

## From Critic to Theorist: Themes in Skinner's Development from 1928 to 1938

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Nine themes help in understanding B.F. Skinner's development from graduate student in 1928 to the publication of his *Behavior of Organisms* in 1938. It is claimed (1) that Skinner's primary personal development was from the role of precocious critic to mature theorist; (2) that Skinner's discoveries of behavioral lawfulness enabled him to shed major portions of his earlier reflexological commitment; (3) that his postulation of operants served several nonempirical functions; and (4) that the postulation required that he depart from the restrictive philosophical framework in which he had been working.

The purpose of this paper is to provide an interpretation of B.F. Skinner's development as a researcher and theorist in the decade that ended with the publication of *The Behavior of Organisms* (Skinner, 1938). Although information concerning the 1928-1938 period is available, few studies have made a detailed and interpretive use of such materials for a thematic understanding of Skinner's development in that decade. Skinner himself self-consciously tried to minimize interpretive practices in his autobiographical writings (Skinner, 1979a). His "Case History" (Skinner, 1956) of the evolution of his 1928-1931 research practices emphasized the role of unexpected discoveries and de-emphasized the importance of guidance by theory, but he never attempted a systematic characterization of his development as a researcher in the next stage (between 1931 and 1938); his *Shaping of a Behaviorist* is,

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instead, a thickly detailed chronology that makes integrative themes less than obvious.

Others have simply chosen to take *The Behavior of Organisms* as the starting point for an exposition of research and theory in radical behaviorism and have not worried about its historical context (e.g., Modgil and Modgil, 1987). Given its landmark status in behavior analysis, there has been little pressure, apart from historical curiosity, to scrutinize the papers preceding that book. Nonetheless, the major theoretical tools and conceptual distinctions (e.g., reflex reserve, extinction ratio, operant vs. respondent, drive and emotion as "intervening variables") that were presented in 1938 rest on previous findings and on developments from prior positions.

To provide a narrative understanding of the 1931–1938 period, we will present nine themes, describing each separately as well as their relative importance and mutual fit. In composing the themes, we will make primary use of published materials and, to a smaller degree, Skinner's archival records. We will concentrate on three elements: Skinner's personal predilections, the Harvard context, and the resultant nine-stranded development as theorist and researcher. We will treat the first two items as background for our interpretive framework of nine developmental themes.

#### *Pre-Harvard Personal Disposition to Criticize*

Efforts to understand Skinner's work entirely from within a context of behavior theory and research (i.e., internalistic historiography) are unlikely to start from the possibility that the larger culture of ideas had much to do with Skinner's research and theoretical development; and yet, Skinner's first autobiographical volume (Skinner, 1976) is dense with cultural references. Among them are numerous mentions of unconventional ideas (e.g., Shakespeare's identity: pp. 128–129), unconventional people (e.g., pp. 149–155), unpopular claims (e.g., those of Tom Paine: p. 9), and cultural criticism expressed in life style (e.g., pp. 302–307) and in written form (e.g., Pound, Russell, Wells) in the 1920s.

At college Skinner found himself embarrassed about his small-town origins and protectively cultivated a cosmopolitan and disdainful air. Lamponery, a safe way to express aggression and to assert personal superiority, was a stock-in-trade of his repertoire as a writer for college publications and in the period after graduation. As a struggling writer in his "Dark Year" spent in Scranton after graduation, he read free-thinking and socialist literature, his father serving as a target for the young Skinner's observations regarding the bankruptcy of national and local bourgeois values. He enthusiastically participated in the cultural criticism that typified American and British writings of the postwar period: Mencken, Pound, Russell, Wells, Dreiser, and Lewis are

only a few of the writers whom he read. He even tried out a bohemian sort of lifestyle in New York City in late 1927 and early 1928. He came to Harvard in 1928 already disposed to question conventional notions and values, although he had not yet become a complete behaviorist.

Consequently, at Harvard, through personal contacts and by following up on literary endorsements, it was easy for Skinner to find his way initially into a framework that could be called positivistic, because of its critical impatience with unrigorous thinking and with lax standards for evaluating assertions; the framework could also be called mechanistic, because of linkage to writers in the mechanistic tradition (e.g., Jacques Loeb). This positivistic/mechanistic framework—a cognitive framework of claims made by respected authors in authoritative books, and a social framework of individuals committed to those claims—supported the hard-headedness of Skinner's early psychology.

As an intellectual structure, this positivistic/mechanistic framework included books by Loeb, Sherrington, Magnus, and Pavlov in psychology–physiology; and Mach, Meyerson, and Poincaré in philosophy of science. There was also an infrastructure of reference lists, dedications, and recommendations. For example, Loeb's (1900) *Comparative Physiology of the Brain and Comparative Psychology* was dedicated to Mach. E.B. Holt, who had been at Harvard from 1901 to 1918 and developed a philosophical form of behaviorism, appealed to Meyerson in philosophy and to the findings of Pavlov, Sherrington, and Magnus as a scientific foundation for his bio-philosophical behaviorism (Holt, 1931, p. 253, note 7; see also Kuklick, 1977, pp. 417–434).

As we noted, the framework was also social, and it included Skinner's teachers and acquaintances. Skinner had substantial personal contact with W.J. Crozier, who embodied positivistic attitudes as part of a very personal style (Pauly, 1987, pp. 183–192; 197–198). Endorsement of Mach by Lawrence Henderson and by Crozier<sup>1</sup> were probably important, inasmuch as Skinner seems, in the absence of extensive reading of the psychology literature, to rely on local testimonials regarding important sources of ideas. The enduring influence of Mach (esp. his 1893) on Skinner's theoretical style has been described in detail by Smith (1986). (See also Day, 1980.)

The framework was also genealogical. Crozier was a student of G.H. Parker in zoology, and Parker had taken Loeb's side in the Loeb–Jennings debate; Skinner carried out a piece of research on spinal reflexes in the laboratory of Alexander Forbes, who was a student of Parker; and Crozier was an ardent proponent of Loeb's research program. Pauly (1987) has provided a well documented picture of the Crozier circle at Harvard and its intellectual commit-

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<sup>1</sup> W.J. Crozier to B.F. Skinner, 27 July, 1931; in Box 4, Accession 9710, Harvard University Archives.

ment to the ideas of Loeb and Mach. This positivistic/mechanistic framework stretched beyond the merely scientific: for example, Skinner had read Sinclair Lewis's *Arrowsmith* during his Dark Year and had been inspired by the figure of Max Gottlieb, a fictional cover for Jacques Loeb. As the 1928–1938 decade unrolled, Skinner's initially strong and single-minded commitment to this framework softened; indeed, several of our nine developmental themes reflect this softening.

*Psychology at Harvard University: Fall, 1928*

In comparison with other American universities, Harvard was slow to grant institutional autonomy to psychology, which did not become a distinct Department separate from philosophy until 1936. Prior to that division, the Philosophy–Psychology arrangement guaranteed the administrative subordination of psychology. With the exception of Hugo Munsterberg, chairmen of the Department of Philosophy and Psychology had been and continued to be chosen primarily from the Department's philosophers.

