

## Some More Similarities Between Peirce and Skinner

Roy A. Moxley  
West Virginia University

C. S. Peirce is noted for pioneering a variety of views, and the case is made here for the similarities and parallels between his views and B. F. Skinner's radical behaviorism. In addition to parallels previously noted, these similarities include an advancement of experimental science, a behavioral psychology, a shift from nominalism to realism, an opposition to positivism, a selectionist account for strengthening behavior, the importance of a community of selves, a recursive approach to method, and the probabilistic nature of truth. Questions are raised as to the extent to which Skinner's radical behaviorism, as distinguished from his S-R positivism, may be seen as an extension of Peirce's pragmatism.

*Key words:* C. S. Peirce, radical behaviorism, B. F. Skinner, nominalism, positivism, pragmatism, realism, selves, truth

Writing on C. S. Peirce as an innovator in various areas, Fisch (1986) said, "The amazing range of his relevance we are only beginning to guess at" (p. 446). Although Peirce's pursuits diverged in many different directions, he anticipated key characteristics of behavioral psychology and B. F. Skinner's radical behaviorism (Cadwallader, 1974; Moxley, 2001a, 2001b; Schneider, 1997; Staddon, 2001, p. 96). In particular, a close connection between the views of Peirce and Skinner has been shown in comparing Peirce's (1878/1992b) "How to Make Our Ideas Clear" with the version of pragmatism that Skinner (1945/1972) put forth in "The Operational Analysis of Psychological Terms." Skinner's essay was a dramatic turning point in his views (Moxley, 2001b) and was reflective of broader cultural changes (Moxley, 2001a). Similarities in the two papers were pronounced in their endorsement of probability, their treatment of private events, and their three-term contingency that focused on consequences. It is here that the case for a derivation of Skinner's views from the views of Peirce can be made most strongly. However, the similarities be-

tween Peirce and Skinner are not exhausted with these comparisons. There are additional similarities, some of which Skinner seems unlikely to have derived simply from Peirce. If Skinner had been aware of these similarities, however, he may have become more favorably inclined toward Peirce and read further. These additional similarities include a high regard for experimental science, an emphasis on a behavioral psychology, a shift from nominalism to realism, an opposition to positivism, accounts of how behavior is strengthened, conceptions of a community of selves, recursive methods for scientific investigation, and conceptions of truth. This listing is presented primarily as a further guide to tracing a succession of ideas shared by Peirce and Skinner, irrespective of causal relations.

### ADVOCACY OF EXPERIMENTAL SCIENCE

Peirce had a substantial background in experimental science as a source of derivation for his pragmatism. Peirce (1905/1998) said of himself that

His disposition is to think of everything just as everything is thought of in the laboratory, that is, as a question of experimentation. . . . You will find that whatever assertion you may make to him [the typical experimentalist], he will either understand as meaning that if a given prescrip-

---

Address correspondence to Roy A. Moxley, 604 Allen Hall, West Virginia University, Morgantown, West Virginia 26506.

tion for an experiment ever can be and ever is carried out in act, an experience of a given description will result, or else he will see no sense at all in what you say. . . . What adds to that confidence in this which the writer owes to his conversations with experimentalists is that he himself may almost be said to have inhabited a laboratory from the age of six until long past maturity; and having all his life associated mostly with experimentalists, it has always been with a confident sense of understanding them and of being understood by them.

That laboratory life did not prevent the writer (who here and in what follows simply exemplifies the experimentalist type) from becoming interested in methods of thinking. (p. 332)

Cadwallader (1974; also cf. Fisch, 1986, pp. 376–383) pointed out that Peirce received “a B.S. in chemistry summa cum laude in 1863—the first such degree to be awarded by Harvard” (p. 291); and Peirce was a practicing scientist:

From 1859–1860 and 1861–1891 while Peirce was in the employ of the U.S. Coast Survey as a mathematician, astronomer and physicist, he made fundamental discoveries in all of these areas and was elected to the National Academy of Sciences in 1877. (p. 291)

Scholarly assessments of Peirce have concluded, “Peirce was in the first place a scientist” (Fisch, 1986, p. 376), and “Peirce was primarily a scientist and only secondarily a philosopher” (Moore, 1993, p. 2; also cf. Eisele, 1979; Schneider, 1997, p. 17).

