of science. In the “Science Wars” of the 1990s, there was an apparent conflict between scientists and those in social studies of science. The psychology of science can contribute to this debate, exploring the ways in which scientific inquiry, social-political worlds, and personal-experiential processes construct each other over time.

*Keywords:* psychobiography, psychology of science, history of psychology, personality, biography

The question that I begin with is a deceptively simple one: How important is the personal side of psychological theory? This leads to a second question: How are psychobiographical studies of relations between the life and work of individual psychologists related to the psychology of science?

The discipline of psychology is concerned with at least three different levels of generality: Learning what is true about people in general, about groups of people, and about individual lives (Kluckhohn & Murray, 1948; Runyan, 1982). Similarly, the psychology of science is concerned with learning what is true about scientists in general or populations of scientists (Simonton, 1988, 2002), about groups of scientists (Feist & Gorman, 1998; Maslow, 1966; Roe, 1953a, 1953b), and about the work and lives of individual scientists (Gardner, 1993; Gruber, 1974). This paper presents initial interpretive hypotheses about relations between the life and work of a number of fascinating individual psychologists: Sigmund Freud, Karen

Horney, Henry Murray, B. F. Skinner, and Paul Meehl.

In some cases, it seems there are obvious connections between personal experience and psychological theorizing. In psychoanalysis, Freud’s self-analysis beginning around age 40 after his father died was seen by admirers as a heroic journey of self discovery, which combined with his clinical, scientific, and cultural inquiry led to the achievements of psychoanalytic theory. Critics, however, saw it very differently, more as the start of the problems.

Analyses of personal experience also seem relevant in understanding the work of individuals like C.G. Jung, Alfred Adler, Otto Rank, Karen Horney, Margaret Mead, Erik Erikson, Carl Rogers, Abraham Maslow, Albert Ellis, and a great many others. Understanding relations between life and work can help in understanding the sources and meanings of a theory. I should make clear at the beginning, however, my own view that personal experience can be a source of great insights or great errors, and that identifying personal, social, or cultural sources of a theory does not answer questions about its more general validity.

Several powerful traditions in the history of science deny or minimize the role played by personal factors. An internalist tradition in history of science focusing on the interplay of

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scientific theory and research might see personal-experiential factors as little more than distractions from rigorously scientific inquiry. Externalist traditions analyze science in its social and cultural contexts, ranging from Marxist critiques of science, through the sociology of science, to postmodern social and cultural constructivist views of science, each of which often slight or ignores the personal-psychological dimensions of science.

The psychology of science analyzes the cognitive, emotional, experiential, personal, social, and other psychological dimensions of science. These are not minor issues: "Individuality is found in feeling; and the recesses of feeling, the darker, blinder strata of character, are the only place in the world in which we catch real fact in the making, and directly perceive how events happen and how work is actually done" (James, 1902, in Murray, 1967, p. 293). The "blinder strata of character" is at least one of the places in the world in which we can see facts in the making, along with social, cultural and historical levels of analysis. Science studies can profit from each of them.

### My Personal Journey Through This Deceptively Simple Question

Like any intellectual project, this inquiry into the biographical sources of psychological theory has unfolded in changing social, cultural, and personal contexts, several of which are discussed here. I had long been interested in the study of lives within the social sciences, writing a dissertation on "Life Histories: A Field of Inquiry and a Framework for Intervention" in a program in Clinical Psychology and Public Practice at Harvard in 1975. Parts of this dissertation along with later work were developed into *Life Histories and Psychobiography: Explorations in Theory and Method* (Runyan, 1982). The focus was on describing and interpreting individual lives, with chapters on problems in describing and interpreting individual lives, and on the case study method, idiographic methods, and the psychobiography debate.

In 1988 I started teaching a course on "Personality Theory." In order to better understand the theories, I added more material over time not only on the interplay of theory and empirical research, but also on the sources and uses of the theories, including attention to their biographi-

cal, social, and cultural contexts. I was attempting to write a book on these issues, still in progress, and it dawned on me that this was at least partly a project in the history of science. In order to do a more rigorous job, I wanted to learn about recent developments in the history and philosophy of science. On a sabbatical in the spring of 1994, and a leave in 1995-1997, I spent a good deal of time learning about developments in the history of science at Harvard's History of Science Department, M.I.T.'s Dibner Institute for the History of Science and Technology, and Boston University's Colloquium series in the philosophy and history of science.

These experiences were tremendously thought-provoking, challenging many of my assumptions about what science is and how it fits into the world. Yet, they were also tremendously stressful, in that much recent literature in history and social studies of science explicitly discounted the role of personal, psychological, or experiential factors in science, topics that were of primary interest to me. For example, in the first year graduate seminar on Methods of Research in the History of Science at Harvard in the fall of 1995, one of the instructors said that "Last year's seminar decided that biography is not a useful or appropriate method in the history of science." After some initial shock, I raised my hand, and asked, "What is the argument here?" As far as I could tell, there was not much of an argument, but that social studies of science were valued, with an emphasis on social and cultural dimensions of science. Within this view, talk of biography was lumped with a discredited "Great Man Theory of History." From this perspective, talk of individuals and their psychology was ignoring important developments in the sociology of scientific knowledge and could be seen as intellectually and politically regressive for overemphasizing individuals and neglecting the extent to which science is socially constructed.

An obvious response is that one can pay attention to both. Social studies of science have made major contributions in illuminating social and cultural dimensions of science, and the ways in which science is socially constructed, with attention to political, economic, rhetorical, and material dimensions of science. It may not be easy, but is it possible to integrate this with what is being learned in psychology and the study of individual lives? I will argue that ana-

lyzing relations between the life and work of individual scientists is a valuable component of the psychology of science, a place where the "rubber meets the road," with scientific tasks being performed by particular individuals and groups in particular social, cultural, and historical contexts (Runyan, 1988, 2005a). Examples are from the history of psychology, the scientific field I am most familiar with.

I had gone to the history of science looking for more powerful intellectual instruments and had found an approach to understanding science that was more detailed and sophisticated than what I had previously been exposed to. Yet, at the same time, I felt I had found a severely flawed telescope, bringing the social and cultural dimensions of science into the foreground, yet blurring, or sometimes ignoring the personal-psychological dimensions. The psychology of science can help to bring the personal-psychological dimensions of science back into focus.

