Quantitative Order in B. F. Skinner's Early Research Program, 1928–1931

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The purpose of this article is to provide a coherent story of Skinner's graduate-school (1928–1931) research projects, adding to Skinner's own accounts a different emphasis and a number of interesting details. The story is guided by the proposal that a search for quantitative order was the "unifying force" amid the variety of apparatus changes and shifts of research topic in Skinner's early development as a researcher. Archival laboratory-research records from several apparatuses which Skinner constructed between 1928 and 1931 (1) indicate that his research program was more complicated than he has implied; (2) show that he worked on three interdependent lines of investigation simultaneously; (3) suggest that change or abandonment of an apparatus or a project was markedly affected by his success (and failure) in his primary objective, which was to find quantitative orderliness in measured behavior. Frequent apparatus change in the period of 1928 to 1930 ceased when he obtained quantitative orderliness in the panel-press and lever-box preparations. In the examination of archival records, questions about the enterprise of biographical understanding are considered.

Key words: Skinner, B. F., history of psychology, quantification, cumulative record, apparatus, methodology

It is argued that research would be *aimless and disorganized* without a theory to guide it. The view is supported by psychological texts that . . . describe thinking as necessarily involving stages of hypothesis, deduction, experimental test, and confirmation. But this is not the way most scientists actually work. (Skinner, 1950, pp. 194–195, emphasis added)

I never attacked a problem by constructing a Hypothesis. I never deduced Theorems or submitted them to Experimental Check. So far as I can see, I had no preconceived Model of behavior . . . Of course, I was working on a basic Assumption—that there was order in behavior if I could only discover it. (Skinner, 1956a, p. 227)

When we try to make sense of an extended series of happenings by using the rough-and-ready conceptual system of daily-life explanation, we engage in sev-

eral different activities. We try to recognize how disparate items are related as parts of *the same* development, or we try to see how their similarities (*qua* parts) are greater, or at least more important, than their more readily noticed differences. Moreover, we think of the development as a unitary thing which is tending toward a goal, and we evaluate the distinguishable parts as contributing to or hindering progress toward that goal. Occasionally, we make informed guesses at the "causes" of particular parts of the development, especially of those that are hindrances to progress; but, when it is difficult to ascertain the cause of an event, we try to see how it fits into the overall pattern of surrounding events or what contribution it makes to reaching the goal of the development. The study of historical developments of all sorts, including personal history, makes extensive use of this pattern-seeking strategy for understanding, since experimentation and other sources of knowledge based on manipulation are usually unavailable.

In looking for a pattern in personal development, we typically call attention to something that we see as *central* (e.g., an enduring personality trait). We may think of the trait as the principal causal factor behind various observable activities of

Reprints are available from S. R. Coleman, Department of Psychology, Cleveland State University, Cleveland, Ohio 44115. This research was supported through the Expense Grant Program of the College of Graduate Studies at Cleveland State University. A preliminary version of this paper was presented at the annual meeting of the Cheiron Society, June 13, 1986, at the University of Guelph, Ontario.

Archival figures, data, and notes are used with permission of the Harvard University Archives and of B. F. Skinner. I wish to acknowledge courteous assistance from B. F. Skinner, Clark Elliott, and Cuthbert Daniel; good suggestions by this journal's editor and reviewers; and helpful conversation with Jack Seigel, Phil Emerson, Louis Milic, and Brian Clearmountain.

the individual. Or we may imagine that a unifying process (e.g., a personal theme) is more powerful than the "forces" responsible for diverse and *peripheral* phenomena. Continuing the spatial metaphor, we could call the latter forces "centrifugal," since they tend away from the center and produce diversity and inconsistency that make patterns hard to discern. It is possible, though, to assume that centrifugal forces are very significant. There is a novelistic tradition, the novel of manners and circumstance, in which detailed description of social life, conventions, and their binding effect on main characters is the primary stuff of the novel. Examples include the novels of Jane Austen and Honoré de Balzac, Dickens's *Hard Times*, Thackeray's *Vanity Fair*, Sterne's *Tristram Shandy*, many of Trollope's works, and much of the American twentieth-century Realistic novel (Upton Sinclair, John Dos Passos, Sinclair Lewis, etc.). While it is true that the principal characters in these novels do provide narrative unification, it is clear that in the tug of war between unity and diversity a considerable latitude of emphasis is permissible. Accordingly, in making sense of Skinner's early development as a laboratory researcher, it would be legitimate to emphasize either unifying factors or centrifugal factors, even though daily-life explanation might incline one more to search for unity.

Unity and coherence are not readily apparent, however, when one examines B. F. Skinner's personal development as reported in his autobiographies (Skinner, 1976, 1979, 1983) and compares it with the lives of individuals who claim to have been dominated by a small number of related and enduring concerns, for example, Carl Jung (Jung, 1961). One might even be persuaded that the search for coherence is an illusory pursuit after reading Skinner's pronouncements on personal identity (e.g., Skinner, 1953, pp. 284–288; 1974, pp. 164–167, 247), especially if one suspects such pronouncements are at least partially autobiographical (cf. Skinner, 1976, p. 255). Of course, difficulty in finding one or more unifying

themes in Skinner's autobiographical writings could merely be a consequence of his lifelong preference for a descriptive, circumstantial writing style (Coleman, 1985), combined with a hard-headed reluctance to hypostatize unifying forces. Skinner's literary descriptivism leaves the reader with a manifold of particulars, in which diversity is the dominant impression. While this state of affairs might conceivably stem from mere willfulness as an autobiographer, "the facts" of Skinner's life usually support an impression of diversity. For example, between 1928, when he enrolled for graduate study at Harvard, and 1931, when he received the doctorate, Skinner constructed eight or nine different apparatuses to carry out a variety of laboratory investigations of animal behavior. Such diversity in his research projects of that period justifies attention to circumstance, happenstance, and other centrifugal factors. In describing that period, Skinner's own "Case History in Scientific Method" (Skinner, 1956a), therefore, placed appropriate emphasis on the importance of luck, accident, simple curiosity, and even laziness, in his scientific development. Though he argued that such factors were very important in his own development, they had been given virtually no recognition in the formalistic and theory-conscious logical-positivist philosophy of science of that time.¹

In dismissing the alleged importance of theories—in the specific sense of "the-

¹ It is useful and consistent with Skinner's own pronouncements (e.g., Skinner, 1957, pp. 453–456) to regard his construction of a case history as behavior involved in a contingency: His emphasis on circumstance in his own conduct as a scientist (Skinner, 1956a) served the function of questioning a philosophical picture of the behavior of scientists (e.g., Skinner, 1956a, pp. 221–222), a function which is understandable enough in the B. F. Skinner of the early 1950s (e.g., Skinner, 1950). Of course, an emphasis on centrifugal factors is also *simply consistent* with his highly descriptive practices of the same period (e.g., Ferster & Skinner, 1957), apart from any plan to take exception to a philosophy of science that was widely endorsed by his contemporaries. We will not try to decide in favor of either possibility.