Although Skinner’s association with experimentalists did not begin as early as Peirce, he (1961/1972) was a dedicated advocate of experimental science in the laboratory. At times, Skinner (e.g., 1972; also cf. 1987, pp. 114–115) even credited science with more accomplishments than the evidence substantiated: “Science seems to have emerged from efforts to solve practical problems. . . . As science advances, however, the direction changes. . . . Most improvements in technology now come from what is essentially basic research” (p. 276). Science has indeed come to influence technology more than it has in the past, but it is inaccurate to imply that the direction has been reversed and that most improvements in technology now come from

basic research. It is more accurate to say that science and technology have had reciprocal influences and that science has become more like technology and technology has become more like science (Moxley, 1989).

## A BEHAVIORAL PSYCHOLOGY

Cadwallader (1974) considered Peirce to be “the first American experimental psychologist” (p. 291). According to Behrens (1993),

Peirce’s interests in the problems of psychology, such as sensory awareness, sensory discrimination, and cognition, had a decidedly empiricist perspective. . . . Early in his career Peirce became acquainted with the writings of Wilhelm Wundt and Gustav Fechner. Their work exemplified the applications of mathematics and laboratory methodology to psychology. By 1868 Peirce referred to the “facts of physiopsychology” and later he called for psychology to follow the lead of Wundt and Fechner (Cadwallader 1974). He became well versed in both Wundt’s *Physiologische Psychologie* and Fechner’s *Elemente der Psychophysik*. (p. 310)

In 1884, Peirce collaborated with Joseph Jastrow, a 1st-year graduate student (later to become the ninth president of the American Psychological Association), to publish “On Small Differences in Sensation,” in the *Memoirs of the National Academy of Sciences*, which is considered to be the first American experimental study in psychology (Behrens, 1993).

Peirce may also be considered as the first American behaviorist. Cadwallader (1974) said, “In 1867 Peirce was contrasting his type of psychology which he called empirical psychology, to introspective psychology, which he regarded as untrustworthy” (p. 292) and saw that Peirce had “many affinities to what later came to be called behaviorism” (p. 296). Peirce (1868/1992), for example, said, “We have no power of Introspection, but all knowledge of the internal world is derived by hypothetical reasoning from our knowledge of external facts” (p. 30). Peirce (1903/1998) at least meant that there was no independent or originating power of introspection:

The elements of every concept enter into logical thought at the gate of perception and make their exit at the gate of purposive action; and whatever cannot show its passports at both those two gates is to be arrested as unauthorized by reason. (p. 241)

The entrance is from the external environment, and the exit is action upon that environment. In a letter of 1912, Peirce (cited in Fisch, 1969) also said he had derived the term *pragmatism* “from *πραγμα*, ‘behavior’—in order that it should be understood that the doctrine is that the only real significance of a general term lies in the general behavior which it implies” (p. 418).

Skinner is well known for his behavioral views in which operants act upon the environment. However, contrary to some misrepresentations of his views, Skinner (1963) did not object to private or mental events as such: “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 positing private events as 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 inside the body is not a beginning” (Skinner, 1989, p. 24; also cf. Skinner, 1988, pp. 486–487). This did not mean that private events cannot be said to control behavior (Skinner, 1980, p. 227) or be called causes (e.g., Skinner, 1988, pp. 486–487). They were simply not originating causes. Causes in the environment had precedence. As with Peirce, the entrance is from the external environment and the exit is action on that environment.

### FROM NOMINALISM TO REALISM

As part of his detailed criticism of nominalism in his shift from nominalism to realism, Peirce (1931–1958) criticized the nominalists for inventing

metaphysical figments such as an underlying “thing in itself” (5.312, volume and paragraph) and disagreed with nominalists who held “that the facts are, in themselves, entirely disconnected, and that it is the mind alone which unites them. One stone dropping to the earth has no real connection with another stone dropping to the earth” (6.99; cf. Moore, 1961, p. 77). Instead, