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THE CONTEMPORARY STATE OF BEHAVIOR THEORY

EL ESTADO ACTUAL DE LA TEORÍA DE LA CONDUCTA

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ABSTRACT

The contemporary state of behavior theory is analyzed, and its development is traced to theories and paradigms for the study of behavior proposed by philosophers and scientists such as Descartes, Sechenov, Pavlov, Darwin and Ebbinghaus among others. It is suggested that the contemporary state of behavior theory is an unstable one, and it cannot be expected to continue that way indefinitely. Its state it not the pre-promotional sort that anticipates simple adjustment in a scientific theory . Rather, it is a transition from long-established concepts and assumptions and dogmas to others that are certain to be radically different. I will demand some difficult and painful shifts in our resistant habits of thought about behavior, in the form and content of our behavior theories.

Key words: behavior theory, behaviorism, current trends

RESUMEN

Se analiza el estado actual de la teoría de la conducta y su desarrollo se rastrea hasta las teorías y paradigmas propuestos por filósofos y científicos como Descartes, Sechenov, Pavlov, Darwin y Ebbinghaus, entre otros. Se sugiere que el estado actual de la teoría de la conducta no es estable y que no se puede esperar que continúe en tal estado indefinidamente. Su estado actual no demanda un simple ajuste sino que es más bien una transición de suposiciones, conceptos y dogmas establecidos por largo tiempo a otros radicalmente diferentes. Esta transición demandará cambios difíciles y dolorosos de nuestras concepciones sobre la conducta, que se reflejarán en la forma y contenido de las nuevas teorías conductuales.

Palabras clave: teoría de la conducta, conductismo, tendencias actuales

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May I say a word at first about how flattered and pleased I am to have been given this honor. It, and this occasion, will always be for me a warm and deeply satisfying memory. I hope that it will also serve to promote a close relation between this University and my own, and that it will help me personally to be effective in urging such a relation.

As you see from my title today, I am venturing to speak as a historian of psychology. I am quite aware of my limitations as a historian, as well as the difficulty of studying the history of any science. But I am led to my theme because my discipline seems to me to be at an important juncture in its historical development, and I am inescapably caught up in its present travail. We who call ourselves behavior theorists must pause today on our accustomed paths of thought and research to re-examine the historical roots and trajectory of our science. Where have we come from? By what roads have we got here? What lies ahead; what direction can we, if at all, anticipate that our science will take in the future? Even more, can we say what direction it *should* take? These are the questions we are being forced to today because behavior theory has not kept up with its empirical discoveries, and the conceptual difficulties that have been accumulating for it over the past half-century. But certain it is that the facts and ideas and assumptions that we have been taking for granted these many years are now less secure and often indefensible. Under such pressures, theorists grow restive. Today, at your invitation, I shall try to be their voice insofar as I can.

I know how risky my venture is, and how presumptuous. To understand our past is to put our intelligence to its fullest test, and we may expect to fail often enough in that effort. To forecast the future of a science is to join foolhardiness to difficulty. And to say what a science's future *should* be is to add what may be merely the historical astigmatism and distorted personal preferences of a would-be visionary. But I must risk all that if I am correct in my initial premise about the contemporary state of behavior theory, to wit, that it is now in a dead end. Not only I, but also those of my colleagues who see the present state as I do, ought not hesitate much longer before taking the same risk. Together we may perhaps help our science into its new paths, or at least keep our future students and colleagues from asking, when they look back upon us, why we in our time did not try to help. We ought not disdain failure, but rather lack of effort; failure to succeed is not blameworthy, but failure of will is. That is the way of science, and so, as I have been touched by the spirit of science, I venture to speak of behavior theory past, present, and future.

As we all know, to speak of history is not merely to speak of the past. History is not alone a record of the past, and the study of history is not a study of that record. The "past" is made up of an indefinitely large number of things, events, facts. The historian has this in common with the scientist: That he needs, and always uses, a guiding idea by which to select facts out of the past, those which he conceives to be important for his stated reasons, and by which he arranges those facts into what he deems to be the significant story of the past. His story is the thread that leads him from the past into the present, and sometimes, albeit more hesitantly, into the future. For the historian, the future will prove the past, or, more strictly, prove his selections from the past, his story, his thread. His thread corresponds to the scientist's "Hypothesis", though his methods and procedures for substantiating it are different from those of the laboratory scientist. So it is with the thread I offer now for your consideration, the one which I believe behavior theory has followed from its past into its contemporary state. As with all matters historical developments are complex; as psychologist, we grant complexity to child development, personality development, and whatever other "developments" we cherish in our academic curricula. We should grant at least comparable complexity to the historical complexities which have produced current behavior theory. The thread I have chosen to follow with you today is a thin one, but I think a correct one. That, however, will be for you and my more distant colleagues, and especially for the future, to judge .

It has been said by others before me that the study of a science's history can be a valuable, even indispensable, analytic method for the scientist. That is, the understanding of a "problem" in science –the question being studied, the assumptions on which it rests and the terms in which it is formulated, the answering data that are sought and the methods for obtaining such data, the interpretations of the data and the implications drawn from them all, all can often, and perhaps only and always, be understood in an historical light. Ernst Mach (1960) was perhaps the most prominent spokesman for this belief in theoretical physics; in psychology, J. R. Kantor (1960, 1963) has spoken out most clearly for the use of history in the analysis of a scientific problem.

Yet, to apply historical methods to a scientific analysis pre-supposes that, both as historians and as scientists, we can really understand what earlier thinkers had in mind, what they understood by the terms they used, what to them were the issues involved, what data they were ready to accept, what "evidence" meant to them, and what were for them proper levels of "explanation", and acceptable "answers" and "reasonable" implications for their next work. It is here that the historian-scientist has his major difficulties, over and above the selectivity he must apply to the facts of the past. And even a "fact," as one philosopher once said, is a fact only within some system: It is that system in the thought of the past scientist whom the historian now is studying which he will have trouble understanding. Or is it the historian's own system which he believes he understands, but may not? Musing over such intricacies, the distinction we are prone to draw in our own time between "science" and "ideology" becomes blurred. That blurring underlies our facile modern pronouncements about the inter-dependencies in any era between scientific and social thought, between politic-economic and philosophic-scientific interests. But blurring alone cannot give rise to good theory. Good theory demands clarification, and that has been in shorter supply among social theorists.

To understand the thinking of the past in all its aspects is probably not possible. When I give my students reading assignments in the history of psychology, or man's thinking about behavior, I warn them that that will not fully grasp what an early Greek or medieval writer, or even an early modern, had in mind. We are simply not familiar with the whole range of meanings of their words, nor do we share the nuances of their expressions. I recall a conversation I once had with a Catholic priest in Brazil who asked me about my field of work; when I told him "psychology," he rejoined that that could not be quite correct since, from my own account, I wasn't studying the "psyche" at all as my science's name said I should be. Of course, he was right. But even so, what do we know of the full meaning-spectrum of the term "psyche" in the hands of Aristotle or any other Greek philosopher: We can only wonder. The problem is akin to that of any translation from one tongue to another, even modern ones. Recall the translator's dilemma in dealing, say, with poetry: Whether to translate literally, or for sense. It is a dilemma never resolved. Add the fact that even the literal meanings or words may have changed, and that the sense is only what he supposes it to be, and we can appreciate why the ancient Talmudist said that all translators are "liars", and why no Islamic authority is willing to accept any translation of the Koran's 8th Century classic Arabic into some modern language.

