Why Skinner Is Difficult

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Skinner's views are commonly misrepresented. One reason for this difficulty is that changes in the way that Skinner formulated his views occurred in a gradual evolution over time throughout Skinner's career, and the changes and their significance were not as conspicuously marked as they might have been. Among these changes were a movement from a two-term necessity to a three-term contingency; a movement from *discriminative stimulus* to *setting* as the first term in his three-term contingency; and a movement from determinism to random variation as a foundational principle in his selectionist behaviorism. When not seen in their historical development over time, a sample reading of Skinner's views may readily result in misleading or inaccurate interpretations, particularly in respect to his later work. Seen in historical context, however, the accounts that survived after the changes Skinner made are well integrated in a selectionist theory of behavior.

Key words: determinism, meaning, selectionism, setting, B. F. Skinner, three-term contingency

Many ways have been identified in which Skinner has been misunderstood and misrepresented (Cooke, 1984; Gentile, 1996; MacCorquodale, 1970; Todd & Morris, 1992). One reason for so many misinterpretations of Skinner's work is that Skinner made changes in his verbal formulations (Moxley, 1992; Timberlake, 1988; Verplanck, 1954), continued to make changes, and these changes were not as well marked with clarifying explanations as they might have been. Although other reasons for the misinterpretations of Skinner's work may well be considered, they will not be addressed here. Instead the following focuses on the circumstances of Skinner's changing accounts as offering one reason for the interpretative problems by readers of Skinner; these problems extend to those who read what such readers have written. In addition, and perhaps more important, these changes show the evolution of key aspects in Skinner's accounts. This evolution, with its often weak markings, is illustrated by drawing on some examples of Skinner's changing verbal behavior: from the two-term necessity to the three-term contingency; from using the

term *discriminative stimulus* to using the term *setting* as one of the key terms in his three-term contingency for operant behavior; and from appeals to determinism to appeals to random variation as a relevant foundational assumption. In making these changes, Skinner reformulated his views in a coherent and comprehensive way that is aligned with natural selection. Skinner commonly expressed his views in terms of selection; and variants of that term have been applied to Skinner's work (e.g., Baum, 1997; Catania, 1995; Palmer & Donahoe, 1992; Vargas, 1993).

FROM THE TWO-TERM NECESSITY TO THE THREE-TERM CONTINGENCY

Often, Skinner did not specify sources for his views or clarify the relation of his views to the views of others, which would have made it easier for readers to see his views against the background and clarification that related views would provide. It also appears that on at least one occasion—with respect to a possible source for his three-term conception of operant behavior—he did not portray a source accurately. Skinner (1979/1984, pp. 10–11, 1989a, p. 121) said that reading Russell's articles in the *Dial* had led

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him to buy Watson's *Behaviorism*, and a year or so later he bought Russell's *Philosophy* (published in Great Britain under the title *An Outline of Philosophy*). However, Skinner failed to accurately report a significant reinterpretation by Russell of Thorndike's law of effect.

Before presenting this inaccuracy, some background is in order. In defining the law of effect, Thorndike (1911/1965) had said in part,

The Law of Effect is that: *Of several responses made to the same situations, those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation, so that, when it recurs, they will be more likely to recur* [italics in original]. (p. 244)

Thorndike presented his law of effect in analogy with evolution and natural selection:

The process involved in the learning was evidently a process of *selection* [italics added]. The animal is confronted by a state of affairs or, as we may call it, a "situation." He reacts in the way that he is moved by his innate nature or previous training to do, by a number of acts. These acts include the particular act that is appropriate and he succeeds. In later trials the impulse to this one act is more and more stamped in, this one act is more and more associated with that situation, is *selected* [italics added] from amongst the others by reason of the pleasure it brings the animal. (p. 283)

In addition, as part of his selectionism, "Thorndike constantly returns to the role of chance to explain even the most seemingly intelligent animal behavior" (Clifford, 1984, p. 142). Thorndike also emphasized the probability, not the necessity, of acting:

When in a certain situation an animal acts so that pleasure results, that act is *selected* [italics added] from all those performed and associated with that situation, so that, when the situation recurs, the act will be *more likely* [italics added] to follow than it was before. (p. 294; also cf. p. 283)

A particular problem with such statements, however, was the reference to *satisfaction* or *pleasure*. These were not objective terms and could not be directly measured.

In *Philosophy*, Russell (1927/1970)

showed how Thorndike's law of effect could be reinterpreted in objective terms:

Thorndike's law, as it stands, does not belong to objective psychology, and is not capable of being experimentally tested. This, however, is not so serious an objection as it looks. Instead of speaking of a result that brings satisfaction we can merely enumerate the results which, in fact, have the character which Thorndike mentions, namely, that the animal tends to behave so as to make them recur. The rat in the maze behaves so as to get the cheese, and when an act has led him to the cheese once, he tends to repeat it. We may say that this is what we mean when we say that the cheese "gives satisfaction," or that the rat "desires" the cheese. That is to say, we may use Thorndike's "Law of Effect" to give us an objective definition of desire, satisfaction, and discomfort. The law should then say: there are situations such that animals tend to repeat acts which have led to them; these are the situations which the animal is said to "desire" and in which it is said to "find satisfaction." This objection to Thorndike's first law is, therefore, not very serious, and need not further trouble us. (pp. 35–36)

Thorndike's law of effect is in terms of situation, response, and satisfaction (or dissatisfaction), but situation and response, an S-R relation, are the only objectively observed terms. Russell reformulated satisfaction as "the animal tends to behave so as to make [results] recur" (p. 35) and later as "the animal tends to repeat acts" with "certain results" (pp. 37–38). This gives a pairing of an observable situation and an observable response (S-R) from Thorndike connected to an observable result from Russell to give an observable S-R-S relation that must have some repetition in order to show a tendency to recur. Together, Thorndike and Russell presented key ingredients that would later appear in Skinner's operant behavior.

On his part, Skinner (1932c) connected two S-R pairs in a "S'-R' → S-R" (p. 274) formulation of two chained reflexes in presenting a different, second type of conditioning. Skinner (1935) later re-presented these pairs in a "S₀-R₀ → S₁-R₁" (*Cumulative Record*, 1972, p. 479) formulation for Type I behavior, which Skinner (1937, *Cumulative Record*, 1972, p.

