

Is a Scientific Revolution Taking Place in Psychology?

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I make no pretence of being a philosopher of science, nor even a historian of psychology. I venture the following only by virtue of having made some observations while being intimately involved in a particular phase of the history of psychology, and I find those observations closely related to at least one important analysis of the manner in which science develops. The non-American psychologist may well wonder at the end what all the fuss is about, since behaviourism has never been as strong on the Eastern side of the Atlantic Ocean as it has been on mine, where Guthrie, Tolman, Hull, Spence, and Skinner (to name but a few of the outstanding behaviourists) have held forth as descendants of John B. Watson.

My plan in this paper is to try to apply to the field of experimental psychology¹ the analysis of the characteristics of science (and, more particu-

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¹ The specification of experimental psychology may encompass too large a portion of the discipline for what follows. It might have been more appropriate to limit the title of this paper to that branch of experimental psychology primarily concerned with learning, but the influence of behaviourism seems to me to be much broader. While there are notable individual exceptions to the thesis which follows, the influence of several of these scientists has been minimal with respect to the paradigm, presumably because no crisis has been evident in the field. The work of Piaget, for example, has not markedly influenced experimental psychology until recently. Kurt Lewin specifically attempted to counter behaviourism, but failed. After his untimely death child psychologists for a while actually moved closer to behaviourism, rather than in the direction Lewin espoused. In any case, clinical psychology, which seems to be in a pre-paradigmatic stage of development, is excluded from present consideration.

larly, the structure of scientific revolutions) as conceptualized by T. S. Kuhn.² Kuhn has not been accepted without criticism,³ but that need not prevent us from using his book as a point of departure for discussion. I assume that the reader will be familiar with Kuhn's analysis, so that only a sketchy summary of his position is necessary. I will then take a brief look at some of the history of psychology, with special emphasis upon the major tenets of behaviourism. Finally, I will advance some evidence to suggest that we may well be living in an era of revolution within psychology (as well as without).

Kuhn argues that periodically within the individual disciplines of science there are *revolutions*. Revolutions are occasions when *normal science* is disrupted by the process of discarding one *paradigm* for another. At such times the traditionally conceived endeavours of the scientist are diverted, in part, to concerns about the nature of the discipline *per se*, and about how the efforts within the discipline should be expended.

Briefly, 'paradigm' refers to the consensually agreed upon *modus operandi* of a mature scientific discipline. It consists of the conceptions of the nature of the theory to be used in guiding research, the types of problems worthy of investigation, the research methods appropriate to investigating those problems, and even, on occasion, the instrumentation which is required. These conceptions are the rules for playing the game of science; they are formed more by common law procedures than by fiat. They determine the way in which the world of that discipline is viewed, and make it difficult for alternative conceptions to be considered.

Acceptance of a paradigm allows the scientists of a discipline to direct their efforts towards normal science, which involves actualizing the promises of the paradigm by agreed means of approaching the goals. Those efforts consist of designing and conducting experiments which will determine significant facts. The facts, in turn, are matched with the theory. The theory, finally, is articulated in more detail. These enterprises are highly cumulative in the sense in which many people conceive of science as a search for more and more empirical information about a circumscribed area. It is a search to solve puzzles, but most of the answers are known in advance since the

² T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962).

³ See for example C. C. Gillispie's review of Kuhn, *op. cit.*, in *Science*, 138 (1962), 1251-3; D. Shapere's review in *Philosophical Review*, 73 (1964), 383-94; S. Toulmin, 'Conceptual Revolutions in Science', *Synthese*, 17 (1967), 75-91; J. W. N. Watkins, 'Against "Normal" Science', in I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge* (Cambridge, 1970); and D. C. Bloor's review of Lakatos and Musgrave, *op. cit.*, in *Science Studies*, 1 (1971), 101-15.

theory largely specifies the outcome. Experiments resulting in anomalies which do not support the theory are generally considered inadequately designed; efforts are then directed towards the discovery of what went wrong, and to the subsequent design of more satisfactory experiments which do demonstrate support for the theory.

There come times in the history of every discipline, however, when anomalous results are either dramatically unexpected, or accumulate to the point where normal science can no longer either ignore the anomalies or force the anomalous results into the current preconceived theory of what the world is like. It is at such times that crises arise and extraordinary investigations occur which may lead to a new set of commitments; that is to say, crises may result in revolutions.

Times of crisis are distinguished by the recognition that something is wrong with the disciplinary paradigm. The continual failure of experiments to produce predicted results becomes recognized as not just another puzzle, to be solved by experimental and theoretical adjustments. Instead, it becomes clear to some of those involved that the current paradigm is simply inadequate. Some scientists then begin to reconsider and re-evaluate the paradigm itself, and cast about for fresh approaches to the problems facing them. At these times in the history of a science there is a blurring and loosening of the paradigm rules. Research is directed not just to the demonstration of the theory (as in times of normal science) but to the testing of the paradigm itself; that is, to the testing of assumptions which are an inherent part of the paradigm.

At times of crisis, there is inevitably resistance to paradigm change. The resistance takes the form of a defence of the old paradigm, and efforts to patch it up so that it will take account of the anomalous results. Heated debates develop about values relating to research methods, the significant problems to investigate, and the way in which the discipline should be conceptualized, with respect to its scope and philosophical foundations. Arguments take the form of showing how a new paradigm can account for the problems which caused the crisis; the success of the new paradigm in accounting for problems not even considered under the old paradigm; the promise of the new paradigm for yet other problems; the quantitative precision of the new paradigm; and the aesthetic characteristics of the new paradigm. The revolution succeeds in so far as there is first a crisis, so that members of the discipline are concerned enough to consider alternatives; the teachers convince their students to accept the new paradigm; and the adherents of the old paradigm die, so that resistance subsides.

