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THE ANALYSIS OF BEHAVIOR: WHAT'S IN IT FOR US?

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When we publish behavioral research, we are not allowed to communicate the thrill, the poetry, or the exhilaration that are outcomes of the discovery process. Yet, these are among our most potent reinforcers. Explicit recognition of the emotional accompaniments to research could help attract students into the experimental analysis of behavior.

Key words: avoidance, free-operant avoidance, equivalence, cumulative record

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The term 'experimental analysis of behavior' does not just summarize a set of behavioral facts and theories. It is also a name for a set of behavioral repertoires; it summarizes features of the behavior of behavior analysts. And, as we all know, if we want to understand what anyone does, we have to identify the reinforcers for their acts. What are the reinforcers for the behavior of behavior analysts? What keeps them going?

Do basic researchers, applied researchers, and practitioners experience different sets of reinforcers? When asked, ''What's in it for you?'' do they each have different answers? Well, yes, they do different things and produce some obviously different consequences. Applied researchers and practitioners, for example, rarely refine the science's systematic principles. Basic researchers rarely bring about improvements in a particular client's troubling or troublesome behavior. Even though they display different response repertoires, however, they still have many reinforcers in common. I believe that a functional analysis—the same kind of functional analysis that tells us why our clients and subjects behave as they do—would reveal many reinforcers that are similar for researchers and practitioners. A more explicit and more general recognition of their reinforcer similarities would perhaps help bring workers in these seemingly disparate kinds of activity into more harmonious relationships.

The kinds of reinforcing consequences I want to emphasize here are not the obvious ones. Much has been written about such matters as salaries, promotions, titles, power, fame, prizes, and so on. Less often discussed are some consequences of scientific activity that are difficult to observe and almost impossible to measure. Even worse, these kinds of consequences seem to be disappearing as major determiners of the conduct of behavior analysts. In trying to enumerate those reinforcers, I will have to appeal largely to my own experiences, because those are the only ones I have been able to observe directly. I cannot believe, however, that other behavioral scientists have not been sustained by the same kinds of reinforcers that it has been my good fortune to experience. There are many who could surely tell the same kinds of stories I am going to tell. I wish they would. I believe that today's young investigators and practitioners are in special need of hearing about those experiences.

Let me summarize my thesis in advance. In our scientific writing about behavior, we fail to transmit the excitement of doing research. We rarely describe the thrill of finding out things no one knew before. Although the prevalent public conception is that scientists are cold, logical creatures, it is easy to demonstrate that scientists are also lovers of worldly pleasures. They are often, for example, quite sophisticated appreciators and even participants in the worlds of music, literature, and the humanities in general. What scientists seem reluctant to acknowledge, however, is the poetry in what they themselves do, the poetry that is intrinsic to the process of discovery.

Nobody acknowledges the musical features that are inherent in the process of reasoning, in the logical progression of thought. A dictionary definition of music is, ''The art of arranging sounds in time so as to produce a continuous, unified, and evocative composition.'' One could apply this definition almost word for word to the progression of an

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experimental investigation: ''The art of interacting with an experimental subject so as to produce a continuous, unified, and evocative study.''

We also fail to reveal the passion with which we try to distill orderliness out of chaos, and the exhilaration we feel in the discovery of such orderliness. And although we try to avoid superstition and unverifiable doctrine, we nevertheless come close to religious fervor when we succeed in placing the conduct of human beings—what humans do and why they do it—within the realm of natural phenomena, thereby bringing the behavior of living beings, including ourselves, into the grand scheme of order in the universe.

People have little problem understanding the reinforcers that are available to behavior-analytic practitioners. Curing the sick, turning nonlearners into learners, getting people to stop smoking, to eat less, to practice safe sex, increasing safety and productivity in the workplace—all of these accomplishments and many others are generally recognized not only as socially worthwhile but also as emotionally satisfying. Researchers, however, even many applied researchers, have not been as successful in conveying to others some notion of the reinforcers that are inherent in their work. Because scientists must evaluate data dispassionately, people mistakenly assume that they are dispassionate also about the implications of their data for human life.

