

AAAS
San Francisco
January 15, 1989

REFLECTIONS ON THE FUTURE OF SCIENTIFIC PSYCHOLOGY

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My assignment in this symposium is to comment on the preceding papers and to add a few remarks of my own on how I see the future of scientific psychology.

All of the papers show that those who offered them felt a certain degree of dissatisfaction or disappointment with contemporary psychology as a science and each offered some corrective suggestions

The issues arise out of the many efforts by the proponents of modern psychology to find its niche in the family of sciences. Before psychology separated itself from philosophy the philosopher-psychologists defined psychology according to their more general philosophical systems. When the decision was made about a century ago to include psychology among the laboratory sciences the new psychologists turned to models based upon advances in the physical and biological sciences. They could pick out model scientists in those areas who had psychological interests such as Hermann von Helmholtz, who was primarily a physicist of distinction, but had physiological and psychological interests as well.

Helmholtz is a good prototype for very specific reasons. For one thing, Wilhelm Wundt, who usually gets credit for starting the first psychological laboratory in Leipzig, had spent many years working in the laboratory along with Helmholtz in Heidelberg. Helmholtz, in turn, although primarily a physicist, had studied physiology with the great Johannes Muller and was one of the first to study the rate of propagation of the nervous impulse along nerve trunks -- of great interest to psychologists because such speeds set limits to the speed of thought and action. So, with such founding fathers as Wundt and Helmholtz we already had ties with both physics and physiology. Wundt, in fact, preferred to use the expression "physiological psychology" to describe the new experimental science. Social sciences were somewhat less developed at the time to serve as models, although it may be noted that Wundt, early in this century, wrote 10 volumes on Volkerpsychologie (1900-1910). This title as he used it meant essentially cultural and historical psychology, anticipating later social psychology, and was translated as social psychology by an American student of his, Charles H. Judd, later a distinguished educator at the University of Chicago.

With this early beginning, an empirical scientific psychology seemed well launched and its domain within the family of sciences fairly well established. Closer

to home here in America, by defining itself as a laboratory science experimental psychology soon found its place in the university and became formally recognized as a science by scientific colleagues from other areas when it became affiliated with the American Association for the Advancement of Science and its members were eligible for election to the National Academy of Sciences.

All did not run smoothly, however, and throughout the years psychologists became worried from time to time about how to achieve unity as a distinctive science with overarching principles to give it coherence without narrowing its focus.

I turn now to the papers just presented as illustrative of the self-searching that still goes on in psychology. I shall consider them in the order in which they were presented.

Dr. Howard, defining his position as favoring new approaches to old issues, was particularly critical of the hope that the physical model, specifically the Newtonian model, can be satisfactory for psychology (Howard, 1989b)

As late as the 1940's and 1950's Clark Hull at Yale University developed a behavioristic system modeled deliberately after Newton, and he often carried Newton's Principia along with him as a symbol of what he was attempting. He chose to develop his system as one of formal definitions, postulates and theorems subject to empirical verification through experiment and measurement. Highly successful for a time, Hull's theory fell into disfavor, and gradually other attacks have appeared on what Toulmin and Leary have called "The Cult of Empiricism" (1985).

Howard picked up this attack from a somewhat different angle, by recommending in place of the Newtonian approach a form of psychological science that will pay less attention to the mechanistic and deterministic forces controlling human life and thought -- his assessment of the Newtonian approach -- and give instead more credit to the human agent. Such a psychology is said to be agenic, that is, one that allows for the influence upon outcomes of the plans and choices of an active agent. In other words, a person's reasoned intentions and wishes may influence what happens, and we need not look entirely to the inevitable outcomes based on causal chains in the person's past. Hence -- at least for heuristic purposes -- he accepts freedom instead of determinism when he confronts the mind-body problem.

I find this refreshing, as it was in the writings of William James, when his first choice to express his freedom was to commit himself to the efficacy of free choice. At the same time for Howard to defend this position in the world of physical and biological science will mean many obstacles to overcome. Ultimate success requires that he and his associates will have to continue in their efforts, already

showing promise, to achieve sufficient quantification to establish an orderly connection with established science.

Another aspect of Howard's new psychology is its recognition of the importance of narration as a psychological resource. We have of course had many life histories, which are essentially narrative reconstructions to deal with the kinds of issues that Howard raises, and over the past few years have seen attention to psychohistory as a special form of biographical writing. I can recommend the book entitled: Psychology and Historical Interpretation by William McK. Runyan (1988). Howard's own narrative approach is represented by his recent book entitled A Tale of Two Stories: Excursions into a Narrative Approach to Psychology (1989). The illustrative stories are his own autobiography and the history of psychology in narrative form, but there is much more.