Once a revolution has succeeded, and the new paradigm has achieved the status of consensual acceptance, then the discipline settles down to normal science once again. It is clear that the disciplinary reorientation which results from a paradigm shift will lead to some discontinuity between the normal science which preceded the shift and that which follows it. The cumulative nature of normal science is disrupted by such an event, although there is always some overlap between what went before and what is subsequently relevant. In particular, at least some portion of the problems which caused the crisis must be satisfactorily handled by the new paradigm, despite the fact that these problems will be conceptualized in quite different ways. At the same time, portions of what may have seemed important problems before the shift will be of little interest to those working in the new paradigm. Thus, normal science, following a paradigm shift, sets about articulating and extending the new paradigm; it attempts to fulfil the promises of the new paradigm by doing research the results of which are once again prefigured in advance, as a function of knowing the new theory which is basic to the paradigm.

Kuhn has not dealt with psychology in his discussions or examples of normal science, paradigms and revolutions. In fact, he does not seem to be sure of the status of the social sciences in general, for he has implied in some of his writings that the social sciences have not yet achieved the maturity of a paradigmatic science, while he has suggested in at least one place that 'this century appears to be characterized by the emergence of a first consensus in parts of a few of the social sciences'.⁴ I would like to argue here that experimental psychology has had two paradigms already, with the appropriate scientific revolution between them.

It certainly does not appear that the shift from the 'armchair' psychology of the British associationists to the ensuing experimental psychology of the German tradition would be considered a revolution in the sense in which Kuhn uses the term. It is, perhaps more appropriately, viewed as the emergence of psychology as a science. The British associationists did not have a scientific paradigm, and I think few scientific historians would argue that psychology was a science, in the traditional sense, at that stage in its development.

It does seem, however, that one might argue that psychology during the period of the late nineteenth and early twentieth centuries could be con-

⁴ T. S. Kuhn, 'The Essential Tension: Tradition and Innovation in Scientific Research', in C. W. Taylor and F. Barron (eds.), *Scientific Creativity: its Recognition and Development* (New York, 1963), 341-54, esp. 347.

sidered to have had a paradigm. In fact, Wundt and his followers made the paradigm clear in their writings about the science of psychology and its relation to the paradigms of the other sciences. 'The ideas of which psychology seeks to investigate the attributes, are identical with those upon which natural science is based. . . .'⁵ Wundt continues, ' . . . the *natural sciences* . . . concern themselves with the *objects* of experience, thought of as independent of the subject',⁶ while the science of psychology was defined as an examination of 'the whole content of experience in its relations to the subject and also in regard to the attributes which this content derives directly from the subject'.⁷ Psychology was thus considered to study 'immediate experience', while the natural sciences studied 'mediate experience'.

The apprehension of the content of experience was studied through the method of pure introspection—'pure' in the sense that scientific, as opposed to common everyday introspection, required training to ensure impartiality, attention, freshness, and a favourable disposition,⁸ as well as a good short term memory. Thus, introspection was important not only as the paradigmatic method for use in research in that phase of the history of psychology but also as used 'to distinguish it from the observation of physical science, which is inspection, a looking-at; psychological observation has been termed introspection, a looking within'.⁹ While the analysis of immediate experience into elements by Wundt and his students was opposed by 'act psychology' at the time, both agreed upon the method, as indicated by Brentano's statement that ' . . . psychic phenomena are the exclusive objects of inner perception'.¹⁰

Thus, the paradigm of normal science at this time established psychology as a science distinct from other sciences, and it specified a method by which acceptable scientific data were to be collected. It should be noted in addition that the definition and, particularly, the method of psychology also implied other assumptions within the paradigm. Since introspection is a verbal report about the mental elements and processes of the mind, psychology pertained to the study of humans only, since lower animals obviously cannot give such reports. Furthermore, introspection required training defined

⁵ W. Wundt, *Outline of Psychology* (7th edition), trans. C. H. Judd (3rd edition) (Leipzig, 1907), 2.

⁶ *Ibid.*, 3. ⁷ *Ibid.*

⁸ E. B. Titchener, *An Outline of Psychology* (New York, 1902).

⁹ E. B. Titchener, *A Textbook of Psychology* (New York, 1910), 20.

¹⁰ F. Brentano, *Psychologie vom empirische Standpunkte* (1874), trans. E. G. Boring in R. J. Herrnstein and E. G. Boring (eds.), *A Source Book in the History of Psychology* (Cambridge, Mass., 1965), 605.

in such a way that there was little likelihood that human children could be studied with confidence. In essence, therefore, psychology was the study of consciousness—the sensations, feelings, emotions, volitions and ideas of the human adult. While this characterization of the paradigm does not take into account the fact that other methods of research were used, even in Wundt's own laboratory,¹¹ it is certainly not unjustified by Wundt's own writings, and is clearly what led to the first scientific revolution in psychology.

Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness. The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute. The behavior of man, with all of its refinement and complexity, forms only a part of the behaviorist's total scheme of investigation.¹²

This revolutionary statement marks the official launching of behaviourism as the new paradigm for experimental psychology—at least, for American experimental psychology. But it moves us too quickly ahead in the story for, as Kuhn has noted, revolution can only succeed in the presence of a crisis. There seem to have been three major factors which were responsible for the crisis in psychology: the unreliability of the introspective method; the interest in animals and the resultant necessity to anthropomorphize about animal consciousness in order to maintain the old paradigm; and the interest in the extension of psychological principles to practical application.

In the first case it had become painfully evident that the requirement of impartiality in the trained introspecting subject could not be maintained. Each laboratory found, in the introspective reports of its own subjects, the kinds of data which the scientist in that laboratory was looking for to support his theoretical conception of the contents of consciousness. This, of course, is not out of line with the typical efforts of normal science, but the fact that experiments in one laboratory could not be replicated in others made the procedure suspect. Watson put it boldly:

If you fail to reproduce my findings, it is not due to some fault in your apparatus or in the control of your stimulus, but it is due to the fact that your intro-

¹¹ J. C. Marshall, review of mentalism and objectivism in linguistics, 'The Sources of Leonard Bloomfield's Psychology of Language', *Semantica* (in the press).

