

Conflicting Approaches: Operant Psychology Arrives at a Primate Laboratory

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During a brief period, from 1955 to 1957, behavior analysts, primarily Charles Ferster, Roger Kelleher, and John Falk, conducted research on chimpanzees at the Yerkes Laboratories of Primate Biology in Orange Park, Florida. This was a time of conflict between operant conditioners and more traditional experimental psychologists at the national level, and there was a similar conflict at the local level in Orange Park. The principal overt issues concerned the use of deprivation procedures, the apparatus utilized, and the naming of animals, although more fundamental differences probably set the occasion for the disputes. The conflicts in Orange Park can be seen as a microcosm of the broader conflicts that occurred during a period when the operant approach was being extended and applied more broadly than before.

Key words: chimpanzees, operant psychology, Charles Ferster, primates

The spread of operant psychology during the 1950s did not always go smoothly, as those favoring more traditional approaches disagreed with some of the fundamental tenets of the newer approach. The interactions of the diverse collection of scientists at the Yerkes Laboratories of Primate Biology show in a microcosm how these tensions were played out. This is the story of the interactions of Henry Nissen, Charles Ferster, and their associates and is based on published material, archival material, and interviews with the surviving principals. It is a story of conflicting approaches to the conduct of science.

THE YERKES LABORATORIES

The founding in 1930 of the facility that was to become the Yerkes Laboratories of Primate Biology in Orange Park, Florida, was the culmination of a long-held dream and plan that Robert Yerkes had formulated while still in graduate school at Harvard University around the turn of the century. It be-

came the largest facility for the scientific study of chimpanzees in the world. The station would remain in Orange Park until 1965, when Emory University moved it to its home campus in Atlanta.

Yerkes would remain as director only until 1941, when he was forced out and succeeded by Karl S. Lashley. Lashley resigned as director in June, 1955, but had been ill during the last part of his tenure as director. While Lashley was ill and approaching retirement age, the operation of the facility was in the hands of Henry Nissen and Karl Pribram, who served as unofficial acting codirectors. After a permanent codirectorship could not be worked out, it was Nissen, a protégé of Yerkes, who succeeded Lashley. The story takes place during this period of transition.

During Yerkes' administration, procedures for the maintenance, breeding, and study of captive chimpanzees were developed and research was completed on sensory function, social and sexual behavior, and the development of behavior. The study by Spragg (1940) on morphine addiction was a classic in demonstrating an appetitive component to addiction as a model for study in nonhuman animals. Many studies of learning were conducted, including a program on animal cognition. The to-

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ken reward studies of Wolfe (1936) and Cowles (1937) and the research of Kenneth Spence also stand out as particularly noteworthy. The methods used included various discrimination learning tasks, especially with Yerkes' multiple-choice apparatus, tool use, and patterned string problems. Yerkes believed that performance in many of these tasks suggested the actions of "ideation"—a process fundamentally different from basic learning that was especially revealed by the quick solutions sometimes reached by the chimpanzees. Nissen, a Carl Warden PhD from Columbia, worked largely within Yerkes' framework. Under Lashley, emphasis shifted to more physiological studies, but analyses of learning continued with studies of discrimination learning, learning sets, delayed response learning, and problem solving. Many of Lashley's staff members had departed before Nissen assumed the directorship. Nissen and Pribram needed to hire new staff during the mid-1950s.

BEHAVIOR ANALYSTS AND TRADITIONAL EXPERIMENTALISTS

The 1950s were a period of a certain mutual isolation between the operant conditioners and the traditional learning psychologists. The two groups tended to segregate themselves into two different camps with different approaches. The tension between them is apparent in what happened in Orange Park, although the specific issues there were not those that created the most controversy more broadly. To understand the tensions that arose at the Yerkes Laboratories, some consideration of the disagreements between the camps is necessary.

Behavior analysts were viewed from the outside, rightly or wrongly, as a closed group, isolated from most experimental psychologists primarily by the methodology that was adopted (see Sidman, 1960). For their part, many behavior analysts felt the need to distance themselves from the traditional-

ists because they believed that traditional methods were inadequate for the development of a true science of behavior and that operant research was not getting a fair hearing from journal editors and the program chairs for psychological meetings. Traditionalists were viewed as placing roadblocks in the way of more modern approaches (Grant, 1958). Many behavior analysts adopted a "militant posture" (Krantz, 1972, p. 91). One respondent told Krantz that it took guts to be an operant conditioner during that time and that "everyone was belligerent" (p. 91). It should be remembered that there were many traditional learning psychologists at the time; Krantz estimated that there were only about two dozen operant conditioners. Surely, the behavior analysts viewed themselves collectively as a David fighting a Goliath.

Many outsiders perceived the behavior analysts as a group of "true believers." Reacting to the Columbia University curriculum in behavior analysis, Wendt (1949) treated the approach as a cult, isolated from the rest of psychology and using simplification as part of a propaganda campaign. Wendt suggested that the isolation from the rest of psychology led to inbred departments. Others treated behavior analysis as a religion (Krantz, 1972). Grant (1958) detected "a certain evangelical tone" present in *The Behavior of Organisms* (Skinner, 1938) but lacking in *Schedules of Reinforcement* (Ferster & Skinner, 1957). For Proctor and Weeks (1990), behavior analysis was a pseudoscience because "a pseudoscience forms a cult following around a charismatic leader who serves as the spiritual leader for the followers" (p. 98). Baars (1986) quoted Hilgard as suggesting that behavior analysis was "almost like a religious commitment" and Skinner as being "so committed to the thing that to his disciples he's just like a god" (p. 292). Harlow (1969) referred to the group as a "tight little club" (p. 27). Proctor and Weeks alleged that the primary goal of

behavior analysis had been its adoption and spread.

Traditionalists favored group designs with tables of means, measures of variability, and inferential statistics. Behavior analysts preferred single-subject designs with control achieved through repeated measures. Finan (1940) disagreed with this approach and criticized *The Behavior of Organisms* (1938) as “lacking the fortification of statistical convention” (p. 444). He sought better criteria concerning which “representative” data would be presented. He disagreed with the behavior analysts’ conclusion that averaged data are of little use. Grant (1958), writing of *Schedules of Reinforcement* (1957), complained that “usually two (or, rarely, four) birds on a given schedule give somewhat differing, and occasionally widely, differing results” (p. 329). He added that “the worker is put in the position of navigating coastal waters with a *Rand McNally* road map as his guide” (p. 329). The behavior analysts, by contrast, believed it essential that prediction and control of behavior be demonstrated in individuals if there was to be a true science of behavior.

Single-subject designs were sometimes rejected for publication in the prestigious journals of the American Psychological Association. This rejection was an important factor in the founding of the *Journal of the Experimental Analysis of Behavior* in 1958 (Krantz, 1972; Laties, 1987). This journal provided a publication outlet but increased the isolation between the groups.

The experimental psychologists such as Grant (1958), in his review of *Schedules of Reinforcement* entitled “Pigeons Peck for Positivism,” bemoaned the lack of explanatory concepts in the operant approach. Grant contended that “no detailed quantitative laws emerge from the 70,000 hours of data-gathering” reported in *Schedules of Reinforcement* (p. 329). Similarly, Hilgard (1939) bemoaned the lack of laws in *The Behavior of Or-*

ganisms. Finan (1940) suggested that the approach could be improved with more hypothesis testing of the sort used by Clark Hull. Behavior analysts distrusted the so-called laws and hypothetical constructs of the traditionalists.

Critical of the methodological approaches of traditionalists and of what they perceived as the limited value of their work, behavior analysts often did not cite the work of those outside the group. Skinner often wrote with few references at all. These patterns offended the traditionalists (e.g., Grant, 1958). Finan (1940) complained that “no serious attempt is made to relate the results to the accumulated literature on learning and conditioning” (p. 445). Verplanck (1954) complained about the lack of effort devoted to explaining data collected outside of the behavior-analytic context. Krantz (1971, 1972) later showed the pattern of increasing self-citation of articles from the *Journal of the Experimental Analysis of Behavior*, relative to other journals, during this period. Even today, the “uncomfortable separation from other disciplines with whom we have much in common” (Brown & Hendy, 2001) is an issue for at least some behavior analysts.

Skinner’s tendency to extrapolate to human behavior also came under criticism. Verplanck (1954) criticized the fact that “it is possible to find no predictions at all of the behavior of rats, or of pigeons, when novel combinations of stimuli are presented to them in a Skinner box, and many predictions among Skinner’s writings with respect to human behavior in a social environment” (p. 311).

