HISTORY OF PSYCHOLOGY AND HISTORY OF SCIENCE: REFLECTIONS ON TWO SUBDISCIPLINES, THEIR RELATIONSHIP, AND THEIR CONVERGENCE

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I first met Josef BROŽEK in June 1968, at the University of New Hampshire. He was in Durham as Director of an eight-week Summer Institute in the History of Psychology (1), one of many similar programs in a wide variety of fields sponsored by the U.S. National Science Foundation during that decade, when an economic boom and an academic boom reinforced each other. Already a scholar distinguished for his work in many areas of psychology, he was a young and vibrant 55 years old that summer (2). In gossiping with several psychologists before the Institute began, I had been told of a tall, bright man with a startling sheaf of pure white hair and had heard admiring descriptions of his dynamic and forceful character. But little did I realize then the role that the Institute, and Dr. BROŽEK, were to play in my life and career.

That June had just marked the completion of my second year of graduate study in history of science and technology at Case Western Reserve University. My work there had focussed on "mainline" problems in the field and had thus been concentrated in the history of the physical sciences, with some work in the history of biology. Indeed, during the summer of 1968 I was supposed to be completing an M.A. essay on the vortex-atomic theories of W.J.M. RANKINE, a 19th-century Scottish physicist and engineer. But traditional history of science topics seemed at times too narrow, so I had

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begun working with Robert C. DAVIS -one of the great men of the western world, and a man to whom I and our field both owe much- in developing a Ph.D. qualifying field in the history of the social and behavioral sciences. When Bob DAVIS told me of the Institute, and that he was planning to attend it, I looked to it as a chance to extend my acquaintance in an area in which I had much to learn. But before I could do so, I had to overcome several obstacles that I had brought with me to New Hampshire that summer. And a review of some of them will lead directly into the topic of this essay.

The explosive growth of academia in the 1960s had seen, among many other developments, the emergence of history of science as a self-identified and externally recognized scholarly discipline (3). To be sure, individuals had been writing histories of science for centuries, journals specializing in the field had been appearing for decades, the (U.S.) History of Science Society had been founded in 1924, and from the 1940s one could earn a Ph.D. in the area at a growing number of American universities. But before the 1950s, most of those who wrote history of science were scientists who identified themselves primarily with their scientific fields, and who concentrated on recording discoveries and inventions, firsts and mosts, and precursors and unacknowledged influences. These men -there were very few women- seemed concerned primarily with tracing the emergence of science, from what they knew positivistically to be the falsity of the past, into the truth of their own work. No wonder, then, that active philosophers of science and working historians found this earlier work irrelevant to their concerns and almost childish (4).

But by the mid-1960s a new generation has emerged as the field's "establishment". Composed primarily of individuals (still dominantly male, but less so) who had been trained as physical scientists (often through the M.S. and sometimes through the Ph.D.) and who had then sought graduate training in history of science. This first large "professional" group of historians of science tended to deride the work of their predecessors as "amateurish" and "antiquarian" -even while building on it- and looked to revolutionizing their field. That is, they sought to understand the development of past science rather than offer a chronology of past achievement. In this goal, they were fortunate in having access to the work of Alexandre KOYRÉ, the distinguished French historian of ideas, which indeed probably led many of them to the field. KOYRÉ's work concentrated on the evolution of past scientific ideas, viewing them narrowly,and explicitly disregarding the priorities of discovery and other such "antiquarian" issues that interested the generation before him. Most who followed his lead focussed their attention on the development of the ideas in the physical sciences in Europe from the Renaissance through 1900, though all recognized that the history of science could not ignore the growth of evolutionary thought for long. This "internalist" vision of the history of science, focussing as it did on the scientific ideas as they had developed in the past, brought the field closer to traditional history in that it began to concern itself with the past in its own terms, rather than simply in terms of issues of interest to current science. But its practitioners, in focussing almost entirely on ideas in isolation, often implicitly adopted the work of theoretical physicists as their model of scientific activity, and through the 1960s analyses of past scientific experiments by historians of science were exceedingly rare. This phenomenon often emerged as what psychologists have come to

call (when seen in their own attitudes) "physics envy", and it did not bode well in the 1960s and 1970s for relations between history of psychology and history of science at large.

