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Comments on the 1950s Applications and Extensions of Skinner's Operant Psychology

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These comments address Laties', Dewsbury's, and Rutherford's papers on the extension and application of Skinner's operant psychology during the 1950s. I begin by reflecting on the papers' overall theme—that the success of behavior analysis lies in its practical applications—and add some comments on Planck's principle. I then turn to the three papers and address such topics as (a) other applications and extensions (e.g., the U.S. space program), (b) relations between the research and researchers at the Yerkes Laboratories of Primate Biology (e.g., a Yerkes' researcher in Skinner's laboratory), and (c) human schedule performance (e.g., continuity and discontinuity with nonhuman behavior). I end with a discussion of the fundamental reason for the success of the extensions and applications of behavior analysis—the experimental analysis of behavior.

Key words: history, operant psychology, experimental analysis of behavior

The preceding papers by Laties (2003), Dewsbury (2003a), and Rutherford (2003b) are original in their research and scholarship, enlightening about actors and actions, and instructive concerning the extensions and applications of operant psychology in the 1950s. They are so excellent that I have only a few substantive remarks to make regarding their contributions to the history of operant psychology and to the history and discipline of behavior analysis more generally. However, some related points are worth noting and several are worth clarifying, while others warrant elaboration. Before proceeding, though, I comment on the papers' overall theme.

THE THEME

Dewsbury (2003b) described the theme this way: "Although the success of competing paradigms is often thought to be a function of their theoretical characteristics and effectiveness in understanding and predicting phenomena, their applicability to a range of diverse, and often practical, situations can be at least as important" (p. 233). More specifically, with respect to operant psychology during the 1950s, "The broad applicability of the approach was a major reason for its later success" (p. 233). Depending on how *success* is defined, I agree. However, let me suggest another reason for success among the competing paradigms and approaches, a nonintellectual reason: Success may sometimes be due to the length of scientist's career. This is a special case of Planck's principle: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather its opponents eventually die, and a new generation grows up that is familiar with it" (Planck, 1949, pp. 33–34; see Hull, Tessner, & Diamond, 1978; Simonton, 2002, pp. 270–274).

The case in point is, of course, Skinner (1904–1990), because in addition to his fundamental contributions to psychology, he outlived his two main rivals in neobehaviorism. By the end of the 1950s, they were dead: Hull (b. 1884) passed away in 1952, Tolman (b. 1886) in 1959. Also to pass away that decade were Wittgenstein (1889–

A chronological listing of B. F. Skinner's primary-source publications during the 1950s is provided in the Appendix. I thank Donald A. Dewsbury for inviting my participation as the discussant for the symposium in which these papers were originally presented, as well as for his constructive comments on the present paper.

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1951), Dewey (1859–1952), Hunter (1889–1954), Yerkes (1876–1956), Lashley (1890–1958), Watson (1878–1958), and Guthrie (1886–1959). Skinner also outlived his closet rival in the 1960s, Spence (1907–1967). Spence had earned a Distinguished Scientific Contribution Award from the American Psychological Association in 1956, the 1st year it was awarded—2 years earlier than Skinner (Coleman, 1982). In addition to outliving his rivals, Skinner also remained professionally active until his death. Between 1960 and 1990, he published more than 115 of his nearly 185 articles, chapters, and books, amounting to over 60% of all such publications in the course of his career.

This special case of Planck's principle, which itself awaits confirmation (Levin, Stephen, & Walker, 1995), suggests a new line of research. Although literatures already exist on relations between (a) chronological age and eminence and (b) life span and eminence (Simonton, 1990, 2002), research on career longevity and success—not just eminence—is nearly nonexistent.

OPERANT PSYCHOLOGY AND THE GROWTH OF BEHAVIORAL PHARMACOLOGY

Turning to the three papers, we see that Laties (2003) has offered new data and insights on the emergence and growth of behavioral pharmacology in the 1950s. These are contributions that only he could make—he was an early contributor to the field and today has a prodigious memory and meticulous records (Laties, 1986; see also Dews, 1981; Pickens, 1977). I have little of substance to add to his history; thus, I (a) address a minor point about the role of the pharmaceutical companies in the early history of behavior analysis and (b) suggest some other 1950s applications of operant psychology worth historical treatment along the lines of Laties' paper.

Pharmaceutical Contributions

The pharmaceutical companies benefited directly from operant psychology during the 1950s, in particular, from its ability to produce steady-state behavior, which allowed carefully controlled research on drug effects. In turn, these companies benefited behavior analysis, often in material ways, for which I offer an example.