Keller (1981) applied the situation to Charles Ferster:

Young behaviorists of Charles’s day often had a difficult row to hoe. The doctoral candidate who offered individual-organism data from a Skinner Box, unaccompanied by hypotheses or reference to distinguished predecessors, could expect an inquisition from his examining committee. Even when that ordeal was over, the newly hooded PhD had to get a job and find a journal that would publish his research. (p. 299)

In another obituary for Ferster, Boren put it as follows:

This is a time when statistics and a large N were somehow associated with "good" science, and single-subject research designs were suspect. Furthermore, the operant conditioners were working on different problems, had a different terminology, and paid little attention or deference to other theoretical positions. (p. 156)

Laties (1987) provided some perspective on this isolation, pointing out that it can be overstated. At the time of the founding of the *Journal of the Experimental Analysis of Behavior*, Keller was serving as president of the Eastern Psychological Association; Skinner was a recent past president. Since 1951 Ferster had published articles in such journals as the *Journal of Experimental Psychology, Science*, and *Psychological Bulletin*, among others. Nevertheless, there were substantial, mutually felt tensions between the behavior analysts and the traditionalists at the time that the first behavior analysts arrived in Orange Park.

BEHAVIOR ANALYSIS COMES TO ORANGE PARK

Karl Pribram was attracted to Skinner's approach and initially to the possibility of using his techniques of operant conditioning in his research with Lawrence Weiskrantz in Hartford, Connecticut, on effects of brain damage in nonhuman primates. He had an extensive correspondence with one of Skinner's close associates, Charles B. Ferster. Ferster had joined Skinner as a research fellow at Harvard and was known especially for the research that led to the massive *Schedules of Reinforcement* (1957) volume written with Skinner (see Boren, 1981; Dinsmoor, 1982; Keller, 1981; Skinner, 1981). Pribram thought that the operant techniques might be useful in Orange Park and approached Ferster about possibly joining the staff. Pribram floated the idea by Nissen, who responded favorably, noting that he liked the idea of studying "the effects of very specific and controlled experiences on later be-

haviors" and "the idea of developing practicable automatic training devices" (H. Nissen, 1954). Nissen was supportive of hiring Ferster, but with a caveat that was somewhat prescient:

My impression is that some people have the greatest admiration for the Skinner approach but that many more are highly critical and even contemptuous of it. The latter view might be accentuated when considered in connection with, or applied to, such a "highly organized" animal as the chimpanzee.

One critic, David Grant (1958), wrote that Ferster "received his *indoc-trination* [italics added] in the experimental analysis of behavior at Columbia University and has been a member of the *Pigeon Staff* [italics added] at Harvard University" (p. 328). Grant called *Schedules of Reinforcement* "a bulky cumbersome atlas and a crude one at that" (p. 329). Ferster appears to have been a man of contradictions; Keller (1981) later wrote that

As a person, Charles embodied many contradictions. He was an unobtrusive man who could make things happen; a shy man who could speak his mind; a kindly man who could be hard; a sober man with a twinkling eye and a bag of little quips. A friend described him as "the most skillful-insightful awkward man I have ever known." (p. 301)

The details were worked out, and Ferster began his official tenure at the Yerkes Laboratories just 2 weeks after Nissen formally assumed the directorship in July, 1955.

A second scientist who would work in the Skinnerian tradition, and who actually arrived a month before Ferster, was Roger T. Kelleher. He was an NYU PhD who appears not to have been a devotee of the operant approach before getting to Orange Park; his doctoral dissertation was on discrimination learning with reversal and nonreversal shifts. John L. Falk joined the staff the next year. When he arrived in Orange Park, Falk knew little of operant psychology. He had been influenced by Donald Hebb at McGill University but he became fascinated by the hard-nosed approach to science of Ferster and Kelleher and the possibilities for

using the techniques in research with drugs. Marilyn Ferster Gilbert (2002) recalled that "Charlie soon seduced John to behaviorism" and called her husband (Charles Ferster) "kind of a Pied Piper for behaviorism."

The trio of operant conditioners, to which John Stamm might be added for some purposes, was balanced by such scientists as Henry Nissen, Cathy Hayes Nissen, Lelon Peacock, and Charles M. Rogers, who worked more in the tradition of Robert Yerkes. Several others were on the staff at various times.

Even in the small community of Orange Park, the operant psychologists were somewhat isolated from others at the laboratories. The Fersters socialized primarily with their neighbors, the Nissens, and the other operant psychologists, including John Stamm (Gilbert, 2002). Gilbert recalled only two formal, laboratory-wide parties during their stay. They often went to neighboring beaches with the Kellehers and Falks. Marilyn Ferster sometimes worked at the laboratories reading the cumulative records generated in the operant experiments. The Fersters' use of a Skinnerian air crib with son Billy drew much interest from laboratory personnel (Gilbert, 2002).

