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SOME ORIGINS OF PSYCHOLOGY AS SCIENCE

♦301

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THE NOMINAL BEGINNING

This year, 1979, marks the 100th anniversary of the consensual beginning of psychology as a distinctive scientific discipline. It seems appropriate to describe and discuss briefly the when, where, and who of those nominal beginnings, and to examine some of the intellectual linkages between those events and some present psychological activities and interests.

It is customary to acknowledge explicitly that the search for the "when" of an event such as the beginning of a discipline, as with the beginning of the decline and fall of the Roman Empire, is uncomfortably arbitrary. As one attempts to decide which one of several persons, or which one of several dates, seems most appropriate to mark a "beginning," it becomes clear that what we gain by such an exercise is found less in the understanding of the "when, where, and who," but more in an appreciation of "what" was begun. As we make the many arbitrary decisions necessary for eliminating candidates we are, in fact, discovering what parts of psychology were available at a given time, and how well and in what manner these parts were articulated.

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The search for the date for a beginning quickly pushes to the forefront the prior question of who was involved in this beginning. Historical search has, by consensus, been narrowed to one main line of intellectual inquiry; then the search rather quickly reduces to three or four candidates. The short list of final entries usually includes Fechner, Helmholtz, Wundt, and James. Fechner remains on this list primarily for his major work in psychology, *The Elements of Psychophysics,* published in 1860, but also for his many other contributions to the philosophy of mind and the theory of measurement, including applications to esthetics. Helmholtz remains on the list because he is the best scientist on any list we might generate, long or short, but more specifically because of his major contributions to the psychology and physiology of hearing and vision. Examples of these contributions are seen in the three volumes of the *Handbook of Physiological Optics,* published over the decade of 1856–1866, and the volume on hearing which appeared in 1863. James remains on the list at this late screening primarily because he forces us to ask how much of the modern flavor of psychology we demand. James was concerned with many topics that occupy current psychologists but that were not treated by the other three possible founders. His *Principles of Psychology* did not appear until 1890, but he had begun teaching topics in psychology at Harvard in the middle 1870s. Wundt is on the list because of a prodigious amount of work he did beginning around 1862 in the area he called physiological psychology. The book that had a major influence on the emergence of psychology as a science was his *Principles of Physiological Psychology* which appeared in 1873–1874 and went through six editions in approximately four decades. His writings are extensive in terms of breadth of subject matter as well as magnitude of output. Wundt wrote on topics that ranged from ethics and logic to animal psychology, physiological psychology, and the natural history of man.

Of the four men listed, James would probably be the first to be deleted on grounds of temporal priority. The other three men were doing something in this developing area when James was deciding that such work should be done. Perhaps this priority is shown most clearly by James himself in a letter he wrote while in Europe in 1867.

> It seems to me that perhaps the time has come for psychology to begin to be a science —some measurements have already been made in the region lying between the physical changes in the nerves and the appearance of consciousness . . ., and more may come of it. Helmholtz and a man named Wundt at Heidelberg are working at it. (5).

Helmholtz would probably be dropped from the list next, primarily because he was too busy doing good science to devote much effort to starting a new "discipline." Fechner poses the most difficult final choice because of the priority of his *Elements of Psychophysics* and the fact that he is widely

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regarded as having exerted an important influence on subsequent measurement and quantitative developments in psychology, notably on the work of Ebbinghaus and later the work of Binet. Some historians of psychology, such as Pillsbury, trace the beginnings jointly to Fechner and Wundt and may be interpreted as giving priority to Fechner. Pillsbury's view is that "Wundt may be said to share with Fechner the honor of founding experimental psychology" (21).

Nevertheless, the consensus is that Wundt should be viewed as the most convincing candidate for "founder" for two major reasons. The first reason is a matter of judged intent; Wundt specifically stated that he intended to mark out psychology as a new science. This can be seen clearly in his preface to the first edition of *Principles of Physiological Psychology* in 1873:

> The work which I here present to the public is an attempt to mark out a new domain of science. I am well aware that the question may be raised, whether the time is yet ripe for such an undertaking. The new discipline rests upon anatomical and physiological foundations which, in certain respects, are themselves very far from solid; while the experimental treatment of psychological problems must be pronounced, from every point of view, to be still at its first beginnings. At the same time the best means of discovering the blanks that our ignorance has left in the subject matter of a developing science is, as we all know, to take a general survey of its present status (28).

This is what Wundt did in the early editions of his *Physiological Psychology.*

The second reason for selecting Wundt derives from the manner in which he and his laboratory were viewed by other scholars. Beginning around 1880 his Leipzig laboratory became the place to go if one was interested in psychology as a science. Within the next 15 years the list of Americans who spent time in the laboratory is impressive. The publication of the first psychological journal, begun in 1881, was associated with this research activity; this was a journal devoted to reporting psychological research in action. The founding of this journal is counted by some historians as a third reason for honoring Wundt as the founder of psychology.