In early 1951, Skinner hired Ferster as a full-time assistant in his Harvard laboratory. Ferster was fresh from Keller and Schoenfeld's doctoral program at Columbia University (Dinsmoor, 1990), so fresh that he would not receive his degree until June. He and Skinner quickly established their groundbreaking research program on schedules of reinforcement. By early 1954, Skinner told his editor at Macmillan that they were going to prepare a book titled *Schedules of Reward,* based on their studies. About the title, Skinner (1983) later wrote that they "lack[ed] the courage to impose the term 'reinforcement' on the reading public or even the profession" (p. 109).

Skinner's editor at first agreed to publish the book, but on discovering more about it—seemingly, that it did "not refer to any applications" (Skinner, 1983, p. 110)—he lost interest. Harvard University Press also turned the book down after estimating the cost of reproducing its many cumulative records, originally in excess of a thousand. Appleton-Century, however, was interested. As Skinner related:

> At Appleton-Century, Dana Ferrin said he would publish it (the Harvard Press added an inducement by releasing its rights to *Verbal Behavior,* which had fallen to it as the William James Lectures), but unless we could find a subsidy the price would be very high. Fortunately, two of the drug companies with operant laboratories—Merck, Sharp, and Dohme and Smith, Kline, and French—made generous contributions, and *Schedules of Reinforcement* appeared at the relatively low price of ten dollars. (p. 110)

The pharmaceutical companies thereby underwrote a basic text in the science of behavior—Ferster and Skinner

(1957). In addition, Smith, Kline, and French soon afterward provided "$1,000 in seed money" (Gilbert, 1987, p. 476) to help launch the *Journal of the Experimental Analysis of Behavior* (*JEAB*)—the first behavior-analytic journal. The early financial viability of operant psychology thus had a basis in the pharmaceutical industry and its interest in application.

Other Applications

In addition to behavioral pharmacology, operant psychology was extended to other areas of research and application during the 1950s. Dewsbury (2003b) noted two of them—animal training and the space program—on which I elaborate briefly, and suggested another—autism—by mentioning behavior modification. I turn to autism first.

Autism. When Ferster left the Yerkes Laboratories of Primate Biology in 1957, he went to the Indiana University Medical Center to work on the problem of autism. Although Wolf's mid-1960s application of Skinner's science to autism treatment at the University of Washington was arguably the start of applied behavior analysis (see, e.g., Risley & Wolf, 1964/1967; Wolf, Risley, & Mees, 1964), Ferster's work was perhaps the first extension of operant psychology to this severe developmental disability (see Ferster, 1961; Ferster & DeMeyer, 1961, 1962). The role and importance of Ferster's contributions, however, have been lost in today's burgeoning literature (see, e.g., Ghezzi, Williams, & Carr, 1999). This is history worth reviewing.

Animal training. As for the extension of operant psychology to animal training for the purposes of entertainment, commerce, and human habilitation, two publications mark the beginning of this work—Breland and Breland's (1951) "The Field of Applied Animal Psychology" and Skinner's (1951) "How to Teach Animals" (see Skinner, 1983, pp. 26, 42). This promising start notwithstanding (see Pryor, 1969; Pryor, Haag, & O'Reilly, 1969), the relations between animal training as a profession and behavior analysis as a discipline were not much evident until the 1990s, when Pryor—author of *Don't Shoot the Dog!* (1984)—and her colleagues established a Special Interest Group for Animal Trainers in the Association for Behavior Analysis (see *The Trainers Forum Newsletter*). This is history yet to be told.

To her great credit, Pryor (1975, 1984, 1995) usually includes material on both operant psychology and ethology in her work, making it a valuable resource for analyses of their interrelations. For example, in his foreword to Pryor's (1975) *Lads Before the Wind*—her "diary of a dolphin trainer"—Lorenz (1975) described the work of operant psychology and ethology more constructively than usual (see, e.g., Garcia, 1981; Garcia & Garcia y Robertson, 1985):

