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Notes from the Beginning of Time

Murray Sidman
New England Center for Children

Some remembrances of things past, and their possible relevance to things now. These remembrances include notes about informality, research as a social process, student training and evaluation, research grants, thesis and dissertation proposals, and interdisciplinary collaboration.

Key words: personal remembrances, research environments, theses and dissertations, evaluating students, interdisciplinary collaboration

The suggestion that I recall how things were in the old days startled me a bit—I had imagined that the old days had taken place long before my time. Then, after a little thought—quantitative thought—I realized that I had started college in 1940, only 2 years after Skinner published *The Behavior of Organisms,* the book that started it all. That, of course, was the beginning of Behavioral Time—real time. For me, however, Behavioral Time did not begin until 1947, when I first read the *B of O.* Let me start with some recollections from a few years following that beginning.

It may actually be a bit late for me to give an accurate account of the old days: I have discovered that I cannot remember it all. Something correlated with age does have relevance to behavior. Then again, such causal theorizing—that is to say, attributing my forgetfulness to age—is perhaps to be viewed skeptically. Many years ago, after Rita had reminded me for the *n*th time of various colleagues' names, she told me, "When you are 80, and you tell me you are losing your memory, I am going to remind you that nothing has really changed—you never had one." Indeed, besides making my life possible in all other ways, Rita has also been my hard disk for many years. Therefore, I just cannot guarantee reliable or complete recollections. (That is also the real reason I usually read my presentations.)

Anyway, I am sure that everyone knows people, particularly old professors, who insist that students solve all the same problems they had to solve, even though subsequent developments might have made much of their experience irrelevant in today's context. I do not want to do that. I do not feel that anyone need be exposed to anything I had to go through unless those experiences of mine might provide useful discriminative stimuli or setting events for behavior today. On thinking back, I have been able to pick out a few features of the old days that seem to have changed, but that might again prove useful if we were to reinstate them.

Before we ever used the term *behavior analysis*—before we even knew we were starting a new academic and applied discipline (we thought we were in psychology)—one outstanding feature of our field was a social characteristic, *informality.* We were few enough so that all of us—including students—knew each other, visiting and interacting in our laboratories, our homes, at parties, at conventions, any place we happened to meet, either on purpose or accidentally. We each knew what the others were up to, whether at Columbia, Harvard, Indiana, Minnesota, or Walter Reed—the major centers in the earliest days. Conventions were occasions for catching up with

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Address correspondence to Murray Sidman, 1700 Ben Franklin Drive, Apt. 9E, Sarasota, Florida 34236-2374 (e-mail: murraysidman@comcast.net).

each other's work. There were no poster presentations then; instead, we used to gather in each other's hotel rooms to show our latest data. The invitation, "Come up and see my data," could be taken at face value. It was common for people to go past a room and see a bunch of us crowded around a bed on which long strips of cumulative records were spread out for examination and comment.

We admired each other's work extravagantly and, at the same time, criticized it mercilessly. We could do that because we all knew that we were trying to build something; nobody was interested in tearing another's work down. We could accept criticism because we knew it was coming from friends who wanted to be able to use our data without having to worry about its reliability or validity. We did not yet have theories that had to be defended to the death because they were ours. The data belonged to everybody.

We talked to our professors not just in class but in their offices and when we caught them in the corridors. At Columbia, at least, informal faculty–student interactions were much more usual among the operant people than among others in the department. Exceptions to this were Professor Clarence Graham and his group. Although they worked on problems of vision, they still understood that seeing is behavior. A consequence was that Graham's students developed a respect for our work, and we learned that psychophysics and, by extension, other traditional areas in psychology were fertile grounds for the extension of behavioral principles and techniques of investigation. And so, we were all excited, for example, when Donald Blough, in his dissertation, showed how to get pigeons to tell us when a spot of light became invisible to them, so that he could then plot how a pigeon's visual threshold varies as a function of wavelength (color).

Formal academic requirements varied from university to university; the story of how Skinner was admitted to Harvard with just an interview is well known. At Columbia, where I went through both college and graduate school, the transition from the MA to the PhD program required an all-day written exam—five essay questions. When I took that exam, I found myself unable to answer two of the five, so I wrote essays on why they were bad questions. Probably because I had already been told (before the exam) that I was to have a graduate assistantship, my brash impudence was overlooked and I was accepted into the PhD program anyway. On looking back at that kind of rule bending, I wonder how many excellent prospects today's rigorous, but fixed, academic criteria have lost us. Is it really impossible, these days, to pick out promising students except through exam scores?

I remember a story that Kenneth Spence—the great Hullian behavior theorist from Iowa—told us when he visited Columbia one summer and gave two graduate courses. We learned a lot from him, but we were most proud when we heard later that his experience with us had caused him to pronounce his theorizing applicable only to Pavlovian conditioning and not to operant conditioning. But at one point during that summer, he taunted us by telling us that the only student he ever had from Columbia had flunked out of his Iowa graduate program. He said that the student had come from Columbia with a record of all As, including his psychology courses. After the student flunked out, Spence looked back into the student's folder and reread the letters of recommendation. All of his professors had said that the young man was "a good C student." This, in spite of the consistent A grades in his courses.

I learned a lot from that little story. As behavior analysts, we pride ourselves on our search for quantitative bases for judgment. We insist on data before we are willing to endorse any theory, treatment, educational technique, or evaluation procedure. Here, though, was a case in which unsupported judgments proved more valid than quantitative test scores and course grades. I am sure that many others have seen similar instances. But who in behavior analysis is working to make the critical numbers—the test scores—more valid? Behavior analysts tend to concentrate more on reliability than on validity. Do we really know enough about what our numbers signify?

That question is important not just in experimentation and therapy but in the selection of the people who are going to be responsible for the future of behavior analysis. I am concerned here both with the selection of students and with the certification of practitioners. Our new certification programs are a tremendous advance, and I am all in favor of them. I keep hoping, however, that the certification process will somehow be made self-evaluating. We are going to need data that will tell us, first: Are certified practitioners doing a more effective job than uncertified practitioners? Second: Is one version of the certification process more valid than other possible versions? It is, of course, too early to answer these questions—the certification process is still new—but it is not too early to ask: Does the certification process itself contain quantitative indexes that could lead to increases in its own validity? The numbers must not only be reliable—that is to say, consistent—but must also be valid: They must measure what they are supposed to measure.

As another example of informality in the old days, I will tell a bit about how my own first publication came to be about 50 years ago (1952). To many behavior analysts, that probably seems like way before the beginning of time. I am sure that very few have ever seen that paper; its title was "A Note on Functional Relations Obtained from Group Data." When I wrote it, I was still a graduate student. The informality I am referring to evidenced itself in a graduate seminar conducted by one of my major teachers, Nat Schoenfeld. I had been assigned a presentation topic for that seminar, but when the time came for my report, I was not prepared. I had been spending most of my waking hours following up a hunch about the averaging of group data. If that hunch worked out, it would demonstrate that the day's popular quantitative models of learning could not possibly be correct.

Those theories held that the fundamental individual learning curve had to have the shape of an exponential growth function. All of the evidence in support of this proposition, however, came from curves that had been obtained in classical experiments on learning with groups of subjects. The examples and rough calculations I had worked up were showing that if individual learning curves really did have the shape of the standard exponential growth function, then a curve obtained from group data could not possibly have that shape. That is to say, it looked to me as though the average of a set of individual exponential growth functions could not itself be an exponential growth function. If I was correct, I had a clear disproof of the most popular learning theories of the day. That is why I was so excited that I could not find the time to work on some prosaic seminar topic that I had been assigned.

When the group met, I explained to Schoenfeld that I was not prepared to report on my scheduled topic because I had been working on a problem that seemed to be raised by learning curves that were obtained from group data. I asked him, "Could I report on that instead?" Schoenfeld swallowed hard, looked around at the class members, who all supported me enthusiastically, and went along with my suggested presentation. The discussion was so fruitful and encouraging that I then enlisted the help of a friend who had more mathematical know-how than I did, and wrote up a formal exposition that was published in the *Psychological Bulletin*.

My question now is, "Would I have gotten away with that today?" I suspect not. We have much stronger convictions today that we know what we are about. There are now authoritative textbooks that tell students exactly what they are supposed to learn. Students do not have much opportunity to do anything more than try to absorb what they are told.

