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Avoidance at Columbia

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Back in the late 1940s and early 1950s, avoidance was in the air at Columbia. We did not avoid each other—far from it. Our environment was intellectually stimulating, and full of positive reinforcement. As Charlie Ferster used to say, "If you want reinforcement, you have to behave," and for those at Columbia who had behavior, there were plenty of reinforcers. But in many ways, things were done quite differently then than they usually are now, and perhaps we can learn something by looking back.

The stimulation and the reinforcers in Schermerhorn Hall came from faculty and students. There were vision people living on the third floor, and behavior people on the second floor. With academic politics being what they are, rumor had all kinds of conflicts existing between the two floors, but the conflicts did not exist in fact. I remember one episode that illustrates the collegial nature of the interaction. When I was preparing the graphs for my dissertation, I asked friends if they thought Professor Graham would let me use the vision lab's drawing table and lettering set—he had grants, and research facilities that did not yet exist on the second floor. Everybody blanched. They advised me not to try; it would just cause trouble.

But I went ahead and asked anyway. Professor Graham seemed both a little surprised and a little pleased; he gruffly told me to be sure to wash the pens and leave everything in order when I was finished. I did so, of course, and was not aware of any difficulties that ever developed. I always felt that Clarence Graham and Connie Mueller were first-class scientists and thinkers, not just about vision but about behavior in general. They were always ready to help the second-floor students with criticism, resources, and lively discussions about problems in behavior analysis.

On the second floor, with Keller, were Hefferline and Schoenfeld. I will say more about them in a moment. Among the more advanced students during my time were Joe Antonitis, Phil Bersh, Jim Dinsmoor, Charlie Ferster, and Joe Notterman. They were our teachers, too, always sharing their current research with us and encouraging us to do the same.

Among my contemporaries (same entering class) were Don Cook, Mike Kaplan, and Charlie Crocetti—the four of us formed a tight little group, exploring and debating the ramifications of everything our professors said, or didn't say when we thought they should have. We were concerned about such things as whether, for the rat, the "click" was part of the food pellet, or was independent of the pellet; or whether a discriminative stimulus for escape behavior would become a conditioned negative reinforcer or a conditioned positive reinforcer; or whether stimulus-response chains could function as "higher units"; or what happened to the overflow of responses when the reflex reserve filled up. For us, the whole world was a gigantic "Skinner box," and we were fascinated by the notion that "free will"—including our own—was just another class of strictly determined behavior.

But, as I said, avoidance was in the air. Avoidance was a real puzzle. What was the reinforcement for the behavior of avoiding? Successful avoidance meant that something—the shock—did not happen, but how could something that did not happen be a reinforcer? As Schoenfeld used to say, "Things are not happening all the time." O. Hobart Mowrer was a hero-villain. We all admired him for the clever and important avoidance experiments he and his colleagues had carried out, but he found it necessary to postulate fear reduction as

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the reinforcer (e.g., Mowrer, 1960). This did not sit well within our behavioral orientation. Not only was fear an invention, with properties that could be assigned arbitrarily to fit any theory, but both the avoidance response and the fear seemed to have the same causes; how could one explain the other?

It was Hefferline (1950) who provided the key. He did a series of experiments in which rats could keep a bright light off by holding a lever down, thereby keeping a switch open and breaking the circuit to the light bulb. Whenever the animal got off the lever, or let it rise to the point where the switch closed, the bright light turned on. With this arrangement, his rats became "couch potatoes," spending most of their time sitting on the lever, avoiding the bright light.

Hefferline hypothesized that proprioceptive stimulation produced by the "up response"—releasing the lever—became a negative reinforcer because it always preceded the bright light. The "down response"—pressing the lever—was then reinforced because it terminated the stimulation from the "up" response. What seemed to be a response that avoided something in the future was actually escaping something in the present.