Eddie Oshins, the physicist on our panel, proved to be less occupied with the classical aspects of either physics or psychology. He offered instead the services of another kind of physics -- quantum physics -- and indeed proposed a quantum psychology. Although I have had considerable opportunity to discuss his proposals with him, I must share with many in this audience some difficulty in following his arguments. The exposition of this kind of material is unusually difficult, especially in condensed oral presentation. Oshins work is serious and insightful. I shall do my best to convey the thrust of his proposals, without considering them in precise detail.

His theory is proposed as an attempt to produce a logical structure for psychology similar to Von Neumann's quantum logic as applied to physical experience. He was careful to explain, however, that a quantum psychology as he has developed it is not an application of quantum physics to psychology, but is instead a logical structure, derived to be sure from work in physics, but appropriate to some basic psychological problems.

His entering wedge into psychological problems is the distorted logic represented in the thinking of schizophrenics interest in his references to von Domarus, a German scholar who was working at Yale during the time I was teaching there in the early 1930's, when J. G. Dusser de Barenne and John Fulton dominated the work in neurophysiology. The work cited appeared in 1944 in a book edited by J. S. Kasanin, a Bay Area psychiatrist, on Language and Thought in Schizophrenia.

(1) The von Domarus principle, representative of the intrapsychic approach -- one of three major theoretical approaches that Oshins discussed--is stated as: "Whereas the logician accepts identity only upon the basis of identical subjects, the paralogician (i.e. the one with distorted logic) accepts identity on the basis of identical predicates." Others Oshins classified with him include Vigotsky, Arieti, and

Boyd. Arieti's example of this distortion is "I am a virgin. The Virgin Mary was a virgin, therefore, I am the Virgin Mary."

(2) The second approach to schizophrenic thinking that Oshins called metallogical is the double-bind theory proposed by the late Gregory Bateson and by Watzlawick, Weakland, and those associated with them in Palo Alto at the time.

(3) Both of these approaches have their limitations, and Oshins proposed a third one, his quantum psychology that serves as a synthesis to deal with the problems that the other approaches tried to solve, but corrects some of their difficulties. The corrections come partly through the quantum method of dealing with ambiguity as a type of equivocation in which the distributive law of set theory and, thus, of classical logic is violated. Oshins proposes replacing it with a quantum principle of operational ambiguity. He went on to relate some of the distinctions that he was making in Bohr's principle of complementarity. Although this principle was first stated in relation to subatomic processes, it has been generalized to more familiar matters. Bohr has used it in relation to the alternative interpretations based on love or justice, and Heisenberg used the illustration of enjoying music and evaluating it.

I recall a dinner conversation I had with Niels Bohr in Washington in the 1940's, when he stopped off on his way to Los Alamos to work on the atomic bomb project. Knowing that I was a psychologist, he assured me that complementarity was a principle not only for physics but was generally applicable and in another generation would be taught to school children. So there may be several ways in which modern physics might become connected with psychology, and Oshins has alerted us to some possibilities.

In further elaboration of his quantum psychology he examined critically the brain model of McCulloch and Pitts, described as a synaptic summation model, and Pribram's hologram model, described as a synaptic superposition model. His own model he called a synaptic spanning model.

His major difference from the former model lies in his use of irreducible ambiguity as a fundamental principle; and from the latter model in his application of superposition to rays instead of to physical waves such as holograms. This is something that only those initiated in quantum theory can understand.

The most important point is that the selection of models should not be a matter of personal preference but should be decidable on empirically based evidence. In other words, the differences between the models have to be specified according to defined observables.

He makes a case for the formal power of his model, as in the development of operational thinking in children as noted by Piaget and going beyond it.

A further point is his emphasis on the role of negation. Here he turns to Freud's conception that unconscious processes (primary processes) lack negation.

Negation, Oshins says, is necessary for conscious processes, for mature judgment and to form boundaries between the self and others. He cites Freud's statement that dreams avoid the categories of antithesis and contradiction. "No" does not exist for a dream. Oshins goes on to use Finklestein's theorem that inquiring question-A of proposition-B requires a synchronization between their information contents in order to code a negative in the information content. If the capacity to form negatives proves to determine the capacity to have consciousness, an existing technology might be able to demonstrate this. The appropriate technology he calls superconducting quantum interference device technology. This is bold suggestion, and something on which we cannot pass judgment until it is tested.

In his own statement of conclusions he finds three advantages in the quantum psychology over the other theories available: (1) It is a critical theory, and provides criteria for discriminating between it and the other models discussed: (2) it is a formal theory, and lends support to such psychological theories as those of Piaget's theories in the steps of development of operational thought, and (3) it is an empirical theory because it suggests possible experimental confirmation.