Perhaps the most explicit example of this attitude occurred at a session on employment opportunities in the field, held at the 1971 History of Science Society meeting in New York. The end of the inflation of academia in the early 1970s brought a mass of young historians of science to hear a panel of leading older scholars reminisce about the difficulties they had faced in finding their first jobs, and admonish their audience (to boos and hisses) to remember that things were tough all over. They also fielded questions, and Tom CADWALLADER, then a young psychologist interested in the history of his science, asked about jobs in the history of psychology. The eminent historian of physics who had comandeered the microphone explicitly referred the question to Allen DEBUS, the distinguished historian of Renaissance alchemy and astrology, "who after all is an expert in the history of the pseudo-sciences". In the late 1960s, then, historians of science typically looked down on the history of psychology as being peripheral to their interests. At best, it focussed on a field that they believed developed outside of the mainstream of scientific ideas, and had matured at a time (the 20th century) and place (America) away from the locus on which they concentrated their attention. And it was practiced typically by psychologists, a sure sign of its "amateur" status. At worst, historians of science viewed the history of psychology in the same light that PLATO saw the passions: as a distraction that distorted and got in the way of a rational understanding of the ideal. In all, it was not a situation to encourage the easy exchange of ideas.

Indeed, this situation influenced me directly and deeply. That summer I came to New Hampshire having unconsciously unsubtly, internalized most of these attitudes and uncritically. The Program in History of Science and Technology at Case Western Reserve was much more open than the typical history of science department of the period; after all, it included as a major component a strong focus on the history of technology -unique at the time- which counteracted overemphasis on idealized science. In addition, if its leaders placed too much stress on a Koyréan approach in the infamous seminar on the historiography of science, they did so to remove any vestiges of antiquarianism that we graduate students might still have had. All my teachers would have been shocked had I expressed the attitudes I've sketched here so baldly. And Bob DAVIS's presence among this group of graduate professors further illustrates how the Program was anything but closed. But my recently completed undergraduate training had been in electrical engineering, a field more firmly convinced of its positivistic validity than almost any science, and I was a very young -I won't note how young- student who had just been introduced to the joys and rigors of scholarship, and who thus overidentified himself with what he sensed unconsciously to be his teachers' beliefs (5). As a result, I approached the Institute standing ready to question all traditional work in the history of psychology, and I was particularly prepared to criticize the work in the field by psychologists. These attitudes came through clearly in my work at New Hampshire that summer and indeed, when I was asked to recommend to other participants one book that would give some idea of what historians of science did that was so different from traditional work in the

history of psychology, I listed an idealistic philosophy of history (6). Others remember my attitudes (7), and they're not something I'm proud of.

But I must have been more open than I remember. After all, I was interested enough in the field to attend the Institute, and I had been working with Bob DAVIS for over a year, and his subtle and unpressured influence -stressing the wealth of approaches available to understand the past -had begun to reveal to me just how much I had to learn. And at the Institute, I -and all of my colleagues- did learn much about the many ways in which history can be written. Indeed, this concern for the historiography of psychology pervaded the sessions, as Josef BROŽEK used his position as Director of the Institute to shape it. His interests in historiography had been long standing -and indeed continues to day (8)- and he believed that the goals of the Institute would be best served if a thread of concern for historiography would run through all of its programs. For the psychologists among us, Dr. BROŽEK arranged for the Institute to open with a series of lectures by Henry GUERLAC -the distinguished Cornell historian of science and biographer of LAVOISIER (9)- which served to illustrate the Koyréan approach to the history of science that I've discussed here. I well remember several other participants in the Institute describing how these presentations revealed to them what history of science could be. Also influential were continuing presentations by Robert I. WATSON, the Institute's Associate Director, which explicitly focussed on different historiographic issues. And several of us surreptitiously circulated a highly critical review essay that hinted -in its title at least- that "scholarship" and "the history of the behavioral sciences" did not typically involve each other (10).

