

*From the Study of Lives and
Psychohistory to Historicizing Psychology:
A Conceptual Journey*

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How are we to conceptualize relationships between psychology and history? Psychobiography and psychohistory have occupied my attention for many years, with books on *Life Histories and Psychobiography* (1982) and *Psychology and Historical Interpretation* (1988a). However, these interests in psychobiography and psychohistory were preceded by an interest in the study of individual life histories, and later evolved into interests in the history of psychology and in psychology as an historical science. These latter interests transform my conception of what science is, of what scientific psychology is, and how it is related to historical inquiry.

I argue that the “two disciplines of scientific psychology,” experimental and correlational psychology, need to be complemented by a third discipline of historical-interpretive psychology. With the rise of “cognitive neuroscience,” the fate of human science traditions such as psychoanalysis and the study of lives need to be rethought. “Historical science,” as developed by Stephen Jay Gould (1986) in relation to evolutionary biology and historical geology, is a valuable recent resource for bringing the methods and accomplishments of the human science traditions into clearer focus.

This essay is, undeniably, an idiosyncratic, historically contingent, and personal look at relationships between psychology and history. It may, however, not be that different from the idiosyncratic intellectual and personal journeys pursued by others, perhaps including yourself, gentle reader. Out of such diverse and partial perspectives, we attempt to communicate with each other, and to move toward at least an incrementally more adequate understanding of the issues.

Life Histories: Can They Be Studied Scientifically?

At an opening overnight retreat in September 1969, faculty and students in a newly begun program at Harvard in “Clinical Psychology and Public Practice” were going around in a circle reporting on their interests and plans for the future. The program was conceived in the late 1960s, outlined in a planning document in 1967 by Robert W. White, the original director of Harvard’s Clinical Psychology program begun in 1946 and ending in 1967. The idea of “public practice” was intended to contrast with “private practice.” What might clinical psychology be able to contribute to social issues such as those raised by civil rights, by the war on poverty, and by the women’s movement? Could wider social problems be addressed through school systems, mass media, or social-institutional change, and not only through the treatment of individuals in long-term psychotherapy?

Senior faculty like John M. Shlien, David C. McClelland, Robert Freed Bales, Chester Pierce, Richard M. Jones, and others could report on careers of research, practice, and scholarship as well as current plans. For the racially and ethnically diverse class of 11 men and women entering the graduate program, we had to rely more heavily on hopes, plans, and dreams for the future. My plan was to explore the possibilities for the systematic or scientific study of life histories. What were the possibilities of scientifically studying the course of lives? This question seemed both of personal interest and relevant to evaluating the effects of social intervention programs.

In graduate school, two of those most supportive of my interests in the study of lives, Henry A. Murray and Robert W. White, were retired. As I came to see it over the years, they had somewhat different interests in the study of lives. For Murray, the study of lives overlapped with a romantic project of including the deepest human experiences within psychology, of Jung, Melville, himself, and others. For Robert White, who was powerfully influenced by working within Murray’s tradition, yet had different priorities, the study of lives overlapped more with the study of normal growth and development (1952, 1972), with the uses of lives in teaching abnormal psychology (1948), and with life history interviewing as a way of relating personally with students and others. From their different stances, I greatly appreciated their support of and criticism of my early work in this area.

By the summer of 1970, I had completed a paper on “A Science of Lives,” which I sent to Murray, White, and several others. Murray responded positively to the glimmers of promise in this document, when there was probably much more that could have been criticized. I found that his encouragement, vitality, and enthusiasm inspired me in some way almost every time we met, from that first meeting in 1970 to a final visit in December 1987, about 6 months before his death at age 95. Compared with other psychologists, Murray seemed more

alive, to have greater depth and humane learning, greater awareness of inner experience, greater wit and expressiveness, and greater sensitivities to the nuances of social interaction (Runyan, 1994). He could also be self-centered, jealous, and harshly critical of himself and others. He seemed to me a person of unusual stature, who gave me a sense of what it might have been like to know Freud or Jung, not as influential as they in his publications, but as charismatic and eye-opening in personal interaction as anyone I had met.