When reading history, and some man's writings about behavior, I recommend that my students keep a few questions always before them: What was the behavioral observations or problem being looked at, or what was the datum? Second, what were the determining factors or variables that were being postulated or explored? Finally, what were the presuppositions involved in the initial observation and the study which followed; that is, what where the cultural myths in which the man's thoughts were set, or as we might say today, what was his "ideology"? There is perhaps some chance, I tell my students, that they might, with regard to the first two questions, be able in some measure to separate the "objective" from the merely "conventional". I warn them that, although what *we* think of as "objective" is part of our present convention, unless we can make some such separation in our terms, then we, in *our* time, will surely never in *our* language make any contact at all with our predecessors. And, if those difficulties were not enough, they are to consider the pitfalls in my third admonition, that concerning the "myths" prevailing in any given historical period. I led them hear the voice of F. M. Cornford (1931):

> "If we look beneath the surface of philosophic discussion, we find that its course is largely governed by assumptions that are seldom or never mentioned. I mean that groundwork of current conceptions shared by all men of any given culture and never mentioned because it is taken for granted."

and of R. N. Whitehead (1925):

> "When you are criticizing the philosophy of an epoch, do not chiefly direct your attention to those intellectual positions which its exponents feel it necessary explicitly to defend. There will be some fundamental assumptions which adherents of all the various systems within the epoch unconsciously presuppose."

I give my students a challenge as they plunge into the history of psychology, which is to compile a list of their own myths, and of those governing the thought of scientists in our own modern age. It is a difficult challenge to meet because, of course, we are usually unaware of our own myths, and we do not recognize them as myths when others point them out to us. But the list of our own modern myths is a fairly long one, as you will discover for yourselves if you will exercise the intellectual discipline needed to ferret our at least some of them. It is no wonder that, daunted by such difficulties, there are modern scholars who avoid history. Some rationalize their aversion with the argument that things must be dealt with as they are now. If they are psychologists, they argue that forces and events make contact with an organism as it is now, and all that its history can tell us are the origins of its present state; in short, if we knew what the organism's condition is now, we could ignore its history. In a way, of course, that is correct, but the rub is, as even those arguers would admit, that we often have no other knowledge of an organism's present behavioral condition than through its history. It is a bit comical to hear one of our prominent behavior scientists and his followers in the "behavior modification" school roundly reject the importance of a patient's history, while in thinking about his behavior they turn always to his "reinforcement history!"

But the usefulness of an historical analysis of a scientific problem and its current answers has been proved often enough to be beyond doubt. Its usefulness holds at any stage in the history of a science, even though its practitioners and their science at *that* stage are also subject to the same qualifications. That is so because our language at any historical point is the repository of human knowledge at that stage and not of divine absolutes. For *that* level of knowledge and insight, some inkling of what went before is an aid. As T. S. Eliot (1928) put it: "If we can really penetrate the life of another age, one is penetrating the life of one's own" (shall we read "thought" for "life"?), "and it appears that to some extent we can make that penetration (into another intellectual context), or, at least, a penetration which is of help to us in our own time, even if we cannot judge in absolute terms whether we have penetrated truly". That level of knowledge and insight may be the slave of our time, but I am willing to accept it, or perhaps have no choice but to accept it, as the starting point of what I offer for your judgment this morning regarding the contemporary state of behavior theory.

I am afraid that all this has been an overly-long introduction to my offering. I must admit, however, that it permits me to proceed with a calmer, if not a clearer, conscience!

Along my historical thread leading to modern behavior theory, I wish to concentrate on two periods: A few years at the turn of the present century, and the decade of the 1930's. In the former period, the important names will be E. L. Thorndike, I. P. Pavlov, and W. S. Small; in the latter, they will be B. F. Skinner, C. L. Hull, and E. R. Guthrie. I shall also throw a glance still further back into the 19th Century, finding there the influences of I. M. Sechenov, C. Darwin, and H. V. Ebbinghaus; give a briefer intervening glance at the years between the turn of our century and the 1980's, the years where J. B. Watson looms; and give a glance, but only a passing one, at the Aristotelian *obbligato* that has been carried over a good part of our century by J. R. Kantor. Occasionally another name may enter stage, but not in the main cast of characters. None of these names will, I am sure, surprise you as a figure in the history of psychology, but I think they may bee seen in a somewhat unaccustomed light when threaded into an evaluation of the contemporary state of behavior theory.

Let me begin with an apparent digression, though it really is not. There will be others as I go along, but I hope they will not, even all together, obscure the tale I wish to tell.

Someone once said that there is Behaviorism and there is behaviorism, and that between the capital and small letters lies a big difference. There is much reason in that. Every psychologist is a small-letter behaviorist when dealing with the behavior of organisms around him. In our study of behavior we look at behavior because there is nothing else to look at. No one can study non-behavior. Indeed, it was W. McDougall who styled himself a "hormic" psychologist and who would hardly accept designation as a capital-letter Behaviorist, who may have been the first to define our science as the study of behavior (small letter). Capital-letter Behaviorism is quite another matter. It is a philosophy and a program; it is a view of what the science of behavior (small-letter) should take as its business, and what its research methods should be; it is an espousal of certain behavior data, and rejection of certain interpretations of those data; it is the adoption of certain goals as the proper ends of behavior science; it is an attitude about science in general, and certain philosophical and metaphysical systems in particular. It is with capital-letter Behaviorism that the public, and professional psychologist as well, identify the names of I. P. Pavlov and J. B. Watson and, presently, B. F. Skinner. It is capital-letter Behaviorism about which debates and controversies swirl in universities and on village greens. It is capital-letter Behaviorism about which, among scientists and philosophers, even more than among laymen individual ill-feelings arise, and personal friendships founder. All this tumult is seldom accompanied by a back ward glance into the history of the matter, one which, if dispassionate and charitable, might quiet the roiling tempers, and possibly even turn sobered eyes round a bit toward the future.

It has also been said of psychology that it has a long past, but a short history. We might paraphrase this and say rather that small-letter behaviorism has a long past, but capital-letter Behaviorism a short history. Interest in behavior has always existed among common folk; and, among intellectuals, the same interest can easily be traced from Aristotle through the Romans, the Middle Ages, the Renaissance, the early and the recent modern periods. I shall not detain you today with a recital like that. Details of attention to behavior are to be found not only in the sciences and philosophies of those eras, but also in their arts and letters. The behavior realism Brueghel the Elder put into his art; the humor and pathos that Chaucer and Shakespeare and Boccaccio and Rebelais and Balzac put into their characters' sayings, their slips-of-the-tongue, and the like; the counter-posing in such folk sayings as "actions speak louder that words" –all of these are well known to you. Freud, when he could keep his mind off his conceit of a "psychopathology" in every day life, off constructions like "consciousness," and off the rest of this psychiatric paraphernalia, had a sharp eye for the objective realities of behavior. Interest and attention to objective behavioral data did not follow a well-defined cycle over the centuries. They waxed and waned in no regular or explicit way; they simply fluctuated among intellectuals just as among the lay public, with the emphasis shifting between behavior itself on the one hand, and the prevailing myths or philosophies about behavior on the other.

The shifting of emphasis began to take on a more recognizable and cyclical form under the pressure of Descartes' thinking and that of his later followers. Starting from this conceptions of the mechanistic nature of behavior (and ignoring the exception he granted to Man), through the French Revolution and its intellectual upwellings, through Europe's religious Reformation and Counter-Reformation, through the 16th to 19th Centuries when our modern biological sciences were founded and grew so remarkably, from all these channels there finally arose the explicit manifestos which declared the character of modern capital-letter Behaviorism, and which still expose to us the doctrinal allegiances of modern capital-letter Behaviorists. Aside from its come-what-may devotees, this doctrinal Behaviorism has been cycling in prominence among the body politic of behavior scientists. It has had a wave of prominence over the last fifteen or twenty years, and this very day we can see a rising reaction against it. We see the literature once again burgeoning with volumes and papers on "consciousness," with college text books carrying titles like *Psychology: The Science of Mental Life* (Miller, 1962). This, however, can be only a side detail in any discussion of the contemporary state of behavior theory, because capital-letter Behaviorism and behavior theory are not the same thing. The two terms do not fully overlap in the usage of theoretical psychologists: A Behaviorist may be a behavior theorist, but not every behavior theorist need be a Behaviorist. These distinctions may seem to many to be over-refined, and perhaps pointless, but they are important historically. Both the over-lap of these terms and the thinking they represent, as well as the distinctions between them, played their parts in bringing behavior theory to its present state.