> One can, for instance, investigate the learning faculties of an animal without studying all of its biology. The behaviorist school of psychology has thus studied a part or "subsystem" of organisms with considerable success. On the other hand, one can approach animal behavior from the side of a wider framework of reference, the widest being the functional unit . . . called an ecosystem. . . . Between the ecological and the behavioristic approach to animal behavior are all kinds of intermediates. . . . In principle, the branches of research concerned with the smallest and those which deal with the largest of living systems are equally legitimate and can prove equally rewarding. . . . Ethologists, on the other hand, try to understand the behavior of any animal species in terms of a system of interactions and events in the wider frame of reference represented by the ecology of the species. Though it is the avowed program of ethology thus to study the behavior of a species as a whole or a system, and although learned behavior undeniably constitutes an intrinsic part of this system, ethologists tend to be interested more in the phylogenetically programmed behavior patterns of a species than its learning processes. (pp. vii–viii)

By the way, in what amounted to a second career, M. Breland later documented the history of behavior analysis by videotaping more than 150 of the field's pioneers and leaders (see Gillaspy & Nihm, 2002). These tapes are today an untapped archival resource.

The space program. Among the most significant application of operant psychology in the 1950s was to the United States space program. This work began with Skinner's (1960) World War II research in which he taught pigeons to peck disks to guide missiles to precise destinations (see Capshew, 1986), but its emergence in the 1950s lay in Brady's (2001) and Rohles' (1966, 1992) extension of Ferster's (1958) and Kelleher's (1965) work at Yerkes. As Rohles related, behavior analysis was well suited to space exploration because (a) the flights employed single subjects, (b) operant methods could produce reliable baselines, (c) satellite housing was similar to operant chambers, (d) automated programming and recording equipment could be adapted to performance test panels, and (e) behavioral pharmacology had already demonstrated that behavior-analytic methods could be extended to other domains, which increased the general confidence that they were applicable to the conditions of space travel, among them acceleration, temperature, and radiation. This is history so compelling that relating it might significantly increase the public's understanding and appreciation of behavior analysis.

CONFLICTING APPROACHES: OPERANT PSYCHOLOGY ARRIVES AT A PRIMATE LABORATORY

Dewsbury's (2003a) paper expands importantly on a little-known interlude at the Yerkes Laboratories of Primate Biology and describes fascinating but sometimes chilling interactions between operant and traditional psychologists at the level of disciplines, sciences, and personalities. Dewsbury describes this interlude as a microcosm of the macrocosm of American experimental psychology during the 1950s at the same three levels. Today's behavior analysts can access this science through primary- and secondary-source literatures and judge the data and conclusions for themselves, but the interactions among the disciplines and personalities are beyond easy reach, except through papers such as Dewsbury's. As for my comments, I can only add a few observations to the product of his fine research, but before doing this I inquire into the characterization of operant psychology during that era.

Ideology or Science

In describing Yerkes as a microcosm of the macrocosm of American experimental psychology, Dewsbury referenced sources that described operant conditioning and the operant conditioners of the 1950s in such unflattering terms as "militant," "true believers," "a cult," "a religion," having "a religious commitment," "evangelical" in tone, an "indoctrination," and "Skinnerite." This language suggests that operant psychology was as much an ideology as a science, an ideology in the sense of being "an extreme sociopolitical program or philosophy constructed wholly or in part on fictitious or hypothetical ideational bases" (*Webster's Third New International Dictionary,* 1971, p. 1123; see Dewsbury, 2002). A good example of this sort of ideology during the 1950s and today was Hubbard's (1950) *Dianetics: The Modern Science of Mental Health* and its extension to the founding of the Church of Scientology. I am certain that Dewsbury did not mean to bring operant psychology and Scientology into stimulus equivalence, but the behavior of the reader might nonetheless be conditioned, just as Chomsky (1959) conditioned the behavior of the reader in his critique of Skinner's (1957) *Verbal Behavior* (see Czuberoff, 1988; Sherrard, 1988). Some of Dewsbury's supporting references themselves can have such an effect, especially when they are polemical to the point of being personal (e.g., Proctor & Weeks, 1990; contra. Wolf, 1991; see also Mahoney, 1989; contra. Catania, 1991).

