THE RISE AND FALL OF MENTALISM

IN EXPERIMENTAL PSYCHOLOGY

by

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PREFACE

If I may be permitted what I imagine is an understatement, an historical analysis of early experimental psychology (c. 1875-1913) is a somewhat unusual dissertation topic for a degree in clinical psychology. Thus some justification seems necessary.

Clinical psychology is still a part of the general field of psychology, despite the feelings of some non-clinicians. For this state of affairs to remain justifiable, the clinical psychologist must have a coherent conception of psychology as a whole, including a recognition of the intersections between clinical and non-clinical psychology. A grounding in general psychology is presumably gained through coursework by the end of the first year of graduate training, but no amount of coursework can supply a unified, coherent picture of the field because there is no such single conception shared by members of the field. Indeed, there seems to be almost as many conceptions of what psychology is all about as there are practitioners of the discipline. To complicate matters further, many of these conceptions would relegate one or another substantial segment of the field to the status of irrelevancy. In general, it is disparity rather than coherence which seems to be characteristic of most conceptions of the field.

There are at least three methods by which a student may acquire his overall conception of the nature of psychology. He may, during the course of his educational career, become a convert to some particular "school." Or he may come to term himself an "eclectic" and consider himself "atheoretical," having assimilated various ideas along the way with no sense of continuity between them. Finally, he may decide to engage in some form of study with the explicit intention of creating for himself a coherent view of the field as a whole. It is this latter alternative which I have chosen for myself and which led me to request the opportunity to write a non-experimental dissertation.

As a result of past influences, the nature of which I have presented in Chapter II, it seemed to me that I could best begin my project with some more intensive study of the history of psychology. Since general experimental psychology has always seemed to hold a central position within the field, I felt that this was the area of psychological history with which I should begin. In addition, experimental psychology has played a dominant role in American psychology generally, and has been a major source of influence in some radical breaks with clinical tradition (i.e., behavior therapy and some of the newer cognitive based approaches). These ties between clinical and experimental psychology need to be strengthened, for to the extent to which the two areas do not resonate with each other, there must be something seriously wrong with at least one of them. After all, the work in both areas is presumably based on a single set of fundamental principles which are responsible for determining the behavior of living organisms. For this reason as well, the clinical psychologist should have an intimate acquaintance with general experimental psychology.

Further justification for the present work is contained in the introductory chapter on the impetus to scientific psychology. In that chapter, I have attempted to make the point that the history of psychology, as well as the history of science generally, is an important data source for investigating the nature of human cognition, and that this focus of psychological activity is relevant to the business of all science.

Chapter II summarizes the new historicist approach to the philosophy of science as typified by Thomas Kuhn's important Structure of Scientific Revolutions, and contrasts this approach to more traditional conceptions of the nature of science. Part of the multi-faceted relationship between this new approach and the field of psychology is also discussed. Kuhn's model provides the conceptual framework from which I have viewed the historical data. Some of the significant objections to Kuhn's approach from other philosophers of science are discussed in the appendix.

Chapters III and IV investigate the early mentalist paradigm of experimental psychology, its various articulations and their interrelations, and the crisis which developed within mentalism and which paved the way for the behaviorist revolution. The concluding chapter examines the fit between the historical data and the conceptual model described in Chapter II.

Having stated my aim of creating a coherent view of the work of psychology, I must note that this ambitious task is not completed by this dissertation. It still remains to extend the investigation to the later historical development of the field and to relate it to the development of clinical psychology. This constitutes part of my planned future activity as a professional psychologist. Nonetheless, I believe that this dissertation represents a good start. In addition, I hope that it will prove of some value to other psychologists who are interested in the history of the field and/or in the nature of human intellectual functioning.

I would like to thank Dr. Arthur Millman of the philosophy department at the University of Southern

California for his willingness to sit on my dissertation committee and for critiquing the project from the philosophical point of view. Thanks are also due to Dr. James Kahan, who sat on my guidance committee, and to Dr. A. Steven Frankel, who participated on both my guidance and dissertation committees. I thank them for their readiness to allow me to undertake such an unusual project for my dissertation, and for their many expressions of confidence in my ability to carry it through. These expressions of confidence have helped to sustain me through some of the more difficult periods of the project.

Special thanks are due to two scholars for whom I have the highest respect. Dr. Michael Wapner first introduced me to the ideas of Kuhn, stimulated me in discussions of the many important implications of those ideas to the business of psychology, and in general, played a major role in determining the direction of my thinking as a psychologist. Dr. Milton Wolpin, the chairman of my dissertation and guidance committees, has nurtured and stimulated me both intellectually and emotionally throughout my graduate career. He has allowed me to feel free to intellectually free-associate, without fear of condemnation. At the same time, his criticisms of my various speculations have been both valuable and incisive.

Drs. Wolpin and Wapner have been my teachers, my colleagues and my friends. To whatever extent I may be judged a scholar, these two men deserve a great deal of the credit.

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CHAPTER I

THE IMPETUS TO SCIENTIFIC PSYCHOLOGY: A RECURRENT PATTERN

Experimental psychology developed as a synthesis between philosophy and nineteenth century physiology. The nature of the human psyche was a subject of speculative philosophical concern for centuries. Aristotle wrote on the subject, as did Descartes, Hobbes, Locke, Berkeley and many others. They wrote on the nature of memory, understanding, vision, learning, dreams, etc., all topics which later became mainstays of psychology as an independent discipline.

In the nineteenth century, physiology, then a fledgling science itself, began to attend to the mechanisms underlying perception. Thomas Young developed a three color theory of color vision, which was elaborated by Hermann von Helmholtz and then opposed by an alternative theory developed by Ewald Hering. Johannes Müller formulated a doctrine of "specific energies" of nerves which he held responsible for qualitative differences in sensation. The study of quantitative aspects of perception (psychophysics) was developed by Ernst Heinrich Weber and Gustav Theodor Fechner, and later became a staple of experimental psychology.

But the specific impetus for the development of experimental psychology as an independent field came from the oldest and one of the most respected of the physical sciences, astronomy. It was a methodological problem in astronomy that convinced the scientific community that their knowledge of the physical world could not be complete without an understanding of the mental processes through which the scientist was able to make his observations. The first purely psychological experiments and the main area of work during the first decade of the psychological laboratory at Leipzig developed directly out of the efforts of astronomers to solve their problem.

Later, a much more difficult problem in theoretical physics caused many physicists to turn towards questions of psychology. They too became convinced that their efforts to understand the physical world needed to be supplemented by an understanding of their own mental processes. The effects of this latest impetus from physical science is still being felt in psychology. These two parallel stories are the subject of this introductory chapter.

From Astronomy to Psychology

The Personal Equation

Our first story begins at the Royal Observatory at Greenwich in 1795. One of the important activities at the observatory was the recording of the times of stellar transits which could then be used for calibrating the clock for all other astronomical observations. The technique that was used for determining the time of transit was known as the "eye and ear" method. The field of the telescope was divided by a number of parallel wires set at equal distances apart. The middle wire corresponded to the meridian which the star was to cross. The observer would note the exact time and would then begin counting the beats of a pendulum. After noting the positions of the star at the time of the beats immediately preceding and immediately following the crossing of the meridian, he would be able to estimate to within a tenth of a second the exact time at which the meridian was crossed.

In 1795, this method was considered accurate to within two tenths of a second. But in August of that year, Nicholas Maskelyne, the "astronomer royal" at the Greenwich Observatory noticed that his assistant's estimates of the times of transits were five tenths of a second later than his own. These anomalous results were handled in the manner in which anomalies are usually initially treated; they were blamed on the ineptitude of the individual scientist:

> I cannot persuade myself [wrote Maskelyne] that my late assistant continued in the use of this excellent method (Bradley's) of observing, but rather suppose he fell into some irregular and confused method of his own, as I do not see how he could have otherwise committed such gross errors.[1]

The assistant, David Kinnebrook, was cautioned to take greater care in the accuracy of his observations. But despite Kinnebrook's best efforts, the discrepancy between him and his supervisor increased rather than decreased. By January, 1796, it had increased to eight tenths of a second and Kinnebrook was dismissed from his position.

This incident might have gone by as less than a minor footnote in the history of science had not a young German astronomer, Friedrich Wilhelm Bessel, taken notice of it some twenty years later. Bessel hypothesized that Kinnebrook's "error" was not due to ineptitude or sloppiness, but that it might instead be due to involuntary individual differences which might be found even between the most capable observers. Testing this proposition by comparing his own observations with those of other astronomers, he determined that these differences did exist, and further, that there was even variability in the magnitude of the difference for any given two observers between different sets of observations. Bessel represented the average difference between any two observers of stellar transits as a "personal equation." It took the form A - B = t, in which A and B are the estimates of particular astronomers, and t is the average discrepancy between them in their estimates of the time of stellar transits.

The discovery of the personal equation had a number of reprecussions. At first, equations were calculated for individual pairs of astronomers at a number of observatories, and were used to correct for the differences. But because of the continued variability in the personal equations of any two observers, a search was undertaken for a method of observation which would reduce or even eliminate the discrepancies. Finally, in 1854, the eye and ear method was replaced at the Royal Observatory by the newly developed chronograph, which reduced the average discrepancy between observers to about one tenth of a second.

But this did not end the interest of astronomers in the personal equation. During the 1860's and 1870's experiments were undertaken with points of light replacing stars. Since the actual time at which the artificial star crossed the wire could be automatically recorded, it was possible to derive an absolute personal equation for any observer; i.e., the difference between the real and the estimated time at which the bisection occurred. In addition, continued experiments with actual stars, but using the chronograph as a control, led to the discovery that the magnitude of the personal equation varied as a function of a number of astronomical factors, such as the magnitude of the star in question.

The Complication Experiment

After the studies of the 1870's, astronomers lost interest in the personal equation. But by that time, it had attracted the attention of physiologists, and through them, made its way into the field of psychology. Among others, it attracted the attention of Wilhelm Wundt, who, in the early 1860's, was an assistant in the physiological practicum at the university in Heidelberg. Wundt focused on the fact that the eye and ear method depended on input from two distinct sensory modalities and developed what became known as the complication experiment.

Complication, a term which Wundt borrowed from the psychological philosopher Johann Friedrich Herbart, was used to denote any mental complex which involved input from more than one sensory modality. Wundt's interest was in the time relations involved in complications, as manifested in the personal equation. He constructed a pendulum which swung across a scale marked off in degrees and which caused a click to sound at some predetermined point of its journey. The task for the observer was to determine the point on the scale which the pendulum had reached at the time of the click.

Two sets of independent variables were investigated in these early experiments: the rates of both pointer and click and the number of simultaneously presented stimuli. Wundt discovered that at moderate velocities and with only the pointer and one sound as stimuli, the sound was associated with too early a position on the scale, as if it had been heard before it actually occurred. As the rate of the pendulum and its accompanying sound increased, and as new simultaneous stimuli were added, such as additional sounds or electric shocks, the direction of the error changed and the click was perceived to occur later than it had actually occurred. Wundt explained these results on the basis of an adjustment of attention which he hypothesized to be necessary if the comparison between the position of the pointer and the occurrence of the sound was to be made.[2] When the rate of the pendulum is slower than the rate of this adjustment of attention, then the sound could be anticipated before its actual occurrence. On the other hand, when the adjustment requires more time than is provided between cycles, either because the rate of the pendulum is increased or because the adjustment task is complicated and made more lengthy by additional stimuli, then the adjustment is made too late and the pointer is seen beyond where it was when the sound actually occurred.

Later modifications of the complication experiment involved the locus of the observer's attention, and it was in this form that it survived in experimental psychology.[3] First the observer was directed to attend to the visual stimulus; i.e., the pointer, and then, in a subsequent observation, to attend to the sound. It was found that the former condition resulted in the perception that the sound occurred later than it actually had, and in the latter condition, in the impression that it occurred earlier. Wundt's interpretation of this phenomenon was essentially the same as his explanation of the results of the earlier form of the complication experiment. The task of comparison required an accommodation of attention, either to sight or to sound. Thus, when attention is initially directed toward the visual stimulus, "the pointer gets to 30° before the bell (which rang at 22°) is heard; in the second [condition], the bell is heard (at 22°) when the observed position of the pointer is only some 15°."[4]

Complication experiments never became a primary focus of experimental psychology, yet they were to exercise "a determining influence upon Wundt's psychological system,"[5] and thereby on all experimental psychology. In Wundt's hands, the personal equation became the first purely psychological experiment. Experimental psychophysics had already gotten under way with Weber and Fechner, but this work was never seen as purely psychological. Rather, it was a hybrid area, like biochemistry, and had a legitimate place only at the periphery of psychology. Psychophysics was concerned with the mapping of correspondences between physical and mental events. The central concern of psychology, however, was the investigation of the mental event itself. In the hands of astronomers, interest in the personal equation was tied to the need for precise objective observation. Their concern was to eliminate, or at least control for, the influence of contaminating psychological factors. Wundt, on the other hand, used the complication experiment to measure a purely psychological process: the accommodation of attention. He finally determined that this process required about one and a half seconds, a fact which was attended to in the design of later introspective experiments.

The Reaction Experiment

The complication experiment was not the only progeny of the personal equation. It also gave birth to what was to become a central focus of early experimental psychology: the reaction-time experiment. The absolute personal equation as determined by the eye and ear method consisted of a reaction to stimuli complicated by the fact that there were two stimuli involved, each related to a different sense organ. With the development of the chronoscope, the complication was removed; only one stimulus was involved (the visual stimulus) and the error was thus considerably reduced. Physiologists were able to further modify this procedure in order to measure the speed of neural impulses, and subsequent modifications led to the attempt to measure the time involved in various mental processes.

The physiologists' modifications constituted quite a breakthrough for the field. In 1837, Johannes Müller had declared that science would "never develop methods for determining the speed of nerve action."[6] But a few years later, Hermann von Helmholtz developed a method of doing just that and put it to use, first measuring the speed of conduction in the motor nerves of a frog and then in the sensory nerves of human beings. Helmholtz's strategy consisted of stimulating the motor nerve of a frog at two different distances from the muscle, or sensory nerves at two different locations on the human body, one closer and the other further away from the brain. From the difference in response latency, he then calculated the speed of conduction.

One particular aspect of Helmholt's interpretation of his results with humans had a profound effect on the development of experimental psychology. Helmholtz was able to measure directly only the total time elapsing between initial stimulation and muscular contraction. By subtracting the interval associated with stimulation close to the brain from that associated with stimulation further away from the brain, he obtained his measure of the speed of conduction in sensory nerves, Then, assuming that the rate of conduction was the same for both sensory and motor nerves, and taking the time involved in activating a muscle contraction as determined by other, independent experiments, Helmholtz attempted to determine the time involved in the act of willing the muscle to contract. This was done by subtracting the time involved in conduction and activation from the total reaction time.

Early in the 1860's, the subtractive method was picked up by a Dutch physiologist, Franciscus Cornelius Donders, and his student Johan de Jaager, who turned to the measurement of explicitly mental processes. The result was de Jaager's doctoral dissertation, Reaction Time and Mental Processes,[7] a study which was planned, designed and supervised by Donders. The dissertation began with the calculation of the speed of conduction in sensory nerves according to the strategy devised by

Helmholtz. Then the psychological work began. Subjects were required to discriminate between two stimuli and to react with either the left or right hand, depending on which stimulus was presented. The extra time required for this task was interpreted as the time involved in discrimination of the stimulus and choice of the appropriate response.

Later, Donders[8] attempted to separate these two component mental processes. This led to the development of his 'a', 'b' and 'c' methods. The a-method called for a given reaction to a known stimulus. The b-method required the subject to discriminate a particular stimulus and to react with the corresponding response. These are essentially the methods used in de Jaager's dissertation. The time difference between them was taken to represent both discrimination and choice. To these, Donders added his c-method, reacting with a pre-determined response only to the correct stimulus; the other stimuli were not to be responded to at all. This c-method required discrimination of the appropriate stimulus, but no choice of response. Thus it was possible to calculate the time required for both discrimination (c-a) and choice (b-c).

Unlike the complication experiment, the reaction experiment was to become a major focus of early experimental psychology. More generally, the focus was on the task set by both types of study: psychometry, the measurement of the duration of mental processes. During the first decade of Wundt's Leipzig laboratory, no other subject received more attention, and the method of studying this question was essentially that which had been developed by Donders and his students. "Nearly half the researches undertaken in the Leipsic [sic] laboratory are concerned with this subject," wrote James Cattell in 1888.[9] The second most researched area in this first period was psychophysics, but, in contrast to psychometry, this area was not considered "purely" psychological.

Later, the subtractive method was called into question, but the reaction experiment remained an important component of experimental psychology. In these later days, it was used as a "control of introspection," aiding the study of the quality of mental processes.[10] Thus, if reaction times differed substantially for two introspective observers who were supposedly engaged in identical tasks, it was assumed that different mental processes were taking place.

From Physics to Psychology

We have seen that the initial impetus for experimental psychology came from a problem in astronomy. Eventually, the astronomical problem was circumvented by non-psychological means, but by that time, it had caught the attention of physiologists and was seen to pose problems which were interesting in their own right. More generally, it had drawn attention to the role of the human observer in scientific observation and to the importance of studying the nature of the observational process.

Today, a similar question is being posed by problems in physics, and a number of physicists have turned their attention to questions of psychology. The problems are those which have been posed by the unprecedented revolutions in theoretical physics which occurred early in the twentieth century. Before these revolutions, physics was more or less rooted in a world which was closer to the common sense world of the normal adult. Both the physicist and the layman lived in a world of real numbers, Euclidean space and linear time. Imaginary numbers were first used in the sixteenth century by Jerome Cardan, but the label "imaginary" which was attached to such numbers clearly specified their assumed unreal status. It was not until the nineteenth century, some three hundred years later, that the mathematician Karl Gauss suggested that an objective existence could be assigned to so-called imaginary numbers, and he recognized that in so-doing he was swimming against the tide; that imaginary numbers were "still rather tolerated than fully naturalized; they appear more like an empty play upon symbols, to which a thinkable substratum is unhesitatingly denied."[11]

Non-Euclidean geometries were invented in the nineteenth century, but as was the case with imaginary numbers before them, they were not conceived of as representing physical reality. But then, early in the twentieth century, Einsteinian relativity and later the development of quantum mechanics transformed these physical impossibilities into representations of the fundamental reality which the science of physics was attempting to describe.

These examples may easily be multiplied. Prior to the modern period, time was a linear phenomenon, both transitive and additive. Thus, if time A is earlier than time B, and if time B is earlier than time C, then time A is earlier than time C. Also, the interval between time A and time C, in such a case, is equal to the interval between A and B plus the interval between B and C. But in the non-Euclidian, non-linear space-time of the modern physicist's reality, neither of these principles necessarily holds, so that where A and B are simultaneous and B and C are simultaneous, A and C may not be simultaneous.

Similarly, dimensions which to both common sense and classical physics are continuous, are now being conceived of by some physicists as composed of discrete units. The conceptual peculiarities involved in such a position become clear when one considers the effect that it would have on our ideas of motion. We measure motion by the length of the distance travelled. But if space is discontinuous, then we must conclude that there exists two points through which a body has traveled without transversing the space between them. A similar situation holds with regard to velocity and acceleration. Since velocity is measured by distance divided by time, it stands to reason that if distance and time are composed of discrete units, then there are only a limited number of possible velocities. Therefore, acceleration must also be discrete; an event must accelerate from velocity A to velocity B without ever being characterized as traveling at some velocity between A and B. All of these "impossibilities" have become realities, or at least hypothetical realities of the world of the contemporary physicists.

It is these kinds of conceptual transformations, flowing from relativity and quantum mechanics, which have led a number of physicists to become concerned with questions of psychology. This occurred not so much because of the new data which has been uncovered by twentieth century developments, but because of the effects which the conceptual revolution had on the old data, demolishing much of what had been accepted as proven scientific fact. Linear, continuous, Euclidean space and time, which had been the unquestioned and unquestionable ground of observation, is now taken by some scientists to refer "only to comparatively superficial aspects of physical reality."[12]

The ground has been shifted, and as a result, the character of the observed data has changed. This quite naturally leads to a focus of attention on the contribution of the ground to the character of the observed figure. In non-psychological terms this became the concern of relativity theory, which takes into account the position and velocity of the observer in his account of the position and velocity of the observed. But it subsequently led to a focus on the conceptual position of the observer, and this necessarily enters into the realm of psychology.

Of all the physicists who have attended to this concern, P. W. Bridgman, through his discussion of operationalism has had the greatest impact on psychology. Bridgman acknowledged that he felt himself forced "to map out the possibilities and the limitations of the human mind in dealing with the problems presented to it,"[13] by the failures of the physicists preconceptions, as revealed by the adjustments demanded by relativity. He proposed to accomplish this task by analyzing the "operations" by which the scientist observed and measured the entities which he was studying.

During the 1930's, experimental psychologists attempted to apply Bridgman's concept of operationism to their own field. But in so doing, they concentrated only on physical operations which were "public" and "repeatable." S. S. Stevens asserted that we could know nothing about private experience, "because an operation for penetrating privacy is self-contradictory."[14]

Now it is true that Bridgman concentrated a great deal of his attention on the physical operations, and in particular, on the measuring instruments used in physical science. This was especially important because many of the instruments of contemporary physics allow observation or measurement only by altering that which is being observed or measured. But Bridgman's far from finished project involved "mapping the limitations of mind," and as such, he was concerned with "private" mental operations as well as with "public" physical operations. This was made even more explicit in his later work,[15] in which he proposed the mental operation of "projection" as a means of giving public meaning to the private words of introspective observation. Projection is, so to speak, "an operation for penetrating privacy." The main point that Bridgman seems to be making is that even in pure physics (or perhaps, especially in physics), it is not possible to understand the world around us without "understanding the process of understanding, that is, . . . understanding the nature of the intellectual tools with which we attempt to understand the world around us."[16] It is to this latter task that Bridgman now gives priority.