Some psychology specialties, particularly comparative psychology and animal behavior, suffered under this outdated arrangement. When McDougall, who regularly taught comparative, left for Duke University in 1927, he was not replaced by an established comparative psychologist or even by a full-time comparative psychologist (in contradistinction to a *visiting* instructor). Moreover, the comparative–psychology course was removed from the core of three exclusively undergraduate courses in the psychology curriculum. Instruction in comparative was not picked up on a regular basis until Morgan Upton, of the Department of Physiology, taught a course on the subject in the 1930–1931 academic year.<sup>2</sup>

Boring probably surveyed comparative–psychology topics from an historical perspective in his systems and theories course (Psy 31) in the late 1920s. His *History of Experimental Psychology* (Boring, 1929) provides hints as to what he may have claimed in class. Predictably, the relevant sections of the book are historical; but, true to Boring's style of doing history, persons and systematic issues (mechanism vs. vitalism, the existence of animal mentality) occupy center stage. Very few data are presented—not altogether surprising in a book on history—but the reader would not even derive the impression that an important objective of animal and comparative psychology is to discover *laws of behavior*. Such an oversight is certainly not present in the section on Fechner, in which the reader encounters a flood of equations in the derivation of psychophysical laws. In Boring's account of comparative psy-

<sup>2</sup> The information on the Harvard curriculum was obtained from these two Harvard University publications: *Harvard University Catalogue* (annual) and *Report of the President and the Treasurer of Harvard College* (annual). Copies were examined at the Harvard University Archives.

chology, however, the primary question concerns the existence and nature of animal mentality. This emphasis is consistent with Boring's acknowledged reliance on the third edition of Margaret Floy Washburn's (1926) *The Animal Mind* (Boring, 1929, pp. 560–561; 566).

According to Boring (1929, pp. 550–552), an exclusively behavioral approach in animal psychology is inadequate because it must “leave unanswered every question as to [its] mind” (p. 550). Could this omission be legitimate? No, replied Boring: “An animal *must* be said to have mind when it exhibits the kind of behavior that is characteristically conscious in human beings” (p. 552, emphasis added), obviously a variation on the argument from analogy. Boring described and defended that analogy as the basis for a kind of “animal introspection”—as did Washburn (1926) in her entire introductory chapter—using it to account for the inference of both the animal mind and the minds of other human beings in a “community of meaning” (Boring, 1929, p. 551).

Whether Skinner's challenge to these ideas in his earliest papers was a direct answer to Boring is probably undecidable. Nonetheless, Skinner rejected the argument from analogy and the inference of animal mentality. For example, in defending his nonmentalistic definition of hunger as a construct for summarizing co-variations in the observable strength of “eating reflexes,” Skinner claimed:

We say that an animal is hungry if, when we give it food, it eats. We also say it is hungry if it responds in characteristic ways to an empty food dish, to the approach of the experimenter, or, in short, to any stimulus that has been conditioned to behavior in some way concerned with eating . . . In our everyday use of the word only the first of these conditions is important, for we ordinarily attribute hunger to an animal *only* because it eats or because it exhibits behavior that we have frequently observed to be followed by eating. In the latter case the use of the word rests upon the *conditioning* of the experimenter. (1932a, p. 32)

This behavioral analysis was a far cry from Boring's argument from analogy. Skinner went on to pursue questions about the origin of mentalistic terms and the establishment of a community of meaning for a goodly part of his professional career, and he never relied on the argument from analogy (e.g., Skinner, 1938, pp. 41–43; 1945, 1963).

What of the mechanistic approach, which avoids appealing to mind? Boring praised workers in that tradition but designated it a part of biology, not of psychology. The present-day (i.e., 1929) embodiment of the mechanistic approach was said to be in “the subject matter that is called the physiology of conduct” (Boring, 1929, p. 553), in apparent reference to W.J. Crozier's course in the Department of Biology titled *Physiology 3: Analysis of Conduct*. Not only did the mechanistic school lie outside of psychology, according to Boring, but that approach applied principally and most successfully to so-called lower organisms, not to higher ones, because: “The reactions of these

[i.e., higher] animals are too variable, too readily modified. Now it is just this modifiability that creates the suspicion of mind" (p. 554). As we shall see below, Skinner's earliest systematic concern was to reject the inference that leads from variability to indeterminism and thence to mentality (e.g., Skinner, 1930).

Given the sparse offerings in behavioral psychology in the Department of Philosophy and Psychology, given the unsympathetic and off-target (at least by Skinner's standards) depiction of behaviorism by the senior psychologist of the Department, it is understandable that Skinner spent so much time in Crozier's department. The presence of two psychology Ph.D.s (Hudson Hoagland and Morgan Upton) in Crozier's department supported a recognized option of taking behavior courses outside Philosophy and Psychology, so Skinner's choice coincided with established practice. Consequently, his behavior theory and research developed in a setting that was distinct from psychology, with the consequence that his contact with relevant psychological literature was partial and selective.

### Skinner's Development From 1928 to 1938

Figure 1 provides a visual realization of proposed relationships among our nine developmental themes. The top and bottom halves represent earlier and later portions of the 1928–1938 decade: each of the nine boxes in the top half contains the first stage of a developmental theme, and the boxes vertically below contain the terminal positions. Boxes representing early and late positions in three major themes are connected by descending arrows, with the thematic importance represented by increasing thickness of the arrows. In six of the nine themes, arrows are omitted, as a pictorial convention to reflect their secondary importance and to minimize clutter in the figure. The most derivative themes are located on the right side of the figure; the central themes, containing thick descending arrows, are obviously near the center of Figure 1. Horizontal and branching lines with arrows represent historical *influence*; those ending with a bracket indicate only an historical or other relationship. The two wavy lines represent "affordance," a notion we will use in discussing historical functions of the concept of operant under the eighth theme.

It is apparent from Figure 1 that the themes are of unequal importance. The principal arguments of this paper—those concerning the reflex-to-operant and the Nominalism-to-Realism<sub>1</sub> themes, numbered as the seventh and eighth themes, respectively—occur near the end of the paper, preceded by a description of all but one of the other themes. Although this arrangement threatens to divert attention from those central themes, prior exposition of the other themes is necessary to develop the argument.