492) later identified as operant behavior. A distinguishing feature of operant behavior was that "It is the nature of this kind of behavior that it should occur [spontaneously as it were] without an eliciting stimulus, although discriminative stimuli are practically inevitable after conditioning" (Skinner, 1937, *Cumulative Record*, 1972, p. 491). Discriminative stimuli were practically inevitable because only the first instance of the behavior could be considered to be spontaneous. A relation to antecedent stimuli was inevitable on the next occurrence of the behavior; and operant behavior could not remain as spontaneous behavior when the terms of the operant were class concepts (Skinner, 1935). More than one instance of behavior was required for a class concept, and repeated instances meant the introduction of functional relations to "inevitable" antecedent stimuli. The sequence of paired terms in Skinner's operant formula at this time was illustrated by S_0 (lever)– R_0 (pressing) and S_1 (food)– R_1 (salivation, eating) or S_1 (shock)– R_1 (withdrawal, emotional change). This four-term sequence displayed a reflexological framework. However, only three terms were in Thorndike's situation-response-satisfaction relation, although Thorndike often referred to the objective situation and response relation by itself. And only three terms were needed to satisfy Russell's more objective account of Thorndikian conditioning. Skinner also came to conclude that only three terms were needed.

Skinner (1984/1988) identified Russell as "an early and important influence" (p. 333). More specifically, Skinner (1976/1977, p. 299) quoted from pages 33 and 36 of Russell's (1927/1970) work, and he referred to phrases he had underlined on pages 33 and 35 in his copy of Russell. Inasmuch as the above citation from Russell (1927/1970) occurred on pages 35 and 36, it is unlikely that Skinner missed Russell's reformulation of Thorndike. However, Skinner identified no helpful contribution from Rus-

sell in respect to Russell's modification of Thorndike's law of effect. On the contrary, in what amounts to a curious kind of memory reversal, Skinner (1976/1977) said, "Russell, again following Watson, was trying to interpret the Law of Effect as an example of the substitution of stimuli" (p. 299).

However, as Boakes (1984, p. 259, note 31) pointed out, Russell was doing no such thing. Immediately after the above passage in which Russell provided a supportive reinterpretation for Thorndike's law of effect, Russell (1927/1970) explicitly stated his *disagreement* with Watson in this respect:

Dr. Watson considers one principle alone sufficient to account for all animal and human learning, namely the principle of "learned reactions." This principle may be stated as follows:

When the body of an animal or human being has been exposed sufficiently often to two rough simultaneous stimuli, the earlier of them alone tends to call out the response previously called out by the other [italics in original].

Although I do not agree [italics added] with Dr. Watson in thinking this principle alone sufficient, I do agree that it is a principle of very great importance. (p. 36)

Part of Russell's expressed disagreement with Watson was even quoted by Skinner in support of Skinner's (1976/1977, p. 299) own interpretation: Skinner quoted the middle italicized section of the above citation, which he noted was Pavlov's principle of the conditioned reflex, and the sentence in which Russell says, "I do not agree with Dr. Watson." Considering that Skinner probably had Russell's book before him (Skinner refers to previously underlined sections), it is difficult to understand how Skinner could have misinterpreted Russell so badly in writing that Russell followed Watson and was trying to interpret the law of effect as an example of the substitution of stimuli.

Skinner's claim about Russell is patently in error. Russell was not following Watson's single element or principle of learning. Instead, Russell (1927/1970) was showing that there was at

least another element or principle—that of Thorndike's law of effect:

There is, however, another element in learning. . . . This is the element dealt with by Thorndike's "Law of Effect." Animals tend to repeat acts which have pleasant consequences, and to avoid such as have unpleasant consequence. But, as we saw a moment ago, "pleasant" and "unpleasant" are words which we cannot verify by objective observation. What we can verify by observation is that an animal seeks situations which in fact have had certain results, and avoids situations which in fact have had certain other results. Moreover, broadly speaking, the animal seeks results which tend to survival of itself or its offspring, and avoids results which tend in the opposite direction. (pp. 37–38)

This passage is noteworthy because of Russell's more objective reformulation of satisfaction as a tendency "to repeat acts" with "certain results." Thorndike (1911/1965) had attempted an objective statement of satisfaction or pleasure: "By a satisfying state of affairs is meant one which the animal does nothing to avoid, often doing such things as attain and preserve it" (p. 244), but Russell's is better. Russell's passage is also noteworthy because of the selectionist observation that "an animal seeks results which tend to survival of itself or its offspring."

The two elements (or principles) of Pavlovian and Thorndikian conditioning that Russell discussed parallel Skinner's distinction between respondent and operant behavior. Skinner actually shifted his views, with the result that they came to be more in line with Russell's two elements than with Watson's single element. Nevertheless, Skinner (1976/1977) puts Russell with Watson as representing a view he (Skinner) later opposed:

It would be a long time before I saw the mistake which Russell and Watson were making and in which I concurred when I underlined those phrases, because the course of psychology as a science of behavior was to follow the unproductive path of stimulus-response psychology for many years. (p. 299)

Note that Skinner is saying that he (Skinner) was following the "unproductive path of stimulus-response psychology" at that time. Perhaps Skinner

recalled (a) that he had made a mistake and (b) that he had disagreed with Russell, but not the details of that mistake or the disagreement. Skinner's actual mistake at the time had been to follow Watson and the conditioned reflex rather than Russell and the law of effect. In his recollection, Skinner attributed the mistake to Russell rather than to himself. It is nevertheless curious that such a recollection was impervious to a more accurate refreshing when Skinner had the concrete evidence before him again and quoted Russell's statement of disagreement with Watson.

At first, Skinner (1938) acknowledged Thorndike only briefly in saying that the process in Type R conditioning "is very probably that referred to in Thorndike's Law of Effect" (p. 111), but he was prodded by Hilgard to do more. In his largely favorable review of Skinner's *The Behavior of Organisms*, Hilgard (1939) said,

The distinction between respondent and operant has been implicit in Thorndike's work all along, but it did not become explicit because the use of "situation" to cover discriminated as well as eliciting stimuli permitted a spurious application of the stimulus-response formula. This clear distinction is perhaps Skinner's most significant conceptual contribution. He hopes to correct the disproportionate emphasis upon respondent behavior by basing all of his work on operant behavior. (p. 122)

Crediting Skinner with this clear distinction, Hilgard then chided Skinner for not providing more detail on the relation of his work to the work of others: "If Skinner has been unable to relate his work to that of other investigators, how can a reader, coming fresh upon this new body of material, be asked to make the transitions?" (p. 124).