The operant period in Orange Park lasted just 2 years, from the summer of 1955 though the summer of 1957. This was a critical period of activity in the development of behavior analysis; the timing of the events in Orange Park needs to be understood in relation to the development of the broader field of behavior analysis. It is likely that the broader events exacerbated the tensions in Florida. Ferster and Skinner's *Schedules of Reinforcement*, published in 1957, was in preparation as the events discussed here unfolded. The meeting that resulted in the founding of *Journal of the Experimental Analysis of Behavior* was held at the convention of the Eastern Psychological Association in April of 1957, a few months before Ferster's departure from Orange Park; two Orange Park scien-

tists, Ferster and Falk, participated. At the same time, the core group incorporated as the Society for the Experimental Analysis of Behavior. When the journal appeared the next year, Ferster would be the founding editor.

The operant conditioning program at the Yerkes Laboratories was a whirlwind event. The group swept in, dominated the research program for 2 years, and swept out again. Falk (1994) suggested that "they felt that Henry [Nissen] really didn't want them to do what they were doing." Although Nissen and others were attracted to the precision of the approach, they were put off by other aspects. There was continuous tension between the groups. In an interview, Peacock (1993) admitted that "the rest of us were really quite delighted that Ferster and Kelleher decided to leave." Surely the issues discussed above set the occasion for the tension between the two camps. The surface issues were somewhat more specific.

One issue involved equipment. Peacock recalled that Ferster and Kelleher tended to look down on the rest of the staff because they were using methods that appeared less sophisticated. For their part, the other camp was skeptical of the value of the expensive gear. Ferster frequently requested funds to purchase more equipment that had multiple uses. Peacock (1993) recalled that "Lashley said that he simply would not expend money for equipment that was not designed for a specific purpose. He said that in the first place if you can't do it with soda straws, twine, and rubber bands, it's probably not worth doing anyway."

The most contentious issue was that of the use of food deprivation. Peacock (1993) recalled that Nissen "deplored the technique of starving down to 80% of body weight as a motivator for chimpanzees" and that "he and Ferster continually argued about that." "There were lots of comments made at weekly staff meetings about the psychology of half-starved animals."

Different staff members remember

the effects of food deprivation differently. According to Rogers, in a 1994 interview, "Ferster believed it was written in stone that if an animal's body weight was reduced to 80% they were going to work. And they would rather work than starve. But that wasn't true for chimps." That same year, Menzel (1994) told me that "Nissen used to say that . . . you try to starve them into submission and some animals would starve themselves to death. They'd just quit working. It was like they'd lose appetite." According to Falk (1994), however, "the animals that were reduced in body weight, and concurrently on schedules, were bright eyed and seemed to have a purpose in life."

In his annual report for 1957, Nissen (1957a) noted that "quantitative inanition, occurring in the course of experimental procedures, was probably at least a contributory cause in the death of one adult and one juvenile male." Rogers (1994) told the story less delicately, contending that Ferster had gotten scared by animal deaths and had thrown a lot of food in the cage of one of the chimpanzees. According to Rogers, the animal "ate everything in sight" and then upchucked, aspirated, and died.

When some animals failed to respond in ways characteristic of other species, accusations were made that staff members had been secretly supplementing the chimpanzee's diets. There are contradictory versions about possible sabotage of the operant conditioners' deprivation procedures. Nissen was accused of occasionally slipping the animals some extra fruit and other food when they were not supposed to be getting it. Nissen denied the charge.

In response to a letter from Leonard Carmichael, Nissen (1957b) put the situation in a more delicate, and probably sanitized, perspective than that generally attributed to him. Clearly, word had gotten to the Chairman of the Board of Scientific Advisors that Nissen had been critical of the operant

conditioners, and Nissen had been accused of interfering with their attempts to regulate diet. Nissen replied that "the statement . . . that the dietary restrictions necessary for Skinnerian techniques cannot be imposed on our chimpanzees is certainly misleading." Nissen wrote that in some cases there had been more difficulty than with other species "in getting stable performances, uniform day after day" and "in holding weights constant from day to day." He suggested that the working spaces in which the animals had been tested were less insulated from distraction than in the traditional Skinner boxes. He added that newer boxes, built by Kelleher and Falk, had largely overcome this difficulty. Nissen noted that one investigator "has not been satisfied that irregularities of water-drinking, urination, defecation, coprophagy, and gross bodily movement, plus factors in chronic starvation about which we are completely ignorant" may have accounted for the variability. Nissen agreed that, in some cases, the apes had been slow in making certain discriminations. "In one case there was as much responding without 'reinforcement' as with reinforcement." He attributed this, in part, to the use of commercial laboratory chow, a nonpreferred food, as the primary reinforcement.

