Contingencies over B. F. Skinner's Discovery of Contingencies

Julie S. Vargas
B. F. Skinner Foundation

B. F. Skinner began graduate school thinking he would extend Pavlov and Watson's stimulus-response analysis of behavior. He met the physiologist William Crozier who encouraged Skinner's inductive approach. With little supervision and a willingness to build new equipment and start over, Skinner's work was determined largely by the data he was getting. Bit by bit those data shaped the operant chamber that allowed Skinner to discover that postcedents, not antecedents determined what his rats did. Thus began a new science. The science eliminates the role of hypothesized inner agencies and instead relates properties of behavior directly to contingent events.

In *The Behavior Analyst*, the main journal of the American Association for Behavior Analysis, behavior analysis is called an "approach" (Dewsbury, 2003; Roche and Barnes-Holmes, 2003), a "view" (Moore, 2003), a "discipline" (Malott, 2004), a "field" (Malott, 2004; Madden, Klatt, Jewett, and Morse, 2004), or a "theory" (Vyse, 2004). What B. F. Skinner began is not an "approach", 'view", "discipline", "field", or "theory". It was, and is, a science**,** differing from psychology in its dependent variables, its measurement system, its procedures, and its analytic framework.[1] Skinner and his colleagues left us thousands of studies documenting functional relationships between contingencies and behavior. Behavior, they found, can be explained without appealing to internal physiology or hypothesized mental processes.

All sciences develop. Refinements of Skinner's experimental analysis of behavior continue to be made, but the science of the interplay between contingent events and behavior holds as solidly in the 21st century as it did in the 20th century when B. F. Skinner first made his discoveries. The revolutionary nature of those discoveries, in particular the excising of an internal agency credited with initiating behavior, still raises objections. But no objection can alter the way that contingencies work. Increasingly Skinner's legacy is spawning effective technologies in education (especially autism and physical education), pharmacology and counseling, business and industry, animal training and instructional design.

How did this small town boy discover the principles that continue to cause so much furor? In his article "A Case History in Scientific Method," Skinner makes light of the circumstances responsible for his discovery of the operant, suggesting "unformalized principles of scientific practice" such as "some people are lucky," (Skinner, 1956/1999). How much luck entered into his discoveries isn't certain but, as he was fond of quoting, "Fortune favors the prepared mind." Three "preparations" were critical; acquiring shop skills, adopting an inductive scientific approach, and responding to behavioral evidence rather than theories.

Skinner grew up in Susquehanna, a small railroad town in the hills of Pennsylvania. As a young boy, one of his primary activities was building things. Materials were plentiful in both his parents' home and that of his grandfather a few blocks away. He had few restrictions over

[1] Because behavior analysts seem reluctant to call behavior analysis a science, a small group has proposed the term "behaviorology" for the science of contingent relations between actions and other events. For a discussion of the relationship of behaviorology to behavior analysis, see Vargas, E. A. (2000).

what he could build. In addition to building tables and chairs, a cart that turned left when the wheel was turned right, and numerous gadgets, he made a steam cannon that, when enough steam built up, shot plugs of carrot across the unkempt backyard.

By the time he reached high school, Fred, as he was then known, had an extensive repertoire of trying things out to see how they worked. This experimental approach to life gained philosophical justification through challenging a teacher, Miss Graves, in whose class he announced that Bacon, not Shakespeare wrote "As you Like it". When Miss Graves said "You don't know what you are talking about," Skinner went to the library to bolster his position. In addition to a book called "*Bacon is Shakespeare,*" the library had Bacon's *Advancement of Learning, Novum Organum,* and a book of his essays. Since the *Advancement of Learning* begins with a long praise of the British king, it is difficult to imagine an American teenager reading much of that book. But *Novum Organum* is another story. At the beginning of the 20th century, science had given citizens electricity, the telephone, the automobile, and the radio. Though *Novum Organum* offered no help for the young Fred's arguments in school, Bacon equated with science the kind of tinkering Fred loved to do. Bacon did not believe in following an idea just because it was sanctioned by an established authority. He advocated examining events directly. Bacon's distrust of authority as the source of truth must have appealed to a teenager who was challenging his own teacher. In any case, what Skinner read of *Novum Organum* stayed with him. In his experimental research in graduate school, he used procedures consistent with those Bacon recommended—direct observation and a search for functional relations between dependent and independent variables.