Also, students who deviate from their assigned tasks now face another obstacle: They are likely to be viewed as behaving in ways that are incompatible with the objectives of the grant that is supporting them. Somehow, though, most of the discoveries during the "old days" came about without the help of research grants. Would that really be impossible today? Well, I can only say that the last research grant I applied for ran out in 1985. Since then, I have been able to support my research out of my own pocket. I cannot go down to Canal Street in New York City any more to buy scrap components from which to build experimental apparatus—that is what we did as graduate students—but my lab now can be wherever my computer is. I have even run subjects at home. An occasional computer upgrade—to a model that is a couple of years behind the latest version and therefore costs very little—and a few dollars to supply reinforcers to my subjects prove to be enough. (Actually, although I have been unemployed for the past 15 years—that is to say, unsalaried—I have had the benefit of facilities support from the New England Center for Children.)

Furthermore, because I no longer have the grant monkey on my back, and do not have to write and rewrite applications and progress reports to fill the file cabinets of uninformed and unappreciative granting agency administrators, I have time to do research. Indeed, I have time to think. I credit my behavioral history for enabling me to work that way. The informality and the spirit of inquiry that characterized most of our interactions in the old days taught me that rules, once learned, then become breakable, and that the obvious is not necessarily the best. That is true whether we are dealing with experimental procedures, therapeutic procedures, data, theory, or even with our way of life.

Nat Schoenfeld was always unsure that any of us—including himself—really understood why we were doing what we were doing, or why we talked the way we did about what we and others were doing. In class, we spent a lot of time arguing about things that seemed "obvious." For example, if delayed consequences have so little influence on behavior, how did we ever discover the relation between sexual intercourse and childbirth? How did some intellectual giant among the cavemen come to realize that the reason his pet animals and birds always died was that he never fed them? In carrying out experiments, why is an individual curve better than group data? After all, most psychologists argued the opposite—and still do.

We were all taken with Skinner's ingenious and productive experimental methodology, but we had adopted that methodology without having justified it with any rules of scientific procedure. It was something like the way most of us grow up with the English language: We follow the rules of grammar and syntax long before we ever learn to state those rules. That is the way it is with many scientists: They follow the rules of good science without ever having become aware of those rules. In general, such rules have been left largely to philosophers and logicians, many of whom have never performed an experiment and would probably be horrified if they saw what experimental scientists actually do. I have always said that my *Tactics of Scientific Research* (1960) did not set up rules for scientific practice but rather provided a description of how productive scientists act.

I believe the old days taught us, then, that it is important for us to recognize that beginning researchers, like beginning speakers and listeners, even though they cannot articulate rules to justify their procedures, can still produce interesting and valid data. Keep an eye out for the kids who seem to be able to do experiments even before they have learned the grammar—the tactics—of scientific research.

Along those same lines, I have to mention some features of the environment that produced my master's thesis and my doctoral dissertation. The MA program at Columbia gave us two options for completing the requirements: We could either take an extra course or we could do an experimental thesis. My good friend, Donald Cook, and I never thought, though, that we were there to take courses. With some notable exceptions, formal courses were simply the price we had to pay for being able to do what we really wanted to do; we had been captured by the experimental science, and we wanted nothing more than the opportunity to do experiments.

Our major sponsors, however, Nat Schoenfeld and Fred Keller, had not yet gotten into the grant scene, so they did not have ongoing labs into which they could plug students who were in search of thesis and dissertation topics. Along with several of their students who had preceded us, however, they had set up their pioneering undergraduate Psych 1–2 course. That was the first undergraduate course that included an operant research laboratory; all students performed a series of behavioral experiments with white rats as subjects. Because the addition of a required laboratory made the catalogue description of the new elementary psychology course resemble the descriptions of the physics, chemistry, and biology courses, the university then relocated psychology within the academic hierarchy: When my time came, therefore, my diploma read, "Doctor of Philosophy in the Faculty of Pure Science." It did not even mention psychology.