Robert White was supportive in different ways: thoughtful, reserved, even patrician, yet responsible and helpful, taking time late in his life to write appreciative comments for my books in 1982 and 1988. I was fortunate to have the opportunity to talk more with White at the end of his life, and to rethink his relations with Henry Murray and the personal meanings that the study of lives had for him, helped by Jim Anderson's biographical interviews (2000). A sabbatical and leave in 1999–2001 provided the opportunity to talk with White a number of times, even to stand in for him to receive and convey to him several awards, including the Henry Murray Award from APA, before he died in February 2001 at 96.

In graduate school, I was also seriously concerned with methodological and philosophical issues in the study of lives, in relation to debates in the philosophy of science (Hempel, Nagel, Popper, Kuhn, and others), and in the philosophy of history (Dray, M. White, Danto, Berlin, and others). William H. Dray visited Harvard in the summer of 1973 and taught an excellent course in the philosophy of history (Dray, 1964), which seemed to have substantial implications for the study of lives, so I drew on that literature in my dissertation (1975) and in *Life Histories and Psychobiography* (1982).

When I began graduate school, I was unaware of the extent and nature of the opposition to the study of individual lives. In graduate school from 1969–1975, I got more of a feel for it. My interests in conceptual and methodological issues in the study of individual lives were not warmly received by some of the faculty. David McClelland wrote me a letter on May 25, 1971, the end of my second year of graduate school, saying that these philosophical interests were not suited to the program. "So, I would urge you strongly to leave Harvard before you waste more time here, your time and our time." I declined the offer. I tried to understand what there was about my approach that so disturbed him, and went to talk with him about it, but he refused. Years later, in a chapter on "Personal Sources of My Intellectual Interests," McClelland (1984) said he used to have philosophical debates with his father, a Methodist minister and college president, which would start out objectively, but "often became very heated." McClelland wrote that he "came to hate these family arguments" and sought to put as much distance as possible between himself and his family. These debates had "profound effects" as his father could take either side of an issue, and "never once admitted that he was wrong, no matter how powerful my arguments were. . . . I feel certain

that my interest in empirical science came in part from my desire to find incontrovertible facts that could not be disputed” (p. 3). I wondered if these earlier experiences with philosophy were related to our interactions.

Fortunately, it was possible to assemble a dissertation committee that was vastly more sympathetic to conceptual and methodological issues in the study of lives. The committee included Edwin N. Barker (chair), Lawrence Kohlberg, Alexander Leighton, David F. Ricks, and Zick Rubin. J. Milton Yinger (1965) had also been an important earlier influence at Oberlin College.

My dissertation on “Life Histories: A Field of Inquiry and a Framework for Intervention” (1975) was a personal response to questions emerging in college and graduate school: What to study? How to live? And, How to help other people?

As a post-doc at UC Berkeley, I had a chance to learn more about personality psychology, then struggling with Walter Mischel’s *Personality and Assessment* (1968), which had critiqued trait and psychodynamic psychology in favor of experimental social learning theory. This led to a growing consensus on the value of person-situation interactionism, a concept I used in an early article on “The Life Course as a Theoretical Orientation: Sequences of Person-Situation Interaction” (1978). As a visiting lecturer at UC Santa Cruz, I had many enjoyable discussions with Elliot Aronson, although disagreeing about the place of experimental methods in psychology.

Experimental social psychologists had a way of looking at the world in which there were a hierarchy of methods, starting with case studies to generate hypotheses, then correlational studies and, finally, controlled experimental studies to test general causal relations. This seemed true, but only part of the story.