After college, and a year and a half trying, and failing, to become a writer, Skinner applied to Harvard University to study psychology. He began graduate school in 1928 with three books he thought would prepare him for a scientific study of behavior, Bertrand Russell's 1927 *Philosophy,* John B. Watson's 1924 *Behaviorism*, and the new 1927 English translation of I. P. Pavlov's *Conditioned Reflexes.* For the department he was to enter, he couldn't have picked less appropriate books. The Harvard Psychology Department was dominated by E. G. Boring, a disciple of Titchener whose school of psychology Watson called, in the very book Skinner had on his bookshelf "the ancient days of superstition and magic."

Perhaps it was fortunate that Boring was on sabbatical that first semester of 1928. Skinner signed up for a course in the Department of Physiology whose text discussed Pavlov. Through this course Skinner met the physiology chairman, William Crozier, who was a disciple of Jacques Loeb. Crozier, like Loeb, insisted on studying the organism as a whole. Crozier's love was tropisms and he encouraged tropism research in the department's courses. By the end of Skinner's first year of graduate school, he and another student submitted a tropism article (on ant behavior!) to the journal of which Crozier was editor. The five references for this article give an idea of what Skinner was reading: All five references are from journals in physiology.

Crozier encouraged Skinner in looking for dependent variables that involved the behavior of the whole organism, and in finding functional relationships between experimental treatments and behavior. Tropisms did not interest Skinner, but Pavlov and Watson's work did. Pavlov's respondent conditioning showed clear effects of pairing a neutral antecedent stimulus with a stimulus that already produced a reflex response. Watson extended this analysis to children's behavior, again attributing the cause of what they did to antecedent stimuli. Skinner began his own line of research in the summer of 1929, unaware that he was to challenge both Pavlov's and Watson's stimulus-response analysis. Both Pavlov and Watson had used "trials" where an animal was placed in an experimental space and its response to a stimulus was measured. Intending to continue their line of research, Skinner built a six foot long runway starting with three steps. He called it the "Parthenon." At the runway's end he placed food. Careful to control extraneous variables, the whole runway was enclosed in a large box. Observation was possible through a small peep hole. To release the experimental subject (a rat) without disruption, Skinner constructed a pneumatic release to make sure the door opened silently at

the start of each run. When the rat came out of his start box and down the steps, Skinner made a carefully calibrated sound, and the rat's behavior was recorded. The records from this experiment are pencil lines on six foot long rolls of paper. It looks as though Skinner held a pencil against paper unwinding from a rotating drum, perhaps using one of the kymographs available from the psychology department. When the rat came out of the tunnel, Skinner would move the pencil up for each step descended. Then he sounded the click. When the rat ran back into the tunnel he moved the pencil back down to the original line making stalagmite shapes on the line. On his paper strips, Skinner noted the temperature, the times, and the weights used to calibrate the sounds. It was all very scientific.

For nearly a month Skinner used this apparatus with at least six different rats. He varied the weights and examined his long paper strips, but he could not see any clear relationship between his experimental procedures and the rats' behavior other than their adaptation to the clicks. On the last record he wrote, "Apparently not responding to click at all." Adaptation, though clearly shown, was nothing new and he tore apart the equipment. Meanwhile his rats had babies. He designed another piece of apparatus to see how baby rats responded to a pull on their tails. The records from this experiment are wiggly kymograph scratches on smoked paper. Crozier, returning after a summer out of town, was impressed with Skinner's work. But Skinner could not see any clear functional relations. He had reached a dead end.

Harvard's curriculum permitted students to take mostly research courses, which suited Skinner well. Shuttling between departments, no one kept track of what he was doing. Of his work, he wrote,

> In my research courses ... I worked entirely without supervision. No one knew what I was doing until I handed in some kind of flimsy report. Possibly the psychologists thought I was being counseled by Crozier and Hoagland, and they may have thought that someone in psychology was keeping an eye on me, but the fact was that I was doing exactly as I pleased (Skinner, 1979, p. 35).

With no one suggesting experiments or offering him apparatus to use, Skinner's behavior became increasing under control of his experimental results, namely how his rats were responding to his experimental procedures. There could not be any better contingencies for the discovery of something entirely new.