As I said in introducing my remarks on Oshins' paper, I can find much in it that intrigues me, as my summarization suggests, while at the same time there are many details that I cannot comprehend. I confess this, because I sympathize with those in the audience who may have heard some of this for the first time. All I can say is that both Oshins and I understand the difficulty, but his purpose has been served if he has opened our thinking to some new possibilities. Speaking for psychologists, I am afraid that any enthusiastic acceptance of quantum psychology awaits further developments, but we should be open-minded after having learned of the possibilities.

Dr. Arciero, a psychiatrist, proposed that we cease celebrating psychology's divorce from either philosophy or biology, but ought to be aware that evolutionary biology bears upon epistemology, and such integrating ideas, as system theory have their philosophical roots. He is also interested in the distinction between tacit knowledge and explicit knowledge, a concept treated so well by Michael Polyani (1966).

One of Dr. Arciero's colleagues now living in Santa Barbara, Dr. James G. Miller, trained both as a psychologist and as a psychiatrist, with a career that has included the headship of the Department of Psychology at the University of Chicago and the presidency of the University of Louisville, before he moved to Santa Barbara, has done much to promote systems theory, as in his large book on Living Systems (Miller, 1978). Such ideas are provocative of the many options open to psychology in the future. Both Dr. Arciero and Dr. Maloney, whose paper followed his, have also

collaborated in advancing what is known as a constructivist metatheory. In this they take the position that human knowing is active, anticipatory, and literally "constructive", i.e. form-giving. There are other forms of constructivism: one of the most widely cited is that of George Kelly, presented in his Psychology of Personal Constructs (1955).

Finally, there is the paper of Dr. Mahoney, who organized this symposium. As expressed in the title of his paper Encouraging the Shift from Scientistic to Scientific Psychology, he shows his fear that psychology in trying to be ultrascientific has worshipped the trappings of science, and in so doing has run the danger of trivializing the science.

Psychologists, for example, spend many years refining the measurement of sensory thresholds, in support or emendation of the Weber-Fechner Law. This foundation principle, while limited, is not a bad one. One illustration is that the logarithmic relationship found between stimulus intensity and the perception of differences has proved to have practical usefulness, as in the decibel as a unit for differences has proved to have practical usefulness, as in the decibel as a unit for loudness measurements. Yet this "law" of relative judgments falls far short of the kind of overarching law that a full-bodied psychology requires. That much of psychology is piecemeal and somewhat narrow in application is but one form that current self-criticism takes.

There is another kind of criticism that best characterizes Mahoney's remarks. He calls attention to the danger of defining psychology too narrowly as a science, so that it may become exclusionary and thus eliminate from psychology the pursuit of understanding of some of the fundamental problems posed by the human mind. He directs his attack against radical behaviorism, by turning to the tension between radical behaviorism and the developing cognitive sciences that show a renewed interest in the brain and mental processes. By contrast, radical behaviorism appears to cling to the notion that it is better to think of the brain as merely a "black box" to integrate incoming influences and outgoing results--a position sometimes described as a "finger-tip" psychology because of its emphasis upon observed activity.

While it is clear that he favors a cognitive approach against radical behaviorism, some of the substance of his paper rests on a general review of the philosophy of science over the years, with its criticisms of the concept of observer-free objectivity and of the related methodologies, such as operationism, to which it led. In addition, he adds notes from his own longitudinal study of a group of "leading experts in cognitive and behavioral perspectives," a total of 37 who had replied in both 1977 and 1985 to answers about their perspectives on behavioral and cognitive aspects of psychology. They were from a wide range of psychologists, but it

is not clear how they were selected, and, although the original participants are listed by name it is difficult to infer the common core. Most of them are practitioners of psychology rather than the more academic types. It matters little for his purposes that this is not a normative sample, because what interests him is the changes that have taken place in the eight years between the two surveys. The classification into behaviorists, cognitive-behaviorists, and cognitivists showed consistent differences in that order. Within each group there were also some changes.

Cognitivists, for example, were slightly more willing in the later sample to acknowledge some role for unconscious processes. Both cognitive-behavioral and extreme behavioral respondents showed signs of shifts in the direction of personal agency as an important counterbalance to an exclusive environmental determinism.

What of the Future?

Having made these remarks on the papers that have been presented, I wish now to add a few comments of my own.

I suppose there are two reactions common to an oldtimer such as I. The first is to wish for the good-old-days when, at least in retrospect, all seemed simpler and more promising. I became an instructor at Yale in the fall of 1929--nearly 60 years ago when, instead of 60,000 there were only 1000 members of the American Psychological Association and we had just hosted an International Congress of Psychology in New Haven, the first in America. Although there were plenty of disagreements among the participants, this to me provides a picture of the good old days, when psychology was about to take off on a large scale.