In view of the popular misconception that scientists are detached and uncaring, I may perhaps be excused for feeling some pride when a former student dedicated her book ''to…Murray Sidman for proving to me that being scientific and data based is the operational definition of caring.'' These days, we seem not to be passing along this definition of caring. As a result, many potential students, as well as the general public, turn away from a science of behavior because it seems cold and uncaring. Many who go on and do become behavior analysts are not only turning away from research but are coming to devalue it—basic research for sure, but even applied research.

Changing the World

How did I get to the point where I experienced poetry, music, and passion in the experimental investigation of behavior? Like many young people, both then and now, I was worried, not so much about what kind of a job I was going to end up in, but rather, how I was going to go about helping to change the world for the better.

My readings and other observations had convinced me that people create their own world. Therefore, if the world was going to change, people would have to change. Considering the intensity with which people seemed bent on subjugating or destroying each other, even on setting up the conditions for eventual self-destruction, it was clear to me that changes were going to have to be engineered deliberately, not left to the slow pace and uncertain outcome of natural evolution. What kinds of changes would do the job? How were those changes to be brought about? Was change even possible? In college, none of the many sciences I looked into suggested practical answers to those questions until I found myself in the pioneer introductory psychology lab that Keller and Schoenfeld were initiating at Columbia University back in the late 1940's (Keller & Schoenfeld, 1949).

Creating behavior. There, in the very first lab session, I found myself creating behavior. Without any words being exchanged between me and my experimental subject, that little white furry animal was doing exactly what I told it to do—things it had never done before, things that gave it no evolutionary advantage, and even more incredibly, exactly what the lab manual said the animal was going to do when I set up specified contingencies.

As we moved along in the course, I was able not only to get that little beast to press its lever and pull its chain, but to stop whenever I turned on a light; to work rapidly, slowly, or cyclically as I changed the reinforcement schedule; to press or pull with a force greater than its own body weight; to work for money and then use its money to get food from a slot machine; to tell me whether it wanted food or water; and much more. To belittle my excitement at all this as merely the aberration of a control freak would miss the point. Who cares about controlling the behavior of a lab rat? That the experimental organism was so insignificant made the demonstration impressive. If one could communicate so effectively with such an intellectually impoverished creature, what might one accomplish with human beings who were capable of so much more?

Here was a whole new universe opening up for exploration! Behavior could not only be changed, but could be changed in specifiable and measurable ways by specifiable and measurable operations. Yes, those lab operations and measurements were simple. They did not nearly get at the kinds of problems that made me feel that the world needed changing. This was obviously just the beginning. A good deal of the excitement came from the realization that there must still be much to find out, much to bring into the laboratory and learn more about, much to extrapolate into the world outside the lab.

That experience set me on my life's path. I know that happened to many others; I have heard similar stories countless times. Today, however, few students have the opportunity to discover for themselves that the behavior of an intact being is changeable in subtle but predictable ways by operations that are just as specifiable as those that change the behavior of their internal organs and the behavior of inanimate objects in the world around them. That is a tragic shame, because the world of the future will be in the hands of those young men and women. They are quite aware of that, no less so than those of us from another generation were when we were young. But they are exposed these days mainly to verbal accounts of other people's discoveries. Many who would have become exhilarated by their own first success in shaping behavior in the laboratory turn instead in other directions to make their existence count.

Discovery

I would guess that B. F. Skinner experienced similar reactions, although his were probably even more intense than mine because he had no lab manual to set him off. What he concluded about his earliest work, which began as the investigation of eating reflexes, shows that even wider considerations than the significance of his contributions to the understanding of food ingestion were the source of his reinforcers. He describes what he did as follows:

> Pellets of food of uniform size … were prepared … . The rate of eating could then be expressed as the rate at which such pellets were taken up and eaten by the rat. Such a rate may be recorded in the following way. The rat stands on a platform and obtains pellets by pushing inward a light door hanging in the opening to a pocket at one edge. The door is counterbalanced and moves with ease. The food is placed below the level of the platform so that the rat must withdraw from the tray before eating. Each time the door is opened, a contact is made and recorded in the usual way. (Skinner, 1938, p. 343)

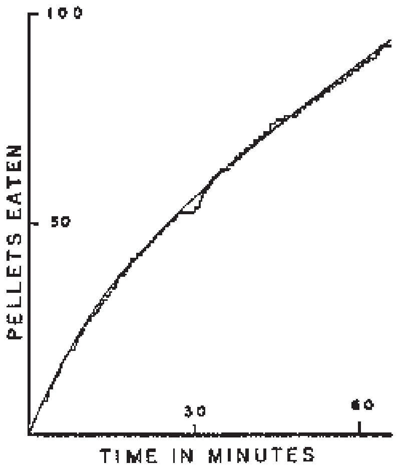

Fig. 1. Cumulative eating curve, from Skinner, 1938.