Having decided that Wundt is the preferred candidate, the selection of a date to mark the beginning of experimental psychology becomes more directed. If we take seriously the multiple-criterion argument we used in selecting Wundt, certain dates that might have been plausible on other grounds are eliminated. If the reasons Wundt was chosen are to be based on the joint facts that he articulated that psychology was a science and outlined some of its properties, that he had a functional laboratory which served as more than a demonstration room housing a few items of equipment, and that this laboratory was perceived by others as a place where they could work, then events that satisfy only one of these criteria cannot be used. We have quoted the statement of intent with a dateline of 1873; with

the criterion of intent and belief alone, this date could be used. Yet, using only that criterion, we could find passages in his earlier (1862) *Contributions to the Theory of Sense Perception* that would have been almost as convincing. With the intent established by 1873, the date of choice must now involve a judgment of when the laboratory became functional. There is some difference of opinion on this point; yet the differences are not great, and it is not clear that all of the dates suggested are meant to refer to when the laboratory became functional, as opposed to when Wundt went to Leipzig and had some space assigned (1875). Statements by three students of Wundt cover the range of dates offered, 1875 (Scripture), 1878 (Titchener), and 1879 (Cattell). Although it is not good historical technique, one is tempted to let Wundt decide the issue, unless his version seems to be in gross violation of other "facts." Boring (6) quotes Wundt as writing of this period in the following way: ". . . there grew up in the autumn of 1879 some independent research which made use of this space for experimental work." Thus, with Wundt's help we can suggest the when (1879), the where (Leipzig), and the who (Wundt).

AN INTERPRETATION OF THIS BEGINNING

We have picked Wundt, Wundt has picked 1879, and most psychologists are willing to accept these choices as an appropriate solution to the question of what marks the beginning of psychology as science. This acquiescence to the judgment of psychology's historians is very likely to be accompanied by two other observations. The first is that if we ask most psychologists what Wundt did experimentally, or what he represented theoretically or systematically, we find that answers are surprisingly brief. The reply is likely to include some statement about introspection as an experimental method, perhaps a brief reference to a theory of feeling. From some psychologists there may be a few additional comments about attention or apperception; in some of these cases, if the comments start to get detailed, we will usually find that the respondent is talking about Titchener rather than Wundt. Perhaps this observation should not be surprising because there is a second observation. If we inform the respondent of some of the detailed discussions of Wundt's work as it appears in history textbooks, he or she is very likely to feel that the work does not bear importantly on the kind of work and theory that characterizes the last 50 years. For example, for most psychologists, limiting the experimental method to controlled introspection seems to restrict the new science unduly and to lead to a definition of the subject matter of psychology that leaves most of the interesting questions outside of the "new domain of science."

In Wundt's defense the point can be justly made, and it has been made recently by Blumenthal (2), that Wundt has been incompletely treated in

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most psychology-history textbooks. Setting aside, for the present purposes, Wundt's contributions in the areas of logic and ethics, and restricting ourselves to his writings on psychology, it is clear that his work falls into two broad categories. The first is his work on experimental psychology (as he defines it), the second is his work on *Völkerpsychologie.* The second aspect of his contribution is frequently not even mentioned in history textbooks, and rarely is it discussed in a systematic way. In some cases this treatment may be considered a matter of benign neglect, but a failure to comment on this half of Wundt's work frequently leads to a misinterpretation of what he considered the subject matter of psychology to be. For example, one recent textbook (24) states that "For Wundt, 'psychology,' 'experimental psychology,' and 'physiological psychology' were three terms for the same subject." This gives the reader an incorrect impression of Wundt's views. For Wundt "objective psychology," and "experimental psychology" can both be included under psychology, but the first two terms do not deal with the same subject matters because of the methods that must be used. For Wundt psychology includes many topics that *cannot* be studied by the experimental method, but can be studied objectively. Wundt is so explicit about this issue that it seems puzzling that any systematic treatment of his works would fail to emphasize this point. For example, in his preface to the fifth edition of *Principles of Physiological Psychology,* after describing some of the properties of experimental method he states ". . . fortunately for the science, there are other sources of objective psychological knowledge, which become accessible at the very point where the experimental method fails us." He then briefly describes what *Völkerpsychologie* is, and then says, "The results of ethnic psychology constitute, at the same time, our chief source of information regarding the general psychology of the complex mental processes. In this way, experimental psychology and ethnic psychology form the two principal departments of scientific psychology at large" (28).[1]

Boring's account (3) of Wundt's work is perhaps symptomatic of this unbalanced treatment. In writing a 700 page volume dedicated to Wundt, he devoted less than ten lines to this second aspect of Wundt's work. "The new century brought him the leisure to return to the unfulfilled task, outlined in the *Beiträge* of 1862, the writing of *Völkerpsychologie,* the natural history of man, which only, Wundt thought, could give the scientific answer to the problem of the higher mental processes." That is the extent of the substantive treatment of what Wundt thought was one of the "two principal

[1]Thousands of words have been written about the appropriate translation of the term *Völkerpsychologie;* it probably should be left in the original German to avoid incorrect images of the subject matter. The actual material that is discussed can be used to define the subject matter.

departments" of scientific psychology. Murphy (18), although also very brief in his coverage, seems more receptive to the nature of the work and provides more information as to its substance.

> To folk psychology Wundt devoted some of his best energies (1904–1914). Believing that "cultural products," as well as introspective reports, are a legitimate subject matter for psychology, he undertook a systematic psychological interpretation of the data of anthropology and history. His studies on the psychological interpretation of language are perhaps his best-known contributions. He emphasized the interpenetration of psychical and physiological factors in linguistic structure, protesting against that naive psychologism to which phonetics was a mere incident and, with equal explicitness, against that merely philological approach which had sought to explain all linguistic change in terms of the laws of vocal utterance.

Nonetheless, it is important to emphasize that, even with all of the possible amendments to the historical treatment of Wundt, it is still the case that our hypothetical respondent who feels that the Wundtian beginning does not significantly anticipate psychology as it has been for the last 50 years would still be correct. This conclusion is not altered by the fact that no other single individual's work at that time can be viewed as anticipating the subsequent developments more accurately. It is to this problem that we must now turn.

SOME EVENTS 25 YEARS LATER

At the outset the point was made that while the date, the man, and the place per se are not key factors in the understanding of our origins, the search could help us understand what was begun and the manner in which it was articulated. What can we say about "what" was begun? There is general agreement on several conclusions: 1. a functional laboratory devoted to experimental research in psychology was begun; 2. a psychological journal was begun; and 3. a way of thinking about psychology was begun. This way of thinking stated that psychology was a science. Allowing for the previous discussion about Wundt's system to the effect that it included methods other than his experimental method, the fact is that Wundt's work (and his impact) in the first quarter-century of psychology's history, are limited to the experimental phase of his program; it was this method and its related subject matter that were begun in this period. Even accepting that Wundt would eventually write *Völkerpsychologie* (and he does anticipate this work in 1862), it is fair to say that one cannot anticipate from Wundt's work the kind of explosion of ideas and research that engulfed psychology at the end of its first quarter-century. This is not meant as a criticism of Wundt as, I hope, will be clear in a moment. It does say something important about the origins of modern psychology.

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Let us select the year 1904 as an anchor date, the end of the first quarter-century of psychology's history. Let us open up a window in time, say 4 years, on each side of that date, and look at the diversity of published works that signal what forces were acting to define psychology. Choosing just a few authors representative of the diversity of ideas related to psychology, we have such works as Adler's *Study of Organic Inferiority* (1907); Baldwin's *Development and Evolution* (1902); Binet's *Suggestability* (1900) and *Experimental Study of Intelligence* (1903); Claraparede's *Psychology of the Child* (1905); Freud's *Interpretation of Dreams* (1900), *Psychopathology of Everyday Life* (1901), and *Three Essays on the Theory of Sexuality* (1905); Hall's *Adolescence* (1904); Hobhouse's *Mind in Evolution* (1901); Jennings's *The Behavior of Lower Organisms* (1906); Jung's *The Psychology of Dementia Praecox* (1907); Loeb's *Forced Movements, Tropisms and Animal Conduct* (1908); McDougall's *Social Psychology* (1908); Morgan's *Animal Behavior* (1900); Sherrington's *Integrative Action of the Nervous System* (1906); Thorndike's *Educational Psychology* (1903) and *The Measurement of Twins* (1905); and Washburn's *The Animal Mind* (1908).

Such a list could easily be tripled, and this is only a list of books. Many other events of equal or greater importance could be listed. Pavlov had just begun his work on conditioning; the first dissertation to use the term "conditioned reflex" appeared in 1903. Pearson, the outstanding statistician, published several papers on the inheritance of mental characteristics in man. Spearman published a factor analysis of intelligence. Obviously none of these works occurred in an intellectual vacuum; for each we could trace an historical path. The point is that in writing the history of this limited number of works, we would be taken to names, ideas, and experiments that are not encountered in the path to Wundt and, through him, to 1904. Not only does the historic path through Wundt not anticipate these events, there is no other particular vantage point in 1879, no other person's work at that time, that would have permitted the forecasting of all of these events. Any interpretation of psychology's origins must deal with this rapid emergence of thinking and research on psychological matters around the turn of the century.

A SUGGESTED HISTORICAL MODEL

What kind of historical model should we use? The common textbook model suggests that experimental psychology as science started with Wundt and that this tradition brought scientific psychology to the turn of the century. By this time criticism had grown from a number of quarters, including some of Wundt's students. At about this time the main features of Wundt's views, with modifications, were systematically articulated by Titchener and he

became the focal point for the beginning of a second quarter of a century of debate over the nature, scope, and methods of psychology. Everyone agrees that "other influences" coming from different lines of research played an important role in this debate. This second quarter-century is the period of the so-called "schools" or "systems." If the historian goes beyond that period, the position adopted will range between two main models. One model says that the schools are modified by the debate of the first two decades of the twentieth century; some schools may drop out, but the others continue to exert influence in modified form to the present. The other model says that the schools gradually disappear, or are absorbed, and what emerges is what is euphemistically called the mainstream of psychology. Let us look at two examples of views of the postschool era.

> Although . . . most of the factionalism and intense controversy has diminished, the effect of these questions of the Age of Schools upon the science still remains despite the apparent lack of concern by contemporary psychologists. Thus the events and issues of the Age of Schools are not simply a matter of cultural heritage, but when seen in historical perspective, emerge as valid, ongoing concerns for the field (13).

> We have seen how the various systems of psychological thought came into being, prospered for a time, and then, with the exception of psychoanalysis, were absorbed by the mainstream of contemporary psychology. We also saw that each movement grew strong through opposition to another system. When there was no longer any need for strong and vociferous protest, the schools, as such, died a gradual death. Yet each of these protests died a successful death because it made substantial contributions to psychological thought. Thus, each was a fruitful protest—each accomplished its mission (25).

