Psychobiography and the Psychology of Science: Encounters With Psychology, Philosophy, and Statistics

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*T*he 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 (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).

Lee Cronbach argued in his presidential address to the American Psychological Association that there are "Two Disciplines of Scientific Psychology," correlational and experimental (1957). He later wrote about the interaction of personal and social factors in "Beyond the Two Disciplines of Scientific Psychology" (1975) and about the importance of historical accounts of individual cases (1982). There are at least "Three Disciplines of Scientific Psychology:" quantitative, experimental, and historical-interpretive (Runyan, 2005). This chapter explores examples of historical-interpretive analyses of single cases in psychology, philosophy, and statistics.

Previously, I argued for the relationship of personal experience to psychological theorizing in a paper on "Psychobiography and the Psychology of Science: Understanding Relations Between the Life and Work of Individual Psychologists" (Runyan, 2006). Examples discussed included Sigmund Freud, Karen Horney, B. F. Skinner, Henry A. Murray, Paul Meehl, Edwin G. Boring, and Michel Foucault. This chapter builds on that paper, and argues that personal experience can be relevant not only in psychology, but can also be influential (perhaps in somewhat different ways) in philosophy and statistics.

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 of 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 (Popper, 1959). An internalist tradition in history of science focusing on the interplay of scientific theory and research might see personal-experiential factors as little more than distractions from rigorous scientific inquiry. Externalist traditions analyze science in its social and cultural contexts, ranging from Marxist analyses of science in society, through the sociology of science, to postmodern social and cultural constructivist views, each of which sometimes slights 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" (William 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.

This chapter begins with two examples from philosophy, Bertrand Russell and Ludwig Wittgenstein. I start there because the life stories are dramatic and because the work has great conceptual scope, with implications for how we think about science in the world. The next section discusses relationships between the work and life of several eminent psychologists including Freud, B.F. Skinner, Karen Horney, and others (drawing from Runyan, 2006). The third section is on statisticians, with examples from Karl Pearson, R. A. Fisher, and Jerzy Neyman. The argument, in brief, is that personal experience as studied in psychological biography can be relevant to understanding work not only in psychology, but also in philosophy, and statistics.

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 from which it originated "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 chapter 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 hopes 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 later 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 was extended in Feist's *The Psychology of Science and the Origins of the Scientific Mind* (2006). Here there were individual chapters reviewing work in Biological Psychology of Science (Chap. 2), Developmental Psychology of Science (Chap. 3), Cognitive Psychology of Science (Chap. 4), Personality Psychology of Science (Chap. 5), and Social Psychology of Science (Chap. 6). In Chapter 1, Feist situated 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. Drawing on earlier work by Nicholas Mullins (1973), Feist argues that disciplines can 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. In the second stage of identification, after intellectual achievements by the founders outline a field of inquiry, students and other scholars can 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 develop.

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), in the social psychology of science (Shadish & Fuller, 1994), or in "The Psychology of Science" Special Issue of *The Review of General Psychology* (June, 2006). Indeed, more recently, a journal devoted to the psychology of science was started as well as a society for the same (*International Society for the Psychology of Science and Technology, ISPT*).

MY PERSONAL JOURNEY THROUGH THESE QUESTIONS

Like any intellectual project, this inquiry into the biographical sources of psychological theory, philosophy, and statistics 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. This was followed by a book on *Life Histories and Psychobiography: Explorations in Theory and Method* (Runyan, 1982), analyzing alternative accounts of lives, the case study method, idiographic methods, and the psychobiography debate.

In 1988 I started teaching a course on "Personality Theory." To better understand the theories, I attended not only to the interaction of theory and empirical research, but also discussed their biographical, social, and cultural contexts. It dawned on me that this was at least partly a project in the history of science. To do a more rigorous job, I tried 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 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, cultural, and material studies of science were valued, and were seen as the cutting edge. 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 seen as intellectually or politically regressive for overemphasizing individuals and neglecting the extent to which science is socially constructed.

An obvious response is that, although it may not be easy, one can pay attention to social and cultural dimensions of science, along with studying individuals, groups, and populations. I will argue that analyzing 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.

I had gone to the history of science looking for more powerful intellectual instruments and 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. The following sections discuss relations between work and life in philosophy, psychology, and statistics.

THE PERSONAL SIDE OF PHILOSOPHY

On Bertrand Russell and Ludwig Wittgenstein

Bertrand Russell and Ludwig Wittgenstein are sometimes seen as two of the 20th century's most influential philosophers. They both had influences on the development of logical positivism in the Vienna Circle in the 1920s and early 1930s. However, they arrived at dramatically different conceptions of philosophy in relation to science, with Russell favoring a more "scientific philosophy" and Wittgenstein opposing such a view.