Figure 1 shows one of the cumulative eating curves that Skinner published (1938, p. 345), with the comment that it is ''not exceptional.'' He meant that the empirical curve was easily reproducible. He fitted a curve to it that he demonstrated in several ingenious ways to be quite general. The curve shows a continuous picture of the rate at which an individual animal obtained and ate pellets over a period of about an hour, a picture that nobody had ever seen before. It could easily have marked the beginning of a lifetime of research on food ingestion; of attempts, perhaps, to validate a mathematical model based on the original fitted curves, and with the recruitment of scores of students devoted to the perpetuation of that model. Where would we behavior analysts be now if he had gone in that direction? Skinner's background had somehow prepared him to see something more general in his data. In his words:

> The value of the present demonstration lies, I think, in its bearing upon the lawfulness of behavior. … Under other experimental conditions it should be possible to give a similar quantitative treatment of variations in reflex

> strength by appeal to the variables that are responsible for the change. (p. 350)

Incredible words, not just in 1938 but even now, when students rarely hear them or their equivalent even from the few behavior analysts with whom they might come into contact. Incredible words, generalizing from the dry observation of regularity in the behavior that leads to food ingestion to the prediction that similar regularity will be seen in controlled experiments on behavior in general. The consistency and regularity of his data convinced him that he was seeing something that would apply to any behavior that any organism could perform, something that also would be happening in the world outside the laboratory. He concluded that the laws of behavior are general, that the laboratory, with all of its restrictions, is not an artificial world; it is simply a rarely visited part of the real world.

Can such a conviction have been unaccompanied by excitement, passion, and exhilaration? Skinner wanted to find a science of behavior. He was ready when he broke the first ground, just as I and others were ready when we discovered what Skinner said we would discover if we manipulated relevant variables and measured the resulting changes in behavior, just as today's students who are ready to change the world would discover if we gave them the tools and let them experience the reinforcers that go with discovery, that go with the recognition that one's dreams are actual possibilities.

The Search for Order in Nature

When we publish our research findings, we are not allowed to communicate the thrill of research, the poetry in the discovery process, or the exhilaration in the discovery of order. I wish some of that were permitted. True, it would not add to the logic of our demonstrations or give valid support to any particular conclusions or conjectures. Still, some expression of the emotional ''vibes'' that research generates could help to attract potential contributors to the experimental analysis of behavior. Students might appreciate that in performing behavioral research they could encounter something more than methodology and analytic techniques. They might receive a hint that feelings just as strong and fierce as those they experience when interacting with people can also characterize interactions with data.

One does not have to open up a whole new field of investigation to experience an emotional payoff in doing research. It helps, I think, to have had a background that makes it important to place one's work in a broader context than its immediate results, or that makes one open to the excitement of practical or intellectual challenges. No particular payoff can be promised. In my student days I had become convinced—I credit Freud for that conviction—that many problems usually classified as psychiatric were the result of individuals' behavioral histories of punishment and negative reinforcement. I therefore wanted to bring components of such histories into the laboratory for more precise study. At the time, Keller, Schoenfeld, and Hefferline at Columbia were advancing new conceptions of avoidance behavior. For me, their formulations led to the possibility of a new lab procedure that could reveal as yet unexplored features of avoidance behavior. It might, perhaps, even permit a more effective approach to psychiatric problems. Let me share with you some of my early experiences.

That new procedure presented mild electric shocks to an animal's feet periodically, without warning the animal when a shock was about to occur. By pressing a lever at any time, however, the animal could postpone the next shock that was due. The more frequently it pressed the lever, the fewer shocks it received. If, for example, the shock was scheduled to come every 22 seconds, the animal could keep shocks away completely by never waiting as long as 22 seconds without pressing the lever. The first question was, ''Would the procedure work? Would the animal learn to press the lever?''