I should like to propose a different kind of model for the history of psychology. This model has two components. It acknowledges that Wundt established the first experimental psychology laboratory and the first psychological journal, and that this laboratory became one important fountainhead for research in psychology. It credits Wundt with institutionalizing psychology as a discipline. It suggests that Wundt, the tradition that he established, and the tradition from which he came, represent only one path leading from the seventeenth century, when the era of modern science began, to the twentieth century when psychology as science took on its modern form. There were many other paths of science from the seventeenth to the twentieth century that had psychological questions as a major part of their concern. It cannot be assumed that the path through Wundt was the most important path and it is probably not fruitful at this state to try to establish the relative importance of the different paths of intellectual inquiry. Each path branched many times and, in so doing, partially intertwined with other influences; the interdependence of these paths is obvious, yet very difficult to assess. Along each of these paths any one of many individuals *could* have declared and implemented the notion of psychology

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as an experimental science. While others could have, Wundt did. That is the event and the man that we commemorate in 1979. The event and the man serve as a token or a symbol that, in some nontrivial sense, things were happening in the second half of the nineteenth century that were going to produce a sharp change in thinking. It was the acceleration in the acquisition of information and ideas along each of these paths of inquiry, and the relevance of much of the information and many of the ideas to the formulation of what psychology was all about, that led to the conceptual explosions around the turn of the century.

The second component of the suggested model is that, as we examine each of the quarter-centuries since the first, i.e. since 1904, there is no discernible long-term systematic direction that has emerged following these explosions. There is no agreed-upon systematic mainstream. Psychology has been searching and sampling a variety of directions, trying different ways of talking about its subject matter, but no systematic view can be said to have become dominant for more than a couple of decades. This position may seem unduly pessimistic. This is not to say that there has been no scientific progress. Quite the contrary; the advances in information and understanding about the *specific questions of science,* for example, the day-to-day research concerns that relate to problems of human or animal learning, physiological mechanisms of hunger and thirst, and interpretations of group decision-making processes, have been great indeed. It is only when psychology tries to articulate what the science is all about that it encounters difficulty. Most psychologists feel the need to think along systematic lines, to put their research in some broader context. It is when psychology attempts to do this with some unity that the situation shows itself as being analogous to Truesdell's description of physicists and the laws of thermodynamics, "Every physicist knows exactly what the first and second law mean, but . . . no two physicists agree about them" (22, p. 22).

FIFTY YEARS OF SYSTEMATIC DEBATE

Even a brief examination of the periods that follow the turn of the century shows clear signs of the search for a unified way of thinking about the nature of psychology. The quarter-century from 1904 to 1929 was a period in which the scope and method of research in psychology were intensely debated. The "systematic" (as opposed to the "school") aspects of this period have probably been overdrawn; many of the diverse positions were too vague, too fluid and short-lasting to qualify as systematic positions. Nevertheless, it was an important period precisely because relatively dogmatic positions were adopted which provided targets for debate; such debate held the promise of laying a foundation for the kind of critical analysis that

would be required by the science at a later stage. Histories of this period paint the distinctions among the various "psychologies" much more clearly than they appear in the original, but that may be because our descriptions tend to focus on the "leader" rather than on the large number of scientists actually doing research. Boring said of these leaders, "Psychologically the attitude of these men tends to be dynamic and positive; they are quick to attack or to defend, they are possessed of a productive intolerance . . ." (4).

There can be no doubt that the controversy was intense; but the battles were less often between organized, united camps, and they were more often hand-to-hand fights on a shifting terrain and with mixed objectives. Consider structuralism, for example. It probably arose from a footnote in the writings of William James. It became a "system" rubric on the basis of an article by Titchener on the postulates of a structural psychology in 1898. That analogy with the morphological and physiological areas of biology etched into history a distinction that was the basis of much discussion, but was a very small part of the actual area of disagreement. Within 20 years Titchener had decided that ". . . 'functional' and 'structural,' as qualifications of 'psychology,' are now obsolete terms" (26). By the time Murchison began to capture the diversities of early twentieth century psychology in his volumes on the *Psychologies,* Madison Bentley, whose task it was to cover structural psychology, wrote in *Psychologies of 1925* "If we ask today who represents the psychology of 'structure,' I doubt whether we shall find anyone to acknowledge that his own brand is of that kind; though the epithet will often be accompanied by a gesture of indication toward a fellow-psychologist." Later in that same chapter Bentley wrote as if structuralism was already of another time. "We can hardly expect, therefore, to recover the structural point of view of that time unless we consider the counter doctrine of function against which it contended" (1). Five years later in the *Psychologies of 1930,* Nafe, a student of Titchener's, also wrote of structuralism as belonging to another generation.

> Many of the logical and metaphysical questions so important to another generation of psychologists have faded, unanswered, from the picture, and the present generation, impatient of such matters, prefers the risk of untenable positions and temporary confusions to the certainty of time lost in attempts to take positions upon question of fact before the facts are known . . . With the shift of emphasis from systematic considerations to experimentally observed facts, the distinctions between schools of psychology have tended to disappear. . . . (19).

Similar comments could be made about the functionalist view. In *Psychologies of 1930* the reviewer of functionalism says, "There is no functional psychology; rather there are many functional psychologies." That chapter was written by Harvey Carr of the "Chicago" school (within the functionalism school), and he provides an excellent example of the lack of specificity

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of the "systems" that were extant when he says ". . . we shall use the term functionalism to refer to the American empirical movement that rebelled against the proposed limitations of the structural or existential school of Titchener . . ." (10).

