An Interview with W. N. Schoenfeld (July 22, 1990)

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This article is a transcription of an interview with William N. Schoenfeld made in 1990. William N. Schoenfeld (1915–1996) is one of the outstanding contributors to the initial stages of the experimental analysis of behavior. After graduating from Columbia University, Schoenfeld wrote, in collaboration with Fred S. Keller, what was at that time the most significant book introducing, and updating, the seminal contributions of B. F. Skinner and other behavioral psychologists: *Principles of Psychology* (1950). Schoenfeld was also instrumental in the design of a psychology course at Columbia University based on the integration of theory and laboratory work, as well as in the foundation of the *Journal of the Experimental Analysis of Behavior* in 1958. In 1968 Schoenfeld moved to the Queens College of the City University of New York, where he developed a doctoral program based in the research of stimulus schedules (T system), the analysis of conceptual and theoretical problems in operant conditioning, and the study of respondent and operant interactions under pharmacological agents. After his retirement from Queens College, Schoenfeld taught at Bar-Ilan University and the Hebrew University in Israel. The interview forms part of a research project on the experimental analysis of scientific behavior (Ribes, 1994), which also included two other distinguished and original contributors of the experimental analysis of behavior, B. F. Skinner and R. J. Herrnstein.

This interview was taped directly by William N. Schoenfeld responding to a list of questions I had previously mailed to him. This prevented the possibility of any further interaction regarding his answers. The questions deal with Schoenfeld's retrospective opinions about his scientific career and contribution. The content of this interview shows how an outstanding behaviorist may sometimes have contrasting beliefs and opinions about his endeavor and the significance of various scientific problems and concepts. At the same time, some of the answers were so straightforward that they proved most illuminating about this scholar's positions on critical problems in behavior analysis, such as the concept of contingency, and the role of theory in science.

Questions were organized in three groups. One group of questions dealt with general theoretical and methodological issues shared by all scientists who advocate a radical behaviorism or

an operant conditioning approach to psychology. These questions explored opinions about the concepts of reflex and contingency, the place of private events, the role of mathematics in behavior theory, and so on. A second group of questions was related to Schoenfeld's opinions about his personal research career, questions related for example to the importance of particular experiments, the strategy followed in developing research projects, and so on. Finally, a third group of questions explored Schoenfeld's retrospective appreciations regarding personal contributions and findings, such as the development of the T system (Schoenfeld, Cumming, & Hearst, 1956).

I will include some references or specific information to cover references alluded to in the text or to describe some concepts that may be unfamiliar to some readers. In addition, the texts of the questions and the transcriptions were edited for consistency.

INTERVIEW

1. Do you think that conditioning theory, as derived from a reflex model, is an adequate conceptual framework

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for building up a taxonomy and methodology fundamental for the development of a theory of behavior?

I am not sure that the reflex model is an adequate conceptual framework for building up a theory of behavior. The problem involved here is one which in philosophy would be called the problem of reduction. Can you take phenomena at their level of observation and reduce them to some lower level without losing the original observation? So, for example, the people who think that they will analyze the nature of thinking by studying neural action are certainly going to fail, because no neuron in the discharge of its impulses contains the category of behavior or event called thinking. On this same problem, the question of whether a reflex model suffices to handle all forms of behavior rests. It is a question which affects many people even those who are not aware of it. I presume that Skinner, for one, is aware of it, because he has spoken about how when you dissect a behavioral instance into its so-called fundamental components, you lose the original instance. Yet, he still has not overtly rejected the reflex, even though in his later writings the term does not even appear in the index at the end of his books. So, in response to your first question, my reaction is that the answer must be left open. There is a partly empirical component to that answer, that is, for any particular behavioral instance would a reflex model suffice? Although there is always the possibility, the philosophical possibility, that behavioral instances cannot be reduced to the reflex. This, of course, is a position taken by the resurgent interest in cognitive psychology even though the people in that field do not speak in these terms.

2. Which do you think are the critical experiments in your research career? Could you comment about the reasons you consider each of them important?

I do not think of any of my experiments as being critical to my research career. When I review these experi-

ments, each one still seems to me centered on a valid and interesting question. My research interests have shifted over the years amongst a number of topics, so that I don't really think in terms of critical value to any one study. On the other hand, there are in my list of publications three papers which I would happily have withdrawn from that list and would happily say that I have never written.

3. Do you recognize in any of your experiments anomalies or contradictions with the fundamental findings of your research or the fundamental concepts of your theory?

I do not think that any of my experiments have turned up anomalies or contradictions of fundamental concepts in my behavioral thinking. On the other hand, I did once publish a negative result, that is, an experiment which did not work the way it was supposed to work [Schoenfeld, Antonitis, & Bersh, 1950]. I don't think that contradiction was significant. It did provoke something of a flurry of discussion in the literature but I don't think that discussion ever went very far, and the solution of the contradiction was not far to find. Again, when you come to Jerusalem, we may discuss the failure of the experimental finding to meet the expectation of theorists at the time the experiment was done. You may recall that the failure had to do with one of the conditions for establishing a stimulus as a secondary reinforcer.

4. To what extent do you think that reinforcement schedules and related parameters and measures constitute a beginning point in analyzing human behavior? Do correlative concepts obtained in the research with nonsocial simpler organisms lose their meaning and usefulness when they are extrapolated?

I do not believe that reinforcement schedules constitute a necessary beginning for the analysis of human behavior. On the other hand, in response to the second part of this question, I think that the problem of extrapolating from animal work to humans is not quite the

problem that should be focused on. We do not study, I think, lower organisms because they are "simpler" than human beings, although that is what is commonly taught to students. I think even more important is the use of animals in experiments to establish the validity of what is asserted to be true of human behavior, because people say anything about human behavior, whereas if we can find an animal analogue it gives us some confidence that the human observation is a valid one. Else, we may be dealing only with humans talking, in conventional terms, about their beliefs and prejudices. In short, animals can provide us with a sound foundation for evaluating the worth of an idea on the human level. By evaluating I mean fulfilling some confidence in our observation. Without such an analogue, we are never sure of what it is we are talking about precisely.

5. What limitations do you perceive in operationalism as a logical rule to construct concepts? Do you think that relations between general operations and systematic effects support the identification of behavioral processes? Do you think that the limitations you perceive of operationalism may be pointed out in conditioning theory such as it has been developed by radical behaviorism?

I believe that the early emphasis on operationalism was somewhat overblown and I was amused in later years to read that Bridgman also came to this conclusion (Bridgman, 1945). I remember once at a psychological convention when Greg Kimble was holding forth on the importance and essentialness of operationalism, I stood up and said that I thought operational approaches can be wrong in at least two ways. I remember that Greg was just horrified at the notion and said to me something publicly, something like "But Schoenfeld you can't think that operationalism can be wrong," and I said "yes, at least in two ways. First, that you have selected the wrong operation, that there are in fact an infi-

nite number of operations in any one instance, and that when you select those which you think are essential to the definition you may have selected the wrong ones. And second, that what appears to be operationally different between two operations, in fact may be the same according both to theory and to later experimental findings." For example, if you want to boil water you can light a match to a stove, get a flame and put a pot of water on it, and eventually the water will boil. Or, you can put the water pot out in the sun and with a magnifying glass focus sun rays upon it so that the water boils. These operations, as operations, are quite different, but both by theory and later experimental findings they result in the same boiling water and you interpret boiling water as a matter of molecular movement. Now, operationally, you were doing different things, but the outcomes were the same.

6. Do you think that your career as a researcher illustrates a research program? What would be the fundamental components and criteria?

I do not think my research career has been encompassed by a single research program. Instead, as I look back, I think I have had several programs, that is to say, several areas within which I did have a concentration of thought and research. But they are, I think, kind of subprograms for me. There was no single overarching program.

7. Do you think that deprivation states and historical variables are adequately represented in operant conditioning formulations?

I do not think that deprivation states and historical variables are adequately represented in operant conditioning formulations. In general, such groups of variables have been designated by group names, such as "state of the organism" or "reinforcement history." I have no doubt myself that greater specification, greater fineness of specification, needs to be done at some time, but whether they would be useful now is not entirely clear to me.

8. Do you think that private events have, before self-tacting, a physical independent functional status?

I always tell my students that I do not believe in private events. It seems to me that the phrase which Skinner has, I think, introduced is used mainly as a cop-out. It is the way of trying to say that operant theory can incorporate topics, which at one time would have been thought of as mentalistic, or subjective, or nonbehavioral. I tell my students that if private events are truly events then they must have their occurrence somewhere and sometime. The fact that they are private at the moment may simply mean that we simply do not have the instrumentation to measure these events. But they are not private in the sense that they are in principle inaccessible. So that when I say I do not believe in private events it simply means that I think all the behaviors which are encompassed by some people under the rubric of private events are in principle just as readily studied as any other behaviors.

9. According to you, what is the role to be played by mathematics in behavior research and theory?

Quantification has become something of a popular faith in our time. There are people who believe that no progress can be made in science until we quantify the science. That does not seem to me to be correct. Science is after all man's attempt to understand the physical universe, and the various subdivisions of science represent man's subdivisions, the way he subdivides the natural world around him. Whether it is true that quantifying a science, any science, will produce a more powerful or a more comprehensible or a more useful science, whether that is true seems to me dubious at least. No one has yet proved, to my knowledge, that quantification of a science does produce greater comprehensibility and greater power. To me it seems as if in some ways when any science is quantified, some measure of its comprehensibility is lost. I believe that it is true even of modern physics which

is highly mathematical. Some of modern physics has become more incomprehensible by being quantified. But if this question really is aiming at whether quantification in psychology is desirable, then I would have probably one or two different reactions. Most quantification in psychology, up to this point, has taken the relatively primitive form of curve fitting. That was true of Hull [Hull, 1943] and those who followed in his footsteps. The other kinds of mathematics which psychologists have tried have begun with various assumptions or hypotheses from which deductions have been made, following somebody's model of what science should be like. Unfortunately it seems to me, most of these efforts have not made progress, they have failed to produce a more comprehensible and more powerful behavioral theory. Still, the faith that many people have in quantification is undiminished, and far be it from me to decry that, although I do wish that it were sometimes less a matter of faith, less blind and little more rational.

10. Do you think that variation and selection of behavior are fundamental concepts in the understanding of the various classes of animal and human behavior?

The selection of behavior and the variations of behavior are of course the observational groundwork from which the science of behavior must start. Whether the selection of any particular category of response or any particular variation in response is a correct one or a useful one is an empirical question. At one time it was thought, following Skinner's lead, that it was not necessary for behavior theory to botanize the reflex, that is, that it would be sufficient to have one representative reflex and having worked out its governing rules those rules could then be extrapolated to other response categories. I am not sure that this is quite right. If it be true that different categories of response may in fact obey somewhat different or varied rules, then we would have to know

more about each of the categories, and while there may be an infinite number of them, which is unfortunate, it may not turn out that we need an infinite set of rules for those categories. There may be enough overlap from one behavioral category to another so that generality of governance is possible to discern. But that is an empirical question and I think, therefore, that the various classes of animal and human behavior cannot be ignored, that we must at least be alert to the possibility that having studied one particular class of response we may have lost sight or lost control of the governing variables for other classes of response.

11. According to you, which would be the most outstanding differences between your way of doing research—including the theoretical stages involved—and that of B. F. Skinner?

The outstanding difference between my way of carrying out experimental research and that of Skinner's is that Skinner tends to do experiments which are demonstrational or exemplary, that is, for a given set of values of the determining variables Skinner's experiments tend to show the existence of a behavioral phenomenon. For my part, I tend to work parametrically, that is, when I select the experimental variables which I want to study I, in my experiments, vary them across their range, so that I tend to get more functional relationships than Skinner does.

12. Do you consider your work on the T system as a divergent formulation with respect to the analysis by Skinner of reinforcement schedules? Why? [The T system was developed by Schoenfeld et al. (1956) as an alternative framework for classifying reinforcement schedules. Instead of identifying, as B. F. Skinner did, two rules for providing reinforcement contingently on a response, one rule based on the passage of time (interval schedules) and another based on the number of emitted responses (ratio schedules), Schoenfeld and his collaborators designed a temporal domain initially including two parameters: T, the length

of a repeating cycle, and $\bar{\tau}$, the ratio of the duration of a subcycle t_D in which a reinforcer is available for the first emitted response to the overall duration T of the whole cycle (i.e., $\bar{\tau} = t_D/T$). Later, Schoenfeld and his group (Schoenfeld & Cole, 1972) added an additional parameter p describing the proportion of cycles in which a reinforcer would be available for the first response emitted in a cycle.]

My work on the T system is not divergent from Skinner's, except in the sense that I have just mentioned. The T system identifies the temporal and reinforcement variables which are together the defining features of a so-called reinforcement schedule, and within those features to make the studies of behavioral effects parametric, that is, dependent upon ranges of time and probability which go from one end of a practical continuum to another.

13. Does the initial concern in the T- τ system about the contingency of reinforcement upon R reflect a lack of recognition of the relational nature of the operant?

The T- τ systems began with the contingency of reinforcement upon R. The purpose of that was simply to make more obvious the contact of the T- τ systems with what the field was doing when it specified its reinforcement schedules. You will remember that the T- τ systems soon expanded into a non-contingent delivery of reinforcement, what at the time was called free reinforcements, so that we had a true schedule of reinforcement delivery, and contingency was then defined as the dependence of the distribution of reinforcements in time upon the distribution of responses in time, as well as the converse, the distribution of responses in time as dependent upon the distribution of reinforcements in time. This way of defining contingency frees it from the taint of the notion of procurement or teleology between response and its outcome, although Skinner always emphasized the dependency of the response upon its outcomes.

14. How do you consider your anal-

ysis of the contingency to be distinct from Skinner's (including the problem of superstition) and how did this conceptual difference influence your research methodology?

I do not think that my analysis of contingency, as given in the paper we published, called "Contingency in Behavior Theory" [Schoenfeld, Cole, Lang, & Mankoff, 1973] represents any significant departure from what Skinner had in mind when he focused upon schedules of reinforcement, other than the point made in my answer to the preceding question.

15. Do you think that the distinction between stable and transition states is useful?

The distinction between stable and transition states is more than just useful. It is in fact inevitable. I understand stable state to mean the state to which behavior comes when the values of the determining variables remain fixed, whereas a transition state is one in which you observe behavioral effects of changing the values of the determining variables. This is obviously a distinction which is fundamental and must be made. It must be made not only in logic but in the actual behavioral consequences of the two experimental operations of keeping values of the determining variables fixed or allowing them to change.

16. To what extent do the conceptual limitations of conditioning theory restricted the heuristic usefulness of the T system? Do you think that the T system conceived as a periodic table, arranged according to the parameters of stimulation and contingency, might be applicable outside conditioning theory?

I do not think that the heuristic usefulness of the T system is restricted by any conceptual limitation of conditioning theory. After all, the T system is a way of ordering the temporal parameters that define or describe a schedule for the occurrence of stimulus events. Even conceived as a periodic table, I am not sure that the T system would be applicable outside conditioning theory,

but that, of course is an empirical question. Anyone who wishes to describe temporally the occurrence of events might very well use the T system or some aspect of it.

17. The definition of response seems to be the main obstacle to overcoming the limitations of the reflex paradigm. Why should we continue measuring the rate of a molecular response? If behavior consists of a relation, would it not be more convenient to measure relations instead of effects?

The key place of the term and concept of response does impose some restrictions upon the way in which behavior theory must develop. This was one of the principal views of J. R. Kantor. For my part, I would predict that the future development of behavior theory may cease to emphasize or rest upon the punctate temporal character of response and move more toward a continuous description of the flow of the behavioral stream, which, after all, is a continuous flow. It seems to me that in the history of physics, that the description of motion went through similar phases, when, for example, at the time of Zeno, and at the bottom of his famous paradoxes, motion was conceived as a set of successive positions, whereas later and into the modern time, certainly from the age of Newton, motion is conceived as a continuous function of displacement in space across time. This change in the way motion is conceived has made possible much of the development of modern physics, and I believe that something analogous to this shift lies ahead of us in behavior theory, when someone with enough imagination and power of thought can see his way clear to doing that.

18. What conceptual bond may a methodology derived from the T system have with the analysis of human behavior?

I am not sure that there is any conceptual bond which can a priori be derived from the T system for the analysis of human behavior. Of course, human behavior is also subject to the tempo-

ral distribution of events bearing upon it and in that sense, the T system being an objective physical schema will be applicable. But anything more profound than that I am not certain of.

19. What do you think are the fundamental conceptual deficiencies of conditioning theory, and how do they reflect upon current problems and issues in the experimental research on behavior?

I think that my notions of the fundamental weaknesses or deficiencies of modern conditioning theory are described in my answers to some of the foregoing questions and in several of my published papers [Schoenfeld, 1966, 1976; Schoenfeld et al., 1973], but in general I still believe that conditioning theory, although we have to take into account the shifts in meaning of that term from the time of Pavlov, so that today we are probably more justified in speaking of general behavior theory rather than conditioning theory, I do think that we are still on the right path. If our analysis of behavior does not stay on the objective level and if our experimental research programs do not deal with physical realities, then a theory of behavior, I do not believe, will ever be possible in a scientific sense.

20. Do you consider that the T system became a diagnostic instrument for the existing anomalies, in the sense of Kuhn (1970), in the theory and methodology of conditioning?

I do not consider that the T system constitutes a general diagnostic instrument for dealing with whatever anomalies exist in the theory and methodology of conditioning. The T system, after all, is a relatively modest and obvious effort to organize stimulus events that impinge upon an organism within a temporal framework. To extend it or to think that it may be extended to broad questions of behavior theory is, I think, not warranted.

21. The T system did begin as an organizing framework for stimulus schedules in time. The concern for the ordinality of contingency reflected in

such a proposal (in a schedule of reinforcement the administration of a reinforcer typically depends on the ordinal position of the response in a series of responses or in a period of time). And the functional properties of behavior in space, was that problem solved? What is presently your view about this?

I refer back to my answer to your Question 17. It does not seem to me as if the properties of the behavior stream when referred to spatial coordinates has been solved. I think this is an important problem and awaits insights from some creative mind.

22. Could your experimental strategy be characterized as exploratory and systematic rather than technological and outcome oriented, as Skinner's could be thought of?

As intimated in my answer to earlier questions, I think that my own experimental strategy can be thought of as more systematic than Skinner's. That is, I've always been interested in identifying the parameters of behavioral phenomena and in experimental research exploring the values of those parameters across their range. On the other hand, it seems to me that Skinner's approach has been largely demonstrational and exemplary, and while he has been very creative and imaginative in this approach, it is not one that I myself find congenial as a long-range method, although I must confess that I have also done some demonstrational and exemplary experiments, but they do not represent the major trend of my thinking.

23. How do you "see" your *Principles of Psychology* (Keller & Schoenfeld, 1950) through the light of current conditions in conditioning theory and radical behaviorism?

The text we wrote, Principles of Psychology, was published in 1950, and written shortly before that, so that it is now almost half a century old. In my view, it has outlived its usefulness in its present form. After all if we had not learned anything about behavior in the last 50 years, there is something fun-

damentally wrong with us and our approach to behavior as a subject matter for scientific study. As you may know, I wrote the major part of that book, and so I speak with some feeling of responsibility. I think that it would not be sufficient today simply to write a second edition in which references were updated but the basic conceptual approach was left untouched. What is needed is a new text, bringing the findings of behavioral science up to date and also lining itself up more accurately with current thinking about theoretical questions. After all, operant thinking or operant conditioning theory in 1950 was very different from what it is today. I do not mean that the new book would have to emphasize theoretical issues, because that, in my opinion, would be an error, a mistake, for a book intended to serve as an introduction to the science of behavior, as K&S was intended. But the selection of material and the arrangement of material, and the inferences drawn from the new material, would be, perhaps in some cases, quite different from what they were half a century ago. A good part of the present K&S could very well be salvaged—as well as portions that I wrote, which were larger than appear now, that were trimmed by Keller and myself because they threatened to swamp the rest of the book—those portions could be salvaged but the way in which they are framed by the systematic thinking of the day would necessarily be different. Part of the success of K&S came from the fact that we consciously kept the student in mind and did not write for the approval or impression of our fellow faculty members. A second consideration, and one which I pushed quite often, was that new decisions might have to be taken for our book as to

what was elementary and what was advanced, decisions that might depart considerably, as it turned out they did, from what were the conventional views of elementary and advanced, conventional in the sense that other authors of other textbooks were all using them. It was necessary to be slightly revolutionary in this regard and I did not hesitate to do that. The conceptual approach of this new book would, of course still be that of behaviorism and conditioning theory, but it would take account of shifts in emphasis that have occurred in the last 50 years.

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