Bertrand Russell (1872–1970) was coauthor of *Principia Mathematica* with Alfred North Whitehead (3 volumes, 1910, 1912, 1913), analyzing the logical bases of mathematics. This book was drawn upon in the development of logical positivism in the Vienna Circle of the late 1920s and early 1930s. The Vienna Circle also drew upon Wittgenstein's *Tractatus Logico-Philosophicus* (1921). Wittgenstein (1889–1951) started as a potential protégé to Russell, but they ended up with dramatically different views of philosophy in relation to science, Russell favoring a more "scientific philosophy" and Wittgenstein opposing such a view in *Philosophical Investigations* (1953). The relations of Russell and Wittgenstein illustrate ways in which philosophical beliefs can be related to personal psychology and interpersonal relationships.

What was the personal context of Russell's work in philosophy? Russell's three-volume autobiography opens with an inspiring Prologue, "What I Have Lived For":

> Three passions, simple but overwhelmingly strong, have governed my life: the longing for love, the search for knowledge, and unbearable pity for the suffering of mankind.
>
> These passions, like great winds, have blown me hither and thither, in a wayward course, over a deep ocean of anguish, reaching to the very depths of despair.
>
> I have sought love, first because it brings ecstasy—ecstasy so great that I would have sacrificed all the rest of life for a few hours of this joy. I have sought it, next, because it relieves loneliness—that terrible loneliness in which one shivering consciousness looks over the rim of the world into the cold unfathomable lifeless abyss. . . .
>
> With equal passion I have sought knowledge. I have wished to understand the hearts of men. I have wished to know why the stars shine. And I have tried to apprehend the Pythagorean power

by which number holds sway above the flux. A little of this, but not much, I have achieved.

> Love and knowledge, so far as they were possible, led up to the heavens. But always pity brought me back to earth. Echoes of cries of pain reverberate in my heart. Children in famine, victims tortured by oppressors, helpless old people, a hated burden to their sons, and the whole world of loneliness, poverty, and pain make a mockery of what human life should be. I long to alleviate the evil, but I cannot, and I too suffer. (Russell, 1967, pp. 3,4)

This prologue is an eloquent statement of major human values. It impressed me when I first read it in the summer of 1969, as I was beginning graduate school that Fall. Russell wrote so much and knew so many eminent people in philosophy, literature, and politics. Were there things I could learn from him then? Are there things that we can learn from his work and life now?

Is Russell a model of intellectual productivity in many different fields? Or, is the lesson that great intellectual achievement can come at a high personal cost to those around one, as in Russell's relations with his first three wives and with his two children, Kate and John?

A two-volume psychological biography of Russell by Ray Monk (1996, 2000) provides a different interpretation of Russell's life, more critical than his autobiography or than three prior biographies. Monk says that three earlier biographies of Russell failed to adequately relate his work and life. The first by Alan Wood (1957) had Russell's cooperation, but failed to explore his inner life. The latter two by Ronald Clark (1975) and Carolyn Moorehead (1992) had more on Russell's inner life, but did not seriously relate his life to his work. Monk tries to relate Russell's three passions for love, knowledge, and politics to each other in his two-volume *Bertrand Russell: The Spirit of Solitude, 1872–1921* (1996) and *Bertrand Russell: The Ghost of Madness, 1921–1970* (2000).

Monk provides a detailed interpretation of Russell's work and its relationship to his inner life. It may change your perception of Russell's work and life; it changed mine. Monk argues that his three great passions were attempts by Russell "to overcome his solitariness through contact with something outside himself: another individual, humanity at large, or the external world" (p. xviii). To an extent greater than I had realized in reading Russell's work or in reading his autobiography, he feared the depths of his emotions, felt cut off from others, and was afraid of going mad. Monk may be too critical of Russell, but provides a level of detail about Russell's work and life that requires reexamination of both. Russell lived from 1872 to 1970, and was enormously productive, having published 70 books and more than 2,000 articles.

Ludwig Wittgenstein, however, lived from 1889 to 1951 and during his lifetime published a total of one book review (1912), one book, *Tractatus Logico-Philosophicus* (1921), and one article (1929), his third and final publication. After his death in 1951, *Philosophical Investigations* (1953) was published, along with many other volumes based on Wittgenstein's lectures, conversations, and notebooks.

Russell continued to argue for making philosophy more scientific, while Wittgenstein criticized such a view, as in *Philosophical Investigations* (1953).

I won't try to summarize their whole history, but rather I focus on Russell's relationship with Wittgenstein from 1911 to 1914. The younger Wittgenstein, born in 1889 to one of the wealthiest families in Vienna, then a student in aeronautical engineering at Manchester University, age 22, wanted to know if he had the talent to make a significant contribution to philosophy. He first visited the logician Gottlob Frege in Jena, who advised him to consult Russell.