Nissen defended his own conduct: "The statement is incorrect and misleading if it suggests that I have in any way interfered with, or attempted to regulate, the diets used with the subjects." He wrote that both he and Ferster agreed that the use of prolonged inanition was not consistent with the best aims of the program and that chimpanzees were not the ideal subjects for such research. He concluded by affirming the value of the Skinnerian technique as applied elsewhere but not at the Yerkes Laboratories.

Marilyn Ferster Gilbert's (2002) recollections differ in a few details but basically support Nissen's version:

When the chimps failed to lose weight although on a much reduced diet, some paranoia set in. At first, it was a joke: maybe Cathy [Nissen] was

feeding them spaghetti. But it grew until Charlie and Roger were hanging out at the cages for a good part of the night, spying to make sure that neither Cathy nor Henry went down there to feed the animals. They saw nothing, of course, and abandoned their watch after a few days. Eventually, they decided that the chimps were eating acorns.

There were also disagreements about the treatment of the chimpanzees. The tradition in Orange Park had been to name the animals and for the humans to interact with them. Ferster and Kelleher believed, by contrast, that the only way to get the animals to respond appropriately was with automation so that there would be no eye contact between subject and experimenter. With eye contact, animals might respond for social reasons, rather than responding to the varying treatments. The hard-nosed Ferster and Kelleher would not refer to animals by name, preferring to use numbers.

Most traditionalists at the laboratories did not like the way in which the operant group treated the animals. On the other hand, Ferster noted that punishment was used frequently in the day-to-day operation of the laboratories when positive reinforcement might have been more effective. Rogers (1957) agreed that "aversive control is used much more than reinforcement." Rogers added that "punishment and threat are used in many situations where positive reinforcement might be applied, but Ferster bases his arguments on comparisons with his animals which are maintained at 80% of normal body weight."

Cathy Hayes Nissen appears to have been generally sympathetic, as she saw great possibilities for applying operant technology to her studies of infants. In her continuing correspondence, she kept the Yerkes family in Connecticut informed of the situation as she perceived it and contrasted Ferster and Kelleher:

They are very different from each other. Roger is more "Yerkes Lab" (Henry thinks I'm a snob) by which I mean, he is getting to know the animals, trying to get a sense of what sorts of problems can be done thru cage wires, in-

forming himself on past work, the people, animals, techniques and degrees of success, etc. Charlie is scared to death of the animals, keeps thinking they will get out and rend him asunder. He prepares to do his research in the shop, assembling lengthy orders of mysterious-sounding bits of metal which go into his apparatus. Introduced to his animals, Art and Bard, he said, "Big, aren't they?" What a variety of people have passed thru these halls! (C. Nissen, 1955)

Marilyn Ferster Gilbert (personal communication, October 31, 2002) disagreed with the contention that her husband had been afraid of the chimpanzees.

Later, Cathy Nissen wrote of an incident that occurred during the visit of the Board:

Dr. Ferster gave a very fine presentation of the Skinner discipline which completely sold the audience with its potentialities. But when he expressed the opinion that the chimpanzees were not so bright as his Harvard pigeons, even Dr. Lashley jumped into the fray with the reminder that there are no stupid animals, only inadequate experiments. Charlie granted that the lever-pressing may simply bore the apes, and so a good time was had by all. (C. Nissen, 1956)

As Ferster and Kelleher were leaving Orange Park, Rogers wrote to Frank Beach,

I suppose that you have heard that Ferster is leaving. I can't say that I am sorry to see him go but it will do the lab no good. He has formed a small sphere of influence and sown enough dissension to make things pretty unpleasant. There is no research worth a damn but the stuff put out by the operant conditioning boys. With him depart the disciples.

These characters are going to hurt the place with their gripes. Whenever they have trouble with an experiment it is because they are given the worst animals in the place, according to them. (Rogers, 1957)

Beach (1957) replied that "I can imagine that Charlie Ferster could get pretty tedious. He is such a dedicated Skinnerite that he tends to suffer from tubular vision at times."

Skinner (1981), in a personal memoir written upon Ferster's death, presented a different view of Ferster's experience: "He went to the Yerkes Laboratories in Florida, where, unfortunately, he found the atmosphere uncongenial. (Tender-hearted colleagues frustrated his efforts to reduce

chimpanzees to a satisfactory state of deprivation)" (p. 261).