¹² J. B. Watson, 'Psychology as the Behaviorist Views It', *Psychological Review*, 20 (1913), 158–77, esp. 158.

spection is untrained. The attack is made upon the observer and not upon the experimental conditions. . . . Take the case of sensation. A sensation is defined in terms of its attributes. One psychologist will state with readiness that the attributes of a visual sensation are *quality*, *extension*, *duration*, and *intensity*. Another will add *clearness*. Still another that of *order*. I doubt if any one psychologist can draw up a set of statements describing what he means by sensation which will be agreed to by three other psychologists of different training.¹³

Watson did not wish to rule out the study of verbal behaviour, nor even introspective reports, a position which he has often been accused of holding. Verbalizations are behaviours in the same sense as any others and, therefore, subject to study. The difference, clearly, is that in the former case the verbal report was the method, and in the latter case it was part of the behaviour to be studied.

The second important factor in the crisis relates to the study of animal behaviour. Darwinian theory, of course, led some psychologists to the study of animal behaviour. Since evolutionary theory argued for the continuity of the species, there seemed every reason to suppose that animals, too, might evidence some aspects of consciousness or mind. Romanes was one of the early comparative psychologists and, operating within the paradigm, he began with the premise that psychology, whether of lower animals or man, is the study of the mind. Since animals cannot introspect,

. . . we can only *infer* the existence and the nature of thoughts and feelings from the activities of the organisms which appear to exhibit them . . . Starting from what I know subjectively of the operations of my own individual mind, and the activities which in my own organism they prompt, I proceed by analogy to infer from the observable activities of other organisms what are the mental operations that underlie them.¹⁴

Continuing within the tradition of the paradigm, Morgan argued that 'systematic training in introspective or subjective analysis and induction'¹⁵ is as essential for the comparative psychologist as for the student of human psychology. He took the first step towards greater objectivity, however, in the statement of his famous canon: 'In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale.'¹⁶ Gradually psychologists studying animal behaviour

¹³ *Ibid.*, 163-4.

¹⁴ G. J. Romanes, *Animal Intelligence* (London, 1882), 12.

¹⁵ C. L. Morgan, *An Introduction to Comparative Psychology* (London, 1894), 50.

¹⁶ *Ibid.*, 53.

seemed to become more and more aware that they were moving away from inferences about mental states or processes of consciousness, and towards the description of animal responses in relation to the experimentally presented stimuli. Experiments were dealing with associative processes of memory, discrimination learning and perception. It seemed unnecessary to bring in consciousness to describe or account for the results. This happened so quickly that only thirty-one years after Romanes' book appeared in 1882, Watson was able to say :

Surely this doctrine which calls for an analogical interpretation of all behavior data may be shown to be false : the position that the standing of an observation upon behavior is determined by its fruitfulness in yielding results which are interpretable only in the narrow realm of (really human) consciousness. . . . One can assume either the presence or absence of consciousness anywhere in the phylogenetic scale without affecting the problems of behavior by one jot or one tittle; and without influencing in any way the mode of experimental attack upon them.¹⁷

Thus, animal psychologists embraced behaviourism because it allowed them to proceed unencumbered by consciousness. Similarly, psychologists interested in child behaviour were able to attack the problems at hand under the behaviourist umbrella.

Finally, in the American pragmatic fashion, the psychologists in the United States, in particular, were interested in things practical. The impetus for this aspect of behaviourism came from the functionalist school of psychology initiated by James, and carried on by Angell within psychology, and Dewey in education. James emphasized the usefulness of consciousness in achieving the ends which allow the survival of the organism. Angell, influenced by Dewey, placed greater emphasis upon the desire to 'portray the typical *operations* of consciousness under actual life conditions . . .',¹⁸ and the extension of the principles derived to the problems of pedagogy and mental hygiene. Thus, functionalism preceded behaviourism in beginning to break the ties with the old paradigm, but functionalism was more of an effort to patch up the old paradigm than to break away from it. Introspection was not rejected but supplemented; and the analysis of the elements of consciousness and their combination was not rejected—instead, the emphasis was shifted to the analysis of how and why conscious processes operate as they do. Functionalism, however, found the paradigm of the

¹⁷ Watson, *op. cit.*, 160–1.

¹⁸ J. R. Angell, 'The Province of Functional Psychology', *Psychological Review*, 14 (1907), 61–91, esp. 62–3.

structuralists stultifying in the sense that neither in theory nor in practice could it make any contributions to education or mental health, which were concerns of many American psychologists. Functionalism failed to produce the revolution because it had no strong evangelical spokesman to make its case, nor even anyone who would take the courageous step of cutting the silver cord. It was left to Angell's student Watson to step to the front and boldly state that '... I, as an experimental student, feel that something is wrong with our premises and the types of problems which develop from them'.¹⁹

It should be noted that, in addition to the functionalist school, the crisis also produced *gestalt* theory as another candidate for the future paradigm for psychology. Largely because of Watson's persuasive presentation, however, this rival to behaviourism failed to achieve the allegiance of the majority of scientists within the discipline, and behaviourism became the paradigm of psychology.

True to Kuhn's analysis of the other sciences, psychologists of the old paradigm resisted the new. Titchener continued to espouse structuralism at Cornell and wrote a volume, published posthumously, which continued within that framework.²⁰ In 1920 Watson participated in a symposium at Oxford specifically designed to speak to the behaviourist point of view—in this case, on the topic of the relation between thinking and language. In that symposium only Bartlett and Smith²¹ could find any basis for agreeing with Watson on methodology, and the other three protagonists all made specific statements similar to Thomson's that '... I do not think that there is any reason for giving up introspection'.²² Nafe, writing on structural psychology as late as 1930, argued that while some experimental psychologists of the time thought that there were systematic differences dividing them into factions which were not able to work together, this was not true, and psychologists really were '... a homogeneous group whose interests, problems and methods are similar'.²³ Boring, in the same volume, attempts to encompass functionalism and behaviourism within a patched up structuralism.²⁴

¹⁹ Watson, *op. cit.*, 163.

