

## REMEMBERING NAT SCHOENFELD

### RECORDANDO A NAT SCHOENFELD

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#### ABSTRACT

This reminiscence in memory of Nat Schoefneld describes some of the interactions he had with one of his students. It may help to illustrate why he was so effective as a teacher.

Key words: W. N. Schoenfeld.

#### RESUMEN

Esta reminiscencia en memoria de Nat Schoenfeld describe algunas de las interacciones que él tuvo con uno de sus estudiantes. Esta reminiscencia puede ayudar para ilustrar porque él fue tan efectivo como maestro.

Palabras clave: W. N. Schoenfeld.

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The trouble with trying to write a reminiscence about someone who's been important in your life is that it too easily becomes a piece about yourself instead of a piece about the other person. To avoid that problem, I might have prepared for telling about Nat Schoenfeld by reviewing his curriculum vitae and rereading some of his most influential writings. But I didn't get to know him that way and I don't think that would have helped me to talk about him. Fortunately, others writing for this special issue of the *Mexican Journal of Behavior Analysis* will no doubt discuss some of the basic facts of his life and work, and that allows me the luxury of concentrating on what mattered most to me.

I grew up in New York City. Even after I became an undergraduate

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student in Columbia College I continued to live at home with my parents in the Washington Heights section of upper Manhattan. I worked summers and during the winter break, but with some scholarship help I was able to make ends meet without having to work while classes were in session. In my sophomore year I took Fred Keller's introductory course, Psychology 1-2, and my experience in that course led me to enroll in Nat Schoenfeld's course in Experimental Psychology (Psychology 3) in the fall semester of my junior year. Though not called by that name, Psychology 3 was effectively a course in sensation and perception; its text was the 1938 edition of Woodworth's "Experimental Psychology". Its spring continuation, Psychology 4, was effectively a course in learning and motivation; its two texts were Skinner's "Behavior of Organisms" and Hull's "Principles of Psychology". The class met every Tuesday and Thursday morning, with the laboratory scheduled later each day. Eliot Hearst was Nat's teaching assistant during that academic year.

A little before the start of my junior year, my father fulfilled a lifelong ambition: he bought a car (traffic, parking and the costs of car ownership were less of a problem in the New York City of the mid-1950s than they would be later). He began dropping me off on campus on his way to work to save me the expense of subway fare (and probably to have time to talk too, because he also worked nights and weekends in those days). As a result, I usually arrived at the Psychology 3 classroom in the annex of Schermerhorn Hall an hour or so before the scheduled 9:00 a.m. start of classes.

At Columbia, Nat Schoenfeld routinely began his working day early. Some time during the first week or so of classes, I had just entered the classroom and was probably reviewing the syllabus or flipping through the textbook when Nat looked in and saw me sitting there. He continued on to his office a little further down the hall, came back a few minutes later and sat down across from me, and we began talking. From then on we did that on most class days throughout that academic year. The class was small: probably not more than a dozen students, all male. As class time approached the other students began to appear and to join in the conversation. I don't know whether any of them had any idea how often or how long Nat and I had been talking each morning before their arrival.

The topics we discussed were usually unpredictable, and I don't recall any systematic differences between what Nat and I talked about before the others arrived and what we all discussed together. Once when just he and I were talking, our conversation turned to whether I was interested in going on in psychology, and he suggested that I shouldn't do it if it interested me because I thought it might help me to solve my own problems. The possibility hadn't occurred to me and it took me a while to understand the comment and why he had made it. I'd read some Freud before college and my summer job

at the time was as an attendant on a psychiatric ward, but the psychology I was learning at Columbia seemed remote from such issues (I was to learn more about the connections in Ralph Hefferline's course on behavior pathology). I now believe that my reaction appealed to him because it implied that I was interested in the science of behavior for its own sake and not because it was a way-station leading somewhere else.

Mostly, both before and after the other students arrived, we talked about behavior. Sometimes the topic was obviously relevant to the course; at other times its relevance became obvious only a long time afterward, if it became obvious at all. Sometimes the talk was structured around something we'd read or something we'd be doing in the laboratory, and sometimes Nat would pick up on something that someone said and follow up on it in a digression that would continue for the remainder of that class. It's impossible for me to say how many times I plagiarized his examples when I later went on to do my own teaching (I wish I could also have plagiarized his style).

We considered the arbitrariness of the concept of a stimulus threshold in the context of discussions about physiological limits in sports and whether, given a sufficiently large payoff, you could get reaction times shorter than any that had ever been seen before; as some of us would realize long afterward, the work that would be formalized as signal detection theory over the next decade or so was just beginning to appear in the literature at the time.

We considered the priorities of physiology and behavior in the context of discussions of rods and cones and dark adaption functions and visual acuity in the retinal periphery; those who followed the argument were persuaded that if you didn't have the behavioral data first, you couldn't tell a physiologist what to look for in the visual system.

We considered the contributions of learning and instinct to behavior in the context of discussions about whether a boy and girl stranded on a deserted island as children would ever discover sex; whatever our initial guesses, by the time we had reviewed what was known about the related question of whether a feral child would ever learn to talk, we had begun to understand how little we knew and how much we took for granted.

We considered the role of the nervous system in behavior in the context of discussions of the relative impact on someone's personality of cutting off the tip of the person's nose or removing an equal volume of the cerebral cortex; we were forced to concede that we had never seen a brain doing anything. In worrying about whether muscles or other organs played a role in learning, we began to appreciate the significance of thinking about behavior as an interaction between a whole organism and its environment.

We considered the meaning of hunger in the context of discussions of a hypothetical caveman who caught and caged a small animal and then began

to observe its eating habits; in its treatment of hypothetical constructs and invented causes the discussion was an exorcism of sorts, but anyone who later read the opening paragraphs of Skinner's 1932 paper called "Drive and reflex strength" would discover that it was also a preparation for the analysis of verbal behavior and the turning of that analysis on the behavior of the scientist.