In all, this concern for illustrating the many ways in which the past can be recovered, investigated, and presented -all stimulated by Josef BROŽEK- influenced all who attended it and I, at least, had my eyes opened. The concerns of historiography flowed through all of our conversations, and I remember in particular several talks with John SULLIVAN that did much to show me the limitations -as well as the power- of an overemphasis on a highly narrow focus on past science. The formal speakers at the Institute, whose participation Dr. BROŽEK had arranged, were also important for the way in which they broadened the perspectives of all the participants, including me. David KRANTZ, for example, showed well just how psychologists, using techniques not traditionally employed by historians, could uncover much about past science (11). And Julian JAYNES's highly erudite lectures stressed a plurality of approaches (12) and showed well just why he argued explicitly that historiographers should "let a thousand flowers bloom". These presentations also revealed to me the full range of work in the history of psychology that had been done by psychologists -such as Solomon DIAMOND and others of his generation (13)- that was, and is, excellent by any standard. By the end of the summer, I was a convert to the field, and had decided to concentrate my activity within the history of science on the history of psychology. Over a pitcher of beer one afternoon I convinced Bob DAVIS to take me on as a doctoral student, and agreed to write my dissertation on the work of James MkKeen CATTELL (14). And though I've expanded my focus beyond CATTELL and have written on other subjects in the field (15), my work as a historian of science has clearly revolved around the history of psychology.

For this reason I was honored when Josef BROŽEK asked me to review, for his Festschrift, the relations between the history of psychology and the history of science in general. In tracing my ties to Dr. BROŽEK, I've begun to do so. But behind this relationship stands a fact unique to the history of psychology, at least as it is practiced today. That is, each science has always had a few of its practitioners who've worked in the history of their science and while several of these -e.g., Martin KLEIN in physics and Joseph FRUTON in biochemistry -do history that could not be better, the work of most reflects the uncritical antiquarian concern for firsts and mosts. To be sure, the history of psychology is not free of such problems, unfortunately, and at most meetings where papers in the field are welcome, psychologists can still be heard reporting on previously unknown precursors or analyzing just how psychology fits into this or that artificial classifactory scheme of the sciences. But such presentations and articles are becoming rarer, and the most remarkable aspect of the history of psychology today is the extent to which excellent work in the field is done by psychologists. Certainly no other science can claim that its practioners do as much or, especially, as good work, despite an active Division of the History of Chemistry of the American Chemical Society, and the emergence of similar groups for physics, electrical engineering, and other fields (16). The American Psychological Association's Division (26) of the History of Psychology antedates them all, and the Archives of the History of American Psychology at the University of Akron is one of the oldest such disciplinary centers. The history of psychology as a branch of psychology is thriving, and those who approach it in this way have every reason to be proud of what they are doing.

The reasons for this state of affairs are not hard to find, though even to sketch them fully is beyond the goals of this essay. A major stimulus for work in the history of psychology is the full range of institutions that support it. These include the APA Division and the Archives already cited. Other important institutions include the *Journal of the History of the Behavioral Sciences,* and Cheiron: The International Society for the History of the Behavioral and Social Sciences, which forms an important institutional base for historians and other non-psychologists who are interested in the field. Also important are the graduate programs which train psychologists for specialization in the history of psychology. In particular, the program at the University of New Hampshire has done much to help shape the field, both under its original Director, Robert I. WATSON, and under his successors, William R. WOODWARD and David LEARY. This point leads of course to a second important stimulus for the field's growth: the leadership of individuals who have influenced the way in which much of the history of psychology has been, and is being,written. Many of these men and women are connected with the institutions I've mentioned -Robert I. WATSON with the APA Division, the *Journal*, and the University of New Hampshire; John POPPLESTONE and Marion White McPHERSON with the Archives; and Julian JAYNES and John SULLIVAN with Cheiron -and of course Josef BROŽEK's organization of the 1968 Summer Institute is a prime example of such leadership. With such institutional activity is often linked intellectual inspiration, and again Dr. BROŽEK's influence on his contemporaries and students may be traced through the way in which his ideas and approaches have been used (17). But the work of other scholars, not necessarily connected strongly with any institutional

base, has often stimulated interest and excellent work in the field. Here the brillant studies of Solomon DIAMOND may be cited, especially as most of his work in the history of psychology has appeared after he assumed his well earned emeritus status. Many other examples can also be chosen.