In perhaps the most influential study in the history of experimental social psychology, Milgram (1974) did research on factors that affected “obedience to authority,” which is often discussed in relation to the case of Adolf Eichmann. As head of the Jewish department in the Reich’s Main Security Office, Eichmann was involved in managing transportation to the extermination camps. At his trial for war crimes in 1961, Eichmann argued that he had never wished to harm a single Jew, but felt impelled to obey the orders of his superiors. “I was in the iron grip of orders,” he argued, and personally, he considered “the whole solution by violence to be a dreadful thing” (Hausner, 1966, p. 366). Before being executed by hanging, Eichmann’s last words were “I had to obey the rules of war and my flag” (p. 446).

In his research on obedience to authority, Milgram (1974) found that more than 60 percent of normal subjects could be induced to administer what they believed to be extremely painful or even life-threatening shocks to innocent subjects in a learning experiment. What relationships, if any, do these experiments have to our understanding of Eichmann? Is it fair to suggest that he was not a bad man, but that like the subjects in the experiments, he was forced to perform destructive actions against his inner objections?

As for the view that he was just following orders, which forced him to violate his conscience, Eichmann said in 1957 in a tape-recorded talk with a Dutch Nazi journalist, "I could make it easy for myself. I could claim it was an order I had to carry out because of my oath of allegiance. But that would be just a cheap excuse, which I am not prepared to give" (Hausner, 1966, p. 11). "I was not just a recipient of orders. Had I been that, I would have been an imbecile. I was an idealist" (p. 11). When ordered to be lenient, Eichmann sometimes protested. In 1944, Hitler authorized that 8700 Jewish families and 1000 children be allowed to emigrate from Hungary so that the Hungarian government would accede to the deportation and execution of the 300,000 Jews remaining in Budapest. Eichmann was outraged at this leniency, appealed to Himmler, and got the order reversed.

The general point is that interpretation can draw on general theories, but must also draw on extensive idiographic research about the person in question. Academic psychology had developed in such a way that quantitative and experimental methods were prized, but case studies were often devalued or marginalized. In the heyday of experimentation, in one of the most widely used methodology texts, Campbell and Stanley (1966) stated that "one-shot" case studies "have such a total absence of control as to be of almost no scientific value. It seems well-nigh unethical at the present time to allow, as a thesis or dissertation in education, case studies of this nature (i.e., involving a single group observed at one time only)" (pp. 6–7). In such a context, what is one interested in the study of individual lives to do?

In *Life Histories and Psychobiography: Explorations in Theory and Method* (1982), I attempted to review and respond to major criticisms of the study of individual lives, and to suggest appropriate criteria and methods for evaluating and improving in-depth studies of individual lives.

The four objectives of the book were:

- (1) to provide a rationale for the study of individual life histories within psychology and the social sciences
- (2) to examine methodological problems that arise in describing and interpreting the course of events in individual lives
- (3) to suggest ways of conceptualizing the causal and probabilistic structure of the life course, and
- (4) to critically examine those methods used in the in-depth study of individual life histories, namely, the case study, idiographic and psychobiographical methods.

I have written extensively about methodological debates in psychobiography (Runyan 1981, 1982, 1983, 1988a, b, 1997), and won't try to summarize all that here, except to recommend the 1982 and 1997 publications to those interested in methodological issues. Related issues have also been investigated in the

philosophy of history, in methodology of the human sciences, and in narrative studies (as in the series edited by Josselson, Lieblich, and McAdams, 2003).

Like many in my generation, I had been exposed to philosophy of science in graduate school through reading Carl Hempel, Ernest Nagel, Karl Popper, Abraham Kaplan, Thomas Kuhn, and others. I so much admired Hempel's writing (1965), that I thought of making a trip to Princeton to visit him, so it was a thrill to meet him when he visited Berkeley in 1977, and to audit his course on philosophy of science. His style of thinking influenced the article, "Why Did Van Gogh Cut Off His Ear? The Problem of Alternative Explanations in Psychobiography" (1981). I was flattered to learn from Hempel that he subsequently included the article in a later philosophy of science course. I greatly admired his combination of intellectual clarity and human warmth.

Psychobiography and Psychohistory

The bulk of work in psychobiography has undoubtedly been done using psychoanalytic theory, starting with Freud and followers such as Ernest Jones or Karl Abraham who saw psychobiography as part of "applied psychoanalysis." However, since at least the 1970s, many have defined psychohistory to include the use of any form of psychology. Psychoanalytic psychobiography could, in principle, be complemented by phenomenological psychobiography, behavioral psychobiography, or cognitive psychobiography (Runyan, 1988a).

If behaviorism, to take one example, was such an influential general approach in psychology, how had it been used in psychobiography? I agreed to give a talk on "Alternatives to Psychoanalytic Psychobiography" for a conference at Stanford on "History and Psychology: Recent Studies in the Family, Biography, and Theory," organized by historian of psychoanalysis Nathan Hale in 1982.

What could be found in behavioral psychobiography? There were a few examples, such as Wolpe and Rachman's (1960) behavioral critique and reinterpretation of Freud's Little Hans case. Freud (1909) provided an oedipal interpretation of five-year-old Hans's fear that a horse would bite him, while Wolpe and Rachman argued that there is no persuasive evidence for this interpretation, and that the phobia could be explained in terms of learning theory, in that Hans saw a horse fall down and had several other frightening experiences related to horses shortly before the phobia developed. Their article was followed by debates about the relative merits of psychoanalytic and behavioral interpretations of the case. Another example was an article on "Ben Franklin the Protobehaviorist I: Self-Management of Behavior" (Mountjoy and Sundberg, 1981), arguing that Franklin's efforts to arrive at moral perfection were an early example of behavioral self-management, but compelling examples were difficult to find.

By far the most prominent and extensive example was Skinner's three-volume autobiography. Skinner (1967) had said earlier that "whether from narcissism or

scientific curiosity, I have been as much interested in myself as in rats and pigeons. I have applied the same formulation, I have looked for the same kinds of causal relations, and I have manipulated behavior in the same way and sometimes with comparable success" (p. 407).

As the time of the Stanford conference approached, I grew increasingly anxious, as I was to be in a session chaired by Ernest Hilgard and discussed by Robert Sears, and I wanted to have a more substantial body of work to review. I wondered if Skinner knew of behavioral biographies or autobiographies in addition to his own, or had thoughts on their infrequency. When I wrote him on April 6, 1982, I mentioned the conference, reviewed a few of the studies cited above, and told him how difficult it was to find examples of behavioral psychobiography.

I asked him why there hadn't been more work along this line. "Is it, perhaps, because behaviorists are unwilling to speculate from such limited data? Because it just hasn't been tried? Because behavioral theory is extremely useful for the design of experiments, for clinical intervention, and for self-management, but not for this particular kind of task?" In attempting to frame the issues in a way that might engage him, I asked, "Is psychobiography a domain best left to psychoanalysts, who have so far dominated work in the field, or one in which behavioral psychology has a significant contribution to make?"

The question did not engage his thinking on the issue as I had hoped, although he did briefly respond. On April 19, 1982, Skinner's response was:

Dear Dr. Runyan:

I have no strong confidence in the psychobiography of my colleague Erik Erikson nor am I well read in related fields. I am afraid I can give you no help.

Yours Sincerely,
B. F. Skinner

I presume Skinner was enormously busy, but I imagine that if he knew offhand of other examples of behavioral biography or autobiography, he might have mentioned them. For me, the question remains: What are some of the features of behaviorism or experimental psychology that affected its use or lack of use in historical interpretation? How are we to account for the growth of psychoanalytic approaches to psychohistory, and the relative lack of development of behavioral, cognitive, phenomenological, social, and other approaches? One possible interpretation, out of a variety of others (Runyan, 1988a), is that perhaps psychoanalytic theory provides a set of conceptual tools that can be used in flexible and idiographic ways in interpreting patterns of evidence found in a wide variety of individual cases. Critics of psychoanalysis such as Karl Popper or Fred Crews have said that this apparent strength is actually a weakness, in that psychoanalytic theory can explain any outcome or its opposite. Some say that this is a fatal flaw of the theory, but I disagree. Evolution can explain both