#### What Psychology to Include in the Psychology of Science?

If psychology of science is going to be included in science studies, how can this integration be achieved? What kinds of psychological theory, research, and research methods are available for developing the psychology of science?

One valuable resource is *Psychology of Science: Contributions to Metascience* (Gholson, Shadish, Neimeyer & Houts, 1989). This edited collection includes a guide to the literature on psychological epistemology by Donald Campbell. Campbell expresses hope that this volume and the 1985 conference it originated from "will catalyze the critical mass needed to establish psychology of science as a discipline with its own journals, organizations, courses and doctoral programs" (Campbell, 1989, p. 21). As this critical mass may currently be forming, the present paper argues that psychobiographical inquiry into relations between the life and work of individual scientists can be a valuable part of an evolving psychology of science.

Campbell says that origins of his chapter, "Fragments of the fragile history of psychological epistemology and theory of science" result from 45 years of "back burner" attention to these issues (as early as a 1950 lecture on "The psychology of knowledge" at the University of Chicago), and he hoped that the thread will be picked

up by younger scholars. One step in this direction is the *Psychology of Science* volume itself (1989), developing from a 1985 conference at Memphis State (now the University of Memphis). The volume includes chapters by a number of major contributors to the psychology of science, including Dean Simonton, Howard Gruber, William J. McGuire, Ryan Tweney, the four editors of the volume (Gholson, Shadish, Neimeyer, Houts), Donald Campbell, and others.

A further step in the institutionalization of the psychology of science was *The Social Psychology of Science* (Shadish & Fuller, 1994). This book was intended to counter the view that the psychology of science consists solely of the cognitive psychology of science. The book emphasizes contributions of social psychology to the psychology of science. It anthologizes contributions to the social psychology of science, including both psychological and social perspectives, examines conceptual underpinnings, and suggests future directions for the social psychology of science.

A recent contribution to the psychology of science by Greg Feist and Michael Gorman (1998) reviewed work in five different areas of psychology contributing to the psychology of science. This discussion is extended in Feist's *The Psychology of Science and the Origins of the Scientific Mind* (2006). Here there are individual chapters reviewing work in Biological Psychology of Science (Ch. 2), Developmental Psychology of Science (Ch. 3), Cognitive Psychology of Science (Ch. 4), Personality Psychology of Science (Ch. 5), and Social Psychology of Science (Ch. 6). I will not try to summarize all of the arguments here, but those working in the field will want to review it.

I will, however, briefly summarize some of the main points of Chapter 1, in which Feist situates the psychology of science in relation to the three more established disciplines of the history of science, the philosophy of science, and the sociology of science. Modifying earlier work by Nicholas Mullins (1973), Feist argues that disciplines go through three distinct stages of development: Isolation, identification, and institutionalization. In the first stage of isolation, scholars work on problems in isolation, yet without the social organization of training centers, conferences, or professional organizations which all come later. In the second stage of identification, after intellectual achievements by

the founders outline a field of inquiry, students and other scholars identify themselves with the field and may begin to meet with each other and establish journals. Third, in the stage of institutionalization, professional societies are more formally organized, annual conferences are established, and training centers proliferate. Subgroups within a discipline with different interests and agendas may form.

Feist suggests that the history, philosophy, and sociology of science are each well into formal institutionalization, whereas psychology of science is slowly emerging out of the isolation stage. Individual isolated workers are increasingly identifying and communicating with each other, as in the edited volumes referred to above in the psychology of science (Gholson et al., 1989) or in the social psychology of science (Shadish & Fuller, 1994).

In this paper, I examine how understanding the personal lives of psychological theorists and researchers can sometimes shed light on their work. In the last section of the paper, I suggest that life historical and psychobiographical perspectives may also have something to contribute to resolving the conflict between internalist and externalist perspectives in the so-called "science wars."

### The Personal Side of Psychological Theorists: Case Studies

#### *Sigmund Freud*

There is an enormous literature on the relations between Freud's personal biography and his intellectual development, concentrating on his self-analysis, interpretations of his dreams, or his identification with historical figures such as Leonardo daVinci or Moses, starting with Wittels in 1923, through Jones (1953-1957), Ellenberger (1970); Roazen (1975); Sulloway (1979); Gay (1988); Breger (2000); Elms (2005), and many others.

I focus on two brief examples, each controversial or contested in its own way. Part of the story of psychoanalysis is how the theory drew upon Freud's self-analysis, as well as from his clinical work and cultural resources. In two key letters to his friend Wilhelm Fliess, Freud wrote on September 21, 1897, "And now I want to confide in you immediately the great secret that has been slowly dawning on me in the last few

months. I no longer believe in my *neurotica* [theory of the neuroses]" (i.e., no longer believing in childhood sexual seduction as the cause of neuroses). And on October 15, 1897:

Dear Wilhelm,

My self-analysis is in fact the most essential thing I have at present and promises to become of the greatest value to me if it reaches its end. . . . Being totally honest with oneself is a good exercise. A single idea of general value dawned on me. I have found, in my own case too, [the phenomenon of] being in love with my mother and jealous of my father, and I now consider it a universal event in early childhood, even if not so early as in children who have been made hysterical. . . . If this is so, we can understand the gripping power of Oedipus Rex. . . . the Greek legend seizes upon a compulsion which everyone recognizes because he senses its existence within himself. (Freud, quoted by Masson, 1984, p. 272)

This certainly sounds as if Freud's personal experience is being used to support his belief in the Oedipal theory. (The cautious methodologist may be concerned about overgeneralization as Freud moves from his own case to a "universal event in early childhood.") There are, of course, controversies about the extent to which this abandonment of the seduction theory and conception of the Oedipus complex was shaped by his self-analysis, his clinical patients, assumptions about the prevalence of childhood sexual abuse, and/or political expediency (e.g., Breger, 2000; Malcolm, 1984; Masson, 1984).