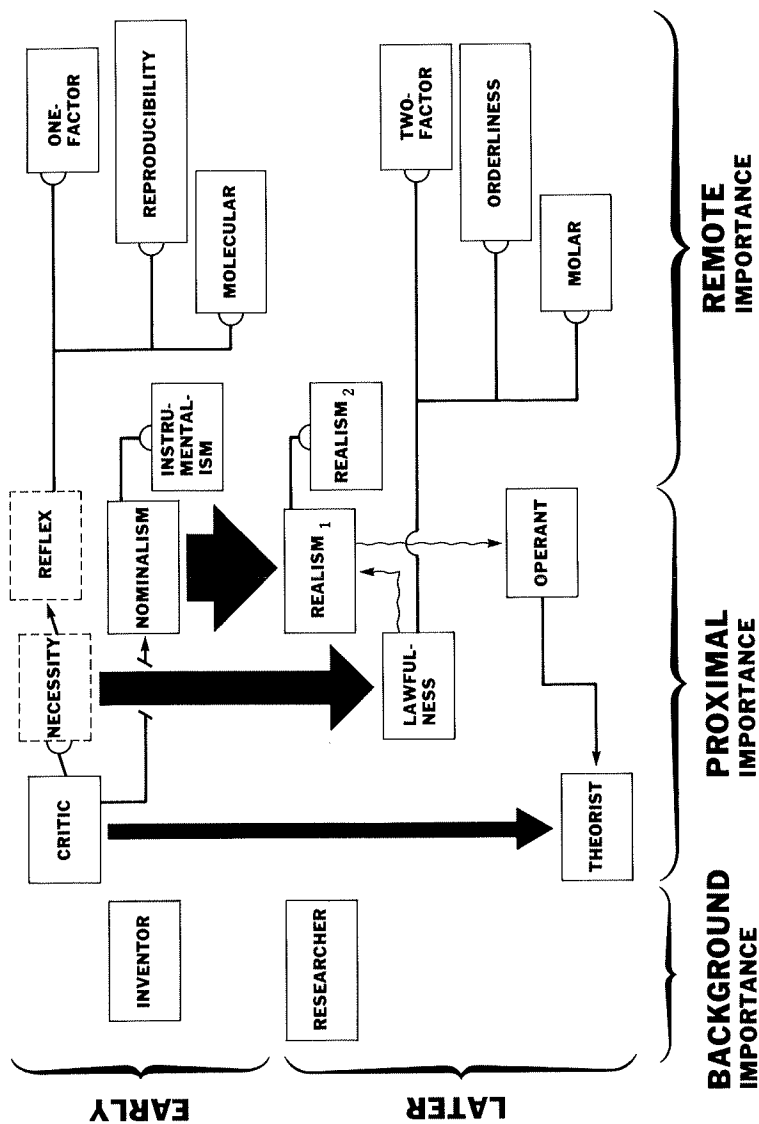


Figure 1. Schematic relationships of the nine bipolar themes. Each theme is represented as two boxes, lined up vertically and with the upper box representing Skinner's "early" position, the lower box representing the ("later") position he adopted as a result of that thematic development. The most important themes are centrally located in the figure and are connected by arrows. (See text for further description.)

One more aspect of this figure merits comment before we turn to the nine themes. The dotted-line border of two boxes in the upper portion of the figure indicate historical accident. Skinner had tried to become a writer, and he presumably could have trained as an attorney (after all, his father was an attorney), or as a physician (he had interests in biology), or in some other profession; in whatever field he happened to work, his personal inclinations would have found expression through the concepts, assumptions, and tools of that particular profession. That Skinner chose psychology rather than any of several other fields was mostly accidental (Coleman, 1985a), a feature suggesting that his adoption of determinism was largely contingent, although the adoption may also have reflected his critical learnings. His use of the concept of reflex was more obviously an accident of his arriving at Harvard with a predilection to behaviorism that soon brought him into the Department of Physiology.

(1) *From Inventor to Researcher*

Skinner arrived on the Harvard scene with well developed tool-and-carpentry skills, which he had acquired during his youth in Susquehanna, a railroad town in northeastern Pennsylvania. As a boy he had designed and built a number of toys and other devices (Skinner, 1976). At Harvard, this repertoire found a useful outlet in making and altering several pieces of research apparatus during his graduate-school years (Skinner, 1979b). Indeed, in making his decision to remain in the psychology program rather than to shift over to physiology sometime in 1929, he was swayed by the excellent shop facilities in psychology (Skinner, 1979b, pp. 31–32). In his three years as a graduate student he constructed eight or nine different apparatuses for the study of rat behaviors, and stopped replacing apparatuses only when he had devised the lever-press box in 1931 (Coleman, 1987). Subsequently he modified the box but retained its essential features while pursuing a program of research that continued beyond *The Behavior of Organisms*.

The commitment of substantial time and effort to making apparatus and the importance he gave to the shop facilitates in deciding on a Departmental affiliation suggest that working on apparatus was satisfying for a while. Moreover, the activity permitted his findings—that is, success and failure in obtaining quantitative orderliness in behavior—to shift his interests and hunches opportunistically from topic to topic, from apparatus to apparatus, from posture and locomotion, through hunger and satiation, and finally to conditioning. By the time he began a postdoctoral appointment in Harvard Physiology in 1931, his early, somewhat *recreational* exploits in the Department shop had been subordinated to more compelling *investigative* interests.



And yet by listening so intently to his own drummer, he found himself out of step with the parade of other animal researchers. He did not cite contemporary work by his peers, and most of his publications appeared in a somewhat peripheral outlet, *The Journal of General Psychology*. Isolation from and ambivalence toward the mainstream were to remain standard features of his endeavor for many years; clearly their roots were present from the very start.

## (2) *From Critic to Theorist*

We treat this theme as the major personal dynamic of Skinner's development. In the narrative above, we described Skinner as already inclined to criticism. The Harvard scientific world acquainted him with models (e.g., Mach, Bridgman, Crozier), institutional support (fellowships, etc.), and role-opportunities for playing out critical predispositions fully and with satisfying results (e.g., Skinner, 1979b, pp. 46; 62-64). One of his objectives, in his "Concept of the Reflex" (Skinner, 1931), was to purify psychological (pp. 434-439) and physiological (pp. 441-443; 454) terms by removal of nonempirical, superfluous, and unjustifiable connotations and by operational redefinition. Skinner applied this strategy to the concepts of reflex, synapse, drive, and emotion.

Although Skinner's 1931 paper might be dismissed retrospectively as merely an operationist exercise, a sensitive reading shows that the paper was no exercise. Hal Davis had let Skinner in on the professional secret that a young scholar's reputation depends on the reception of his first major publication (Skinner, 1979b, pp. 70-71); Skinner's paper was undoubtedly written with that realization in mind. Moreover, the paper differs only slightly from the historical portion of his Ph.D. dissertation that he had resolutely pursued in spite of Boring's firm disapproval (Coleman, 1985b), so the 1931 paper was also a personal testament.

The 1931 paper is brilliant and combative, and it promotes a Great Truth to which Skinner appears to have entrusted his budding reputation, namely that behavioral psychology can stand on its own, because its concepts are distinct from and epistemologically prior to those of physiology. With his Great Truth, Skinner self-consciously countered an "all but universal" (Skinner, 1938, p. 420) conviction that psychology is fundamentally dependent upon nervous-system physiology. Another case of Skinner's running against majority opinion was his operational clarification of the concept of synapse, which ran counter to the established Realist interpretation of the synapse as "a physico-chemical system" (Skinner, 1931, p. 443); his redefinition of the synapse in Machian fashion was a *tour de force* of conceptual criticism.