Inquiring further into the meaning of *ideology,* we find, of course, that context is determinant. First, in the history of science, new paradigms are often called ideological, especially when they challenge traditions, which is when insularity may provide needed protection. As new paradigms become normal science, however, they are no longer called ideological, even though they may be the same science. Second, the extent to which scientific practices are called ideological depends on perspective. For example, the traditional psychologists at Yerkes viewed the practice of depriving primates of food as ideological and called it "starvation" (Dewsbury, 2003a; see also Garcia, 1981, p. 33; Garcia & Garcia y Robertson, 1985, p. 210), whereas the operant psychologists viewed it as an experimental control and called it "deprivation." In this context, Dewsbury's (2003a) citing Rumbaugh's view of deprivation as "chronic starvation" (personal communication, September 1, 2002) is a somewhat presentist style of historical argumentation (Stocking, 1965), where the facts of the matter may deserve more careful consideration. For example, in some species, food deprivation to the point of 80% ad libitum in weight is no more restrictive than the conditions under which they live in good health outside the laboratory (see, e.g., Poling, Nickel, & Aling, 1990). Third, we should distinguish the linguistic contexts concerning "operant psycholog*ists*" from those concerning "operant psycholo*gy*." The former may have been (and be) ideological at times, but it does not follow that the latter must be.

The foregoing issues warrant further research, especially through interviews of the psychologists and primatologists who participated in the conflict Dewsbury described, both in the microcosm of Yerkes and the macrocosm of American experimental psychology of the 1950s. If operant psychologists were ideological in the 1950s, the reasons are worth exploring; if not, then the reasons for the perception that they were need analysis (cf. Lubek & Apflebaum, 1987). That, though, is beyond my purview. I turn instead to some supplementary observations about the events that occurred before and after the operant interlude at Yerkes.

Before Yerkes

I begin by noting that operant research with primates in the 1950s was not an unprecedented expansion of operant psychology's vision. As early as the fall of 1928, Skinner was studying "insight" in a course he took from Hunter. As Skinner (1979) noted, "A squirrel might be able to use its hands nearly as well as Kohler's chimpanzees, and we proposed to try some of the experiments reported in *The Mentality of Apes* [Kohler, 1925]" (p. 30). In describing this project, Skinner's still-developing views about psychology were apparent. On the one hand, he wrote that he was "leaving too much to the supposed mental processes of the squirrels" (p. 31), implying that something might be left to mental processes. On the other hand, when Hunter suggested that they publish the research, Skinner wrote, " 'insight' was not a respectable word for a behaviorist, and I refused" (p. 31).

Yerkes at Operant Psychology

As for Yerkes itself, about 7 years before operant psychology arrived there, Yerkes arrived at Skinner's laboratory. As Skinner (1983) related,

> Paul Schiller, a young Hungarian psychologist at . . . Yerkes . . . had been watching monkeys and chimpanzees as they solved the kinds of problems discussed by Kohler in *The Mentality of Apes.* Whereas Kohler's apes seemed to reach solutions suddenly through insight, Schiller found that they acquired behavior in stages which Kohler had missed. Some of the behavior also appeared to be innate; apes would put two sticks together to form a longer stick whether or not there was a banana to be raked in [Schiller, 1952, 1957]. To familiarize himself with operant techniques, Schiller asked if he might spend some time in my laboratory. (p. 14)

In 1949, Skinner welcomed Schiller,

who began a program of research on respondent and operant "attack" in Siamese fighting fish. Unfortunately, this project went uncompleted and others were never begun because Schiller died in a skiing accident that spring. Looking more deeply into Schiller's motivation for working with Skinner might provide different insights into the conflicts at Yerkes than what Dewsbury (2003a) related of the views of the traditional psychologists (e.g., Menzel, Nissen; see Dewsbury, 1984, pp. 318–319; 1994, 1996). By the way, had Schiller not died in 1949, we might ask if Skinner would have hired Ferster in 1951. If not, and if Skinner had collaborated with Schiller instead, of his work might have expanded in different directions during the 1950s, or at least he might not have waited 30 more years before again conducting research related to Kohler's observations (see Epstein, 1981, 1984, 1986).

Operant Psychology Travels to Yerkes

As for the details of operant psychology's arrival at Yerkes, the behavior-analytic literature offers little information or insight. Skinner mentioned it a few times, but only in passing. For instance, he wrote in his autobiography,

> By 1955 Charlie [Ferster] had received a grant, but it could be given only to a holding company rather than to him directly. He had hoped that it might go to . . . Yerkes . . ., where he would be able to work with chimpanzees. . . . For a time, he had planned to work with Walter Rosenblith at MIT on the operant conditioning of nerve impulses from the brain of various preparations, allowing efferent impulses to operate the programming equipment normally operated by pecking a key or pressing a lever. But eventually the Orange Park job came through. (Skinner, 1983, p. 111; see also Skinner, 1987)

This passage raises several questions; among them are why did Ferster want to work with chimpanzees—or was Yerkes just a job? And, if the position had not become available, would Ferster have become the first behavioral neuroscientist? Perhaps, because he and Skinner were already analyzing the effects of brain lesions on schedule performance (Ferster & Skinner, 1957, pp. 85–87, 322–325, 577–579).