²⁰ E. B. Titchener, *Systematic Psychology: Prolegomena* (New York, 1929).

²¹ F. C. Bartlett and E. M. Smith, 'Is Thinking Merely the Action of Language Mechanisms?', part 1, *British Journal of Psychology*, 11 (1920), 55–62.

²² G. H. Thomson, *loc. cit.*, note 21, part 2, 63–70, esp. 65.

²³ J. P. Nafe, 'Structural Psychology', in C. Murchison (ed.), *Psychologies of 1930* (Worcester, Mass., 1930), 128–40, esp. 139.

²⁴ E. G. Boring, 'Psychology for Eclectics', *loc. cit.*, note 23.

Despite the opposition, behaviourism did become the new paradigm and rose to its zenith in the 1940s and early 1950s, when the journals were filled with experimental work fulfilling the promises which Watson had foretold. The research efforts of Tolman, Guthrie, Hull and their students marked the period of normal science within the behaviourist paradigm. Data were collected at fever pitch. Different theoretical points of view led to controversy, but all played the game of psychology by essentially the same ground rules: all accepted the behaviourist paradigm, and thought little of questioning its premises.

They were all working upon problems of learning which were considered the central core of psychology. Pavlovian classical conditioning, seized upon early by Watson as an important methodological and analytical tool, was taken as the simplest and most basic form of learning. Instrumental conditioning was also extensively studied as a slightly more complicated first cousin of classical conditioning. Occasionally research ventured into the problems of trial-and-error learning, but this was usually considered to be a bit too complicated.

The study of the basic conditioning unit of learning led to a second tenet of behaviourism, which was that you must build from the simple forms of behaviour to the more complex forms. The argument runs as follows: behaviour is learned; the simplest form of learning is conditioning; all other complex forms of learning derive from conditioning; once we know the basic laws pertaining to simple conditioning, we will be able to study more and more complex forms of behaviour which will involve the laws of conditioning related by various composition rules; the composition laws will be determined once the simple laws are known. In essence, complex behaviour was agreed to be paradigmatically possible but too problematical for that stage in the development of the discipline.

Compatible with this latter premise were the assumptions about working with animals. The behaviour of animals was considered of interest in its own right, but of more importance in the long run was the argument that lower animals are simpler organisms; the research strategy was to build from the simple to the complex. One built from an understanding of animals lower on the phylogenetic scale to animals higher on that scale, with a promissory note to the eventual understanding of human behaviour in all its complexity. Obviously, humans were used in experiments, too. Large numbers of classical conditioning studies were conducted with humans, as well as extensive research dealing with verbal behaviour. The latter (while always a bit outside the mainstream in terms of the construction of learning

theories) was acceptable because the serial and paired-associate learning tasks were also regarded and analysed as forms of conditioning.

This continuum of complexity from the lower animals to man speaks to yet another premise. Behaviourism was anti-emergent. The behaviour of human organisms was considered basically no different from that of the chimpanzee, the dog or the rat. It was accepted that the behaviour of these various species differs in complexity, but not in terms of the basic underlying mechanisms. There was, therefore, no difficulty in relating the behaviour of rats to that of humans, except in so far as one always indicated that it is necessary to take into account more variables in talking about human behaviour.

Since learning was the heart of the subject matter of psychology, and most behaviour was assumed to be learned, another aspect of behaviourism was its anti-nativistic premises. No behaviourist would argue that the genetic characteristics of the organism play no part in its behaviour, but the basic assumption was that one need be little concerned about heredity, because of its relative unimportance. Primary attention should be directed towards the effects of environmental factors on the determination of behaviour. Watson probably took the most extreme position on this point when he said

... the cry of the behaviorist is, 'Give me the baby and my world to bring it up in and I'll make it crawl and walk; I'll make it climb and use its hands in constructing buildings of stone or wood; I'll make it a thief, a gunman, or a dope fiend.' The possibility of shaping in any direction is almost endless.²⁵

Thus, behaviourism left genetics to the geneticist. Much the same attitude was directed towards physiology. While behaviourists were interested in comparative psychology because it provided the opportunity to study principles of behaviour in a less complicated form, they were not basically interested in the physiological differences among animals, nor in physiological behaviour as such within any species. Psychologists limited themselves to overt behaviour and its environmental determinants. They took the position that while physiological correlates of behaviour certainly could be established, that was the task of the physiologists; whatever correlates were found were of little more interest for the laws of behaviour than correlations of any other kind. It is true that Hull attempted to relate some physiological facts to his principles of behaviour,²⁶ but it was generally conceded that one could ignore Hull's physiology without losing any of the power of his psychological theory. Thus, it seemed, as Spence put it, that 'we should be

²⁵ J. B. Watson, *The Ways of Behaviorism* (New York, 1928), 31-2.

²⁶ C. L. Hull, *Principles of Behavior* (New York, 1943).

able to work out the laws governing behavioral changes without having to stop and consider . . . physiological matters'.²⁷

The psychology of behaviourism was the study of stimuli and responses. The laws of behaviour were those which showed the relationships between variations in stimulus input and variations in response output. Since S-R psychology, as it has often been abbreviated, was a reaction against structuralism and its method of introspection, any 'looking within' was rejected. The new empiricism of behaviourism could get along without introspection, and needed to concern itself only with externally defined observable relationships measured by the same techniques as are used by all the other sciences. So characterized, behaviourism implied the additional paradigmatic characteristic that the organism is a passive receiver of stimuli which produce, in a mechanical fashion, particular responses determined by the past history of the organism. It was assumed that no consideration need be given to the contents of the 'black box', nor to the influence which those contents might have upon the incoming stimulus or the outgoing response. The concept of motivation provided for an active organism, but the particular actions were determined by the stimuli present and the past learning of the organism.