Later in the decade of 1928-1938, however, Skinner's research program ceased to be as strongly driven by conceptual criticism, and he less frequently

played the role of precocious youngster making outrageous observations. This thematic development from critic to theorist was reflected in many features of Skinner's scientific practice and, therefore, involved other developmental themes that we discuss below. For example, he came to tolerate and use concepts that go beyond the evidence (but cf. Skinner, 1936a): the concepts of operant behavior and reflex reserve and the "envelopes" that he used as presumptive upper limits in plotting his cumulative records (see Verplanck, 1954, pp. 280; 292–293 for discussion) all go beyond merely summarizing data (e.g., Skinner, 1938, pp. 26–28; 228–231; see Coleman, 1984). Although Skinner could recommend conceptual asceticism and could abstain from making ontological commitments while serving as a critic, a theorist's role requires a more relaxed and expansive attitude concerning the ontological status of his constructs, as Mackenzie (1977) has noted in the general case. As we shall see (especially in themes 7 and 8 below), in developing a system of behavior for his 1938 book, Skinner necessarily moved far beyond the conceptual restraint that was so apparent in his positivistic paper of 1931.

If it is right to think of Skinner's development from critic to theorist as a shift from the restrictive, individualistic stance of his graduate-student years to an expansive, socially defined role, then one ought to find evidence of expansion in his personal life. That was surely so, and the interested reader can find enough detail in *Shaping of a Behaviorist* (Skinner, 1979b) to regard this second theme as one of *personal* development.

### (3) *From Necessity (Determinism) to Lawfulness (Order)*

For reasons that are largely accidental, Skinner embraced the concept of reflex. Reflexes had been promoted in literature (Watson, Pavlov) that he had read before coming to Harvard, and his first-year coursework in physiology strengthened whatever inclination he may already have had to adopt the concept. Reflexes became the principal vehicle for his defense of determinism or necessity.

The endorsement and defense of determinism was probably a component in Skinner's original attraction to behaviorism; it would have been, therefore, a rather personal matter. Certainly determinism was a suitable theme for a young critic, because commonsense psychology is mostly indeterministic, and because Skinner was inclined, well before arriving at Harvard, to question culturally established belief. Determinism also was a happy choice in the Harvard context, and especially in the positivistic/mechanistic network in which Skinner worked. Why determinism was *personally* suited to the young B.F. Skinner will be left to character analysts to figure out.

As Skinner observed in his first sole-author research publication, "variability of behavior . . . has led to protestation of the adequacy of the reflex con-

cept" (Skinner, 1930, p. 434; see also Skinner, 1931, pp. 435-436; 437). Because even reflexive behavior varied from one elicitation to another, it was plausible to claim that behavior was not fully determined by the stimuli the experimenter applied. Moreover, the fact that much of the freely moving organism's behavior occurs in the absence of identifiable or experimenter-presented stimuli could suggest that most behavior occurs spontaneously (i.e., is not reflexive). Therefore, one of Skinner's early concerns was to vindicate determinism (necessity) by showing that the evident variability and spontaneity of behavior are not embarrassments to determinism (see Crozier, 1929, p. 87; cf. Coleman, 1984, esp. pp. 472-475).

Not only was Skinner's 1931 paper an exercise in conceptual revision—as we noted above—but it also worked out an historical argument for the applicability of the concept of reflex to the behavior of freely moving animals. On the basis of a summer's reading of historical materials, Skinner proposed that the history of reflex physiology had been "essentially the account of the discovery of [eliciting] stimuli and of the concurrent passage of the corresponding behavior from the field of volition into the field of reflex action" (Skinner, 1931, p. 436).

Skinner's (1931) "Concept of the Reflex" is a blend of contradictions: historical description is combined with philosophical interpretation; caution exists alongside enthusiastic speculation. It is also positivistic: his story of two centuries of progress in reflex research is a *moral* tale in which "right thinking" (i.e., be hard-headed, stick to the facts, and be skeptical of inferential leaps) inevitably triumphs over intellectual error (theological presuppositions, belief in fictitious entities, lack of trust in the inevitability of scientific progress). The paper was up-to-date in its operationism because Skinner had read Bridgman (1927), but it was anachronistic in defending the extension of the reflex in psychology after that movement was pretty much spent by the end of the 1920s (Coleman and Gormezano, 1979, pp. 8-9).

Skinner made for himself an honorable place in his positivistic tale of hard-won progress: his first task was to purify the concept of reflex of superfluous and metaphysical contaminations that had become attached to the concept during its historical evolution; this he accomplished by conceptual criticism aided by historical illustration, and also by operationalization, a strategy discussed above. His second task was to liberate the reflex from its historical linkage to very restrictive animal preparations and to demonstrate, in his own laboratory research, its adequacy for the study of freely moving organisms. The concept *would* be adequate if "the behavior of an organism is an exact, if involved, function of the forces acting upon the organism" (Skinner, 1931, p. 446).

In working out his defense of determinism during the first half of the 1930s, Skinner followed our third developmental theme, in the sense that he shifted

from his original concern with causal necessity to a less insistently deterministic search for lawfulness in behavior (see Scharff, 1982). Laboratory discoveries were crucial for this transition from necessity to lawfulness, and the process was protracted. Although the interested reader can consult various primary (e.g., Skinner, 1932b, 1935a) and secondary (e.g., Coleman, 1984; Scharff, 1982) sources on the theme, the following telescopic summary will suffice for our present purposes.

Skinner stopped replacing his research apparatus when he succeeded in demonstrating quantitative orderliness in the rate of panel-pressing during the process of satiation of hunger (Skinner, 1930; cf. Skinner, 1956; Coleman, 1987): rats in his apparatus reliably produced panel-press data that could be plotted as concave-down cumulative-response curves that were closely fit by a power function of time (Skinner, 1930). When he substituted a lever for the panel, he found that a power function of time with the same exponent fit cumulative records of lever-pressing also, in spite of the topographical differences between the acts (panel-pushing and lever-pressing) that initiated what Skinner thought of as a sequence of ingestive reflexes. Finding such uniformity ("lawfulness" in Figure 1) implied that topographical differences in the initiating acts are irrelevant (Skinner, 1932b, p. 47), a suggestion that is entirely contrary to the great significance of topographical characteristics in reflexological analyses of behavior. Skinner added his own casual observation that *lever-pressing itself* involved a variety of ways in which the lever is pressed; and he brilliantly drew the conclusion that lever-pressing "behaves experimentally as a unitary thing" in spite of topographical variability in how the lever is pressed (Skinner, 1935a, p. 45). His demonstration of orderly changes in rate as a function of manipulations (i.e., lawfulness) provided him with empirical support for a molar behavior unit (i.e., the *class* of lever-presses).