The undergraduate laboratory was, for its time, well equipped, with a set of operant equipment in each of several cubicles. There was a small box full of electrical components, an experimental chamber, a lever, a dispenser for pellet reinforcers, a cumulative recorder, and an ordinary goose-neck lamp that could be placed above the chamber to deliver visual stimuli. Lab assistants had to make the food pellets, squeezing gunk out of a tube and then cutting it into pieces about a quarter inch in length. Liquid reinforcers had to be dispensed manually by dipping a glass rod into a cup of water and then inserting the wet rod through a hole in the side of the chamber so the animal could lick it off. The student experimenters had, themselves, to turn the light on and off to provide positive and negative stimuli, and had to time and record (with pencil and paper) stimulus durations, response latencies, interresponse times, and other numerical aspects of the data. And then, they also had to summarize the numbers into tables and graphs by hand (computers were not around yet).

Today, of course, when students work with nonhuman subjects in the lab course, computers do everything and the student experimenters do not interact with their subjects. I am not sure that the lack of subject–student interaction is a good thing. Skinner always emphasized that good experimentation is a social process, a continuing interchange between subject and experimenter. What each of them does determines the other's next move. I have nothing against computer-programmed experimentation, of course, but I wonder whether students who start that way are not missing something important.

Anyway, Donald and I got permission to use the undergraduate laboratory early in the morning as long as we guaranteed that we would leave it in shape for the first lab session, scheduled for 9:00 a.m. each day. So throughout the year, we came in early enough to run an experimental session and then put everything back into good shape for the upcoming undergraduate lab session. We actually managed to get four experiments done that way. We felt that two of them had not told us very much, but the other two, even though probably not publishable, seemed to us to be usable for our MA theses. So we tossed a coin to determine who would take each one, and we then wrote up our theses. As we expected, these turned out to be not noteworthy, but were acceptable.

I want to emphasize one feature of this whole process: Note the complete absence of any thesis proposal. Keller and Schoenfeld were somehow certain that we would turn out products that, if not definitive, would at least have taught us something, and they let us go our own way. That reinforcer was enough to keep me into research for a long time—much longer, I think, than if I had turned out a publishable piece of work that had been handed to me to help meet the obligations imposed by a research grant.

My PhD thesis started in a similar informal way. Again, there was no existing lab I could take advantage of, but this time, I did not ask; I was not taking any chances. About the time I was ready to start experimenting, Joe Antonitis was just completing his pioneering work on response variability in an isolated room in the basement of Schermerhorn Hall. I waited outside the door of that room as Joe, on his way to teach at the University of Maine, moved his stuff out, and then I took possession of the now-empty space: sqatter's rights. I was soon running back and forth to Canal Street and filling the room with the apparatus I was building to do some work on avoidance behavior.

As an aside, I recall an interesting story about Joe Antonitis. When he went up to Maine, he had to teach three courses, Introductory, Developmental, and Industrial Psychology. In all three of those courses, he used the same textbook: Keller and Schoenfeld's *Principles of Psychology: A Systematic Text in the Science of Behavior* (1950). That is still a great book; try it sometime. One can get it from the B. F. Skinner Foundation or through ABA.

Back to the point of this particular reminiscence, I ran my first animal overnight on an unsignaled avoidance procedure, got a record of marks on a long roll of waxed tape, and took the tape home over my Christmas vacation. When I had measured (with a ruler) the time between hundreds of successive marks on the tape and manually plotted a cumulative record, I found that once the animal had started pressing the lever, its response rate was high and steady enough over several hours to convince me that the procedure had worked. It looked as though avoidance behavior could be learned even in the absence of exteroceptive warning signals. Furthermore, the use of the rate measure could bring negatively reinforced behavior into the same scientific framework as positively reinforced behavior.

Schoenfeld, whose original theory of avoidance behavior had led me to predict the possibility of avoidance conditioning without exteroceptive warning signals, was on a sabbatical leave at Indiana University (where, by the way, he was much influenced by J. R. Kantor; Skinner, too, once said that Kantor had forgotten more than he, Skinner, had ever learned). So I wrote Schoenfeld a note, describing what I and my animal had done, and asked him if it would be all right if I investigated some variables that might affect the rate of avoidance responding under that procedure. He replied quickly, "Sure, go ahead." And that was it—my thesis proposal. (Many years later, he confided in me that he really thought the proposal was far-fetched and would never work.) Fred Keller and Ralph Hefferline, whose earlier work on escape and avoidance behavior had also inspired me to try the free-operant avoidance procedure, went along with no more than a face-to-face conversation about what I was doing.