Capital-letter Behaviorism in psychology was god-fathered by J. B. Watson. B. F. Skinner, writing "Behaviorism at Fifty" (Skinner, 1963), dated the anniversary from Watson's declaration of 1913 (Watson, 1913). Watson's most influential writings on Behaviorism antedated the 1930's which was the decade which largely defined the characters of contemporary behavior theory. His influence is clearly discernible upon the theorist who, close upon his heels, made the 1930's the most active decade yet in the developmental history of what we call today "behavior theory". Afterwards, his influence became more remote, and he became more an exemplar (forgetting he was its founder) of the scientific partisanship of a capital-letter Behaviorist. He became a butt of jokes (such as he who would "throw out the baby with the bath") even in some scientific circles. Whatever promise he may have had as a source, or as a specific and direct guide for behavioral research, was lost to the behavior theorists who came after him.

Looking back at Watson's place in the history of psychology, we can now see more clearly three things about him. First, he was a polemicist rather than a true theorist. His capital-letter Behaviorism was an avowal of personal viewpoint, a statement of scientific faith, and a programmatic declaration without a real program. His contribution to the behavior theorists of the 1930's and after, was akin to that of Sechenov's to Pavlov: An ardent spokesman for a viewpoint which some theorists were later to adopt and partially absorb into their general behavior theories. It was left to others later to try to implement some of this notions. Second, Watson had taken fire from the prior work of reflexologists like Pavlov, and of biophysicists like Jacques Loeb. In Watson's argument for capital-letter Behaviorism the earlier work of E. L. Thorndike, though known to him, had little place, though Thorndike had already begun to figure large among some psychological writers, and was destined to assume an immense influence upon the theoretical discussions of the 1930's and the subsequent development of behavior theory. Third, Watson was interested in practical control over behavior, particularly human behavior. Control was the thing: Its achievement would be both the pragmatic proof of scientific behavioral principles and the proper goal of scientists who should be concerned with the human condition and man's fate. The outcome of a true understanding of the principles of behavior would be increased control over that behavior, a control which should be fully exercised for social purposes. He accepted the principles which Pavlov and Loeb, among others, had written about, and gladly anticipated the control over behavior which he believed they foreshadowed. The theme of social control loomed even larger for him in his later non-academic career. It is the same theme made notorious in recent years by B. F. Skinner, who in similar fashion took it up after earlier focus on the laboratory.

Three quotations from Watson's writings will illustrate his views. Believing (in parallel with Loeb) that instincts could be rejected as a source of behavioral explanations, and that environmental forces and learning (in parallel with Pavlov) were all-important in the formation and maintenance of behavior, he wrote the often-quoted statement (Watson, 1919):

> Give me a dozen healthy infants, well formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to be any type of specialist I might select–doctor, lawyer, artist, merchant-chief and, yes, even beggarman and thief- regardless of his talents, penchants, tendencies, abilities, vocations and the race of his ancestors.

He believed long before the advent of "signal detection theory" and "animal psychophysics" that even psychophysics could use reflex procedures. They would be alternative to the verbal responses of human subjects which, to his mind, were wrongly interpreted as "subjective" or "introspective." So, for exploring the spectral limits of human color vision, we read (Watson, 1913):

> We start with any intermediate wave length and by the use of electrical shock establish a conditioned reflex. Each time the light appears the reflex occurs. We then increase the wave length. We finally reach a point where the reflex breaks down, even when punishment is used to restore it –approximately at 760 mμ. This wave length represents the human beings' spectral range at the red end. We then follow the same procedure with respect to the violet end (397 mμ). In this way we determine the individual's range, just as surely as if we had monochromatic light varying in wave lengths and asked him if he saw

them.

And on behavioral control, Watson went the whole route from trainer of laboratory rats to that of moral philosopher and social reformer. It is the same route taken later by B. F. Skinner, who by his own preference has acceded to the current public and professional opinion of him as the new father figure or patriarch, of Behaviorism. We read Watson (1924):

> I am trying to dangle a stimulus in front of you, a verbal stimulus which, if acted upon will gradually change this universe. For the universe will change if you bring up your children, not in the freedom of the libertine, but in behavioristic freedom--a freedom which we cannot ever picture in words, so little we know of it. Will not these children in turn, with their better ways of living and thinking replace us as society, and in turn bring up their children in a still more scientific may until the world finally becomes a place fit for human habitation.

So much for Watson's contributions to modern behavior theory. Because they were only polemical, wishfully programmatic, and social-reformist, they are of no great moment for the specific problems that affect contemporary behavior theory. Those problems come more directly from the decade of the 1930's and it is to these, which are more immediate to the title of my talk today, that we must now turn.

The very term "behavior theory" seems to have been born in the 1930's or at least to have taken its present identity and direction in that decade. Of course, there were in that decade more behavior theorists than would agree to call themselves "behaviorists", whether the small or capital letter variety: Among these were Tolman, Köhler, Lewin, McDougall, Freud, and still others. Among those who did agree that they were some variety of "behaviorist" were, of course, Guthrie, Hull, and Skinner; and in this group we must include as equally important, but less visible and less frequently cited, the name of J. R. Kantor. It is to the latter group that the meaning of the term "behavior theorist" as we encounter it today in our journals and textbooks and seminars, is most directly linked. Although Skinner has emerged as the most influential on the contemporary scene, to understand what has become of behavior theory we need to keep our eyes on these four key men even if that involves a certain amount of historical injustice to others. As I said earlier, however, the selections of an historian are always biased, and therefore partially unjust, though also, hopefully, partially correct.

These key men had, I should say, at least the following six formative influences upon their thinking, and upon their empirical research. First, perhaps were the forceful position statements of men with the prestige of H. S. Jennings who was, for a time, carried away by the promise of the aborning science of behavior (Jennings, 1908a):

"In each division (of the science of animal behavior) the slate was, as it were, wiped clean some ten or fifteen years ago; the existing structure was razed to the ground, and we have been building it up again ever since. In the lower organism *Loeb* reduced the phenomena to almost inorganic simplicity. For the ants, bees and other higher invertebrates *Bethe* took similar actions; they were stripped of their fanciful decorations of memory, intelligence, etc., and left absolutely devoid of 'psychic qualities' of any sort; their behavior was composed of invariable reflexes and tropisms of the simplest character. *Thorndike* performed the same operation for the vertebrates. Not only did they not reason (preposterous nation!), but they did not imitate, could not learn by seeing a thing done nor by being put through an act, not by any other way than by simply gradually dropping our useless movements from among those made at random; and they had not even *ideas* of things past, to say nothing of perceiving relations or being capable of trains of thought or of formulating a plan.

In all three divisions of the subject the work since these operations has consisted largely in the slow and painful restoration, by precise experimental methods, of what was thus wiped out at one fell swoop. The three authors named, with those that aided them, perhaps did the *science of behavior the greatest possible service at that time.* Before them where was hardly and ordered science in this subject; there was a jungle of suppositions, assumptions and anecdotes. Loeb, Bethe, Thorndike and Company destroyed all this and compelled us to rebuild from the ground up, a solid structure, based on precise scientific methods."

Even so, though enthusiastic about the future–in which Watson's "Behaviorism" and the "behavior theory" of the 1930's were still to be defined-Jennings ended this declaration with a word of caution:

"...animal behavior as a science is merely in its swaddling clothes... It will be long before our science is coextensive with the phenomena with which it is attempting to deal."

He repeated that caution in that same early year (Jennings, 1908b):

"If anyone attempts to explain all behavior on any one basis, to unlock all its secrets by any catchword whatever, be it 'trial and error', 'selection,' 'tropisms,' or whatnot, he lacks a realization of the complexity of his field of investigation."

Did he have Thorndike in mind when he mentioned "trial and error," and was Pavlov an example he included in his "what not"? What would he have said of the later elaboration of Thorndike's work, and the far-flung applications of "reinforcement" at the hands of our present-day "operant conditioners"?

A second influence on our key men was Aristotle's approach to behavior, and to science generally; that approach is particularly evident in the thought and writings of Kantor, but to expand on this would require an occasion of its own like ours today, and I shall not attempt even a summary of it here (Schoenfeld, 1969).

A third background influence were the polemics of Sechenov and Watson. These, in turn, need to be projected against the history of the reflex concept which was their source and background. So strong is this projection that Skinner was impelled in one of his very first publications to trace what he considered to be history of the reflex concept, and to assert in conclusion his own allegiance to that concept: "From the point of view of scientific method, at least, the description of behavior is adequately embraced by the principle of the reflex" (Skinner, 1931, 1935b, 1937). He confirmed his emphasis on the reflex by the important place he gave to its "static" and "dynamic" laws in which "learning" itself was one variant of the dynamics laws (Skinner, 1938).

Leaving aside the possible reservation hidden in the "at least," Skinner's problem then was, he thought, to rationalize his work with the bar-pressing response of the rat by relating it to the concept of the reflex. He tried to do so in his paper on the "generic nature" of the stimulus and response terms of a reflex (Skinner 1935a), apparently not seeing what is so clear to us a half-century later, namely, that the two ideas, namely, the "generic" bar press and the "reflex," are incompatible insofar as his reconciliatory purpose was concerned. The inevitable consequence, which is now plain in the current literature of operant conditioning, is that the "operant" and the "reflex" have gone their separate ways. Books on operant conditioning no longer even mention the reflex. Accordingly, the question of types of conditioning, whether they be one or two or more, comes alive again because, if an "operant" like the bar-press or the keypeck is no longer to be subsumed under the "principle of the reflex," why are we to speak of "conditioning" them when that term was coined by Pavlov to cover his own case of the salivary S-R reflex connection? I shall return again to this question because it is a core problem in contemporary behavior theory.

The fourth background influence on the thinking of behavior theorists in the 1930's, including our subset of four, came from W. S. Small's introduction of the maze into the study of "learning." The fifth influence was the work of Pavlov, and the sixth was the work of Thorndike. To each of these last three were must now give separate attention.

When W. S. Small, in the first years of this century, introduced the maze into the learning laboratory, he began a line of research which was to dominate the study of learning until at least mid-century. There was hardly a psychology department in any university anywhere which did not boast a maze, and hardly an issue of any journal in experimental psychology which did not have at least one maze study gracing its pages. The area of "learning" –aside from some attention (like Köhler's) to "problem solving"-was blanket by the maze as chief instrument (even in the hands of Tolman who used mazes and pathways as *his* variety of "problem solving" situations). This was partly a novelty effect, such as we may see it in any science, when a new instrument and procedure are invented; and partly it was because the maze was easy to use, offered ready data, and had obvious parameters to explore. But certainly as much as for those reasons, the maze surged into popularity because of the concept of "learning" that was common currency in experimental psychology at the time. It was a concept that was not born of the maze, but is traceable to Ebbinghaus and his nonsense syllable. In 1885, some twenty years before Small, Ebbinghaus published his studies of learning and memory. In them he had used the elimination of "errors" across trials as his measure of "learning." That, of course, comported with common sense: A subject was said to have "learned" the list of syllables when he no longer made "mistakes" in reciting it. The same model held for the maze: The "learning curve," which was soon taken as representative and prototypical of all learning, was a picture of how "errors" (now in the form of cul entrances) were eliminated in the trial-by-trial procedure used with the maze. Time measures of running the maze were sometimes also promoted as an index of learning, but usually in a secondary way because time-per-run was correlated with the number of detouring cul entrance, and in view of this partial confounding "error" elimination seemed the more acceptable of the two measures. It seemed so because, in addition to the conventional feeling at the time, that model of "learning" had Ebbinghaus's prestige behind it.

The maze has now almost disappeared from the learning laboratory, displaced by a new popular apparatus which you all know and to which I shall return in a moment. From about 1950 on, it became increasingly unusual to see a maze study in the literature. By 1970 or so, very few graduate students in psychology had ever seen a picture of a classic maze like the Warner-Warden one, and it was quite rare for a student to have had any laboratory experience with the instrument either with human or animal subjects. Just recently here are signs of a renewed interest in the maze, though it is not yet clear for what present behavioral problems the maze would be the most appropriate research instrument. Does scientific history always have cycles like this? Are they a function of cycles of knowledge, or only of nostalgia or intellectual inertia among scientists?

But apart from the fate of the maze, the concept of "learning" as a process of "error" elimination has survived. It come to the fore again whenever "learning" is studied by laboratory procedures having two features: First, when the behavior under observation is a sequence of separately recordable "responses" between defined starting and stopping points; and, second, when the procedure is a trial-by-trial one. Impressively enough, even when such a procedure is not actually being used, the notion that learning is "error" elimination often is latent in a worker's thinking despite any later model of learning, developed after the halcyon days of the nonsense syllable and the maze, he may profess. The notion persists in a kind of intellectual underground from which it continues to influence the contemporary state of behavior theory, as we shall see.

Pavlov came from a medical training and a reflexological (this term, though really Bechterev's, seems by now to have achieved a more general status) tradition which made Sechenov one of his heroes (along with Descartes and Darwin, whose three names he inscribed in his laboratory). He was already renowned for his studies of digestion in the dog when he entered upon the formal study of what today we call "behavior." The precipitating question for him was how, starting from a "natural" reflex like salivating-to-food-in-the-mouth, an "unnatural" reflex like salivating-to-the-sight-of-food could ever arise. Whatever neural pathways subserved the former, he could accept that reflex as "natural" because he believed it had a biological function and evolutionary basis. But the sensory afferent pathways of the latter had to implicate a remote cerebro-cortical projection area, and how could such distant cortical stimuli ever becomes adequate for, or substitute for, the "natural" stimulus of food-in-the-mouth? It seemed obvious to Pavlov that the locus for such a substitution had to be the cortex because that is where the afferent pathways from exteroceptive sense organs terminate. To the end of his career in behavior research his expressed intention was to learn how the cerebral cortex operates in forming such new reflex connections. The psychological ideas and inventions he generated to explain that formation have never sat well with later physiological scientists, but his "conditioning" work has had a lasting impact upon behavior theorists.

Pavlov took the dog as the subject for his new research because he knew that animal best from his earlier work. He took the salivary reflex out of the digestive chain because he knew it well, and also because it was convenient, could yield ready data, and did not involve extensive invasion of the animal's body. He settled on several exteroceptive stimuli which were remote enough from the "natural" stimulus to serve as good starting bases for new to-be-formed associations with the response of salivation. He had several givens in his experimental situation, and several silent or suppressed assumption that were woven into the thought of his time. Among these were the specific animal he chose as his subject and its specific nervous system; the sense organs being stimulated; the state of hunger itself (what we now call the "motivation"); the concepts of the "reflex" and its constituent "stimulus" and "response"; the notion of "association" of a stimulus with a stimulus, and of a stimulus with a response; and still others. His procedure seemed straightforward enough: The "natural" and "unnatural" stimuli were paired or "associated" in certain temporal relations, and the emergence of the CS-CR "connection" was observed.