#### (4) *From Molecular to Molar*

Skinner's conclusion that lever-pressing is unitary amounted to a defense of a molar behaviorism over against molecular viewpoints such as those of reflex physiologists with whose work he was familiar and theorists in psychology who were influenced by physiological ideas (Skinner, 1935a, pp. 47–48). This theme is represented in Figure 1 as a less-than-crucial theme, because that shift was very dependent on Skinner's movement from determinism (necessity) to orderliness (lawfulness); in addition, Skinner retained the concept of reflex (elicited behavior)—a concept with a history of linkage to the molecularist research strategy of reflexology—beyond the point at which he had published a full statement of molar behaviorism (Skinner, 1935a).

Skinner's attachment to the concept of reflex throughout the 1930s merits emphasis for a couple of reasons. It is arguable that the single most weighty

factor in his pre-1938 research program is that he operated from a reflexological perspective, in terms of systematic pronouncements and in his laboratory research. In his first systematic paper Skinner (1931) was concerned to extend reflexes by arguing, on historical and philosophical grounds, that there is no reason to expect insuperable obstacles to the continuation of an actual historical pattern of (physiologists') discovering eliciting stimuli and thereby demonstrating that yet another behavior is actually reflexive (Skinner, 1931, pp. 431; 436–437). To say Skinner felt he had history on his side would be no exaggeration. He also *assumed* that the “ingestive sequence” whose rate he recorded was a reflex chain (e.g., Skinner, 1930, 1932a, 1932b). Since the rate was an orderly function of time (Skinner, 1930), he presumably had demonstrated that behavior which could plausibly be regarded as nonreflexive—because ingestion is highly variable—is actually lawful, i.e., reflexive.

Characterizing Skinner as a sort of reflexologist also makes sense of his early laboratory–research projects. First of all, Skinner searched for reflexes in the behavior of his freely moving rats (e.g., Skinner, 1979b, pp. 32–33; 36–37); he tried out a naturalistic study of reflexes in infant rats and planned to record them photographically in the manner of Magnus's (1924) *Körperstellung*, but the project was not carried beyond preliminary narrative records of rat behavior. Second, Skinner investigated such reflexes as the locomotor thrusting that can be elicited by holding the rat's tail, as well as noise-elicited “inhibition” of free locomotion on an enclosed runway (Coleman, 1987, pp. 51–52; Skinner, 1956). Third, from rats in that same runway apparatus Skinner obtained measures that closely resembled standard reflex–physiology dependent variables: these included on-trial response magnitude and latency, and even an R/S ratio (ratio of response magnitude to stimulus intensity) [Coleman, 1987, pp. 51–52]. Fourth, Skinner thought in terms of reflexological constructs such as the interaction of antagonistic reflexes (Skinner, 1979b, p. 33) and the chaining of components of serially complex acts, such as panel-pressing (Skinner, 1932a, pp. 23; 31–34; 1938, pp. 52–55). Skinner was so thoroughly accustomed to reflexological mechanisms and constructs that he continued to employ them in the lever-press preparation even though his response-rate measure precluded the identification, measurement, and manipulation of reflex components, as he candidly admitted (Skinner, 1932b, p. 46); so, there was a bit of inconsistency in Skinner's theoretical outlook.

In making sense of Skinner's inconsistency, it helps to keep in mind that, because he was so attached to reflexological ideas, his relinquishment of that framework was piecemeal and protracted. Sometime in the winter of 1930–1931, he put aside his study of noise-elicited halt of running in the enclosed runway, *even though* he had achieved his preliminary objective of

obtaining traditional reflex measures from freely moving animals (Coleman, 1987, pp. 50–52; 62); he gave up on translating Magnus's (1924) book at some point after he had made fruitless contact with Williams and Wilkins (Philadelphia) and Blakiston publishing houses in the spring of 1930; his Medical School project on spinal block in the cat during the 1932–1933 academic year was a disappointment and was not repeated (Skinner, 1979b, pp. 107–109); eventually, he distinguished operants from respondents (i.e., true reflexes), and yet he *still* held onto the concept of reflex as a very abstract umbrella-term for the two behavioral categories (see theme 7 below).

During his piecemeal retreat from the reflex, Skinner positioned himself somewhere between reflexological and holistic (e.g., Gestalt) behavior theories; this is nicely illustrated by his conclusions regarding the structure of behavior, that is, the degree of mutual influence and interdependence among the components of a complex behavioral sequence. Skinner's discoveries convinced him that lever-pressing functions as a unit even if located in a complex chain, such as the chain consisting of {sight of lever}—{press}—{"click"}—{approach feeder}—{sight of food}—{eat}. Skinner experimented with the effect of breaking the {press}—{"click"} connection or the connection of {approach feeder}—{sight of food}. He found that breaking the chain extinguishes only the links prior to the break: in other words, extinction of a response requires that it be emitted without consequence. This rule held *no matter where the break occurs in the chain*, a conclusion implying that the parts of the chain are equivalent in their dependence on the extinction rule; finally, that conclusion in turn suggested that the components enjoy a functionally "autonomous status" (Skinner, 1938, p. 106) in their independent susceptibility to reinforcement and extinction manipulations. The latter suggestion was contrary to "a doctrine of the 'wholeness' of the [molar] act of pressing the lever to get food" (p. 106).<sup>3</sup>

The resulting position that Skinner staked out resembled Tolman's (1932, e.g., chapter 1) notion that behavior has descriptively emergent (molar) properties. Skinner's conclusion suggested (1) that behavior involves relatively little intrinsic structure—a position that is contrary to reflexology (Skinner, 1932b)—and (2) that the sequential connectedness of behaviors found in mature organisms is primarily the result of reinforcement operations (see theme 6 below).

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<sup>3</sup> The distinction between the unitariness of pressing and the unitary character of pressing-in-order-to-get-food was missed by Krechevsky (1939, see p. 407) in his review of Skinner's book. Skinner accepted the generic nature of pressing as a class of behaviors that have the same consequence, but he rejected the notion that food is part of the action. The relationship of food to the operant behavior is extrinsic, not definitional; the measured behavior is "lever-pressing," not "pressing-lever-to-get-food" (see Koffka, 1927, p. 172).

(5) *From Reproducibility to Orderliness*

A piecemeal drift away from reflexology—and the more specific change from a molecular unit to a molar behavioral class—entailed modifications in Skinner's criterion of adequacy for evaluating possible behavioral units. His criterion shifted from requiring demonstration of exact reproducibility (Skinner, 1931, pp. 436-437; 449) to requiring demonstration of orderly and smoothly graded changes in response rate under manipulation of the relevant variables (Skinner, 1935a, pp. 52; 55; 57). Demonstrating exact reproducibility was important in his history-of-physiology scenario vindicating determinism (Skinner, 1931), but it was impossible to carry out in his own research, because he could neither control the stimuli that he presumed will unfailingly elicit lever-pressing (Skinner, 1932b, p. 23; cf. Verplanck, 1954, pp. 283-287) nor demonstrate the static laws of the presumptive lever-press reflex.<sup>4</sup>

Skinner (1935a) drew a firm distinction in interest, practice, and subject matter between the field of physiology and what he simply called a "science of behavior." The commitment of physiologists was to the study of "the physiological events mediating a reflex" (Skinner, 1935a, p. 47; see also Skinner, 1931, p. 444) in preparations that were restricted in order to achieve exact reproducibility of the behavioral unit.

There was an implicit suggestion that the results of the physiologist may, therefore, not be applicable to the actions of the normal, unhampered organism, because the parts of behavior and environment which the investigator obtains by severe restriction of his preparation do not take account of the "natural lines of fracture along which behavior and environment actually break" (Skinner, 1935a, p. 40). By contrast, the behaviorist was "interested in the behavior of the intact organism" (Skinner, 1935a, pp. 47-48) and was thereby committed to the use of relatively unrestricted preparations and to a criterion that did not require exact reproducibility.

Procedural implications followed from these considerations. The appropriate research strategy for behavior researchers would be to relax the kind of restrictions (surgical, etc.) that typify the "main-force" strategy of physiologists. A justifiable degree of restriction is one that produces the most regular functions ("dynamic laws") as important independent variables are manipulated. Relaxation of restrictions allows behavior to exhibit spontaneity and variability, behavioral features that Skinner had earlier regarded as the greatest obstacles to demonstrating determinism (necessity); in the post-1935 program, they were treated as unproblematic, having been *neutralized* in his

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<sup>4</sup> A "static" law of some reflexive behavior is of the form  $F=f(S)$ , where  $R$  is a dependent-variable measure of the behavior and  $S$  is a measurable property of the eliciting stimulus. For lever-pressing,  $R$  would be an appropriate dependent variable and  $S$  would be a manipulatable property of the stimulus complex that evokes pressing. See Verplanck (1954, pp. 283-287).

abandonment of a criterion of exact reproducibility. Skinner's companion paper in the 1935 *Journal of General Psychology* argued for a demarcation of the Pavlovian arrangement from his lever-press conditioning preparation on the basis of differences in the two types of preparation and in the two types of conditioned reflex.

(6) *From One-Factor to Two-Factor*

Let us again turn back to 1931. Although Skinner's early position assumed a single type of conditioned reflex (Skinner, 1931, pp. 437; 454), his research eventually led him to a two-types formulation; he and several of his contemporaries would later be placed in the category of "two-factor learning theory" (Skinner, 1932c, 1935b; see Kimble, 1961, and Kimmel, 1976). Skinner's first version of a two-factor distinction, presented in a footnote (Skinner, 1932c), distinguished between Pavlovian Type I and his own arrangement, which he simply called Type II. Three years later, Skinner (1935b) reversed the labels to distinguish his own Type I and the Pavlovian Type II. In 1937, he revised his position, and Types I and II were identified with "operant" and "respondent" behavior, respectively; they were renamed Types R and S in *The Behavior of Organisms*. At the present point in our narrative we are concerned with the 1935 two-types distinction of Types I and II (Skinner, 1935b); we will treat the operant-respondent distinction in the seventh developmental theme. The shift from one to two factors involved Skinner in three questions, regarding (1) the comparative importance of the two types of conditioning, (2) distinctive features of research fields concerned with each type, and (3) the interrelationship of the two types in ordinary laboratory arrangements.

The first question was answered by his assertion that the most important part of the learned behavior of the freely moving organism is of the type that he was studying (Skinner, 1935b, p. 75). In regard to the second issue, he noted that the Pavlovian enterprise was committed to more restrictive preparations than those favored by researchers like himself, and he suggested that his type of research involved less arbitrary "forcing" of behavior and more acquiescence to "natural" categories of moderately unrestricted behavior (Skinner, 1935a, pp. 151-160). In his explanation of the effects of punishment on response rate we have an illustration of his position on the third matter: he made use of the idea that a Pavlovian conditioned emotional effect temporarily hindered response output while leaving the total number of emitted responses unaffected (Skinner, 1938, pp. 151-160). Thus he endorsed two factors, of which the more important factor was operant; although Pavlovian processes do affect operant behavior, their influence is mediated by variables classified as "emotional."



The next two themes are more central to this paper than are the two themes (5 and 6) with which we have most recently been concerned, so a pause and a glance back at Figure 1 would be useful. We have already claimed that the central personal dynamic in Skinner's development was his effort to stake a claim as a major theorist. In the next two themes, we will argue that philosophical concerns were the primary fulcrum for his bid.

(7) *From Reflex to Operant*

Let us briefly situate the argument. We suggested that Skinner's main-spring was a great ambition to make his mark in psychology; given the example provided by mature scholars, such as those with whom he was acquainted in Harvard's Society of Fellows, he knew that would involve attaining recognition as a theorist who had made a major discovery or conceptual innovation. But his discoveries up through 1935 were not quite up to that mark. True, he had demonstrated that the behavior of the freely moving organism is characterized by a lawfulness that is more subtle than the demonstration of nearly exact reproducibility of behavior from one elicitation to the next. Moreover, his lever-press preparation exhibited features that apparently set it apart from Pavlov-type conditioning, such as that it involved strengthening an existing reflex rather than creating a new (i.e., CS–CR) reflex (Skinner, 1935b).

But Skinner's (1935b) distinction of two types was just one of several versions of a two-factor approach. (How it and other versions were received and how they fared in subsequent years are questions that would take us too far afield, although they are important for assessing the perceived significance of the theoretical contribution made by Skinner's "Two Types" paper of 1935). Even if we concede that Skinner's (1935b) two-types distinction was judged important, it only made more technically elaborate a distinction that others were in the process of making or had already made in the mid-1930s (e.g., Schlosberg, 1934, 1937; Tolman, 1934); other theorists would have known this because, like Skinner, most of them were East Coast researchers.

Skinner's (1935a) formulation of molar behaviorism would also have been regarded as insufficient. Although Skinner had apparently shown that the molar orderliness of lever-pressing was descriptively emergent (and had made a technically elaborate defense of it in his 1935 paper), promoting that feature would merely add support to Tolman's already formulated (e.g., 1932) position on the nature of purposive behavior. So, there are reasons to regard Skinner's discoveries and publications up to the mid-1930s, although solid, as insufficient to be considered *great* contributions; and indeed they were not, because, judging from citations, his work had not achieved great visibility.

In addition to these obstacles, there were dangers. His two-types formulation was couched in terms of reflexes, that is, elicited behaviors of two types. Nothing ruled out a reduction of Skinner's Type I to (Pavlovian) Type II, because both involved reflexes. Such a reduction was actually attempted by Konorski and Miller (1937), who rejected Skinner's grounds for two types and offered their own version of the distinction. Elsewhere we described the Konorski-Miller proposal, which would have absorbed Skinner's research into a subcategory of Pavlovian conditioning that they called Pavlovian Type II (Coleman, 1981). In much technical detail, a review of which would be distractive here, we suggested that Skinner's postulation of operant behavior served the theoretical function of protecting his (1935b) distinction between two types of conditioned reflex (Coleman, 1981).