how species come into being and how they become extinct. Is this a fatal flaw of evolutionary theory? The theory identified processes of variation and natural selection which can be used to explain diverse outcomes and their opposites. Similarly, the processes in psychoanalytic theory can be used to explain a wide array of outcomes. The adequacy of interpretation needs to be assessed on multiple other grounds, such as consistency with the full range of available evidence, survival of tests of attempted falsification, consistency with more general knowledge, and credibility relative to alternative interpretations (Runyan, 1981, 1982, 1988b).

Psychohistory

At the 1982 Stanford Conference on “History and Psychology: Recent Studies in the Family, Biography and Theory,” Nathan Hale invited psychologists and historians from Stanford, such as Ernest Hilgard and Robert Sears in psychology, Alexander Dallin and Peter Paret from history, and others from around the country, such as Peter Gay, Peter Lowenberg, Jean Strouse, Robert Tucker, Fred Crews, Fred Weinstein, and Kenneth Craik and myself from UC Berkeley.

In 1984, I agreed to edit a book on the conference, which led to a number of years of work on an Introduction on “A Historical and Conceptual Background to Psychohistory” and a Conclusion on “Reconceptualizing the Relationships Between History and Psychology.” The following discussion is adapted from these chapters.

Historical summaries of psychohistory often begin with Freud on Leonardo da Vinci, but there are ways to review the uses of psychology in history that begin earlier. Frank Manuel (1972), for instance, begins his discussion of the uses of psychology in history with Giambattista Vico and his *New Science* in 1725, and traces the history through Michelet, Herder, Hegel, and Dilthey, before reaching Freud.

I made an effort to trace the quantitative growth of psychohistorical literature from pre-1920 to 1980, with a major increase beginning in the 1960s. In 1925–1929, there were 2 dissertations, 10 books, and 20 articles that could be identified by title as psychohistorical; in 1965–1969, 12 dissertations, 65 books, and 150 articles; and in 1975–1979, 65 dissertations, 122 books, and 428 articles.

Work in psychohistory can be conceived as developing not within one unified stream, but as evolving within several partially independent traditions and lines of influence. The semiautonomous subtraditions include those in psychoanalysis and psychiatry, academic psychology, history, political science, literature, the deMause group, and an assortment of others from religion, education, social work, and other fields. Details of work in each of these traditions are reviewed in Runyan (1988a).

Historians often have reservations about the uses of psychology and an overemphasis on the study of individuals. “The spirit of the modern period, with

its new respect for collectivism, with its hatred of all individualism, has cried out with great conviction, 'It is not heroes, the few, who make history; it is groups, classes, peoples, races'' (Huizinga, 1972, p. 299). From this perspective, if individuals are not so important, then their internal psychological processes are not so important either. Perhaps there is little need for historians to be concerned with the details of psychological processes of perception, interpretation, unconscious motivation, experiential histories, and psychodynamics.

On the other hand, I argue that psychological processes are important for understanding the flow of historical events and processes at six different levels of analysis. Six different social system levels relevant to understanding historical processes may be conceived as persons, groups, organizations, institutions, nations, and, finally, international or intersocietal relationships. Each of these 6 system levels may be studied at 3 levels of aggregation, of single entities, groups, or populations, yielding a matrix with 6 rows and 3 columns suggesting 18 different types of psychohistorical analysis. In Nazi Germany, psychohistorical attention has been directed toward individuals, such as Adolf Hitler, Heinrich Himmler, Joseph Goebbels, Herman Goering, Adolf Eichmann, and others; at groups of individuals, such as those voting for the Nazi Party or those engaged in resistance movements; or at populations of individuals, such as the Nazi youth cohort. Examples of psychohistorical literature on Nazi Germany covering topics in each of these six system levels and three levels of aggregation within each level are reviewed in Runyan (1988a).