If I have focused on Bridgman, it is not because he is alone in his concerns, but rather because his voice has been heard above others by psychologists generally. Similar concerns have been raised by other physical scientists.[17] The same issues have also become a "hot" topic in philosophy of science,[18] but it is to psychology that the appeal is made. After some preliminary considerations, Bridgman[19] jumps from a chapter on physics to a chapter on psychology. He attributes his general approach to the insight he gained from the perceptual illusion demonstrations of Ames and Cantril and their associates. Polanyi makes a similar appeal to Gestalt psychology, and Oppenheimer cites with unrestrained admiration the work of Jean Piaget.

Indeed, it is Piaget's model of psychology which has come closest to answering the physicist's call. The mental operations which he infers from the behavior of children correspond to the mental operations of scientists discussed by Bridgman. Conant's "conceptual schemes" of physical scientists parallels Piaget's "schemata." Indeed, Conant seems to have anticipated Piaget in calling attention to this comparison. In 1951 Conant pointed to "the connection between the attempts of generations of scientists to develop and improve a series of conceptual schemes connected with experimentation and the process by which an infant learns to find its way around objects and personalities."[20] Piaget had already been attempting to analyze the process by which children developed the cognitive schemata of Euclidean space and Newtonian time out of more primitive structures, and in 1971, he too suggested that this work might be related to the process of scientific development, a subject which he refers to as empirical epistimology.[21]

Physical Science and the History of Psychology

The parallels between the nineteenth century impetus and the twentieth century impetus from physical science to psychology are obvious. In the nineteenth century, a relatively minor problem in astronomy raised the question of the role of the observer in quantitative terms, that is, in terms of the time required for mental processes and the effect of that interval on the accuracy of observation. From this concern, the science of experimental psychology developed.

In the twentieth century, a major problem resulting from revolutionary advances in physics called attention to the role of the observer in qualitative terms, that is, in terms of the nature of the mental operations by which information is processed and the effect of those operations on the nature of the observed event. Toobe sure, not all physicists have seen the problem in this way. But some have, and their concerns have caught the attention of psychologists. At this point, it is still too early to judge the impact of these concerns on the field of psychology. In 1964, Sigmund Koch noted that ". . . While this wave of interest has gathered, psychology and the social sciences have stood on shore, almost untouched by the spray."[22] Yet there are indications that more and more social scientists are leaving the shore and entering the water, as seen in the spread of the new structuralism and related approaches into more and more areas of psychology and in its increasing influence in social science generally.[23]

The present wave should also affect our view of the function of studying the history of psychology. This study is generally justified by recourse to

Santayana's classic maxim that those who do not know their own history will be condemned to repeat it. The scientist must know what has been tried in the past and what the consequences were if he is to avoid making the same mistakes. There are two problems with this approach. First, it does not tell us what aspects of our past to look at unless we can fully anticipate our future concerns. More importantly, what we see as the mistakes of the past depends in part on where we are conceptually in the present, and is therefore subject to change. The attempt to make the study of mind the subject of psychology was once a mistake of the past. Today it is becoming less of a mistake.

A second reason for studying the history of psychology is as a means of gaining an overall conception of the nature of the field and of its direction. This is both important and valid. But there is yet another reason, one which is intimately tied to the tasks demanded of psychology by the problems of the revolution in physics. Psychology has itself become an independent scientific discipline for 100 years. We have undergone our own conceptual transformations and conflicts. If the history of science in general is a source of data which may be used to study the dynamics of conceptual development and the structure of conceptual schemata, then the study of the history of psychology can serve a double function. In addition to helping us clarify our conception of the field, it is also a legitimate data source for new work in cognitive psychology.

This dissertation was begun with both of these latter aims in mind. It begins with a discussion of the conceptual framework from which I have viewed the historical data. For if a conceptual framework can have so profound an impact on the data of physics as the twentieth century scientific revolution suggests, then certainly it must have no less important an impact on our view of historical data. To profess to some sort of pure objectivity in this kind of endeavor would be delusory at best. Rather, it seems necessary to make explicit, insofar as this is possible, my own assumptive biases. This is what I have attempted to accomplish in the next chapter.

FOOTNOTES TO CHAPTER I

[1]Edmund C. Sanford, "Personal Equation," American Journal of Psychology 2 (1888):4.

[2]Ibid., pp. 415-416.

[3]Edward B. Titchener, A Text-Book of Psychology, enlarged ed. (New York: MacMillan, 1910), pp. 296-299.

[4]Ibid., p. 298.

[5]Titchener, Experimental Psychology (New York: MacMillan, 1905), 2, pt. 2:407.

[6]Quoted by Johan de Jaager, "Reaction Time and Mental Processes" (1888), in Josef Brozek and Maarten S. Sibinga, trans. and eds., Origins of Psychometry (Nieuwkoop: B. de Graaf, 1970), p. 34.

[7]Ibid., pp. 33-76.

[8]Franciscus C. Donders, "On the Speed of Mental Processes" (1868), in W. G. Kester, trans. and ed., Attention and Performance II (Amsterdam: North-Holland Publishing Co., 1969), pp. 412-431.

[9]James M. Cattell, "The Psychological Laboratory at Leipsic," Mind, o.s. 13 (1888):45.

[10]Titchener, Text-Book, p. 431.

[11]Quoted in Tobias Dantzig, Number: The Language of Science (New York: Free Press, 1954), p. 190.

[12]Michael Polany, Personal Knowledge (Chicago: University of Chicago Press, 1958), p. 15.

[13]P. W. Bridgman, The Nature of Physical Theory (New York: Dover, 1936), p. 2.

[14]S. S. Stevens, "Psychology and the Science of Science," Psychological Bulletin 36 (1939): 221-263. Reprinted in Melvin H. Marx, ed., Theories in Contemporary Psychology (London: MacMillan, 1963), p. 53.

[15]Bridgman, The Way Things Are (Cambridge: Harvard University Press, 1959).

[16]Ibid., p. 1.

[17]Polanyi, *Personal Knowledge*; James B. Conant, *Science and Common Sense* (New Haven: Yale University Press, 1951); Robert Oppenheimer, "Analogy in Science," *American Psychologist* 11 (1956): 127-125.

[18]Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2d ed. (Chicago: University of Chicago Press, 1970); Norwood R. Hansen, *Patterns of Discovery* (Cambridge: University Press, 1965); Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: University Press, 1970).

[19]*Way Things Are*.

[20]*Science and Common Sense*, pp. 31-32.

[21]Jean Piaget, *Psychology and Epistimology* (New York: Viking Press, 1971), pp. 23-27.

[22]Sigmund Koch, "Psychology and Conceptions of Knowledge as Unitary," in T. W. Wann, ed., *Behaviorism and Phenomenology* (Chicago: University of Chicago Press, 1964), p. 4.

[23]It is an interesting coincidence that the personal equation gave birth to what eventually developed into "structuralist" psychology, and the psychological concerns of modern physicists are being attended to by social scientists of a new school, which also refers to itself as "structuralist."

CHAPTER II

THE CONCEPTUAL FRAMEWORK

Traditional Conceptions of Science

In 1960, Robert Watson bemoaned a general lack of interest in the history of psychology among American psychologists. Reviewing the contents of "three journals that publish most of the historically oriented publications of psychologists in the United States," he reported that during a 20 year period only 38 articles out of a total of 2800 were primarily historical.[1] Further, based on a sampling of statements of interests in the APA Directory, he estimated that only 60 psychologists, 0.36% of the membership of the APA at the time, considered the history of psychology as one of their interests. It would appear that most psychologists in the United States were following Henry Ford's old maxim: "History is bunk!"

Since the publication of Watson's plea, there have been some indications of increased interest in the area. In 1965, the Journal of the History of the Behavioral Sciences came into existence, an interdisciplinary journal devoted not only to the history of psychology, but also to the histories of anthropology, sociology, psychiatry, and other behavioral fields. In the same year, a division of the history of psychology was established by the APA. As of 1973, however, only 463 psychologists, 1% of the total membership, were affiliated with this division. There are also indications of some heightened historical interests in a number of university psychology departments. Cornell, for example, now offers the history of psychology as a specialty area in its graduate program. But for the most part, the field's history is not of great concern to most practitioners.

This lack of interest in the past is not surprising considering the traditional conception of the nature of scientific history. According to this conception, the history of science consists of names, dates, places, and achievements, a chronicle of the successive accumulation of facts, theories, and laws. Viewed from this perspective, history is dead. The ideas and theories of historical figures often seem naive and esoteric. In their original forms, the old debates seem irrelevant to contemporary concerns. Surviving achievements may be learned as a part of present training without any special study of history. Some amusement may be found in reports of early forerunners of modern scientific ideas, as in Descarte's suggestion of animal reflex activity consisting of a series of stimulus-response connections, which predated Pavlov by almost 300 years. But when we learn that Descarte's conception was based on the supposed transmission of "spirits" through tubes in the animal's body, the idea may seem more quaint than profound, interesting trivia, but hardly the stuff of serious study.

In a later article, Watson attempted to convince his academic colleagues of the value of teaching the history of psychology to their students.[2] As an enticement, he offers the following historical fact: The first psychological laboratory was not established by Wundt in 1879, as is generally reported, but rather in 1875. Not only that, but a psychological laboratory was also established in 1875 by William James at Harvard. Facts of this nature may be of great interest to historians specializing in the history of psychology or to trivia collectors, but their relevancetto contemporary work in the field of psychology is not at all clear. Even if it were discovered that a psychological laboratory had been established hundreds of years earlier, it would not have the slightest effect on the day to day work of a practicing psychologist.

The Positivist Tradition

The static conception of scientific history, which results in an appeal to trivia as its justification, is a natural concomitant of the philosophical ideas of empiricism and logical positivism. These ideas, in a somewhat distilled form, have been a major influence on the thinking of scientists in general and of experimental psychologists in particular. They have resulted in a modal conception of the nature of scientific activity, i.e., a conception which is held by more scientists than any other particular conception. In the field of psychology, the ideas of logical positivism were supplemented by an interpretation of Bridgeman's operationism and were presented by S. S. Stevens in 1939 in a highly influential article in the Psychological Bulletin.[3] The following is a summary of these ideas:

A. An empirical statement is scientifically meaninfgul only if it is testable and thus subject to confirmation or falsification. This is Steven's formulation of the "verification theory of meaning." Like most popular presentations, it ignores some of the unresolved problems and complexities of this position. Yet another statement of this principle is that "a sentence is meaningful if and only if there is some conceivable way for us to verify it."[4] Its meaning is then the procedure by which it may be verified, and carrying out the procedure informs us whether the statement is true or not.

B. There exists a set of statements which may be verified directly through observation. These statements are often referred to as "observation statements" or "protocol-sentences," and in their purest form may be exemplified by a pointed finger and the words "there red."

C. Complex propositions may be formed by combining two or more meaningful component statements in accordance with the rules of a formal system of logic. The axioms of the logical system require no verification, but are simply adopted by convention. Complex propositions must be reducable to observation statements which by definition are testable or verifiable. The complex proposition is then testable and its truth may be confirmed or disconfirmed (at least in degree) by further observation.

Hidden in this definition of scientific meaning are two prior assumptions. First, there is an assumption that there exists something equivalent to the idea of an atomic fact, an observable, non-reducable fact which retains its identity from one independent observation to the next, and that protocol-sentences describing these facts may form the basis of all scientific statements. "Metaphysical" speculations concerning the existence of these facts independent of observation are not required. However, the assumption of constancy between observations and therefore of the possibility of objective observation _is_ necessary. Herbert Feigl invokes this assumption as one of the "most important" ideals and defining characteristics of science. The confirmation of a statement must be capable of being "independently. . . checked by anyone else," and the results of such a test may be seen as free of "personal or cultural bias." In this way, observational constancy is seen as the cornerstone of scientific "objectivity."[5] Similarly, Israel Scheffler notes that according to the "standard view" of science, "observation supplies us with hard data independent of our conceptions and assertions," and "an unalterable observable somewhat underlies all conceptualization."[6]

A second and related assumption which is fundamental to the positivist definition of scientific meaning, is the utility of the Aristotelian law of identity (A=A) and its corollary laws of non-contradiction (not A and not-A) and the excluded middle (A or not-A). These laws, which are not themselves subject to verification, form the basis of most formal systems of logic. While there now exist some formal systems which do not maintain these laws as axiomatic, they are not among those which have been influential in modal conceptions of science and are alien to empiricist traditions.[7]

As a formal system, the axioms of logic require no justification and may be regarded as matters of convention. However, logical positivist approaches to science involve the application of logical systems to empirical referents and this should require some form of justification beyond the mere convention argument of the positivist. Such justification may be found in the relationship between the assumption of observational constancy and the law of identity. Identity, by definition, requires constancy between the various instantiations of any given element within an argument. Thus, absolute identity is assumed between A appearing in step one of an argument and A appearing in some subsequent step. This is paralleled by the assumption of constancy between independent observations of atomic facts. Without this constancy of empirical facts, the application of conventional logic to empirical data may be seen as problematic.

Also stemming from the empiricist tradition is the so called hypothetico-deductive or scientific method. As presented in elementary science textbooks, the method consists of a process involving the following four steps: (1) observation of facts, (2) the formation of hypotheses concerning the relations between the observed facts, (3) the deduction of specific predictions such that if a hypothesis is true, then the specific predictions corresponding to that hypothesis must also be true, and (4) verification of predictions as a means of supporting or refuting hypotheses. This schema parallels that described above as the criteria for scientifically meaningful statements. Observation of facts implies acceptance of the assumption of constancy of atomic facts which may be used as the basis of scientific investigation. Hypotheses are complex propositions. Their potential for verification may be actualized by deriving from them more specific complex propositions (predictions) which may be verified by further observation of atomic facts. Thus, the hypothetico-deductive method implies acceptance of the prior assumptions implied by the positivist criteria for meaning: atomic facts and the utility of the law of identity.

The principle of verification (the suggestion that a hypothesis be tested by observation) is presumed to assure that through the use of the scientific method, scientific activity will be self correcting, and that as long as the rules are adhered to, the results of investigation may without embarrassment be called scientific knowledge. It follows that the development of scientific knowledge may be characterized as a process of incremental accumulation. Given a set of observations and a set of rules for relating observation statements (rules provided by the language of logic), then there must be a finite matrix of meaningful hypotheses which may be formulated, each of which is at least in principle verifiable.[8] Since the negation of a statement is also a statement, and since the falsification of a statement implies the verification of its negation (if A is not the case, then not-A is true), then the falsification of any hypothesis is equivalent to the verification of a directly opposing statement. Therefore, every valid scientific study (i.e., one which has not violated the rules of the game) must yield at least one verified proposition, thereby increasing the number of scientific facts in our arsenal of knowledge.

Given the above argument, scientific progress can only be seen as a process of the gradual accumulation of facts, theories, and laws, and the history of science can only be a chronical of the names, dates, and events associated with the discovery of those facts, theories, and laws considered significant by the historian. The contribution of the historian must be limited to the discovery of important achievements which were overlooked by previous chroniclers and the discovery of errors in previous historical summaries (such as the discovery that 1879 is not the correct date for the establishment of Wundt's laboratory).

The New Historicism

Although the empiricist view of science is still dominant, there is a recent historicist movement within the philosophy of science which has been gaining increasing attention among scientists and philosophers alike. Inspired by the conceptual problems attendant to the revolution in physics, discussed in the previous chapter, the historicists have stressed two essential points. First is the idea that the observation of facts is never independent of a theoretical context, and second, that scientific progress is neither incremental nor cumulative, that it is rather a process marked by fundamental discontinuities through which facts are lost as well as gained.

The implications of these notions are far-reaching. First, the idea that science is not cumulative leads to a rejection of the hypothetico-deductive method as a description of scientific activity, since, as has already been demonstrated, if the premises of the method are correct, science must be a cumulative enterprise. Second, if observational facts are dependent on a theoretical context, then the implicit justification for the application of formal logic to empirical referents is lost, for it suggests that any given fact may not retain its identity through a shift in theoretical context.

The Structure of Scientific Revolutions

The most influential of the historicists is Thomas Kuhn, whose 1962 book, The Structure of Scientific Revolutions, has already come to be recognized as "the principal challenger to the positivist or logical-empiricist ideas."[9] Central to Kuhn's conception of the dynamics of scientific development is his notion of a "paradigm," which in its broadest context may be defined as a contentual model of the scientist's universe. A paradigm includes theories and laws but it is more than just a sum of theories and laws. It is an interlocking network of beliefs, values, commitments, assumptions, procedures and techniques shared by a scientific community. A paradigm defines a field of study, as well as the entities which comprise the field. Paradigms are open ended. They provide problems for the scientist to investigate and rule out other potential problems as either metaphysical or as belonging to the domain of another discipline. They supply clues as to the nature of problem solutions and procedures by which a solution should be possible. In addition, they establish limits of acceptability for both procedures and solutions. Of greatest importance, a paradigm involves what Kuhn "metaphorically" describes as a mode of perception which affects what the scientist sees when he looks at his data.

According to Kuhn, all sciences pass through an invariant sequence of phases. The first of these phases, termed pre-paradigmatic by Kuhn, is characterized by the lack of a universally accepted paradigm for the field. Each practitioner must begin from scratch, setting forth his conception of the field in book form. Eventually, an achievement in the field is sufficiently impressive to win over a group of adherents from competing modes of activity, thereby unifying the field and providing it with an independent identity as a mature science.

Following the unification of a discipline around a paradigmatic achievement, there is a sustained period of "mopping up" activity, which Kuhn refers to as normal science. During this phase, puzzles provided by the paradigm are solved through methods prescribed by the paradigm. Normal science consists of three types of puzzle-solving activity: (1) further investigation of those facts which are designated by the paradigm as particularly significant, (2) attempts at increasing the predictive scope and accuracy of the paradigm through extension to new situations and through refinement of techniques, and (3) further articulation or "fleshing out" of the paradigm itself through extension and refinement of theory. All of these activities are aimed at increasing the degree of fit between the paradigm and nature.

Eventually ordinary research activity leads to the emergence of anomalies, phenomena which fail to conform to paradigmatic expectations. This may occur in one of two ways. Either a problem which ought to be solvable by normal research methods repeatedly resists solution, or a piece of equipment consistently fails to function as expected in certain situations. Initially there is considerable resistance to recognition of the anomaly and the failure may be blamed on the incompetence of the individual practitioner, as was the case when Maskelyne blamed what would later be known as the personal equation on his assistant's ineptitude. Kuhn hypothesizes that this resistance may in part be a function of the fact that acceptance of the anomalous finding may call into question the validity of much earlier research, perhaps by revealing the existence of a previously uncontrolled variable. In any case, novelty that is potentially subversive to the paradigm from which it was produced is generally suppressed for a time. However, if the phenomenon is reliable and continues to crop up, it may eventually become recognized as a problem which the field can no longer ignore, in which case increased attention is paid to it.

At this point, one of three alternatives may occur: (1) The anomaly may be assimilated into the existing paradigm through successful non-paradigm-shattering theory modification; (2) it may be shunted aside as being too difficult a problem at the time with the assumption that at some future date it will be solvable within the paradigm, or (3) the field may enter into a state of crisis, eventually leading to a rejection of the old paradigm and its replacement by a new and basically incompatable model. This latter alternative is what Kuhn describes as a scientific revolution. Kuhn concedes that he is unable to specify the conditions that determine which of the latter two alternatives follow from the discovery of a non-assimilatable anomaly, although he does suggest that crisis states may be characterized by a proliferation of anomalous data, as in the period preceding the Copernican revolution, when "the state of Ptolemaic astronomy was a scandal."[10]

As a discipline enters a stage of deepening crisis, there is a turn from the ordinary research of normal science to what Kuhn refers to as extraordinary scientific activity. Eminent practitioners give more and more attention to the anomalous area, sometimes to such an extent that the resolution of the anomaly may seem to have become the essential subject matter of the field. At first there is an attempt at ad hoc modifications of existing theory in ways which do not require any fundamental alteration of the paradigm. As the anomaly resists assimilation, however, increasingly radical theory modifications tend to blur the outlines of the paradigm. There also may be a reemergence of competing schools, each professing a different articulation of the paradigm.

Extraordinary research may involve much experimentation without prediction, "just to see what will happen,"[11] a loosening of the rules permitting a seemingly random manipulation of variables. There may even appear experiments aimed at magnifying the breakdown by further exposing the limitations of the paradigm. Such periods may also involve explicit statements of professional insecurity and discontent, with occasional desertions of the field altogether. For example, Kuhn quotes Pauli's reaction to the early twentieth century crisis in physics: "It [physics] is too difficult for me, and I wish I had been a movie comedian or something of the sort and had never heard of physics." Einstein described a similar situation as one in which "the ground had been pulled out from under one, with no firm foundation to be seen anywhere, upon which one could have built."[12]

In three important ways, the stage of crisis, as it reaches its depth, resembles the earlier, pre-paradigmatic phase. Rather than a unified field of study, there exists a proliferation of competing schools. There are renewed debates between these schools over the basic fundamentals of the field, debates involving what constitutes legitimate problems, methods, and standards of solution, the stuff which may be taken for granted during normal periods. Finally, there is a recourse to philosophical discussion in the attempt to resolve these disputes.