Is there something different about science today that makes it impossible for students to proceed without a formal plan? Can we no longer trust student experimenters to let their subjects' behavior tell them what to do next? I do not recall seeing any discussions about the pros and cons of requiring formal dissertation proposals before allowing students to proceed. Indeed, I think we may have lost something valuable by insisting on the formalities. The reinforcement for completing a dissertation becomes negative instead of positive; doing experimentation becomes something to be avoided.

The whole conception of the interactive nature of experimentation seems to have fallen into disrepute. Our field's rejection of the notion shows up even in our vocabulary. In the current newspeak, for example, we are no longer permitted to call our subjects "subjects." The term is supposed to be dehumanizing, and so we are supposed to call them "participants." I think this is completely misguided. Experimenters, too, are participants in their experiments. What does making them nonparticipants do to our perception of science and of scientists? Are experimenters merely robots who follow prescribed and unbreakable scientific rules? Are they supposed just to manipulate variables and coldly record the results of their manipulations? Separating them as nonparticipating manipulators and recorders of the behavior of participants really dehumanizes not only experimenters but, along with them, the whole scientific process.

The present-day isolation of behavior analysis from traditional areas in psychology and from other productive areas of science can undoubtedly be traced to many factors, not all of them of the mea culpa variety. But it was not always that way; we were not always isolated. In the early years, we were receptive to extensions of operant conditioning into other areas. I can recall a few of the developments that excited me when I was a graduate student and soon afterward. Donald Cook and I spent a lot of time, for example, playing around with one of Skinner's little-known inventions, the Verbal Summator. This was a record player that would keep repeating a selectable track until we selected a new track. On each track, a set of spoken sounds was repeated over and over—for example, "eye er uh oh, eye er uh oh. . .." The person listening to the sounds—a subject, patient, or client—was supposed to tell what was being said. It was a kind of auditory Rorschach test. We once tried this with a friend as subject; the friend told us that he was hearing, "I am a cuckold, I am a cuckold . . .." Perhaps it was just a coincidence, but we happened to know that his wife was having an affair, and we lost our nerve; we put the Verbal Summator away, hoping that some day we might know enough about the environmental control of verbal behavior to understand what was happening.

Another area that today's students hear little about was Ralph Hefferline's work on the control of overt behavior by internal stimuli that we are unaware of—that is to say, stimuli that give rise to no self-description. Hefferline learned to measure minute levels of proprioceptive stimulation (internal stimuli produced by movements of our own muscles), and then showed that tiny muscle actions that subjects were unaware of—that is to say, did not identify verbally—could nonetheless serve as warning signals for their avoidance behavior. These and other ingeniously designed and conducted studies lend support to radical behaviorism's claim that it is concerned with matters of feeling and thought.

Among other early extensions of stimulus control technology were Thom Verhave's demonstrations that pigeons could serve as reliable quality-control inspectors for drug pills and capsules; Bill Cumming's similar applications of behavioral technology to the inspection of electrical resistors; Jim Holland's demonstrations that radar watchers' detection of characteristically rare signals—like unaccounted-for flying objects—could be improved significantly by occasionally generating signals on their screens artificially, thereby increasing the frequency of reinforcement for detections; and there was Og Lindsley's use of the olfactory capabilities of dogs to design a reliable urine test for pregnancy. These kinds of developments led mainly to largely secret military applications, for example, the use of dolphins to carry out dangerous tasks like the underwater detection and destruction of submarines. If applied in areas like those originally proposed, however, such techniques could go a long way toward increasing the public acceptance of behavior analysis.

The current training of behavior analysts places little emphasis on possible extensions to other basic sciences, but my own background—that is to say, my environment during the old days—set me up for exciting and productive collaboration with scientists in other fields.