A second example from Freud's work illustrates some of the difficulties in linking personal experience to the development of theory and also suggests something about the possibilities of critically examining such claims. Freud's first biographer, Fritz Wittels (1880-1950), had suggested in 1923 that Freud's idea of the "death instinct," introduced in *Beyond the Pleasure Principle* (1959/1920) occurred to Freud while "under the impress" of the death of his daughter, Sophie (Wittels, 1923). Freud read the biography and wrote to Wittels on December 18, 1923:

That seems to me most interesting, and I regard it as a warning. Beyond question, if I had myself been analyzing another person in such circumstances, I should have presumed the existence of a connection between my daughter's death and the train of thought present in *Beyond the Pleasure Principle*. But the inference that such a sequence exists would have been false. The book was written in 1919, when my daughter was still in excellent health. She died in January, 1920. In September, 1919, I had sent the manuscript of the little

book to be read by some friends in Berlin. . . . What seems true is not always the truth. (Vol. 19, p. 187)

This last sentence may be a useful motto for work in this area: "What seems true is not always the truth." In this case, what seems a personal connection may not actually be one. However, Freud's disclaimer may itself not be entirely true in that he originally sent out the manuscript in 1919, but he also worked on the manuscript for several additional months in 1920, after Sophie had died. Freud was correct in that the death of his daughter could not have started this line of thought, but it is possible that her death influenced his later revisions of the manuscript.

Another personal factor proposed as related to his origin of the death instinct was that of Freud's cancer of the jaw. This, however, was not diagnosed until 1923, so it is clearly after the introduction of the concept in 1920. Others have suggested that Freud was influenced by the traumas of the Great War and by anxiety about his 2 sons serving in the military. Another explanation is that the concept of a death instinct played a significant role in the structure of Freud's theorizing, with intimations of it going as far back as his unpublished *Project for a Scientific Psychology* in 1895. I will not attempt to resolve all these issues here, but it is clear that a whole field of personal factors can be proposed as sources of a concept. However, as Freud argued, apparent connections are not always true and it is necessary to critically assess them.

### *Karen Horney*

Karen Horney (1885–1952), the distinguished neo-analytic or social psychoanalyst, is best known for works such as *The Neurotic Personality of Our Time* (1937), *New Ways in Psychoanalysis* (1939), *Self-Analysis* (1942), and *Neurosis and Human Growth* (1950). She was an early advocate for understanding the cultural contexts of psychopathology, and a critic of Freud's misunderstanding of women's psychology with a posthumous collection of papers titled *Feminine Psychology* (1967).

A major recent biography of Horney is by Bernard Paris, a Horneyan literary critic, professor of English at the University of Florida, and founder and director of the International

Karen Horney Society. Paris says that working on the biography *Karen Horney: A Psychoanalyst's Search for Self-Understanding* (1994) changed his perception of her, and his sense of how the person was related to her work. Reading her books over the years, Paris had "formed an image of her as a wise, benign, supportive woman who, having worked through her own problems, was now free to help others" (p. 175). However, earlier biographies of Horney by Jack Rubins and Susan Quinn, and his own research led to revisions in his understanding of her. He now sees her as a "tormented woman with many compulsions and conflicts who violated professional ethics and had difficulties in her relationships" (1994, p. 175).

In particular, she had compulsive affairs with colleagues and with students in training or in supervision with her for many years. She had a relationship with Erich Fromm from approximately 1934 to 1939, while also having affairs during this time with Paul Tillich and Erich Maria Remarque. She also had several affairs with analysts of hers including Harold Kelman in the 1940s, who was a major figure in the Association for the Advancement of Psychoanalysis, which she had cofounded in 1941.

In Horney's *Self-Analysis* (1942), she writes about a patient named Clare, who is struggling to sort out problems in her relationship with a man named Peter. Paris speculates that Horney is really writing about her relationship with Erich Fromm, which romantically ended around 1939 and continued professionally for a few years beyond that. Paris suggests that the Clare-Peter relationship was similar to the Horney-Fromm relationship with an "unworkable combination of a dependent woman and a man hypersensitive to any demands upon him" (p. 146). Paris also suggests that Fromm's *Escape from Freedom* (1941) also indirectly discusses their relationship, and that perhaps "Fromm and Horney were writing in part for each other, each trying to show the other how much he or she understood" (Paris, 1994, p. 147).

Those disturbed by Horney's character might "wish to discard her ideas" (Paris, 1994, p. 175). In contrast, Paris argues that being disturbed by her behavior, or even considering it pathological, need not lead to rejecting her ideas. His view is that although Horney had significant character flaws, she was "also a rather heroic figure whose courage in seeking the truth about

herself enabled her to make a major contribution to human thought" (1994, p. 176). Her difficulties may well have been the sources of her ideas, leading to continuing self-analysis and to continuing theoretical creativity: "We do not achieve profound psychological understanding without having had the need to look deeply into ourselves. Where would Horney's insights have come from had she not experienced her difficulties?" (Paris, 1994, p. 176).

To this last question, I would respond that insights can come not only from personal difficulties and experience, but also from clinical work, empirical research, cultural sources, from integrative reading and thinking, or various combinations of these (a point which Paris may well agree with). There is no need to weaken the claim for the relevance of personal experience to theoretical creativity by exaggerating it. An interesting set of questions are raised: To what extent does profound psychological understanding require deep introspection, and to what extent is such self-understanding a precondition for other kinds of learning and creativity?

### Henry A. Murray

Henry A. Murray (1893–1988) was a founder of personality psychology, author of *Explorations in Personality* (1938), coinventor of the T.A.T. (Thematic Apperception Test) in 1935, editor with Clyde Kluckhohn of *Personality in Nature, Society and Culture* (1948), and director of the Harvard Psychological Clinic from 1928. He was admired by many, including myself, as a critic of sterile scientism, a champion in linking psychodynamic and academic psychology, and a personally compelling advocate of the study of whole persons and the deepest human experiences.