The period of crisis finally comes to an end with the rejection of the old paradigm and its replacement with a new and incompatible contentual model of the universe. This constitutes a scientific revolution. It is a re-adjustment of conceptual categories which transforms former anomalies into current anticipated outcomes. It may even involve a change in the definition of the field itself. To paraphrase Peter Weiss, the field pulls it-self up by its own bootstraps, turns itself inside out, and looks at the world through fresh eyes.

Here the process comes full circle and begins anew. Normal science activity resumes, but it looks quite different from the normal science of the old paradigm. Eventually, it generates its own anomalies which lead to a new crisis, thereby setting the stage for another revolution. And so the never ending cycle continues in a dialectical progression of thesis, anti-thesis, and revolutionary synthesis.

Paradigms as a Mode of Observation

Throughout his work, Kuhn stresses the function of a paradigm as a mode of observation which effects the content of an observation, the way in which it is re-ported, and the inferences that are drawn from it. This is a point of view that Kuhn shares with others who have taken an historico-critical approach to the philosophy and history of science. For example, Kuhn's notion of paradigmatic observation is almost identical to Hanson's notion of perception as a "theory-laden" enterprise, and it exhibits a striking resemblance to Feyerabend's suggestion of "meaning variance" of the same term between incompatible theories.[13] The central point is that stimuli are perceived as meaningful elements, and that perception is therefore a function, not only of the qualities of the stimulus, but also of the knowledge beliefs, theories and assumptions which the observer brings with him to the situation. As a result, they suggest, when scientists with different frames of reference observe what in some sense may be agreed upon as the same data point, the resulting perception may be quite different, especially in terms of the meanings which are attached to it. Further, this difference might exist despite identical verbal descriptions of their observations.

In general, paradigmatic observation involves that way of looking at the world which allows one to recognize electrons and neutrons, or reinforcing stimuli and responses, as basic ingredients of nature, so that one may proceed to observe the way in which these events behave. The existence of a paradigm manifests itself in the ability of a physicist, but not an untrained layman, to see electron tracks rather than meaningless bubbles in a cloud chamber, in the ability of a biologist to see particular specimens rather than some formless stuff under a microscope, and in the ability of a psychologist to see units of response rather than marks on paper in the output of a cumulative recorder; or for that matter, in the psychologist's ability to see response units rather than an amorphous mass of unusual behavior when he directly observes his subject in an experimental situation.

Differences in paradigms allow a post-Galilean physicist to see the swing of a pendulum where Aristotle saw a constrained fall,[14] and they allow the cognitive psychologist to see inductive concept formation where the stimulus-response psychologist observes a conditioned leg flexion response.[15] They allow the word "mass" to have one meaning for a post Einsteinian physicist and a somewhat different meaning for a Newtonian physicist,[16] and they allow "cognition" to mean something different to a contemporary cognitivist than it meant to an earlier introspectionist, for whom a congitive process, by definition, had to be a conscious event. It is in these senses, that observed data points may lose absolute identity in shifts from one paradigm to another, with or without a corresponding change in terminology. And it is these changes which call into question the notion of anything corresponding to an atomic fact and therefore the implicit justification for the unrestrained use of any logical system based on an unqualified identity assumption. This is also one way in which the notion of scientific progress as cumulative becomes a problematic conception, since it allows scientific change to involve the alteration, rather than just the accumulation, of facts.

A concrete example may help clarify these points. I have noted that an S-R psychologist and a cognitivist might view the studies on conditioned responses in animals in quite different ways. Where the S-R theorist sees a conditioned leg flexion response, the cognitivist may see a process of inductive concept formation. This would seem to be a matter of different conscious interpretations of the same data and, <u>in part</u>, it is indeed just that. But it also involves more than after the fact interpretation; at the very least, it involves the phenomenon of selective perception at the time of observation. When in the early part of a conditioning experiment, the behaviorist focuses on the absence of a conditioned leg flexion response, the cognitive psychologist is observing an entire constellation of already learned avoidance behaviors connected to the subject's apparent attempt to escape from the entire situation, behaviors which are generally regarded by the behaviorist "as an uninteresting nuisance, to be minimized if possible and, if not, ignored."[17]

Further, for the behaviorist, both stimulus and response exist prior to conditioning and the task is one of establishing a connection between them. For the cognitivist, on the other hand, the subject's task involves abstracting a stimulus from a complex environmental situation, a task which is not too different from that of learning to recognize a simple figure embedded in a complex design. Thus the cognitivist "recognizes" that the stimulus does not exist as such for the subject prior to some point towards the end of the so-called conditioning process. Similarly, for the behaviorist, the subject must learn to respond in a prescribed manner at a certain point in time, whereas for the cognitivist, the subject must learn to cease responding in a whole variety of ways during most points of time. Finally, in order to study the phenomenon in greater detail, the experimenter must be allowed to handle his answers to these questions as observational givens. If the S-R psychologist, for example, is to study generalization, stimulus intensity effects, latency, etc., he must first be able to assume the identity of the stimulus as well as the existence of the basic conditioning phenomenon.

It should be clear at this point, that both the shape or scope and the constituent elements of the phenomenon are not the same for our two theorists, and that in that sense, we may say that they are examining different phenomena. Further, their differences in interpretation cannot clearly be distinguished from observational differences. The particular interpretation cannot be made independently of the observed scope and constituent elements which define the phenomena to begin with. Conversely, that which is attended to and that which is ignored, as well as the definition of the constituent elements (which are assumed to exist as such in nature and which form the very units of observation) are determined and implied by the character of the conscious interpretation.

In explaining his conception of observation as theory-laden, Hanson makes use of the ambiguous figure demonstrations of the Gestalt psychologists.[18] What is important for Hanson in these demonstrations is that the perceived content of the drawings is not a function of a conscious interpretive process. In the "young woman/old hag" drawing, for example, we <u>see</u> one figure or the other. We do not see an abstract pattern which may intellectually be interpreted as a representation of an old or young woman. On seeing the drawing for the first time, a subject may report that he sees only an old hag and may, in fact, be unaware of any other representational content. If the subject were a scientist reporting an observation, his reported fact might be that he observed a black and white line drawing of an old woman, perhaps with some additional specification as to the orientation of the face, the quality of the drawing, etc. One could hardly fault him for the quality of this observational report. An alternative might have been to measure the angle, length, width, and curvature of each line, along with the exact size, shape, and location of black and white areas in the drawing. But for most purposes (including that of journal publication), such a description would be inappropriate.

A second point of correspondence between ambiguous figures and paradigmatic observation, is that seeing both qualities of the figure at the same time may not be possible, at least not without considerable practice. In other words, at any given point in time, seeing one content excludes the possibility of seeing an alternative. The incompatibility of competing ways of seeing the subject matter of a field is also an aspect of Kuhn's conception of paradigmatic observation.

Admittedly, the ambiguous figure phenomenon is not identical to the phenomenon that the historicist philosophers of science are concerned with. While the data of any scientific field may indeed contain a high degree of ambiguity, it is not merely a situation in which what different observers may agree upon as a common unit may be seen as having this or that meaning attached to it.

Rather, paradigms influence the way in which the subject matter of the field is divided into units, almost as if one might be able to see part of the goblet-faces drawing and part of the young woman-old hag drawing as a single unit, rather than as parts of two separate units. A second point of difference is that changes in paradigm do not allow for the relatively easy switch which is generally possible with the gestalt demonstrations. Third, it must be recognized that the gestalt drawings are intentionally constructed so as to be ambiguous, whereas Kuhn and the others are discussing what they consider to be a rule which is characteristic of observation in general. Finally, the paradigm defines what is seen as the shape of the field itself, so that one model might include phenomena excluded by a competing model in what is generally recognized to be the same field. For example, not all psychologists would agree on whether thoughts or feelings ought to be considered as aspects of behavior lying within the domain of scientific psychology.

As a result of the difference between perceptual illusions and the notion of paradigmatic observation, Kuhn feels constrained to use the phrase "mode of perception" only as a metaphor, whereas Hanson discusses the theory-laden nature of scientific observation as a phenomenon which is inseparable from the act of perceiving. On the other hand, Kuhn shares Hanson's concern in differentiating between the effects of paradigms on observation and what is commonly thought of as interpretation. Interpretation, they stress, is a conscious process applied after the fact of perception. We first see something and then interpret its significance. In dealing with paradigmatic observation, no such temporal separation is possible.

It should be clear that the differences between Kuhn and Hanson as to whether the paradigmatic influences on observation may legitimately be classified as a perceptual phenomenon are secondary to their general agreement on the nature and implications of these influences. In any case, more recent approaches to the nature of perception generally support Hanson's readiness to remove the qualification "metaphorical" from discussions of theory, assumption, experience, etc., as elements of perception. Jerome Bruner, for example, has suggested that <u>all</u> perceptual activity necessarily involves an act of categorization which includes a determination of the way in which a field is divided into units. He further notes that modes of perceptual categorization are often quite resistant to change, despite conceptual evidence of their inadequacy. In a similar vein, Vernon has argued that "members of a group having, for instance, a common culture or type of education" may share a common system of perceptual classification, with the result that "the percepts of these people will be alike but will differ from those of other groups."[19] If the words "scientific community" are substituted for the word "group" in Vernon's statement, we have a fairly accurate description of Kuhn's notion of paradigmatic observation. Thus in terms of current conceptions of the psychology of perception, we may speak of paradigms as ways of seeing the world, without qualification or apology.

Competition Between Paradigms

A revolutionary change in conceptual categorization, Kuhn maintains, involves a change in the way in which the scientist perceives his data, a change which he likens to the gestalt switch perceptual change experienced with reversible figure drawings, but without the relatively easy reversibility which is characteristic of those drawings. As a result, each paradigm may be thought of as comprising a different observation language and there is, therefore, no neutral language in which the two models can be objectively compared. Each camp must defend its point of view from within its own point of view, using its own standards and criteria. Thus, definitive tests of competing paradigms are not possible. Although there are scientific values which may transcend differences between theories emanating from opposing paradigms, even those values fail to provide any absolute yardsticks with which the competing models can be compared. Recall the example of the conditioning experiment discussed above. Reporting that it resulted in a conditioned leg flexion response, is a function of observing that experiment through the eyes of a stimulus-response psychologist. Observing the same kind of experiment, a cognitivist reported his observation of a process of inductive concept formation. While the former interpretation is more parsimonious, in that it does not postulate more complex, higher level processes in its explanation of the phenomenon, the latter interpretation is more elegant, in that it accounts for a broader range of phenomena as manifestations of a unitary process.

Lacking a means of objective comparison, the struggle between incompatible paradigms takes on characteristics of political conversion. Some of the older members of the field may be converted to the new fold, but generally, it is the younger and newer practitioners, who, not being as tightly locked into the old paradigm, originate the new model and become the bulk of its adherents. The older members of the field simply die out, still clinging to the old model, or, if they live long enough, they may be read out of the profession, or they may retire into the field of philosophy from which so many sciences originally sprang.

As in political and social development, the existence of a crisis may be seen as a necessary precursor to revolution. Quite often, the essential characteristics of a new approach are suggested prior to the development of a profound crisis in the field, but these suggestions are generally ignored, only to be adopted after the reigning paradigm has produced a profound crisis situation. For example, it has been noted by many authors that all or at least most of the fundamental tenets of classical behaviorism were current in one form or another for years prior to the publication of Watson's manifesto in 1913. In 1914, Titchener traced the idea of using observation rather than introspection as a method of study back to Comte's work more than fifty years earlier, and William McDougall defined psychology as the science of <u>conduct</u> as early as 1905.[20] Yet it was not until the winter of 1912-13, when introspective psychology was emersed in unresolvable problems, that the spark of behaviorism was able to kindle the imaginations or inflame the furies of large numbers of professionals.

The analogy between social revolutions and scientific revolutions may be further extended. Just as political revolutions generally result in some casualties, so do scientific revolutions, not only in terms of individual practitioners who are unable to adapt to the new conception of their field of study, but more importantly, in terms of the loss of a substantial bulk of previously accepted scientific knowledge or fact. While the new paradigm may be able to account for a wider range of phenomena with greater precision, it does so only at the expense of some formerly accepted principles, beliefs, elementary generalizations, and procedures. As Kuhn notes, there is a loss of "some actual and much potential explanatory power."[21] Given such a change, it is difficult to justify a conception of science as cumulative, and without such a conception, as has already been demonstrated, the currently modal conception of what constitutes the scientific method becomes untenable.

Levels of Paradigms

To this point, the notion of paradigmatic development has been presented as representing quite general, fundamental models, which reign over a field of study for extended periods of time and which result in the scientific revolutions with which all well-educated laymen are familiar (Copernican, Newtonian, Eisteinian, etc.). However, Kuhn's presentation implies that he is dealing with a multi-level phenomenon. In addition to referring to the contentual model which unites an entire scientific discipline, the notion of paradigm and its dynamics also apply to specialty areas within the discipline and to even smaller groups of practitioners working on a specific problem within a specialty area. This hierarchical matrix may also be extended upward to embrace science as a whole, as a description of those shared values, commitments and assumptions which allow us to distinguish a member of the scientific community from a professional in some non-scientific field. For example, prior to the twentieth century, all of science was committed to an assumption of absolute determinism in the domain of physical events. Heisenberg's principle of indeterminacy challenges this assumption and thus may constitute the most fundamental scientific crisis of our time.

That which constitutes a conceptual revolution for a lower level paradigm may constitute only a normal puzzle solution for the larger paradigm which encompasses it. An example from within psychology is readily available: Systematic desensitization was initially conceived as being based on a process of reciprocal inhibition. Subsequent research on the importance of relaxation and hierarchy construction produced data which was anomalous to the reciprocal inhibition hypothesis and which led to a proliferation of competing models, each attempting to account for the same phenomenon. However, most of these models were not incompatible with the neo-behaviorist paradigm which gave birth to the reciprocal inhibition conceptualization. The relationship of paradigmatic levels involved in desensitization research is illustrated in Figure 1. Reciprocal inhibition and extinction are both mechanisms governing behavior which are acceptable to neo-behaviorist theory. Thus a shift from a reciprocal inhibition model to an extinction model, while involving a conceptual revolution for the initial desensitization paradigm (even to the point of reducing a large bulk of desensitization studies to the status of irrelevance), would constitute a normal puzzle solution for neo-behaviorism in general, answering the question: Which behavioral mechanism is responsible for the result? On the other hand, adopting the position that the results of desensitization may be explained as a manipulation of expectancy would require a shift from the neo-behaviorist camp altogether. But even this change would leave the so-called "methodological" aspects of Watson's behaviorist revolution intact. In this sense, revolution and normality may be coextensive across levels yet sequential within any given level.

In a postscript written for the second edition of his monograph, Kuhn attempted to deal with the multi-leveled nature of paradigms by admitting to two basic senses in which he had used the term: "a global sense in which it represented an entire constellation of beliefs, values, technique, and so on,"[22] and a more restricted

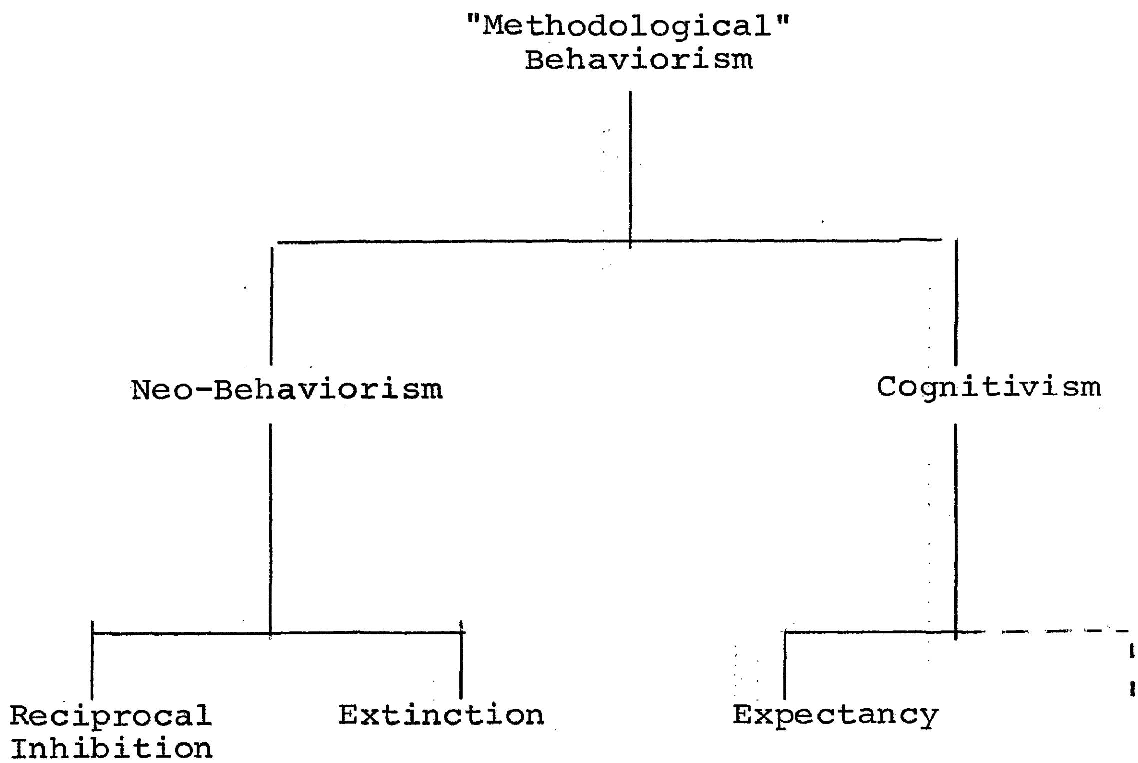
"Methodological"
Behaviorism
Neo-Behaviorism
Cognitivism
Reciprocal
Inhibition
Extinction
Expectancy

Figure 1

sense in which it represented a particular element of this constellation, the concrete puzzle solutions which serve as examples of work in the field, replacing explicit rules in the training of new practitioners and in guiding further research. He then suggested that the global constellation be referred to as a "disciplinary matrix" and that the term paradigm be reserved for the mode of observation which is assimilated through practice with the concrete puzzle solution examples, which he renames "exemplars."

If anything, this attempted resolution of the problem of the different senses in which Kuhn uses the term "paradigm" only confuses matters. In the first place, Kuhn's work has already come to serve as an exemplar for work in the history and development of science and as such it is often other elements of the particular field's "disciplinary matrix" that is attended to in the name of a paradigm. Second, many of Kuhn's examples of anomaly induced crisis result not only from contradiction with specifiable exemplars, but also with consciously held theories and laws, metaphysical assumptions, or various other elements of the broader disciplinary matrix. Third, while exemplars undoubtedly play a major role in transmitting "group licensed ways of seeing," there is no reason to assume _a priori_ that it is the only source of such effects on observation and

and observational report. The question of component sources of paradigmatic observation should, at this point, be regarded as one of the open puzzles provided by Kuhn's historicist paradigm and be left open to empirical test. For these reasons, I have decided to conform to what has become a common practice, rather than to heed Kuhn's later injunction, and will use the term paradigm in both its more and its less global senses. It is my suggestion that conceiving of paradigms as multi-level, hierarchical phenomena may be one way of avoiding some of these difficulties.[23]

Scientific Revolutions in Psychology

Kuhn's work has already had a significant impact on the thinking of a number of psychologists, and has, in fact, come to serve as an exemplar, a paradigm giving rise to Kuhnian studies on the nature of psychological research. Cartwright, for example, examined the line of research around the risky shift phenomenon. Noting that the early work on the question constituted "something analogous to what Kuhn has called a paradigm,"[24] in that it provided an entity for study (the risky shift), an open-ended problem (the determinants of the shift), and a particular set of research procedures for solving the problem, Cartwright then proceeded to demonstrate that continued research in the area led to the emergence of anomalous data and to the development of a subsequent crisis in which the assumption of the existence of a risky shift came to be called into question.

Following a more traditional hypothesis testing approach, Krantz used Kuhn's model to generate an hypothesis concerning "research activity curves" in anomalous versus non-anomalous areas in psychology.[25] He suggested that whereas normal research activity would be relatively constant in rate over a period of time, investigation in anomalous areas should be characterized by a rapid decrease in interest if the anomalous data could be assimilated into the existing paradigm or if it were found to be unreliable. Selecting latent learning and verbal reminiscence as anomalous areas, and secondary reinforcement and retroactive-proactive inhibition as corresponding normal areas, he generated research activity curves from frequency counts of publications in these areas. In support of his hypothesis, he noted that the curves for secondary reinforcement and latent learning followed a similar pattern until the latter was assimilated into the then current model, at which point there was a marked decline in latent learning investigations. A similar relationship was found between the activity curves for retroactive-proactive inhibition research and verbal reminiscence research, with the decline in the latter area associated with an accumulation of evidence that the verbal reminiscence phenomenon was unreliable.

A broader question which needs to be answered is that of where psychology as a discipline fits within the Kuhnian model. The general assumption has been that psychology, along with the other social sciences, is at a pre-paradigmatic level of development, as evidenced by the continued proliferation of competing schools within the field. For example, Robert Watson, who in 1960 had maintained that science was a cumulative enterprise, later adopted the Kuhnian perspective and assuming that psychology fit Kuhn's notion of a pre-paradigmatic field, began searching for the mysterious something that in some respects resembles a paradigm and that functions in its stead until the adoption of a universally recognized model.[26] Although his conclusion that psychology is a "prescriptive" science is provocative, the prior assumption that it fits Kuhn's model of a pre-paradigmatic field is questionable.