The number of psychologies represented in *Psychologies of 1925* was six; by the time of the *Psychologies of 1930* this number had grown to eleven. The sign that perhaps the science was beginning to mature by the early 1930s is seen not only in the quote from Nafe concerning the shift to experimental emphasis, but also by the fact that Murchison chose *not* to continue this series with a volume of *Psychologies of 1935.* Instead, in 1934 Murchison edited the *Handbook of General Experimental Psychology,* which still stands as a monument to science in action.

In America the third quarter of psychology's century, 1929–1954, was a period of self-consciousness about psychology as science and a period of concern about the nature of science. This was also a period in which systematic issues were hotly debated. It was a period that was ushered in by wide discussion of Bridgman's book *The Logic of Modern Physics,* and a concern about the impact of Operationism on the use of psychological concepts. Bridgman's book had a much greater impact on psychologists than on physicists; as someone said of this period, psychologists mistook a footnote in the philosophy of science for a philosophy of science. But the change in the third quarter far transcended that particular influence. The period was one of a search for an appropriate model or metaphor for psychology. Psychology was still puzzling over what kind of science it was, and it needed some guidelines to pull itself away from the disappointing fruits of the arguments of the "schools." This period of search for a scientific model began at a time when philosophy of science was itself in a kind of euphoric period. Bertrand Russell, Alfred North Whitehead, and Ludwig Wittgenstein were dominant figures in philosophy, and the prevailing mood was that "the empirical content of science could be expressed in the formulae of classical mathematics, and would therefore be arranged ultimately in a closed system of axioms . . ." (9). The goal then would be ". . . to establish a system of axioms from which all the phenomena of nature could be derived" (9). The proof that not even algebra could be represented in the manner being prescribed for science was not appreciated soon enough to save some of the energies expended in the early part of this period by psychologists.

Psychology's third quarter-century was a period that produced a book entitled *A Dynamic Theory of Personality* (15) that devoted the first 42 pages to a discussion of the differences between Aristotelian and Galilean modes of thought in psychology. It was a period that produced the volume by Hull et al on *The Mathematicodeductive Theory of Rote Learning* (12), in which

some 18 postulates and 86 definitions were generated with the net effect of accounting for the serial position curve in verbal learning and a few related phenomena. It was a period of topological diagrams of an individual's "life space," and vector diagrams of the dynamic forces acting on (or in) the organism.

In spite of the uncertain advances along systematic lines, this was a period of rapid growth of solid scientific research in many different areas. While there may have been an overly optimistic view of the fruits of mathematization and postulational procedures, it was a period in which the foundations for such techniques as factor analysis, test theory, scaling, etc were broadly expanded. In areas such as physiological and sensory psychology, human and animal learning, and social and abnormal psychology, the empirical bases of psychology as science were made as firm in that period as they have ever been. This period established psychology as a provable science, not just an "in-principle" science.

Yet, at the systematic level, this period also ended in disenchantment. The plan to solve psychology's systematic differences by being quantitative, precise, and explicit, the plan to formulate issues in such a way that they would be testable against alternative formulations did not succeed in any general way. Formulations that could be solved to many decimal places included terms that could not be contained. The broad programmatic formulations were still found to be remarkably immune to disproof. To borrow a metaphor from Medawar used in another context, "A lava flow of *ad hoc* explanation pours over and around all difficulties, leaving only a few smoothly rounded prominences to mark where they might have lain" (17).

There are two reasons for thinking that the changes in emphasis and direction that we have seen in the last 75 years cannot be viewed as a permanent progressive development, a systematic mainstream. The first reason is the nature of the rhetoric of change. The words that were used when (and after) these changes took place are not the words of scientific advance; they are more characteristic of the words of moral victory. For example, one very prominent cognitive psychologist, in commenting on the approximately three decades following the demise of the introspective method, referred to them as "thirty arid years" (20). Another psychologist summarized some of the changes in personality theory in this 75 year period under the title *The Death and Rebirth of Psychology* (23). Such phrases offer a sign of the apocalyptic view of some of these changes of viewpoint. Compare the phrase "thirty arid years" to the manner in which Max Born, the great quantum physicist, handled the changeover from Bohr's atomic model to quantum mechanical theory. Born had been asked to give a series of lectures at MIT just after the appearance of Heisenberg's first paper on the new quantum theory and after Born had written a paper with Heisenberg giving the problem a new mathematical framework.

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> Though the results contained in this third paper left no doubt in my mind as to the superiority of the new methods to the old, I could not bring myself (in beginning the lecture series) to plunge directly into the new quantum mechanics. To do this would not only be to deny to Bohr's great achievement its due need of credit, but even more to deprive the reader of the natural and marvelous development of an idea (7).

It is this notion of a cumulative building on previous achievements that seems to be lacking in our systematic debates. We seem to be unable to generate the kind of attitude expressed by Brett in discussing Wundt's contributions: "But no generous student of history would care to emphasize this change of mood as a disparagement of the life-work of Wundt. Progress is itself a kind of critic; but it does not despise the things it must discard" (8).

The second consideration that leads me to view recent changes more as exploration rather than advance is the transferability of attitudes from one period to another with frequent reversals back to earlier periods. Consider the following statements:

> The inclusion of subjective experiences in the world of reality knowable by the scientist (for us now defined as wanting to know all of reality, not just the shared, public portions of it) breeds two consequences at least. One is the obvious differentiation between the immediacy of experiential knowledge and the distance of what I have called "spectator knowledge" (16).