Winnick (1949) provided some direct confirmation of Hefferline's hypothesis (publication dates of Hefferline's, Winnick's, and other key experiments do not correspond to the sequence in which the studies were actually carried out. Keller, whose memory is much better than mine, will undoubtedly fill in the missing details in his autobiography.). Winnick's animals could keep a bright light off by pushing a vertical hinged panel. What Winnick did to add to the Hefferline story was to attach a pen to the panel, and thereby provide a continuous record of the panel's movements. The record of panel pushing revealed vacillation that was not easily apparent to the eye. As Keller and Schoenfeld (1950) summarized, "The panel-pushing was not steady in force or extent, but was marked by large variations even though the animal might not for a long time release it sufficiently to allow the light to come on. Incipient movements that stopped short of the light-switching-on point alternated with retreats from the switching-on point and pushing with renewed vigor" (pp. 323–324).

Winnick's data made Hefferline's hypothesized "up" and "down" responses visible. Movements of the panel in one direction provided danger signals, and escape from those signals reinforced movements in the other direction.

Schoenfeld (1950) then extended Hefferline's analysis to the classical avoidance experiment, in which exteroceptive warning signals preceded the bright lights, or more usually, preceded shocks. He suggested that the actual warning signals in avoidance come not just from the external environment, but from environmental stimuli in compound with proprioceptive stimuli that arise from behavior itself. He pointed out that in the presence of an exteroceptive warning signal, shock could follow anything the animal did except the avoidance response. Then, reinforcement for the avoidance response would come from its termination of other behavior that had come to signal shock.

In Schoenfeld's (1950) words, "An everyday way of putting it would be that 'the creature, in this situation, stops doing the unpleasant things—and even doing nothing is unpleasant now—and does the only pleasant thing left.' The avoidance response is not really avoidance at all, or at least is only incidentally so. Its function is not to avoid, and it is not made 'in order to avoid.' Rather, it is primarily an escape response, reinforced by the termination of secondary noxious stimuli, including proprioceptive and tactile ones." (p.88).

This was heady stuff. From a scientific point of view, here was a particularly bothersome problem—behavior that was seemingly purposive—being explained by appeal to an observable behavioral history. Many of us were convinced that escape and avoidance were somehow basic to many clinical phenomena, and we were encouraged that behavior analysis was ready to make contributions in those areas.

For Hefferline, muscle tension was an obvious candidate as an avoidance response—reinforced by escape from tendencies to use those muscles in other ways. For example, how better to keep yourself from striking your father than to tense your muscles in a way that was incompatible with striking? Hefferline told us fascinating stories about patients whom he had taught to relieve their back pains by relaxing their shoulder muscles. Frequently, the muscle relaxation was accompanied by declarations of intense hostility, and even murderous intent directed at family members and other close associates. (It was in the course of these investigations that Hefferline introduced techniques and data that made him a founder of what is now known as biofeedback.)

In the light of the atmosphere in Schermerhorn Hall at that time, it can be seen that my own contribution was only a small step. We called Schoenfeld's account the "squeeze tube" theory of avoidance. As the shocks pack more and more behavior into the aversive tube, the one response that is never shocked gets squeezed out. It seemed to me that the formulation of avoidance that had emerged did not require that there be any exteroceptive warning signal at all. If enough of a subject's actions could be turned into conditioned negative reinforcers by having them precede shock, then any arbitrary response that was protected from shock would become dominant.

Part of my purpose in describing all this is to emphasize that avoidance was in the air only because: (a) it posed some important scientific puzzles, (b) it was an active area of controversy between the new behaviorism and the then current versions of cognitivism, and (c) it seemed to have some practical relevance. Research grants had not yet come to the second floor. Nobody had a grant that required students to work on a particular topic, or to do any specific experiments. Indeed, we had to find our own topics, and work out our own ways to investigate them. We had to have some behavior, but the behavior we had to have was determined by the science itself, not by administrators of science.