The assumption that psychology is a pre-paradigmatic field was first suggested by Kuhn, who indicated that differences between the social and natural sciences led to his notion of stages in scientific development. In particular, he was impressed by the extent to which there continue to exist debates over fundamentals in the social sciences. However, his subsequent investigation of the history of the natural sciences pointed to an entire set of criteria distinguishing pre-paradigmatic fields from mature sciences. In addition to a continuing debate over fundamentals and the existence of competing schools (both of which are also characteristic of mature sciences during periods of crisis), pre-paradigmatic fields could be distinguished by the use of books as the major vehicle for reporting developments in the field, with each author finding it necessary to reconstruct the field from scratch, justifying each new concept as it is presented. The pre-paradigmatic phase of a field is also characterized by a total lack of normal research activity, activity which Kuhn maintains is <u>impossible</u> in the absence of a paradigm. Mature fields, on the other hand, may be distinguished by specialized journals containing reports of normal puzzle-solving activity, reports in which fundamentals may be taken for granted. Other characteristics of maturity include the claim to independent status in the university curriculum and the existence of independent professional societies.

Kuhn's characterization of pre-paradigmatic science easily fits common descriptions of psychology prior to the last quarter of the nineteenth century. But even a cursory glance at twentieth century psychology indicates that it does not easily fit the model of either a pre-paradigmatic field or a mature science, but rather possesses some characteristics of both. While on the one hand, debate over fundamentals, the proliferation of competing schools, and the use of the pre-paradigmatic type of book as an important vehicle of professional communication continue to exist in the field, there is also an independent professional society (APA), independent status in the university curriculum, and an overabundance of normal research reported each year within specialized journals. There even exist journals whose major function is to summarize the bulk of normal puzzle solving research in particular areas of the field.

Of these latter characteristics, the existence of a continuing body of normal research is of greatest importance, since according to Kuhn, this should not be possible in the absence of a paradigm. There can be no doubt, however, that paradigms (as opposed to something resembling paradigms) do exist in psychology, not only on the molecular level, as in Cartwright's analysis of the risky shift line of research, but also on a broader scale as well. Skinner's experimental analysis of behavior, for example, provides a perfect concrete match for Kuhn's abstract description of a paradigm, despite the fact that it has not been universally adopted. It categorizes the psychological universe in terms of stimuli and responses which may be viewed as elements of nature from within the Skinnerian paradigm. It postulates relationships between these entities which must be taken as givens by virtue of the definitions of the entities. It provides puzzles to be solved and procedures, technical equipment, and standards of acceptance for their solution. Finally, it limits the range of phenomena which may be studied, proscribing some as metaphysical and others as falling within the province of another discipline, physiology for example.

Yet, if it is clear that broad paradigms exist within experimental psychology, the lack of unity within the field is equally clear. Psychology is neither pre-paradigmatic nor mature in the Kuhnian sense of the term. In many ways, it most closely resembles what Kuhn describes as the crisis phase of a mature science, seemingly without having first gone through a period of cohesiveness. It is as if the field as a whole had harkened to Feyerabend's battle cry (borrowed from Marx and Trotsky), "the revolution in permanance!"[27]

Perhaps it is this apparent lack of fit between the nature of psychology, as well as the other social sciences, and his schematic model of scientific development that has led Kuhn to suggest that these fields are currently in a process of transition towards maturity.

"These transitions to maturity have seldom been so sudden or so unequivocal as my necessarily schematic discussion may have implied....In parts of the social sciences they may well be occurring today."[28] Nonetheless, Kuhn does not seem to envisage the kind of protracted period of transition which would be implied by such an analysis of multi-paradigmatic disciplines. It has become traditional to date the end of pre-scientific psychology as coinciding with the establishment of Wundt's laboratory. In the almost 100 years since that paradigmatic achievement, we have yet to reach agreement on the most basic definition of the field, as witnessed by "neo-behaviorist" Hebb's recent suggestion that psychology be defined as the science of mind.[29] Nor is there any agreement on what entities comprise the subject matter of the field (percepts, images, stimuli, responses, cogitions, cognitive structures, emotions, needs, etc.).

It is with this problem in mind that I set out to investigate the early history of experimental psychology. At first, I assumed that our discipline could be adequately characterized as "multi-paradigmatic," a phrase coined be Margaret Masterman[30] to describe fields like psychology, in which there seemed to be a number or incompatible paradigms coexisting in time, each giving rise to its own program of normal research. The problem was, it seemed to me, to determine how it came to be that a certain subset of scientific fields had acquired a multi-paradigmatic character. But as I read more of the work of the early experimentalists, it occurred to me that there actually had been a period of paradigmatic unity, that the entire Kuhnian drama of unifying paradigm, anomaly, crisis and revolution had already played itself out at least once within psychology, but within a highly compressed time scale. The question to be answered was not why psychology had failed to unite around a single paradigm, but rather, why the initial unity had disintegrated so rapidly. This then, is the story to which we turn in the next two chapters: the first paradigm of scientific psychology and the revolutionary crisis, which ensued from the work within that paradigm and which led to its replacement.

FOOTNOTES TO CHAPTER II

[1]Robert I. Watson, "The History of Psychology: A Neglected Area," American Psychologist 15 (1960): 251-255. Reprinted in Virginia S. Sexton and Henryk Misiak, Eds., Historical Perspectives in Psychology: Readings (Belmont, Ca:Brooks/Cole, 1971), pp. 30-39.

[2]R. I. Watson, "Psychology: A Prescriptive Science," American Psychologist 22 (1967): 435-443. Reprinted in Sexton and Misiak, pp. 183-200.

[3]"Psychology and the Science of Science."

[4]F. Waismann, quoted in Merle B. Turner, Philosophy and the Science of Behavior (New York: Appleton-Century -Crofts, 1967), p. 109.

[5]"The Scientific Outlook: Naturalism and Humanism," in Herbert Fiegl and May Brodbeck, eds., Readings in the Philosophy of Science (New York: Appleton-Century-Crofts, 1953), p. 11.

[6]Science and Subjectivity (Indianapolis: Bobbs-Merrill, 1967), p. 13.

[7]Intuitionist systems reject the law of the excluded middle, so that while the negation of A implies not-A, the negation of not-A does not imply A. Also, Michael Kosak has attempted a formalization of the Hegelian dialectic in "The Formalization of Hegel's Dialectical Logic," International Philosophical Quarterly 6 (1966).

[8]The early Wittgenstein has been summarized as writing that "all meaningful sentences must be analyzable into basic, simple sentences that 'picture' simple facts about the world....Their combination into complex sentences will be accomplished by the combinatory principles of Russellian logic. The completed language will be sufficient to express every possible truth about the world. It is the job of the sciences to determine which of the possible truths are actually true." Encyclopedia Americana, 1973 ed., s.v. "Linguistic Analysis."

[9]P. Durbin, Philosophy of Science: An Introduction (New York: McGraw-Hill, 1967), p. xiv.

[10]Kuhn, p. 67.

[11]Ibid., p. 87.

[12]Ibid., P. 83-84.

[13]Hanson, Patterns, p. 19: Paul K. Feyerabend, "Problems of Empiricism," in Robert G. Colodny, ed., Beyond the Edge of Certainty (Englewood Cliffs, N.J.: Prentice-Hall, 1965), pp. 145-260.

[14]Kuhn, pp. 123-125.

[15]Robert Leeper, "Cognitive Processes," in S. S. Stevens, ed., Handbook of Experimental Psychology (New York: Wiley, 1951) pp. 730-757.

[16]Kuhn, p. 101.

[17]Hobart O Mowrer, quoted in Leeper, p. 742.

[18]pp. 8-12.

[19]Jerome S. Bruner, "On Perceptual Readiness," Psychological Review 64 (1957): 123-152; M. D. Vernon, "The Functions of Schemata in Perceiving," Psychological Review 62 (1955): 182.

[20]John C. Burnham, "On the Origins of Behaviorism," Journal of the History of the Behavioral Sciences 4 (1968): 143-151.

[21]p. 107.

[22]p. 175.

[23]A number of other possible difficulties with Kuhn's model have been raised by various critics. A discussion of some of these criticisms is presented in the Appendix.

[24]Dorwin O. Cartwright, "Determinants of Scientific Progress: The Case of Research on the Risky Shift," American Psychologist 28 (1973); 224.

[25]David L. Krantz, "Research Activity in 'Normal' and 'Anomalous' Areas," Journal of the History of the Behavioral Sciences 1 (1965); 39-42.

[26]"Prescriptive Science."

[27]"Consolations for the Specialist," in Lakatos and Musgrave, pp. 197-229.

[28]Kuhn, p. 21.

[29]Donald O. Hebb, "What Psychology is About," American Psychologist 29 (1974): 71-79.

[30]"The Nature of a Paradigm," in Lakatos and Musgrave, pp. 59-89.

CHAPTER III

PSYCHOLOGY'S FIRST PARADIGM

The Unity of Early Experimental Psychology

The history of psychology has generally been presented as the stories of a large number of competing schools, differing from each other in their objectives, their methods, and even in their basic definitions of the field. Connections between the schools are often presented only in terms of the great debates that have raged between them, or less often, in terms of hidden similarities which are revealed by viewing them in respect to any of a number of dimensions. What is missing from each of these accounts is any sense of continuity. One may even begin to wonder how it ever occurred that such a motley collection of disconnected concerns came to share a common label.

Yet it seems to me that by the end of the nineteenth century, psychology had become a mature, paradigmatic field, that it subsequently underwent a fundamental conceptual revolution, and that the various schools of the early period represented what Kuhn has referred to as different articulations of a common paradigm. The failure to recognize the fundamental unity underlying all of experimental psychology prior to 1913, may have been a function of the attention paid to the many important metaphysical, programatic and methodological issues that were the subject of heated debates between the schools. It may also have been due to a failure to heed Titchener's injunction that despite the existence of these debates, the student should "look for underlying agreements rather than for superficial differences."[1] But in any case, the existence of such a fundamental unity seems undeniable.

The Titchenerian structuralists were the dominant school of scientific American psychology during the early 1900's. Their primary opposition came from those calling themselves functionalists. These were the two basic warring factions of pre-1913 psychology. Yet the classic article in which James Roland Angell outlined the basic functionalist platform included a statement emphasizing the essential unity which served as the ground for the structural-functional debate.

> When the structural psychologists define their field as that of mental process, they really preempt under a fictitious name the field of function, so that I should be disposed to allege fearlessly and with a clear conscience that <u>a large part of the doctrine of the psychologists of nominally structural proclivities is in point of fact precisely what I mean by one essential part of functional psychology, i.e., an account of psychical operations</u>. Certain of the official exponents of structuralism explicitly lay claim to this as their field and do so with a flourish of scientific rectitude. There is therefore after all a small but nutritious core of agreement in the structure-function apple of discord. For this reason, as well

> as because I consider extremely useful the analysis of mental life into its elementary forms, I regard much of the actual work of my structuralist friends with highest respect and confidence.[2]

This reveals an intra-disciplinary debate which may be viewed from the functionalist camp as largely a matter of emphasis. The analysis of consciousness into irreducible elements, the very heart of the structuralist approach, is appraised by the opposition as "extremely useful" and worthy of "highest respect and confidence." That the basic concern of psychology is the study of mental process is also agreed upon, but the functionalists maintain that process is a functional as opposed to structural category.

Discovering a paradigm

How may we find the core of the apple, the common contentual model which united the various opponent tendencies? The suggestion offered by Titchener was, I believe, a good one. We may examine the introductory psychology textbooks of these schools with a view to uncovering their basic areas of agreement. If the elements of a paradigm are to be found anywhere, they are to be found in these texts used in the training of future professionals, for it is the function of early training to teach the fundamentals which may later be taken for granted. Students need to be initiated into the professionals' paradigm. They must be taught to see the world through the glasses of their field.

What are the questions which must be asked if our aim is to find unity on fundamental questions and not just superficial resemblances? Clearly, our first question must be the very definition of the field itself, that is: What is its object of study?

Science is generally distinguished from other kinds of inquiry by its emphasis on some forms of systematic observation, and in particular, experimentation, as opposed, for example, to speculative argument, as the method of study. Thus our second question is: What are the modes of observation by which the subject matter of the field are studied?

Related to this second question is a more subtle, but very important, question: What do the psychologists see when they observe their subject matter? This is the question to which Kuhn has drawn particular attention in his description of paradigms as "ways of seeing." It is also a point which unites Kuhn's ideas with those of other historicist philosophers of science (i.e., Hanson, Feyerabend, etc.). Let us once again examine what is meant by a "way of seeing."

Point a person in the direction of a house and ask him to report what he sees. In most cases he will tell you that he sees a house. But there is nothing inherent in the pattern of stimulation on the sense organs that in and of itself conveys the information, house. House is a construction which is imposed on the sensory input by the perceiving organism. Titchener referred to the type of observational report exemplified by the response "I see a house" as the stimulus-error, which is the insertion of statements of meaning into reports of perceptual experience. Later theorists have argued that meaning is an inseparable part of perception, that we actually "see" a house; we do not see an image or a sensory pattern which we then infer to be a house.

The point is, that we must learn to see a house, that the perception of a house is dependent on culturally determined experiences with houses. In the same way, scientists must be taught to see their objects of study. A layman may see bubbles in a physicist's cloud chamber. With sufficient training, however, he may learn to commit the "stimulus-error" of seeing electron tracks. Thus, when we ask what the early psychologists saw when they observed their data, we are inquiring as to the nature of the particular "stimulus-error" they had learned to make as a function of their paradigm.

The definition of psychology

Returning to the first question, we find a virtually unanimous opinion that psychology is concerned with the study of mental life and an agreement that the elements of mental life are processes. Other more or less synonymous terms employed included mind, consciousness, mental activity and immediate experience. Specialized definitions were developed for some of these terms. Thus, the structuralists defined mind as the sum total of an individual's conscious experiences, reserving the term consciousness for the individual's experience at a given moment in time.

The emphasis on mind may be seen today in contrast with the later emphasis on behavior. The behaviorists were keenly aware of this distinction and employed the epithet "mentalist" in referring to the categories used by their predecessors. I think it appropriate to take the term mentalism, though without its pejorative connotation, as a label for the first basic paradigm of scientific psychology. It is true that these early mentalists did not employ this label as a description of their approach. They were simply psychologists. If further sub-classification were necessary, it was based on the distinctions existing between them at the time and the terms used were structuralist, functionalist, associationist, etc., though it must be noted that even these distinctions were not finely drawn and are used more in retrospect than they were at the time.

That the early psychologists did not characterize their approach as mentalism or use any other distinct label is quite understandable. After all, there was nothing at the time to which a specific label could be contrasted. Behaviorism had not yet been invented. It is only the overthrow of the first paradigm which allows us to characterize it as something less than all of psychology. The name and distinguishing nature of its successor helps us to find that appropriate name and unifying characteristic of the early approaches. The new revolutionaries called their school behaviorism in opposition to the mentalism of all the predecessors.

The pervasiveness of mentalism in pre-behaviorist psychology cannot be overemphasized. It cropped up in what today seems like the most unlikely places: animal and comparative psychology, areas which were to have quite an impact in the development of the behaviorist alternative. Comparative psychology was defined by Titchener as the "compative study, either of various types of animal mind, or of the minds of animals and of man," and by functionalist Harvey Carr, twelve years after the beginning of behaviorism, as the study of "mental capacities of animals in comparison with those of man." Thorndike titled his pioneering monograph on animal psychology Animal Intelligence and described his aim as "an attempt at an explanation of the nature of the process of association in the animal mind."[3]

Finally, because of the emphasis on mind, animal psychology was seen as a peripheral field and was generally dealt with only in passing in introductory texts. The primary focus was on generalized mind, that is, "the formulation of concepts and principles that are applicable to all varieties of mental operation no matter where found."[4] Therefore the basic texts of all schools were devoted to consideration of the normal adult human mind. The structuralists have, at times been accused of reading the peripheral fields out of the domain of psychology. But this is simply not the case. Titchener emphasized that:

> All these various fields of psychology may be cultivated for their own sake, on account of their intrinsic interest and value; they must, indeed, be so cultivated, if psychology is to progress. At the same time, their facts and laws often throw light upon the problems of normal human psychology.[5]

The modes of observation

Mentalist psychology recognized two basic modes of observation or types of data. The first was direct introspective observation of mental life; the second, objective observation of behavior. The point is often made that behaviorism adopted the latter method and accepted a modified form of the first, renaming it "verbal report." While this is in some sense true, it ignores the issue raised by our third question, i.e., the particular constructions which were involved in the perception of data.

Introspection. To the mentalists, the term "introspection" meant the direct observation of one's own conscious experience. It was agreed upon by all concerned, that this mode of observation required special training if accurate reports were to be gained. "Naturally the use of the subjective method must be confined to subjects of training and ability," wrote functionalist Harvey Carr.[6] Training was necessary to avoid the stimulus-error, in which the "inferred" stimulus-objects, rather than the elementary sensations, are reported. Furthermore, it was asserted, with sufficient practice accurate introspection becomes an ingrained habit,

> so that it is possible for him [the introspective observer], not only to take mental notes while the observation is in progress, without interfering with consciousness, but even to jot down written notes, as the histologist does while his eye is still held to the ocular of the microscope.[7]

The term introspection was used to distinguish the method from inspection, the observational mode of physical science. But the distinction was not based on the need for specialized training. Observational training is necessary in the natural sciences as well. The biologist must learn to see a specimen under his microscope, the radiologist to see an organ in an X-ray, and so on. The practitioner of each discipline must learn, through intensive training, to commit his field's particular "stimulus-error" before he can be considered a competent observer. This is merely another indication of the maturity of a scientific specialty area.

In this and other ways, both structuralists and functionalists emphasized the close correspondence between inspection and introspection. They were differentiated only in terms of the objects of observation. With inspection, one looked outwards at a physical object. With introspection, one looked inward at a mental process. In both cases, the resultant observation is personal. Only one person, the individual observer, is privy to any particular observation. Others must rely on reports of that observation or on his own observations of that physical object or mental process. In physics, similarities in the reports of various observers are taken as evidence of an underlying physical reality. In the same way, similarities in the reports of various introspective observers were taken as evidence of an underlying psychical reality.

In the mentalist schema, the introspective observer held the status of experimenter rather than that of subject. This was true even if the experimental reports contained the responses of a number introspective observers, as was often the case. In this situation the introspective observers held a status resembling that of research assistants today, each running an independent replication of the entire experiment. Thus, the introspective observations of the mentalists produced data which were quite different from that produced by the verbal reports of the behaviorists. The former produced the experimenters' reports of his direct observations of mental process. In the latter, only the subjects' verbal reports are directly observed. Anything else must be indirectly inferred.

Objective behavioral observation. On the surface, this second mode of mentalist observation was almost identical to the observational methods of the later behaviorists. Thorndike, for example, placed hungry cats in cages from which they could escape by pressing a lever, stepping on a platform, or some other simple act. The cat's behavior was then carefully observed until it had "formed a perfect association." But the observed phenomenon was altogether different from that observed by the later behaviorists. Thorndike observed the "real mental content" of the cat's learned association, the feeling it had while it acted, and the impulses that the cat had learned to associate with the feeling of confinement. By "impulse," Thorndike meant "the consciousness accompanying a muscular innervation."[8] Thus, Thorndike was studying not stimuli and responses, but the conscious mediation between the two.

This method of studying the animal mind was later described by Titchener as an indirect form of introspection:

Now the psychologist argues...that

> the movements of animals are, to a large extent, gestures; that they express or record the animal's mental processes....He calls experiment to his assistance, and places the animal in curcumstances which permit of the repetition, isolation and variation of certain types of movement or behavior. The animal is thus made, so to say, to observe, to introspect; it attends to certain stimuli, and registers its experience by gesture....The psychologist...observes the gesture, and transcribes the animal consciousness in the light of his own introspection.[9]

What is being observed, then, is not the animal's behavior as such, but rather its consciousness as reflected in its behavior and as interpreted by the psychologist in the light of his own conscious experiences.[10] Behavioral analysis is thus the <u>indirect</u> mode of psychological observation as contrasted with introspection. It is the method which is forced upon the psychologist in his study of animals, children, or any other subjects who for one reason or another cannot be trained to scientifically observe and report on their conscious processes.

If this approach to behavioral observation seems artificial or unscientific, it is only a function of how far away we are from the mentalist paradigm. Think of how incredible it would have seemed to an early experimentalist had he been confronted with a behaviorist purporting to study perception or memory by the indirect method of verbal report because he has ruled out the direct introspection of consciousness on "methodological" grounds.

The mentalist paradigm

What I have described above was the core of the mentalist paradigm. It consisted of a definition of psychology as the science of mind, a direct and an indirect mode of experimental observation, and particular ways of seeing the data thus observed. Introspection was considered to be the direct observation of mental activity. Where introspection was insufficient or impossible, the observation of behavior provided data from which the psychologist could infer mental activity. This was the indirect mode of observation. The central task of scientific psychology was the analysis of mental processes into their basic components, the accurate classification of these components, and the discovery of their laws of combination. A less central, but to many psychologists a far more interesting endeaver, was the investigation of the genesis of the various mental processes. It was this task that particularly attracted the functionalist camp.