Two incidents from his life will be presented as illustrations of the connections between life and work. When Forrest Robinson first proposed doing a biography of Murray in 1970, Murray replied that a central theme was a 40-year secret love affair that had revolutionized his life (Robinson, 1992). The object of his affections was Christiana Morgan, born in 1897, daughter of a professor at Harvard Medical School, and coinventor of the Thematic Apperception Test in 1935. Murray and Morgan, both married, first met each other in 1923. By Easter vacation, 1925, Murray, with an MD

and a PhD in biochemistry near completion, was talking with Jung about his growing attachment to Christiana Morgan, and Jung told Murray about his own relationship with his wife Emma Jung and his "inspiratrice" Toni Wolff.

Jung advised Murray against going into psychology and was not encouraging about the relationship with Christiana, but Murray ended up following Jung's example more than his advice. Murray and Morgan told their spouses of their relationship, yet remained married, and pursued a passionate, emotionally involved relationship until the end of her life in 1967. They saw each other as paths to the study of the unconscious and to their own deepest selves. Morgan saw Jung in therapy in 1926, and Jung taught a series of Vision Seminars on her visions from 1930–1934, which have recently been published in 2 volumes.

In 1959, Murray published a chapter on "Vicissitudes of Creativity" in which he describes the experience of a couple he called Adam and Eve, both of them coming out of dead marriages:

The hypothesis that is suggested by the history of this particular dyad is that periodic complete emotional expression within the compass of an envisaged creative enterprise—not unlike the orgiastic Dionysian rites of early Greek religion in which all participated—is a highly enjoyable and effective manner of eliminating maleficent . . . tendencies as well as of bringing into play beneficent modes of thought and action. . . . In sharp contrast to this is both the traditional Christian doctrine of repression of primitive impulses and the psychoanalytic notion of the replacement of the id by the ego (rationality), which results so often in a half-gelded, cautious, guarded, conformist, uncreative, and dogmatic way of coping with the world. (Shneidman, 1981, p. 327)

Murray elaborates on the power of dyads for regenerating culture, but without knowing something of Murray's relationship with Christiana, it is sometimes hard to see what he is talking about.

A second moment in Murray's life is his tenure meeting in 1936, chaired by Harvard President James Bryant Conant. As an illustration of the passions aroused by debates about the place of psychoanalysis in the university, Karl Lashley, a neuropsychologist recently hired by Harvard as supposedly the most distinguished psychologist in the country, said that he would resign if Murray received tenure. A major supporter, social and personality psychologist Gordon Allport said that

he would resign if Murray did not receive tenure. Edwin G. Boring, an experimental psychologist who was chair of the psychology department, and who will be discussed later, also opposed tenure. They later reached a compromise in which Murray was given two 5-year appointments, but not tenure, and to mollify Lashley, he was made a research professor, with no teaching responsibilities.

As an indication of Lashley's hostility to psychoanalysis, there is a story that Lashley had briefly been in psychoanalysis with Franz Alexander at the University of Chicago, had left in a rage, and then unsuccessfully tried to get Alexander fired from the university. This story needs additional evidence to support or refute it, to move it from the penumbra of possibly true to the categories of probably true or probably false. In the meantime, what is more certain is that even Lashley's friends, like Boring, said Lashley was irrationally hostile to psychoanalysis.

Examples we have considered so far are from the psychodynamic, experiential side of psychology, such as Freud, Karen Horney, and Henry Murray. Are personal-experiential factors operative only in such "soft" traditions, but not in "hard" natural science traditions? I will argue that personal-psychological-experiential factors can also be important within quantitative or experimental natural science traditions, although perhaps in somewhat different ways. Examples will be drawn from the life and work of B.F. Skinner on behaviorism and Paul Meehl in psychological measurement.

### B. F. Skinner

In an excellent book on psychobiography, *Uncovering Lives: The Uneasy Alliance of Biography and Psychology* (1994), Alan Elms argues that even though B. F. Skinner (1904-1990) was the preeminent behaviorist of his time and, in the view of some, the preeminent psychologist, the personal sources of his ideas may be somewhat obscure.

Elms argues that Skinner's *Walden Two* (1948), his best-selling book with more than 2 million copies sold, provides some insight into Skinner's changing self-conceptions and his relations with behaviorism. Skinner indicates that he usually wrote slowly and in longhand, but that "*Walden Two* was an entirely different ex-

perience. I wrote it on the typewriter in seven weeks." Parts of it were written "with an emotional intensity that I have never experienced at any other time" (Elms, 1994, p. 86).

*Walden Two* is partly a dialogue between Burris, "a pedestrian college teacher," and Frazier, "a self-proclaimed genius who has deserted academic psychology for behavioral engineering." B. F. Skinner, whose full name was Burrhus Frederic Skinner, says the novel was "pretty obviously a venture in self-therapy, in which I was struggling to reconcile two aspects of my own behavior represented by Burris and Frazier" (Elms, 1994, p. 87). As Skinner told Elms in an interview in 1977, when he wrote *Walden Two*, he was not really a Frazierian, a social engineer. However, writing the book convinced him: "I'm now a thoroughgoing Frazierian as a result and I'm no longer Burris" (Elms, 1994, p. 99). In other words, Skinner was no longer the pedestrian college teacher, but more a brilliant maverick applying behavioral principles to the redesign of society.

Elms argued that writing *Walden Two* was Skinner's response to a midlife crisis at age 41. This may have reactivated an earlier identity crisis Skinner had during his "Dark Year" at age 22, when he concluded that he could not be a fiction writer as he had nothing to say, which led to confusion and disastrous consequence for his self-respect. "The crisis (at age 22) was finally resolved, as such intense identity crises often are through the wholehearted acceptance of an ideology—indeed, an extreme ideology. In Skinner's case, the ideology was radical behaviorism" (Elms, 1994, p. 90).

### Paul E. Meehl

Paul Meehl (1920-2003) was a major contributor to psychological measurement, taxonomy, and philosophical psychology. He received his BA in 1941 and PhD in 1945 from the University of Minnesota, where he spent his entire career. He is author of the classic *Clinical versus Statistical Prediction* (1954), *Psychodiagnosis: Selected Papers* (1973a), and *Selected Philosophical and Methodological Papers* (1991). Most recently, *A Paul Meehl Reader: Essays on the Practice of Scientific Psychology* (2005) has been published. He has a reputation among many as one of the most brilliant psychologists in the history of the discipline and

was elected President of the American Psychological Association in 1962.

In his 1973 book, *Psychodiagnosis: Selected Papers*, my favorite piece is a 75-page paper, "Why I Do Not Attend Case Conferences." Meehl describes this as a diatribe, a polemic against the kind of faulty reasoning he sees as endemic in clinical case conferences because of inadequate training of most clinicians in logic, statistics, diagnosis, psychometrics, and biology. He says this paper is intended as destructive criticism in that you have to shake people up before you can get them to do something different.

Meehl wants to change both the quality of reasoning and the "buddy-buddy" norms in case conferences, in which everything, "gold and garbage alike" is positively received: "The most inane remark is received with joy and open arms as part of the groupthink process" (Meehl, 1973b, p. 228). Negative feedback is heard with horror and disbelief, and if it is delivered, one is seen as an ogre. In clinical case conferences and other academic groups, he says, people seem to undergo a kind of intellectual deterioration when they gather around a table in one room. Meehl decries what he sees as the "groupy" attitude, in which all evidence is seen as equally good, and a "mush-headed approach which says that everybody in the room has something to contribute (absurd on the face of it, since most persons don't usually have anything worthwhile to contribute about anything, especially if it's the least bit complicated)" (Meehl, 1973, p. 227). In a similar tone, he goes on to identify and make fun of common fallacies in clinical reasoning.

Personally, I love this paper and find its aggressive polemics amusing. I have used it in classes, with students split on it, some loving it, finding it one of the most illuminating things they have ever read, as well as funny; while others find it threatening, or intimidating, and get so upset they do not finish reading it. I once wrote Meehl a letter about the piece saying that these strong criticisms may make clinicians feel anxious, defensive, or misunderstood, and perhaps angry at the critic, but will not necessarily lead to significant change. Would it not be more effective to also provide models of more rigorous clinical reasoning, which practitioners could draw from? He wrote back, "We're not quite communicating. You assume I hope to

cure the slobs by attack. But when did I ever assert such? Different readership aimed at" (personal communication, Sept. 16, 1974). In another letter, "I agree entirely with your view that clinicians are largely unaffected by tough, incisive, aggressive argument—I spend more of my time with lawyers and philosophers, and so have fallen into 'nontherapeutic' habits. . . . On the subjective side, you should remember that I have been in this field for over 30 years, and one becomes impatient after the tenth time he has to hear the same dumb errors made by PhD's. (That's no excuse, it's by way of personal explanation.)" (personal communication, Aug. 10, 1974). Meehl's letter led me to write a paper trying to follow my own advice, outlining average, optimal, and the best feasible approaches to clinical decision-making in "How Should Treatment Recommendations Be Made? Three Studies in the Logical and Empirical Bases of Clinical Decision-Making" (Runyan, 1977).

Paul Meehl published an autobiographical chapter in 1989, and I want to raise here the question of whether a few of these biographical facts contribute anything to understanding the content or tone of his writing.

My father was a bank clerk, who, despite extraordinary intelligence quit high school to help support a widowed mother and unmarried sister. He was fond of me in a cool way, and I knew it. Fortunately, I got his "brain" genes, because he held Admiral Rickover's view that if a man is dumb he might just as well be dead. I identified strongly with him. . . . In 1931 my father, who had embezzled money to play the stock market, committed suicide. (Meehl, 1989, p. 337)

Meehl's mother had been misdiagnosed for over a year as having Meniere's disease, a disturbance of the semicircular canal in the ear. Finally, a neurologist was called in, who correctly diagnosed a brain tumor. When Meehl was 16, his mother died after surgery for this brain tumor: "This episode of gross medical bungling permanently immunized me from the childlike faith in physician's omniscience that one finds among most persons, including educated ones" (Meehl, 1989, p. 340).

A question for psychologists of science arises here: Is there any connection of this event to his interests in correct diagnosis with the strong affect and anger associated with it? The answer is not, as I see it, absolutely certain, although on first glance, it seems there might well be a connection. Even if there is, other factors may



also be at work, including his cyclothymic temperament, and his social and cultural contexts, such as his association with Herbert Feigl and other philosophers in the Minnesota Center for the Philosophy of Science, which Meehl helped create in 1953. Meehl also spent time in the medical school and with lawyers who may each have different cultures and styles of argument than in the clinical case conferences of which he was so critical.

### Changing Perspectives in the History of Psychology

What are the different ways that the personal or biographical dimension has been included or not in different histories of psychology? Historians of psychology may focus on the internal interplay of theory and research, on external social-political or cultural factors, and/or on the personal-biographical contexts of psychology.

I will not attempt a comprehensive review here, but rather discuss the views of two individuals with views at the two ends of the continuum: first, a sophisticated advocate of biography in the history of psychology, Edwin Boring, and second, a major postmodernist critic of personal-experiential approaches to the history of science, Michel Foucault. This is followed by a brief review of some major recent contributors to psychobiographical studies of psychologists, indicating the potentials for paying attention both to personal psychobiography, and to aspects of the wider social, cultural, and historical worlds.

#### *Edwin G. Boring*

Edwin G. Boring (1886–1968) was a professor at Harvard from 1922, Director of the Psychological Laboratory from 1924, president of the American Psychological Association in 1927, and author of the dominant history of academic psychology *A History of Experimental Psychology* (1929/1950). Boring's lineage may be traced back to the founding of experimental psychology, with Wundt's establishment of his lab in 1879 in Leipzig. Boring was the favorite student of E.B. Titchener (1867–1927), an Englishman who had studied with Wundt in Leipzig, and then came to Cornell University in 1892, where he became a major figure in translating Wundt's work (at least the experimental and physiological parts of it),

and in organizing experimental psychologists in the United States. After Titchener's death in 1927, Boring, as long term chair of the Harvard Psychology Department, may have been the most influential experimental psychologist in the United States, at least institutionally, if not intellectually, and a recognized founder of the history of psychology.

Boring's *A History of Experimental Psychology* (1929/1950) is a massively informed history of the work and lives of experimental psychologists, which became standard reading as psychology attempted to stake out its territory as a natural science. Boring's text included a tremendous amount of biographical information on experimental psychologists and was an indispensable resource: "Perhaps I should say also why there is so much biographical material in this book, why I have centered the exposition more upon the personalities of men than upon the genesis of the traditional chapters of psychology. My reason is that the history of experimental psychology seems to me to have been so intensely personal. Men have mattered much" (1950/1929, p. viii). The authority of particular individuals was sometimes influential "quite independently of the weight of experimental evidence" for their views. Personalities were important in shaping schools and "the systematic traditions of the schools have colored the research" (1950/1929, p. viii).

Boring's interest in more biographical information led him to write a letter to Carl Murchison at Clark proposing a series of autobiographical essays in psychology, which began in 1930 as *The History of Psychology in Autobiography* (Murchison, 1930) and continued, after a break (Boring & Lindzey, 1967), up to the present (Lindzey, 1989; Lindzey & Runyan, in press). In 1929, Boring emphasized the importance of individual great psychologists in shaping the field, but by the 1950 edition, he was also attending to the "zeitgeist" or cultural factors of the age.

Boring was a leading advocate of experimental psychology, so it may be somewhat surprising to see him try his hand at psychobiography in explaining the divisions between different types of psychologists. In a 1942 essay on William James, on the centennial of James's birth, Boring explores the differences between phenomenologists, like William James, and experimentalists, like himself. He speculates that "the

phenomenologist must have faith in himself and his own observations, whereas the experimentalist mistrusts himself and is forever looking to controls. . . to correct his own errors" (as quoted in Boring, 1961, p. 203). How are these 2 stances generated?

Perhaps some future empiricist will, indeed, solve the problem, will show that a phenomenologist must have had a happy childhood with love and security to spare, a childhood in which it was natural to accept the givens without demanding accounts of their origins. The empiricists and reductionists would then turn out to be the insecure children, who learned early to look beyond the given, suspecting a catch in what is free. . . . Sensed insecurity is nevertheless the sanction for science itself. (Boring, 1961, p. 208)

This seems to me too monolithic an interpretation of the personal motives for experimentation. It may well be consistent with Boring's self-understanding, as he saw himself as insecure and not attaining "maturity" until in his 50s, but like Freud, he may well have overgeneralized from his own experience. One could also argue the converse, that experimentalists are more secure adults, who are willing to have their ideas tested experimentally. One can think of examples like Edward Tolman of the University of California, Berkeley who seemed both self-confident and secure, and a dedicated experimental psychologist working primarily with rats, whose bookplate contains an image of a rat in a maze. There need not be any one-to-one relation of personality to theoretical or methodological preferences, although in some contexts there may be aggregate group differences.

Like many psychologists, Boring's view of psychology was changed by his experience in World War II. Boring became more open to applied psychology, seeing its value in the war effort, and made efforts to be more eclectic. In the 1961 introduction to his William James essay of 1942, Boring writes "the progress of thought and discovery depends to some extent upon the personalities of the thinkers and the discoverers. . . . Psychology's great scientific divide needs not only division of labor but also the division of personality that makes complementary and even incompatible activities essential for progress" (Boring, 1961, p. 194).

### *Michel Foucault*

It is sometimes charged that biographical approaches to the history of science have been

overemphasized while the social and cultural sides have been neglected. Sometimes the personal-psychological-experiential side of the human sciences is downplayed or denied, whether by Marxists, sociologists of scientific knowledge, or by some postmodernists. An extreme case of this is in the work of Michel Foucault (1926-1984) who has been enormously influential in the history and social studies of science.

He and many others emphasize the ways in which science is socially, politically, economically, culturally, materially, and historically constructed. These are important perspectives, sometimes supported with exquisitely detailed social analysis of topics in the history of science (Galison, 1997; Shapin & Shaffer, 1989). They can open one's eyes to processes previously not seen or attended to.

Foucault often denied the relevance of the personal or psychological and said that what counts is the political aspect of his work. This view was expressed through most of his career with an unexpected change at the end. I will discuss a few elements of his work because he is one of the most influential postmodern historians and critics of the human sciences. In a 1969 interview about his book *The Archaeology of Knowledge* (1969), Foucault said he absolutely refuses the psychological and wants to focus on discourse itself without "looking underneath discourse for the thought of man" (Foucault, 1996, p. 58).

The denial of the psychological can be done for intellectual, political, and/or personal reasons. I would guess that all three are operative in Foucault. To mention just one of his political and intellectual objections to the psychological, he says in an interview in 1974 on the Attica prison uprising that does not "everything that is a psychological or individual solution for the problem, mask the profoundly political character both of society's elimination of these people and of those people's attack on society. All of that profound struggle is, I believe, political. Crime is a 'coup d'etat from below.'" (1996, p. 121).

My response to Foucault is that: Yes, psychological analysis can mask the political. However, the converse can also happen, in which the political masks the personal and the psychological. Sometimes personal hurt or rage is projected onto wider political arenas. Often, the personal-experiential, the political, and the in-

tellectual-cultural are interwoven in complex and reciprocally influencing ways. And there are few better examples of this than Foucault himself.

What are the sources of Foucault's desire to critique modernist culture, to critique the human sciences, or to dismantle extant power relations? Does this come from disinterested intellectual reflection, from social-political contexts, and/or from personal experience? It seems plausible, or is at least an interpretive hypothesis worth investigating, that aspects of Foucault's critical stance can be related to his personal experience of feeling persecuted as a homosexual in France, attempting suicide in 1948, threatening or attempting suicide a number of other times, and feeling mistreated by the mental health establishment. A doctor at the "Ecole Normale Supérieure," citing confidentiality, would say only that "these troubles resulted from an extreme difficulty in experiencing and accepting his homosexuality" (Eribon, 1991, p. 21). According to Eribon, after homosexual encounters, "Foucault would be prostrate for hours, ill, overwhelmed with shame" (p. 27), and a doctor was called on frequently to keep him from committing suicide. These personal experiences, in a particular social and cultural context, may well be a source of his antipathy to the mental health establishment and of his perceptions of the human sciences as invasive and harmful rather than beneficent. These personal experiences and others may be interwoven with the formation of political stances and changing intellectual programs throughout Foucault's career.