In any case, that was Pavlov's view of the "connections" he was producing. That such also was the concept of "association" held by later "behavior theorists" is indicated by Skinner's statement of his "Law of Conditioning of Type S," by which he meant Pavlov's "respondent"conditioning (Skinner, 1938): "The approximately simultaneous presentation of two stimuli, one of which (the 'reinforcing' stimulus) belongs to a reflex existing at the moment at some strength, may produce an increase in the strength of a third reflex composed of the response of the reinforcing reflex and the other stimulus."

No matter that Pavlov himself, always the alert scientist, soon came to question why the taste of smell of food was so "natural" a stimulus for salivation, since they, too, apparently had to be learned. The question was indeed correct, but it was never answered, either by Pavlov or anyone else because it is conceptually incorrect. It is unanswerable so long as terms like "natural" and "unnatural" and "innate" and "evolutionary survival value" are applied to the question and its putative answers. Historically it was enough that his experimental procedures, and the resulting empirical data on behavior changes, affected the thinking and theories and research strategies of the behavior scientists who came after him. Though the drooling of a hungry dog to food signals was commonplace knowledge, and Aristotle and Plato had spoken of association by contiguity, no one had ever before measure just how temporal factors influenced association, and what the veritable course of acquisition might be. The actual measurement of a common observation is reminiscent of Galileo's report to timing the fall of a body; no one had doubted that a dropped body would fall, and there were many notions of how and why it fell, but the actual times of fall, which no one had thought of or bothered about, once they were measured had lasting consequences in the science of mechanics. So it was with Pavlov: He measured the "obvious," with lasting consequences for behavior theory. The reaction of the community of behavior scientists to Pavlov's discoveries was somewhat like that of present-day-physicists to the discovery of a new subatomic particle, and to the method by which it was disclosed: Pavlov was seen as having created a new bit of behavior, a new reflex association that had not existed before, and also (through his procedure of "experimental extinction") of showing how that new bit could be dissolved. It was those accomplishments which excited behavior scientists then, and continue to do so today.

In step with his experimental studies, Pavlov developed a vocabulary for speaking of the behavioral phenomena being discovered and explored in his laboratory. We have inherited that vocabulary: The conditioning procedure, conditioned and unconditioned stimuli and responses, inhibition (a term already available to Pavlov from his neuro-medical background) and disinhibition, stimulus generalization, and others familiar now to researchers in "learning." Even the term "reinforcement," today much in favor among workers who insist that their learning theory is not "Pavlovian," was used by him, though only as an operational term equivalent to "stimulus pairing" (of the "CS" with the "UCS") or "stimulus association," rather than with any important theoretical implications or connotative nuances. His vocabulary became, and still is, so firmly established among workers in the field that it is in continual use for "types" of learning allegedly far removed from salivary conditioning. More, it is used as well for other reflexes which, though involving other effectors such as smooth and striate muscle, rather than a gland, are still lumped together for reasons that theoretically obscure. Thus, eye blink, pupillary contraction, the galvanic skin response, foot withdrawal from electric shock, and others, are treated alike, and laboratory studies of how they are stimulus-shifted are described in Pavlov's "conditioning" vocabulary, although it has never been shown, nor is it on *prima facie* grounds obvious, that all these "response" –duct and ductless glandular, smooth and striate muscular and all these differing reflexes- *ought* to be expected to obey the same laws and show the same properties. Discussion of this question is not active today although I believe it certain that it will become so again because the issue was never properly resolved in the first place (Schoenfeld, 1966, 1972). When a problem in science is not resolved, but is dealt only by an agreed vocabulary, or is side-stepped by means of a fiction, you have the true scientific irritant; the itch may have a long latency in science, but itch it will, and attention it will finally get.

The impact of Pavlov's discoveries was felt beyond the small, albeit international, group of researchers in "conditioning." In child education, in social psychology, in personality development, in psychiatry, and in other areas, it became routine to invoke the name of Pavlov and his principles of behavioral acquisition and maintenance. This would probably not have happened, and Pavlov would have remained an obscure name in an esoteric circle, had it not been for his demonstration of "higher order conditioning," namely, that a CS established for one CR could itself be used to reinforce and establish still another stimulus as a CS, and this last as the reinforcement for still another stimulus, and so on. The disclosure that it was possible to set up sequences of CS's by which the CR could be brought under the control of stimuli more and more remote from the original "natural UCS" seemed to justify extrapolation of conditioning principles to the farthest reaches of human personal and social conduct. The literature reflects the early readiness of some writers to widen their claims for a Pavlovian basis to all behavior, but it also reflects how uncritical they were. Uncritical, because, for one thing, the higher order conditioning which could be demonstrated even in the rigorous setting of Pavlov's laboratory did not get beyond the third or fourth order; moreover, it was unstable and easily extinguished even at those close orders. The widening claims for a "conditioning" basis of all behavior would require orders considerably, even indefinitely, higher than the third or fourth, and surely more stable. Pavlov's higher order conditioning has "secondary or derived reinforcement" as its later cognate in "operant conditioning," which is also used to justify ever-widening claims for the "operant" nature of complex human behavior (Schoenfeld, 1965). Here again, such claims require the sequencing of "secondary reinforcements" in ever longer chains of behavior without loss of strength or stability as compared with the "original" reinforcer, whereas at least some laboratory studies specifically directed at the power of new secondary reinforcers do not confirm these properties (Bersh, 1951; Libby, 1951; Wike, 1966).

The historical claims and counter-claims for Pavlovian conditioning should not distract us from some key features of the training method he and his followers used (nor, for that matter, from the method used by Bechterev and his followers). These features included, first, a trial-by-trial procedure, each trial involving a single pairing of CS and UCS. This alone imposed certain restrictions upon what could be studied, and upon the data coming out. The procedure seemed natural to Pavlov, both because of his reflexiological training, and because the depletion of a salivary response was presumed to dictate a recovery time before the gland could be tapped again in another trial. We today, however, may muse over the possible consequences for behavior theory if Pavlov had used a "free respondent" procedure (if we may call it so in parallel with the later "free operant" procedure of Skinner) in which UCS was intruded on some "reinforcement schedule" into an on-going stream of salivary drops. A second feature was that the training began from an already established reflex (the "natural" one, or "UCR") which operationally meant that the response to be measured was, in advance of any stimulus pairings, guaranteed (or nearly so, if we wish to stand on Skinner's statement of the "Law of Type S Conditioning" quoted above) to appear on every trial. This feature was of importance to Guthrie's analysis of Pavlov's "conditioning," and was also absent from the Thorndike-Skinner procedures. A third feature was that Pavlov took as his "response" the total secretory output of the gland on each trial, rather than each drop of saliva as a separate response making up the total. The reasons he did so, reasons both of personal background, and of strictures imposed by his apparatus, are not far to seek, but it is amusing to speculate what the later relations might have been between "Pavlovian" and "operant" theorists if he had recorded the separate drops in a cumulative response curve.