The second reaction from an oldster, complementary to the first, is to be patient with evident signs of change because of having lived through so many changes that seemed like crises at the time but were eventually acceptable. This is where I find myself today. I am not discouraged about the future of psychology because tensions do get resolved and each generation finds some way to invent arrangements that will make it possible to live with the inevitable changes.

Although there is novelty in the suggestions made today, they are not all wholly new, and there always have been some who were ready for change. No one thought I was particularly bold when in my presidential address before the American Psychological Association in 1949--a half century ago--I talked on human motives and the concept of the self, urging that we recognize how important and valuable the self-image is to the person as he or she engages in planned goal-directed behavior (Hilgard, 1949).

There are two major sources of tension within psychology today. One has to do with the proper manner in which to conceive of psychology as a science, the other is how to organize psychology so that the individual psychologist, including those engaged in the private practice of psychology, can identify with other psychologists united in a common enterprise. The papers today dealt almost exclusively with the first of these issues, that is, issues of the philosophy-of-science kind, rather than those dealing with the sociology or politics of psychology as a profession as well as a science.

On the preoccupation with scientific status of psychological theorizing, I am reminded of a meeting in 1949 with Robert Oppenheimer of a group of psychologists invited to join with him in Princeton at the Institute for Advanced Study to discuss the desirability of representing psychology at the Institute. After we had gone after each other on definitional and logic-of-science problems, Robert remarked:

"Psychology must not be much of a science if you have to talk so much about what makes it a science." One of our number provoked by his remark, questioned him: "If you don't like the way we talk, tell us how you define a science." He replied simply: "I see science as the search for new and communicable information." This was a good answer, but a little exasperating as those who knew him often found Robert Oppenheimer's replies to be.

I agree with the intent of his remark, although without a good deal of expansion, seeking communicable information could apply as well to journalism as to science. In essence, he meant that we are not limited by any formal definition of the appropriate scientific logic and are free to define our problems in such a way as to make the answers communicable, that is, understandable by those who are competent, and subject to substantiation by them. One could add a remark that this is no only an intellectual game, but the hope is usually implied that what is found out will be not only esthetically satisfying in our search for understanding, but socially useful as well.

The other crisis that psychology is facing is of another sort--less intellectual, perhaps, but relevant to identifying oneself as a psychologist. The American Psychological Association, essentially the source of identity for 60,000 -- the main bulk of psychologists in this country -- has become victimized by its size and by the imbalance in interests among its members. Despite serious attempts over the last 20 years to work out a plan to accommodate all the final plan which held much promise was defeated last year by membership vote as a result of active lobbying against it by those in the private practice of psychology.

The prompt reaction by academic psychologists and applied psychologists who had an interest in the research base for their applications was to found a new organization, called the American Psychological Society which in its first month enrolled some 3500 members, and includes in its Advisory Board more than half the living past-presidents of the American Psychological Association. It has set its first annual convention for June 1989 in Washington, DC with no intention of competing with the APA convention to be held in August. The new American Psychological Society (APS) is more like the "old" APA after it matured enough to recognize the legitimacy of applied psychology but before it became overwhelmed by those in private practice for a fee.

Through bad management, the American Psychological Association, the APA, which formerly had carefully maintained ample reserves, is practically bankrupt, has sold its valuable buildings to pay its debts, and now is reorganizing itself to become more like the American Medical Association than the American Chemical Society, which includes both academic research chemists and chemical engineers. Many of us who have tried in the past to hold psychology together have continued our memberships in the APA and at the same time have joined the new APS, hoping that in time some new kind of federation of all psychologists will become feasible. Should it turn out that separate organizations appear to serve better the diversity of psychologists there is no more reason that all should be under one roof than, by analogy, all electrical engineers should be physicists, because it is possible to consider electrical engineering to be a form of applied physics.

Organizations are social inventions to meet needs and to solve problems. Most scientists find their identities, through the university departments that are named for their disciplines, where they have obtained degrees or where they serve, through national and international meetings where they meet with like-minded scientists, and through their journals where they keep abreast of what is happening in their fields. The departments, institutes, journals, and societies define for them what it feels like to be a scientist in their chosen fields, and this applies to psychologists as well as to other disciplines. When things get out of balance, there are changes that at first may appear to be disruptive, but in the end serve new purposes, as when biochemistry became so large that it often pulled away from chemistry departments and found a place for itself in medical schools.

Change can be a sign of vitality as well as of disintegration. I have sufficient confidence in the wisdom of younger colleagues in psychology to expect them to find a way out of psychology's present predicaments. They will attend to such critiques as we have heard today and to the suggestions that were made -- along with many others, and I wish only that I might be around to see some of the new syntheses that will be achieved.

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