ory" described in "Are Theories of Learning Necessary?" (Skinner, 1950, p. 193)—Skinner (1956a) took the search for order more or less for granted, as is shown in the second quote which opened this article. Skinner's quarrel was with the alleged necessity of behavior theory, not with fundamental assumptions such as the lawfulness of nature. In discounting the necessity of theory in behavioral research, it was perhaps inevitable that he would emphasize centrifugal forces in his development, since, in relation to theory, they lie at the opposite end of the central-peripheral contrast. It is true that in his autobiographical statements, he has acknowledged the importance of such fundamental assumptions as the idea that behavior is quantitatively orderly and that the scientist's task is to demonstrate such order (Skinner, 1956a, pp. 223, 224, 227; 1979, pp. 59–60, 67–68, 99–100). Nonetheless, since he has devoted more space to the description of circumstance, the drift of his autobiographical accounts is decidedly centrifugal. In his descriptions of his own scientific conduct (e.g., Skinner, 1956a), this feature, or its presumed consequence of "aimless and disorganized" research—see the first quote above—has bothered various writers, prompting some to criticism of his research style (e.g., Bixenstine, 1964) and others to defense (Sidman, 1960). An objective of the present article is to make more apparent the importance of Skinner's search for quantitative order as a unifying factor in his early research program.

MATERIALS AND METHOD

Skinner saved an assortment of records from the research projects of his graduate-student period, and gave these materials to the Harvard University Archives in early 1983. They contain much of the information that Skinner used in his own published accounts of his graduate-student development as a scientist (Skinner, 1956a, 1967, 1979). Of course, they also include records and notes that Skinner did not include in his accounts.

The present paper was based primarily on a study of these materials,² though Skinner's autobiographical writings and his published research of the 1930s were also consulted. Prof. Skinner aided in deciphering a few records that were difficult to understand, and he responded to questions about all the records, during a ten-day period in 1985, in subsequent correspondence, and in remarks on an earlier draft of this paper.

The present paper adds to Skinner's (1979) own account enough additional details for a separate story, and it makes more explicit than does his "Case History" (Skinner, 1956a) that his early program of research was much affected by successes and failures in his exploratory quest for quantitative behavioral regularity. In the narrative, questions and considerations regarding biographical understanding are raised.

BACKGROUND SKETCH

A history of the idea that nature exhibits quantitatively expressible orderliness would be distracting, even if it were within our capacity. That idea and its successful demonstration go back into ancient science but also were prominent features of the Scientific Revolution of the seventeenth century (e.g., Hall, 1954/1956, pp. 224–234; 1963/1981), which first affected the physics and astronomy of that period. Large-scale efforts at quantification in the life sciences (including physiology) had to await the second half of the nineteenth century. W. J. Crozier, Skinner's graduate-school mentor and dissertation supervisor in Harvard's Department of Physiology, was a champion of quantitative physiological research. Because none of the other figures

² These materials are cataloged in the Harvard University Archives as: Burrhus Frederic SKINNER, Laboratory Research Records, 1929–1940, Shelf Number HUG (B) - S485.45. The materials are in 15 folders, roughly chronologically arranged and preserving the order in which Prof. Skinner arranged the materials for accession by the Archives. References to these materials in the present article will include only the Folder number.

who were important sources of ideas or inspiration for Skinner—such as Watson, Pavlov, Bertrand Russell, H. G. Wells, Sherrington, and Magnus (Skinner, 1976, 1979)—were predominantly quantitative in their research methods or objectives, we must assume that Crozier was the principal source for the quantitative emphasis.

Since Skinner's quantitative background was not particularly strong (e.g., Skinner, 1979, p. 67), there is no obvious affinity between Crozier's quantitative scientific approach and whatever leanings Skinner may have had in that direction. The affinity of these two people was probably in other issues and dimensions, with quantification of lesser significance. Skinner lacked the quantitative background to become a Crozier disciple, and in addition he had no wish to be one (B. F. Skinner, personal communication, July 9, 1985). Nevertheless, Skinner's scientific strategy as a graduate-student researcher was to assume that orderliness, of at least a simple quantitative sort, existed in the behavior of the freely moving organism, and to seek to demonstrate this in his various preparations. To make a coherent story out of the available archival records is the task of the remainder of this paper.

COURSEWORK ORIGINS

Skinner's first two behavioral projects were undertaken as course requirements. His first published research was carried out in collaboration with T. C. Barnes for Harvard's Physiology 20a course (titled "Dynamics of Vital Phenomena") which Skinner took in the spring of 1929.³ The project employed ants, and the behavior of interest was their locomotion upward on a slanted surface (negative geotropism). The degree of slant of the surface could be made to vary as the independent variable, and the angle of straight-line sections of the ant's path along the sur-

face served as the dependent variable. During the spring and summer, he and Barnes worked together in the project's execution, analysis, and write-up. Skinner's autobiography contains an amusing description of the writing process (Skinner, 1979, p. 19).

In the spring of 1929, Skinner also took a research course (Psychology 20c) supervised by Walter Hunter, visiting from Clark University. Skinner and Dwight Chapman collaborated on a project involving insightful learning in squirrels, a topic that Skinner said was prompted by the arrangements for studying problem-solving in apes which Köhler (1925) had described in *The Mentality of Apes* (B. F. Skinner, personal communication, August 18, 1986; Skinner, 1979, pp. 30–31). The project was probably supervised by Walter Hunter, but got no publishable results (Skinner, 1979, p. 30). Skinner eventually provided his squirrels a running wheel, apparently for purpose of exercise and, at least initially, without plans to obtain behavioral data from that apparatus (Skinner, 1979, pp. 77–78).