In 1939, Skinner (1979/1984) wrote to Thorndike and said,

Hilgard's review of my book [*The Behavior of Organisms*] in the *Bulletin* has reminded me of how much of your work in the same vein I failed to acknowledge. . . . It has always been obvious that I was merely carrying on your puzzle box experiments but it never occurred to me to remind my readers of the fact. (p. 233)

Skinner also acknowledged that his

Type R (operant) conditioning “probably” was (1938, p. 111), and indeed was (1979/1984, p. 157), similar or roughly equivalent to Thorndike’s law of effect. Skinner (1953, pp. 62–66) described similarities and differences between operant behavior and Thorndike’s law of effect as well as differences between “Pavlovian or ‘respondent’ conditioning” and “operant conditioning” (p. 65). In doing so, Skinner paralleled Russell’s account, although no mention was made of Russell, which would have established a more direct historical influence on Skinner from Thorndike through Russell. Instead, Skinner (e.g., 1966a, pp. 12–14) frequently credited Thorndike’s early work as a precursor of, if not a direct influence on, the concept of operant behavior. Skinner’s crediting of Thorndike became so generous that Iversen (1992, p. 1323) questioned whether Skinner gave Thorndike too much credit on one point (i.e., “hand shaping”).

Skinner’s somewhat belated but lavish crediting of Thorndike is noteworthy in that, in most psychological accounts, Thorndike receives little credit as a direct influence on Skinner. For example,

The historical linkage between the work upon which Thorndike’s Law of Effect was based and Skinner’s development of the concept of operant reinforcement appears to be quite indirect. . . . In *The Behavior of Organisms* (1938), Skinner appears to have regarded Thorndike’s work as a contribution to “the traditional field of learning” [p. 111] and his own work as an attempt to establish a systematic approach to the analysis of behavior, where the analysis of operant behavior happened to help explain what Thorndike had found. (Day, 1980, p. 222)

This picture would be rendered differently if Russell’s (1927/1970) account of the differences between Thorndikian and Pavlovian conditioning influenced Skinner, as it probably did—if not immediately, then later on in making Skinner’s arrival at a similar account more likely.

If Skinner had shown a development from Thorndikian conditioning through Russell’s interpretation of Thorndike’s

satisfaction to his (Skinner’s) operant behavior, readers might have seen a greater distinction between operant behavior and the reflex. The traditions from which the reflex and the operant came might also have been seen as more distinctive. Skinner (1931) traced the reflex back to Descartes, which located it in the tradition of mechanistic philosophy. On the other hand, Thorndike (1911/1965, pp. 272–294) had discussed the analogy between natural selection and the learning of behavior in some detail. Skinner’s operant behavior might then have been seen earlier and more prominently in a distinctively different selectionist tradition. However, this may have presented a problem for Skinner. His operant might have been considered more of a reconceptualization of Thorndike’s earlier work with the help of Russell than as an original outcome of Skinner’s own experimentation.

As it was, somewhat buried in the body of his text, Skinner (1938) reformulated the pairings in his $S_0-R_0 \rightarrow S_1-R_1$ formulation for Type I behavior as a three-term relation with “a prior discriminative stimulus (S^D), the response (R^0), and the reinforcing stimulus (S^I)” (p. 178) for operant behavior. (Note that Skinner moved from a four-term arrangement to a three-term arrangement. Skinner did not present a viable two-term R-S relation as the unit of operant behavior before adopting a three-term relation. Skinner, 1938, pp. 177–179, considered such a two-term relation, but he rejected it.) The three-term relation brought Skinner’s analysis more closely into line with Russell’s. However, Skinner retained the terminology of *mechanical necessity* from the S-R reflex relation (see Skinner 1931, *Cumulative Record*, 1972, p. 449) in referring to the three-term relation as “the mechanical necessities of reinforcement” (p. 178), and Skinner’s diagram for operant behavior continued to reflect a reflexological organization in connecting two-term pairs of relations in a four-term arrangement (p. 65). Later, Skinner

Figure 1. Skinner's SOR diagram.

(1953) made conspicuous analogies between operant behavior and natural selection.

Although Skinner (1977/1978, pp. 119–120, 1984/1988, p. 460) indicated that he was no longer a stimulus and response psychologist by 1938, he did not make a clean break in abandoning a two-term organization of stimulus and response in order to incorporate consequences as an ever-present essential requirement for the contingencies of reinforcement. Skinner (1945) referred to “the contingencies of reinforcement which account for the functional relation between a term, as a verbal response, and a given stimulus” (*Cumulative Record*, 1972, p. 380) and identified only two terms in the contingencies of reinforcement, a stimulus and a response. Even after Skinner (e.g., 1953, p. 108) returned again to a formulation of a three-term relation that explicitly included consequences, Skinner (1956) produced what amounted to another reflexological framework with an S-O-R diagram that showed “Hereditary and Environmental Conditions and Events” as “independent variables” or stimuli; an encircled “Organism”; and “Behavior” as the “Dependent Variable” (*Cumulative Record*, 1972, p. 261). The adaptation in Figure 1 illustrates this diagram and its conformity to an S-O-R model. There is no identification of the consequences of behavior in this diagram. Skinner (1956) described his illustration as follows:

Our organism emits the behavior we are to account for, as our dependent variable, at the right.

To explain this, we appeal to certain external, generally observable, and possible controllable hereditary and environmental conditions, as indicated at the left. These are the independent variables of which behavior is to be expressed as a function. Both input and output of such a system may be treated with the accepted dimensional systems of physics and biology. A complete set of such relations would permit us to predict and, insofar as the independent variables are under our control, to modify or generate behavior at will. (*Cumulative Record*, 1972, p. 261)

Except perhaps for the term *emitted*, it is hard to see that Skinner has abandoned a two-term S-R orientation. Skinner (1966b) would later describe such an account as an S-R formulation: “For S-R psychologists, response stands for output and stimulus for input, and when the one cannot be accounted for in terms of the other, mediating activities are again invented” (p. xi). An S-O-R formulation readily allows for such inventions within the O (organism). This statement by Skinner in 1966 amounts to placing his 1956 statements with S-R psychology.

Looking back, Skinner (1977/1978) said that he should have abandoned the term *reflex* when he used the term *operant* for the first time in his 1937 paper:

In my reply I used the term “operant” for the first time and applied “respondent” to the Pavlovian case. It would have been the right time to abandon “reflex,” but I was still strongly under the control of Sherrington, Magnus, and Pavlov, and I continued to hold to the term doggedly when I wrote *The Behavior of Organisms* (1938). It took me several years to break free of my own stimulus control in the field of operant behavior. From this point on, however, I was

clearly no longer a stimulus-response psychologist. (*Cumulative Record*, 1972, pp. 119-120)

Later, Skinner (1984/1988, p. 460) said he had not been a stimulus-response psychologist for more than 50 years. Skinner's (1956) statement and diagram, however, suggest that his abandonment of S-R psychology was not as confirmed in 1956 as it was in retrospect. Skinner indicated that the 1930s would have been an appropriate time to abandon S-R formulations for operant behavior, yet it took him several more years to complete this abandonment, with a pronounced return to a two-term formulation for behavior in 1956 that is like an extinction blip.