> In other words, whatever all men inevitably mean by the word I (the empirical *ego* of philosophy), whenever they say I think, or feel, or intend this or that; and whatever they understand others to mean by using similar language—thus much and no more, we propose at first to include under the term "mind" (14).

Is the first a part of the debate in the Wundt-Titchener era? Is the second from a discussion of self- or ego-psychology? The answer is no on both counts. The first is a 1967 expression of intent and interest from humanistic psychology; the second is a quote from Ladd's *Elements of Physiological Psychology* (1887), published over 90 years ago.

When we look at the changes that have taken place in each of the quarter-centuries since 1904, we do not find that they have been brought about by any crucial or provocative set of experiments, nor do we find any strikingly new or different set of ideas or theoretical developments that demanded the change. There are, in each case, new ideas and new experiments, but they did not cause the change in emphasis, they arose to fill a vacuum. The changes seem to have taken place when some particular effort or emphasis or strategy had run its course. One is reminded of the passage from T. S. Eliot:

> So here I am . . . Twenty years largely wasted/ Trying to use words, and every attempt/ Is a wholly new start, and a different kind of failure/ Because one has only learnt to get the better of words/ For the thing one no longer has to say, or the ways in which/ One is no longer disposed to say it. And so each adventure is a new beginning, a raid on the inarticulate/ With shabby equipment always deteriorating . . . (11).

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Reference was made earlier to the fact that one author had characterized the years from approximately 1930–1960 as "thirty arid years." In my opinion any objective analysis of the twentieth century will view that period as establishing psychology as a science in a sense that was not true in any other period. Yet the difference of opinion is important. The basis for that acrid view was that during that period it was relatively uncommon to encounter the use of the term "mind." The adjective mental was frequently used, but only in some areas of the psychological literature. It was a period that placed great emphasis on methodology and definition, with a resulting emphasis on behavior (although, it must be emphasized, not necessarily behaviorism in the "school" sense of that term). By the 1970s the wave of rejection of that third quarter and the return to an earlier mode of thinking had grown to the point that a popular textbook dealing with the psychobiology of sensory coding stated that psychology "is a science whose proper content is the set of the inner awareness . . ." A few pages later we find that

> . . . it is to the behavior of others that we must turn for our experimental data. This methodological twist often obscures the fact that the behavior is not the subject matter of the psychological sciences, but is rather only an approach to the real content, which is represented by such symbolic terms as consciousness, awareness, thought . . . (27).

It is clear that, for this author, conceding the methodological position of the behaviorist did not mean that the subject matter of the science had changed. Perhaps there is a little ambiguity with terms such as "subject matter" and "real content," but the passage raises an interesting problem. It is a problem similar to one encountered during the intense debate over operational definition of terms. The point being made is that the kinds of measurements one makes do not define a discipline. There are thousands of scientists whose laboratory measurements have the dimensions of voltage, current, or impedance. These scientists may be physicists, chemists, neurophysiologists, physiological psychologists, or members of any one of a number of other disciplines. Similarly, there are thousands of scientists studying behavior; zoologists, psychologists, anthropologists, linguists, sociologists, and economists are representatives of just a few disciplines. While measuring instruments may in some important sense define the "subject matter," a discipline is defined by the questions that it asks. That topic, the questions that psychologists ask, must be the focal point in any attempt to resolve systematic differences.

In trying to understand the systematic differences that have persisted within psychology for the past 75 years, perhaps the most important step we could take would be to search through our past and try to outline explicitly what kinds of questions were being asked. In particular, what questions were brought to the twentieth century along the many paths of

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scientific inquiry. Therein may lie a solution to the systematic deadlock that has existed in psychology. It may not be a solution in the sense that some view will "win." It may be a solution in the sense that there might be some agreement on what questions are empirically solvable, at least in principle. Stated differently, we may be able to agree on what problems are matters for science and what parts of our disagreements are matters for metaphysics. There could be no more fitting goal in commemorating Wundt, and the discipline he institutionalized, than to ask what parts of the psychological endeavor are science and what parts, by the nature of the rules of evidence and decision making, are more correctly placed in philosophy. While we think of Wundt as setting up a new domain of science, we must also remember that Wundt, for most of his tenure at Leipzig, was against separating psychology from philosophy within the university.

A SEARCH OF PSYCHOLOGY'S PAST

I would like to suggest that the hope psychology has for clarifying the nature of its differences in systematic viewpoint will depend on its ability to look back at the lines of inquiry that come into the twentieth century and to ask what questions were being asked.

I have been deliberately unspecific with respect to labeling the various paths of scientific inquiry that had a bearing on the development of psychology as science. This has been for good reason; any such partitioning of past research and theory into major paths is itself a theoretical statement about what the key questions are. I have also tried, perhaps to the point of being repetitious, to use the term "past" rather than the term "history." The reason for this stems directly from the nature of history. The search of our past that is being suggested here is not the same as a suggestion that we go back and read history. Psychology's systematic problems are not problems for history, they are problems for science, and it is important to recognize that the goals of history and the goals of science do not always coincide. A history is a story, and the writing of history is the telling of a story. There are many different ways of writing history, but in very few of them is the vocabulary and the syntax that of science. The kinds of questions asked, the rules of evidence, the guidelines for the presentation of evidence, the question of what constitutes an explanation, all of these are subject to different interpretations in these two domains of scholarship.