In any case, by the 1930's, workers in learning theory believed themselves to be confronted by the problem of how many "types" of learning there were. Some thought there was only one fundamental "type," others thought there were two, others three, and still others as many as a dozen or even more. Science is always reductionist in its thrust, and also so it could be expected that each group of workers would attempt to reduce their observation of multiple varieties of behavior modification to prototypes or fundamental paradigms. Those who believed in one fundamental "type" seem largely (except, perhaps, for McDougall and his "Hormic" Psychology, or for Guthrie to whom the reflex, and not Pavlov, was fundamental) to have taken it to be the Pavlovian case to which all others could be reduced, though there was then, and still is today, no *a priori* reason why some other "type" (say, the "operant") should not be taken as the fundamental one and Pavlov's be reduced to that. As for those who believed in three or a dozen "types," the reductionist urge was intellectually as strong, but not so appealing in its solution to a wide public because the number they wound up with was not as dramatic as one or two. Those who came to believe in two fundamental "types" –those which Skinner dubbed Pavlovian or Type S or "respondent," and Thorndikian or Type R or "operant"- finally came through after the 1930's as the victors in this intra-disciplinary debate. They won out not because the problem of number of "types" had been satisfactorily resolved either theoretically or empirically, but because over the years they converted more students than did their rival theorists, and because they proved to have the greater continuing energy for research and debate. For such irrelevant reasons, they came to dominate the field of behavior theory as we know it today. But the failure in the 1930's and thereafter to have solved the problem of "types of learning" is one source of the deplorable contemporary state of behavior theory.

Thorndike's first important work in learning, appearing in 1898, preceded Pavlov. It was on "problem solving" (which, incidentally for our story, was also the focus of Köhler's later "Gestalt" studies of chimpanzee learning). The problem Thorndike set for his cat-in-the-box was, as we all know, to escape from the box and obtain the bit of food which was exposed as bait outside the box. Escape was possible by pulling a string which was the manipulandum for opening a door in the box; with the door opened, the subject could emerge, be "rewarded" with the food, and then returned to the box to repeat the escape in another "trial." Like Pavlov's and Small's procedures which were still in the future, Thorndike's design was a trial-by-trial one. In the early trials the subject's behavior appeared somewhat random, but as trials were repeated the behavior became more channeled, and the escaping string-pull came more and more quickly. "Learning" was thought to be exhibited in the falling escape times, and a graph of those times was the "learning curve."

Thorndike's description of his experimental situation and its behavioral outcome was largely in common-sense terms. The cat was "hungry"; the food was the "reward" for the "escape"; the first efforts to "escape" were "trial-and-error"; the food was the "satisfying" consequence, or effect, of the response which produced the learning (from which came the name "Law of Effect," and the spoof of some early critics who called it the "Law of Affect"). Learning was seen as incremental, gradually accumulating across the trials (the view which was later to be challenged both by Gestalt theory, and by Guthrie). Unlike Pavlov's "unconditional response" in Thorndike's situation the ("escape") response was not nearly guaranteed to appear on every trial, especially early in training, and so did not seem to be in the category of a true "reflex." In later trials, when the response did seem to be "automatic," we may note that the same issue exists as with Skinner's later "discriminative stimulus" once it was strongly trained, but which Skinner argued was not a "true" Pavlovian CS (Skinner, 1935b, 1937). We note also that Thorndike's measure of learning was not that of "errors," as it had been for Ebbinghaus and was to be for Small. In the cat's "trial-and-error" behavior, there were "errors," of course, out they could not be individually identified and recorded in the rush of behavior which was occurring in the open field of the box. The measure of learning was, instead, a temporal one. The evolution in Skinner's hands of the Thorndike box into his own "repeating problem box," now known far wide as the "Skinner box," retained a temporal measure, but in the form of a response *rate* in the open field, since the subject did not leave the box for each "reward" and there were, consequently, no trial "learning" scores.

It was to the Pavlovian procedure that Skinner tried to reduce Thorndike's experiment. Again, the former was accepted as the fundamental "reflex" case. Failing to see how this reduction could be made, he went on to argue for two "types" of learning, though both were to be considered "conditioning." Agitation over whether Thorndikian learning could be described in the same terms as Pavlov's case had been expressed by others during the 1930's. Witness Hilgard's effort (1937) to compare the two "types" of learning "naturalistically," and his conclusion that both were indeed varieties of "conditioning." Today as we read Hilgard's thesis and comparisons, they are transparently wrong. Nevertheless, it was Skinner's division of learning into two types (his "respondent" vs. "operant," or "Type S" vs. "Type R") which has become the common theoretical coin of behavior theory in our day.

There are several features of the Skinnerian formulation of learning, and of his laboratory procedures, to be remarked. The conditioning vocabulary of

Pavlov was adopted (as it was also by Hull whose system had the same structural concepts as Skinner's except for the "inhibition" term). Skinner (1938) simply classified his rat's bar press response as a "reflex," but in the face of the obviously varied movements by which successive bar presses were carried out, this needed to be rationalized. He tried to do so by designating the variations as merely "topographic," and by appeal to the "generic nature" of a response (1935a). But as we are more sharply aware today, the "bar press" is not a "reflex" in the *historical* sense of that term since, for one thing, it brought a whole range of different effectors into play on its successive occurrences. Though few of us might have been clearsighted enough at that time to realize it, we are less surprised now that Skinner's effort to justify the appellation "reflex" for depressions of the bar was doomed in advance. Guthrie was one who did foresee the failure, arguing that each depression was an "act," and resurging to call it a "response" or a "reflex" since, he reminded his readers, the rat's nerves are connected no to the bar, but to his muscles. Skinner's effort to throw the bar-press into the "reflex" class was later effectively abandoned by him, though without public retraction. In this connection we may read again Skinner's argument with Konorski and Miller (Skinner, 1935b, 1937) over whether the "discriminative stimulus" (SD) is properly to be regarded as the equivalent of a "conditional stimulus." Such equivalence, if conceded, would be the very reduction of Thorndikian to Pavlovian learning which Skinner had originally sought. Skinner's claim that his SD and Pavlov's CS were not equivalent, leaves the poor historian to wonder whether he had consciously reversed himself that long ago about the reflex character of the bar press, or whether the reversal was as yet latent and unanticipated.

A temporal measure of operant "strength" still was forced upon Skinner, as it had been upon Thorndike, because the open-field box did not allow a count of "errors." Since the Skinner box left the operant "free" and did not employ a trial-by-trial procedure, response *rate* assumed the temporal emphasis: It was alleged that rate was more important than other measures (some of which Skinner had himself occasionally taken), though the ground for believing so was never made entirely clear. Nevertheless, rate quickly became almost the exclusive reliance of laboratory workers in "operant conditioning."

Of all Skinner's work, the most influential, if I am correctly judging the present and anticipating the future, was that on "schedules of reinforcement." It was a thrust into behavior which had been largely neglected by earlier researchers, including those in the Pavlovian tradition. Whatever criticisms we may have of our behavioral inheritance, the area of "stimulus scheduling" promises to continue as a central concern of theorists whatever form or direction their theories may take in the foreseeable future (Schoenfeld, 1970, 1972; Schoenfeld and Cole, 1975).

So I come to my conclusion: The contemporary state of behavior theory is one of transition. The basic concepts and terms regarding "learning" which we have inherited from our forebears of the 1930's are failing us. They no longer serve our theorists well in their treatment of behavior acquisition and maintenance. Their decline as adequate theoretical tools is clear, but because they have as yet no suitable replacements, they remain in popular use while coming steadily to mean less and less. I may cite here a few of the more basic examples of this disintegration, but without overly prolonging the autopsy, because much of it is already in the literature.