It took quite some time to set up the procedure to run automatically. We used relay circuitry at that time; computers as such had not yet been invented. By the time I was ready to try the procedure with my first subject, it was already the night before I was scheduled to go home for the Christmas holidays, but I couldn't wait. Though it was very late, I placed the first animal in the experimental chamber, turned on the apparatus, and stayed just long enough to make sure the apparatus was working as it was supposed to. The next morning, I returned to the lab and found

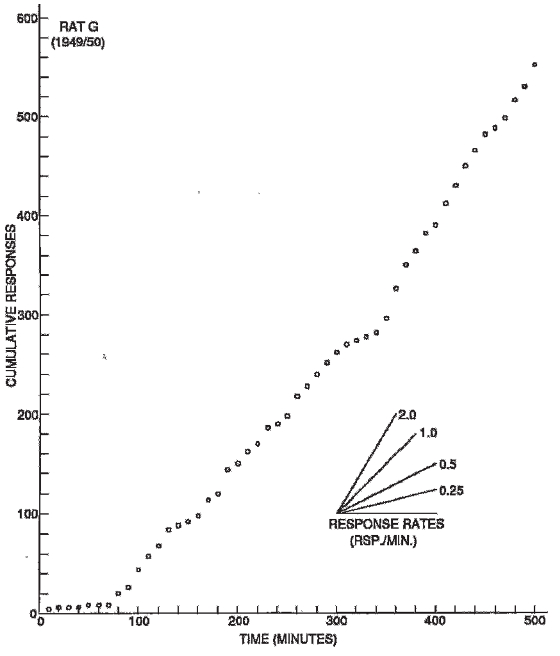

Fig. 2. Cumulative record, in 10-min intervals, of Rat G's behavior of pressing a lever. Each time the animal pressed the lever, it postponed the next shock for 22 s. This was the first animal exposed to the free-operant avoidance procedure, in 1949.

the animal pressing its lever fairly frequently, but if I had stayed to get a better impression of its rate, I would have missed my train to Boston. So in spite of my excitement, I turned off the apparatus, put the animal away, rolled up the waxed-tape record that would show me when each shock and each response occurred (I did not yet have a cumulative recorder), and hurried off to the railroad station.

As soon as I could free up a block of time after arriving home, I sat down with the tape record, a ruler, and graph paper. After spending some hours measuring the distance between each mark on the tape and converting the distances to times, I drew a cumulative record manually. Figure 2 is what that first animal, Rat G, showed me.

Does anyone think I looked at those data dispassionately, that I just coldly entered numbers into a table and then unfeelingly transferred the numbers into a graph? Was I just mechanically going through the standard routines that the textbooks say differentiates scientists from nonscientists? No, you can probably empathize with me when I say I was floating on cloud nine for the rest of my vacation. I knew, first of all, that I was seeing something that nobody had ever seen before—the record of an animal successfully avoiding shocks even without any warning signal to tell it when a shock was imminent. Did I sit there worrying about methodological problems? For example, did I feel that I needed more subjects to convince me that the effect of the procedure was real? That problem never arose; I knew that rats did not normally spend their time pressing levers, even at a slow rate, over a period of several hours. This effect was real. If the next animal did not give the same results, I would just have to find out why—in its own right, a potentially exciting prospect.

My imagination, of course, was active. It was clear to me that two variables had to be evaluated: the rate at which shocks were delivered if the animal did not respond, and the amount of time that the animal postponed shocks when it did respond. This was so obviously going to be my dissertation research that I later presented the idea to my sponsors in just about those same words, without a formal proposal. They went right along with me. How many students today—when the best of them, especially, are recruited into grant-supported research projects—are ever given either time or opportunity to do their own research, to exult in their own discoveries and gain enough independence to plan their own subsequent research?

It also was clear to me that the procedure provided a way to integrate aversively controlled behavior into the operant framework, along with positively reinforced behavior. That is why my preferred name for the procedure was ''free operant avoidance.'' I was already formulating a long series of experiments directed at that systematic goal, a research program that occupied me for much of the next ten years. I never did write the book that I hoped might be a sequel to ''The Behavior of Organisms,'' but I was able to summarize much of the work as a book chapter (Sidman, 1966). Achieving that kind of systematic integration involves more than the quiet satisfaction of getting papers published, or the economic advantages of academic promotion, or even the gratification that comes from professional recognition. Reasoning is akin to singing; the logical progression of thought in planning and carrying out an integrated research program resembles, to me, the composition of a piece of music. The course of my research has generated moments when I burst into song (but only when I was alone), and other moments when, instead of composing music—which I do not know how to do—I turned on a piece of recorded music that seemed to me to match what had just happened in the lab.