Skinner continued to make modifications and clarifications that more emphatically distinguished his three-term contingency for operant behavior from the two-term necessity of the reflex (Moxley, 1992). However, Skinner did not produce diagrams that kept up with these changes. Even when his later diagrams showed a three-term contingency, Skinner (e.g., 1957, pp. 57, 84-85) showed the relationships among these terms as specific instances in a linear sequence of specific instances rather than in a cyclical arrangement of class concepts as his general formulation indicated. Indeed, Skinner never formulated a diagram as clearly as he might have for his fundamental three-term contingency (see Moxley, 1982, 1984, 1987). Given the labored way that Skinner worked through a reflexological framework to his three-term contingency, with a late S-R account in 1956, it should not be surprising that some readers would inadequately distinguish Skinner's work from S-R psychology despite Skinner's (e.g., 1986, p. 716) protests.

FROM DISCRIMINATIVE STIMULUS TO SETTING

In an early work, Skinner (1932c) referred to the "*set* [*italics added*] of elements" that comprise "conditioned stimuli" (p. 283), and he later noted (1966/1968b) a limitation in using the

term *stimulus*: "A stimulus never exercises complete control. It is effective only as part of a *set* [*italics added*] of conditions, which build up to the point at which a response is emitted" (p. 137). This would seem to have been an appropriate time to adopt *setting* (for the *set* of conditions) as the first term in his three-term contingency, but that would also take some years.

Instead, Skinner's (1938, p. 178) first formulation of his three-term contingency used *discriminative stimulus* as its first term. By limiting the antecedent conditions of his three-term contingency to a discriminative stimulus, Skinner (1966/1988) needed to add an awkward qualification: "The simplest contingencies involve at least three terms—stimulus, response, and reinforcer—and at least one other variable (the deprivation associated with the reinforcer) is implied" (p. 356). Why the implied variable? Is the three-term contingency really a four-term contingency? Skinner appears to be caught between wanting to address more than three variables and retaining the three-term contingency formulation. In an extended passage that distinguished his three-term contingency from S-R formulations, Skinner (1968/1969) used *occasion* instead of *discriminative stimulus*:

By using rate of responding as a dependent variable, it has been possible to formulate the interaction between an organism and its environment more adequately. The kinds of consequences which increase the rate ("reinforcers") are positive or negative, depending upon whether they reinforce, when they appear or when they disappear. The class of responses upon which a reinforcer is contingent is called an operant, to suggest the action on the environment followed by reinforcement. We construct an operant by making a reinforcer contingent on a response, but the important fact about the resulting unit is not its topography but its probability of occurrence, observed as rate of emission. Prior stimuli are not irrelevant. Any stimulus present when an operant is reinforced acquires control in the sense that the rate will be higher when it is present. Such a stimulus does not act as a goad; it does not elicit the response in the sense of forcing it to occur. It is simply an essential aspect of the occasion upon which a response is made

and reinforced. The difference is made clear by calling it a discriminative stimulus (or S^D).

An adequate formulation of the interaction between an organism and its environment must always specify three things (1) the occasion upon which a response occurs, (2) the response itself and (3) the reinforcing consequences. The interrelationships among them are the "contingencies of reinforcement." (p. 7; also cf. Skinner, 1971b, *Cumulative Record*, 1972, p. 423)

The term *occasion* allowed Skinner to address more than one variable within the occasion. However, Skinner did not mark or note any difference in meaning in moving from discriminative stimulus to occasion in the above passage, as Watson (1924/1990) did in contrasting *stimulus* and *situation*: "We can throw all of our psychological problems and their solutions into terms of stimulus and response. Let us use the abbreviations S for *stimulus* (or the more complex *situation*) and R for *response*" (p. 22). Nor did Skinner clearly indicate dissatisfaction with using discriminative stimulus in his three-term contingency until several years later after trying out a variety of terms in its place. Eventually, Skinner (1984/1988) explained his dissatisfaction with the term *discrimination*:

In reporting my original work on discrimination (*The Behavior of Organisms*, 1938) I used the word "discrimination" mainly because of current interest in the subject in psychology. The choice was unfortunate. The issue is not discriminability but how stimuli acquire control of behavior from their role in contingencies of reinforcement. (p. 471)

Skinner (1986/1987) restated this view:

The term discrimination which I took from contemporary work on animal behavior, was not quite right. I did not really care whether a rat could tell the difference between light-on and light-off. The control acquired by a stimulus was the issue. I began to speak of a stimulus as "the occasion" for a response or even of responding "in" a stimulus as short for "in the presence of," but neither expression quite represents the way in which a stimulus gains control of an operant. (p. 201)

Instead of presenting these explanations at the time he began to explore more inclusive terms, Skinner presented them after trying out different terms

that replaced discriminative stimulus in his three-term relation.

After exploring terms such as *occasion* (e.g., 1968/1969, p. 7, 1973, p. 257), *situation* (e.g., 1971b, *Cumulative Record*, 1972, p. 424), and *circumstances* (e.g., 1974, pp. 90–92), Skinner increased the frequency in which he used *setting* (e.g., 1973, pp. 257–258, 1984/1988, pp. 215, 265, 1989a, p. 126, 1989c, p. 13, 1989d, p. 10, 1989e, pp. 62–63; also compare its use by Skinner in his interview with Segal, 1988) as the first term in his three-term contingency. For example, "So-called contingencies of reinforcement represent subtle and complex relations among three things: the *setting* [*italics added*] or occasion upon which behavior occurs, the behavior itself, and its reinforcing consequences" (1973, pp. 257–258); "The selective action of operant conditioning establishes a controlling relation among *three* [*italics in original*] things—stimuli (the *setting* [*italics added*]), behavior (in this case verbal), and the reinforcing consequences (in this case, arranged by a verbal community)" (1984/1988, p. 215); "Once one has specified the *setting* [*italics added*] (the lever), a rough specification of topography (pressing), and a consequence (e.g., receipt of food if the organism is hungry), the topography will be more and more sharply determined by its effect in producing a consequence" (1984/1988, p. 265); and "My first arrangement of *setting* [*italics added*], response, and consequence was quite simple" (1989e, p. 62). One advantage of the term *setting* is its suggestion of comprehensive inclusion for the "set of conditions" in Skinner's earlier phrasing. In this inclusive sense, Skinner's (e.g., 1981a, p. 6) use of *setting* may refer to the current setting (1985/1987, p. 107) or to more than the current setting (1974, pp. 53, 90, 1985/1987, p. 107, 1989e, p. 63) (i.e., genetic, cultural, and personal histories). However, Skinner did not detail the categories for subclasses that might be contained within the setting.