On April 27, 1958, less than a year after the behavior analysts departed, Nissen took his own life with a drug overdose.

ACCOMPLISHMENTS OF THE BEHAVIOR ANALYSTS

Despite all the controversy, the work of the operant conditioning group was probably the most productive done in Orange Park during this period. Several of the studies were published in *Science*. The research included studies of schedules of reinforcement, time-out from reinforcement, conditioned reinforcement, and discrimination learning. Most of these studies appear to have been conducted, and conducted quite successfully, with chimpanzees maintained at approximately 80% of resting laboratory body weight. I will consider briefly the research done in some of these areas.

With regard to the use of schedules of reinforcement, Nissen recorded in his 1956 annual report that

Intensive work with six adults and two young animals has demonstrated the feasibility of using this method with anthropoid apes. Various schedules of reinforcement have been explored, and in respect to most basic characteristics of responses the behavior of these primates has paralleled those of other animals. It is difficult at this time to interpret the differences found between chimpanzees and other species in certain details of behavior. (H. Nissen, 1956)

Typically, the food-deprived chimpanzees were studied in a compartment with one or two telegraph keys mounted so that they could be operated easily. The rewards were generally alternated, with either a 50-calorie portion of whole-wheat cracker or Purina® monkey chow provided on successive occasions. Typically, the animals were fed fruit, skim milk, and other supplements immediately after the conclusion of daily testing. Various complex schedules were used.

In a study of multiple schedules described in an annual report, in the presence of a red light, a fixed-ratio (FR)

schedule prevailed in which 50 responses produced one reward (FR 50). In the presence of a blue light there was a fixed-interval (FI) schedule in which the first response made after a 10-min interval was rewarded (FI 10). The two schedule-light combinations were presented alternately. Behavior appropriate to each schedule was observed. The method was chosen so that the effects of radiation could be studied. Previous work had shown performance on the FR schedule to be more resistant to treatments such as drugs than was the FI schedule. To anticipate, radiation had no effect on 1 of 2 animals trained and radiated. The other, which died on the 28th day, showed no effect on the FR schedule until death but a sharp rate drop on the 25th day of the FI schedule (H. Nissen, 1957a).

In another study, a chain schedule was used with an adult female with a free-feeding weight of 105 lb who had been brought down to and maintained at 75 lb. Pressing the right key operated the food magazine (i.e., delivered a reward) if the press had been preceded by a requisite number of presses of the left key. Ferster (1958b) reported that "under intermittent reinforcement of the sequence of responses, the behavior was maintained considerably more accurately than under continuous reinforcement" (p. 164).

In studies with concurrent schedules of reinforcement, the two keys were available and active simultaneously, but there was a different schedule operative on each of the two keys. Ferster (1957a) studied chimpanzees reduced to 80% body weight with an FR schedule operative on one key and a variable-interval (VI) schedule on the other. Ferster reported that "for the most part, the performances of the animal on both keys are similar to those that would develop singly without interference from another key" (p. 1091). He speculated that the method might be useful in studying bilateral independence, such as that needed in piano playing, and in studying effects of

emotions on behavior maintained by the two different schedules.

In a later study with 2 subjects (identified only as Yerkes No. 93 and 95 and not by name), Ferster (1959) expanded on these results by studying the animals' performance while there was no change in the VI schedule on the one key but there was a transition to a more complex concurrent multiple FR FI schedule on the other. The performance on the VI key was unaffected by, and did not affect, the transitions on the other key. Again, there was independence between the two. A number of other variations were explored.

Having shown that chimpanzees respond appropriately in situations using positive reinforcement, Ferster (1957b) studied control of behavior by punishment. Withdrawal of a situation in which positive reinforcement can occur constitutes a time-out and is an aversive event. Chimpanzees were trained to respond for food reward on a VI schedule. They then were exposed to conditions in which the overhead light was turned off and the food magazine was disconnected from the response key simultaneously (a time-out). Control of the time-out was established as follows:

A red lamp, called the pre-time-out or pre-aversive stimulus, was then installed next to the key. The light appeared every 15 minutes for 160 to 180 seconds, depending on the animals' performance. If an animal pressed the key during the last 20 seconds of the pre-time-out period, a 60-minute time out followed; but if it did not press the key during the final 20 seconds, the red light terminated and no time out could occur until 15 minutes later, when the pre-time-out stimulus reappeared. (Ferster, 1957b, p. 509)

The chimpanzees responded appropriately to the conditions, coming to continue responding normally during the first part of the pre-aversive-stimulus period but to cease responding during the last 20 s. Ferster noted that the time-out manipulation produced behavior quite similar to that which had been produced using shocks as an aversive event in pigeons. He likened the time-outs to human situations with fines, disapproval, or incarceration.