LOCOMOTION RESEARCH

Skinner's earliest independently conducted research concerned locomotion (Skinner, 1979, pp. 32–34). He devised an apparatus he called the Parthenon (Skinner, 1956a, Fig. 1), a tunnel into one end of which a rat could be introduced from Skinner's silent-release carrying box, and out the other end of which the rat would emerge and hesitantly start to descend a couple of steps until Skinner presented a calibrated click which (so he thought at the time) inhibited the reflexes of progression (Magnus, 1924) and elicited withdrawal back into the tunnel (Skinner, 1956a, p. 223 and Fig. 1). Skinner manually recorded the progress, withdrawal, and re-emergence of the rat by moving a pencil across chart paper driven at a known speed by a motor (B. F. Skinner, personal communication, August 18, 1986). His equipment permitted him to measure what he took to be a period of prepotency of the click-elicited withdrawal reflexes over the re-

³ We acknowledge the assistance of Dr. Margaret Law, Registrar of Harvard University, who made available a transcript of Skinner's courses, from which the course titles and numbers are taken.

flexes of progression. That period could be measured as a response latency: the time interval between the rat's withdrawal into the tunnel and its subsequent complete re-emergence, at which point the click was sounded again. Surviving records from the Parthenon work are poorly labeled, and it was impossible—even with Professor Skinner's aid—to determine what was the meaning of some of the numerals that he had written many years ago on the chart paper. The records do not explicitly tell why he abandoned the Parthenon, of course, but had he plotted the data of the most easily deciphered records (Folder 1), he would have found the results displayed in Figure 1.

The figure shows that the duration of withdrawal decreased with repeated presentations of the click: that is, the figure depicts "adaptation," as he called it, or habituation of withdrawal. Though habituation is suggested by the course of the function, large variability is probably the more noticeable feature of Figure 1. Elsewhere we have suggested that Skinner was philosophically concerned to "conquer" variability and to defend determinism (Coleman, 1984, pp. 474–475, 479–483). The most direct strategy for reducing variability is to change or replace unsatisfactory apparatus. Since he was already acquainted with standard physiological-laboratory apparatus, it is unlikely that Skinner was attached to his own crude manual-recording procedures. We suggest that these—and, of course, those described by Skinner (1956a)—are the prototypical circumstances under which Skinner made apparatus changes at this stage of his career.

Though Skinner had abandoned the Parthenon, he continued to study locomotion and posture in several other devices. In the summer of 1929, he planned observational studies of posture and locomotor activity in very young rats. According to surviving notes, which Skinner later dated to the summer of 1929, his plan was to photograph or draw different postures that resulted from being lifted up by the tail with more or less support of the torso; and to determine how additional support—for example,

Figure 1. Habituation of withdrawal in Skinner's Parthenon research. Duration of withdrawal as a function of repeated presentations of a calibrated sound. Figure drawn from values obtained from raw-data record. *Note.* Question-mark indicates that the archival records were not explicitly labeled with trial numbers, necessitating guesswork in creating this figure.

support of the forelimbs—altered the position of the head and tail, how it affected the amount of body tremor, and how it affected the various flexion and extension reflexes which could be elicited (uncataloged material in Accession 9710, Harvard University Archives). The influence of Magnus (1924) is certainly noticeable in this surviving research plan, but the archival collection includes no results from the project, which was probably never carried out (B. F. Skinner, personal communication, August 18, 1986). It is reasonable to conjecture that the absence of a measurement procedure or device, thus precluding quantitative results, was the Achilles' heel of this summer project.

According to his later reconstruction (Skinner, 1956a, pp. 223–224), Skinner abandoned the possibilities of drawing and photography in favor of a mechanical recording device, with Skinner recording the force with which baby rats thrust against a horizontal surface when the tail is firmly held (Skinner, 1956a, Fig. 2; 1979, pp. 36–37). He did not mention any processes that he actually investigated, but since each archival record is marked with an indication of the room

Figure 2. Drawing of a section of kymographic record from Skinner's torsion-wire runway. A downward pen deflection indicates anterior-to-posterior runway movement produced by locomotor thrust of the rat. A rising pen excursion indicates posterior-to-anterior movement of the runway.

temperature, it is likely that a study of the temperature coefficients of reflexes was his primary concern (Skinner, 1979, p. 36). It is worth noting that his Parthenon records also include penciled notations of temperature.

The torsion-wire recording method was carried over into yet another device, a runway suspended on tightly drawn, transverse piano wires. The force of the locomotor thrusts, which constitute the running of the rat along the runway on its way to the food which Skinner delivered to it at the end of the runway, caused the runway to move slightly along its longitudinal axis, and these slight movements were transferred onto a paper kymographic record by means of a writing lever connected appropriately to the device, as in Figure 2 of his "Case History" (Skinner, 1956a, p. 222). Skinner planned to deliver a carefully calibrated sound that would elicit a sudden halt in locomotion, and to study habituation of this reflexive halt under repeated presentations.

This apparatus, developed in the fall of 1929, lasted into 1931. From this device, he collected celluloid-like gelatin records (Skinner, 1979, p. 37) of rat locomotion in the fall of 1929. Little or no amplification was used, and the records are tiny horizontal squiggles of one or two millimeters in amplitude (Folders 1 and 6). He recorded latency and amplitude (standard reflex measures) of the halt, and he took the duration of the halt to be analogous to the after-discharge of re-

flexes reported by reflex physiologists (e.g., Sherrington, 1906/1961, pp. 27–35). In his application for a National Research Council (NRC) fellowship in 1930, he included a transcribed record from the runway, demonstrating "the measurement of reflex characteristics in the intact organism," as the project was titled in his application. Figure 2 is a redrawn portion of the record that Skinner included in his NRC application (BFS to NRC, September 1, 1930, in uncataloged material of Accession 9710, Harvard University Archives).

The figure shows the small pips that resulted from footfalls of the rat, and the sudden lurch of the runway that resulted from the click. Skinner's objectives were (1) to measure these "reflex characteristics" in the intact, freely moving organism, (2) to investigate the phenomenon of habituation to the sound, and (3) "to determine whether or not the behavior of the rat was as uniform as that of a 'preparation'" (BFS to NRC, September 1, 1930, p. 7).

In his application for the NRC fellowship, Skinner complained that the wire-supported substrate had a natural frequency that was uncomfortably close to the frequency of rodent footfalls during a fast run in such a runway (28–32 per second), threatening an artifactual oscillation that would interfere with measurement. It appears that the runway also bounced vertically in response to the up-and-down motion of the running rat (Skinner, 1979, p. 52). Skinner proposed to replace the wires, during his fellowship year, with vertical glass plates whose length could be so chosen that the oscillation frequency of the device would be outside the range found in rat locomotion. In his NRC application, he proposed to "amplify the movement electrically by means of a carbon button and an amplifier unit" (BFS to NRC, September 1, 1930, p. 10; cf. Skinner, 1956a, Figure 4), but, though Ralph Gerbrands constructed the glass-plate device at Skinner's request, "I never got around to electrical amplification" (B. F. Skinner, personal communication, July 8, 1986).