And so, after camping outside the door of the room in which Joe Antonitis was finishing his dissertation, and moving in as he was moving out—filling the vacuum before somebody else discovered it—I set up a procedure that was designed to produce avoidance behavior without any exteroceptive warning signal (Sidman, 1953a). I arranged for a shock to come every 15 s (later changed to 22 s) unless the animal pressed the lever. Each time it pressed the lever, it postponed the next shock for 15 s. Anything the animal did, therefore, except pressing the lever, could be followed immediately by shock. This arrangement provided for at least 15 s between any depression of the lever and a subsequent shock. The question was, "Would the lever-pressing response be squeezed out of the tube?"

I had no cumulative recorder at that time, only a constant-speed waxed-paper polygraph. A stylus etched a continuous line that was displaced whenever the animal pressed the lever, and returned when the animal let go. After a session lasting more than 8 hr, I had to measure hundreds of feet of tape, with a ruler, in order to determine the time between consecutive responses. The first tape did not look encouraging; responses were sparsely distributed, with much blank tape between each one. But, I measured the distance between each response, transformed distance into time, and plotted a cumulative record.

Here (Figure 1) is that first animal's cumulative response record, plotted in 10-min intervals. Little happened for 35 min, but then a slow, steady response rate emerged. Although the slope of this curve is visually steep, the scale of response rates shows that the animal was not pressing the lever very often—the rate rarely exceeded 2 responses per minute, and was usually less. Had this low response rate not continued steadily for more than 8 hr, it would not have been an encouraging finding.

But it was clear to me that I had something. Schoenfeld was on sabbatical at Indiana at the time, so I wrote to him,