In addition to this "nutritious core" of accepted beliefs and attitudes, there was also widespread agreement on a large bulk of more specific matters. It was noted with obvious pleasure by some early writers, and displeasure by others, that there was substantial agreement in regard to the basic sub-topics to be covered by psychology.[11] These were the basic psychical processes: sensation, perception, affection, emotion, attention, association, memory, volition, etc. Within each of these topics, there was a body of accepted procedures, facts, theories, and puzzles to be solved. Of course, there were also differences, both between schools and within schools. But the fundamental differences between schools were often seen as differences in emphasis. The functionalist, for example, "throws emphasis upon the biological significance of conscious process instead of upon the analysis of conscious states into introspectively isolable elements."[12]

These differences were both real and important. The difference in emphasis may be traced to a more fundamental dispute over the relation of psychology to the natural sciences. Structuralists followed the mechanist tradition of science, distinguishing the subject matter of psychology from that of physics and adopting chemistry as the model for its approach. Functionalists, on the other hand, turned to the organicist scientific tradition, conceptualizing the same subject matter in opposition to that of physiology, and patterning their approach after the model of Darwinian evolution. This opposition is well worthy of further study, but an adequate conceptualization is possible only in terms of the underlying unity on which the debates were grounded.

Structuralism and Functionalism

Structuralism

In order to flesh out the mentalist paradigm and explicate the relationship between the two major articulations to which it gave rise, we must first turn to the period of time before the division of mentalism into structural and functional factions. The final quarter of the nineteenth century was a honeymoon period for experimental psychology. As a new science with a radically different subject matter from those of the established disciplines, it had to defend itself from the skepticism coming from outside its ranks. But within the rapidly growing new field, there existed a sense of unity and solidarity. Wundt's laboratory attracted students from all over the world, including quite a few who would later export the "new psychology" to the United States. Many of these, like Angell, Scripture, Judd and Baldwin, would become the champions of the American functionalist attitude, but Edward Bradford Titchener remained true to the Wundtian faith to the end. At Cornell, he was to be the acknowledged leader of the structuralist movement, offering a program which differed from Wundt's only in minor details.

During the first decade of the Leipzig laboratory, attention was centered primarily on the reaction experiment and the determination of the duration of mental acts, but later, the focus shifted toward the qualitative analysis of sensation and perception. Even the reaction experiment tended to be used more and more for this purpose. "Observers," i.e., the subject-experimenters of introspective studies, were required to report on their conscious experiences and the reaction times were used as a control to insure that the observer had performed the appropriate mental act.

That this was the focus of what later became known as structural psychology was determined by Wundt's conception of the nature of the field as a science. Psychology was defined in terms of its relation to the "hard" physical sciences. Its subject matter was contrasted to that of physics and its method of approach took chemistry as its model. Physics, said Wundt, concerns itself with the study of "mediate" experience, i.e., with the objects of the world which are known not through sensory awareness alone, but through conceptual mediation of the sensory data.[13] Its elements are not directly given to experience, but are inferred from what is immediately given. By contrast, psychology is concerned with the study of "immediate" experience, that phenomonal experience of which the person is directly and immediately aware, the experience from which the inferences of physics are made. Thus the sensation of color is the psychological data corresponding to the light waves which are the subject matter of physics.

Titchener's approach to this question illustrates the limited nature of his divergence from Wundt.[14] Titchener defined the subject matter of psychology also by contrasting it to that of physics. Physics, he explained, studies the experienced world as independent of the experiencing individual. It searches for that commonality in the world which is not dependent on the nature of the experiencing organism. Thus, a light wave of say 500 nanometers retains its identity whether or not the observer is color blind. Psychology, on the other hand, studies the world "with man left in." Its subject matter is experience as dependent on an experiencing person. Its data is not the light wave, which is presumed to exist independently of the observer, but rather the sensation of color, which cannot exist independently.

At first glance, Titchener's conception might seem diametrically opposed to that of Wundt. Whereas Titchener characterized the subject matter of physics as independent of the experiencing person, Wundt saw it as being formed only by the observer's mediation. Where Titchener saw experience as dependent on the experiencing organism, Wundt found experience which was independent of the observer's conceptual mediation. But at a more fundamental level, the two formulations were actually complimentary. Wundt's mediation was only the process by which Titchener's physicist abstracted himself from the world, leaving only that which is independent of him as an experienceing person. For both men, psychological introspection required intensive training, the purpose of which was to eliminate mediation from the observer's experience. According to both, psychology and physics studied the same external world. Only the method of observation differed; the physicist inspected the things of the world, whereas the psychologist introspected his immediate experience of those inferred things.

The strategy of the experimental psychologists was patterned after the science of chemistry. Chemistry analyzes the things of the world into its constituent elements and seeks to discover the laws governing the combination of those elements into chemical compounds. The program of the experimentalists was "mental chemistry." By careful observation through the microscope of introspection, the basic elements of conscious experience would be isolated, and through further experimentation, their combinations into mental compounds or "complexes" and the laws governing those combinations would be discovered.

By the turn of the century, some broad general agreement had been obtained as to two of these basic psychic elements, sensations and feelings. Sensations, it was claimed, were the fundamental elements of perception, and feelings or affections were the basic elements of emotion. A third category, images, was also recognized as the elemental basis of ideas or thought. It should be noted that these images were not necessarily visual. The term was used to denote a mental representation of any sensation. Thus, in addition to visual images, there were also auditory images, kinesthetic images, etc. These images were regarded by most experimentalists as a sub-category of sensation. But this divergence of opinion as to whether or not images constituted an independent class of elements was not regarded as a particularly serious problem, and in any case, was seen as one which would be resolved through further experimental observation.[15]

Prior to the turn of the century, it seemed as though basic agreement had been reached that these and only these constituted the basic elements of mind. In principle, it was recognized that new elements might in the future be discovered, but in fact, this was considered an unlikely possibility. "No fourth candidate for elemental rank has appeared," wrote Titchener in 1898. "No trace has been found, in all the last twenty years, of a mental krypton or argon."[16] This sense of unity was soon to disappear, but in the earlier period, the question of mental elements seemed more or less settled, and disagreements were limited to the subordinate task of analyzing the attributes of the psychic elements, attributes like quality, intensity, duration and clearness.

If the first task of the new experimental psychology was to analyze mental compounds into their constituent elements, then the second task was to investigate the laws governing the combination of those elements into the complexes which the untrained observer perceived as undifferentiated wholes. This combination of elements was viewed as a process of association, a view which was inspired by the notions of the British associationists. Distinct types of combination were specified. There were the fusions of elements from the same sensory mode, as in the fusion of a musical tone with its accompanying overtones, and the complications of elements from different sensory modes, as when one sees and hears two objects being struck together. To these may be added the imaginal components which are incorporated into the resulting perception, and so on. In 1885, Ebbinghaus published his experimental studies of memory, and this too was incorporated into the new psychology. Thus, the early experimental psychology could be characterized as both atomistic and associationistic.

This, then, was the program which at first characterized nearly all of experimental psychology, and later, the work of the structuralist camp. Greatest attention was focused on discovering the facts of sensation, and in particular visual sensation. Quantitative methods for analyzing the results of introspective studies were developed and refined. Investigations were extended into new areas, such as the analysis of visual illusions. But all of these efforts remained basically within the framework of mental chemistry.

The Functionalist Alternative

Historian Hugh Kearney has distinguished three traditions of scientific activity: the organic, the magical and the mechanistic.[17] After the scientific revolution of the sixteenth and seventeenth centuries, the magical tradition fell into disfavor and the organic was relegated to a subsidiary status. When the new scientific psychology began, it turned directly to the mechanist tradition, taking physics and chemistry as its models. However, during this same period of time, the organic tradition was receiving fresh impetus from the development of Darwin's theory of evolution, and when experimental psychology was transported to the United States, it was transformed into a biological science.

American psychologists soon wearied of the structural analysis of mind, which they viewed as valid, but arid and lifeless. Inspired by the work of Darwin, as well as by the new American philosophy of pragmatism, they turned away from questions of structure and began to inquire into questions of function. Where the structuralist concentrated primarily on the "what" of mental life, attempting to analyze it into constituent elements, the functionalist looked for the "how" and the "why". Explanation is a task of all science, and the structuralists had looked to physiology for the explanation or "why" of mental life. But the functionalists looked for another form of explanation. For them, physiology was the "how" of mind; the "why" was to be explained in terms of purpose. What purpose or function did consciousness in general, as well as in its particular forms, serve in the life of the organism? How did it evolve? What was its adaptive value? How does it operate in actual life situations? These were the questions which intrigued the functionalists.

We have seen that the structuralists had distinguished their subject matter from that of physics and described their approach as analogous to that of chemistry. Turning to the biological sciences, the functionalists characterized their field in relation to physiology and biological evolution. Both physiology and psychology study the reactions of organisms to their environment, wrote the functionalists. The physiologists are concerned with the study of the "vital activities" of the organism, with respiration, digestion, etc., psychologists, on the other hand, study the adaptive reactions of organisms to novel environmental stimuli, as dependent on prior experience. Implicit in this distinction of the subject matter of psychology, is the functionalist's evolutionary orientation. Consciousness emerged from the struggle for survival as an adaptive mechanism, and, indeed, the functionalists treated mental activity explicitly as "substantially synonymous with adaptive reactions to novel situations."[19]

Despite the fact that until 1913, functionalism represented the major opposition to mainstream experimental psychology, i.e., to structuralism, and also despite the fact that the functionalist label could be applied to more American psychologists than any other label, functionalism fell short of constituting a well defined "school" of psychology. In writing about the field, the differences were easy to see. The functionalists accused their more traditional opponents of being too restrictive, and limiting their potentially exciting field to its most boring and least relevant aspects. The structuralists rejoined that the brash upstarts were jumping the gun, addressing complex problems prematurely, and being lured away from the important activity of "pure" science by practical considerations. But in the day to day experimental work and in the longer and less polemical expositions of the field (i.e., in the textbooks), the differences between the two groups were more difficult to find.

All were agreed that general psychology, i.e., the study of the normal, adult, human mind, was the center of the discipline and the appropriate subject matter of an introductory text, and that abnormal psychology, genetic psychology, comparative psychology, etc., were at its periphery. As a result of their attraction to the theory of evolution, the functionalists found work in the peripheral areas more interesting and considered it highly important. The structuralists worried, lest too much energy be diverted from the central concern of analysis. Within the area of general psychology, functionalists were attracted to the less introspective forms of experimentation, typified by Ebbinghaus' famous study of memory. In the area of perception, they tended to emphasize studies on the physiological concomitants of perceptual activity. Structuralists preferred the more molecular, introspective forms of experimentation. But they too endorsed the Ebbinghaus studies whole-heartedly, and while they considered the functionalists' physiological theories of perception to be "both premature and one-sided," they certainly endorsed the value of the experimental work on which these theories were based.[20] Proponents of both camps agreed that introspection required extensive training, but the functionalists were less dogmatic about the matter, and defended the scientific balue of data obtained from more naive subjects.

If functionalism were to have represented a viable alternative to the traditional structuralist approach, it would have had to have developed an alternative approach to the central work of general psychology. That it failed to do so is made clear in Angell's Chapters from Modern Psychology, which consisted of a series of lectures he had delivered early in 1911. Angell was one of the foremost proponents of functionalism. Yet in his very first lecture, he conceded that it was the work of the structuralists which constituted the core of psychology:

> Every science is under obligation to analyze the phenomena with which it deals. Accordingly, the first business of general psychology is to unravel the tangled skein of mental life....Complicated mental states... must be analyzed and dissected until the secrets of their composition are laid bare.[21]

In his third lecture, Angell extolled the virtues of psychological experimentation. "It has in a single generation wholly altered the face of psychology," he wrote, "and given it a place once and for all among the firmly established sciences."[22] In this entire lecture on what the author discusses as the distinguishing aspect of scientific psychology, the only sections which would allow one to distinguish the author as a functionalist are a few brief paragraphs on the subject of individual differences, a subject which the functionalists held to be of greater significance than did the structuralists. The rest of his chapter was devoted to a description of basic experiments on sensation, association, attention and reaction time. Indeed, with the exception of the paragraphs on individual differences, the entire lecture might easily have formed a chapter from a structuralist's textbook.

The functionalist movement is generally considered to have begun with John Dewey's 1896 article on "The Reflex Arc Concept in Psychology."[23] Dewey had criticized the notion of stimulus and response as discrete units and had emphasized, instead, the extent to which they were interdependent. The stimulus-response unit, he argued, is an irreducible whole. Any given perceived stimulus is itself the product of a sensory motor coordination, as is any response. When we label one such coordinated act a stimulus and another a response, then we are making a teleological distinction on the basis of the function each plays with regard to some end. We may wish to make such a distinction for some purpose, but in so doing, we should recognize that we are abstracting from reality and that stimulus and response actually have no independent existence. It is from this article by Dewey that functionalism first acquired its reputation for being anti-atomistic.

A more pertinent anti-atomistic position was advanced by William James, who is also widely considered to be an inspirational source of American functionalism.[24] James criticized the analytical approach of the Wundtians, insisting that consciousness consisted of a continuous "stream of thought," in which the most conspicuous aspect was flux or change; the supposed mental elements of the experimentalists, he argued, were no more than artifacts of their methodology. These views further strengthened the anti-atomistic reputation of the functionalists.

Still, it should be clear that the anti-atomism of most functionalists was not of the extreme variety implied by James. Angell, for example, the developer of the functionalist school at Chicago, criticized elemental analysis as arid and lacking in relevance to people's vital concerns, but as we have just seen, he had no fundamental objection to the quest for basic elements, and, in fact, supported the structuralist endeavor as a principle aim of psychology. This was the position which most typified the functionalist objections to structuralism. In general, the differences were differences in emphasis, not in substance.

The Structure of Psychology

The traditional conception of the relationship of the various schools of psychology is schematized in Figure 2. According to this view, the field has been divided into a plethora of factions, each complete unto itself. Points or similarity or difference between schools may be discussed, but these relations are not seen as suggesting any kind of structure which could lead to a coherent view of the field.

Figure 3 represents an alternative approach, according to which the paradigmatic structure of a field may be hierarchically conceived. Abstracting any two adjacent levels produces a triangular structure, in which the apex may be labeled "paradigm" and the base points "articulations." Thus, scientific psychology could be

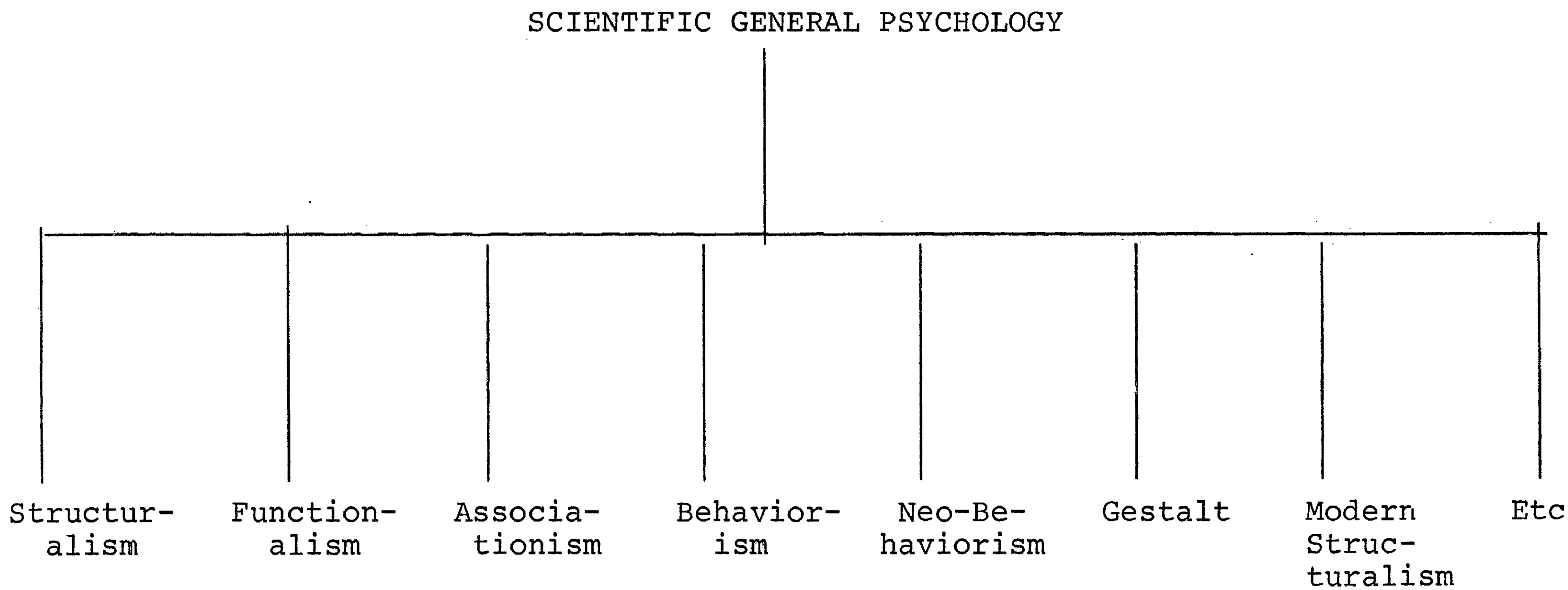
SCIENTIFIC GENERAL PSYCHOLOGY
Structur-alism
Function-alism
Associa-tionism
Behavior-ism
Neo-Be-haviorism
Gestalt
Modern Struc-turalism
Etc

Figure 2.

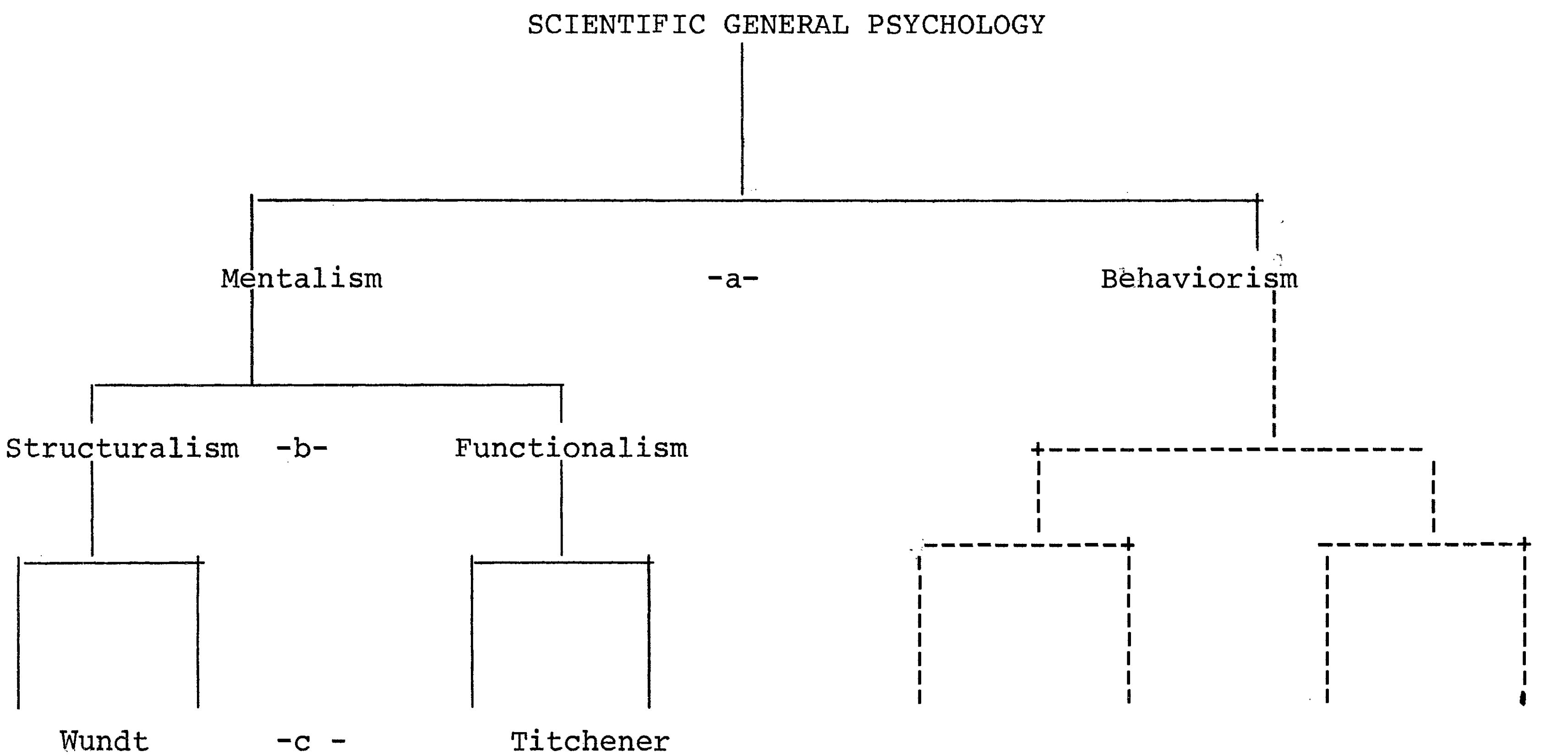
SCIENTIFIC GENERAL PSYCHOLOGY
Mentalism
-a-
Behaviorism
Structuralism
-b-
Functionalism
Wundt
-c -
Titchener

Figure 3

viewed as a paradigm which is distinguishable either from other branches of science or from speculative psychology. At this level of analysis, mentalism and behaviorism would be seen as competing articulations. Or, mentalism can be viewed as a single paradigm, and structuralism and functionalism as its articulations. This, of course, has been the level of analysis dealt with in this chapter. Finally, analysis could be extended downward to deal with intra-school disputes.

It should be noted that this method of analysis is synchronic and in this sense resembles Levi-Strauss' synchronic structural analysis of anthropological myths. This is to say that the various levels are logical levels and are independent of temporal distinctions. On a given level, there exists a binary opposition, such as that of mentalism-behaviorism. Either term of this opposition may be translated into its complement by following a simple transformation rule: Given the definition of psychology and the characterization of its methods of study as described by the adherents of either one of these camps, replace the term designating the object of study with its opposite and exchange direct with indirect in the characterization of the method of study. Similar binary transformations are possible at any logical level.