For example, for almost 10 years, I was proud to be a member of what was to become one of the most productive interdisciplinary research groups that ever existed: the Neuropsychiatry Division of the Walter Reed Army Institute of Research, headed by David Rioch, the distinguished neuroanatomist and psychiatrist. He had picked out Joe Brady to head the psychology laboratory, and Brady, looking to obtain a Schoenfeld-trained student, recruited me from Columbia. At the time, Brady was an Army major, and the job he hired me for was in the civil service. (A standard civil service joke held that the only way one could tell that a civil servant had died was if he did not get up from his desk at 5:00 o'clock. I hope I did better than that.) Brady was able to hire me right away in spite of a long list of civil service applicants for positions in psychology. Ordinarily, each one on that list would have had to be interviewed. He got around that delay by writing up a new job description, one that required a person whose background included a PhD dissertation on free-operant, nondiscriminated avoidance behavior. Somehow, Joe was always able to make the system behave the way he wanted it to.

When I arrived at Walter Reed, I found all kinds of different laboratories and offices crowded into a small space—neurophysiology, audition, vision, neuroanatomy, chemistry, neurosurgery, and others. You could not get to one person's area, or even to the bathroom, the animal quarters, or the elevator, without walking through space that belonged to someone else. To get to my own desk, I had to walk between John Armington (who worked in the psychophysics of vision) and his desk. We all had to be friendly, if only to maintain a reasonably peaceful community, but because of the quality of the researchers, the crowding produced a more important outcome. In the process of scrambling through each other's territory, we could not avoid seeing interesting things going on. We stopped, asked questions of each other, and became interested enough in what we saw and heard to suggest possible collaborative interchanges. It turned out that with such a competent group of investigators sitting practically in each other's laps, interdisciplinary collaboration became inevitable. I know this is contrary to current wisdom, which states that productive researchers need their own space, with the opportunity to work undisturbed, but I offer the Walter Reed story as a bit of counterevidence.

One reason I was pleased to take the job at Walter Reed was that I knew of Brady's dissertation research, another provocative extension of operant methodology, this time into a clinical area. Brady, sponsored by Howard Hunt at the University of Chicago, had done a remarkable series of experiments in which he demonstrated, first, that electroconvulsive shock therapy can seem to cure conditioned anxiety. He used a technique that Estes and Skinner had introduced. Estes and Skinner showed that a warning signal for unavoidable foot shock comes eventually completely to suppress all of a rat's productive behavior during the signal. When the signal comes on, the animal stops all positively reinforced work. It freezes in position, defecates, urinates, trembles, shows piloerection, and so on. The signal produces all of the hallmarks of intense, paralyzing, debilitating anxiety. In Brady's experiments, animals that had learned the Estes–Skinner conditioned anxiety response were then given a series of standard electroconvulsive shocks. After the shock therapy, the animals were put back into the operant chamber, but now, in the presence of the warning signal, they no longer showed either the conditioned suppression of productive activity or any of the autonomic responses characteristic of anxiety. Instead, they continued working both between and during the warning signals. Their anxiety seemed to have been cured.

The second thing that Brady showed, however, was that only a few weeks later, the anxiety returned; the "therapy" was not permanent. That, and other findings, emerged from a long series of studies, and I was happy to become involved, although my contribution consisted largely of helping to refine and maintain the experimental apparatus that Brady had originally designed to run up to six animals at the same time—a remarkable advance in experimental technology.

Getting into this kind of "hard-nosed" application of behavioral know-how to a problem that was originally generated in the clinic not only whetted my own appetite for such collaboration, but, what is probably more important, did the same for others who worked in that environment either before or after their formal academic training. As a consequence, the Walter Reed behavior lab turned out a number of researchers who not only became known in behavior analysis but were also responsible for major advances in other areas. For example: Irv Geller, who made lasting contributions to the screening of "psychoactive" drugs; Larry Stein, who has done groundbreaking work in behavioral pharmacology and in the application of operant conditioning to the behavior of individual cells; Bill Stebbins, who provided an enduring model for the application of behavioral stimulus control techniques to problems of hearing and auditory physiology; Bill Hodos, whose neuronanatomic atlas of the pigeon brain is a classic; John Boren, whose many accomplishments include major methodological contributions to behavioral pharmacology; Eliot Valenstein, whose criticisms of psychosurgery and of loose theorizing about drugs and mental illness were early entries into the developing field of ethics in science. The list could go on. The point of this recital is that if we were to resume some of the kinds of collaborative research enterprises that used to characterize our field, thereby exposing young basic and clinical investigators to the reinforcers that such collaborations make available, we might help behavior analysis break out of its current isolation.