As an embracing conceptual term, the *setting* may be, and has been, unpacked in different ways: as containing discriminative stimuli, but now as a subset of the setting; as containing establishing operations (Michael, 1993); as having formal and informal sources of stimulus control (Sloane, Endo, & Della-Piana, 1980, pp. 12–14); and as stimulus equivalence relations (Sidman, 1986). In whatever way it is unpacked, the setting suggests a wider range of component members than a discriminative stimulus does and makes a more conspicuous allowance for multiple sources of control including those for complex creative behavior (cf. Sloane et al., 1980). Note that using *setting* in place of *discriminative stimulus* in the three-term contingency does not mean abandoning or discarding the discriminative stimulus. It simply suggests that Skinner wanted to clarify the way that the discriminative stimulus should be viewed in relation to the three-term contingency. The discriminative stimulus remains a useful concept, but *setting* helps to clarify that more was involved than what a *discriminative stimulus* implied when it was used as one of the three key terms in Skinner's three-term contingency.

Using a term like *setting* (which embraces the entire *set* of relevant conditions) as a component of his three-term contingency also allows relevant private events to be brought more easily into consideration. Skinner (1963) did not so much object to the use of private or mental events in explanations as he objected to the way they were used: "No entity or process which has any useful explanatory force is to be rejected on the ground that it is subjective or mental. The data which have made it important must, however, be studied and formulated in effective ways" (p. 958). What Skinner opposed was the consideration of private events as origins that are sufficient explanations: "No account of what is happening inside the human body, no matter how complete, will explain the origins of human behavior. What happens in-

side the body is not a beginning" (Skinner, 1989c, p. 24). This did not mean that private events cannot control behavior:

In *About Behaviorism* I say that the states of the body which are felt are not the causes of behavior but the collateral products of the causes. This does not mean that private event cannot control behavior—as they do when we describe them (even if necessarily inaccurately). (Skinner, 1980, p. 227)

Private events may be causes, but they are not initiating causes:

As I indicate in "Terms," private events can be brought under the control of (especially public) behavior. In that case they may be called *causes* [italics added], but not initiating causes. . . . We do engage in productive private verbal behavior in which some initiation certainly occurs, if that term means anything, but if my analysis is correct, public versions must have been established first. In that case, the initiation passes to the environment. (Skinner, 1984/1988, pp. 486–487)

Although a qualified causality may be attributed to private events, these events are not ultimately originating causes of behavior that can be divorced from previous histories (cf. Skinner, 1989c, p. 24, 1974, p. 17).

Private events became an important consideration for Skinner and distinguished his views from earlier versions of behavior. In addition to advantages in speed and in preparation for overt action (1974, p. 112), covert verbal behavior has advantages in being safe from external commitments and perhaps punishments (1957, p. 436). These advantages are considerable. In particular, it is difficult to overestimate the value of thought in preparation for overt action. Whether public or private, thinking is behaving for Skinner (e.g., 1945, *Cumulative Record*, 1972, p. 374; 1957, pp. 449–452; 1974, pp. 104, 117; 1977/1978, p. 125; 1984/1988, p. 485; 1989b, p. 46). For example, "The simplest and most satisfactory view is that thought is simply behavior—verbal or nonverbal, covert or overt" (1957, p. 449), and this behavior was operant: "Tradition and experience seem to agree in confining the analysis of human thought to operant behavior" (1957, p. 449). However, the

terms *thinking* or *thought* were most likely to be applied to covert behavior (1974, p. 112). As to the origin of that covert behavior, "Any behavior may recede to the private or covert level so long as the contingencies of reinforcement are maintained, and they are so maintained when reinforcement is either automatic or derived from the effectiveness of subsequent overt behavior" (1966/1968b, p. 124). Covert thinking depends on previous and subsequent overt behavior.

Emotions and feelings are other private events. In considering emotions as unconditioned behavior, Skinner (1953) said, "Some of the behavior involved in an emotion is apparently unconditioned [and] must be explained in terms of evolutionary consequences" (p. 163). Emotional stimuli also alter the probability of responding:

Emotional stimuli not only elicit responses, they establish dispositions to behave that comprise a more practical part of the field of emotion. The result changes the probability the organism will behave in a given way, and this change may or may not be accompanied by the glandular and smooth muscle responses classically regarded as the emotion. (Skinner, 1957, p. 158)

Emotional stimuli that change the probability of responding play a causal role. However, the role of emotions in operant conditioning is more clearly seen when the signs of emotional effects from a listener are reinforcing to a speaker: "Insofar as the speaker has been conditioned by the emotional effects he has achieved, we may point to a functional connection between his behavior and the emotional behavior of the listener" (1957, p. 157). Of the conditions that cause an organism to be emotional, "Many are clearly related to reinforcement and to appropriate states of deprivation and aversive stimulation" (1957, p. 214).

Although the terms *emotion* and *feeling* may overlap in meaning, emotion may refer to a greater intensity than feeling (e.g., we are more likely to say that someone was "overcome by emotion" rather than "overcome by feeling"), and Skinner discussed feel-

ings separately. For Skinner (1989d), "How people feel is often as important as what they do" (p. 3). One reason why feelings are important is that they are indicators: "There are good reasons why people talk about their feelings. What they say is often a useful indication of what has happened to them or of what they may do" (1989d, p. 11). In addition, reports of what is felt and introspectively observed "are clues (1) to past behavior and the conditions affecting it, (2) to current behavior and the conditions affecting it, and (3) to conditions related to future behavior" (1974, p. 31).

If private events are to be considered as behavior, and if these private events may also be considered as causes—but not originating causes—then the term *setting*, as opposed to *discriminative stimulus*, makes it easier for the private events of thoughts, feelings, and emotions to be brought into play in behavioral accounts. The term *setting* also makes it easier for any feature of genetic, personal, and cultural history to be brought into play when the setting is considered as extending beyond the current setting. Indeed, when the term *setting* is considered to embrace the set of conditions that Skinner identified as relevant for operant behavior, it is difficult to see any contextual consideration that was not or could not be addressed within Skinner's analysis.

By using a highly inclusive term such as *setting* as the first term, Skinner also brought the three-term contingency of *setting*, *behavior*, and *consequences* into closer alignment with Darwin's (e.g., 1859/1958) frequently repeated three-term contingency of *conditions of life*, *variation*, and *selection*: "Natural Selection [*italics added*], or the Survival of the Fittest . . . implies only the preservations of such *variations* as arise and are beneficial to the being under its *conditions of life* [*italics added*]" (p. 88). Skinner (e.g., 1981b) repeatedly emphasized the parallel between operant behavior and natural selection, and the term *setting* makes this parallel more salient than

the term *discriminative stimulus* does. It is difficult to see how *discriminative stimulus* could be regarded as even roughly equivalent to *conditions of life*, but it is not hard to see how the *setting* could.