Three such transformations (labeled a, b, and c in figure 3) may be explicated at this point:

a. Mentalism-behaviorism. Mentalist psychology may be characterized as the study of mind. Mind may be studied through introspection, which is the direct mode of observation, or by objective observation, which is the indirect mode. Behaviorist psychology may be characterized as the study of behavior. Behavior may be studied by objective observation, which is the direct mode, or by verbal report, which is the indirect mode. In both cases, the indirect mode of observation is distinguished from the direct mode in so far as it provides information about the subject matter (mind or behavior) only by inference. The italicized terms are those which are critical to a transformation of one definiton into the other.

b. Structuralism-functionalism. Following the mechanistic tradition in science, structuralists defined the subject matter of psychology (mind) in opposition to that of physics and patterned their approach to the subject on the model of chemistry. Following the organic tradition, functionalists defined the subject matter of psychology in opposition to that of physiology and patterned their approach to the subject on the model of Darwinian evolution.

c. Wundt-Titchener. According to Wundt, the study of consciousness is the study of immediate experience, whereas, physics studies mediate experience. According to Titchener, the study of consciousness is the study of experience as _dependent_ on the experiencing organism, whereas physics studies experience as _independent_ of the experiencing organism.

A functionalist statement at level _c_ would involve a characterization of consciousness as equivilant to adaptive reactions, which is to be distinguished from the vital reactions studied by the physiologists. In order to transform this functionalist statement into either the Wundtian or Titchenerian statement, it would be necessary to transpose not only the terms defining the subject matter (vital reactions to mediate experience or independent experience, and adaptive reactions to immediate or dependent experience), but also to substitute the disciplinary terms which were central to the transformation at level _b_. It is this characteristic which suggests that level _c_ is logically subordinate to level _b_, as level _b_ is to level _a_.

In writing this chapter, I have found myself suffering some embarrasment. The basic unity of mentalist psychology seems to be so self evident that I, at times, wonder why I have devoted so much energy to establishing the point. My sense of embarrasment is eased only when I remind myself that this fundamental paradigmatic unity seems to have escaped the attention of others, and that I myself was unaware of it until after I began to seriously study the period. Historian of psychology Robert I.

Watson seems to be the only scholar to have considered the possibility. Prior to describing the prescriptive themes by which he felt psychological history might be analyzed, he entertained the notion that perhaps psychology had arrived at an initial paradigm, but that it had not been recognized for what it was. "Although the presence of an unrecognized paradigm is not ruled out completely," he wrote, "it would seem plausible to proceed on the assumption that psychology has not yet had its initial paradigmatic revolution."[25]

Throughout this chapter, I have argued that Watson's rejected notion is indeed valid, that psychology did in fact unify around a paradigm which has subsequently not been recognized as such. The question remains: Why has this basic commonality between the various pre-behaviorist "schools" not generally been recognized? The first Watson had certainly recognized it when he issued his behaviorist platform:

> The last fifteen years have seen the growth of what is called functional psychology [he wrote] ....I have done my best to understand the difference between functional psychology and structural psychology. Instead of clarity, confusion grows upon me.[26]

How, then, could such an obvious fact have escaped the notice of later historians of psychology?

There are a number of factors which may have contributed to our inability to recognize the common mentalist model. First, psychology is a young field. A paradigm, in its more global meaning, is a large, all-embracing phenomenon. Just as large physical objects require sufficient distance for the recognition of shape, so too a broad paradigm may require temporal distance for easy recognition.

A second factor is the rapidity with which the mentalist model was overthrown. The Newtonian model in physics also gave rise to competing schools over various questions as well as a host of metaphysical debates, but its essential features remained dominant for some two hundred years. The behaviorist revolution, which reversed the basic tenets of mentalist psychology in much the same way as Marx stood the Hegelian dialectic on its head, began within half a century of the founding of scientific psychology.

Finally, and perhaps most important, by the time the revolution had been completed and "methodological" unity had been achieved, the new behaviorism had already spawned its own competing articulations. To complicate matters further, the new articulations tended more and more to retreat slowly and cautiously back to the conception of mind, beginning with Tolman's introduction of purpose, insight, and cognitive maps, continuing in the 1950's with a reawakened interest among behaviorists in such topics as imagery and thought, and culminating in

Hebb's pronouncement that psychology is after all the science of mind.

Still, these comments on lack of recognition of the mentalist paradigm are both speculative and incomplete. It remains a question which is well worthy of further study.

FOOTNOTES TO CHAPTER III

[1]Titchener, *Text-Book*, p. 58.

[2]"The Province of Functional Psychology," *Psychological Review* 14 (1907). Reprinted in Wayne Dennis, ed., *Readingsin the History of Psychology* (New York:Appleton-Century-Crofts, 1948), p. 443 (emphasis added).

[3]Titchener, *Text-Book*, p. 44; Harvey A Carr, *Psychology* (New York: Longmans, Green and Co., 1925), p. 12; Edward L. Thorndike, "Animal Intelligence," *Psychological Review Monograph Supplements* 8 (1898). Reprinted in Dennis, p. 377.

[4]Carr, p. 13.

[5]Titchener, *Text-Book*, p. 29.

[6]Carr, p. 8.

[7]Titchener, *Text-Book*, p. 23.

[8]Thorndike, p. 384.

[9]Titchener, *Text-Book*, pp. 31-32.

[10]This, of course is nothing less than Bridgeman's operation of "projection," his method of operationalizing another haman being's introspective report (see chapter one).

[11]Charles H. Judd, *Psychology* (Boston: Ginn, 1907); p. v; John B. Watson, "Psychology as the Behaviorist Views It," *Psychological Review* 20 (1913). Reprinted in Dennis, pp. 462-463; Titchener, *Text-Book*, p. 58.

[12]J.B. Watson, p. 462.

[13]Wilhelm Wundt, *Outlines of Psychology*, trans. Charles H. Judd; 3d revised English ed. (London: Williams and Norgate, 1907), pp. 2-3.

[14]Titchener, *Text-Book*, pp. 6-9.

[15]In 1910, Perky provided what was seen as strong experimental evidence in favor of the theory that images

were a sub-category of sensation by demonstrating that there was a fringe area in which perception and imagination could not be introspectively differentiated. Her subjects were instructed to project mental images on a glass screen. The subjects were not told that on some trials actual images would be projected on the screen. Although the actual images were faint, they were perceptible, as was demonstrated by correspondences between the actual projection and the reported imaginary construction. However, the subjects were unable to distinguish between their projections and the actual images. "An Experimental Study of Imagination," American Journal of Psychology 21 (1910): 422-452.

[16]"The Postulates of a Structural Psychology," Philosophical Review 7 (1898). Reprinted in Dennis, p. 372.

[17]Science and Change: 1500-1700 (New York:McGraw Hill, 1971).

[18]Angell, pp. 452-453.

[19]Ibid., p. 447.

[20]Titchener, "The Past Decade in Experimental Psychology," American Journal of Psychology 21 (1910):415.

[21]Angell, Chapters from Modern Psychology (New York: Longmans, Green and Co., 1915), p. 7.

[22]Ibid., p. 115.

[23]Psychological Review 3 (1896): 357-370. Reprinted in Dennis, pp. 355-356.

[24]William James, Principles of Psychology, 2 vols. (New York:Holt, Rinehart and Winston, 1890) 1:157 ff.

[25]"Prescriptive Science," p. 186.

[26]J. B. Watson, p. 462.

CHAPTER IV

THE CRISIS OF MENTALISM

At the turn of the twentieth century, introspection was universally hailed as the prime method of psychology, the only direct means of observing the subject matter of the field. Even the functionalists, who were more free in their use of objective methods, defended introspection as an indispensible tool. But ten years later, introspection as method had become an issue of heated debate. Some writers despaired of its limitations; more extreme critics decried the use of the term altogether. It was this disaffection within the ranks of mentalism which in large measure paved the way for the coming behaviorist revolution.

What had happened during the first decade of the twentieth century that could lead to such a profound disturbance at the very heart of the fledgling field? Kuhn has argued that disciplines enter stages of crisis when they are confronted with serious anomalies. What were the anomalous data of the new psychology? There were two interrelated events which constituted serious and basically unresolvable problems for the prevailing conceptions of the use of introspection. The first of these was the development of seemingly unresolvable differences over the nature of the thought processes, the so-called "imageless thought" controversy. The second was the discovery of non-conscious, and therefore non-introspectable determinants of mind.

The establishment of experimental psychology had required that introspection be justified as a method. Its survival as a mature, problem-solving discipline demanded a justification firm enough to be treated as an assumption. This justification took the form that introspection was in essence no different from "inspection," its counterpart in the physical sciences. All that distinguished the two methods were the objects of observation, or in the words of Wundt and Titchener, the observer's special point of view. One inspected the "things" of the world; one introspected the experiences of those things which constituted the mind. In the physical sciences, the actual existence of things are inferred from the agreement between the reported observations of different observers. These observations need not be simultaneous, however, nor need they be of the same identical event. Rather, the conditions of observation must be specified to the degree that they may be duplicated well enough, so that "any competent observer," i.e., one who is sufficiently trained in the field, will be able to report a similar observation. This same criterion of reliability was explicitly accepted as necessary to the justification of the introspective method. One can directly observe only one's own consciousness. But the conditions of observation, the stimuli, the instructions, the physical setting, etc., can be specified for psychological experiments just as fully as they can be for physical experiments. It was assumed that there was enough similarity in the operation of the minds of different people that "any competent observer," i.e., one sufficiently trained in introspective observation, would produce a similar report given similar conditions. Without this assumption, it was recognized, a science of introspective psychology would not have been possible.

In 1900, it seemed that this assumption could fairly well be justified by the extent of agreement which had already been obtained from experimental studies of mental life, and the psychologists expressed their confidence that the level of agreement would continue to grow. But by the end of the decade, it was fundamental disagreements between indisputably competent observers which had become all to conspicuous. And thus it was that the method of observation itself became a major focus of concern.

The assumption of commonality between different minds was made quite explicit. But there was also an important <u>implicit</u> assumption which helped to determine the nature of the new psychology. This was the assumption which equated mind with consciousness or awareness. Mind had been defined as the sum total of mental processes in the lifetime of an individual; consciousness as the sum total of mental processes occurring at a given time.[1] Thus, by definition, mind was limited to that which at somepoint in time had been conscious and therefore available to introspection. Since mental life was a conscious phenomenon, and since introspection was, by definition, the observation of consciousness, therefore and empirical psychology was possible. But to the extent that non-conscious factors played a role in mental life, to that extent introspection must be inadequate. (Though it must be noted that introspection was later identified with free association as a method of bringing the unconscious to consciousness. "The psychoanalytic method is itself introspection raised to the _n_th power," wrote one author,[2] and another wrote somewhat less approvingly: "Psychoanalysis itself is of course, nothing but a rough and unscientific form of introspection."[3]) It was during the decade of 1900 to 1910, that Freud's doctrine of the unconscious first became popular among psychologists, but though anomalous, it was not devastating. The products of the psychoanalytic method were dismissed as unreliable by many experimentalists. What proved more troublesome was the firm establishment of non-conscious determinants of mental activity as a by-product of the same series of studies from which the imageless thought controversy grew.

It is to these two issues, the imageless thought controversy and the discovery of non-conscious mental factors, that we must now turn.

Imageless Thought

After its initial preoccupation with the reaction experiment, early experimental psychology focused primarily on the analysis of simple sensation, with occasional excursions in the realms of feeling, attention, association and the like. From the first, there was some doubt as to whether the experimental method would prove sufficient to the study of the higher mental processes, and for this purpose, Wundt had turned to what he called *Völkerpsycholegie*, or folk-psychology, the study of the history of human nature. Some of Wundt's students, however, were more optimistic about the possibilities of introspective experimentation. In 1894, Oswald Külpe left the Leipzig laboratory to accept a chair at Würzburg, where he sponsored a series of experimental studies on the process of thought. It was these studies, supported by two additional independent series of studies--one by Binet in France, and the other by Woodworth in the United States--that led to the most serious internal conflict in the history of introspective psychology.

The first of these studies, published in 1901, was conducted on the psychology of judgment by Marbe at

Würzburg. Marbe conceived of his task as that of determining "what experiences must supervene upon a conscious process in order to raise it to the rank of a judgment?"[4] To accomplish this, he set up a number of tasks requiring judgments on the part of his observers, such as lifting two weights and inverting the heavier, performing specified arithmetical operations, and answering simple questions of fact, following which they were required to report on the conscious experiences which had intervened between the presentation of the stimulus and their response. These experiments had negative results, and Marbe was forced to conclude that there were no psychological conditions which distinguished judgment from other mental processes. This in itself was disturbing, but there was also an unexpected positive result. Marbe's well trained introspective observers reported the occurrence of conscious experiences which were not analyzable into the traditional categories of sensations, images, or affections. Some of these were expressed as doubt or certainty, as the remembrance of information contained in the instructions (but not in verbal form, since this would have constituted a type of image), as the realization of some fact (e.g., that the product of a given division will leave no remainder), or as "conscious processes which obviously refused description."[5] These experiences were termed

Bewusstseinlage, or conscious attitudes, by J. Orth, one of Marbe's observers.

Marbe began his study of judgment consciousness in the year 1900. That same year, Alfred Binet independently began a similar series of studies on the higher mental processes, using his two teenage daughters as observers. Binet had already spent a number of years developing mental tests for the purpose of identifying individual differences. In this new study, he added the task of introspection with the expressed aim of investigating the qualitative nature of the thought process. His daughters were asked to write lists of words, to write sentences, to complete sentences, to write compositions on specified topics, and so on. Following each task, they were asked to report on the nature of the conscious experience intervening between the stimulus and the response. In writing a particular word, the word "table" for example, had they thought of a particular table, of a table in general without any particular example in mind, or had they written the word without thinking of anything in particular? Like Marbe's observers at Würzburg, Binet's daughters reported that at times they became aware of thought components which could not be classified as sensational or imaginal.

Yet another independent series of studies yielding similar results was begun at Columbia University by

Robert Sessions Woodworth in 1903. His subject matter was voluntary movement, and his aim was to demonstrate that such movement was not necessarily preceded by kinesthetic imagery. He did not, at the outset, anticipate the existence of imageless thought; his expectation was only that the imagery might not be kinesthetic. Later, he wrote to a colleague of his surprise when "it became evident that the subjects were often unable to detect any image whatever. . . [and he was forced to conclude] that one could have the thought of a movement without any image of it."[6] Woodworth continued his work on the thought process with tasks which were more similar to those used at Würzburg. Observers were asked to answer questions, to complete analogies, and so on, although there were characteristically national differences in the nature of the questions asked. While the Germans asked questions like: "Does Monism really involve the negation of personality," the brash American asked, "Should a man be allowed to marry his widow's sister," a question in which Titchener was able to find no food for thought.[7] As a result of these experimental studies, Woodworth concluded that the thought often appeared in consciousness prior to the auditory image of the word, and that there therefore existed an imageless thought element.

Meanwhile, back at Würzburg, studies on the nature of the thought processes were continued by Watt, Ach, Messer and Bühler. In these studies, new tasks were set for the observers and various refinements of method were developed. Yet the same results were forthcoming: the existence of imageless thought contents. In 1904, Watt employed "partially constrained associative reactions" as an experimental task. Observers were presented with a stimulus word and were required to respond with a word standing in some specified logical relation to the stimulus (such as subordinate, superordinate, etc.). Two years later, Messer added free association to the list of tasks. In 1905, Ach formulated the methodological refinement which became known as "systematic experimental introspection." It consisted of dividing the experiment into three temporal periods: the fore-period, extending from the signal that the stimulus is to be given to the actual presentation, the mid-period, from the presentation of the stimulus to the emission of the response, and the after-period, consisting of the few minutes immediately following the response. The observer was interrogated during the after-period about his conscious experiences during one of the three periods. This procedure soon became a hallmark of the work done at Würzburg. Actually, most of the components of systematic experimental introspection had already been used by Watt, but it was Ach who gave it its name and called attention to it as an important methodological advance. Finally, in 1907, Bühler developed the controversial *Ausfragemethode*, in which the experimenter was allowed to interrogate the observer more freely.

With each of these studies, the position of imageless thought was strengthened. Ach was able to find in his observers the existence of imageless thought elements which were described as an awareness of knowledge. These he termed *Bewusstheit*, subdivided into two types: awareness of meaning and awareness of relation. The *Bewusstheit* were to be distinguished from Marbe's even less palpable *Bewusstseinlage*. Finally, Bühler reported, in 1907, the clearest examples of thought elements described by observers as totally independent of any sensory, imaginal or affective qualities, yet possessing a high degree of vividness and clarity.

In and of itself, the discovery of imageless thought could have no serious negative effect on the mentalist paradigm. Earlier, it had been assumed that images were the elements of thought, just as sensations were the elements of perception, but the door had explicitly been left open for the discovery of new elements. The intangible nature of the proposed new elements discovered at Würzburg was somewhat problematic, but certainly not devastating. What proved to be more difficult was the fact that those investigators, who, like Titchener, were skeptical of the existence of the new imageless thought elements, were able to find no trace of them in their own introspective thought experiments.

One of the earliest and most important criticisms of the imageless thought experimental results was that leveled against Bühler by Dürr. It was especially important because Dürr had been one of Bühler's observers, upon whose introspective reports Bühler had based his conclusions. Following the publication of Bühler's study, Dürr wrote:

> Over and over again, as I was observing for Bühler, I had the impression, though I was not able at the time to formulate it very clearly, that my report was simply a somewhat modified verbal statement of the thoughts aroused in me by the experimenter, and that this verbal statement could not properly be regarded as a psychological description of the thoughts.[8]

This criticism caught the attention of Titchener, who saw in it something akin to what he had previously referred to as the stimulus error. The stimulus error was Titchener's label for the reports of untrained observers, who tended to report on the external things which they saw rather than on the psychological experiences to which the things gave rise. So also in the Würzburg experiments, Titchener charged, "the observers tell us, not what consciousness is, but what it is about."[9] In charging the Würzburg observers with committing the stimulus error, he was on rather shaky grounds, however, for these were not naive students, but well trained introspectors, including Titchener's former colleague from Leipzig, Oswald Külpe.

Adding experimental fuel to the discord, Titchener's associates at Cornell and other experimentalists at Clark repeated the work done at Würzburg, but found that the imageless thoughts were actually not imageless at all. Pyle studied expectation and concluded that it was reducible to organic and kinesthetic sensations;[10] Okabe found the same for belief;[11] Clarke was able to reduce Ach's Bewusstheit, or awareness of knowledge, to muscular strains and other organic sensations;[12] and Kakise discovered the so-called imageless thought elements to actually be a very primitive and rudimentary form of image.[13]

Meanwhile, other investigators continued to find new and different kinds of imageless thought elements. Dürr, in criticizing Bühler, had not abandoned the notion of imageless thought, only the specific variety of imageless thought which Bühler had claimed to find. In its place, Dürr found elements of a "relational consciousness." Similarly, von Aster contested Bühler's conclusions and suggested that what had been observed were actually "affectively toned attitudes."[14] Titchener finally commented, with some evident exasperation, that "every year sees the proposal of some fresh process as candidate for elemental rank."[15]

Some writers attempted to ameliorate the dispute by attributing the discrepant results to genuine individual differences. Some folks have imageless thoughts, they suggested, and others do not. But for most, this solution would not hold water, for it became evident that the observer's answer to the question of the nature of the thought process depended largely on the school in which he was trained. Along with this realization came an alternative explanation of the discrepancies, one which would prove to be more acceptable to mainstream psychology. But it was also an explanation which ultimately undermined the earlier confidence with which the introspective method had been used. In order to fully appreciate this new explanation, however, we must first explore the development of the scond major anomaly to which I have referred: the discovery of non-conscious determinants of mind.

The Non-conscious Mind

The discovery of non-conscious and non-introspectable mental determinants began with the problems associated with the early use of the reaction experiment. As has been noted earlier, paradigms exist on many levels and may be arranged in a hierarchical structure. The early reaction experiment constituted a lower level paradigm within the broader, mentalist, experimental paradigm. The exemplars were the studies produced by Donders and de Jaager in the 1860's. The critical aspect of these studies was the use and interpretation of the subtractive procedure. The essence of this procedure consisted of requiring a given reaction to some stimulus and then altering the conditions so that a more complex reaction was required. The difference in reaction times was taken to represent the duration of the new mental processes required by the more complicated task. Thus, the time difference between a simple reaction and a reaction requiring that the appropriate stimulus first be discriminated from among other stimuli constituted the duration of the mental act of discrimination. Similarly, subtracting the time of a discrimination reaction from one which also required the choice of an appropriate response resulted, it was believed, in the duration of the act of choice.

The duration of both of these mental acts, discrimination and choice, had been measured by Donders and de Jaager. Later, in Leipzig, Wundt extended the method in order to measure the durations of voluntary impulses, of perception, cognition, association, judgment and so on. But unfortunately, the procedure did not work well and the time differences thus calculated proved to be highly unreliable. In particular, the reaction times for more complex reactions proved, at times, to be equal to those for the simple reaction. What was needed was a new conception of the reaction experiment, a reconceptualization such as that provided by Külpe in 1893. This reconceptualization constituted the first micro-revolution in the field of experimental psychology. It simultaneously led to the discovery of the non-conscious psychological factors which were to prove most troublesome for the more global mentalist paradigm.