But perhaps the largest influence on the growth of the history of psychology among psychologists lies in the nature of psychology itself. That is, its intellectual roots lie in philosophy and many of its more distinguished practitioners are drawn to the field by philosophical or quasi-philosophical concerns. Such men and women can contribute greatly to psychology, while philosophically-minded physicists and chemists find it difficult to play a role in their sciences. And such psychologists are often interested in such issues as epistemology, (the philosophy of knowledge), an area to which history has much to contribute. For these psychologists, the study of their science's past is as much a part of psychology as their colleagues' experiments. And when their work is as historically subtle and insightful as it often is, its value as history, too, is immense.

This quasi-philosophical interest in psychology's past also has another effect on the way in which the history of psychology is done, which relates directly to Josef BROŽEK's career and to the focus of this paper. That is, in their concern for the epistemological standing of their psychology, these psychologists also want to be sure that their history can stand up to epistemological criticism. This leads directly to the great interest in historiography among psychologists concerned with the history of psychology. It is probably stronger than that of any other group besides professional philosophers of history. Unfortunately, some of this interest emerges in didactic papers by psychologists who have never investigated the past. They are all too often full of meaningless and unworkable distinctions (18). But, as with the historically naive papers and presentations discussed earlier, these are becoming rarer. More happily, this concern for historiography expresses itselft well in the serious self-reflection seen at such sessions as the Critical History Workshops, held annually in connection with the meetings of Cheiron. At these meetings, the importance of other intellectual influences on the history of psychology becomes clear and, in recent years, the highly insightful historical work of Kurt DANZIGER, Franz SAMELSON, and other psychologists has emerged as especially influential (19). Furthermore, the extensive, critical discussions at these workshops often lead their participants to a deepened appreciation of the ways in which current psychology and its history are intertwined. At times, these sessions have opened with those attending it reviewing one or more "discussion papers" that had been circulated before the meeting and these, in passing from hand to hand in the months that followed, have been influential (20). Some of the themes common to many of the workshop discussions have indeed recently been used by Jill MORAWSKI, one of the leading figures at these workshops, to organize a very instructive overview of "psychology's moral heritage" (21).

But these workshops and their concerns can finally come to illustrate just how the history of psychology and the history of science in general are coming together, to the benefit of both. That is, the 1983 workshop focussed its initial discussion around the work of Henrika KUKLICK, a sociologist of science and historian of the social sciences,

who teaches in the Department of History and Sociology of Science at the University of Pennsylvania. Through this workshop, Professor KUKLICK clarified admirably just how the approaches of the sociology of knowledge to the past could aid those of us studying the history of psychology to understand our subject better and see its development as a whole (22). It is difficult to overstate the potential value to our field of such a sociologically informed methodology, but here I want to stress just how well it meshes with what some believe is the dominant framework within which the history of science is today being written. And if I have any conclusions to present here, it is to stress the value of this perspective.

Of course, this approach -often called the social history of science- has its own history, it grew out of the situation of the 1960s. In that decade, no matter how else they clashed, the mainline history of science and the traditional history of psychology shared a fundamental, overwhelming concern for the ideas of past science, to the exclusion of just about any other concern. To be sure, this "internalist" focus had its value, but it often forgot -or explicitly denied- that ideas have no existence apart from people, and that people live in a given world. In reaction to this approach, various scholars tried to develop an "externalist" history of science, focussing on the way in which science and those who practiced it interacted with those around them. But in playing down the ideas of science, this approach ignored that which can be claimed to be uniquely scientific -its ideas- and by the middle 1970s few if any historians would claim to be "externalists". Instead, by the end of the decade a new approach to past science had emerged, combining -and, I believe, going beyond- internalism and externalism into a synthesizing framework of the social history of science. It is sometimes identified with the Department of History and Sociology of Science at the University of Pennsylvania, though the work of many other departments reflect this point of view. However, the intellectual roots of this new social history seem to lie in the work of Charles ROSENBERG, a historian of medicine long associated with the University of Pennsylvania, whose stress on the importance of disciplinary developments for the history of science has meant much to many historians of American science. Also important has been the work of Susan F. CANNON -born Walter F. CANNON- who taught sporadically at the University while holding a curatorship in the history of the physical sciences at the National Museum of History and Technology of the Smithsonian Institution (23). Institutionally, the major influence has been the work of Arnold THACKRAY, the entrepreneur who has built the Department with the moral and financial support of the University. In doing so, he has put together an active group of scholars whose work well represents this approach, that influences heavily the work of other institutions in the field, and that is not at all shy in making its work known. Indeed, just as the department's title and Professor KUKLICK's position among the members of its graduate faculty illustrate its breadth, Professor THACKRAY's role as editor of *Isis*, the journal of the (U.S.) History of Science Society, and as author of several reviews of the "state of the field" -including that which I've cited continually here -allows him to publicize the merits of the approach he calls "The New Eclecticism" (24).