The progress towards that discovery was not smooth. By the beginning of his second year Skinner still did not have a good dependent variable, although he was using the kymograph for recording responses along a continuous line. He was still looking at the response to a click, and went back to a runway. The "Parthenon" had not produced results, but maybe a longer runway would. By the middle of October he was running rats down a runway 8 to 10 feet long. He attached a kymograph that had three lines all running in real time. The top one had hatch marks for portions of a second. The second showed the click, and the third the behavior of the rat. The records from the long runway end November 23, about a month after the first records. Another dead end.

At the beginning of the next semester, Skinner designed a runway to save himself work. This new runway was rectangular in shape. After the initial run, the rat could return to the start position without being carried from the end to the start. To get the rat to go around the rectangular runway, a food dish was added close to the start for the next run. Without realizing it, Skinner had made a breakthrough. He had eliminated "trials". Instead of interrupting the flow of behavior, he no longer interceded during an experimental session. As Skinner sat and watched his rat's behavior, he found that they paused at the food dish sometimes as long as five to ten minutes before starting another run. This was more interesting than the response to a click. Skinner began timing those pauses. But though happy with his new data, he found sitting and timing the intervals tedious. As usual he solved his problem with a new piece of equipment. He put the whole runway on a fulcrum so that the rats tipped the runway as they ran from one end to the other. Hooking the kymograph up to this system, Skinner made

a further refinement: By adding a weight to the needle that scratched the line on the black smoked paper, his records would be curves instead of being hatch marks on a straight line. Here was another breakthrough. Now, not only were there no "trials", the rats recorded their own behavior in a cumulative record whose slope showed the rate of their activity. Skinner could set up an experiment, leave, and return a couple of hours later. The records Skinner was now generating had no specific antecedent stimulus for the rats' actions. But he was still looking for causes in conditions that preceded the behavior to be explained.

The surviving records from the rectangular runway show nearly a month's work (February 6 to March 1, 1930). Skinner graphed time in minutes for the last 50 runs. The lines on the graphs are jagged, bouncing between half an hour to two hours descending to a generally lower level over the last five days. No independent variable is in evidence. Skinner describes the move to the next piece of apparatus as follows:

> Eventually, of course, the runway was seen to be unnecessary. The rat could simply reach into a covered tray for pieces of food, and each movement of the cover could operate a solenoid to move a pen one step in a cumulative curve (Skinner, 1956/1999, p. 116)

"Of course"? No one else who was using runways in 1930-31 switched to a box with a door. But others were not measuring "rate of eating behavior". They were recording percent correct in T-mazes or time for each trial in experimenter-initiated runs.

Skinner had finally found a good dependent variable and a recording system that showed each response at the exact moment it occurred. The slope of the curve showed changes in rate of response and provided a sensitive record over the entire session. With his new apparatus and cumulative recorder, Skinner started getting results. In March of 1930 he wrote home,

> The greatest birthday present I got was some remarkable results from the data of my experiment. Crozier is quite worked up about it. It is a complicated business and deep in mathematics. In a word, I have demonstrated that the rate in which a rat eats food, over a period of two hours, is a square function of the time. In other words, what heretofore was supposed to be "free" behavior on the part of the rat is now shown to be just as much subject to natural laws as, for example, the rate of his pulse (Skinner, 1979, pp. 59-60).

Ingestion was the first in a long line of behaviors previously thought to be "free" that Skinner was to show to be under experimental control. In 1930 he had not yet rejected antecedents for the source of explanation of behavior, nor had he relinquished the term "reflex", but his research was bearing fruit, and it took on a fever pitch.

> Everything I touched suggested new and promising things to do. I slept well at night, but my days were feverishly active... I...tried to relax, but it was no use. I thought constantly of my rats, designing new pieces of equipment and formulating new questions to be answered. I lost weight and my heart began to skip beats. I went to a doctor for a checkup and learned there was nothing the matter (Skinner, 1979, p. 38).

Every day in April of 1930 he ran at least two rats for two-hour sessions each. The curves of "ingestion" were remarkably smooth, starting steep and gradually flattening out as time went by. Different rats produced curves of differing heights, but all showed the same reliable path. Occasionally a rat would stop for as much as fifteen minutes, but when it again ate it made up for the lost time, soon reaching, and following, the usual curve. That suggested another procedure. Skinner tried locking the door to produce an interruption. Sure enough, when the door was again unlocked, the rats speeded up until the recording line joined the projection for the original curve. Skinner was tremendously excited. He showed his results to Crozier who urged publication. Without skipping a day of research, Skinner managed to submit "On the Conditions of Elicitation of Certain Eating Reflexes" by April 21, 1930.