Historicizing Psychology

"Historicizing psychology" can refer either to the history of psychology, or to conceptualizing psychology as an historical science, which is the focus of this discussion. My relations with the history of psychology and the history of science will have to be discussed elsewhere. Academic psychology often conceives itself as starting with experimental psychology, with Edwin G. Boring's (1929) history of experimental psychology, long the canonical history of the discipline. It became clear over the years, however, that Boring appreciated only part of Wundt's legacy, his experimental work, while underplaying Wundt's cultural-historical psychology, published in 10 volumes from 1900 to 1920.

I was on sabbatical at Harvard in the spring of 1986, and again in the spring of 1990, and was much influenced by Stephen Jay Gould's arguments for the importance of "historical science" in both evolutionary biology and in historical geology. In auditing his lectures and occasionally discussing the issues with him, I continually felt excited at the sense of this having enormous implications for the social sciences and for our conception of psychology. I was affected particularly by Gould's *Time's Arrow, Time's Cycle* (1987), by *Wonderful Life: The Burgess Shale and the Nature of History* (1989), and an earlier article on

why history matters (1986). Most recently, these themes were elaborated in Gould's *The Structure of Evolutionary Theory* (2002), completed shortly before his death in May 2002.

What is Gould's conception of historical science? Gould (1989) argues that we often hold an oversimplified conception of "the scientific method," with images of a scientist in a "white lab coat twirling dials in a laboratory—experiment, quantification, repetition, prediction, and restriction of complexity to a few variables that can be controlled and manipulated" (p. 277). These are powerful procedures, but they are not adequate for explaining all of nature, particularly not for explaining complex sequences of historical events. Gould argues that "many large domains of nature—cosmology, geology, and evolution among them—must be studied with the tools of history" (p. 277).

Historical science is concerned with explaining complex sequences of historically contingent events and processes, which often cannot be predicted, exactly replicated, nor subsumed under general laws. If, for example, we want to understand why dinosaurs became extinct about 65 million years ago, our analysis depends on the discovery in the late 1970s that one or more asteroids hit the earth, changed its climate, and led to the extinction of dinosaurs, with evidence embedded in geological strata of the time.

Consider the evolution of humans. Gould argues that the whole history of life depends on historically contingent sequences of events. If those comets that hit the earth had gone into different harmless orbits, then "dinosaurs still rule the earth, precluding the rise of large mammals, including humans" (p. 280). Given the multiple contingencies of evolution, capable of cascading down many different paths, "We came *this close* (put your thumb about a millimeter away from your index finger), thousands and thousands of times, to erasure by the veering of history down another sensible channel" (p. 289).

Gould argues that Darwin was "the greatest of all historical scientists" (p. 282), and he contrasts this with the "hypothetico-deductive" conception of science (so central to experimental science) given classic formulation in Carl Hempel's (1965) *Aspects of Scientific Explanation*.

How do we bring into focus the different perspectives of Hempel (logical empiricism), Dray (1964) (philosophy of history), and Gould (advocate of "historical science")? In the history of psychology, the experimental psychology championed by Titchener and Boring won out over the human science psychology of Dilthey and the cultural-historical side of Wundt. The logical empiricism of Hempel, Nagel, and others was sometimes used as an ally of experimental psychology. Gould's argument for historical science can be seen as an ally of "human science" traditions such as psychoanalysis, phenomenological psychology, and the study of lives, making clearer appropriate standards of scientific rigor.

The relations between "natural science" and "historical science" are formulated in a useful way in Harvard's "Core Curriculum" (1981–1982), with

undergraduates required to take electives in both Science A and Science B. Science A courses “are intended to introduce students to areas of science dealing primarily with deductive and quantitative aspects and to increase the student’s understanding of the physical world.” For example, Science A-16 is “Modern Physics: Concepts and Development,” or Science A-25 is “Chemistry of the 20th Century”.