As for Skinner's comments on Ferster's interlude at Yerkes, he wrote only what Dewsbury (2003a) related: "unfortunately, [Ferster] found the atmosphere uncongenial. Tender-hearted colleagues frustrated his efforts to reduce chimpanzees to a satisfactory level of deprivation" (Skinner, 1981, p. 261). What Skinner wrote about Ferster's departure was largely tangential: "Charlie Ferster, who had left the Yerkes Laboratories and was studying the behavior of autistic children at the Indiana University Medical Center, was appointed editor [of *JEAB*]" (Skinner, 1983, p. 138; see also Skinner, 1987). As the first behavior-analytic journal, *JEAB* was also part of the expansion of operant psychology in the 1950s, and thus is a worthy subject for institutional history (see *JEAB,* 1987, Vol. 48, No. 3).

Operant Psychology at Yerkes

In commenting on what life was like for the operant psychologists at Yerkes, Marilyn B. Gilbert (1987), Ferster's wife at the time, offered the following observations:

> The year 1957 was one of transition, both for Yerkes labs and for us. The aura of the legendary chimp Vicky [*sic*] (star of Kathy [*sic*] Hayes' *The Ape in Our House*) still lingered, although Vicky had died. . . . Kathy had nurtured Vicky much as we nurtured our natural children. . . . They wore diapers, and were rocked by nursemaids who fed them milk from baby bottles. Charlie's lab was quite a contrast. Over there, the grown-up chimps were placed on strict diets and taught to weigh themselves so they could participate in operant-conditioning experiments. And when these grown-ups were too stubborn to reproduce, Charlie applied operant-conditioning techniques to teach them how to copulate. (p. 476)

Gilbert concluded, "All of us would be leaving that summer for more exciting prospects. Meanwhile, life at Yerkes was strange but uneventful" (p. 476).

Air Cribs and Apes

Although Viki was raised in a conventionally human manner (see C.

Hayes, 1951), shortly after Skinner (1945) published "Baby in a Box," he received a letter recommending the use of the "air crib" for chimpanzees. The letter was from Mrs. Armand Denis at the Anthropoid Ape Research Foundation in Dania, Florida. Skinner (1979) summarized it this way:

> She said they had a nine-month-old human baby "for whom we want to adopt your method"; they also had thirty-nine chimpanzee babies and expected more. The chimpanzees were treated exactly like human babies, and the baby tender . . . would help in protecting them from the respiratory infections to which they were especially susceptible. (p. 314)

Today, hospital incubators for premature human infants serve this function and others. They are the most widespread analogue of Skinner's (1945) technology.

Yerkes Begets Behavior Analysis

Yerkes was also where *JEAB* was fathered. As Kelleher and Morse (1987) related in their article, "The Yerkes Connection," while at Orange Park, Ferster, Kelleher, and Falk "often talked about the editorial practices of psychology journals" (p. 456). Based on these discussions, Ferster drew up and "circulated among a number of friends a document entitled 'Proposal for the Establishment of a New Journal' just before the 1957 meeting of the Eastern Psychological Association [EPA] in New York" (Laties, 1987, p. 495). On April 12, 1957, at the EPA meetings, in a bedroom in the Statler Hotel, a decision was made to move ahead with *JEAB* and the journal was born.

SKINNER BOXES FOR PSYCHOTICS: OPERANT CONDITIONING AT METROPOLITAN STATE HOSPITAL

Rutherford's (2003b) paper describes, with fresh data and details, Skinner and Lindsley's widely cited but poorly understood and appreciated research on the operant behavior of adults with psychoses (Lindsley & Skinner, 1954; Skinner, Solomon, & Lindsley, 1954; see Lindsley, 2001). In this and her other work, Rutherford is to be commended because, although she is not a behavior analyst—Skinner is her subject matter, not her psychology—she has a fine grasp of his science and system (e.g., Rutherford, 2000a, 2000b, 2002, 2003a). In the present context, I comment on four major points and mention two others in passing.