Skinner's adoption of the term *setting* clearly undermines criticisms that his analysis ignored contexts (e.g., Brown, 1994, p. 4). In addition, Skinner's insistence on a minimum of three terms for operant contingencies, which permits contextual considerations, undermines claims that Skinner's operant can be represented by a decontextualized two-term R-S contingency consisting of a single response–stimulus or response–consequence relation (cf. Moxley, 1996). Any reader who interprets Skinner as simply using a term in the vernacular (*setting*) as a casual synonym for a technical term (*discriminative stimulus*) without any further implications has misunderstood the function of that term in Skinner's analysis.

The role that the *setting* serves in his three-term contingency might have been clearer, however, if Skinner had given some background in the literature for the use of that term. Skinner might have identified a connection for *setting* in its use in Kantor's (e.g., 1959, p. 174) interbehaviorism, its use in ecological psychology (e.g., Gump, 1975; Rogers-Warren & Warren, 1977), its use in the behavior analysis of child development (e.g., Bijou & Baer, 1978, pp. 26–29), or its use in applied behavior analysis (e.g., Wahler & Fox, 1981); and Skinner might have provided similarities and differences between his use of the term and other uses of the term. Even in respect to his own work, Skinner did not relate the "setting" for his three-term contingency to his earlier use of "set" in the "set of elements" that comprise "conditioned stimuli" (1932c, p. 283) and the "set of conditions, which build up to the point at which a response is emitted" (1966/1968b, p. 137). As a result, Hilgard's (1939, p. 124) comment about Skinner's failure to provide more help

to the reader also seems applicable here.

FROM DETERMINISM TO RANDOM VARIATION

As characterized by Gould (1997), Laplace was "the archetypal defender of a certain view of science" that embraced "stability in the heavens" and "determinism of all events under the aegis of natural laws with clean, mathematical formulation—an almost anti-historical view" (p. 29). Laplace's view was emphatically pre-Darwinian in time and assumptions: According to Laplace (cited in Gould, 1997), "It seems that nature arranged all bodies in the heavens in order to assure the duration of the system, and by means similar to those so admirably followed on earth for the conservation of individuals and the perpetuation of species" (p. 33). Laplace's view had no place for evolutionary change and development.

In addition to arguing for stability in the heavens and the perpetuation of species, Laplace (1814/1951) gave the classic formulation for scientific determinism:

We ought . . . to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it—an intelligence sufficiently vast to submit these data to analysis—it would embrace in the same formula the movement of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. (p. 4)

In his detailed discussion of scientific or Laplacean determinism—the central variety of determinism considered here—Popper (1982) found that this determinism "was held by physicists, practically without exception, until 1927, and by Einstein, it seems, almost until his death in 1955" (p. 2). In Popper's analysis of scientific determinism, "If every event is to be *predictable*, it *must be predictable with any desired degree of precision*: For even the most

minute difference in measurement may be claimed to distinguish between *different events*" (p. 6). The full comprehension of this determinism would be the comprehension of an absolutely fixed, unchanging universe. On this account, the changes we observe in our world are an illusion, as when we see a frame-by-frame projection of a movie film. The reality is fixed and unchanging, as it would appear to a supernatural being who could see the unrolled film at a glance (cf. Popper, 1982, p. 5, also cf. pp. xx–6; also cf. Miles, 1995, p. 365). However, this underlying reality has never been observed by mere mortals. In historical context, it may be regarded as one of the variants of a Platonic reality, an unseen underlying formal structure, more unified than what Plato offered but equally elusive.

Laplace's definition of determinism has continued as the central source for the meaning of scientific determinism (cf. Audi, 1995), and the following assumes that Skinner held a conception of determinism, at least for a time, that was like that of Laplace. The Laplacean conception represented the standard received view of scientific determinism at the start of Skinner's career. If Skinner wanted his determinism (e.g., of 1947) to be seen as different from Laplace's, he never bothered to distinguish his determinism from an interpretation that many readers would assume in the absence of any qualifying distinction. In addition, there is no evidence that Skinner's central concept of determinism ever changed. What did change were Skinner's views on various aspects of determinism—on the relation of determinism to other concepts (e.g., in respect to whether or not the realization of determinism in the world at large could be demonstrated and whether or not determinism must be assumed as an essential foundational principle for a science of behavior). In some passages, it is difficult to decide which aspects Skinner may have been assuming. At other times, he

(e.g., 1947) gave some fairly definitive statements about them.

Skinner (1931) showed an early acceptance of determinism in saying that "the reflex is important in the description of behavior because it is by definition a statement of the *necessity* [italics in original] of this relation" (between behavior and its stimulus) (*Cumulative Record*, 1972, p. 449). Skinner was echoing one of the assumptions of the deterministic (and mechanistic) tradition in physiology. The physiologist Dubois-Reymond (1874), for example, had paraphrased Laplace approvingly; and Pavlov (1927/1960) had said, "Our starting point has been Descartes' idea of the nervous reflex. This is a genuine scientific conception, since it implies necessity" (p. 7). The assumption of necessary relations was not offered as an empirical induction from observation; it was offered as an assumption of science.

Initially, Skinner appeared to believe the discovery of necessity could be accomplished in formal detail. In addition to his early affirmation of the importance of the necessity of the reflex, Skinner presented mathematical formulas such as " $R = f(S, A)$ " (1931, *Cumulative Record*, 1972, p. 454); " $N = Kr^t$ " (1932b, p. 47); or " $N = K \log t + C + ct$ " (1933, p. 341). Later, Skinner (1938, pp. 11, 433) expressed some doubt as to whether the exactly detailed implications of determinism would ever be empirically observed. In retrospect, however, Skinner (1977) indicated he nevertheless thought at that time (1938) that "a completely predictive science of behavior . . . might be possible in the laboratory" (p. 1008).

In 1947, Skinner presented relatively definitive statements on what he meant by determinism, saying it was essential to assume "that [behavior] is undisturbed by the capricious act of any free agent—in other words, that it is completely determined" (p. 23). He also assumed a thorough reductionism would be forthcoming: "Eventually, we may assume, the facts and principles of psychology will be reducible

not only to physiology but through biology and chemistry to physics and subatomic physics" (p. 31), and he looked forward to a time when theory would account for "the behavior of an individual in such a way that measurement would be feasible if he were the only individual on earth. This would be done by determining the values of certain constants in equations describing his behavior" (p. 39). This position is closely in line with the classical determinism of an exactly detailing, Laplacean formula for every event in the universe. Skinner's claims for *complete determinism* implied absolute predictability, as in Laplace's formulation. In addition, Skinner's claim that this predictability would be done with equations suggests that the past could also be predicted (or retrodicted) from the present, as in Laplace's world formula.

How probability fits in here Skinner did not say, but elsewhere (e.g., 1932a, p. 32, 1953, p. 112) he suggested that probability simply reflected an incomplete account of all the relevant variables. For example, Skinner (1953) said, "The discriminative stimulus . . . shares its control with other variables, so that the inevitability of its effect cannot be easily demonstrated" (p. 112). But this acknowledged weakness in the empirical evidence did not keep Skinner from maintaining necessity was really there: "But when all relevant variables have been taken into account, it is not difficult to guarantee the result—to force the discriminative operant as inexorably as the eliciting stimulus forces its response" (p. 112). Skinner provided no evidence, however, to show that this *inexorable forcing* of the discriminative operant had been empirically demonstrated. These statements, of 1947 and 1953, may be regarded as the high tide of Skinner's explicit advocacy of determinism. Later (1961, *Cumulative Record*, 1972, p. 314) he admitted that the predictions he had made in 1947 were not as close to realization as he had indicated.

Skinner continued to make deterministic assertions in *Beyond Freedom*

and *Dignity* (1971a), saying, "Personal exemption from a complete determinism is revoked as a scientific analysis progresses" (p. 18). The critical reaction to this publication was remarkably widespread, intense, and often vitriolic. This publication became a turning point. Afterwards, Skinner continued to advance determinism for a time, but with judicious qualifications. Control was not presented as an inexorable forcing of the discriminative operant. Instead, Skinner (1973) presented control as a more modest change in probability:

Human behavior is controlled not by physical manipulation but by changing the environmental conditions of which it is a function. The control is probabilistic. The organism is not forced to behave in a given way; it is simply made more likely to do so. (p. 259)

There is no inexorable force here. Control is not deterministic but probabilistic. Behavior is simply made "more likely." In addition, Skinner presented determinism as an issue of plausibility rather than as an essentially necessary assumption: "We cannot prove, of course, that human behavior as a whole is fully determined, but the proposition becomes more plausible as facts accumulate" (Skinner, 1974, p. 189). However, no accumulation of facts with probabilistic contingencies can justify the necessary relations of determinism. Determinism cannot be an induction from probabilistic instances. At best, determinism is an abduction, an inference to the best hypothetical explanation that someone finds for explaining diverse facts. Skinner (1984/1988) also indicated that a mechanical model might be appropriate when "selection as a causal mode has done its work and a mechanical model may suffice" (p. 25). The selectionist model has temporal priority, and it is difficult to see how the antecedent determinism implied by a mechanistic model would function here as a prerequisite guide for scientific investigation. From the confident statements of 1947 and 1953, determinism is now qualified with

"cannot prove," "more plausible," and "may."

Increasing strains had appeared in Skinner's acceptance of determinism even before the publication of *Beyond Freedom and Dignity* (1971a). These strains showed up in the way that he stretched his use of determinism to a broader coverage at odds with its more commonly used sense. For example, in discussing the creative artist, Skinner (1970) presented determinism as an assumption that he illustrated in a way that contradicts the classical use of determinism:

Novelty or originality can occur in a wholly deterministic system. A convenient archetypal pattern is the theory of evolution. The living forms on the earth show a variety far beyond that of works of art. . . . The multiplicity of living forms is accounted for in terms of mutation and selection, *without appealing to any prior design* [italics added]. . . . We may not like to credit any aspect of a successful painting to chance, but, if we are willing to admit that chance does make a contribution, we can take steps to improve the chances. (*Cumulative Record*, 1972, pp. 339–340)

Classical determinism, however, *does* appeal to design in a Laplacean formula, and this design is commonly presumed to have an existence that awaits discovery. Repudiation of a prior design is a repudiation of what many have understood as determinism. In addition, Skinner gives chance a role that is at odds with the traditional view of determinism that regarded a universe of chaotic chance as an abhorrent forced-choice alternative to an orderly deterministic universe (cf. Gigerenzer et al., 1989; Hacking, 1975, 1990; Schweber, 1977). However, a reasonable case can be made that the alternative to determinism is a probabilistic universe in which an increase in orderly probability is a common experience, although some element of chance always remains. Previously, Skinner (1947, p. 53, 1966/1968a, p. 171) had presented "capricious" action as the unacceptable alternative to "determinism." Now he sees that chance makes a contribution.

The strains on Skinner's determinism continued to appear when he ac-

knowledgeed that a role for chance appeared to be at odds with a deterministic system and he passed up the opportunity to defend or reassert his acceptance of determinism:

As accidental traits, arising from mutation, are selected by their contributions to survival, so accidental variations in behavior are selected by their reinforcing consequences.

That chance can play a part in the production of anything as important as mathematics, science, or art has often been questioned. Moreover, at first glance, there seems to be no room for chance in any completely determined system. . . . Yet the biographies of writer, composers, artists, scientists, mathematicians, and inventors all reveal the importance of happy accidents in the production of original behavior.

The concept of selection is again the key. The mutations in genetic and evolutionary theory are random, and the topographies of response selected by reinforcement are, if not random, at least not necessarily related to the contingencies under which they will be selected. (1974, p. 114)

Instead of attempting to reconcile the apparent conflict that he identified between chance and determinism, Skinner went on to stress the role of accidental or random variation in human behavior in analogy with natural selection.

This failure to defend determinism takes on further significance in Skinner's autobiography, in which he passed up another opportunity to defend or reassert a maintained belief in determinism. Skinner (1983/1984, p. 60) commented on being "caught" by Bridgman in regard to his (Skinner's) 1953 assumption of determinism: Previously, Skinner (1953) had defended determinism against the objection "that physical science has been unable to maintain its philosophy of determinism particularly at the subatomic level" (p. 17). Skinner argued, in part, "The Principle of Indeterminacy states that there are circumstances under which the physicist cannot put himself in possession of all relevant information" (p. 17). In saying this, Skinner implied that the relevant information existed somewhere, and Bridgman criticized Skinner for this claim. As Skinner (1983/1984) presents Bridgman's response,

When [Bridgman] saw the manuscript of *Science and Human Behavior*, he caught me up on two subtle points. He wrote, "I think it would be better in discussing the principle of indeterminacy to say that relevant information does not exist than to say we cannot put ourselves in possession of it. And I would not like to say, as seems implied, that science has to assume that the universe is lawful and determined, but rather that science proceeds by exploiting those lawfulnesses that it can discover. Anything smacking of faith I think we can get along without." (p. 60)

The way that Skinner recounts this episode suggests he was aware (at least in 1983) of difficulties in assuming determinism within a functional, descriptive, and pragmatic view of science; and he did not offer a defense against Bridgman's objection.