Ferster (1958a) followed up this work with five experiments that resulted in a monograph on the same general subject. The chimpanzee subjects were identified as No. 67 (Jed) and No. 97 (Verb). In one experiment, the behavior of pigeons was studied for comparison. In general, the temporal parameters of this basic procedure were manipulated in order to study the effects on performance. Increased responding during the early part of the preaversive stimulus was produced with some parameters. In one experiment, the chimpanzees had a second key which, if pressed, would delay the time-out; avoidance behavior was observed. In another experiment, fast rates of responding were punished. The chimpanzees' behavior generally changed in predictable ways in response to changed response contingencies.

It will be recalled that during the Yerkes years at the laboratories, both Cowles (1937) and Wolfe (1936) studied conditioned reinforcement by training chimpanzees to work for token rewards that later could be exchanged for food. This is analogous, of course, to the human situation in which we work for money or other conditioned rewards. Kelleher (1956, 1957b) conducted a similar research program using operant methodology. Not surprisingly, Kelleher automated the process. Chimpanzees were put on an FI 5-min schedule of reinforcement. They were then trained that insertion of a poker chip into a slot, when and only when a red light was on, would produce food. The chimpanzees worked as well for tokens as for food as long as immediate exchange was possible. However, they would not respond reliably if they had to wait until the end of an hour or to accumulate more than six tokens before exchange was permitted. When multiple FI 5 and FR 20 schedules were used, signaled by an orange and a green light that were varied randomly, however, the frequency of exchange had less of a detrimental effect on operant performance and it was possible to extend the delay to 200 min

(Kelleher, 1957c). One of 2 subjects responded differentially and appropriately for both schedules; the other responded at a uniformly high rate during both schedules. When schedules from FR 20 to FR 60 were used alone with token exchange permitted after accumulation of 60 tokens, both animals often paused early in the session but then responded at a stable high rate.

When Kelleher (1957a) compared performance on an FR 60 schedule with food versus token rewards, he found that the tokens supported higher rates of response in early sessions. In later sessions, however, the food became more effective than the tokens. The results showed that the pattern of responding depended on the conditions of reinforcement. In a follow-up study, Kelleher (1958b) studied schedules ranging from FR 30 to FR 125 and found performance comparable to that with food rewards, except for pauses at the beginning of each session. These pauses were eliminated when the chimpanzees were given 50 tokens at the start of the session.

In an experiment on discrimination learning, Kelleher (1958c) investigated behavior in which animals could make responses solely to gain information about the contingencies of reinforcement currently in force. In the presence of one of two stimuli, responses of operating a key were rewarded on an intermittent schedule. When the other appeared, there was no reinforcement. However, the animal could see the discriminative stimuli only by pressing a second key. Thus, responding to the second key produced no food rewards but only information about whether or not rewards would be delivered for operating the first manipulandum. It was found that when the discriminative stimuli disappeared, the animals interrupted their responding on the first key in order to operate the second key and thus gain information concerning the current contingencies. Kelleher explored the variables affecting this behavior.

Kelleher (1958a) also conducted an early study of concept formation. Two chimpanzees were faced with a stimulus board with an array of nine small panels, any of which might be illuminated or dark. Different combinations of panels were illuminated on successive trials. Responses on a telegraph key were rewarded in the presence of some patterns but not others. In the first problem, all positive stimuli had in common the fact that the bottom row of panels was illuminated. For the second problem, positive stimuli had in common that three panels, rather than two or four, were illuminated. Animals were trained with six positive and six negative arrays. Once they were learned, new arrays, not yet seen, were used to test generalization. If the animal had formed a "concept" of what the positive stimuli had in common, they should be able to generalize correctly from the familiar patterns to arrays not previously seen. They did this successfully for the first, but not the second, problem.

In his 1957 annual report, Nissen (1957a) described an experiment on delayed nonspatial matching to sample by Falk that appears never to have been published. With this procedure, the subject is shown a sample stimulus that then disappears for a delay, after which it can make a choice between one stimulus that matches and one that does not match the original. In previous studies it had been found that performance was better when the sample and test stimuli were similar with respect to spatial cues (i. e., location), rather than nonspatial cues, such as colors or forms. Falk used operant methodology by testing the animal in a location completely apart from the experimenter, requiring that the animal make a set response to make the sample stimulus disappear and begin the delay, and training the subject to press at different rates depending on related cues. These methodological improvements appeared to have produced better nonspatial delayed responding than in previous studies, as delays of up to

90 s were achieved. However, delays still were not as long as with spatial cues.