I am not there going to review in detail the tenets of this "new eclecticism" nor to sketch in any detail how the "social history of psychology" can emerge. Others

can do so more competently than I. Indeed, a paper by Mitchell ASH entitled "Toward a Social History of Psychology" formed the basis of discussion at the Critical History Workshop held at the 1980 Cheiron meeting, and the year before saw the publication of a paper by Robert WATSON making several similar points (25). More than this, much excellent work now being done in the history of psychology reflects this perspective, and well illustrates just how much of the field is converging with the history of science in general. I'm also not going to review this literature (26). Instead, here I just want to outline briefly my understanding of what the new social history of psychology involves, and provide some illustrations of its value. Of course, nothing I present here is definitive.

Of all the formulations of the new social history of science that I've seen, the one that presents its concerns most clearly (to me) stresses the Interplay of Individuals, Ideas, and Institutions, within the Context both of the larger Culture in which science is developed and the disciplinary and subdisciplinary Community in which those doing science actually work. To be sure, this outline is too catchy to be trusted readily; all too many scholars get edgy around so much alliteration. But I believe that it does represent all that the history of science in general, and the history of psychology in particular, must consider critically, if they are to achieve the deep understanding of the past that we want from them. Fortunately, several scholars working in the history of psychology have produced analyses that well illustrate the value of the approach I've sketched, even if they've been totally unaware of it as something that's been formally described. Examples of such excellent work include Franz SAMELSON's insightful review of American psychology in the 1920s (27) and Mitchell ASH's marvelous dissertation on the origins of Gestalt psychology (28). Both can be cited as models from which we can all learn, and well demonstrate that formal prescriptions are not always needed to stimulate excellent scholarship.

Another example of the value of the social history of psychology lies not in the work of any one scholar, but rather in the sum of the many recent analyses of the life, career, science, and influence of Wilhelm WUNDT. I can't of course review this literature here, much of it stimulated by a celebratory interest. Much of it contradicts not only some long-held opinions about WUNDT, but also research on his life and career made available just a few years ago. Those who write on WUNDT can not agree among themselves as to the ultimate meaning of much of what they've learned, and disputes among Wundtian scholars form no exception to the rule that among four academics, five contrary opinions exist (29). Still, all that's emerged over the past decade or so about WUNDT and his ideas, about his relations with his family and students, about his early life and his interests outside of the University, about the development of his character and temperament, about the way in which his career fits into the history of the German University system, and so on, leads us to know much more about the origins of psychology as a science than otherwise would have been possible. It's too simple to say, of course, that scholarship is cumulative; it's difficult to stand steadily on any pair of shoulders, and giants' shoulders are especially slippery. But undoubtedly we know more about all aspects of WUNDT and his ideas -including his psychology- from the multiplicity of questions that have been asked about him recently than we would have had our colleagues focussed simply on his system and its development.

A penultimate example of the value of such a broadly conceived social -history perspective on the history of science may, I believe, be found in this article. I'm not claiming for it any definitive status, and I know that many who read it will realize all that I've omitted, slighted, and misunderstood here. But that's not the point, no matter how true it may be. What I want to stress is that, in sketching the "state of the art" of history of science in general in the 1960s and the 1980s, and even in sketching the growth of the history of psychology as a field in the 1970s, I've tried to show how intellectual, institutional, and individual factors have interacted within the context of the larger cultures and disciplinary communities involved. I may not have succeeded in doing all that I tried to do; indeed, I'll bet that I haven't. But I think I've shown that none of these factors, nor the interplay among them, can be ignored if we want to understand how we got to where we are.