I also was aware, right from the start, that free operant avoidance had to underlie many real problems outside the laboratory. That exposition, however, had to wait for more data. It did eventuate in the *Coercion* book many years later (Sidman, 2000). The material in that book is clearly not just of academic interest to me. My feelings show in ways that our standard data presentation does not allow. Readers can tell—and listeners can, too. I have never been an advocate of the lecture system of instruction, and my own course lectures probably reflected that disposition. On one occasion, however, after I had delivered a lecture based on the *Coercion* book, which I was just writing at the time, a couple of undergraduate students came up to me afterwards (in itself, an unusual occurrence) and said outright, ''You seemed to be much more involved than usual in your material today.'' Students can tell. Methodology is important, of course, but the significance of research, and the extent to which it generates personal involvement, is critical. We should give students more opportunities to see that in us.

The moments of exultation became even more frequent when I began doing experimental investigations with people as subjects. One of the earliest was with a severely retarded man about my own age. In those days, his medical diagnosis was ''microcephalic idiot.'' He had no language, was able to indulge in a few simple pleasures like tossing and trying to catch a ball, drinking and eating, and stringing beads—probably the most complicated thing he had ever learned to do. He appears with me in Figure 3.

Working with him, my collaborators—especially Larry Stoddard—and I were able to develop teaching techniques that were so widely generalizable that we came to wonder if even his name, Cosmo, did not foretell a special role for him. We had developed a stepwise fading program to teach children errorlessly to discriminate circles from ellipses. The day Cosmo went through that program successfully, I went home and listened to Sousa marches, imagining Cosmo and me leading the band down the street. Why are we so unwilling to let people know that laboratory work can generate such reactions?

Later, we were able to adapt the circle-ellipse program to make the ellipses gradually become more and more circular, and thereby determine a discrimination threshold. We did this successfully with Cosmo in front of a group of site visitors for a research grant we were applying for. The successes of our methodology had generated such confidence that we were willing to take the chance. (That grant was funded enthusiastically.) Such cockiness is not one of the touted virtues of the scientific enterprise, but it can be part of the picture. Students should be aware of the possibility.

As many practitioners know very well, one of the joys that comes from working with the same person for a long period of time is the affection—the mutual affection—that often develops. That happens in the laboratory, too. One of the features that I prize the most about the photograph is the sight of Cosmo's fingers, indicating that he had placed his arm about my waist. Unlike his home environment (institutional), he was almost always successful in our lab. Here, he could feel unafraid and secure. When a data-based approach to teaching can generate such personal satisfactions, others should be let in on the secret. Science produces more than theories and data.

Another one of my big moments came when I first saw what we later realized were equivalence relations but which, at the time, we saw as an experiment on reading comprehension. I have told this story many times so I will skip most of the details. Our subject was Kent, a severely retarded boy who showed no evidence of being able to understand written or printed words. For example, he was completely unable to match the printed word car to the picture of a car, or words like dog, cat, ear, hat, and so on, to their corresponding pictures. The critical part of the experiment was an attempt to teach such simple reading comprehension in an indirect way—that is to say, by teaching something else, instead. I was not optimistic. If the method were to succeed, it would seem like magic.

Here is what we did. Instead of teaching him directly to match printed words to pictures, we