In his final years, Skinner clearly presented random variation rather than determinism as a source or origin for his explanatory accounts. Skinner (Trudeau, 1990) insisted, "The origin of human behavior, like the origin of species, has got to be interpreted in terms of randomness and accident" (p. 2). In discussing the three types of variation and selection in natural selection, operant conditioning, and cultural evolution, Skinner (1990a) pointed out that "variations are random and contingencies of selection accidental" (p. 1207) and that "if there is freedom, it is to be found in the randomness of variations" (p. 1208). Skinner (1990b) also pointed out how no verbal formula could be an absolute prediction of the future: "Too much of what will happen depends upon unforeseen variations and adventitious contingencies of selection. The future is largely a matter of chance" (p. 197). Appeals to determinism are now irrelevant to the explanatory accounts Skinner is advancing.

The irrelevance of determinism for Skinner's selectionist account of behavior can perhaps be seen more clearly in Skinner's role for verbal behavior within his three-term contingency. This role is quite different from the role of verbal behavior in accounts in which idealized verbal behavior in geometri-

cal, logical, or mathematical form is presented as the foundation for experience. For Skinner (1984/1988), the "verbal descriptions of reality are never as detailed as reality itself, and rule-guided or -governed behavior is never as subtle as behavior shaped directly by the contingencies described by the rules" (p. 325). Probabilistic contingencies are foundational for Skinner, not verbal rules. Skinner (1966a) clarified some of these points with an example of the development of a path that need not be deliberately planned:

A path need not be constructed because it serves this function, but the advantages gained may reinforce the explicit leaving of traces. A trail is "blazed," for example, precisely because it is more easily followed. Comparable reinforcing advantages have led men to construct pictures and verbal description of paths. (p. 28; also cf. 1966/1969, pp. 137-144)

We may also derive more streamlined descriptions or rules from previous descriptions without making a direct exploration of the situation described. Although we may explain our behavior by a rule we are following, the source for that rule lies in previous probabilistic contingencies such as those for constructing paths. A more complete explanation indicates those origins.

This movement from a general responsiveness to contingencies to a more focused responsiveness to rules is a common experience. We respond, often creatively, to multiple, diverse, informal contingencies in new and unfamiliar settings. Later, after a series of selections by consequences, our responses may be described with a more formal relation to particular discriminative stimuli and rules. The new formal relations did not exist at the start of the operant cycles. The new relations are outcomes of the three-term contingency process. These outcomes accumulate in personal histories, and some are passed on by cultures.

Thus, verbal behavior that expresses exact necessary relations cannot be the origin for any empirical event. Probabilistic contingencies underlie rules that express necessary relations. Rules

do not underlie contingencies. This view extends to logic, and if it "invalidates our scientific structure from the point of view of logic and truth-value, then so much the worse for logic, which will also have been embraced by our analysis" (Skinner, 1945, *Cumulative Record*, 1972, p. 380). No logical rule or world formula can come first with any guarantee of empirical certainty, because, in the world we experience, "The contingencies always come first" (Skinner, 1989b, p. 44). Accordingly, scientific laws "are not, of course, obeyed by nature but by men who deal effectively with nature" (Skinner, 1966/1969, p. 141). This verbal or rule-governed human behavior, like all such behavior, is derived from probabilistic contingencies, and it is impossible for human scientific laws to be more exactly certain than probabilistic relations allow. This does not prevent such laws from supporting highly successful actions, and accepting probabilistic relations does not prevent acting effectively with practical certainty (i.e., without doubt).

Skinner may have maintained some belief in a metaphysical determinism, just as some Darwinian advocates maintained a belief in a metaphysical supernatural being. Whether Skinner maintained some belief in determinism or not, determinism no longer played its previous role, just as a belief in a supernatural being no longer played its argument-from-design role among the scientists who accepted Darwin's natural selection. In fact, determinism may be regarded as a variation on the argument from design with a reduced, if any, role for a designer—retaining an imagined causal agency in the world formula rather than in the designer. In his later selectionist accounts of behavior, determinism had become irrelevant to Skinner's scientific explanations if not to his personal beliefs. Although Skinner never explicitly rejected determinism, he had marginalized it to the point at which it was irrelevant to his selectionism.

Inasmuch as determinism was an in-

herent assumption of classical mechanistic philosophy (cf. Dewey, 1916/1985, p. 293; Peirce, 1931–1963, 6.36–6.38), it is a short if reckless step for a reader to infer that Skinner favored a mechanistic S-R psychology if the reader assumed that determinism remained foundational to Skinner's behavior analysis. This interpretation would be less likely if Skinner had explicitly rejected determinism. As it is, the reader must attend to the weakening of Skinner's views over time in support of determinism and to its eventual replacement with random variation. This would then confirm Skinner's movement away from a mechanistic account and to a selectionist one. Further background on the issues involved in Skinner's shift from determinism to random variation can be found in Moxley (1997).

CONCLUSION

To recapitulate, Skinner worked through a reflexological paradigm that was aligned with the mechanistic framework of two-term S-R psychology and classical physics to arrive at a three-term contingency for operant behavior within a selectionist paradigm. That three-term contingency bore a striking resemblance to Thorndike's law of effect as adjusted by Russell, which was seen by both Thorndike and Russell in analogy with Darwin's natural selection. In respect to the resemblance between operant behavior and the law of effect, Skinner subsequently acknowledged the influence of Thorndike but not of Russell, whose view he badly misrepresented on a later rereading. Upon reaching his three-term contingency, Skinner then proceeded to dismantle his S-R framework. One important step was to clarify the range of application of his three-term contingency so that it would be easier to see that it embraced more than a two-part S-R connection to a consequence. Skinner eventually did this by replacing the term *discriminative stimulus* with the term *setting* within his three-

term contingency. Skinner finally completed the dismantling of his original S-R apparatus by rendering determinism irrelevant, not explicitly, but by omission and by replacing determinism with random variation as an originating source. The result was a three-term contingency of probabilistic relations that was solidly placed within a selectionist paradigm. Given the gradual evolution of Skinner's verbal formulations and the often weak markings for significant changes in the way he revised his views, it is not surprising that some who have read samples of Skinner's writings, particularly from his early work, may have come to interpretations that are at odds with the development of his work over time. It may be regretted that Skinner did not provide more conspicuous marks for these changes. However, the fact that Skinner revised and reintegrated the formulation of his views over time when he saw how to improve them should be regarded as a commendable feature of his work.

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