And if we look to the career of Josef BROŽEK, we find a final example of the value of this broad concern with the full range of factors and their interaction. To cite but one example, his writings on the state of Soviet psychology (30) -though not strictly concerned with the past -well illustrate the value of the type of approach described here. Here then is another point at which we can learn from Dr. BROŽEK. His influence has been so great, and he's done so much for us in the past, that it's a pleasure to see, and acknowledge, that this influence is continuing. We all hope it continues for many years to come.

NOTES

(1) Josef BROŽEK, Robert I. WATSON, and Barbara ROSS, "A Summer Institute on the History of Psychology". *Journal of the History of the Behavioral Sciences,* 1969, *5*, 307-319; 1970, *6*, 25-35.

(2) Josef BROŽEK, "The American Adventure". *The Psychologists.* Edited by T.S. KRAWIEC, New York. Oxford University Press, 1974, Vol. II, 50-84.

(3) A useful and insightful history of the history of science in Arnold THACKRAY, "History of Science", *A Guide to the Culture of Science, Technology, and Medicine.* Edited by Paul T. DURBIN (New York: The Free Press, 1980), pp. 3-36.

(4) See especially Joseph AGASSI, "Towards a Historiography of Science", *History and Theory,* monograph no. 2, 1963.

(5) That not all graduate students in the history of science at this time shared these biases is well illustrated by the way in which William R. WOODWARD -also a participant at the Institute and then a graduate student at Princeton- escaped such prejudices and, as early as the mid 1960s, was able to learn from both traditions.

(6) Robin George COLLINGWOOD, *The Idea of History* (Oxford: Oxford University Press, 1946).

(7) Elizabeth Scarborough (GOODMAN), "Uncle Sam's Dollars at Work: Follow-up Study of the 1968 NSF Institute" (Cheiron, University of New Hampshire, Durham, New Hampshire, June 1974).

(8) Josef BROŽEK, "The American Adventure"; Josef BROŽEK and Ludwig J. PONGRATZ, editors, *Historiography of Modern Psychology: Aims, Resources, Approaches* (Toronto: C.J. Hogrefe, 1980).

(9) Henry GUERLAC, *Lavoisier: The Crucial Year* (Ithaca: Cornell University Press, 1961).

(10) Robert M. YOUNG, "Scholarship and the History of the Behavioural Sciences", *History of Science,* 1966, *5*, 1-51.

(11) David L. KRANTZ, "The Baldwin-Titchener Controversy: A Case Study in the Functioning and Malfunctioning of Schools", *Schools of Psychology: A Symposium,* edited by David L. KRANTZ (New York: Appleton-Century-Crofts, 1969), pp. 1-19; David L. KRANTZ, "Schools and Systems: The Mutual Isolation of Operant and Non-Operant Psychology as a Case Study", *Journal of the History of the Behavioral Sciences,* 1972, *8*, 86-102.

(12) Julian JAYNES, "The Problem of Animate Motion in the Seventeenth Century", *Journal of the History of Ideas,* 1970, *31*, 219-234.

(13) Solomon DIAMOND, "Gestation of the Instinct Concept", *Journal of the History of the Behavioral Sciences,* 1971, *7*, 323-336; Solomon DIAMOND, "The Debt of Leibniz to Pardies", *Journal of the History of the Behavioral Sciences,* 1972, *8*, 109-114.

(14) Michael M. SOKAL, *The Education and Psychological Career of James McKeen Cattell, 1860-1904* (Ph.D. dissertation, Case Western Reserve University, 1972); Michael M. SOKAL, editor *An Education in Psychology: James McKeen Cattell's Journal and Letters from Germany and England, 1880-1888* (Cambridge: MIT Press, 1981).

(15) Michael M. SOKAL, "The Gestalt Psychologists in Behaviorist America", *American Historical Review,* 1984, *89,* 1240-1263.

(16) See Arnold THACKRAY, "History of Science", for a more detailed analysis of the roots and effects of these developments.

(17) Dr. BROŽEK's autobiography, "The American Adventure", modestly plays down this aspect of his great influence. But any issue of *Science Citation Index* well testifies to the way in which his work means much to many others. Indeed, the use of such citation-analysis techniques was pioneered for our field in a 1970 paper of his, "Citation 'Longevity' as Criterion of Significance: F.C.DONDERS and the Timing of Mental Operations", *APA Proceedings* 1970, 787-788.