While at Walter Reed, I had the privilege of collaborating with first-class scientists in several fields. Bob Galambos, for example, a foremost researcher in the neuroanatomy and physiology of hearing, provided guidance in matters of scientific procedure and interpretive logic that helped keep us on the straight and narrow in our ventures into unfamiliar kinds of collaboration. More directly, I worked with Brady and others in the exciting early days of the new science of behavioral pharmacology. In addition to some notable findings that emerged from our own work, I can recall the thrill we all felt when we learned of Peter Dews' demonstrations that reinforcement schedules helped to determine the behavioral effects of drugs; the same drug could either increase or decrease a subject's response rate, depending, for example, on whether the behavior was being maintained on a fixed-ratio or a fixed-interval schedule. This was heady stuff. It was also a kind of basic research that broadened the scope of behavior analysis.

In our own group, we also did some

of the early follow-ups of Jim Olds' fascinating discovery that stimulation of certain areas of the brain via implanted electrodes would reinforce behavior that produced the stimulation—the phenomenon called *intracranial self-stimulation*. Again, we showed reinforcement schedules and other behavioral variables to be determiners of the effectiveness and strength of this anatomic and physiologic effect—one more extension from the basic behavior laboratory into another part of the world. We worked with the renowned neuroanatomist Walle Nauta on interactions between brain lesions and schedule-controlled behavior. Collaborating with the endocrinologist John Mason, we not only found interactions between behavior and pituitary-adrenal cortical activity, as evidenced by Mason's exquisitely controlled measurements of plasma 17-hydroxycorticosteroid levels, but we were able to show that the effects could go in both directions, from behavior to steroids, and from steroids to behavior: experimentally demonstrated psychosomatic effects. Much other exciting research during those days maintained connections between behavior analysis and the rest of the world: Brady's research that involved completely controlled 24-hr human environments, derived from Jack Findley's original basic studies with pigeons, led to his involvement in the training of both nonhuman and human participants in space flights; Bob Schuster's pioneer work on the self-administration of drugs by nonhuman primates revolutionized the screening of drugs for addictive properties.

These and other successful collaborations demonstrated that the kinds of behavior and behavioral variables we had been looking at, and the kinds of behavioral quantification we engaged in, were not just artificial and meaningless. Their applicability constituted direct evidence of their relevance to a wider universe than our own laboratories.

Our interactions with other basic sciences, however, have slipped. As a result, our students are losing contact with the basic behavioral science in which their applications are rooted and from which future applications are to be derived. I think it has been just as damaging internally that we have lost standing outside, in the general world of science, among biologists of all kinds, neural scientists, geneticists, and so on. Their respect, if we could regain it, would provide a solid starting point for support by society in general. There is a two-way street here: Our basic scientists need to reestablish productive collaborative ventures in other areas, and our applied scientists need to turn more to their basic science for data, principles, and procedures. If applied behavior analysis fails to respect its own basic science, behavioral practitioners will find that the rest of the world will fail to respect them—applied behavior analysis will lose its status as a desirable career path.

I hope I have not just been echoing the stereotypical old-timers' cry, "Nothing is as good as it used to be." That has not been my intention. Today, of course, we have many successful data-based applications of behavior-analytic science to areas such as mental retardation, autism, education and training, psychotherapy, industrial productivity, safety in the workplace, and so on. These are advances to be proud of. Many things about today are better than they ever were in the old days. We cannot, however, just maintain the status quo. I have stated many times that when we have stopped learning, we have died, even though our hearts and some parts of our nervous system may still be functioning. I think, though, that we might be even better off now if we continued to value some of the ways things used to be, and to follow up some of the early advances that were left uncompleted.

Like individuals, a culture, too, forgets things as it ages. An examination of history can reveal not just mistakes that should now be avoided—sources of negative reinforcement—but can also remind us of forgotten, but productive, pathways that would lead to positive reinforcement if we continued to follow them.

And so, I hope that this little return visit to some of the events that took place near the beginning of time will help add to the large bag of positive reinforcers that we already have available.