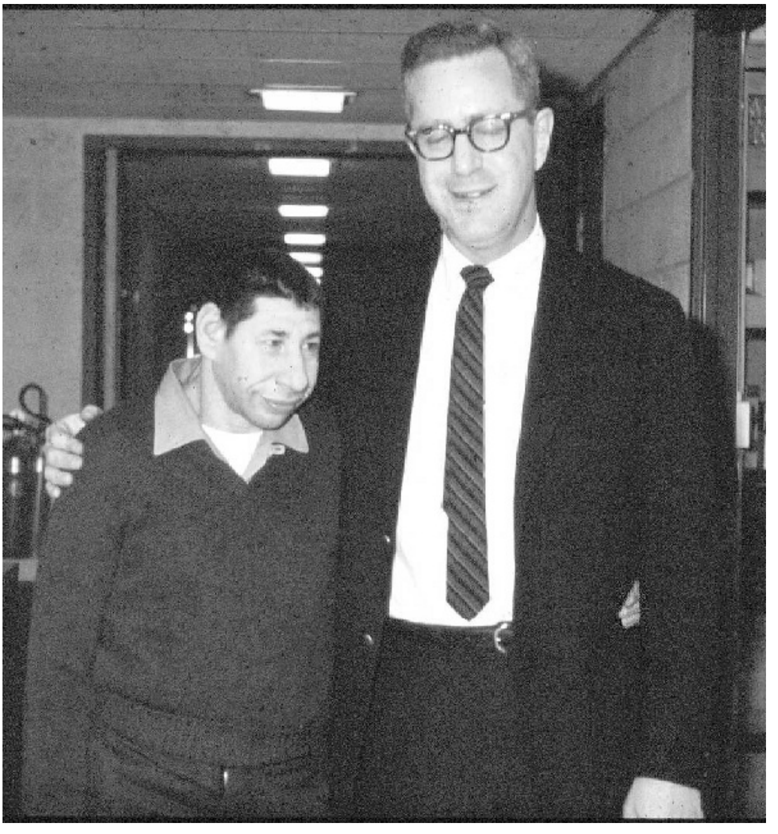

Fig. 3. Cosmo and author, 1964.

taught him to match dictated words – words that we spoke to him – to match these first to pictures and then to printed words. Altogether, we taught him to match each of 20 auditory words to its picture and to its printed counterpart. For example, when he heard the word ''car,'' he learned to select a picture of a car and not any other picture; when he heard ''hat,'' he learned to select a hat picture and no other; and so on with 18 other word-picture combinations. Then we taught him to match each of those same 20 dictated words to its printed counterpart. For example, when he heard ''car,'' he picked car and no other printed word; when he heard ''hat,'' he picked hat and no other word; and so on with 18 other dictated word–printed-word combinations. It was fairly easy to teach him to match dictated words to pictures because he already knew many of those auditory-word to picture correspondences, but it took a month to teach him to match each of the 20 dictated words to its corresponding printed word.

Then came the magic moment. With Kent now matching dictated words both to pictures and to printed words, we repeated the reading comprehension tests in which he had to match the printed words with their corresponding pictures, tests that Kent had been completely unable to do before. As the tests progressed, we could not believe what we were seeing. Trial after trial, Kent correctly matched the car, the cap, the cat, the box, the cow—each of the 20 pictures—to its printed name, and each of the 20 printed names to its corresponding picture. The lab technician, sitting behind Kent in the experimental room, could hardly contain himself. At the end, he leaped up, grabbed the boy in a bear hug, and shouted, ''Dammit, Kent, you can read!'' Outside the room, where the rest of us were watching through a one-way window, I was dancing the twist; my son, who happened to be in the lab at that moment, said to me later, ''Dad, I've never seen you like that before!''

Well, moments like that do not require an original discovery. We went on later to use the same method to teach the correspondence of colors to printed color names, numbers to number names and quantities, upper-case to lower-case letters, and many others. The thrill has never diminished. Although it takes more preparation, this way of creating new performances is, in some ways, even more exciting than is response shaping. Students respond to it in the same way.

Practitioners, of course, have this kind of experience all the time, as they successfully create adaptive behavior to replace maladaptive behavior. What many of them do not realize, however, is that experimenters, too, even in controlled laboratories, have the same kinds of experiences. Those supposedly cold, emotionally sterile laboratories create plenty of heat.

Conclusion

I wondered, in preparing this article, whether it would make readers say, ''What has happened to Sidman? He seems to have gone soft.'' That can happen when one becomes interested in what behavior analysts call ''private events,'' which is really what I have been talking about. I was, therefore, delighted to read a statement by Skinner, as related by Charlie Catania:

> Private events … remain inferences to the experimenter or philosopher, but they are just as directly observed by the person in whose skin they exist as any environmental stimulus (Catania, 2003, p. 317).

Private events are real. Behavior analysts experience them, just like everyone else. Somehow, those private events become reinforcers, sometimes positive and sometimes negative—how that happens needs to be looked into. But just as they reinforce other endeavors, they also reinforce the behavior of behavioral scientists. They, too, are one of the fruits of our science—part, at least, of what's in it for us. I think it is important that we let people know that. It is especially important that we let students know.

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