Without a prior appointment, Wittgenstein arrived at Russell's rooms in Trinity College, Cambridge on October 18, 1911, to introduce himself. Wittgenstein actively participated in Russell's seminar through the term and argued with him afterwards. Before Christmas, Wittgenstein asked Russell whether he had the ability to make a contribution to philosophy. Russell said he didn't know, and asked to see a piece of his writing. Wittgenstein returned to Cambridge in January 1912 with a manuscript he had written over vacation. Russell was impressed, and believed that Wittgenstein might do great things. (Unfortunately the manuscript has not survived.) Wittgenstein later told a friend that Russell's encouragement "had proved his salvation, and had ended nine years of loneliness and suffering, during which he had continually thought of suicide" (Monk, 1990, p. 41).

Over the next term, Wittgenstein worked so intently in mathematical logic that Russell felt he had learned what Russell had to teach and maybe gone beyond him. Russell felt that Wittgenstein might be the protégé he had been looking for. However, by June 1913, Wittgenstein became severely critical of Russell's work. Russell was devastated by the criticisms. He wrote to his lover Lady Ottoline Morrell, that after Wittgenstein's severe criticism of his work, he "felt ready for suicide" (June 19, 1913). In a letter to her several years later Russell wrote that he didn't think she realized this at the time, but Wittgenstein's criticism in 1913 "was an event of first-rate importance in my life, and affected everything I have done since. I saw he was right and I saw that I could not hope ever again to do fundamental work in philosophy. My impulse was shattered like a wave dashed to pieces against a breakwater" (March 4, 1916; in Monk, 1996, pp. 301,302). In Russell's autobiography he wrote that Wittgenstein was "perhaps the most perfect example I have ever known of genius as traditionally conceived, passionate, profound, intense, and dominating" (p. 46).

In Wittgenstein's later work he became strongly critical of the view that scientific knowledge is the model of all knowledge. An overly scientific view gets in the way "not just of philosophical clarity, but of a full understanding of art, music, literature, and, above all, ourselves" (Monk, 2005, p. 106).

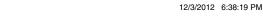## THE PERSONAL SIDE OF PSYCHOLOGICAL THEORISTS

Case Studies

Sigmund Freud

There is 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 to 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 two 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 neoanalytic 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 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 analysands 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" (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" (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?" (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 with which Paris may well agree). 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 is 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 (Runyan, 2008).

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 to 1934, which have recently been published in two 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, cautions, 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 Homey, 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 two 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 experience. 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 Burris 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).

Skinner's identity crisis and formulation of a new identity as a more scientific psychologist advocating radical behaviorism, may be related to a wider field of social and cultural issues. Skinner's major supporter in graduate school was not a professor in the psychology department, but an experimental biologist, W. J. Crozier who established a Laboratory of General Physiology at Harvard in 1925. Crozier's stance toward biology was strongly influenced by Jacques Loeb (1859–1924) and was dedicated to the experimental study of whole behaving organisms, as in tropisms, as contrasted with biochemical experiments in physiology. Crozier's world view was one that resonated with Skinner's experimental study of behavior, and Crozier was a major supporter in getting Skinner National Science Foundation (NSF) fellowships and getting him elected to the first class of Junior Fellows at Harvard in 1933.

One theme found over and over again among some of the more eminent experimental psychologists was insecurity that gets converted to conceit and arrogance. Pauly (1987), for instance, argues that there was a shared social background in many in this aggressively experimental research tradition. Many in this tradition felt like social outsiders, were not psychologically well adjusted, "lived with feelings of insecurity and inferiority, and compensated with exaggerated displays of conceit and self-assertion" (Pauly, 1987). When I first read this, it struck me that some of this may apply to Skinner's relations with other psychologists.

Paul E. Meehl

Paul E. 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, 1973b, 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?" (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.
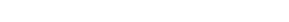
> My father was a bank clerk, who, despite extraordinary intelligence quit high school to help support a widowed mother and unmarried sister. He was fond of me in a cool way, and I knew it. Fortunately, I got his "brain" genes, because he held Admiral Rickover's view that if a man is dumb he might just 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 child-like 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 at 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 (Runyan, 1988; Smith, 1997).

I will not attempt a comprehensive review here, but rather discuss the views of two individuals who exemplified 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.

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 laboratory 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, 2007). 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 two 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 fifties, 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 self-confident and secure, at least in some ways, 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 (Simonton, 2000; Stolorow & Atwood, 1979).

Like many psychologists, Boring's view of psychology 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: 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 intellectual–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 possible 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 Superieure," 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 prostate 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, prisons, and the history of sexuality.

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* (2012) Raymond Fancher and Alexandra Rutherford demonstrate the advantages of a biographical approach to psychological theory in 15 chapters, beginning with Rene Descartes, and including Wundt, Darwin, Galton, William James, Pavlov, Watson and Skinner, Freud, Binet, and Piaget, with the next-to-last chapter organized not around a single person but around a machine, the computer with the last chapter on a variety of applied psychologies.