THE DIVERSITY OF SKINNER'S RESEARCH

We have gotten a bit ahead of the story, chronologically, because the NRC fellowship application was made in the fall of 1930. At that point, Skinner had already devised a prototypical operant chamber (which we will describe in the section called "Breakthrough," below), and he proposed in his NRC application "to carry on both [i.e., the runway and prototype operant chamber] lines of investigation simultaneously" (BFS to NRC, September 1, 1930, p. 4). Moreover, he had a third project, which involved a running wheel. Therefore, not only did Skinner make serial modifications in a given class of apparatus in which runways gave rise to the Skinner box—that is the development which he described in his "Case History" (Skinner, 1956a)—but he simultaneously maintained several lines of investigation using distinct apparatus: (1) the runway-to-box development (Skinner, 1956a); (2) suspended runways which originally were part of the runway-to-box line, but which, for a period of time, enjoyed a life of their own and were used from 1929 into 1930; and (3) the running wheels that he first used in 1929 and described at length in 1933 (Skinner, 1933a).

It is not clear why Skinner abandoned the suspended runways some time in 1931—he did not mention the runways in his application for reappointment for a second year as NRC Fellow (BFS to NRC, December 8, 1931). He certainly had not abandoned reflexological ideas (e.g., Skinner, 1931) nor work on locomotion (e.g., Skinner, 1933a). In fact, the successful measurement of reflex characteristics is likely to strike us, knowing his NRC plans, as a major achievement. The fact that the project "did not go anywhere" (cf. Skinner, 1979, pp. 51–53) should appear strange. In setting aside the runways, he dropped his efforts to assess directly the validity of the reflex as a construct for the description of the behavior of the freely moving organism (Skinner, 1931). The fact that the archi-

val holdings contain only raw data from the wire- and glass-mounted runways and no tables or graphs of dependent variables suggests a mishap of some sort in his efforts to find quantitative orderliness or to assess the generality of the concept of the reflex (Folders 1 and 6), but we will not pursue what might be a lengthy tangent in trying to resolve that discrepancy. Prof. Skinner offered the simple and direct explanation that "I soon turned to plotting ingestion curves [see below, "Quantification"] Again it was a question of finding which job seemed to be more important" (B. F. Skinner, personal communication, July 8, 1986). We offer, as a friendly amendment, the possibility that "the runway became a running wheel," which we will now elaborate briefly.

WHEELS

Skinner did not begin systematic running-wheel research until early 1931 (Skinner, 1979, pp. 77–78). The research on the running wheel eventually produced a paper that was completed and received in the editorial office of the *Journal of General Psychology* over a year later (August 23, 1932) and was published the following year (Skinner, 1933a). It is his only published paper that used the running wheel. This 21-page paper is swelled by technical exposition of the construction of a virtually ideal running wheel and of the physics of converting the forces of rat locomotion on a level surface into circular motion. Though somewhat irrelevant to concerns of psychologists other than those planning to use a wheel, the physics of locomotion in the wheel sets limits to whether any "running wheel may . . . be regarded as a substitute for a level straightaway" (Skinner, 1933a, p. 7). Skinner's physical exposition, for all its technicality, described a wheel that could substitute for a runway; the paper included a six-page section on fabricating an ideal wheel, recording the rat's locomotion, and arranging for the rat to have access to the wheel only for an automatically timed

period each day. In the construction of the wheel, Skinner even made use of information he had obtained from his suspended runways, namely the number of footfalls per second (28–32) that are made by a rat at a “fairly brisk” pace. These considerations suggest that some of the interests behind the runway investigations were pursued through running-wheel apparatuses.

Of far greater importance for our story, Skinner gave up the runway’s discrete-trial, reflex-type measures in favor of a cumulative measure afforded by the wheel. This shift from discrete-trial measures to cumulative and rate measures also occurred in the runway-to-box apparatus development, and we will describe that shift in the next section. A running wheel was the first apparatus to provide him with quantitative data in the form of a cumulative measure, since it yielded cumulative number of wheel-turns as a function of time. Since Skinner’s earliest records of running-wheel cumulative data, covering the period June 19 to August 4, are on the reverse side of a June 12, 1929 letter to B. F. Skinner from Harvard Dean Lawrence Mayo (Folder 2), it would seem that Skinner was using a cumulative behavioral measure in a portion of his research as early as June of 1929. He did not publish results from his running wheel research until somewhat later (Skinner, 1933a), and he did not obtain the data for that publication until the spring of 1931, by which time he had already developed a version of the operant chamber. The running-wheel project was probably of secondary importance even at the time, and he does not even mention it in his “Case History” (Skinner, 1956a). Nonetheless, though the records do not permit proof of the assertion, it is possible that he borrowed the running wheel paradigm—that is, cumulative performance as a function of time—for his other research. We can, at the bare minimum, say that he had already done cumulative recording before he applied that measurement paradigm to the operant-chamber prototypes.

We will not try to determine which shift—that in the runway-to-box line or

that in the runway-to-wheel line—had causal priority in Skinner’s development. The most conservative interpretation is that Skinner’s different apparatuses were a source of mutual influence in his search for quantitative behavioral order.

TEMPORAL BEHAVIORAL MEASURES

In describing all three concurrent lines of investigation and their possible interdependence, we have gotten as much as two years ahead of our chronology. Therefore, let us return to the fall of 1929 in the runway-to-Skinner-box line of development, about which Skinner has written (Skinner, 1956a). Around that time, Skinner devised yet another apparatus in the runway-to-Skinner-box development, a double runway that permitted the rat to go from starting point to goal box and then to return down a back alley to the starting point, from which it could repeat the cycle.⁴ The records are dated in the original hand to November, 1929 (Folder 6). Skinner’s procedure was to start a clock at the beginning of a session and to note manually the time of each return to the starting point as the clock continued to run. He also transcribed a stream of naturalistic observations, for example, that the rat “stopped at other end before coming down [the back alley],” showed a “sudden start” or a “very smooth run,” exhibited “gnawing in the food box,” or was observed to “run hesitant [sic].” And, of course, he caused various noises, including conversation, to occur during the rat’s running, thus continuing his earlier interest in noise-elicited disturbance of locomotion. When Skinner plotted the run time against the ordinal number of the run, he found a wavelike function, as the facsimile in Figure 3 shows, with his free-hand smooth curve passing through most of the data points (Folder 6).

Skinner would have been aware that

⁴ It is possible that “the running wheel became a double runway,” since the double runway permitted continual locomotion as in a wheel, but we will not attempt to decide which apparatus had causal priority.