Falk (1958) conducted a study that was more compatible with the approach of the traditional laboratory psychologists than were most of the operant studies. It did not require any controversial food restriction but capitalized on the social grooming patterns of chimpanzee. He noticed that, after testing in the delayed matching-to-sample study, a chimpanzee, identified as both Malcolm and No. 141 in his report, would throw "a tantrum" unless permitted to groom the experimenter's arm. Falk built a simple hand-held plastic device with two patterns made of tape. Each day after the day's regular delayed-response testing, Falk conducted a series of trials in which Malcolm was to touch the correct pattern on the Plexiglas device. If he did, Falk sounded a "cricket clicker" and presented his right arm for a 30-s period of grooming. The chimpanzee learned the discrimination, responding to the square when it was positive and to the cross-stimulus when conditions were reversed.

Ferster's widow, Marilyn Gibert (1987) recalled that some of the chimpanzees failed to copulate as adults. She reported that "Charlie applied operant-conditioning techniques to teach them how to copulate" (p. 476). She also recalled that he had taught them to weigh themselves. I have not been able to confirm these recollections in other sources, but they are interesting.

In addition, while in Orange Park, Ferster conducted some research on the effects of behavioral stress on chronic infection at the University of Florida in Gainesville and received a job offer to establish a program of teaching and research in experimental psychology in its medical school (Ferster, 1956).

The annual reports for 1957 to 1959 show a total of 28 publications from the Yerkes staff; 12 of these had Ferster, Kelleher, or Falk as an author. It is clear that the procedures they brought to Orange Park were effective at least

insofar as they were able to generate a reasonable publication record.

CHANGING VIEWS ON DEPRIVATION

The use of food deprivation was a key issue; it remained a preferred method of motivation for some time in several laboratories. In the extensive research program with chimpanzees of Frederick H. Rohles, for example, one finds animals tested in various studies at 90% of body weight, after 30 hr of food deprivation, or after 18 hr of deprivation (Belleville, Rohles, Grunzke, & Clark, 1963; Rohles, 1961; Rohles & Devine, 1966).

In an effort to obtain contemporary perspective, I contacted Duane Rumbaugh of the Language Center at Emory University, a leading student of learning in chimpanzees. He replied that

I know of no study regarding the effects of privation upon operant conditioning with chimpanzees. Frankly, it is unnecessary. Chimpanzees can learn from early on that working on tasks can be interesting. . . . It would be unconscionable to reduce an ape to 80% body weight. (Rumbaugh, personal communication, September 1, 2002)

Later, he added,

It is no more appropriate to reduce the ape to 80% body weight than it would be to reduce the body weights of office and factory employees in order to get more work out of them or to do so with researchers so as to get more published papers from their laboratories. To do so brings chimpanzee and human alike to become so preoccupied with their hunger and food that it warps their social behavior and their ability to learn relationally—cognitively. They might work to survive, but not to benefit emotionally, socially, or intellectually from their work. It is both unnecessary and morally wrong to drive behavior and performance by such means as chronic starvation. (Rumbaugh, personal communication, September 2, 2002)

Perspectives and ethical views of research have changed dramatically over the 45 years since the operant program in Orange Park, and one must be careful not to evaluate the earlier work by standards not prevalent at the time. Further, the views of one scientist can-

not be said to represent a broad sample of scientific opinion. However, it is interesting that some of the underlying issues from the 1950s remain.

CONCLUSION

The operant conditioners swept into Orange Park rather suddenly and departed just as quickly. There is little evidence that they had a lasting impact on the facility. Research methodology appears to have continued along lines very similar to those prevalent before the operant phase. One wonders what happened to the equipment that was purchased. Like many psychologists of the time, the resident Orange Park scientists were not receptive to the new methodology.

The interactions between Henry Nissen and his associates and Charles Ferster and his group are a microcosm of factors that affected the reception of operant methodology in experimental psychology at large. A new experimental paradigm had been introduced and was gaining adherents. Although it may have been more appropriate for some experimental problems than for others, it was clearly successful in appropriate domains. The resistance of the traditionalists to a paradigm that challenged some long-held views is predictable and is illustrated in these interactions.

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