We considered the nature of science in the context of asking how we could deal with an independent variable as such if it was changed by its own dependent variable, as when the scheduled delivery of reinforcers was affected by the very behavior it was arranged for. It seemed at the time that Nat had a blind spot about interaction, because such interaction seemed inseparable from the concept of reinforcement itself. If the only proper independent variable in science is one that is isolated from its dependent variables, then what do we say about reinforcers when they are produced by the very behavior that we want to measure as our dependent variable?

I was only peripherally familiar with Nat's research at the time, but his system of schedule classification was built on methods for getting around that problem. It's too bad computers weren't available then. For example, the problem that obtained reinforcers deviate from scheduled reinforcers can now be finessed by allowing reinforcement setups to accumulate so that the organism can produce several reinforcers in a row; the equipment available at the time made such procedures technically unmanageable even when they were feasible in principle. Nat brought the issue up from time to time even in my correspondence with him very near the end of his life.

Nat seemed always to be asking questions of the students in that class, and often when one of us answered he would follow up by asking how we knew. As he uncovered the further assumptions behind those assumptions we'd already had to acknowledge, some students found him exasperating or even intimidating. Those who thought that his point was to illustrate styles of argument or to teach us how to succeed in debate were seriously mistaken. In fact, the surest way to get his disapproval was to give an answer that seemed designed more to satisfy him or to demonstrate argumentative skill than to get at the point at issue. He cared about those questions, and even if he often managed them so that we felt we were doing most of the work for him, it was clear that nothing mattered more to him in that classroom than to have made a little progress toward the answers.

He obviously enjoyed his work and even his jokes were on target. There was the one about the distinguished professor who was given a chauffeur to escort him to colloquia. The chauffeur sat in on the lectures and after a few told the professor that he thought he could give them too. So they changed places before the colloquium at a university where the professor wasn't known by sight, and the professor, in the chauffeur's uniform, sat in the

front row. But the chauffeur talked a little faster than the professor, so on this occasion there was time for questions. Someone in the audience offered one with the remark that it would be hard to answer. And the chauffeur just waved toward the professor, saying, "That question's not really difficult. In fact, even my chauffeur can answer it!"

Then there was the one about the opera singer who had just finished singing an encore. One person in the balcony stood up and shouted "Bis! Bis! Bis!", a traditional request for a repeat. So the singer sang the number again, and again came the shout of "Bis!". After several repetitions the singer called up to the person in the balcony and asked how long this would go on. Back came the reply, "Until you get it right!". What could be more appropriate than jokes about asking and answering questions and about getting it right?

Toward the end of the spring semester, Eliot Hearst talked to me about the possibility of becoming Nat's teaching assistant the following year. Eliot was leaving Columbia that summer, and Nat preferred teaching assistants who were familiar with the undergraduate program and his teaching style. Apparently none of the available graduate students qualified. If my continuing in psychology wasn't inevitable already, that offer clinched it. The following year, during my assistantship, Nat taught me a range of laboratory and teaching skills from the calibration of a Macbeth illuminometer and the programming of operant equipment to preparation of course handouts and the grading of papers.

At the time, the Columbia department included several chess masters among its graduate students. Nat liked chess but was not in their class, nor was I, so we began to play chess together at lunchtime. Mostly we talked, however, often across the chessboard. He cautioned me about getting into debates with philosophers, and he told me about the difficulties of writing clearly about behavior, and he speculated on the nature of time. In the spring of that year, I also took a seminar Nat co-taught with Fred Keller and Ralph Hefferline. We started with Skinner's William James lectures, and finished them in time to move on to Skinner's book, "Verbal Behavior", when it was published later that semester. A lot of our talk, therefore, was about verbal behavior and its implications for knowing.

Nat moved from Columbia College to the Columbia General Studies program and later to Queens College after that semester. I stayed on for a Master's year at Columbia and then moved on too. Thereafter (but not often enough) we saw each other from time to time in our various travels, usually at professional meetings; later we occasionally talked on the phone and exchanged letters. When I wrote a chapter on timing for his 1970 edited book, "The Theory of Reinforcement Schedules", I was surprised that he made no editorial changes. I was bothered by one of my sentences once the chapter was in print, and some time afterward I mentioned that to him when I saw him

at a meeting. He remembered which sentence it was and launched into a discussion of the short term and long term consequences of editorial practices when editors change what authors say instead of letting authors speak for themselves.

One way to judge people is to ask about the reinforcers that maintain their behavior. By watching what people do, we can often judge what's most important to them. The reinforcers might include recognition and acclaim or social consequences or material things or sexual conquests or political power, among the many possibilities. For Nat, it was figuring out things about behavior, and behavior included all of life. That's why he wrote about verbal behavior and religion as well as about reinforcers and contingencies, and that's why he argued that one shouldn't go into psychology to solve one's own problems. He was so adamant about keeping the personal dimension out of our behavioral inquiries that my one reservation in writing this reminiscence is that had he known about it he might not have approved. And probably that's why the inquisitiveness and integrity of his questions meant so much to so many of us who were lucky enough to have had him as a teacher.

In one of the last few phone conversations I had with him during his last year, he talked about three projects he would like to have finished if he'd had the time: a poem, a philosophical piece on a small point that would be perplexing to philosophers, and a translation of Genesis for physicists (e.g., the world without form and void as a world of maximum entropy). But what moved me most of all was when he talked about the fun we would have had if we could have gotten together to talk more often. Talk for Nat was never competition, nor was it talk just for its own sake. And what fun we did have. His talk made all the difference in the world to me, and I'll miss it, and him, more than I can say.