The term "stimulus" has long been torn among three or four different definitions since its adoption from Roman animal husbandry into the lexicon of psychologists. It has been called "energy impinging upon a receptor," "an environmental change which produces a response," and several other unsatisfactory things. A response-based definition was by indirection entertained even by Skinner (1938) who was otherwise careful to give physical definitions of "reflex" and "behavior," even though in his seminal work he never made any experimental reference to "stimulus" other than in physical and operational terms. It seems unarguable any longer that the physical definition of "stimulus" is the only defensible one in the long run (Schoenfeld and Cumming, 1975): Among other grounds for this conclusion is that it is the only one which makes it possible for one researcher to replicate another's experimental procedures (Cumming and Schoenfeld, 1960). Another reason the term has collapsed in its usefulness to behavior theory is that it has lost its class or categorical bounds. "Stimulus" has been stretched today to include "ideas," "attitudes," "ambitions," "goals," "drugs," "desires," "emotions," and whatnot. A term without limits has no value for science. One currently fashionable theme among behavioristic theorists in their theoretical discussions (say, of "cognitive" psychology, and "humanistic" psychology) today is to make a stimulus into a "private event," but this resort solves none of the old problems and adds some bedevilments of its own for behavior theory. For example, how is it that some "privacy" advocates can sometimes speak of the "event" as a response in addition to a "stimulus"? And, if it is an *event* at all, it must occur in space (even internal) and time, and therefore in principle it is recordable; only temporarily (if that) can it be said to be "private," any more than any internal organ function (Schoenfeld, 1971c). To appeal to "privacy" is merely to robe a naked return of the ancient subjectivism and introspectionism which those same theorists profess a desire to escape. And finally, regarding "stimulus," I think it is correct to say that the term has had over the years a static connotation whether by intention or not: When a "stimulus" is turned on, or applied to the organism, it stayed what it was, to wit, a single unchanging manipulation of the environment. Yet the energy input function, whether to an organism's sensory receptor or to a uni-cellular organism's membrane, is a continuously changing one as the organism moves about in the environment, or as its internal state changes form moment to moment. This is a difficult problem to handle, and no theorist has yet solved it satisfactorily, but it must be borne in mind, as Kantor has long argued (Schoenfeld, 1971b). Future thinkers, as they emerge from our transitional years, will surely not tie themselves to a static "stimulus" a concept from which to launch their behavior theories.

The term "response" has come on equally evil days as "stimulus." Early in its history, "response" meant either the contraction of a muscle, or possibly the secretory action of a gland, and finally it embraced both. In the history of the reflex, the "response" was observed as the result of "stimulation," and was the activity of some "effector." When contemporary behavior theory let one its feet wander down to road of "operant conditioning," the "response" took on a new definition, that of the *outcome* of what was before called the response: The string was pulled, the bar went down, the cul was entered. Whatever the organism accomplished to "procure" the "reinforcement" was the "response" (Guthrie, as we have seen, protested this distortion). Today the degeneration of "response" is as complete as one can unhappily imagine: Writing a novel is a "response" which may be singularly "reinforced" (by such "secondary" reinforcers as money or acclaim); getting married is a "response" which may be "reinforced" (and, may we presume, thereafter be repeated many times, and perhaps to yield an extinction function?); and so on. How far have such usages come from Skinner's original formulations of the "static and dynamic laws of the reflex" (Skinner, 1938). Perhaps uneasy about such developments, in an interview taped by T. Verhave and W. Van Horn, Skinner disavows "building a skyscraper" as an acceptable "response." Like "stimulus," the term "response" now is without bounds and consequently will be of little use to behavior theorists of the future. Aside from these difficulties, two further remarks seem timely about the role "response" in contemporary behavior theory.

When Skinner began his analysis of the behavior of organism, it seemed to him that "botanizing" the reflex was to be avoided (Skinner, 1938). Instead, a "representative" reflex would suffice to disclose all the appropriate laws of behavior. Behavior was otherwise too extensive, too variegated, with a possibly unlimited number of reflexes and of "responses" in the repertory, even of the rat and certainly of the human being, to have experimentalists and theorists go chasing after all of them. That view sufficed to carry Skinner a long way in the analysis of behavior, but it has now begun to appear overly restricted. Researchers have explored the "laws" of barpressing and keypecking with other responses, and in organisms other than the rat and pigeon, and have reported, sometimes with surprise, that neither those responses nor those organisms exhaust the behavioral variety and richness to be found among the creatures of the world (ought we recall here the afore-cited caution of Jennings?). But ought we wonder about that? Can we really expect the reactions of various muscles and glands –and of *all* muscles and *all* glands- to be indifferent examples of *a* "response"? Or all organs of the body to be indifferent examples of an "effector"? [recall the failed effort of W. H. Gantt and his associates to "condition" renal secretion in the dog (Gantt, 1974; Livingston and Gantt, 1968)]. Or all species of animals (and, possibly, of plants) to be indifferent examples of an "organism?" Imagine a chemist choosing a "representative" element or compound with which to study all the laws of chemistry, and believing that the functions he discovers for one substance will hold for all. Of course, the botanist is quite right to "botanize." So, perhaps, ought the behavior analyst. It may be that all our behavioral elements and compounds –all the world's "organisms" with their "reflexes" and "responses"- will share certain universal properties, and that those will be, or should be, the only ones to interest the behavior scientist, but as yet we have no reason to accept either of these propositions.

Even setting such considerations aside, however, the historical focusing of attention by "operant conditioners" upon responses like bar-pressing and key-pecking had immediate implications for the experimental analysis of behavior. Such responses are punctuate events, occurring at the instant when our sensing and recording apparatus tells us a "response" has happened. Measured as a rate of occurrence, it is as if each "response" has no duration. Even when some researchers have measured "duration," what they were actually measuring was how long their sensing and recording apparatus was in the "on" state, rather than how long any specified effector, or combination of effectors, was "on." The treatment of a "response" as a punctate event ignores the fact that any "response" we choose to observe is abstracted from the behavior stream (Schoenfeld and Farmer, 1970), that it can be a short or long a segment of that stream as we choose, and that whatever segment we choose has a duration even if our apparatus may itself function by some other time scale (perhaps approximating the "punctate"). Moreover, because behavior is integrated in a "whole orgánism," any change we force upon it, by whatever "type" of conditioning, will affect not only the response, or stream segment, we isolate for measurement (Schoenfeld, 1971a; Skinner, 1967), but some larger stream segment in which it is embedded. Accepting such facts as inescapable, we ought perhaps build our apparatus to reflect them, because the behavior we choose to look at, whether as experimenters or theorists, whether the segments be large or small, they are our *givens*. Kantor has for many years argued against the static nature of the "response," holding out in his "interbehaviorism" for a continuously changing behavior output function as the proper starting point for behavior theory, but, again, he did not give the thought enough experimental specificity for laboratory workers to deal with. Something of this problem may have underlain Skinner's (1938 definition of "response" as a movement of the animal, or part of the animal, in space). Yet contrariwise he felt no constraint about dealing with the barpress "response" as the movement of the bar rather than the actual "press" movement. Since the movements which produce bar-depressions might be quite varied, his system required that all variations treated as the "response," which led him, as I noted earlier, to argue for a "generic nature of the response," and to dismiss the many movements which produced the bar depressions as mere "topographic" variations (compare with this the stand taken by Guthrie and Horton, 1946). This argument bestowed upon the "bar-press response" a kaleidoscopic anatomical geography. What will be needed in the future is a new approach to the datum, the *given*, of behavior analysis, research and theory. So long as the old "response," punctuate and with an unlimited mutational "topographic" range, is the datum, behavior theorists face a hopeless task. The situation is reminiscent of Zeno and his paradoxes of motion: So long as motion was conceived as a succession of positions each occupied for a finite time, it was correct to conclude that you should never be able to get through the door. To resolve such a paradoxes required a re-conception of motion as a continuous function, and the invention of the infinitesimal calculus to deal with it. For us, a re-conception of "response" and "behavior" is needed. On day, another "F. M. Cornford" will give an address on the "laws of responding in ancient thought," and the ancients will be ourselves! I have myself made a small effort to reclaim the definition of responding as movement of the organism, and to set up an experimental situation for treating "learning" by the mathematics of the random walk, but, I regret to say, without much success as yet. I am convinced, however, that one day some such new departure, or one more radical still, will succeed and behavior theory will again start out on a forward path.