Human Schedule Research

Rutherford (2003b) described Lindsley and Skinner's research as the "first systematic replication of the free-operant paradigm with adult humans" (p. 267). Before this, research on human learning mainly used discrete-trial procedures (see Leahey & Harris, 2001); moreover, the first free-operant studies were not systematic (e.g., Fuller, 1949) and often informal, as in Skinner's and Keller's observations of their own children in the late 1930s and mid-1940s (Skinner, 1983, pp. 107–108). During the period of Lindsley and Skinner's collaboration, however, several important programs of research were begun, notably Greenspoon's (1955, 1962) on verbal conditioning, Hefferline's (1958; Hefferline, Keenan, & Harford, 1959) on the conditioning of covert operant behavior, and Bijou's (1955, 1957) and Baer's (1960; Gewirtz & Baer, 1958) on the experimental analysis of child behavior. Bijou's and Baer's work might well have been prompted by their visits to Lindsley and Skinner's project, a point worth pursuing in any history of the experimental analysis of human behavior.

Basic or Applied Research

Although Rutherford (2003b) noted that the purpose of Lindsley and Skinner's research is often portrayed as therapeutic, she provided no support for the assertion. I myself have had difficulty finding support for this view; what I have found is more suggestive than definitive (e.g., Kanfer & Phillips, 1970, pp. 245–246, 516; Martin &

Pear, 1996, p. 384). Nonetheless, I am inclined to Rutherford's interpretation that the research was basic and methodological, not applied. The applied interpretation, however, is understandable, given the project's early name ("Studies in Behavior Therapy"), its use of the term *behavior therapy* (Skinner, 1983, p. 53), descriptors of its subject matter (e.g., "chronic schizophrenia," see Lindsley, 1956) and its research participants (e.g., "psychotic patients," see Lindsley & Skinner, 1954; Skinner et al., 1954), and its early publication outlets—*Journal of Nervous and Mental Diseases* (Skinner et al.) and *Psychiatric Research Reports* (Lindsley).

Another factor may also be relevant: Neither of the two publications on which Skinner was a coauthor—an abstract in *American Psychologist* (Lindsley & Skinner, 1954) and a four-page article with commentary in the *Journal of Nervous and Mental Diseases* (Skinner et al., 1954)—presented data in the standard behavior-analytic format of the day, that is, with cumulative records. The publications thus did not have the look of basic operant research. Moreover, the commentators on Skinner et al. addressed therapeutic aims, not basic research. In the end, though, even if not therapeutic in purpose, Lindsley and Skinner's research was clearly a move in the direction toward addressing socially important human behavior.

Schedule Effects in Human Behavior

Rutherford (2003b) describes Lindsley and Skinner's research as a "systematic replication of the free-operant paradigm with adult humans" (p. 267), but this does not necessarily mean—nor am I suggesting that she meant—that the project directly replicated nonhuman free-operant schedule *performance* with humans. Even if she did mean performance, its relevant dimensions were left unspecified. For example, in writing that "the effects of different schedules of reinforcement on the behavior of the BRL's subjects were similar to those found in rats, pigeons, and dogs" (p. 269; see also Skinner, 1954, p. 419; Lindsley & Skinner, 1954, pp. 403–404), Rutherford could have meant "similar" on at least three dimensions—rate, scalloping, and stability. However, the replication of stable within-individual performances in rate and scalloping does not mean that the rates and scalloping were themselves replicated. In fact, when Lindsley and Skinner reported "stable individual differences" (p. 419), they may have meant stable within-individual differences because, on some occasions (e.g., Barrett & Lindsley, 1962; Lindsley, 1960), the cross-species differences were noted as failures to replicate (see Davey, 1988).

Contemporary reviews of the research on human schedule performance caution that cross-species replication is not always assured because of (a) uncontrolled human preexperimental histories, (b) differences in nonhuman and human research preparations, and (c) the interplay of human verbal and nonverbal behavior (see Baron & Perone, 1982; Baron, Perone, & Galizio, 1991; Catania, 1998, pp. 270–273). Given that some of Lindsley and Skinner's research participants might have lacked self-referential verbal behavior, cross-species replications were more likely (see, e.g., Bentall, Lowe, & Beasty, 1985). This is a point worth pursuing, especially when the meaning of *systematic replication* is ambiguous about the paradigms, performances, and dimensions.