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, and 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 psychobiography 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 and 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 valuable resources for later biographers, psychobiographers, and historians. Excellent examples are the biographies of Darwin by Desmond and Moore (1991) and the two volumes by Janet Browne (1995, 2002). A major recent biography on William James is by Richardson (2006) and on James and his early associates in *The Metaphysical Club* by Menand (2001).

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 nine published volumes. 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, 2007).

Statisticians: R. A. Fisher and Jerzy Neyman

There are complex relations between thinking statistically, thinking historically, and thinking personally or experientially. What might be learned about these issues from looking at the lives and interpersonal relationships of statisticians? This discussion will focus on Sir Ronald A. Fisher (1890–1962) and Jerzy Neyman (1894–1981), two of the most influential statisticians of the 20th century, with an introductory note on Karl Pearson, a founder of the field.

Statistics was developed and instutionalized in part by Karl Pearson (1857–1936). Pearson's book, *The Grammar of Science* (1892/1900) presented a view of the importance of statistics in relation to science which inspired many over the years, including both Fisher and Neyman. *The Grammar of Science* made a claim for the unlimited scope of science, and "a moral vision of scientific method as the very basis of modern citizenship, because it provides standards of knowing that are independent of all individual interests and biases" (Porter, 2004, p. 7.) The second edition of *The Grammar of Science* in 1900 included a philosophical rationale for statistics, which was not in the first edition. Until the end of his life, Pearson saw it as his mission to "reshape science using the tools of statistical mathematics" (Porter, 2004, p. 8). Pearson was a follower of Francis Galton (1822–1911), author of *Hereditary Genius* (1869) and other works. In collaboration with Galton and W. F. R. Weldon, Pearson founded the journal *Biometrika* in 1901, which Pearson edited with a heavy editorial hand until his retirement in 1934.

An excellent biography of Pearson by Theodore Porter (2004) analyzes the complex personal meanings that statistics had for Pearson, who earlier planned to be a poet, and wrote a romantic novel (*The New Werther*) modeled after Goethe's *The Sorrows of Young Werther* (1774). Pearson had earlier been a German Scholar, a socialist, and an advocate for women's causes before turning to science by 1892 and to statistics by 1900.

Pearson was one of the individuals who helped to define what it meant to be a statistically sophisticated scientist. In *Karl Pearson: Scientific Life in a Statistical Age* (Porter, 2004) Porter writes about how Pearson's statistical and scientific work is related to his personal life, religious anxieties, and family dynamics. In 1901, Pearson began editing *Biometrika,* which he edited with an iron hand, advocating his own views of statistics until almost the end of his life in 1936.

R. A. Fisher (1890–1962) had more mathematical training than Pearson and became critical of Pearson's work. Pearson would not allow Fisher's work to appear in *Biometrika*, the journal he had founded and edited. Fisher had manuscripts refused at *Biometrika* in 1916, 1918, and 1920. As editor, Pearson wrote "I am regretfully compelled to exclude all that I think is erroneous in my own judgment, because I cannot afford controversy" (Box, 1978, p. 83).

In 1919, Fisher was offered an appointment in the Galton Laboratory at University College, London, which he declined as it seemed he would not be able to publish his ideas without Pearson's approval (Box, 1978, p. 82). Instead, Fisher joined the staff of the Rothamsted Experimental Station in October 1919. They had collected many years of data on agricultural experiments that had not been adequately analyzed and Fisher drew on this data in his classic books *Statistical Methods for Research Workers* (1925) and *The Design of Experiments* (1935), each of which went through many later editions. In 1922, Fisher published an article severely criticizing Pearson's chi-square test in the *Journal of the Royal Statistical Society*. Pearson counterattacked in *Biometrika* and the two spent the rest of their life in conflict.

Who replaced Karl Pearson in the Galton Chair at University College, London, after he retired in 1933? Pearson asked that it be anyone but Fisher. The chair was eventually divided into two positions, one in statistics and one in eugenics. The Head of the Department of Applied Statistics, went to Egon Pearson (Karl's son), and directorship of the Galton Laboratory went to R. A. Fisher.

The University of California at Berkeley was interested in hiring a distinguished statistician, and invited R. A. Fisher to give the Hitchcock Lectures in the Fall of 1936. Fisher came and gave the Hitchcock Lectures at UC Berkeley on *The Design of Experiments*. This was supposed to include a 3- or 4-week stay at the campus so that faculty and students could meet informally with him. Raymond Birge, Chair of the Physics department at UC Berkeley who was involved with the recruitment, felt that the visit did not go well. Fisher spent the first week with a friend in San Francisco, rather than at the Berkeley campus. Fisher was seen as so arrogant that the department did not offer him a position. Birge wrote that Fisher was the most conceited man he had ever met, and "that is saying a lot with such competitors as Millikan et al." (Reid, 1998, p. 144).