The suggestion is, therefore, that as we search the past in an attempt to identify the questions that might be a source of the systematic deadlock, we execute this search as scientists, not as historians. This implies that we should also temporarily suspend many of the views of our heritage that come from existing histories of our past. Many of our historical accounts

have been heavily influenced by the partitioning of problems and approaches into the rubrics of the "schools" of the early twentieth century. In many cases the histories were guided by the way in which a few psychologists of the first two decades of this century saw the influences acting on them. We have already seen the selective role that this kind of thinking can play when we examined the partial representation of Wundt's work. Historians, looking through the eyes of the "schools" could find no place for a *Völkerpsychologie* so it was ignored. In a similar vein Boring, in the Preface to his *History of Experimental Psychology* in 1929, could find no ancestors of behaviorism, while Esper (3) could write a *History of Psychology* years later with a strong behaviorist emphasis in which most of the analysis was based on the writings of the Greek philosophers.

As we search the past we will encounter ideas, events, and people. As historians we would have the freedom to select any one of these as the basis for constructing a story. As scientists looking for some resolution of systematic differences our interest will, of necessity, be on ideas. Yet we must strike a delicate balance between the ideas expressed and the empirical work that was being done in the context of those ideas. It is frequently easy to emphasize what a few individuals said about a few topics, and to forget to ask what difference these ideas made in what many other individuals did. After all, the number of distinctly different ideas that have been generated about "psychological" matters is very limited. What has tended to change over time are the different ways of asking specific questions related to these ideas and devising procedures for answering those specific questions. What we should be interested in is the manner in which, and the extent to which, the form and the content of the questions have been influenced by empirical observations, by experiments.

Questions will be encountered at two levels. The first level will involve what will be called short-term questions, i.e. those questions that directed individual experiments, or sets of experiments, perhaps individual programs of research. A few representative lines of inquiry can be listed. All of these lines had their roots partly in philosophy as did all branches of science at some stage. One line of inquiry came jointly from physics and from biology, but mostly from the former. This line of questions dealt with sensations and images. It included such investigators as Newton, Young, Müller, Fechner, Helmholtz, and Wundt. This line was to have widespread influence on all quantitative analyses in psychology.

A second line comes primarily from biology. It deals with questions of the localization of psychological functions. It has two main branches of research. One of these branches deals with questions of reflexive as opposed to voluntary functions. It would include the works of such men as Bell and Magendie, Sechenov, Sherrington, and Pavlov. The second branch would

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be the more traditional one of localization of function, and would include the names of such workers as Willis, Whytt, Gall, Fritsch and Hitzig, Hughlings Jackson, Goltz, Munk, and others. This branch would include the clinical literature on brain damage and psychological function.

A third line would have an inclusive label such as sociomedical. It would include such workers as Pinel, Charcot, Janet, and Freud; it would also include men such as Binet and Ebbinghaus, both of whom worked on mental testing in an applied setting. This branch would obviously overlap with a neurophysiological research line, as in the case of both Pinel and Freud, who had done physiological research in addition to the work for which they are most widely known. It would also contain the major lines of research in medical psychology with which all of these names (except Ebbinghaus) are associated.

A final line of inquiry comes jointly from chemistry and biology; it is linked to philosophy through its views on materialism. The early forms of this line of inquiry dealt with vitalism but found expression in attempts to reduce all living functions to physiochemical analysis. By the late nineteenth century this found clear representation in analyses of behavior of intact organisms in the work of men such as Loeb and Jennings.

The above outline provides one example of the kind of delineation that one would encounter if the emphasis is on specific experimental questions. The list includes paths of research that would provide the background for such areas as sensation, perception, learning, personality, and abnormal psychology. There is no doubt that this specific list blurs some areas and misses others. It could be extended to include other areas, and it could be elaborated to describe in detail more specific research questions. It is offered as an example of the first level of ideas that could be examined. While there could be extended discussion about the details of such an outline of questions, it is not likely that there would be intractable parts. Because each entry in such an outline is relatively closely tied to a research area and because a given experiment or the work of a given person could appear in several lines of inquiry, each line of questions can be established without regard to other lines even though they may turn out to be highly related. No intransigent problems are likely to arise as long as one avoids questions of the relative importance of the various lines of inquiry. It would be at that point that one would uncover systematic differences.

There is a second level at which questions have been asked. If one looks back at the history of science, one can trace at least three lines of inquiry, three sets of questions whose articulation was becoming provocative by the turn of the century. The answers to each of these sets of questions were to have a profound influence on the way individuals viewed the problems of psychology. These questions concern (*a*) the nature of mind, (*b*) the nature

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of man, and (*c*) man's place in nature. These sets of questions are highly related, yet they are clearly distinct. Each category has a long background both in philosophy and in science. All of the different scientific paths to modern psychology dealt in key ways with one or more of these questions. By the first decade of the twentieth century it was clear that how one dealt with these questions had a direct bearing on what psychology was to become as a science and whether psychology would lie exclusively within the cloak of science. Every psychological system was to acquire, at least implicitly, some position with respect to these three issues. Yet each system could place a different weighting on each of these lines of inquiry.