We also claimed, in that earlier paper, that the concept of operant played the role of keeping separate the response-reinforcer (i.e., R-S<sup>R</sup>) contingency of his own preparation from the Pavlovian stimulus-reinforcer (i.e., CS-US) contingency, thus assuring the definitional separateness of his two types of conditioning (Coleman, 1981, pp. 220-221; see also Schoenfeld, 1976 and Hearst, 1975). His postulation of operants in 1937 amounted to an acceptance of spontaneity as a behavioral criterion, thus reversing his earlier positivistic annoyance with the concept of spontaneity (Skinner, 1931, pp. 435-437). Accordingly, we suggested that Skinner's postulation of operants, seen in its historical context, was prompted more obviously by considerations of theory-defense than it was necessitated by particular research findings.<sup>5</sup>

The significance of the shift from reflex to operant should not be underestimated, and there is certainly no indication that it has been underestimated by those trained in the Skinnerian tradition. Quite the opposite seems to be the case. However, the historical importance of the concept of operants lay primarily in its serving to emphasize, through consistent labeling, the distinctiveness of Skinner's work as a theorist and (even more important later, years after the publication of *The Behavior of Organisms*) to provide terminological demarcation of his research enterprise (i.e., operant conditioning) from other specialties. The second matter (i.e., disciplinary) has guaranteed that the concept of operant would assume increasing significance as the operant conditioning enterprise became established institutionally.

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<sup>5</sup> Thus far this idea has not been specifically addressed in the literature, nor has it been incorporated into discussion of the conceptual status of operants. The notion that operants are nonelicited behavior appears still to be the standard position (e.g., Hinson, 1987, p. 184; see also Catania, 1968; Boakes and Halliday, 1970, pp. 351-352; Verplanck, 1954, pp. 273-274; 288-289; Wetherington, 1982, p. 330; but see modifications suggested in Catania [1971], Herrnstein [1977], and Segal [1972]; on the other hand, the emission-elicitation distinction is completely bypassed in Pear and Eldridge [1984] and Schick [1971]. See Kimmel [1976] and Schoenfeld [1976] for other perspectives on the issue).

An additional function of operants was to provide a distinctive character for the research specialty of which he was the primary founder; his *Behavior of Organisms* articulated many features of the field of operant behavior and legitimated his claim to the role of major theorist. Figure 1 represents this importance by an arrow from the Operant box to the Theorist box.

(8) *From Nominalist to Realist<sub>1</sub>*<sup>6</sup>

Figure 1 accommodates our claim that Skinner's primary personal concern in the late 1920s and early 1930s, namely to be a good critic, predisposed him—in Figure 1, the reader will notice an arrow from the Critic box to the Nominalism box—to the view that scientific concepts are *not* names of objects to which they refer but are merely signs. They might be useful (i.e., Instrumentalism in our ninth theme, below) in the summarization of scientific findings, an idea that was supplied by Skinner's reading of Mach (e.g., Mach, 1893; see Smith, 1986).

Figure 1 indicates that Skinner's postulation of operants was a derivative consequence of two conditions: (1) his experimental demonstration of the molar Lawfulness of lever-pressing, and (2) the attendant shift to a Realist<sub>1</sub> attitude<sup>7</sup> regarding the ontological status of scientific constructs [the shift

<sup>6</sup> In an earlier publication, Professor Skinner objected to the use of these "isms" (see Coleman, 1984, p. 472, Note 1). Our reason for using these isms—in spite of Skinner's objection—is that his claims recognizably constituted a position on the traditional philosophical issue of the ontological status of universals. A minimal consideration is that Skinner's three years of experience in the Department of Philosophy and Psychology would surely have introduced him to that issue; other considerations, such as his friendship with Willard Quine and his reading in philosophy of science, support the same. Finally, at least one of his contemporaries used the term in a letter to Skinner: "I should especially like to know whether you feel, as I do, that the name nominalist is not so much a term of reproach as the philosophers, who know so much about concepts, seem to wish to make it appear" [Leonard Carmichael to BFS, March 21, 1934, in Box 2 of Accession 9710, Harvard University Archives]. The letter, in response to Skinner's colloquium on "Experimental Studies in the Definition of Stimulus and Response" at Brown University, suggests that the philosophical term would at least not have appeared unfamiliar in a conversation between Skinner and his contemporaries. Whether a position is identified by a label is irrelevant: Skinner's early position on the ontological status of concepts was "nominalistic," whether or not he was acquainted with the term; his later position, explicated in 1935 and elaborated in subsequent papers, was a "realist" position, whether so named or not.

<sup>7</sup> "Realism" has been used as the label for two different claims. The first is the contention that universals (class-constructs) have referents that actually exist; class-concepts do not merely serve as verbal constructions. Similarly, the Realist<sub>1</sub> claims that referents exist for problematic scientific concepts (hypothetical constructs) whose identification is not a matter of unproblematic observation. Thus the Realist<sub>1</sub> of the late nineteenth century asserts that atoms exist, while the Nominalist regards atoms as mere fictions. The second use of "Realism" involves an opinion concerning the use of concepts, particularly those that concern "problematic entities." The Instrumentalist contends that concepts are more appropriately regarded as tools which aid us in attaining practical objectives, such as predicting with accuracy some phenomenon. The Realist<sub>2</sub> claims that the terms are names, not tools, and that they refer to objects or properties different from those of the common-sense world. The doctrines are obviously related.

from Nominalism to Realism<sub>1</sub> in the figure]. The left-most wavy line connects these two resultant states and shows that the first state (Lawfulness) contributed to the second one (Realism<sub>1</sub>); his postulation of operant behavior was "afforded" by the demonstration of Lawfulness and by the adoption of Realism<sub>1</sub>. The concept of operant was to be the major conceptual innovation that would give him a distinctive niche among theorists.

The thickness of the arrows represents our suggestion (which we will elaborate below) that, of the two conditions—that is, the shift to Realism<sub>1</sub> and the demonstration of the lawfulness of pressing levers—the adoption of Realism<sub>1</sub> was the more important; the adoption of Realism<sub>1</sub> was the *sine qua non* that afforded the postulation of operants on the basis of his laboratory findings (i.e., Lawfulness). We will propose (1) that the concept of operant was derivative, not primary, in Skinner's conceptual development, and (2) that the concept of operant was not merely or primarily an empirical discovery but was the product of a philosophical intermediary. These claims run counter to the disciplinary proposal that the concept of the operant is historically or nomologically fundamental to the field of research that Skinner originated (e.g., Schoenfeld, 1976, p. 132), as the name of the field suggests. Historically—and from an entirely presentist perspective—the operant has indeed been accorded that status, but such an evaluation is not justified by an historical reconstruction of the conceptual standing of operants, no matter how important the concept may have become in establishing and maintaining disciplinary distinctiveness.

At the heart of our suggestions is the fact that Skinner's postulation of operants *required* that he abandon his earlier Nominalist position and make a number of ontological claims that he could not verify. First, he could not, *par impossibilité*, demonstrate the *absence* of stimuli that evoke operant behavior. Second, he could not demonstrate the existence of behaviors whose principal defining feature is that they *are* spontaneous; he could only point to behaviors that appear to observers as spontaneous acts. Indeed, he had studied lever-pressing without identifying, controlling, or manipulating the eliciting stimuli that he *assumed to be operative*; nonetheless, prior to the exchange of papers with Konorski and Miller, he referred to these actions as elicited behaviors; these facts are congruent with our calling Skinner a sort of reflexologist. In spite of his earlier shifts (stated in his 1935a paper) to molar behaviorism (Theme 4), to lawfulness (Theme 3), and to orderliness as a criterion of research adequacy (Theme 5), a category of operant behavior was not required or suggested at that time.

Third, finding that the power function which fit cumulative data of panel-pushing also fit lever-pressing data indicated to Skinner that "the particular reflex with which the eating behavior begins" (Skinner, 1932b, p. 47) does not have as strong an effect on measured aspects of the sequence (e.g., rate)

as reflexology would imply. But he did not thereby demonstrate that lever-pressing is not a chain of elicited behaviors, because he could not demonstrate the independence of molar response rate from the static functions of its molecular constituents unless he were able to measure and control the stimuli that elicit the components of lever-pressing. Given that he could not control, much less experimentally vary, the stimuli that the lever afforded, his behavioral formula was not correctly rendered as  $B=f(S,A)$ , as he admitted (Skinner, 1932b, p. 23). The notion that lever-pressing is a chain of reflex components survived in a sort of operant disguise after he had disowned the reflexological version of that idea (Skinner, 1938, pp. 52-55).

Fourth, he conducted no research in the period up to *The Behavior of Organisms* that would show that lever-pressing (or other operant behavior) cannot be conditioned by Pavlovian procedures, but he *assumed* the same, on the basis of the purported absence of an eliciting stimulus for operant behaviors (Skinner, 1938, p. 22). (It is ironic, therefore, that later investigators during the 1970s and 1980s succeeded in conditioning, with Pavlovian methods, a variety of lever-oriented behaviors. These acts would qualify as operants, on Skinner's criterion that operants operate upon a part of the environment.)

Fifth and last, Skinner's evidence for a functional divergence of dynamic laws of true reflexes and operants—for example, the latency of signaled lever-pressing does not increase during extinction, but the latency of Pavlovian CRs does—was certainly suggestive but not at all decisive for postulating operant behavior. First of all, the divergence was only partial. There was a large enough pool of common properties of instrumental habits and Pavlovian CRs (e.g., generalization, discrimination, extinction, and so on: see Kimble, 1961, pp. 81-98 for a later summary) to justify rejection of the two-factor approach in favor of a unifactor theory such as that of Hull or Guthrie. Second, the divergence to which he appealed was not a divergence of the *same* dependent variable for an operant and a reflex but rather a divergence of measures that are only analogous. For example, lever-press force was taken to be analogous to reflex magnitude, and lever-press duration was taken to be analogous to reflex after-discharge. The strength of the claim for divergence is only as strong as the closeness of the relationship between the two sets of dependent variables.

Our intention in making these points was to show, *not that Skinner had no evidence for a distinction* of operant and respondent, but only (1) that the postulation of operants was based on evidence that would not have withstood critical and Nominalistic blasts from the B.F. Skinner of his graduate-student period—which implies that he had at least partially repudiated his earlier Nominalism; (2) that the postulation of operants was prompted historically by considerations in protecting his distinction of two types (Skinner, 1935b)

from the reductionist efforts of Konorski and Miller (see Coleman, 1981); and (3) that the postulation of operants was afforded by Skinner's demonstration of the molar orderliness of lever-pressing *and* by adoption of a Realist<sub>1</sub> ontology in place of his earlier Nominalism. This reasoning does justice to the importance that has been attached to the concept of operant; at the same time, it accommodates the fact that, for empirical purposes (as Skinner's research antedating the postulation of operants clearly shows), the allegedly nonelicited status of operant behavior was beside the point. The role of Realist<sub>1</sub> ontology in affording the postulation of operants and, thereby, in legitimating Skinner's claims to the status of theorist warrants our giving it a central role, as Figure 1 shows.

#### (9) *From Instrumentalist to Realist<sub>2</sub>*

Skinner's attitude toward *the role of concepts in scientific work* shifted from an Instrumentalist to a Realist<sub>2</sub> stance (Coleman, 1987, pp. 493-494). In his graduate-school years Skinner was quite impressed by Mach's historical analysis of concepts. Mach's "economy of concepts" involved an Instrumentalist treatment of concepts as merely useful shorthand for a longer list of particular facts or regularities; in his "Concept of the Reflex," Skinner carried out a conceptual criticism by applying this formulation to the concepts of synapse, reflex, and reflex strength (Skinner, 1931, pp. 427; 441-443; 446; and 452-454), treating them as tools for aggregating empirical regularities. This Instrumentalist view on the role of concepts reflected the personal appeal of critical hard-headedness, as we argued earlier in this essay; and it was symptomatic of Skinner's adoption of a general positivist orientation. But, as Skinner shifted into the *constructive phase* (Mackenzie, 1977) and developed confidence in his own distinctive formulation, his stance shifted to Realism<sub>2</sub>, a position congruent with the social role of theorist. We have already covered that point and need not belabor this closely related theme.

### Summary

Our set of nine themes is not exhaustive. For example, we have paid no detailed attention to Skinner's language projects, which at times assumed priority (e.g., Skinner, 1934, 1936b). Moreover, our focus has been primarily intellectual rather than social. We have also paid little more than unidimensional attention to the personal development of B.F. Skinner. Lastly, although our focus has been technical, we have neglected entire areas of Skinner's rat research, for example, in discrimination and in schedules; a closer look at these may suggest conceptual themes that escaped our notice; and it is possible that the relationships we have suggested among the themes may have to be revamped. We leave such possibilities to others.

These nine themes may be useful in several different ways. They provide an issue-related framework to appreciate Skinner's development as a behavior theorist. They serve as a corrective to the tendency to treat *The Behavior of Organisms* as a starting point for radical behaviorism. They raise investigatable questions about the functional role that various concepts, discoveries, and shifts served during Skinner's development.

The nine themes provide bases for comparison of Skinner's development with that of other theorists in the behavior-theory enterprise; it would be interesting to work out a comparable examination of Hull or Tolman (cf. Smith, 1982). In view of the fact that Skinner's development has been presented and treated as a potential prototype of scientific-career development (Skinner, 1956; cf. Coleman, 1987, pp. 62-64), the developmental themes disentangle separate aspects of his career trajectory up to 1938. Moreover, they make more convincing our claim that Skinner's philosophical shifts were more significant than many writers, including Skinner, have allowed. Additional themes are undoubtedly required for the period following *The Behavior of Organisms*. Finally, given the thick-narrative style of Skinner's autobiographical works, there is a need for interpretive tools to understand his development. The themes we describe can serve at least as a beginning.

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