On November 10, 1937 a letter was sent to Jerzy Neyman (1894–1981) inviting him to teach statistics in the math department at UC Berkeley, and Neyman arrived in the Fall of 1938. He was able to turn the statistics laboratory into a separate Statistics Department by 1955 (Reid, p. 148). Neyman built Berkeley into "the largest and most important statistics center in the world" in the years after World War II (McGrayne, 2011, p. 98). Neyman was a frequentist and Berkeley was an "anti-Bayesian powerhouse" (p. 51). There is a fascinating story of two centuries of controversy between frequentist and Bayesian views of statistics. The complex story is well told in McGanahan (2011), and I won't try to summarize it all here. In her view, Bayesian views were frequently rejected or attacked, but eventually came to triumph (McGanahan, 2011).

Fisher and Neyman also developed different views of statistics, Fisher concentrating on inductive inferences, and Neyman on inductive behavior. It seems that each could not or would not recognize any merit in the other's viewpoint (Lehmann, 2008, p. 168). Lehmann argues that the two approaches are not as incompatible as Fisher and Neyman seemed to believe (p. 168). Important elements are integrated in decision theory as developed by Abraham Wald (1902–1950), in his *Statistical Decision Functions* (1950), which many saw as a magnificent new integrative framework for the field. Wald saw himself as a follower of Neyman, and Neyman was enthusiastic

about the new decision-theory approach, but Fisher strongly objected to it (Lehmann, 2008, pp. 166,167).

Karl Pearson's idea of statistics as a site of intellectual consensus has not yet been reached, with several alternative views of statistics, such as frequentist or subjectivist (Bayesian) views still influential. Historians of statistics have argued that statistics textbooks often paper-over these unresolved theoretical differences (Gigerenzer et al., 1987).

Jerzy Neyman, a leading frequentist, held the view that the theory of probability with which a statistician works is a matter of taste. When asked in 1979 about his view of the alternative Bayesian view, Neyman said "it does not interest me. I am interested in frequencies" (Reid, 1982, 1998, p. 274).

People's viewpoints are not set in stone, and can be shaped by culture, temperament, intelligences, and experience. When approached about being the subject of a biography, Neyman initially said he was not interested, and refused to read a biographical sketch written at his 80th birthday, which he referred to as his "obituary." However, "It's a free country…and if people want to write about me, I can't stop them" (Reid, 1998, p. 1). Constance Reid came to talk with him about his life on Saturdays in 1978. Near his 85th birthday in 1979, she offered him a ride home from his 85th birthday party celebration, and said she needed to begin writing, and would not be coming to talk with him next week, which he seemed to regret. A valuable history of the Berkeley Statistics Department, with biographical sketches, is provided by Erich Lehmann, graduate student at Berkeley since 1941, since 1942 a student of Neyman's, and later chair of the department (Lehmann, 2008).

Does biography and psychological biography alone determine the history of statistics? No. Are there biographical, psychological, and interpersonal relations that serve as *strands* of the whole history? Yes. My argument is that psychological biography is one important strand of the history of statistics, as it is also a strand of the history of psychology and the history of philosophy.

HOLISM AND PSYCHOBIOGRAPHY

Psychobiography can be one dimension of providing a wider holistic context for the psychology of science. A valuable review of multiple meanings of holism is Holism in *Reenchanted science: Holism in German culture from Wilhelm II to Hitler* (1996) by Anne Harrington. In Germany, holism was linked to high humanistic ideals, yet also used by Nazi theorists to claim that Aryan holistic thinking was to be prized over mechanistic, atomistic "Jewish thinking." Ironically, several Jewish immigrants from Germany, including Max Wertheimer (1880–1943) and Kurt Goldstein (1878–1965) were major contributors to gestalt psychology and to holistic neuroscience in the United States. Wertheimer and Goldstein were both important influences on Abraham Maslow (1908–1970) in his development of humanistic or "third force" psychology in the United States. Harrington uses a "multiple biography" approach in unraveling different strands of holistic and analytic research in relation to each other. This multiple biography approach provides a valuable way of analyzing the different

ways in which "the personal, the scientific, and the sociopolitical continually co-construct each other over time" (Runyan, 1998, p. 390).

CONCLUSION

This chapter has touched on psychobiographical examples from the three fields of philosophy, psychology, and statistics. In studies of Bertrand Russell and Ludwig Wittgenstein in philosophy, the work and interpersonal relationships sometimes have surprising personal meanings. In psychology, the works of Henry Murray, Karen Horney, or Paul Meehl have meanings to them, which can be discovered only in individual biography. With Foucault, the denial of the personal is discovered to have not only political meanings, but also personal ones. Even though statistics can be thought of as an objective search for illuminating quantitative analysis, there has been a surprising amount of disagreement and interpersonal conflict running through the history of the field. What are we to conclude? My own view is that psychobiography is a valuable addition to the uses of cognitive and social psychology in the psychology of science. We may need multiple psychobiographical studies of individuals, as well as experimental and statistical analyses at the aggregate level. Along with social, cultural, and political analysis, psychological biography can be part of the answer to the question of: What is really going on here?