Figure 3. Time to complete one run in the double runway as a function of repeated runs (trials). Skinner drew the free-hand complex curve through the data points. Redrawn from similar figure without axis labels; brackets indicate that axis labels were added.

periodicity was a subject of interest to physiologists, and that very convincing instances of it had already been demonstrated, since he was acquainted with literature that reported such findings (e.g., Richter, 1927, pp. 320–328; cf. the much-cited data on activity and oestrus cycle in the rat, reported in Wang, 1923). Alerted perhaps by such reading, Skinner measured the time between the successive peaks of this wavelike function and plotted the times against the ordinal number of the peak (Folder 6). Folder 6 contains one plot, showing a linear increase; other data (unplotted in Folder 6) show a roughly flat function and a negatively accelerated increase, with marked individual differences in the means (figures not shown). These findings would have precluded regarding the inter-peak score as significant. After making separate plots of daily sequences of run times, however, he did find that the center of the daily distribution decreased over the five days of the experiment (Folder 6; cf.

Figure 4. Redrawn section of a representative kymographic decreasing cumulative record in tilt-box apparatus, from rat #19 on February 25, 1930. Each downward deflection represents completion of one run, and a horizontal section indicates the passage of time.

Skinner, 1956a, p. 224), which meant that, despite the trial-to-trial fluctuations that are apparent in Figure 3, the speed of his rats increased during training. Moreover—and this is the point we would like to emphasize—the results showed that his explorations had reached beyond reflex-type, individual-trial, behavioral measures to encompass behavioral processes that are distributed through time—processes which he called, at that stage, “secondary” changes (Skinner, 1931, pp. 451–454) and, still later, “dynamic” changes (Skinner, 1938, pp. 14–15). This shift to “dynamic” measures was clearly exemplified by his next research project.

In the winter of 1930 and immediately upon the heels of the double-runway experiment, Skinner developed a mechanized version of the double runway that he called a “tilt box.” A transverse, centrally located rod turned the double runway into a device that resembled a playground see-saw. The tilting of the runway, which resulted from the rat’s locomotion into successive sections of the runway, caused a spindle-mounted slotted disk to rotate and thereby to dispense a food pellet automatically into the food cup, and also to change the position of a writing lever so that a decreasing cumulative record resulted (Skinner, 1956a, p. 225). A representative example of such a record is the redrawn record in Figure 4 (from Folder 4). With this apparatus development, Skinner had at least replaced hand-recording of times with a mechanical device. More important for the theme of the present paper, it is apparent that he

Figure 5. Summary curve plotting the duration of a 50-run tilt-box session (in open circles) as a function of days, and amount of food given on the preceding day (in filled circles). Redrawn from similar figure in Folder 4.

had abandoned *trial-to-trial* times, because the cumulative records have no signal-marker to register time. Though he could later have calculated the trial-to-trial times from the known speed of the rotating drum, he had no simple, immediate, and effortless way of retrieving trial-to-trial times from the records. Someone whose scientific conduct was under the control of contingencies involving expenditure of effort (Skinner, 1956a, p. 224) would *surely* have reduced effort by employing a time signal-marker in the kymographic record, if he were interested in those values. After all, he had used such a marker in the records from the Parthenon and suspended-runway research. Instead, he drew straight lines to approximately straight sections of the cumulative record, as Figure 4 shows—an “eyeball” technique that he and Barnes had employed in their ant study (Barnes & Skinner, 1930). Skinner’s use of this technique further supports the suggestion that he had become concerned with “dynamic” changes in performance that do not require the calculation of instance-to-instance scores.

Furthermore, his summary curves from the tilt-box experiment plotted the length of time needed to complete 50 runs, a summarizing statistic rather than a trial-to-trial measure, as Figure 5 shows. Fig-

ure 5 is a redrawn version of one of the figures that have survived from his tilt-box research (in Folder 4). The figure plots the duration of daily 50-run sessions against days (in open circles), and also the amount of food consumed on the preceding day (in filled circles). Clearly, he was looking for a correlation between a summarizing performance measure and motivational level (hunger), but the relationship that he discovered was not very consistent. (Despite marked variability, a decrease in session length as a function of days *is* discernible in the figure.)

BREAKTHROUGH

The irregular data in Figure 5 are not likely to suggest that in early March, 1930 Skinner was about to experience a major breakthrough in his research that resulted in the kind of conditioning preparation on which he then concentrated with only minor apparatus modifications for several years. On the other hand, if his strategy were to abandon projects or devices that did not yield quantitative regularity—as has been intimated thus far, and as assumed in making sense of the abandonment of the Parthenon—then at this point in time he would *at least* have been ready for another apparatus change. The

Figure 6. Copy of cumulative records of panel pressing. Cumulative number of presses on ordinate, time on abscissa. Data from rat #45, March 25, 26, and 27, 1930.

following steps appear decisive in his subsequent development.

The essential first step for Skinner was to recognize that his behavioral preparations had all suffered from excessive variability and that he needed to control a variety of disturbances. He already had plenty of experience with this kind of problem, having read Pavlov (e.g., Pavlov, 1927/1960, pp. 44–46) and having himself investigated the interfering effect of noise upon locomotion in several different apparatuses. The next step was to construct soundproofed boxes in which his behavioral apparatus would be placed. Since he had already devised free-responding apparatus (as opposed to his earlier trial-by-trial equipment), he had at least developed *the kind of apparatus that could be run* without the supervising and potentially interfering presence of the experimenter. Finally, Skinner shortened the length of the repetitive behavioral sequence ending in food ingestion, by requiring no locomotion at all. He chose to study hunger motivation, an important physiological and psychological topic of the period. Hunger was a subject in which he had done some reading (e.g., Cannon, 1915; Richter, 1927)—interestingly, he purchased Cannon's book in November of 1929, according to the fly-leaf date in Prof. Skinner's personal copy—and, of course, he had just finished investigating the influence of hunger in his immediately preceding research project.

Skinner constructed a small chamber that required the 24-hour food-deprived rat to press against a small panel to expose a tray of food behind the panel, from which it could reach in and withdraw a single pellet of food, and then eat it. This sequence could be repeated over and over, with kymographic equipment recording each panel-press. The chamber was placed inside the soundproofed box, thus shielding it from outside disturbances, including the sounds made by the recording equipment.

According to his later account (Skinner, 1956a), he made a rotating spindle lift a writing lever (instead of lowering the lever, as he did for Fig. 4, above), and he produced cumulative records (cf. figures 10 and 11 in Skinner, 1956a), which he called "ingestion curves" at that point, because the curves displayed the time-course of ingestive behavior. Figure 6 shows several "ingestion curves," which plot against time (t) the cumulative number (N) of pieces of food taken in a session that typically lasted one to two hours (Folder 5). His earliest "ingestion curves" were based on rats #45 and #50, which animals he also used in his tilt-box research. The earliest records of food ingestion were obtained in early March, 1930. Skinner would surely have observed that these records were the simplest that he had thus far obtained, and that the general shape of the function, a concave-down function, was approximately the same from rat to rat and from day to day.