The shift in interpretation began with a series of experiments conducted at Leipzig by Ludwig Lange in 1888. Lange studied the difference in reaction time which was a function of the direction of attention, and found that the reaction time was shorter when attention was focused on the sensation of the stimulus (sensorial reaction). Lange's explanation of these results, generally accepted by Wundt, was based on the ideas suggested by the assumptions underlying the subtractive procedure. The muscular reaction was essentially reflexive; the stimulus in this case was perceived, but not apperceived, which is to say that it was not brought to the center of consciousness before the response was initiated. In the sensorial reaction, on the other hand, the stimulus was apperceived, i.e., brought fully into focus. Thus, the time difference between the two types of reaction represented the duration of the act of apperception.

Additional experiments were soon undertaken for the purpose of testing the adequacy of his explanation. Subjects were instructed to react as quickly as possible, "without waiting for a full and clear apperception of the sense-impression which served as stimulus."[16] But, despite this precaution, a reliable difference between the sensorial and muscular reaction remained. Therefore, the explanation was expanded to include the act of preparation for the muscular action. Not only was the sensorial reaction time lengthened by the apperception, but also, the muscular reaction time was shortened because it was preceded by an act of conscious preparation for the required movement.

Both of these factors were duly noted by Külpe in his Outlines of Psychology, in which, for the first time, the implications of this new factor of psychological preparation were clearly spelled out.[17] In order for the subtractive procedure to yield a valid result, the addition of the particular mental act must constitute the only difference between the two reactions. All other factors, the nature of the stimulus, the response, etc., must be held constant. But now a new factor had been added: the degree of preparation for the response, and Külpe proceeded to demonstrate that the preparation is different for each particular type of reaction. Even more importantly, he concluded that there was no way of determining how much of the additional time was due to less complete preparation and how much was due to the additional mental act. The evidence for this position seems to me to have been scanty and inconclusive, but in 1893, it must have seemed highly persuasive. It appears to have been generally accepted, despite the fact that it required the abandonment of the measurement of the duration of mental acts, a problem which had previously been the chief focus of the new science. Yet this new factor was to have even more profound effects in the future. At Würzburg, Külpe's "preparation" would evolve into the Aufgabe and unconscious determining tendencies, and our modern notions of expectancy, and of psychological set can also be traced to this same source.

Reprise: Külpe left Leipzig in 1894, to accept a chair at Würzburg. In 1900, the first in the series of imageless thought experiments was begun at Würzburg by Marbe. The work was continued by Watt in a study published in 1904. Marbe had undertaken his research in order to find the psychological conditions of judgment. He was able to find no conscious conditions which would set judgment apart form other mental acts. He did, however, find conscious contents which did not seem to fit into the traditional categories: the Bewusstseinlage, or conscious attitudes, which gave experimental birth to the notion of imageless thoughts (the idea had already occurred to some pre-experimental writers).

In his follow-up study, Watt was able to find one psychological condition of judgment. This was the

Aufgabe, variously translated as the purpose of the problem, it consisted of the nature of the experimental task as set forth in the instructions. At the outset of the experiment, the observer was aware of the *Aufgabe*, and it would often remain in consciousness during the early experimental trials. During later trials, it generally faded from consciousness. Yet the nature of the later responses clearly indicated that the *Aufgabe* was still operative even though introspection revealed that it was no longer a part of conscious awareness. Let us examine a concrete example. The observer is instructed that he will be given pairs of numbers as stimuli, and that he is to respond by giving the sum of each pair. The *Aufgabe* in this case is that he is to add the two numbers together. During a later experimental trial, he is presented with the stimulus numbers 3 and 4. He responds with the number 7. Afterwards, he reports that during this trial he was at no time aware of thinking consciously that he was engaging in the act of addition. That the *Aufgabe* was still operative, however, is evident in the fact that his response was the sum of the two stimulus numbers. The *Aufgabe* of addition had become habitual; it had faded from consciousness; yet it continued to determine the contents of consciousness. "As conscious experience," wrote Watt, "this psychological factor is itself past and gone, but it still persists as an appreciable influence."[18]

In 1905, Ach gave the name "determining tendency" to the psychological set established by the Aufgabe and clearly specified its unconscious operation. The following year, Messer adopted the procedure of interrupting the experiment at the end of the fore-period and found that even at this point, the Aufgabe might not be consciously experienced. He concluded that "many of the 'problems' that give direction to human activity have this character of the obvious, and in so far of the unconscious, and that philosophical reflection and self examination are needed to raise them into clear light of consciousness."[19]

The ideas contained in these experimental results were fully as important and problematic as the hypothesized imageless thought elements, but they were not nearly as controversial. The concept of unconscious determining tendencies were accepted even by the staunch traditionalist, Titchener, who found nothing revolutionary in their acceptance:

> The notion of an external and precedent determination of consciousness [he wrote] is, of course, familiar enough; we speak of command, of suggestion, of instruction, of the influence of surroundings of classroom atmosphere and laboratory atmosphere, of professional attitude, of class bias, of habit and disposition, of tempermental interests and predilections, of inherited ability and inherited defect; and in all these cases we imply that the trend of a present consciousness, the direction that it takes, is determined beforehand and from without...[20]

What, then, was new about the determining tendencies that had emerged from the Würzburg studies of Watt and Ach? Only that they had now entered the experimental arena and were found to be necessary to any understanding of the thought processes; for "a thing may be a commonplace of the text-books, and yet have escaped experimental study."[21] Külpe had found that an earlier conscious preparation or expectancy affected the outcome of reaction time experiments. Now his associates at Würzburg had found similar effects from the instructions used in the thought experiments. What is more, the instruction could exert its influence while at the same time totally bypassing conscious experience. Thus, here was a factor which all agreed required further experimental investigation, but, which by its very nature was inaccessible to introspection, the only direct observational instrument of empirical psychology.

From Anomaly to Crisis

Had the matter rested there, the course of psychological history might have been quite different. Introspection could have been recognized as a valuable, though somewhat limited, tool, which required additional supplementation by the objective indirect modes of observation. This, after all, was the position taken by the functionalists, and, to some extent, it had also been acceptable to the structuralists. After all, it had already been noted that association was not a conscious process. Yet it had been brought under experimental control by Ebbinghaus in a manner which was hailed, even by the most consistent structuralists, as "the most considerable advance, in this chapter of psychology, since the time of Aristotle."[22]

But the matter could not rest at that point. Discrepant results on the nature of the elements of thought had cropped up, despite the fact that observers on all sides of the issue were well trained introspectionists. This required explanation, especially in a new field, still insecure as to its scientific status. Titchener valiantly attempted to defend the field by comparison to the more solidly established disciplines: "Psychology is not the only science in which the strict application of the best available method leads to opposite conclusions," he wrote, and he proceeded to list a number of similar disputes in the fields of physics, astronomy and physiology.[23] Given continued observations, the dispute would eventually be resolved.

Still, even Titchener himself must have been uncomfortable with this proposed solution, for he devoted considerable attention to the task of explaining the specific causes of the divergent results. In so doing, he returned once again to the question of the stimulus error, the error of attending to the stimulus itself rather than to the sensations to which the stimulus gave rise. To this error, the untrained observer was particularly prone. He might, for example, think of himself as matching colored papers, rather than color-sensations, while supposedly engaging in introspective observation. The danger of the stimulus error was prepared by the habits of every day life, in which we are accustomed to attend to stimuli rather than to sensations. This danger is so great, Titchener argued, that even practiced observers might lapse into it, unless they consistently maintained their guard. For example, his own experimentalists had fallen into the stimulus error when they complained that the introspective method was unreliable in tachistoscopic experiments:

> for if we compare the observer's report with the stimuli actually exposed [Titchener paraphrased their objections], we find that he may see what was not there at all, may fail to see much of what was there, and may misrepresent the little that he really perceived; introspection adds, subtracts and distorts.[24]

All this may be true, Titchener conceded but it says nothing about the reliability or validity of introspection, for:

> The question...is not whether the reports tally with the stimuli, but whether they gave accurate descriptions of the observers experiential consciousness; they might be fantastically wrong in the first regard, and yet absolutely accurate in regard to conscious contents. In other words, the objection issues from the stimulus error.[25]

This, Titchener maintained, is essentially what had happened in the Würzburg experiments, for the danger of slipping into the common sense observational attitude of every day life "is increased tenfold in the case of thought. For the psychology of thought leads straight up to, passes directly over into, a functional logic, a theory of knowledge; you may love the one and hate the other, but you cannot be sure that you are always on your own side of the line."[26] The Bewusstseinlage, the conscious attitudes reported by the Würzburg observers, were statements of meaning which should not be allowable in introspective reports. Just as an observer in an experiment on color sensation must avoid reference to the fact that the stimulus is colored paper, so too the thought observver must confine himself to an analysis of the underlying sensational components of thought.

This, then, was the essense of Titchener's critique. All that remained was to link up the concept of the stimulus error with that of the Aufgabe for the crisis of mentalism to reach full bloom; and this was accomplished in short order. As we have seen, Titchener had already related the notion of determining tendencies to such earlier considerations as professional attitudes, habits and dispositions. What then was the stimulus error, but a failure to adhere to the professional attitude of the experimental psychologist and a reliance on the observational habits of common sense? At Würzburg, Messer in 1906, had extended the notion of the *Aufgabe* to include the attitude of every day observation, with its purpose 'to cognise,' and the special attitude of the introspective psychologist as he studies sensations and ideas in the laboratory. This expanded notion of the *Aufgabe* was fully accepted by Titchener:

> Let us remember that the chances of error are legion, and not be surprised if we succumb. But let us cling to the ideal of writing a psychology; let that *Aufgabe* be perpetually present in consciousness; let us adopt it as a regulative principle of our procedure.[27]

Thus, the stimulus error consisted of slipping from the special *Aufgabe* which is "appropriate" for the experimental psychologist into the generally unconscious and habitual *Aufgabe* of every day observation.

Now once this connection had been made, the next logical step was unavoidable, though it could not be taken by Titchener himself. If the results of an introspective observation is in part dependent on the particular *Aufgabe* which is adopted, if the *Aufgabe* for the purpose of a psychological experiment is necessarily an artificial one requiring intensive training, and if this *Aufgabe* may operate at times unconsciously, composed not only of specific instructions, but also of habits, dispositions and temperamental interests and predilections, then in what sense are we justified in referring to one experimentalist's _Aufgabe_ as an error and of another's as correct? That this step was a logical extension of Titchener's position was first noted in print by Robert Ogden in his 1911 review of the imageless thought controversy. If Titchener can refer to imageless thought observations as being due to a stimulus error, then perhaps Titchener and his followers may with equal justification be accused of committing the counter-error of confining his introspection to the categories of sensation, image and feeling. "May we not carry the point a step farther and deny the value of _all_ introspection? Indeed, in a recent discussion among psychologists, this position was vigorously maintained by two among those present."[28] Ogden, however, was not himself prepared to take this final step, but only to defend the stimulus error of the imageless thought camp as against the counter-error of the Titchenerian observers.

The identity of those two prophetic objectors may forever remain a mystery, but the debate over the value of introspection was now under way, and objections to the method continued to appear in the psychological journals. In 1912, the most influential of the attacks on introspection (as judged by the amount of attention it received from the other writers at the time), appeared in the _American Journal of Psychology_. This was an article by Raymond Dodge titled "The Theory and Limitations of Introspection." Dodge noted two major points which psychologists were then finding disturbing. First, the nature of the imageless thought controversy indicated that the contents of an introspective observation were predetermined by the categories and schemes which had been supplied to the observer through his training, and second, the degree of apparent influence of unconscious factors implied that introspection, even if valid, could do no more than scratch the surface of mental life.

Drawing out the implications of the first point, Dodge wrote that introspection:

> regularly and inevitably contaminates the results....If a factor is expected, it is ipso facto in consciousness. No amount of scientific caution can separate entirely the observed fact from its apperceptive masses. Even if one conjectures that a factor will not appear, its subsequent appearance will not be entirely free from the possiblity of error. To have been considered at all is to have been in consciousness....[For these reasons] no psychological scheme has been too absurd to be supported by introspection. It shows fashions like hysteria and the delusions of the insane. Even the fundamental categories of consciousness change with the years, while new and previously totally unsuspected facts may be readily introspected as soon as there is theoretical ground for belief that they exist.[29]

To a post-Kuhnian reader, this last conclusion might not appear quite as damning as it did in 1912. It seems to parallel exactly the discovery of the planet Uranus, which was observed as such by astronomers only after theoretical grounds for belief in its existence had been publicized. Thus, this characteristic of introspective observation might be seen today as an important positive result, throwing into relief a critical aspect of all observation. But in 1912, it constituted a serious critique of the use of introspection altogether. It implied that the question of the existence of imageless thought, as well as a host of other disputed questions concerning the nature of mental processes, was not only unsolved, but also that it was in principle unsolvable. It began to seem, as John B. Watson would write the following year in his revolutionary manifesto, "that two hundred years from now, unless the introspective method is discarded, psychology will still be divided on the question as to whether auditory sensations have the quality of 'extension,' whether intensity is an attribute which can be applied to color; whether there is a difference in 'texture' between image and sensation and upon many others of like character."[30]

The second major objection to introspection discussed by Dodge was the limitations of the method that were implied by the acceptance of the notion of unconscious mental activities. The finer details of Freud's work were looked upon with skepticism by most experimentalists. But with the Würzburg results in mind, and particularly the non-conscious operation of the Aufgabe, the existence of an unconscious mental reality seemed undeniable. Perhaps the psychoanalytic method (being in the eyes of some experimentalists only a rough and unscientific form of introspection) had led to a distorted view of this unconscious mental reality, but to an ever increasing number of psychologists, it seemed that experimental introspection had similarly been producing a distorted view of consciousness. And in any case, whatever the reality of the "subconscious" was, it seemed to most to be inaccessible to introspection. To Dodge and others, the idea of the unconscious was merged with the idea of the <u>Aufgabe</u>, with determining tendencies and the like, and this, it seemed, must be the real basis of conscious experience. Thus, even if introspection could be made reliable, it would still remain incapable of disclosing "the real elements, in the sense of the stuff of which consciousness is composed."[31]

For a scientific paradigm to be overthrown, an alternative must be available to replace it, and prior to Watson's article in 1913, no acceptable alternative had been made explicit. Psychology was still the science of mind, and introspection, however limited or unreliable, was still the only direct means of observing that subject matter. Thus Dodge might characterize the method as "inevitably contaminating" and compare its results to "the delusions of the insane," yet he was unwilling to dispense with it altogether. Rather, he defended "the reality of introspecion and its 'radical' importance in any science of human experience [as] indisputable."[32] In reading this statement, we must bear in mind that the term "experience" was still read as synon mous with consciousness and/or mind. So long as this remained the definition of psychology, introspection was indispensable.

Still, the crisis of mentalism was now complete. Psychologists were compelled to retain a seemingly insane and delusory tool at the very center of their field. Further, they were faced with fundamental disputes and no adequate method for the resolution of those disputes. This psychological mood of the 1910's provided a ground so fertile, that Watson's brief article in the Psychological Review, suggesting a redefinition of psychology as the science of behavior and therefore allowing the total abandonment of introspection, was able to take root and inaugurate a revolution which would culminate in the eventual burial of the mentalist paradigm.

Revolutions are not completed overnight, of course, and within mentalist circles, the old debates continued to rage. In 1917, experimental introspection, like free association, was seen as a method for making the unconscious conscious, a view which had already been forecasted by Messer in 1906 (see above). Thus, the interpretation of introspective results depended more and more on one's theoretical stance with regard to the nature of the subconscious and its relation to the conscious, a question around which there has never been widespread agreement among psychologists.[33] In the midst of this continued confusion, the ranks of the mentalists continued to suffer greater and greater attrition. Finally, when Titchener died in 1927, the period of classical introspection came to a close. By the 1930's, "methodological" behaviorism had become the only legitimate coin of the realm. "Times are different; things have changed," it might well have been said, "We are all revolutionaries nowadays."

FOOTNOTES TO CHAPTER IV

[1]Titchener, _Text-Book_, pp. 15-19.

[2]F. L. Wells, "Dynamic Psychology," _Psychological Bulletin_ 10 (1913); 435.

[3]Margaret F. Washburn, "Some Thoughts on the Last Quarter Century in Psychology," _Philosophical Review_ 26 (1917): 54.

[4]Quoted in Titchener, _Lectures on the Experimental Psychology of the Thought Processes_ (New York: MacMillan, 1909; reprint ed., New York: Arno Press, 1973), p. 80.

[5]Ibid., p. 100.

[6]Robert M. Ogden, "Imageless Thought: Résumé and Critique," _Psychological Bulletin_ 6 (1911): 184.

[7]Titchener, _Lectures_, pp. 90-92.

[8]Ibid., p. 150.

[9]Ibid., p. 151.

[10]W.H. Pyles, "An Experimental Study of Expectation," _American Journal of Psychology_ 20 (1909):530-569.

[11]T. Okabe, "An Experimental Study of Belief," _American Journal of Psychology_ 21 (1910): 563-596.

[12]H. M. Clarke, "Conscious Attitudes," _American Journal of Psychology_ 22 (1911):214-249.

[13]H. Kakise, "A Preliminary Experimental Study of the Conscious Concomitants of Understanding," _American Journal of Psychology_ 22 (1911): 14-64.

[14]Titchener, "Description vs. Statement of Meaning," _American Journal of Psychology_ 23 (1912):165-182.

[15]Titchener, _Text-Book_, p. 47.

[16]Titchener, "The Leipzig School of Experimental Psychology," _Mind_, n.s. 1 (1892): 219.

[17]Oswald Külpe, _Outlines of Psychology_ (1893). trans., Titchener (London: Swan Sonnenschein & Co., 1895), pp. 406-422.

[18]Quoted in Titchener, _Lectures_, p. 122.

[19]Ibid., p. 124.

[20]Ibid., p. 161.

[21]Ibid., p. 161.

[22]Titchener, _Text-Book_, p. 381.

[23]Titchener, "Prolegomena to a Study of Introspection," _American Journal of Psychology_ 23 (1912): 437.

[24]Titchener, "The Schema of Introspection," _American Journal of Psychology_ 23 (1912): 489.

[25]Ibid., p. 489.

[26]Titchener, _Lectures_, p. 167.

[27]Ibid., p. 168.

[28]Ibid., p. 193.

[29]Raymond Dodge, "The Theory and Limitations of Introspection," _American Journal of Psychology_ 23 (1912): 227.

[30]J. B. Watson, p. 462.

[31]Dodge, p. 225.

[32]Ibid., p. 218.

[33]Lillien J. Martin, "Introspection versus the Subconscious," _Psychological Review_ 24 (1917): 242-243.

CHAPTER V

CONCLUSIONS

In chapter two, I outlined Thomas Kuhn's schema of scientific development and indicated that it would serve as the conceptual framework for this discussion of early experimental psychology. At this point, it seems reasonable to conclude by asking to what extent the actual development of the field, as presented in chapters three and four, has fit the Kuhnian model.

According to Kuhn, a discipline becomes a mature field when some practitioner presents an achievement which is capable of winning over a group of adherents from competing modes of activity. The paradigm establishing achievement in psychology was clearly the work coming out of Wundt's psychological laboratory at Leipzig. Students from different countries flocked to Wundt's side, including the Englishman Titchener, who was to become the leading exponent of structuralism in America; the Americans, Hall, Cattel and Angell; Europeans Külpe, Marbe, and Kraepelin; and many others.

The Wundtian paradigm, which, in its more global sense, I have labeled "mentalism," provided a definition of the field (the experimental study of mind or consciousness). It provided problems for psychologists to investigate (the duration of mental processes, the qualitative and quantitative analysis of mental processes, etc.), as well as procedures for solving those problems (experimental manipulation of stimulus variables, followed by introspective and/or objective observation of the resulting conscious experiences). Finally, it provided a way of looking and a way of seeing the data, which subsequently came to be labeled an Aufgabe.

Kuhn has listed the types of puzzle-solving activity, which he characterizes as "normal science." These include the investigation of particularly relevant facts, the extension of the paradigm to new situations, and the refinement of theory and technique, all of which were represented in the work of the mentalist experimental psychologists. For example, a considerable amount of time was devoted to the further investigation of the elements of sensation, facts which were designated by the mentalist paradigm as particularly significant. The thought experiments at Würzburg, as well as those carried out by Woodworth and Binet, were initially nothing more than attempts to extend the paradigm to a new situation, the higher mental processes. A refinement of technique, "systematic experimental introspection," was also one of the products of the Wurzburg investigations. There were also many refinements of technique in less troubled areas, such as the nonsense syllable method of investigating memory developed by Ebbinghaus. Finally, Külpe's discussion of the role of preparation in the reaction experiment, while constituting a revolutionary change for the reaction time micro-paradigm, provides an example of theory refinement within the broader mentalist paradigm.

Anomalies, it has been noted, may occur through the failure to solve a problem by normal research methods, or by the failure of a piece of equipment to function as expected. The development of the imageless thought controversy easily fits the first of these descriptions. The problem was the qualitative nature of the elements of thought, a problem which ought to have been solvable by introspective experimental methods. Rather than bringing agreement, however, replications of the thought experiments resulted in ever more discrepant discriptions of the basic elements. Perhaps this might also be viewed as a failure of equipment to function as expected, provided that we are willing to interpret the term "equipment" broadly enough to include the introspective observer. The expectation had been that given careful training of the observer (i.e., careful construction of the equipment) and careful control of the experimental conditions, introspective observation should yield highly similar reports. This consistently failed to happen in the thought experiments.