The term and concept of "reinforcement," which Pavlov used in passing, has had as difficult a subsequent career as any in the lexicon of behavior theory. Skinner's announced intention in adopting it as a replacement for Thorndike's "reward" was to avoid all the layman's connotations and the mentalism embedded in the latter. Thus, the Law of Effect held that a "reward" was "satisfying," and since this was deemed an unsatisfactory mentalism, "reinforcement" was to be the technical substitute for it. What has transpired, however, is that contemporary behavior theorists are no longer able to distinguish the two terms. Over the years, they have converged in meaning and usage, so that "reinforcement" is no longer technical, and reward is no longer only mentalistic but also economic. En route to this scrambling of the two terms, theorists have found much to re-consider. New findings have emerged on "reinforcement" in its historical mix of meanings and usages. Early on, there was the question of possible circularity in the definition of "reinforcement" (though this had been adequately dealt with by Skinner, 1938) and in the statement of the Law of Effect (though this had also been dealt with), it seemed troublesome enough to prompt later theorists to dispose of it again (e.g., Kimble, 1961; Meehl, 1950). Later, concern was expressed about how many reinforcers might exist for any given species of organism (e.g., Keller and Schoenfeld, 1950), about the locus of reinforcements such as food or water, whether in mouth or stomach or blood stream, or just where in the ingestive track the "reinforcing" effect operated (e.g., Hull, 1951); about the distinction between "contingent" and "non-contingent" reinforcers (Schoenfeld et al., 1973); about "primary" and "secondary" reinforcers, and the "primary" and "secondary" drives inferred from them (Miller, 1951; Schoenfeld, 1965; Wike, 1966); about the essential "nature" of reinforcement (Glaser, 1971). Time has proved that these are futile questions, either incapable of exact formulation, or incapable of answer, or both (Schoenfeld, 1978; Schoenfeld and Mueller, 1954). But the term "reinforcement" is apparently a seductive one for scientists (as well as for laymen who unhesitatingly equate it with "reward"), so that it continues to be used today on all sides. It is now an unchallenged assumption, that is to say a dogma, among "reinforcement theorists" that, for any learning or behavior acquisition to occur, "reinforcement" is necessary, and conversely, that if any learning has occurred, then "reinforcement" must have been operative. Their security about this stems from a secure prior dogma about cause and effects, and from two errors: A mis-application of Leibnitz's principle of sufficient reason, and from the classic logical error of affirming the consequent. I shall not pursue all these matters at length here because they have already been touched in the literature, but might select just a few things to repeat about "reinforcement" before I close that topic.

First, we now know that *any* stimulus can be a "reinforcer" depending on such parameters as its intensity, site of application of the body, schedule of application, state of the organism, and so on. There is no category of stimulus which, when "procured" by a "response," is in some way uniquely defined by the fact that the "response" is influenced thereby; there is nothing "essential" about its effect-function, nothing about its "nature" that distinguishes it from other stimuli, nothing mystically resident in a "reinforcer" that makes it more than a stimulus (Schoenfeld, 1965). Guthrie, hewing in a "reinforcement" operation as changing the conditions under which the response was originally produced, so that, if the conditions in the "post-reinforcement" period were returned to what they had been in the "pre-reinforcement" period, the animal would repeat the same response. By this view, response rate, the darling of "operant conditioners," is really stimulus rate, that is, the rate at which stimulus conditions are restored after each response to the sufficient state that resulted in the response in the first instance. Any theorist truly in the reflex tradition must, of course, take some such view. But it is doubtful that behavior theory in the future will be so simplistically reflex as the Sechenov-Bechterev-Holt (Holt, 1931). Guthrie tradition exemplifies. As I suggested earlier, Kantor's criticism of the stimulus and response terms on which that tradition rest is undoubtedly correct though it has not yet been implemented in the laboratory. Furthermore, the behavioral data which future theory will be called upon to deal with will not be limited to the Skinner box, nor to the renascent maze. New arrangements for behavioral observation will have been devised, for, though our own vision is short, we cannot believe that the creative minds among our behavior scientists will never progress beyond the limitations of our present instruments.

Second, it is today ritually repeated, in our conventions, books and classrooms, that in "operant" conditioning the "reinforcement" comes after its to-be-conditioned ("procuring") response, and that it is *that* temporal relation above all which distinguishes "operant" from "respondent" conditioning in which it allegedly precedes the response. Yet it is glaringly obvious that the "operant reinforcement" also comes before the next response in the string of responses from which, over some arbitrary time period, the rate measure is calculated (Schoenfeld and Farmer, 1970); and equally obvious that even in trial-by-trial Pavlovian procedures the "reinforcer" on any trial follows the response of the preceding trial. Only if the experiment were ended with the first response or first trial, and its reinforcement, could the "before" of "after" relation alone be said to hold, but no Pavlovian or operant experiment is ended like that because to do so does not meet the customary interest of the researchers. In like fashion, it is becoming increasingly obvious (e.g., Schoenfeld et al, 1973) that our past distinctions between "contingent" and "non-contingent" reinforcement now must be reconsidered; we have both empirical and logical grounds for doing so, and to help us in so doing, which did not, and perhaps could not, figure in the theoretical thought of half a century ago in the 1930's.

Third, since one-time vaunted difference between Skinner's "reinforcement"and Thorndike's "reward" has become vanishingly small, we have over the better part of a century come no distance to speak of with our so-called "modern reinforcement theory." As I remarked earlier, only the area of "schedules of reinforcement," which Skinner almost single-handedly opened for scientific exploration, has proved a breakthrough of theoretical importance lasting into our day (Ferster and Skinner, 1957; Skinner, 1938). Here, too, progress with the rational and operational organization of reinforcement schedules (Schoenfeld and Cole, 1972) is a sign of the continuing significance of this area since its first clear recognition.

Having stated my conclusion earlier, and then having burdened it with supporting argument, allow me to repeat it bare and unadorned: The contemporary state of behavior theory is an unstable one, a parturitional one, and it cannot be expected to continue that way indefinitely. Its state is not the pre-promotional sort that anticipates simple adjustment in a scientific theory. Rather, it is a transition from long-established concepts and assumptions and dogmas to others that are certain to be radically different. It will demand some difficult and painful shifts in our resistant habits of thought about behavior, in the form and content of our behavior theories. But, of course, we shall enjoy our new and better state when once we achieve it.

May I add as an aside to my argument that, if I seem to have been concentrating on so-called "modern reinforcement theory," and on the work of Skinner and his followers in "operant conditioning," there is good reason, to wit, the predominance of that theory and those workers in contemporary behavior theory. Still, it is also true that most, if not all, the other theorists and theories of our day are open to similar criticisms. Leaving aside Skinner's non-scientific writing –such as his philosophy of morality and freedom, and his socio-economic Utopia- his historical importance to behavioral theory is based on several solid achievements that were outstanding at the time. In criticizing the latter, we have the hindsight advantage of later laboratory findings, and of the added opportunity for reflection which the last half-century has provided us. Some thirty-five years ago, I wrote that Skinner's "...influence has so perfused this field (of behavior theory) as to be lost to a proper contemporary perspective" (Keller and Schoenfeld, 1950). With time, we have gained something of that perspective, and because science does not stand still, we must move on. Already, as I believe and have tried to show you, some deep tectonic shifts are imminent in behavior theory. Tremors are beginning to be felt, and are steadily building. I think clear movement must soon occur.

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