Under the rubric of the nature of mind are questions of sensations, images, feelings, ideas, thinking. Historically the study of mind has been tied to the concept of consciousness. The problems center around how to characterize the things that happen and/or the things we do when we are conscious. One must delineate what kinds of questions about mind are permissible. Is the link with consciousness a necessary one? Do we have to define consciousness or is it taken as obvious, therefore as given? If some procedure is to be specified for its recognition, does that procedure depend on the phenomena of learning? Does it require language? Should we limit our conceptualization to the adjectival forms, i.e. restrict our terms to mental and/or conscious? What other questions can we ask? Can we ask where it is located, what it does, how it works? Is controlled introspection an acceptable experimental procedure for studying it? If it is not, then in what ways are the mind (or consciousness and awareness) when defined by the "methodological twist" of behavior different from the mind that is so palpably a subject of everyday discourse?

Under the heading of the nature of man we encounter a set of questions that goes back to the Greeks on the philosophic issues and has had several distinct lines of scientific inquiry since the seventeenth century. The rational, the passionate, and the vegetative souls of the Greek philosophers have had their parallels in the scientific study of man and other animals in several areas of biology as well as in psychology. The questions involve the concepts of volition and choice, notions of voluntary behavior and its relation to reflexive and instinctive behavior. Views along the dimension of determinism and "freedom of choice" are diverse and are found along many of the paths of scientific inquiry. Three quarter-centuries of research have accumulated since the publication of Sherrington's *Integrative Action of the Nervous System* and since the work of Jennings and Loeb on the behavior of "lower organisms." Key theoretical and experimental advances have been made in our understanding of these areas of research, yet labels such as reflexive behavior, instinctive behavior, and voluntary behavior retain an

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emotional component. They take on positive or negative properties depending on one's point of view and the context in which they are used.

The set of questions raised under the heading of man's place in nature also extends back to antiquity. It involves the concept of the Chain of Being that began with Plato and was articulated by Aristotle. We tend to associate the phrase with Thomas Huxley and his exposition of Darwin and the implication of his work, but the issue has two distinct histories. On the one hand, there is the tradition in philosophy dealing with the continuity of forms of life, and the relation to the views of a Supreme Being. On the other hand, there is the tradition in science dealing with fossils, body structures, speciation, and other matters that are associated with the names of Darwin, Lyell, Cuvier, Huxley, and others. These are two different traditions that are frequently not distinguished.

Psychology has been assigned, or has assumed, the task of attending to aspects of the mind and of conduct. When it decided to become a science it assumed an additional responsibility. It had to restrict the way it studied, explained, and talked about these topics. The three lines of inquiry just outlined conspired to make this task difficult. Each possessed face validity as having some bearing on the question of how to define psychology. The answers to each set of questions had clear implications for some of the questions in the other lines of inquiry. One difficulty was that the rules for evaluating the relative importance of the evidence for the different kinds of questions were not clear.

What emerges from a detailed examination of some of the questions just outlined is the view that many of the arguments between and among the "schools" were never joined because they involved different levels of discourse and were based on different assumptions. In looking back at the systematic discussions of the past, it seems necessary to distinguish between (*a*) what they were arguing about and (*b*) the basis on which each was arguing. For example, a behaviorist could adopt a certain view of the nature of man on the basis of a specified line of research. This might lead him to a view of mind that would be difficult to change by evidence restricted to the study of mind alone. Similarly, the introspectionist could adopt a certain view of mind on the basis of specific lines of research, and this view might be relatively immune to data and theory coming from a different set of questions and experiments.

Such considerations suggest an important strategy in cases of important differences in systematic views. For each position the question should be raised regarding what kind of evidence would be required in order to produce a change in view. Such an exchange could lead to the articulation of which of the several origins of psychology is playing the dominant role.

If it is not possible to delineate the kinds of evidence that would lead to a change in position, the primary question then must turn to one of whether the discussion is in the domain of science.

This leads to a final comment on psychology's heritage as a science. It has been suggested that Wundt institutionalized psychology as a discipline. There is no way, and no need, to subtract from that accomplishment. His emphasis on controlled observations and "objectivity" was an attempt to separate psychology from philosophy, although he was against a formal administrative separation at the university. We have also suggested that Wundt's way of thinking about psychology, and the thinking of those that followed in his tradition, did not contain the essential ingredients that could have generated twentieth century psychology. One sense in which this is true is paradoxical. Regardless of the level at which we review the past, whether it is at the empirical level, close to the areas of research of present interest, or whether the review selects the research that skirts the edges of metaphysics, as with some of the questions just outlined, one encounters one compelling observation. It is this: as we examine the many lines of thinking that come from, for example, the study of sensation by Helmholtz, the research on human learning by Ebbinghaus, the developments in mental testing by Binet, the work on localization of psychological function by such men as Fritsch and Hitzig, Ferrier, Hughlings Jackson, and others, the analysis of animal behavior by individuals such as Loeb, Jennings, Lloyd Morgan, and Pavlov, one is struck by the fact that all are characterized by experimental or observational procedures that are still acceptable as bona fide scientific procedure. The one line of inquiry that specified, and was based on, an "experimental" procedure that is not judged acceptable as a method of scientific investigation at the present time is the line established by Wundt. The paradox is that psychology has selected as the founder of its science a man whose line of inquiry brought with it no acceptable experimental method. The Wundtian and related traditions brought to the twentieth century some interesting psychological questions; yet they brought no method for demonstrating whether the questions were for science or philosophy. The other lines of inquiry were to furnish psychology with the method to become a science. Any eventual resolution of the relative importance of psychology's origins will have to evaluate the implications of this paradox.

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