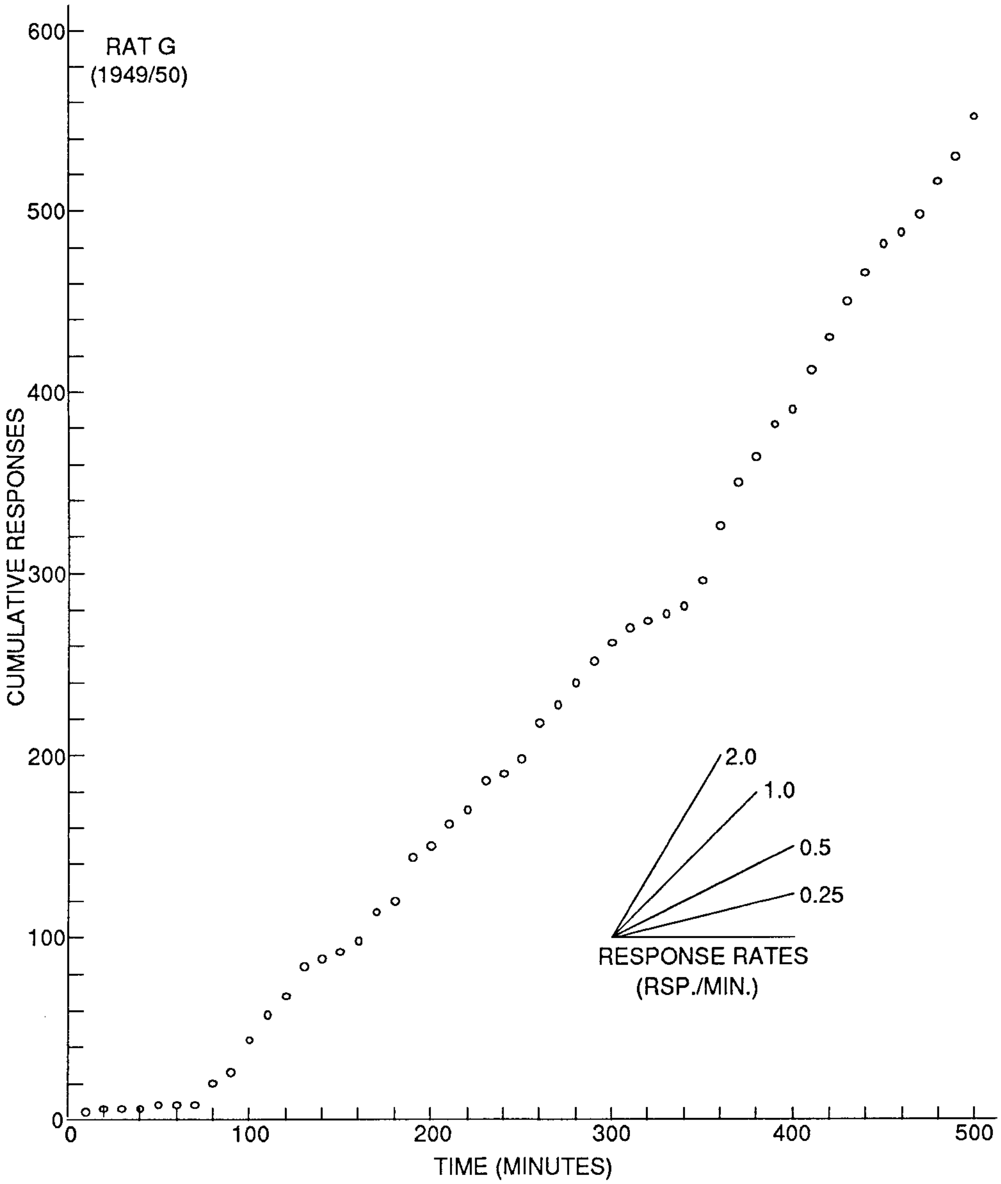

Figure 1. Cumulative record, in 10-min intervals, of Rat G's behavior of pressing a lever. Each time the animal pressed the lever, it postponed the next shock for 22 s. This was the first animal exposed to the free-operant avoidance procedure.

describing what I had done, and proposed that I do my dissertation by following up with a parametric study of the delay intervals. He immediately wrote back that it sounded fine: "Go ahead," he said. I went in to see Keller and told him what I had done, and what I was planning as a dissertation. He said, "Go to it, boy."

And that was my dissertation proposal. I did give a departmental seminar on what I was up to, but by that time, I had more data. In any case, the purpose of the seminar was to inform, not to get permission to proceed. As I went along, I changed procedural and other details. I did not have to ask anyone. The first hour of every session was too variable, so I excluded that hour from the data. When chatting with Jim Dinsmoor about what I was doing, I mentioned that short delay intervals would just make the animals stop responding, so I wasn't going to run those intervals. Jim suggested that I not leave them out. He advised me to get the whole function, and I ended up doing that (Sidman, 1953b). These days, the giver of such important advice would be made a coauthor, but at that time, it was just the way things were done; we took it for granted. I ran three subjects, never dreaming that with the kind of orderliness I had, anyone would ever require me to run more, and nobody did. When I was finished, Schoenfeld and Keller went over my write-up (51 pages of text, but many figures and tables), asked a few discerning questions, and suggested some style changes. That was that until the formal orals.

So began 10 years of investigating avoidance and related topics, and then, after a long break, a kind of return with my recent book (Sidman, 1989).

In discussing events at Columbia in the early 1950s, I have spoken more about other people than about Fred S. Keller. But that is his fault. We talked frequently, both before and after I became his teaching assistant in the undergraduate advanced learning lab. He was always there, but he never played the role of a research director. Avoidance was in the air, and he was responsible for getting it started with his work on light aversion (Keller, 1941), but he was more concerned to make sure that the others got credit for what they had done. When we asked questions, he showed interest and enthusiasm; when he asked questions, he always seemed to be asking them of himself, not us. But we quickly learned that when Keller was puzzled, there were usually good reasons for puzzlement. Nobody else was as good at this as he was, but everybody tried to follow his example. Ideas were shared—questions and answers, data and theory, facts and fancies. It was impossible to tell where most ideas that were in the air actually came from. Columbia was that way because Keller set it up that way. And at 90, he is still that way. His is a tough act to follow.

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