ACKNOWLEDGMENTS

REFERENCES

Alexander, I. (1990). *Personology: Method and content in personality assessment and psychobiography*. Durham, NC: Duke University Press.

Barenbaum, N. B. (2005). Four, two, or one? Gordon Allport and the unique personality. In W. T. Schultz (Ed.), *Handbook of psychobiography* (pp. 223–239). New York, NY: Oxford University Press.

Bloland, S. E. (2005). *In the shadow of fame: A memoir by the daughter of Erik H. Erikson*. New York, NY: Viking.

Boring, E. G. (1950). *A history of experimental psychology*. New York, NY: Appleton Century. (Original work published 1929)

Boring, E. G. (1961). *Psychologist at large: An autobiography and selected essays*. New York, NY: Basic Books.

Boring, E. G., & Lindzey, G. (Eds.). (1967). *A history of psychology in autobiography, Vol. 5.* New York, NY: Appleton-Century-Crofts.

Box, J. F. (1978). *R. A. Fisher: The life of a scientist.* New York, NY: Wiley.

Breger, L. (2000). *Freud: Darkness in the midst of vision.* New York, NY: Wiley.

Browne, J. (1995). *Charles Darwin: Vol 1. Voyaging.* Princeton, NJ: Princeton University Press.

Browne, J. (2002). *Charles Darwin: Vol 2. The Power of Place.* Princeton, NJ: Princeton University Press.

Campbell, D. T. (1989). Fragments of the fragile history of psychological epistemology and theory of science. In B. Gholson et al., (Eds.), *Psychology of science: Contributions to metascience* (pp. 21–46). Cambridge, England: Cambridge University Press.

Cronbach, L. J. (1957). The two disciplines of scientific psychology. *American Psychologist, 12*, 671–684.

Cronbach, L. J. (1975). Beyond the two disciplines of scientific psychology. *American Psychologist, 30*, 116–127.

Cronbach, L. J. (1982). *Designing evaluations of educational and social programs.* San Francisco, CA: Jossey-Bass.

Clark, R. A. (1975). *The life of Bertrand Russell.* London, UK: Cape.

Demorest, A. (2005). *Psychology's grand theorists: How personal experiences shaped professional ideas.* Mahwah, NJ: Lawrence Erlbaum Associates.

Desmond, A., & Moore, J. (1991). *Darwin: The life of a tormented evolutionist.* New York, NY: Norton.

Ellenberger, H. E. (1970). *The discovery of the unconscious.* New York, NY: Basic Books.

Elms, A. (1994). *Uncovering lives: The uneasy alliance of biography and psychology.* New York, NY: Oxford University Press.

Elms, A. (2005). Freud as Leonardo: Why the first psychobiography went wrong. In W. T. Schultz (Ed.), *Handbook of psychobiography* (pp. 210–222). New York, NY: Oxford University Press.

Eribon, D. (1991). *Michel Foucault.* Cambridge, MA: Harvard University Press.

Feist, G. J. (2006). *The psychology of science and the origins of the scientific mind.* New Haven, CT: Yale University Press.

Feist, G. J., & Gorman, M. E. (1998). The psychology of science: Review and integration of a nascent discipline. *Review of General Psychology, 2*, 3–47.

Fisher, R. A. (1922). On the mathematical foundations of theoretical statistics. *Philosophical Transactions of the Royal Society of London*, 222, 309–368.

Fisher, R. A. (1925). *Statistical methods for research workers.* Edinburgh, UK: Oliver & Boyd.

Fisher, R. A. (1935). *The design of experiments.* Edinburgh, UK: Oliver and Boyd.

Foucault, M. (1969). *The archaeology of knowledge.* New York, NY: Pantheon.

Foucault, M. (1996). *Foucault live (interviews, 1961–1984).* New York, NY: Serniotext(e).

Friedman, L. (1999). *Identity's architect: A biography of Erik H. Erikson.* New York, NY: Scribner.

Fromm, E. (1941). *Escape from freedom.* New York, NY: Holt, Rinehart & Winston.

Galison, P. (1997). *Image and logic: A material culture of microphysics.* Chicago, IL: University of Chicago Press.

Galton, F. (1869). *Hereditary genius.* London, UK: Macmillan.

Gardner, H. (1993). *Creating minds: An anatomy of creativity seen through the lives of Freud, Einstein, Picasso, Stravinsky, Eliot, Graham, and Gandhi.* New York, NY: Basic Books.

Gay, P. (1988). *Freud: A life for our times.* New York, NY: Norton.

Gholson, B., Shadish, W. R., Neimeyer, R. A., & Houts, A. C. (Eds.). (1989). *Psychology of science: Contributions to metascience.* Cambridge, England: Cambridge University Press.