The period of normal science within the behaviouristic paradigm produced considerable research which at least partially fulfilled the promises of Watson. The theory of learning was elaborated from the simple classical conditioning model from which it began. Significant variables were isolated and related in mathematical forms to characteristics of the response.²⁸ The theory was extended, at first gradually, to related problems such as transposition²⁹ and then, more broadly, to new areas such as social behaviour³⁰ and personality.³¹

In the midst of this success, however, the inevitable anomalies arose—as

²⁷ K. W. Spence, 'Theoretical Interpretation of Learning', in S. S. Stevens (ed.), *Handbook of Experimental Psychology* (New York, 1951), 725.

²⁸ See for example Hull, *op. cit.*

²⁹ E.g. in K. W. Spence, 'The Differential Response in Animals to Stimuli Varying within a Single Dimension', *Psychological Review*, 44 (1937), 430–44. Transposition is defined as ' . . . a kind of transfer that appears to result from response to relations among stimuli or to patterns of stimulus qualities rather than to absolute qualities of the stimuli. It is exemplified by such phenomena as the recognition of a melody which has been transposed, in the musical sense, from one key to another . . . and by the continuing choice of the larger of two stimulus objects after changes in their absolute sizes.' (H. W. Reese, *The Perception of Stimulus Relations: Discrimination Learning and Transposition* (New York, 1968), 9.)

³⁰ E.g. N. E. Miller and J. Dollard, *Social Learning and Imitation* (New Haven, 1941).

³¹ J. Dollard and N. E. Miller, *Personality and Psychotherapy* (New York, 1950).

Kuhn predicts for all paradigms of science. Just at its zenith, problems began to arise and unpredicted and disturbing experimental results in some areas began to be reported. These problems multiplied; it became more and more difficult to make adjustments in the theory to account for the various results, and individuals became disenchanted with the paradigm. There is now a restlessness among scientists within the discipline. It appears that behaviourism has run into such difficulties that a Kuhnian crisis has arisen. For some time now psychology has been questioning the paradigm of behaviourism.

There are many points in time which might be cited as the beginning of the end (or, at least, a signal that a crisis might be on the horizon), but the appearance of Kuenne's dissertation, under Spence's direction, seems to be a particularly important early experimental anomaly.³² The research dealt with transposition behaviour in children. Children between the ages of thirty and seventy months were first trained to select the larger of two stimuli in a discrimination learning task. Once the learning criterion had been reached, the children were tested for transposition with new pairs of stimuli. The ratio of the difference in size of the members of the pairs of test stimuli was the same as for the training stimuli. The absolute sizes of the various test pairs, however, covered a wide range, and were all larger than the training stimuli. The results indicated that the younger children in the sample exhibited transposition behaviour in the characteristic manner predicted by Spence's theory;³³ but the older children did not show the expected drop in transposition as a function of the size difference between the test stimuli and the training stimuli. The failure to find this 'distance' effect was attributed to the language abilities of the older children. Since the younger children were also highly verbal it was argued that there was a deficiency in the younger children's abilities to use their language to mediate the transposed responses. The argument was of considerable interest and led to a good deal of subsequent research³⁴ and to the postulation of mediational deficiency as an account of a much broader area of behaviours.³⁵ It should be noted, however, that there was nothing within the theory tested by Kuenne, nor

³² M. R. Kuenne, 'Experimental Investigation of the Relation of Language to Transposition Behavior in Young Children', *Journal of Experimental Psychology*, 36 (1946), 471-90. While I have taken the Kuenne experiment as the first significant experimental anomaly, it makes little difference as far as the overall analysis is concerned whether another experiment or set of experiments, earlier or later, is selected.

³³ *Op. cit.*, note 29.

³⁴ E.g. H. H. Kendler and T. S. Kendler, 'Vertical and Horizontal Processes in Problem Solving', *Psychological Review*, 69 (1962), 1-16.

³⁵ H. W. Reese, 'Verbal Mediation as a Function of Age Level', *Psychological Bulletin*, 59 (1962), 502-9.

the account given by Kuenne of her results, which integrated language, or the language deficiency hypothesis, into the theory. In effect, transposition was studied, and when peculiar results were found a set of *ad hoc* hypotheses was developed to account for that particular peculiar result. Subsequent research attempted to extend learning theory so as to include stimuli and responses produced *internally*, but the efforts in this direction have not had the theoretical precision which was characteristic of earlier theory dealing with transposition itself. The theoretical account of human behaviour in reversal and non-reversal discriminations advanced by Kendler and Kendler,³⁶ for example, included many statements which are quite unusual for the behaviourist paradigm, and which reflect a substantial loosening of the paradigm rules; at the same time, they attempt to defend the behaviourist approach to the problems of concept formation. For example, they refer to the human subject's ability to '... automatically ... generate a mediated response that provides the basis for his rapid reversal'.³⁷ They allow that a subject may change his behaviour on the basis of 'logical considerations'.³⁸ In addition, they suggest that there may be '... integration of separate associative chains on the basis of the fact that they "fit into" the subject's ongoing behavioral chains'.³⁹

A similar mediation theory was developed by research workers within the verbal learning area.⁴⁰ The expansion and development of this theory began in part as a result of a study by Bugelski and Sharlock,⁴¹ although earlier research had dealt with the problem⁴² and provided the theoretical tools for the analysis.⁴³ Mediation theory proved a great impetus to further research and helped to hold the paradigm together for some time. The fact that mediated interference cannot be easily demonstrated⁴⁴ and that four stage

³⁶ *Op. cit.*, note 34.

³⁷ *Ibid.*, 11.

³⁸ *Ibid.*, 13.

³⁹ *Ibid.*

⁴⁰ C. N. Cofer and J. P. Foley, Jr., 'Mediated Generalization and the Interpretation of Verbal Behavior: 1, Prolegomena', *Psychological Review*, **49** (1942), 513-40; C. E. Osgood, *Method and Theory in Experimental Psychology* (New York, 1953).

⁴¹ B. R. Bugelski and D. P. Sharlock, 'An Experimental Demonstration of Unconscious Mediated Association', *Journal of Experimental Psychology*, **44** (1952), 334-8.