Foucault maintained what I would describe as a heavily political yet underpsychologized approach to the human sciences through his early archeology of knowledge phase and to his middle genealogical or power/knowledge period. However, after the transformative experience of participating in the gay community in San Francisco in 1975 and of taking LSD in 1975, his intellectual position changed, with attention turned toward the history of sexuality, history and technologies of the self, and ethics. After 1975 and 1976, the style of his writing also changed to a more clear, lucid style.

At the end of his life, in what is said to be his last interview on May 19, 1984, Foucault says that in his earlier books *Madness and Civilization*, *The Order of Things*, and *Discipline and*

*Punish*, "I tried to mark out three types of problems: that of the truth, that of power, and that of individual conduct. These three domains of experience can be understood only in relation to each other, and only with each other. What hampered me in the preceding books was to have considered the first two experiences without taking into account the third" (Foucault, 1996, p. 466). In other words, these early works were concerned first with discourse itself, then with the relations of truth and power, but neglected individual conduct, which he tried to address somewhat more in his last books on the history of sexuality, ethics, and techniques of the self. In adding individual conduct, he said "I had a guiding thread which didn't need to be justified by resorting to RHETORICAL methods [capitalization added] by which one could avoid one of the three fundamental domains of experience" (Foucault, 1996, p. 466). Foucault acknowledges, more so in his later life, that all of his work had origins in fragments of his personal experience, including his writings on madness, on prisons, and the history of sexuality.

#### Recent Psychobiographies of Psychologists

There is some excellent recent work on the biographical side of psychological theory and research. At its best, it includes discussions of individual psychobiography with relevant social, cultural, and historical contexts. I will mention only a few selected books. A strong advocacy of the importance of the personal side of psychological theory came with Stolorow and Atwood's (1979) *Faces in a Cloud: Subjectivity in Personality Theory* inspired in part by Silvan Tomkins' work on the psychology of knowledge. They argued that the subjective experiential worlds of Freud, Jung, Rank, and Reich all powerfully influenced their theories of personality. More recent interpretations of Freud, Skinner, and Carl Rogers are provided in Demorest (2005). Erik Erikson's life and work have been reinterpreted by Friedman (1999) and by Erikson's daughter, Sue Erikson Bloland (2005).

In *Pioneers of Psychology* (1996), Raymond Fancher demonstrates the advantages of a biographical approach to psychological theory in 13 chapters, beginning with Rene Descartes,

and including Wundt, Darwin, Galton, William James, Pavlov, Watson and Skinner, Freud, Binet and Piaget, ending with a final chapter on artificial intelligence, organized not around a single person but around a machine, the computer.

Irving Alexander provides psychobiographical interpretations of Freud, Jung, and most intriguingly, a hypothesis about the missing years in young adulthood of Harry Stack Sullivan (Alexander, 1990). In addition to his study of B.F. Skinner discussed above, Elms also has published studies of Freud, Jung, Allport, and others (Elms, 1994, 2005). Gordon Allport has been the subject of a complex analysis of the social, cultural psychological sources of his thought (Nicholson, 2003) with additional studies of Allport by Barenbaum (2005).

The *Handbook of Psychobiography* (Schultz, 2005) contains a section on the psycho-biography of psychologists, including chapters on the life and work of Freud, Gordon Allport, Erik Erikson, and S.S. Stevens. The *Handbook* also has sections on "Psychobiographies of Artists" (including Elvis Presley, Sylvia Plath, J. M. Barrie, and Edith Wharton) as well of others such as Truman Capote or Diane Arbus.

Scholarly interest remains strong in the lives of both Charles Darwin and William James. Their lives have been studied from social, cultural, and psychological perspectives. Both Darwin and James each have good biographies, standard editions of their works, and published volumes of their correspondence, year by year, providing advantages for later biographers, psychobiographers, and historians.

The psychological interpretation of psychologists is also engaged in by psychologists themselves. Between 1930 until the present, the series *A History of Psychology in Autobiography* has produced 8 published volumes with another in press. Personally, I first became aware of this series in 1967, which contained autobiographies by Gordon Allport, Henry Murray, Carl Rogers, and B.F. Skinner (Boring & Lindzey, 1967). Volume 7 (Lindzey, 1989) includes interesting autobiographies by Roger Brown, Lee Cronbach, Eleanor Maccoby, Paul Meehl, George Miller, and others, whereas Volume 9 has illuminating autobiographies by Elliot Aronson, Gordon Bower, Jerome Kagan, Daniel Kahneman, Elizabeth Loftus, Ulrich Neisser, Walter Mischel, and others (Lindzey & Runyan, in press).

## How to Conceptualize the History of Science?

### *Beyond "The Science Wars"*

There are at least three conceptually distinct approaches to the history of science. First, it can be a within-science analysis of the interaction between theory and empirical research as science is seen as progressing toward a more adequate understanding of the natural world. Second, it can be biographical, relating scientific accomplishments to the lives of eminent scientists, which has been done in a great variety of ways, as illustrated in the works discussed above. Third, it can look at science in its social-political and cultural contexts (e.g., Danziger's *Constructing the Subject: Historical Origins of Psychological Research*, 1990; or Roger Smith's *The Norton History of the Human Sciences*, 1997). This "external" approach to the history of science is often used to critique "internal" approaches, which look primarily at relations between scientific theory and research or is sometimes used to critique biographical or psychological approaches to the history of science.

After decades of this third approach of externalist, "social constructionist," and sometimes politically critical "social studies of science," some physical and biological scientists became upset and aggravated enough to launch a counterattack. This critique and defense of social studies of science became known as the "Science Wars." The fray began in earnest with Gross and Levitt's *Higher Superstition: The Academic Left and Its Quarrels with Science* (1994). This was followed up by "The Flight from Reason and Science" (1996), a conference they organized sponsored by the New York Academy of Sciences leading to the book *The Flight from Reason and Science* (1996).

Another controversial incident began when physicist Alan Sokal published a parody of social studies of science, which was published in a special issue on the "Science Wars" of *Social Text* in spring, 1996. The article titled "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity" combined some social studies of science jargon, with nonsensical physics, and was revealed by Sokal as a hoax three weeks later in *Lingua Franca*.