Gigerenzer, G., et al. (1989). *The empire of chance: How probability changed science and everyday life.* Cambridge, New York, NY: Cambridge University Press.

Gross, P., Levitt, N., & Lewis, M. (Eds.). (1996). *The flight from science and reason.* New York, NY: The New York Academy of Sciences.

Gruber, H. E. (1974). *Darwin on man: A psychological study of scientific creativity.* New York, NY: E. P. Dutton.

Harrington, A. (1996). *Reenchanted science: Holism in German culture from Wilhelm II to Hitler.* Princeton, NJ: Princeton University Press.

Horney, K. (1937). *The neurotic personality of our time.* New York, NY: Norton.

Horney, K. (1939). *New ways in psychoanalysis.* New York, NY: Norton.

Horney, K. (1942). *Self-analysis.* New York, NY: Norton.

Horney, K. (1950). *Neurosis and human growth: The struggle toward self-realization.* New York, NY: Norton.

Horney, K. (1967). *Feminine psychology* (H. Kelman, Ed.). New York, NY: Norton.

Horney, K. (1994). *A psychoanalyst's search for self-understanding.* New Haven, CT: Yale University Press.

Jones, E. (1953–1957). *The life and work of Sigmund Freud* (3 volumes). New York, NY: Basic Books.

Kluckhohn, C., & Murray, H. A. (Eds.). (1948). *Personality in nature, society and culture.* New York, NY: Knopf.

Lehmann, E. L. (2008). *Reminiscences of a statistician: The company I kept.* New York, NY: Springer.

Lindzey, G. (Ed.). (1989). *A history of psychology in autobiography, Vol. VIII.* Stanford, CA: Stanford University Press.

Lindzey, G., & Runyan, W. M. (Eds.). (2007). *A history of psychology in autobiography*, Vol. IX. Washington, DC: American Psychological Association.

Malcolm, J. (1984). *In the Freud archives.* New York, NY: Knopf.

Maslow, A. (1966). *The psychology of science.* New York, NY: Harper & Row.

Masson, J. (1984). *The assault on truth: Freud's suppression of the seduction theory.* New York, NY: Farrar, Straus & Giroux.

McGrayne, S. (2011). *The theory that would not die.* New Haven, CT: Yale University Press.

Meehl, P. E. (1954). *Clinical versus statistical prediction.* Minneapolis, MN: University of Minnesota Press.

Meehl, P. E. (1973a). *Psychodiagnosis: Selected papers.* Minneapolis, MN: University of Minnesota Press.

Meehl, P. E. (1973b). Why I do not attend case conferences. In P. E. Meehl (Ed.), *Psychodiagnosis: Selected papers* (pp. 225–302). Minneapolis, MN: University of Minnesota Press.

Meehl, P. E. (1989). Paul E. Meehl. In G. Lindzey (Ed.), *History of psychology in autobiography* (Vol. VIII, pp. 337–389). Stanford, CA: Stanford University Press.

Meehl, P. E. (1991). *Selected philosophical and methodological papers.* (C. A. Anderson & K. Gunderson, Eds.). Minneapolis, MN: University of Minnesota Press.

Meehl, P. E. (2005). *A Paul Meehl reader: Essays on the practice of scientific psychology.* Mahwah, NJ: Erlbaum.

Menand, L. (2001). *The metaphysical club.* New York, NY: Farrar, Straus and Giroux.

Monk, R. (1990). *Ludwig Wittgenstein: The Duty of Genius.* New York, NY: Free Press.

Monk, R. (1996). *Bertrand Russell: 1872–1920, The Spirit of Solitude.* New York, NY: Free Press.

Monk, R. (2000). *Bertrand Russell: 1921–1970, The Ghost of Madness.* New York, NY: Free Press

Monk, R. (2005). *How to Read Wittgenstein*? New York, NY: Norton.

Moorehead, C. (1993). *Bertrand Russell: A life.* New York, NY: Viking.

Mullins, N. (1973). *Theories and contemporary theory groups in contemporary American sociology.* New York, NY: Harper and Row.

Murchison, C. (Ed.). (1930). *A history of psychology in autobiography, Vol. 1.* Worcester, MA: Clark University Press.

Murray, H. A. (1938). *Explorations in personality.* New York, NY: Oxford University Press.

Murray, H. A. (1959). Vicissitudes of creativity. In E. S. Shneidman (Ed.), *Endeavors in psychology: Selections from the personology of Henry A. Murray* (pp. 312–330). New York, NY: Oxford University Press.

Murray, H. A. (1967). Henry A. Murray. In E. G. Boring & G. Lindzey (Eds.), *A history of psychology in autobiography,* (Vol. V, pp. 283–310). New York, NY: Appleton-Century-Crofts.

Nicholson. I. A. M. (2003). *Inventing personality: Gordon Allport and the science of selfhood.* Washington, DC: American Psychological Association.