(18) Michael M. SOKAL, "History of the Social and Behavioral Sciences at Edinburgh: Notes on the Fifteenth International Congress of the History of Science, 10-19 August 1977, *Journal of the History of the Behavioral Sciences,* 1978, *14*, 387-390.

(19) Perhaps the most influential of each of these authors' many important essays are: Kurt DANZIGER, "The Positivist Repudiation of Wundt", *Journal of the History of the Behavioral Sciences,* 1979, *15*, 205-230; and Franz SAMELSON, "History, Origin Myth, and Ideology: Comte's Discovery of Social Psychology", *Journal for the Theory of Social Behavior,* 1974, *4,* 217-231.

(20) E.g., Mitchell G. ASH, "Toward a Social History of Psychology: A Very Long Review Essay" (Cheiron, Bowdoin College, Brunswick, Maine, June 1980); Kurt DANZIGER, "An Approach to a Critical History of Psychology" (Cheiron, University of Wisconsin, River Falls, Wisconsin, June 1981); Jill G. MORAWSKI, "The Limits of 'Contextualist' and 'Objectivist' Historiography of Psychology" (Cheiron, June, 1981).

(21) Jill G. MORAWSKI, "Assessing Psychology's Moral Heritage Through Our Neglected Utopias", *American Psychologist,* 1982, *37*, 1082-1095.

(22) The discussion at the 1983 Cheiron critical workshop revolved around Henrika KUKLICK, "The Sociology of Knowledge: Retrospect and Prospect", *Annual Review of Sociology,* 1983, 9, 287-310, which had been circulated in proof.

(23) E.g., Charles ROSENBERG, "On Writing the History of American Science", *The State of American History,* edited by Herbert J. BASS (Chicago: Quadrangle Books, 1970), pp. 183-196; Charles ROSENBERG, "Science in American Society: A Generation of Historical Debate", *Isis,* 1983, *74*, 356-367; Susan F. CANNON, *Science in Culture: The Early Victorian Period* (New York: Science History Publications, 1978).

(24) Arnold THACKRAY, "History of Science", reviews these developments well, and without institutional chauvinism. See also Arnold THACKRAY, "Making History", *Isis,* 1981, *72*, 7-10.

(25) Mitchell G. ASH, "Toward a Social History of Psychology"; Robert I. WATSON, Sr., "The History of Psychology Conceived as Social Psychology of the Past", *Journal of the History of the Behavioral Sciences,* 1979, *15*, 103-114.

(26) For important insights about this work, see Mitchell G. ASH, "The Self-Presentation of a Discipline: History of Psychology in the United States Between Pedagogy and Scholarship", *Functions and Uses of Disciplinary Histories,* edited by Loren GRAHAM, Wolf LEPENIS, and Peter WEINGART (Sociology of the Sciences, Vol. 7) (Dordrecht: Reidel, 1983), pp. 143-189.

(27) Franz SAMELSON, "Putting Psychology on the Map: Ideology and Intelligence Testing", *Psychology in Social Context,* edited by Allan R. BUSS (New York: Irvington, 1979), pp. 103-168. I have also sketched an overview of psychology in this decade that tries to approach its subject broadly: Michael M. SOKAL, "James McKeen Cattell and American Psychology in the 1920s", *Explorations in the History of Psychology in the United States,* edited by Josef BROŽEK: Lewisburg: Bucknell University Press, 1984, pp. 273-323.

(28) Mitchell G. ASH, *The Emergence of Gestalt Theory: Experimental Psychology in Germany, 1890-1920* (Ph.D. dissertation, Harvard University, 1982).

(29) By far the best brief overview and analysis of the results of the first decade of the WUNDT industry is by a British historian of science who has long written on the history of psychology: Roger SMITH, "Wilhelm Wundt Resurrected", *British Journal for the History of Science,* 1982, *15,* 285-293.

(30) E.g., Josef BROŽEK, "Spectrum of Soviet Psychology: 1968 Model", *American Psychologist,* 1969, *24,* 944-946.