⁴² H. N. Peters, 'Mediate Association', *Journal of Experimental Psychology*, **18** (1935), 20-48.

⁴³ W. C. Shipley, 'Indirect Conditioning', *Journal of General Psychology*, **12** (1935), 337-57; A. A. Lumsdaine, 'Conditioned Eyelid Responses as Mediating Generalized Finger Reaction', *Psychological Bulletin*, **36** (1939), 650.

⁴⁴ E.g. D. S. Palermo, 'Mediated Association in the Paired-Associate Learning of Children using Heterogeneous and Homogeneous Lists', *Journal of Experimental Psychology*, **71** (1966), 711-17.

mediation is difficult to demonstrate,⁴⁵ along with the problems encountered when the theory was extended to language behaviour,⁴⁶ have led many to become disillusioned with the adequacy of the paradigm.⁴⁷

Even more traditional verbal learning research has not been untouched. Postman, for example, has postulated 'selector mechanisms' and coding processes in connection with the interference theory of forgetting.⁴⁸ These hardly seem to be mechanisms which may easily be discussed in S-R terms and, in fact, Postman makes little effort to do so. Another example of movement away from S-R analyses within this area is the recent theory of verbal discrimination learning advanced by Ekstrand, Wallace and Underwood,⁴⁹ which accounts for performance in this situation in terms of 'rules'. Rules are postulated largely because the typical S-R conditioning model does not work, especially in the various types of transfer studied.

There are many other areas in which anomalies have arisen. Harlow's work on learning to learn or set phenomena⁵⁰ has not fitted well in the usual S-R analysis. Appeals have been made to such notions as the idea that the subject is learning responses to the general stimulus situation which carry over from one task to another, but this seems a loose and weak discursive device which is difficult to fit into the theory. Harlow's work on curiosity (and, more recently, love) has posed additional problems not easily handled within the paradigm.⁵¹

In the area of perception there has also been concern about the behaviour-

⁴⁵ J. J. Jenkins, 'Mediated Associations: Paradigms and Situations', in C. N. Cofer and B. S. Musgrave (eds.), *Verbal Behavior and Learning: Problems and Processes* (New York, 1963).

⁴⁶ E.g. C. E. Osgood, 'On Understanding and Creating Sentences', *American Psychologist*, 18 (1963), 735-51; J. J. Jenkins and D. S. Palermo, 'Mediation Processes and the Acquisition of Linguistic Structure', in U. Bellugi and R. Brown (eds.), 'The Acquisition of Language', *Monographs of the Society for Research in Child Development*, 29 (1964), no. 1, 141-69.

⁴⁷ E.g. R. W. Brown and U. Bellugi, 'Three Processes in the Child's Acquisition of Syntax', *Harvard Educational Review*, 4 (1964), 133-51; J. A. Fodor, 'Could Meaning be an r_m ?', *Journal of Verbal Learning and Verbal Behaviour*, 4 (1965), 73-81; D. S. Palermo, 'Research on Language Acquisition: Do we know where we are going?', in L. R. Goulet and P. B. Baltes (eds.), *Theory and Research in Life-span Developmental Psychology* (New York, 1970).

⁴⁸ L. Postman, 'Does Interference Theory Predict Too Much Forgetting?', *Journal of Verbal Learning and Verbal Behavior*, 2 (1963), 40-8.

⁴⁹ B. R. Ekstrand, W. P. Wallace and B. J. Underwood, 'A Frequency Theory of Verbal-Discrimination Learning', *Psychological Review*, 73 (1966), 566-78.

⁵⁰ H. F. Harlow, 'The Formation of Learning Sets', *Psychological Review*, 56 (1949), 51-65.

⁵¹ H. F. Harlow, 'Motivation as a Factor in the Acquisition of New Responses', in *Current Theory and Research in Motivation: a Symposium* (Lincoln, Nebraska, 1953), 24-49; and his 'The Nature of Love', *American Psychologist*, 13 (1958), 673-85.

ist paradigm, as Riesen⁵² has pointed out in connection with his work on the development of the visual sensory system of animals. Gibson⁵³ has indicated similar feelings about the inadequacies of the paradigm in her recent book on perceptual learning. The research of Hubel and Wiesel⁵⁴ has led to a dramatic reorientation towards the nature of the receptor mechanisms of the visual system which, if nothing else, complicates the simplistic notions of S-R psychology. The work of Posner,⁵⁵ among others, on perception and subsequent recognition of sensory patterns, suggests that the organism imposes a structure upon what it perceives—and that it stores the structure, not necessarily the actual stimulus presented. Research on speech perception⁵⁶ has indicated that the perception of speech is different in kind from perception of other complex acoustic stimuli, which implies particular human receptor capacities for this type of stimulus. The work of Lipsitt and Engen⁵⁷ on the sense of smell, and Bower's results⁵⁸ on visual perception in human infants, all suggest that much less is learned, and much more is built into the organism, than had previously been thought.

Lashley's well-known analysis⁵⁹ of serial sequences of behaviour is yet another example of the questioning of the paradigm. He argued that the typical S-R associative analysis is impossible to apply to motor behaviour because of the time limitations involved. Instead, he proposed that there must be an overall hierarchical organization which integrates the components of a behavioural unit. Incoming stimuli are not imposed upon a static system but integrated into a system which is dynamic and organized. In order to account for and predict such serial behaviour it is necessary to know, in addition to the stimulus input, the characteristics of the internal system and its organization.

⁵² A. H. Riesen, 'Plasticity of Behavior: Psychological Aspects', in H. F. Harlow and C. N. Woolsey (eds.), *Biological and Biochemical Bases of Behavior* (Madison, Wisconsin, 1958).

⁵³ E. J. Gibson, *Principles of Perceptual Learning and Development* (New York, 1969).

⁵⁴ D. H. Hubel and T. N. Wiesel, 'Receptive Fields of Single Neurones in the Cat's Striate Cortex' *Journal of Physiology*, **148** (1959), 574-91.