Figure 7. "Square plot" of cumulative responses (N) in panel-press apparatus, for rat #45 on March 25 and 26, 1930. Figure plotted from Skinner's tables of numerical values obtained from kymographic records.

The only question would be: What is the function that describes these data?

QUANTIFICATION

Skinner's quantitative manipulation of his cumulative records was primarily intended to linearize them. Some curves (e.g., power, log, and exponential functions, hyperbolas, and others) *can* be linearized, while others cannot (Daniel & Wood, 1971, pp. 19–24). Equations for linearizable curves are obtainable through simple algebraic techniques, and function-fitting can be done with least-squares procedures. Since Skinner had no idea whether his cumulative records were linearizable, he tried out several transformations. His efforts show a pattern of exploration, some of it unorthodox, like that which we have seen in his exploration of alternative apparatus. In the records dated to the middle of March, 1930, he was trying out a y -squared transformation by plotting against time (t) the square of the number (N) of pellets ingested by each rat (see Skinner, 1979, p. 59), perhaps because the records in Figure 6 look as though they might be square-root functions. The archival records (Folder 5) contain a number of these "square plots" both of observed performance and also of the idealized perfor-

mance that would have resulted if the obtained cumulative record had not shown irregularities (Skinner, 1933b, pp. 311–318; cf. Skinner, 1979, pp. 58–60).

Figure 7 contains "square plots" of Skinner's tabled values of observed performance for one of his rats on two successive days (filled circles and triangles in the figure), and of idealized performance (in open triangles). Assuming he compared his square plots,⁵ he would have found that there was considerable variation in a given rat's square plot from day to day (as a comparison of the functions with filled data points in Figure 7 shows) and, moreover, that the obtained data departed from a straight-line idealization (as a comparison of open and filled triangles in the figure also shows). He would thereby have discovered that the square transformation did not smooth out day-to-day fluctuations in a given rat's performance. Since his observed-data square plots did not conform to a straight line, he would legitimately have concluded that his observed cumulative data departed from a form resembling the positive limb of the function $N = k\sqrt{t}$.

By the end of March, 1930, he began to use common logarithms, and his tables of data (Folder 5) extracted from cumulative records included both the square of the number of pieces eaten (i.e., N^2 in Fig. 7) and the logs of N and time (t). In plotting the data of rats #45 and #50 for several days in log-log coordinates, the approximately straight lines showed that his cumulative records conformed to a power function of time, $N = kt^n$, with the slope (the exponent, n , of the power function) of the lines approximately the same ($n = .68$) from day to day and from one rat to another, as is apparent in Figure 8. Figure 8 reproduces, with permission of Prof. Skinner, the second figure from the

⁵ Each square plot in Skinner's records in Folder 5 is for a given rat on a given day. None of the records plots data for several rats on the same day, or for a given rat on successive days, though Skinner could easily have superimposed these graph-paper figures and held them up to strong light to compare them. Such comparison is provided for in Figure 7, which depicts data that exist only in a tabular format in the archival materials.

Figure 8. Original caption: "Five records for each of the animals in figure 1, plotted on logarithmic coordinates (units arbitrary). The slope of the four limiting straight lines is for $n = 0.68$." Note. Figure reproduced from Skinner, 1930, Figure 2, with permission of Prof. Skinner.

Proceedings of the National Academy of Sciences, in which he published his findings (Skinner, 1930).

It would be quixotic to trace out the sources for such transformations as the logarithmic, as part of the task of making sense of Skinner's quantitative manipulations, since logarithms were widely used in the biological-physiological literature with which he was acquainted, especially the publications of Crozier and his students (e.g., Crozier, 1928). Moreover, the square and logarithmic transformations made up a standard procedure for the analysis of data in biological, physical, and engineering work (Cuthbert Daniel, personal communication, June 28, 1986). Since Skinner was getting help from Daniel in quantitative matters (Skinner, 1979,

pp. 59–60, 67), he was at least following instructions. Other evidence indicates that Skinner did a bit of exploration on his own. For instance, from the same data (rats #45 and #50 on March 25 and 26, 1930) on which he was making logarithmic transformations, he also obtained an unusual ratio. The ratio was obtained by choosing a handful of values (in arbitrary graph-paper units) along the time axis, and drawing tangents to the cumulative record at these time points, as Figure 9 shows. Figure 9 is a redrawn figure from one of the archival records (Folder 5), showing an ingestion curve with tangent lines drawn at numbered time points.

Skinner measured the angles of these tangents, then obtained the ratio of each angle (in degrees) to the height (in arbi-

Figure 9. Redrawn panel-press cumulative record for rat #45 on March 27, 1930; cumulative number (N) of presses as a function of time, with arbitrary values on both axes of graph-paper original figure. Labeled tangents are drawn to the record at $t = 1, 2, 4$, and 8 .

trary graph-paper units) of the cumulative record at the point of tangency. This ratio is a rather unorthodox instrument for analyzing a function. Moreover, it misses the point with regard to changes in slope *as a function of time*, because the numerator and denominator both reflect values of N , the cumulative number of pieces of food eaten. Perhaps Skinner wished to determine how the momentary rate of eating (angle of tangent) depended on how much food had already been consumed (height of curve). An appropriate research history existed for such a question, since the relationship of performance to amount of consumed food was the subject of interest in part of his tilt-box research, as Figure 5 showed. If that were his objective, he would have done better to plot the angle or its tangent against the height,⁶ instead of forming a

⁶ Had he calculated and then plotted the tangent of the angle as a function either of time or of the height of the curve at the point of tangency, he would have found nonlinear curves with large between-subject and between-day variation (figure not shown), which would have been grounds for abandonment of the technique, according to our interpretation.

Figure 10. Data for rat #45 on March 26 and 27, and for #50 on March 27, 1930. Ratio of angle of tangent to height of the point of tangency on the respective cumulative records is plotted against time. See text and Figure 9 for method of calculating.

ratio of variables (angle and time) that change in opposite directions. Though the ratio that he calculated appears to be without an easily recovered rationale, elaborate second-guessing may be pointless, because surviving records (Folder 5) contain no plots of this ratio, or of the logarithm of the ratio, which Skinner also calculated in his tables of data. If he had plotted his tabled data, he would have found that he had not linearized his cumulative records and that both the ratio and its logarithm showed between-subject variability in form and slope, as Figures 10 and 11 show. He would have been forced to the conclusion that the angular ratio was no improvement over the square and log functions that he was trying out about the same time.