Discontinuity and continuity. Any replications across species might explain the change in Skinner's position regarding discontinuity in nonhuman and human behavior. In 1938, in *The Behavior of Organisms,* he wrote:

> Whether or not extrapolation is justified cannot at the present time be decided. It is possible that there are properties of human behavior which will require a different kind of treatment. But this can be ascertained only by closing in upon the problem in an orderly way and by following the customary procedures of an experimental

science. We can neither assert nor deny discontinuity between the human and subhuman fields so long as we know so little about either. If, nevertheless, the author of a book of this sort is expected to hazard a guess publicly, I may say that the only differences I expect to see revealed between the behavior of rat and man (aside from enormous differences of complexity) lie in the field of verbal behavior. (p. 442)

By 1953, Skinner was still neither asserting nor denying discontinuity, but he had lowered his expectations about discontinuity:

To insist upon . . . discontinuity at the beginning of a scientific investigation is to beg the question. Human behavior is distinguished by its complexity, its variety, and its greater accomplishments, but the basic processes are not therefore necessarily different. Science advances from the simple to the complex; it is constantly concerned with whether the processes and laws discovered at one stage are adequate for the next. It would be rash to assert at this point that there is no essential difference between human behavior and the behavior of lower species; but until an attempt has been made to deal with both in the same terms, it would be equally rash to assert that there is. (p. 38)

Finally, in 1957, in *Verbal Behavior,* he wrote,

The basic processes and relations which give verbal behavior its special characteristics are now fairly well understood. Much of the experimental work responsible for this advance has been carried out with other species, but the results have proved to be surprisingly free of species restrictions. (p. 3; see also pp. 11, 452)

During the early 1950s, then, Skinner changed his position regarding whether the operant psychology of his day was sufficient to account for human verbal behavior. The likelihood that his work with Lindsley was a significant source of this change is supported by an observation he made in 1954 regarding operant psychology:

In all this work, the species of the organism has made surprisingly little difference. It is true that the organisms studied have all been vertebrates, but they still cover a wide range. Comparable results have been obtained with pigeons, rats, dogs, monkeys, human children, and more recently, by the author in collaboration with Ogden R. Lindsley, human psychotic subjects. (p. 89)

The Sequence of Disciplinary Development

Finally, Rutherford (2003b) proposes a four-stage sequence in the historical and conceptual development in behavior analysis, with the last two stages dependent, in part, on the first two. Her sequence extends from (a) basic laboratory research on nonhuman behavior (Skinner, 1938; see Catania, 1998), to (b) basic laboratory research on human behavior (Lindsley, 1956; Skinner et al., 1954; see Lattal & Perone, 1998), to (c) applied research with humans in controlled settings (Skinner, 1972; see Martin & Pear, 1996), and finally to (d) applications in the culture at large, both in community settings (Skinner, 1948; see Mattaini & Thyer, 1996) and individual self-management (e.g., Skinner & Vaughan, 1983; see Watson & Tharp, 2002). These proposed stages proceed from "low to high complexity," both in terms of behavior (e.g., from bar pressing to self-control) and environment (e.g., from operant chambers to the community at large).

This history reflects the received view on the evolution of the natural sciences, particularly in the "context of justification" (Reichenbach, 1951, p. 231), but the course behavior analysis followed was not so seamless, either in the context of justification or of "discovery." For example, although Lindsley and Skinner's work predated applied behavior analysis and "functioned as a link between the experimental analysis of behavior and applied work" (Rutherford, 2003b, p. 275), applied behavior analysis may not have been dependent on the experimental analysis of human behavior. As Michael (1984) pointed out, the latter program of research never amounted to the "careful laboratory extension" (p. 364) expected in the systematic development of a science of behavior. Applied behavior analysts largely took the replicability of the basic behavioral processes for granted and forged ahead.

Hake (1982) made a similar point in noting that the expected simple-to-

complex continuum of research on human behavior (e.g., from reinforcement as a basic process to social reinforcement) quickly became a basic-to-applied continuum. That is, behavior analysts generally studied (a) basic behavioral processes in humans and (b) human behavior of social importance to the relative exclusion of the behavior of interest to most of psychology and the culture at large, for instance, motivation, emotion, language, and cognition. Research on the latter would be the transitional phase between Rutherford's second and third stages, with the latter to a degree dependent on the former, but the emergence of applied behavior analysis seems not to have been overly dependent on either.