Then "all hell broke loose" (Sokal & Bricmont, 1998, p. 9). There was a great hubbub on

both sides, with scientists, on one hand, delighted that someone had showed up the ignorance and pretensions of those in social studies of science. Those in social studies of science, on the other hand, felt they had been treated unfairly, perhaps even unethically. They were also upset at the misunderstandings of their work. In 1998 a useful collection of papers edited by Noretta Koertge, professor of the history and philosophy of science at Indiana University, was published as *A House Built on Sand: Exposing Postmodernist Myths about Science*. This book contains 18 chapters by scientists and humanists largely containing "hardhitting critiques of the postmodernist case studies that are cited over and over again as evidence for the claim that the results of natural science tell us more about social context than they do about the natural world" (Koertge, 1998, p. 4).

Does psychobiographical research in the history of psychology have anything to contribute to the "Science Wars"? Philosopher and historian of science Philip Kitcher in "A Plea for Science Studies" (1998) searches for a middle ground between science and science studies. He says that science studies needs to respond to two clusters of ideas, which are summarized (and slightly modified) below:

*The realist-rational cluster.* This corresponds roughly to scientists' self-image and includes the ideas that scientific research is often progressive, in the sense of leading to increased powers of prediction and intervention. Debates about scientific issues are settled by appeals to reason and evidence (not just by politics). And finally, the canons of reason and evidence can improve over time as we discover how to learn more about the world.

*The socio-historical cluster.* This corresponds roughly to ideas held by those in social and cultural studies of science and includes beliefs that science is done by cognitively limited human beings who are shaped by their social contexts and histories: "No scientist ever comes to the laboratories or the field without categories and preconceptions that have been shaped by the prior history of the group to which he or she belongs" (Kitcher, 1998, p. 36). In other words, scientists are inevitably shaped by their cultural contexts and concepts. Social structures affect the way that research is performed, transmitted, and received, which can affect theoretical debates. Finally, social struc-

tures affect the choice of scientific questions, and sometimes the answers that are proposed or critically evaluated.

Historically, science studies was dominated up until the 1960s by the first cluster of realist-rationalist ideas, as logical positivism emphasized the logical-normative dimensions of scientific explanation, theorizing, and confirmation. Since the 1970s, the second cluster of sociohistorical perspectives has dominated science studies, emphasizing the extent to which science is socially constructed, in which science is only "politics by other means," in which objectivity may be an illusion, in which scientific progress is questionable, as science has been implicated in destructive warfare, racism, sexism, eugenics, imperialism, and environmental disasters. Given the impossibility of scientific objectivity and political neutrality, one ought to pursue progressive political values (Koertge, 1998, pp. 3-4).

Kitcher caricatures these two world views by saying that in the first realist-rational perspective "scientists were conceived as asocial, logically omniscient beings whose work was shaped only by what happened in the lab" (1998, p. 37). On the other extreme, science studies sometimes ignored the realist-rational cluster, and "scientists have been conceived as brain-dead from the moment they enter the laboratory" (1998, p. 37), and that their work is determined by external factors such as class, gender, religion, culture, and politics.

How does one put together a viable understanding of science that includes both its internal and external features, or both the realist-rational cluster and the sociohistorical cluster? Kitcher points to works which he believes fruitfully explore both clusters of issues, such as the widely praised Martin Rudwick's *The Great Devonian Controversy* (1986) on a geological controversy in the 1830's, Peter Galison's *How Experiments End* (1987), and Kitcher's own *The Advancement of Science* (1993).

In the history of psychology, I would add works integrating both internal and external clusters of ideas such as Kurt Danziger's *Constructing the Subject: Historical Origins of Psychological Research* (1990), Mitchell Ash's *Gestalt Psychology in German Culture, 1890-1967* (1995), Roger Smith's *The Norton History of the Human Sciences* (1997), or Anne Harrington's *Reenchanting Science: Ho-*

*lism in German Culture from Wilhelm II to Hitler* (1996). A valuable work that analyzed how personality psychology was founded in its cultural, social and personal contexts is Ian Nicholson's *Inventing Personality* (2003).

My own view is that psychology and other sciences can be viewed as cultural enterprises, each in their own social, cultural, personal, and historical contexts. The psychology of science is a necessary complement to the philosophy, history, and sociology of science. The psychology of science includes quantitative, correlational, and historical-interpretive methods, with historical-interpretive or "historical science" methods particularly relevant in trying to advance our understanding of relations between the life and work of individual scientists. We may need to integrate not two clusters of ideas, but rather three. These are the realist-rational or scientific cluster, the sociohistorical cluster, and a personal-psychological-experiential cluster. It is undeniably complex but can also be illuminating to discover the ways in which scientific-cultural worlds, social-political worlds, and personal-experiential processes continually coconstruct each other over time (cf. Runyan, 2005b).

### Conclusion

The current paper began with the deceptively simple question: How important is the personal side of psychological theory? It then reviewed cases across an array of theoretical traditions in which it seems that there are connections between the life, work, and social contexts of individual lives, as in Freud, Karen Horney, Henry Murray, B. F. Skinner, and Paul Meehl.

The history of psychology provides intriguing examples of what appear to be associations between the life and work of individual theorists. Such interpretations can be critically evaluated, revised, and improved, leading to incrementally more adequate understanding of relations between advances in psychological theory and research and the lives of individual psychologists.

Within the history of science there are a variety of contending views about the relative importance of and relations between factors "internal" to science, and external personal, social, and cultural contexts. This paper discussed the views of Edwin G. Boring, foremost historian of experimental psychology, and of Michel Fou-

cault, usually seen as a postmodern constructivist and a critic of personal-biographical approaches to the history of science.

How are we to understand the history of scientific psychology and the place of psychobiography within it? I am arguing against "internal" "pure science" views attending only to the interaction between theory and empirical research. I am also arguing against "external" sociohistorical or cultural constructionist views, which omit persons and their psychology. Instead, I am arguing for the view that a successful integration in science studies needs to include not only the realist-rationalist views of scientists and the sociohistorical perspectives of those in social studies of science, but also the psychological and psychobiographical perspectives of those in the psychology of science.

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Received January 30, 2006

Accepted February 8, 2006 ■