He now had data for a doctoral thesis. In the fall of his third year, Skinner worked on his thesis. The first half discusses the history of the term reflex. The authors he cites discuss physiological work with reflexes and methodology. The second

part of the thesis examines the eating behavior of the rat, with deprivation (and emotional disturbance) as the independent variables, still on the antecedent end of the responses recorded. The apparatus shown has a door that the rat pushes to obtain food.

Skinner's ambivalence about the nature of what he was studying shows in two contradictory statements in his thesis: The first follows the standard S-R formulation.

> We shall assume that every movement of an organism is in response to a stimulus....and in a suitable experimental situation we may proceed to examine the conditions under which a selected reflex is or is not elicited (Skinner, 1930, pp. 62-63).

But the second, 10 pages later, shows doubt about the requirement for an initiating stimulus.

> The report of the experimental material that follows could very well be made without reference to the reflex: we should then be discussing "rates of eating". Nevertheless the experiments themselves grew out of considerations of the sort we have here been concerned with and the results are satisfactorily interpreted in harmony with reflex doctrine. Accordingly, the experimental report will be made in the terminology of the reflex (Skinner, 1930, p. 73).

Skinner kept working with the box with the door for quite a long time. It is still shown in "Drive and reflex strength" submitted on July 7, 1931, seven months after the date on his thesis. Of course, he was getting nice data with this apparatus. But those data had limitations. Since the push on the door was correlated one to one with obtaining a pellet, the data were described as an "eating response" or "ingestion", not as a push on the door. Looked at that way, the relationship between actions and their consequences could not be seen.

The first indication of a box with a lever appears in notes on cumulative records saved from April of 1931. This new apparatus recorded bar presses with a cumulative recorder and each press turned a disk with holes around the edge into which Skinner put pellets of food. When the rat pressed the bar, the cumulative recorder stepped the pen up one notch, and the disk turned, dropping a pellet of food down a tube to the food dish in the box. The shift from a door to a bar separated the act of pressing the bar from obtaining food, making bar pressing and reinforcement two events rather than one. That permitted varying the relationship between action and reinforcement so that functional relations between action and consequence could be seen. It was not long before a "variation" occurred and Skinner was terribly excited. As he described it in his autobiography,

> A rat was pressing the lever in an experiment on satiation when the pellet dispenser jammed. I was not there at the time, and when I returned I found a beautiful curve... The change was more orderly than the extinction of a salivary reflex in Pavlov's setting and I was terribly excited. It was a Friday afternoon and there was no one in the laboratory whom I could tell. All that weekend I crossed streets with particular care and avoided all unnecessary risks to protect my discovery from loss through my death (Skinner, 1976, p. 95).

He wrote to his colleague and best friend, Fred Keller, about his "brand new theory of learning" (Keller, 1931). He was still trying to fit his "new theory" into Pavlov's reflex frame, talking about the stimulation from the lever. But eventually he realized that what he was seeing were not actions controlled by antecedents like those Pavlov had described, but actions controlled by immediate postcedent events. By February of 1932 he submitted an article describing a "second type of conditioning" (Skinner, 1932) that he later called "Type I" and still later "operant". Finally he had an apparatus to record the rate of a specific act of the whole organism as a function of experimental manipulation. In the next few years, supported by fellowships, he investigated all of the basic contingencies including intermittent reinforcement, discrimination and generalization, delay of reinforcement, and even the effect of some drugs. Much of this research appeared in the *Behavior of Organisms*, the book that launched the science for which he is known. It describes how the interaction between individual actions

and independently measurable events determines rate of behavior.

Skinner did not ignore what psychologists call "cognitive processes". Behavior occurring inside our bodies, like thinking, develops through the same basic processes of operant conditioning as talking or any external operant behavior. Acknowledging that human behavior is no more free of contingencies than our movements are from the physical restraints of gravity has been difficult for the general public. Major changes in our understanding of the world never pass easily into the mainstream. But the downstream effects of Skinner's work on the impact of contingencies on properties of behavior has altered the course of behavioral science forever.

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