⁵⁵ M. I. Posner, 'Abstraction and the Process of Recognition', in J. T. Spence and G. Bower (eds.), *Advances in Learning and Motivation* (New York, 1970).

⁵⁶ A. M. Liberman, F. S. Cooper, D. P. Shankweiler and M. Studdert-Kennedy, 'Perception of the Speech Code', *Psychological Review*, **74** (1967), 431-61.

⁵⁷ E.g. L. P. Lipsitt, T. Engen and H. Kaye, 'Developmental Changes in the Olfactory Threshold of the Neonate', *Child Development*, **34** (1963), 371-6.

⁵⁸ E.g. T. G. R. Bower, 'Phenomenal Identity and Form Perception in an Infant', *Perception and Psychophysics*, **2** (1967), 74-6.

⁵⁹ K. S. Lashley, 'The Problem of Serial Order in Behavior', in L. A. Jeffress (ed.), *Cerebral Mechanisms in Behavior* (New York, 1951), 112-36.

The pioneering work of Olds,⁶⁰ which has led to the explosion of work on brain stimulation and related work on the effects of various brain centres on behaviour,⁶¹ has opened up a whole new area which has little relation to the behaviourist paradigm, but which clearly demands a place in the psychological account of behaviour. In some ways related to this work is the recent research on the effects of drugs on behaviour. Obviously, drugs are stimuli to which the organism responds; but no full account of these stimuli can ignore the characteristics, both physiological and psychological, of the organism which receives them.

There are many more examples which could be discussed, including the one-trial learning controversy raised by Rock⁶² and Estes;⁶³ the problems raised by the genetic studies of behaviour;⁶⁴ the studies of imprinting;⁶⁵ the controversy over the theoretical use of intervening variables and hypothetical constructs;⁶⁶ and the resurgence of interest in Piaget, who has been so long ignored by behaviourists.⁶⁷

The study of language and language acquisition, which has led to the new subspeciality of psycholinguistics, is the last example which I will mention. Here the challenge to behaviourism has been, perhaps, the most open and direct. Furthermore, it is here that we find a suggestion of the new paradigm which may replace behaviourism. Although psycholinguistics has a somewhat longer history, the major impetus for this era of investigation began with the appearance of Chomsky's book *Syntactic Structures*.⁶⁸ This book clearly precipitated a scientific revolution within the discipline of linguistics, changing that science from the behaviouristic paradigm structured by Bloomfield⁶⁹ to a rationalist approach with a transformational

⁶⁰ J. Olds and P. Milner, 'Positive Reinforcement Produced by Electrical Stimulation of Septal Area and Other Regions of the Rat Brain', *Journal of Comparative and Physiological Psychology*, 47 (1954), 419-27.

⁶¹ E.g. R. W. Sperry, 'Hemisphere Deconnection and Unity in Conscious Awareness', *American Psychologist*, 23 (1968), 715-22.

⁶² I. Rock, 'The Role of Repetition in Associative Learning', *American Journal of Psychology*, 70 (1957), 186-93.

⁶³ W. K. Estes, B. L. Hopkins and E. J. Crothers, 'All-or-None and Conservation Effects in the Learning and Retention of Paired-Associates', *Journal of Experimental Psychology*, 60 (1960), 329-39.

⁶⁴ E.g. J. Hirsch (ed.), *Behavior-Genetic Analysis* (New York, 1967).

⁶⁵ E.g. E. H. Hess, 'Imprinting', *Science*, 130 (1959), 133-41.

⁶⁶ K. MacCorquodale and P. E. Meehl, 'On a Distinction between Hypothetical Constructs and Intervening Variables', *Psychological Review*, 55 (1948), 95-107.

⁶⁷ E.g. J. H. Flavell, *The Developmental Psychology of Jean Piaget* (Princeton, NJ, 1963).

⁶⁸ N. Chomsky, *Syntactic Structures* (The Hague, 1957).

⁶⁹ Bloomfield was influenced in the formulation of his behaviouristic paradigm by the psychologist A. P. Weiss, who was a strong behaviourist.

generative theory applied to the newly defined subject matter of that field.

Chomsky rebelled against the empiricist, mechanistic paradigm within which he received his training and, because linguistics was in a crisis state at the time, his persuasive arguments rapidly attracted adherents. His revolutionary ideas were very soon forcefully brought to the attention of psychologists when he reviewed Skinner's *Verbal Behavior*,⁷⁰ only two years after the appearance of *Syntactic Structures*. In that review,⁷¹ and in later elaborations upon the position taken there,⁷² he argued that the terms 'stimulus', 'response' and 'reinforcement' may be useful in some experimental contexts with animals, but when extended to human language they lose all precision and, therefore, all usefulness. Furthermore, in principle, such S-R analyses of language behaviour can never adequately account for the acquisition and maintenance of language. In particular, he pointed to the creative aspects of language as the most crucial. He referred specifically to the fact that children as young as five or six years of age may understand and produce an indefinitely large number of utterances which they have never heard before. In addition, he argued that language is species specific, and is free from stimulus control.

Chomsky made two major points in proposing an alternative approach to the psychology of language—or, more broadly, to human cognitive processes in general. The first point is that it seems essential to understand the nature of what is acquired before we can expect to gain any understanding of the processes by which it is acquired. It is in this area that the linguist hopes to make his contribution. It is the goal of linguistics to provide a theory of language which constitutes the formal properties of the grammars of languages, as well as the specification of those grammars. Thus, grammars of particular languages are only special instances of the general theory of language.

The second point follows from the first. The endeavour of the linguist provides the abstract characteristics of the mind of a person who speaks the language. The characterization of the language gives a characterization of the mind of the human who speaks the language. A person who speaks and understands the language in some sense knows the structure of the language which makes it possible for him to comprehend and use the language. It is in this sense that Chomsky is mentalistic.

⁷⁰ B. F. Skinner, *Verbal Behavior* (New York, 1957).