If Skinner's conduct as a scientist involved widely ranging exploration with subsequent rejection of "unprofitable line[s] of attack" (Skinner, 1956a, p. 221) and sustained involvement in the profitable lines, then he would have tried a variety of quantitative manipulations and abandoned all but the successful ones. Indeed, the archival records show that during March, 1930, he tried all three

Figure 11. Plot of the logarithmic transformations of the ratio values in Figure 10, against time.

quantitative transformations (square, log-log, and angular ratio) on the same data (Folder 5), but published only the logarithmic data (Skinner, 1930).

CONCLUSION

When Skinner discovered that the power function he had obtained from his panel-pressing rats showed constancy of the exponent ($n = .68$) across rats and days—as Figure 8 showed—and when he subsequently found that power functions with the *same* exponent could also be obtained from lever-pressing rats (reported in Skinner, 1932b), then he had indeed obtained the generalizable quantitative orderliness that he had been seeking.⁷ A period of frequent apparatus change during the fall and winter of 1929–1930 came to an end. Though he sub-

sequently made technical improvements, he made no further substantive alterations of his lever-press apparatus for many years, and the device became the standard preparation for studying operant behavior.

Elsewhere we have pointed out various metatheoretical consequences of his discoveries of quantitative regularity, such as a neutralization of the problem of variability, the vindication of generic (molar) concepts, and a shift from a Nominalistic to a Realistic ontology (Coleman, 1984). For our presently more limited concerns with apparatus and projects, it is reasonable to conclude that Skinner's early research program exemplified a unifying strategy of seeking quantitative behavioral orderliness, and that his success and failure in this endeavor was an important factor in the development of his behavioral apparatus and research projects.

SKINNER'S LATER QUANTITATIVE WORK

As is well known, Skinner's quantitative emphasis has never been thoroughgoing. In his 1930 paper, though he calculated constants for his power function (and its derivatives), the particular numerical values were not utilized in further quantitative elaborations (Skinner, 1930). This clearly set his research program apart from that of Crozier. It is a likely inference that it was not the specific quantitative values (e.g., an exponent of .68) that were important to Skinner, but rather *the fact that quantitative regularity was actually exhibited*. This interpretation supports our proposal (Coleman, 1984) that Skinner's early research program had strongly philosophical agenda, which included the defense of determinism (or lawfulness: cf. Scharff, 1982) and the operational clarification of psychological concepts, in this case, "hunger drive" (cf. remarks on the status of the concept of hunger in Skinner, 1930, pp. 433–434; 1931, p. 454; 1932a, pp. 33–34). Though the same exponent of his power function ($n = .68$) turned up in subsequent lever-press research (Skinner, 1932b), again he did nothing with the particular value of the exponent ex-

⁷ The fact that Skinner eventually became disenchanted with the possibility of a general function for satiation of hunger-motivated behavior is irrelevant (Skinner, 1938, pp. 350–351). Well before 1938, his research program in lever-press behavior was solidly established and independent of his earlier research topics of tropisms, locomotion, and hunger. The excessive significance he initially attached to his power function (Skinner, 1930, p. 438; 1932b, pp. 47–48) had led to favorable outcomes that he did not originally anticipate, a feature that is no doubt a variant of serendipity (Skinner, 1956a, p. 227).

cept to note the regularity and to draw important but nonquantitative conclusions (Skinner, 1932b, pp. 47–48).

For a number of years, Skinner periodically tried quantitative attacks on researchable questions. His empirical work in the psychology of literature (e.g., Skinner, 1939, 1941) involved the calculation of probabilities. In the study of extinction, he employed curve-fitting again and proposed that his extinction curves were logarithmic, connecting this possibility with a fluid-reservoir model of extinction processes (Skinner, 1938, pp. 26–28, 83–96, et passim). But his model-building succumbed to quantitative criticism (e.g., Ellson, 1939) and empirical difficulties (e.g., Skinner, 1938, p. 416; 1940). Though it seems safe to hazard the guess that Skinner had neither the highly developed quantitative skill nor the required convictions⁸ to go far down the quantitative road, that is primarily a conjecture about events that are outside the chronological frame of this article and better left to future investigations.

RECONSIDERATION: VARIETIES OF PROGRESS

Our account and our conclusion certainly suggest something like progress in Skinner's early research program, not in a straight-line succession of improvements but at least in an inefficient, evolutionary fashion. Skinner's "Case History" adduces factors, such as luck and accident, that were responsible for progress despite their apparent haphazardness. The present account places more emphasis on a unifying mechanism: He abandoned projects and apparatus that did not lead to quantitative orderliness; he pursued projects that yielded such regularity; and he made progress in the sense that his search resulted in the successful

determination of the power law of satiation. But such a balance sheet of accomplishments presents an abstractive and unidimensional picture of Skinner's progress. We will briefly suggest two additional aspects of his progress or success, and will leave the door open for more.

First of all, Skinner's successful choices of apparatus and project did not always involve shifting to a line of investigation that brought him closer to a specific, previously established research goal. For instance, in setting aside his suspended runways, Skinner moved away from what looked to be a successful line of runway research that was testing the validity of the reflex in its application to the freely moving organism. Instead, he switched to a project, one involving ingestion curves obtained in the panel-press apparatus, that *immediately gave rise to further related activities*. His ingestion curves prompted efforts at linearization, which took him away from the runways (see Skinner's remarks above, "The Diversity of Skinner's Research"). The panel-press apparatus very soon required some technical improvements (e.g., a device to prevent contact chatter), which involved him in shop work, an activity that already was very appealing to him. The "successful" project, that is, created further projects that engaged Skinner's concern, time, and effort.

As a final example: Beginning in November, 1931, Skinner kept notebooks called "Protocols" (presently located in Folder 7) which listed his rats by number and their experimental treatments, and included notations to accompany the cumulative records generated by each rat.⁹ The fact that the Protocols also included

⁸ In response to a query about his commitment to curve fitting and other quantitative techniques, Prof. Skinner described hearing Cuthbert Daniel say in conversation many years ago that "with three constants in an equation you can draw an elephant, and with four you can make him lift his trunk" (B. F. Skinner, personal communication, July 9, 1985; cf. Skinner, 1944, pp. 280–281; 1956c).

⁹ The records, smoked paper sheets mounted on a slowly moving kymograph-drum, could not easily be written upon during a session. On the occasion of an apparatus malfunction or an experimental error (such as a failure of the feeder to operate), it was necessary to make a note in the Protocol to indicate this and the time of its occurrence. Skinner noted in the Protocol the time at which the kymographs were started, so that the malfunction or disturbance could be pinpointed upon the cumulative record, and the effect of the disturbance on responding could be ascertained.

ideas for future experiments with the same apparatus, and the fact that he subsequently carried out many of the planned projects, indicate that his prototype Skinner-box effectively limited his apparatus explorations by monopolizing his time. It seems apparent that the "successful" project or apparatus was typically one that kept the researcher involved so that various other, quite different and unrelated projects could not be started.

A second aspect of progress is less obvious and may owe more to interpretation. The basic idea is that Skinner's development showed an abstract-to-concrete direction, and that his progress or success consisted in choosing alternatives that brought him into more immediate contact with more concretely compelling findings (cf. Skinner, 1986). In terms of this consideration, Skinner's earliest research projects were quite defective. The collaborative project with T. C. Barnes was not entirely his own, and the squirrel project with Dwight Chapman involved ideas that Skinner found repugnant (cf. the skit described in Skinner, 1979, p. 31), reducing the likelihood of deeply personal involvement and findings. It is true that the Parthenon engaged his established commitment to reflexological ideas, as did the "posture in baby rats" project, but his findings were not concretely pertinent to specific research objectives, because neither seems to have yielded data that showed enough regularity (see above). As a result, Skinner's theorizing about rat behavior in the Parthenon was thin and abstract, merely speculative and derived mostly from books and articles (Skinner, 1979, pp. 33-34). His suspended runways yielded a variety of dependent variables (latency, duration, amplitude, and R/S ratio) that could have been significant in relation to his reflexological ideas; but his double runway was a better apparatus, because it gave him data that showed a dynamic change in running speed *which he could see* in the succession of daily distributions of running times. The fortunate accident that he left the spindle on a wooden pellet-delivery disk (Skinner, 1956a, p. 225) yielded him what was at that time

his most effectively concretized representation of an orderly behavioral change (see Fig. 4). In temporal charts like Figure 4, Skinner made a dynamic process concrete, and thus the results of his research were immediately and almost tangibly inspectable.

The subsequent apparatus developments in the runway-to-box line were admittedly "steps closer toward behavioral laws," but, in comparison to the development of the cumulative curve (Fig. 4), and from the standpoint of this second aspect of progress, they look almost anticlimactic. As Skinner was soon to claim, the virtue of the cumulative record was that it afforded something that "is in effect . . . the description of a process [of satiation of "the facilitating condition" of hunger]" (Skinner, 1930, p. 437). To someone seeking behavioral orderliness, an inspectable display of it must have been a powerful payoff. Years later he remarked: "In choosing rate of responding as a basic datum and in recording this conveniently in a cumulative curve, we make important temporal aspects of behavior visible" (Skinner, 1956a, p. 229, emphasis original). The development in which Skinner's activities were influenced less by abstract concerns and more by concrete findings was, for Skinner, a change that probably ought to be called progressive. Other facets of "progress," either idiosyncratic to Skinner or generalizable to the careers of other scientists, remain to be delineated.

RECONSIDERATION: HAPPY ENDINGS

Even if we allow that the idea of progress in a research program may admit of distinguishable facets, we would still want to return to a balance sheet of accomplishments and to say that Skinner made progress in the sense that his search for quantitative regularity ended in the discovery of a power law for satiation of hunger in panel-pressing rats (Skinner, 1930) and later in lever-pressing rats (Skinner, 1932b). If only Skinner had confined his subsequent labors to the lever-press apparatus, we might regard the

discovery of generalizable regularity in the lever-box in 1931 as a happy ending for our story, since the lever box could be seen as terminating the early research program and initiating an operant-conditioning research program.

Skinner, however, continued to pursue locomotion and reflex-type research in a variety of ways (as we already remarked in "The Diversity of Skinner's Research"), which shows that other lines continued to exist alongside the runway-to-box line of apparatus development in his early research program. His abandonment of a reflexological program—a program that included empirical work with runways, reflex-theoretical work with boxes (e.g., Skinner, 1931; 1932a, pp. 31–32; 1932b, pp. 38–39), historical research on the reflex (Skinner, 1931; Skinner & Crozier, 1931), and even a translation project on Magnus (1924) in the spring of 1930—was a protracted affair, by no means complete by the time his development of the lever box had terminated his explorations in *one particular line* of apparatus development in his three-part enterprise. Moreover, he was involved in a spinal-reflex project at the Harvard Medical School during 1932–1933, and he did not publish the distinction between lever-press behavior (operant) and true reflexes (respondents) until 1935. Finally, the very concept of operant behavior was developed in a context of theoretical considerations that simply were not operative in 1931, as we have previously shown (Coleman, 1981). To think his early research program had the happy and singular consequence of opening up an "operant conditioning" line of investigation in 1931 would clearly be a case of reading a later development back into the earlier stages, as though a program of operant conditioning research were an objective of his early research program of 1928–1931. Such thinking is the result of standing upon the vantage point of his later operant-conditioning research and regarding his early program as unified by virtue of serving to bring about the later program. The importance of accident and luck in

Skinner's development (Skinner, 1956a) shows that such a vision of *at least his* scientific development is an idealization. Our emphasis on the diversity of Skinner's early research program shows the same.

Two suggestions seem appropriate to end this piece. First of all, the immediate and strong appeal of happy endings obscures the fact that such endings are *inventions that increase the apparent coherence of the onlooker's story*, rather than simply neutral-descriptive conclusions regarding the subject under study. Even for all of its emphasis on centrifugal factors, Skinner's (1956a) "Case History" is a success story in which the lever box was the primary achievement. Moreover, Skinner (1956a) reduced the apparent diversity of his early research program by focusing upon the runway-to-box development, by omitting discussion of the running wheel research, and by treating the runways as only a phase in the development of the lever box. Though the phrase sounds hackneyed when it occurs in a conclusion, our account suggests that Skinner's early research program was actually *a bit more complicated* than his portrayals indicate (Skinner, 1956a, 1967, 1979), and such complications make happy endings look artificial.

Secondly, the satisfaction afforded by a tidy and complete story of a personal development can serve as a powerful trap that prematurely brings the biographical exploration to a halt (cf. the remarks on mentalistic explanation, in Skinner, 1956b, pp. 81–84). Having made sense of the development, we are tempted to look no further. This danger may be an intrinsic feature of historical-biographical interpretation, since it leans toward circumstantial coherence as a touchstone of interpretative adequacy, but an evaluation of that possibility is not required here. Moreover, by closing the present article with mildly skeptical reflections and with suggestive possibilities, we clearly leave open the question whether different considerations will make even better sense of Skinner's early research program.

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