Pauly, P. J. (1987). *Controlling life: Jacques Loeb & the engineering ideal in biology.* New York, NY: Oxford University Press.

Pearson, K. (1892/1900). *The grammar of science* (2nd ed.). London, UK: Adam and Charles Black.

Popper, K. (1959). *The logic of scientific discovery.* New York, NY: Science Editions.

Porter, T. (2004). *Karl Pearson: The scientific life in a statistical age.* Princeton, NJ: Princeton University Press.

Reid, C. (1998). *Neyman.* New York, NY: Springer.

Richardson, R.D. (2006) *William James: In the maelstrom of American modernism.* Boston, MA: Houghton Mifflin.

Roazen, P. (1975). *Freud and his followers.* New York, NY: Knopf.

Robinson, F. (1992). *Love's story told: A life of Henry A. Murray.* Cambridge, MA: Harvard University Press.

Roe, A. (1953a). *The making of a scientist.* New York, NY: Dodd, Mead.

Roe, A. (1953b). A psychological study of eminent psychologists and anthropologists, and a comparison with biological and physical scientists. *Psychological Monographs, 67*(2, Whole No. 352).

Runyan, W. M. (1977). How should treatment recommendations be made? Three studies in the logical and empirical bases of clinical decision making. *Journal of Consulting and Clinical Psychology,* 45, 552–558.

Runyan, W. M. (1982). *Life histories and psychobiography: Explorations in theory and method.* New York, NY: Oxford University Press.

Runyan, W. M. (Ed.). (1988). *Psychology and historical interpretation.* New York, NY: Oxford University Press.

Runyan, W. M. (1998). The changing meanings of holism: From humanist synthesis to Nazi ideology. (Review of Reenchanted science: Holism in German culture from Wilhelm II to Hitler.) *Contemporary Psychology, 43,* 389–392.

Runyan, W. M. (2005). Evolving conceptions of psychobiography and the study of lives: Encounters with psychoanalysis, personality psychology and historical science. In W. T. Schultz (Ed.), *Handbook of psychobiography* (pp. 19–41). New York, NY: Oxford University Press.

Runyan, W. M. (2006). Psychobiography and the psychology of science: Understanding relations between the life and work of individual psychologists. *Review of General Psychology, 10*(2), 147–162.

Runyan, W. M. (2008). Henry Alexander Murray. In N. Koertge (Ed.), *The new dictionary of scientific biography* (Vol. 1, pp. 214–219). Detroit, MI: Thomson Gale.

Russell, B. (1967). *The autobiography of Bertrand Russell.* (Vol. 1). Boston, MA: Little Brown.

Schultz, W. T. (Ed.). (2005). *Handbook of psychobiography.* New York, NY: Oxford University Press.

Shadish, W. R., & Fuller. S. (Eds.). (1994). *The social psychology of science.* New York, NY: Guilford Press.

Shapin, S., & Schaffer, S. (1989). *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life.* Princeton, NJ: Princeton University Press.

Shneidman, E. S. (Ed.). (1981). *Endeavors in psychology: Selections from the personology of Henry A. Murray.* New York, NY: Oxford University Press.

Simonton, D. K. (1988). *Scientific genius: A psychology of science.* Cambridge, England: Cambridge University Press.

Simonton, D. K. (2000). Methodological and theoretical orientation and the long-term disciplinary impact of 54 eminent psychologists. *Review of General Psychology, 4,* 13–21.

Simonton, D. K. (2002). *Great psychologists and their times: Scientific insights into psychology's history*. Washington, DC: American Psychological Association.

Skinner, B. F. (1948). *Walden two*. New York, NY: Macmillan.

Smith, R. (1997). *The Norton history of the human sciences*. New York, NY: Norton.

Stolorow, R. D., & Atwood, G. E. (1979). *Faces in a cloud: Subjectivity in personality theory*. New York, NY: Jason Aronson.

Sulloway, F. (1979). *Freud, biologist of the mind: Beyond the psychoanalytic legend*. New York, NY: Basic Books.

Wald, A. (1950). Statistical decision functions. New York, NY: Wiley.

Whitehead, A. N., & Russell, B. (1910, 1912, 1913). *Principia mathematica*, (3 vols.). Cambridge, MA: Cambridge University Press.

Wittels, F. (1923). Sigmund Freud: His personality, his teaching, and his school. New York, NY: Dodd, Mead.

Wittgenstein, L. (1913). Review of P. Coffey, The Science of Logic, *The Cambridge Review*, *34*, 853.

Wittgenstein, L. (1922). *Tractatus logico-philosophicus, with an introduction by Bertrand Russell*. New York, NY: Harcourt, Brace & Company.

Wittgenstein, L. (1953). Philosophical investigations; translated by G.E.M. Anscombe. Oxford, MA: Blackwell.

Wood, A. (1957). *Bertrand Russell the passionate skeptic; a biography*. New York, NY: Simon and Schuster.