⁷¹ N. Chomsky, review of Skinner, *op. cit.*, *Language*, 35 (1959), 26–58.

⁷² E.g. N. Chomsky, *Aspects of the Theory of Syntax* (Cambridge, Mass., 1965); and his *Language and Mind* (New York, 1968).

It follows from these points, and from the empirical evidence available, that since language has these creative features, and since much of it is acquired in such a very short period by the young child, the human organism must acquire rules which allow him to understand and produce his language. Furthermore, since all languages have a common or universal set of properties, some of these universal properties must be determined by the innate characteristics of the human organism. At least in part, these innate properties of the human make it possible for him to process the noisy input which he receives in such a way that he can determine the rules which apply to his language.

Chomsky, it may be seen, views linguistics as a branch of psychology, but the contribution of the linguist is to provide a grammar which describes the competence of the ideal speaker/listener of the language. It remains another psychological task to develop a theory accounting for the performance of the language user. While the competence model describes the knowledge a person has of his language, there are many additional variables which affect his performance; his ability to hear, his memory, the distractions during speech, noise, physiological state of the organism, and other variables, may all affect his language performance.

While it is not yet clear whether the rationalism of Chomsky will form the basis for a new paradigm in psychology, it does seem to be the case that many psychologists within the psycholinguistics area have been converted from the paradigm of behaviourism to one which has the same general rules for playing the game as those which Chomsky has advocated.⁷³ In addition, there seems to be a good deal in Chomsky's paradigm which should appeal to those working in other areas, and who also sense a crisis.

There seem to be a number of aspects of the old paradigm towards which the revolution is directed. Some of these are overtly and specifically expressed; others are merely a part of the general dissatisfaction. In addition to the belief that the associationistic S-R analysis based upon a Markov chain type of model, even with the elaboration of mediation, cannot account

⁷³ E.g. T. G. Bever, 'Associations to Stimulus-Response Theories of Language', in T. R. Dixon and D. L. Horton (eds.), *Verbal Behavior and General Behavior Theory* (Englewood Cliffs, NJ, 1968), 478-94; M. Garrett and J. A. Fodor, 'Psychological Theories and Linguistic Constructs', *ibid.*, 451-77; D. McNeill, 'On Theories of Language Acquisition', *ibid.*, 406-20; J. J. Jenkins, 'A Challenge to Psychological Theorists', *ibid.*, 538-49; Brown and Bellugi, *op. cit.*, note 47; Palermo, *op. cit.*, note 47; D. I. Slobin, 'The Acquisition of Russian as a Native Language', in F. Smith and G. A. Miller (eds.), *The Genesis of Language: a Psycholinguistic Approach* (Cambridge, Mass., 1966), 129-48; R. J. Wales and J. C. Marshall, 'The Organization of Linguistic Performance', in J. Lyons and R. J. Wales (eds.), *Psycholinguistic Papers* (Edinburgh, 1966), 29-80.

for complex behaviour, there are a number of other relevant considerations. For example, experimental psychologists seem to be unwilling to wait any longer for the complexities of behaviour to be handled by building from simple conditioned S-R units. This promise of behaviourism has become bogged down in the details of simple conditioning, and there is too little evidence that the original faith was justified. Whether the promise can be fulfilled may be a question of fact, but progress has been too slow for those working in the discipline today.

Second, it seems that researchers have been forced to recognize that both lower animals and, particularly, humans do not merely passively receive stimuli and automatically produce resultant responses. The organism is active. It seems to seek stimulation, organize the input in structured ways, and respond in terms of the rules or the structure which it imposes upon materials both externally and internally available. In other words, the organism has a mind which it uses, and which determines behaviour. The mind is not independent of external stimulation, but is not controlled by it. The mind structures and restructures the information available to it, and responds in terms of the transformations it imposes upon this material.

Considerable work, from a variety of sources, has also led to a reconsideration of the anti-nativistic position held so strongly by Watson and his followers. There is too much evidence now to indicate that many things once thought to be learned seem, rather, to be a function of the structure of the organism as genetically determined. The question now seems to be just how much more. Recognizing that many lower animals behave as they do in part because of their structural characteristics, the question extends to how much of human behaviour is also determined in the same way.

Related to the latter point is a reconsideration of the notion of emergence. Is it appropriate to assume that human behaviour is on a continuum with animal behaviour? Perhaps the human is significantly different, and it will not be possible, except in superficial ways, to generalize from lower animals to humans—or, in fact, from one species of lower animal to another. Certainly the arguments presented by Lenneberg⁷⁴ are a challenge to this long-held behaviourist tenet.

Finally, it should be noted that the interest in behavioural genetics, along with the recognition that something within the organism makes a significant difference in determining the behaviour of the organism, leads naturally to an interest in the brain, and other physiological aspects of the organism, in relation to behaviour. An integration of the physiological with the mental

⁷⁴ E. H. Lenneberg, *Biological Foundations of Language* (New York, 1967).

fits nicely into the general framework advanced here, although it may have played a relatively minor role in the initial reasons for formulating the new paradigm.

In conclusion, there seems little doubt that experimental psychology is ripe for a revolution—if it is not, in fact, currently in the midst of one. All of the historical and social elements which Kuhn has described as symptomatic of revolutions in other sciences appear to be present in psychology at the moment. The major question (which this paper cannot resolve, and only history can decide) is whether a new paradigm has been formulated, and psychology is emerging from the crisis. Can a revolution in one science produce a revolution in another science as well, with the same paradigm for both? If Chomsky is correct that linguistics is really a part of psychology, then this question is vacuous; the major question then reduces to whether the paradigm proposed by Chomsky will be taken up by all of experimental psychology, rather than just by those few interested in psycholinguistics. I am inclined to believe that it will not be limited to psycholinguistics, and that the new paradigm for linguistics will be the new paradigm for psychology. The promise of that paradigm seems great, but perhaps it will require someone within psychology who has the charisma of a Watson to take up the banner and carry it home to the rest of psychology, against all the opposition which will fight it to the end.