Kuhn has noted that the initial resistance with which an anomaly is generally met may take the form of blaming the discrepant results on the alleged incompetence of the individual scientist. This, of course, is precisely what Titchener attempted to do when he argued that the reports of imageless thought elements resulted from the commission of the stimulus error. This explanation did not prove satisfactory to the field as a whole, however, and an increasing amount of attention was directed toward the topic. By 1905, more articles on the higher mental process were being published in psychological journals than on any other subject.[1]

During periods of crisis, competing schools, representing different articulations of the basic paradigm, may appear. On this point, there is only a partial fit with the historical data. The Würzburg school and Titchener's group certainly fit the model, in that their dispute centered around the crisis provoking imageless thought controversy, but the most conspicuous division within the ranks of American psychology was the split between structuralism and functionalism. That split preceded the imageless thought controversy by a few years and did not occur in response to any particular anomaly.[2] On the other hand, we have seen that the differences between these camps have often been magnified out of proportion. By and large, each camp agreed, at least in principle, to the bulk of the work done by the other. A way of viewing this development of functionalism developed out of discussions with Dr. Milton Wolpin. The essense of this approach is that practitioners working under a particular model may be attracted to alternate approaches to which they are exposed as a result of developments in other disciplines, and that this may occur in the absence of any profound crisis, particularly if the new model does not constitute too muchoof a threat to fundamental assumptions. The analogy (suggested by Wolpin) is that of a married man or woman at a party who is attracted to a member of the opposite sex (other than his or her spouse). The probability of further action would in part be a function of the extent it was felt that such action might constitute a threat to the existing relationship. Carrying the analogy a step further, I would suggest that whereas the Titchenerian structuralists viewed the attention devoted by the functionalists to non-analytic activity with jealousy, the functionalists did not seem to feel that their activity necessitated a divorce. "Keep the home fires burning," they told their structuralist colleagues, "while we have some fun outside."

In most other respects, the crisis of mentalism well resembled the Kuhnian model. The debate involved questions of appropriate methods and standards of solution, and as the crisis deepened, these became more and more fundamental. Beginning with the finer details of introspective experimental procedure, the entire concept of whether an introspective method was valid was finally challenged. In the effort to resolve the dispute, recourse was taken to philosophical argument.

Finally, the mentalist paradigm was replaced by the behaviorist model. This was ushered in by Watson's article proposing a new definition for the field. Psychology would be the science of behavior, rather than the science of mind. As Kuhn has suggested is often the case, many aspects of the new approach had previously been suggested, but in the absence of a profound crisis, these suggestions had generally been ignored. In 1913, however, they were seized with fervor and the ranks of behaviorism were swelled with new young recruits. It was at this point that the greatest proliferation of competing schools occurred. Representatives of the older model were fractionated, each proclaiming his own school, and each making behaviorism the major focus of his attack. The structural and functional psychologies of the earlier period were replaced by Woodworth's dynamic psychology, McDougall's hormic psychology, Washburn's motor psychology, Spearman's factor psychology ("a school to end all schools"), and so on. There seemed to be more schools of psychology than there were proponents. It was as though, facing the possibility of extinction, the remaining mentalists, rather than closing ranks against the common enemy, began searching almost in desperation for some formulation which might prove capable of turning the tide.[3]

Along with the new behavioral definition of the field came new ways of seeing the data. Introspection became verbal report, speech responses "to the weak stimulation of obscure receptors."[4] In general, the behaviorists had adopted a set of conceptual categories which enabled them to see units of behavior in the same situations in which the mentalists had seen conscious activity. The differences between the two camps were such that no objective test of their relative merits seemed possible, and none seems to have been attempted. The behaviorists merely won a political struggle for hegemony over the field.

Kuhn has noted that anomalies may be successfully assimilated into the existing paradigm, they may be set aside as temporarily too difficult, or they may lead to revolution, but he is not clear as to the factors which are responsible for determining which of these alternatives will occur. The imageless thought controversy eventually led to a revolutionary crisis in psychology. How did this occur? At first, there was an attempt to assimilate the data, using an explanatory concept (the <u>Aufgabe</u>) which was acceptable to all concerned. It was this proposed solution which itself generated the crisis by implicitly calling into question assumptions which were fundamental to the overall model. Titchener charged that the Würzburg _Aufgabe_ led to the commission of a stimulus error. The opposition already accepted the notion that their results were in part determined by their _Aufgabe_ and countered that Titchener's own _Aufgabe_ might be producing a counter error. But this implied that introspection did not result in the observation of that which exists independently in consciousness. Rather, it reveals that which has been placed into consciousness by the intent of the observational act, and this seemed scientificially unacceptable. In sum, the controversy led to crisis, not necessarily because of anything inherent in the anomalous data, but because of the nature of a proposed solution which was, in principle, acceptable to all. Everyone was willing to accept the notion that the content of an introspective report was in part determined by an _Aufgabe_ which might be operating below the level of consciousness. The disputes centered on whether or not particular _Aufgaben_ could be labeled as errors.

This also seems to have been what happened in response to the anomalous reaction time data. First, the attempt was made to assimilate the new data by proposing that the act of psychological preparation for the response shortened the duration of the reaction. This proposed solution seems to have been accepted prior to Külpe's discussion of its implications for the reaction time paradigm. These, however, are only two instances. Whether this is the general pattern of cases in which anomaly generates a revolutionary crisis must await further historical studies.

FOOTNOTES TO CHAPTER V

[1]Edward F. Buchner, "Psychological Progress in 1906," Psychological Bulletin 4 (1907): 7-8.

[2]The Baldwin-Titchener controversy over the significance of individual differences in reaction time experiments certainly involved the attitudinal differences between functionalists and structuralists, but it seems to have been a result, rather than a cause, of the difference in attitude.

[3]Carl Murchison, ed., Psychologies of 1925 (Worcester, Mass.:Clark University, 1927); Idem., Psychologies of 1930 (Worcester, Mass.: Clark University, 1930).

[4]A. P. Weiss, "Relation between Structural and Behavior Psychology," Psychological Review 24 (1917): 316.

CHAPTER VI

SUMMARY

Thomas Kuhn's Structure of Scientific Revolutions has provided a model for research in the history of science. According to this model, a scientific field becomes a mature, independent discipline when a substantial number of its practitioners are united around a common paradigm. A paradigm may be defined as a contentual model of the scientific universe, including theories, laws, beliefs, values, commitments, assumptions, procedures and techniques, which are shared by a scientific community. The paradigm defines the field of study and the entities which comprise the field. It also provides problems or puzzles for the field to solve.

During the course of "normal" puzzle-solving scientific activity, anomalies, or phenomena which fail to conform to paradigmatic expectations, may arise. If one or more anomalies are seen as important enough by the field's leading practitioners, and if they resist assimilation to the existing paradigm, the field may enter into a stage of crisis, in which the existing paradigm breaks down and normal puzzle-solving activity is replaced by conflict between competing schools. The crisis may eventually be resolved by a scientific revolution, in which the old paradigm is replaced by a new model.

The work of Wilhelm Wundt toward the end of the nineteenth century provided the first paradigm for experimental psychology. This paradigm, which may be labeled "mentalism," consisted of a definition of psychology as the science of mind, introspection as a method of directly observing mental entities, and behavioral observation as an indirect method of observing mental activity. The mentalist paradigm also provided a substantial degree of agreement with regard to the basic sub-topics to be covered by the field, as well as a body of accepted procedures, facts, theories and puzzles to be solved. Differences between competing articulations of the mentalist paradigm, such as the disputes between the structuralists and the functionalists, were subordinate to this core of agreement and were more concerned with differences in emphasis and areas of interest.

During the first decade of the twentieth century, a crisis developed within the mentalist paradigm. The anomaly which led to this crisis was the inability of introspective observation to resolve the differences of opinion regarding the existence of imageless thought. It became generally accepted that these differences in the results of introspective observations were due to the operation of partially non-conscious determining tendencies, or *Aufgaben*. An implication of this conclusion was that introspection could not be depended upon to reliably reveal the contents of consciousness, and that therefore, many of the disputed questions concerning the nature of mental processes were not only unsolved, but also unsolvable. It was, in part, this latter conclusion which made possible the victory of behaviorism as an alternative paradigm to mentalism.

It is concluded that the Kuhnian model of scientific development may be supported by an examination of the early history of experimental psychology. As predicted by the model, the field coalesced around a common paradigm. Puzzle-solving work within the paradigm led to an anomaly-induced crisis, which was resolved by the old model's replacement by a new paradigm. In this instance, however, the anomaly led to a general crisis for the field only as a result of the implications of an agreed upon conception of the cause of the anomaly.

APPENDIX

CRITICS OF THE KUHNIAN MODEL

During the past decade, Kuhn's theory of scientific development has become a "hot" topic within the philosophy of science. So as not to disrupt the flow of this dissertation, I have reserved discussion of some of the current debates over Kuhn's thesis to this appendix.

The recent debates have involved a number of points. Some critics object to the idea of the theory-laden nature of observation; others have suggested that while observation is indeed theory-laden, Kuhn is mistaken in his distinction between normal and extraordinary scientific activity, his implicit approval of normal science activity, or his contention that competing paradigms are not fully commensurable. In face, the only point of agreement between Kuhn's critics seems to be a common recognition of the importance of Kuhn's theses. For example, Dudley Shapere, who is one of Kuhn's harshest critics, considers his monograph "original and richly suggestive... it is bound to have a very wide influence among philosophers and historians of science alike."[1] Feyerabend writes that he has "looked at science in a new way"[2] as a result of his exposure to Kuhn's ideas, and Masterman, prior to chiding Kuhn for using the term paradigm in 21 different senses, refers to him as "one of the outstanding philosophers of science of our time."[3]

Shapere's criticism represents the most consistent defense of more traditional approaches and centers on an attack on the notion of paradigmatic observation, a concept which has already been defended. In any case, Shapere attacks the idea only in a very oblique manner, despite the fact that it appears to be his major reason for rejecting the concept of paradigms entirely. He is concerned lest a preoccupation with discontinuity and meaning variance lead one to ignore continuities and points of contact between competing models. However, given an intellectual climate in which modal conceptions emphasize continuity and incrimental accumulation to the exclusion of discontinuous leaps in the growth of scientific knowledge, Shapere's implicit recognition of the existence of discontinuity can only serve to magnify the importance of the historicist thesis.

Shapere's concern seems to center around the historical relativism which he feels is implicit in Kuhn's position. If competing paradigms are indeed incommensurable, he asks, then "how can we say that 'progress' is made when one paradigm replaces another?"[4] However, Shapere is able to press this attack only by ignoring most of Kuhn's concluding chapter, titled "Progress through

Revolutions," in which Kuhn not only defended the notion of scientific progress, but also attempted to present some of the standards by which the triumph of one paradigm over its predecessor may be considered progressive. In addition to resolving those critical problems which have led the field into crisis, new paradigms "preserve a great deal of the most concrete parts of past achievement and they always permit additional concrete problem-solutions besides."[5] Thus, progress may be found in terms of increased number and precision of problem-solutions.

Another point of contention involves the question of the extent to which competing paradigms are commensurable. If theories embedded in different assumptive contexts may be thought of as representing different observation languages, as Feyerabend's notion of meaning variance implies, then commensurability necessitates either the all but abandoned empiricist dream of a neutral language or translatability. Karl Popper argues that translation between theories is possible since "even totally different languages (like English and Hopi, or Chinese) are not untranslatable."[6] Had Popper written "not <u>totally</u> untranslatable," he and Kuhn and Feyerabend would have been in agreement, for the point being made by the latter philosophers is simply that something is always lost in the process of translation, despite the skill of the translator, as may be seen, for example, in the difference in meaning between Freud's concept of repression and the S-R translation of the concept offered by Dollard and Miller or in Berlyne's attempt at transforming Piaget's concept of disequilibrium among cognitive structures into a Hullian type drive state.[7]

In practice, the question of commensurability is whether or not paradigm choice can be accomplished by means of a "critical test" or "crucial experiment." Watkins and Popper maintain that such a test is always possible and support their argument by merely pointing to a number of such tests in the history of science.[8] It is interesting that the incidents are merely pointed to by these authors as evidence so obvious that discussion of their details would be superflous. The historicists, on the other hand, discuss these same and similar incidents in the history of science as evidence for the impossiblity of critical tests, thus inadvertantly providing yet another interesting example of the effect of theory on the perception of data.

The idea of a critical test or crucial experiment may be attacked on many grounds. In addition to the lack of a neutral or objective observation language, there exists the possibility of alternate interpretations of explanations of the results of any experiment, or the general presence of unchallenged implicit assumptions which are necessary to conclusions that may be drawn from the data in question. For example, arguments based on visual evidence supporting the theory of the earth as spherical, as presented by both Copernicus and Columbus, depended on the unstated assumption that light travels in straight lines and Galileo's alleged demonstration of his law of free fall (it appears questionable whether this oft-cited experiment was ever actually performed) involves an assumption that the perception of simultaneity is independent of the position of the observer.[9] While neither of these underlying assumptions were questioned at the time, Einsteinian relativity theory now justifies such a challenge. In a similar vein, Imre Lakatos notes that any given state of affairs is allowable within "the most admired scientific theories,"[10] unless all factors, some of which may remain hidden and inaccessible, are held constant. Finally, it may be seen that any data can at best constitute support, as opposed to proof, of a given theory, while being at worst an as yet unexplainable anomaly, if not merely an unsolved puzzle, for its competitor. Some anomalous data generally exists for any theory in any field at some points in time, and most of them, rather than leading to a profound crisis, are eventually assimilable within the theory. The existence of unsolved puzzles, on the other hand, is not only characteristic of all scientific paradigms, but, in fact, is a necessary precondition for continuing work within the model.

While the possibility of critical tests may be debated in principle, the actual historical record of the role of such supposed tests in paradigm shifts is more germain. Even if genuine objective crucial tests were possible, that fact would tell us little about the actual process of the growth of scientific knowledge if such tests were not the basis for such shifts in practice. Lakatos argues convincingly that so-called critical tests acquire their generally accepted significance only through hindsight, after the old paradigm had been rejected by the scientific community. He notes, for example, the existence of numerous eighteenth century experiments which at the time were taken as critical tests disproving Newton's theory of gravitation. On the other hand, Galileo's astronomical evidence depended on his interpretation of what was seen through his telescope (he insisted that only his own instrument be used and even then replication was not always successful) at a time when there existed no theory of optics which could justify inferences from what was seen through a lens and what existed on the other side of the lens. As a result, Galileo's telescopic evidence was not generally accepted at the time.[11] Finally, if so-called crucial experiments do indeed provide an objective basis for assessing the worth of competing theories, then one must marvel at the blindness of the preponderance of leading pre-shift scientists who are not won over to the new view. As Kuhn has noted, new paradigms generally come to dominate a field only when defenders of the old view die out or are read out of the profession by their young successors.

A third point of contention concerns Kuhn's distinction between normal and revolutionary science. Stephen Toulmin, while recognizing the value of Kuhn's emphasis on conceptual discontinuities in scientific development, maintains that normal science, as Kuhn describes it, does not exist; that the distinction between revolution and stability in scientific thought, as in politics, is one of degree rather than of quality. "Conceptual incongruities...do introduce real discontinuities, [but] on a small enough scale, indeed, they are very frequent indeed."[12] Therefore, what is generally recognized as major scientific revolutions are merely larger "units of variation."

Toulmin's argument misses two important points. First, it must be as clear to Toulmin as it is to any practitioner of a discipline, that not every piece of research involves conceptual incongruities with the tradition from which it emanates. There is also a very important sense of continuity within a scientific field. Second, as Hegel suggests, it is generally the case that that which at one level may legitimately be seen as a difference in quality, may on another level be reducible to purely quantitative changes. The transmutation of elements in contemporary physics provides one example of the Hegelian transformation of quantity into quality. That transmutation is accomplished through a purely quantitative process, yet this fact does not prevent us from considering different elements as belonging to different qualitative catagories. Similarly, commonalities between revolutionary and non-revolutionary periods in socio-political development has not prevented us from identifying a Russian or French revolution and distinguishing between those events and periods of relative stability. Also, the distinction of levels of paradigms, and therefore of revolutions as well, should not be overlooked. As I have suggested earlier, what constitutes a conceptual revolution for a microparadigm may constitute only a normal puzzle solution for the macroparadigm which encompasses it (see chapter 2).

Paul Feyerabend's dispute with Kuhn involves less fundamental differences than those discussed above. He agrees with Kuhn on the inseparability of observation and theory, the incommensurability of competing paradigms, and the existence of both normal and extraordinary scientific activity. He argues, however, that while both normal and extraordinary research do exist, they are coextensive rather than sequential, even on a given level.

This he claims, is a dialectical materialist approach to the growth of knowledge, akin to the social historical analyses of Marx, Engles, Lenin and Trotsky.[13] In this instance, Feyerabend's identification with the Marxist scholars is mistaken. His approach leads him to consider the major conceptual transformations of scientific history not as predictable consequences of the breakdown of old models, but rather as inexplicable events dependent on various "accidental" factors.[14] Nothing could be further from Marxist conception of revolutionary change. It is, rather, Kuhn's analysis of conceptual discontinuity as the lawful product of progressive contradiction and conflict, predictably emerging from normal development, which seems patterned after the Marxist analysis of socio-political transformations.

Of greater cogency is Feyerabend's critique of Kuhn's normative prescriptions. He notes that Kuhn not only describes normal puzzle solving research as characteristic of protracted periods of scientific development, but also implies that this is the type of work that ought to be done.[15] Later, Kuhn admits to this normative bias, suggesting that during normal periods, the development of competing models ought to be discouraged.[16] While Feyerabend's alternative is overly anarchistic ("there is no need to supress even the most outlandish product of the human brain. Everyone may follow his inclinations"[17]) and certainly is not, as he would have us believe, a description of actual scientific practice, he is correct in calling to attention to Kuhn's failure to recognize the positive role played by the premature suggestion of theoretical alternatives. Alternative approaches have often been advanced in normal periods. Though generally ignored at the time of presentation, they have often been adopted during a subsequent crisis. The notion of the earth as a rotating sphere, for example, was first suggested in 350 B.C., but it was not accepted until almost 2000 years later, when the Copernican resolution of a serious crisis in astronomy made that assumption necessary. In other cases, the interval of delay has been considerably shorter. Arrhenius first suggested that ions were electrically charged atoms as part of his doctoral dissertation, a suggestion which almost cost him his degree. By 1903, the electron had been discovered and Arrhenius' almost rejected dissertation earned him a Nobel Prize.[18] Such premature conceptualizations may help pave the way for the resolution of future crises, just as normal science activity plays a crucial role by creating the crisis which is necessary for the revolutionary replacement of one model by another. Here again, the fact that all the basic tenets of behaviorism had been suggested prior to Watson's manifesto is instructive. Later, Watson was to acknowledge the influential role of many of these precursors.[19]

These are only a few of the questions raised by various philosophers of science. To discuss all of them would require another work, at least the size of this one, and one would probably be safe in assuming that by the time that work had been finished, a host of new questions would have been raised by various writers. That, after all, is the method of inquiry of philosophy.

There is, however, another method of inquiry. We might, as I have done in this dissertation, tentatively adopt Kuhn's model, with or without minor revisions, as a paradigm for historical investigation. If Kuhn is right, then in so doing we will eventually face an anomaly induced crisis which will lead us (or our successors) to conclude that Kuhn is wrong. This is the kind of paradox which we must allow in viewing all useful scientific theories if we adopt Kuhn's point of view. It is a kind of self-reflexive paradox which I think we must be willing to live with, similar to that which ensues once we decide that nothing is absolute (is that "absolutely" true?) or in adopting the existential thesis that the universe is absurd (a thesis which must itself then be absurd). If, on the other hand, Kuhn is wrong and the standard view of science is valid, then our procedure will uncover data which will disconfirm the Kuhnian hypothesis. In either case, the procedure is legitimate as a scientific exploration of Kuhn's model.

FOOTNOTES TO APPENDIX

[1]Dudley Shapere, "The Structure of Scientific Revolutions," Philosophical Review 73 (1964): 383.

[2]"Consolations," p. 197.

[3]p. 59.

[4]p. 391.

[5]p. 169.

[6]Karl R. Pepper, "Normal Science and its Dangers," in Lakatos and Musgrave, p. 56.

[7]John Dollard and Neal E. Miller, Personality and Psychotherapy: An Analysis in Terms of Learning, Thinking and Culture (New York: McGraw-Hill, 1950); D. E. Berlyne, "Recent Developments in Piaget's Work," British Journal of Educational Psychology 27 (1957): 1-12.

[8]John Watkins, "Against 'Normal Science'," in Lakatos and Musgrave, p. 36; Popper, p. 57.

[9]I. M. Copi, Introduction to Logic (New York: MacMillan, 1953).

[10]"Falsification and the Methodology of Scientific Research Programmes," in Lakatos and Musgrave, P. 100, emphasis in original.

[11]Ibid., pp. 98, 173-174.

[12]"Does the Distinction Between Normal and Revolutionary Science Hold Water?" in Lakatos and Musgrave, pp. 44-45.

[13]"Consolations," p. 211.

[14]"The normal elements...may change because the younger generation cannot be bothered to follow their elders; or because some public figure has changed his mind; or because some influential member of the establishment has died and has failed (perhaps because of his suspicious nature) to leave behind a strong and influential school, or because a powerful and non-scientific

institution pushes thought in a definite direction. Revolutions, then, are the outward manifestation of a change of the normal component that cannot be accounted for in any reasonable fashion" (Ibid., p. 214).

[15]Ibid., pp. 198-199.

[16]"Reflections on my Critics," in Lakatos and Musgrave, p. 233.

[17]"Consolations," p. 210.

[18]Isaac Asimov, Asimov's Guide to Science (New York: Basic Books, 1960), pp. 179-180.

[19]Burnham, p. 147.

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