Science B courses “are intended to provide a general understanding of science as a way of looking at man and the world by introducing students to complex natural systems with a substantial historical or evolutionary component.” For example, Science B-15 is Evolutionary Biology, taught by E. O. Wilson, and Science B-16, History of the Earth and of Life, taught by Stephen Jay Gould. A popular Science B course added later was Science B-29, Human Behavioral Biology, long taught by Irven DeVore. Historical science appears in biological, physical, and social sciences. The implications for the study of lives and for the human science traditions in psychology cry to be spelled out.

Conclusion: Psychology as an Historical Science?

To briefly review, I began graduate school with the idea of applying the scientific method to the study of life histories. In the course of graduate school, encounters with the ideas of Carl Rogers and of humanistic psychology, as well as with literature in the philosophy of history, led me to temper these views. A second phase was in exploring methodological problems in psychobiography and in psychohistory (Runyan, 1982, 1988a, b). A third strand was learning about “historical science” as conceptualized by Gould and by Ernst Mayr, and seeing ways in which the study of lives shared many features of historical science. A major interest in recent years has been in the history and biography of psychology, analyzing both “hard” and “soft” or human science traditions in their personal, social and cultural contexts.

Within biology departments, there was a danger in the 1960s and 1970s of evolutionary and organismic biology being replaced by molecular biology. As described by E. O. Wilson, some felt “There was only one biology, and it was molecular biology.” At one point seven of eight appointments in a row in biology at Harvard were in molecular or cellular biology. To preserve organismic and evolutionary biology, the biology department was eventually divided into two groups: biochemistry and molecular biology and organismic and evolutionary biology.

Within psychology departments, there has been an enormous rise in cognitive psychology and in cognitive neuroscience. Is there a similar danger that the “soft” human science traditions and concerns with persons, lives, experience, and social and cultural contexts are being neglected? To preserve the human science traditions and methods, should psychology departments be divided, like biology departments into Science A (cognitive neuroscience) and Science B (human and historical science) divisions?

Is biology about molecular biology? Or about evolution? Clearly both. Is psychology about cognitive neuroscience? Or about human behavior and experience in social, cultural, and historical contexts? Clearly both.

One influential statement on historical psychology was Kenneth Gergen's (1973) "Social Psychology as History," later developed into an edited volume, *Historical Social Psychology* (Gergen and Gergen, 1984), to which I contributed a chapter (Runyan, 1984). Lee Cronbach (1957) proposed the influential phrase of "the two disciplines of scientific psychology," referring to experimental and correlational psychology, which is still widely used. I would like to suggest the possibility of (at least) a third discipline of scientific psychology, namely, historical-interpretive psychology. These methods are concerned with advancing our knowledge and understanding of particular sequences of events and experiences, as in individual lives in social and cultural contexts. These may be the lives of clients, of research subjects, of historical figures, or ourselves (Runyan, 1997, 2002).

The "Big Three" of twentieth century research methodologists in psychology all developed an appreciation for historical-interpretive methods. Donald Campbell did major work in experimental and quasi-experimental design (Campbell and Stanley, 1966), but later retracted what he called "his dogmatic disparagement of case studies," and moved to greater appreciation of ethnographic and interpretive methods. Lee Cronbach (1957) moved from advocacy of the two disciplines of scientific psychology (experimental and correlational) to study of person-situation interactions, and finally to appreciation of historical-interpretive methods in evaluation research. The work of Paul Meehl may also be conceived as including both quantitative-psychometric-taxometric methods and historical science methods (Dahlstrom, 1991).

This conceptual journey has gone from attempting to apply "the scientific method" to the study of individual lives; to the uses of psychology in biography and history; to reconceptualizing science as including both nomothetic science and historical science; and finally, to reconceptualizing the discipline of psychology itself as including the three disciplines of experimental, correlational, and historical-interpretive methods. The conceptual journey is not complete, as much remains to be discovered about the relationships between experimental, quantitative, and historical-interpretive methods and traditions.

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