Although a received view, Rutherford's proposed sequence and Michael's and Hake's expectations overly simplify the historical and conceptual interrelations among purely basic research, use-inspired basic research, applied research, and intervention research. As Bacon (1620/1960) and Mach (1883/1960) noted, science—early science, at least—was often an extension of the "manual arts," making technology sometimes the cause, not just the consequence, of science. At the very least, science and technology evolved in parallel and often interacted. This was, in part, Skinner's view, and reflects the historical course of behavior analysis better than the natural science ideal. When this ideal is the received view in psychology, it may explain some criticisms and misunderstandings about behavior analysis on the relation between science and technology, a point worth further historical and conceptual development (see, e.g., Chiesa, 1992; Marr, 1986; Smith, 1986, pp. 259–297; 1992, 1995, 1996).

Conclusion

As for my final two points on Rutherford's paper, one may be worth pursuing further; the other is but a minor complaint. The point to pursue further is suggested by Lindsley and Skinner's observations of the patterns of within-individual behavior over the course of their hours-long sessions, in particular, the interacting patterns of vocal hallucinations, pacing about the room, and behavior with respect to operant manipulanda (e.g., pulling a plunger). Their "technique of direct, continuous, and simultaneous recording of symptomatic and nonsymptomatic responding" (Rutherford, 2003b, p. 272) paralleled then-current and subsequent research on response preferences and alternatives, multioperant behavior repertoires, and behavior in continuously programmed environments (e.g., Bernstein & Ebbesen, 1978; Brady, 1992; Findley, 1958, 1966; Premack, 1965; see Morris, 1992). The earlier of these programs may have been an extension of Lindsley and Skinner's research, or perhaps vice versa, whereas the later are at least a conceptual, if not real, extension of their research program (e.g., behavior analysis in space).

My minor complaint is about the title of Rutherford's paper—"Skinner Boxes for Psychotics." Although "Skinner box" is today standard terminology, Skinner disliked the eponymous expression. As he wrote in his autobiography, it was "an expression which I have never used and which my friends accept as *verboten*" (Skinner, 1983, p. 164; see also p. 116). Moreover, although catchy, "Skinner Boxes for Psychotics" decontextualizes, and therefore tends to trivialize, the importance of Lindsley and Skinner's research (cf. Czuberoff, 1988; Sherrard, 1988). The paper might have been differently titled.

CONCLUSION

In conclusion, I return to the theme of the foregoing papers: "the broad applicability of [operant psychology] was a major reason for its later success" (Dewsbury, 2003b, p. 233). The theme is compelling; the papers support it; and I agree with it. However, the correlation between applicability and suc-

cess warrants a deeper analysis: Although applicability was a reason for the success of operant psychology, the reason for its applicability was more fundamental. Success can be local and temporary, as may be the case of the current eclipse of behaviorism by cognitivism. It is only an eclipse, not a revolution (Leahey, 1992; Moore, 1995).

The fundamental reason for the applicability of operant psychology lies in the integration of its conceptual system and research methods (see Sidman, 1960; Skinner, 1938). Operant psychology is a science of behavior qua behavior, not of behavior as an index of mind (Skinner, 1977; see Morris, Higgins, & Bickel, 1982). It is a science of generic behavior–environment processes, in which biology and culture are context (Skinner, 1935; see Morris, 1992). It is a science that experimentally controls for sources of variability in behavior and environment, such that basic processes may be observed at the level of individual organisms (Skinner, 1966; see Catania, 1998). And, although prediction and control are usually cast as the goals of behavior analysis (e.g., S. C. Hayes & Brownstein, 1986), I think they are more *means* than *ends*—means toward the end of understanding behavior, not ends in themselves (Morris, Todd, & Midgley, 1993; see Skinner, 1947, pp. 26–27). In addition, prediction and control are not only basic to understanding behavior, they are also directly applicable to problems of individual, social, and cultural importance (Skinner, 1972; see Baer, Wolf, & Risley, 1968; Cooper, Heron, & Heward, 1987; O'Donohue, 1997). As Skinner (1983) put it, "[In the 1950s,] an operant analysis moved directly into a form of behavioral engineering because it pointed to conditions that could be changed to change behavior" (p. 47; see also p. 412). In other words, the applicability of operant psychology was a *result* of its success as a science, and then a *reason* for its success as a paradigm and approach. We sometimes overlook the former when celebrating the latter, but should never forget that behavior analysis is grounded in the experimental analysis of behavior.

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APPENDIX

The Chronology of Skinner's Publications during the 1950s

Skinner, B. F. (1951, December). How to teach animals. *Scientific American, 185,* 26–29.

Brinton, C., Krutch, J. W., Kroeber, A. L., Skinner, B. F., & Haydn, H. (1952). The application of scientific method to the study of human behavior. *The American Scholar, 21,* 208–225.

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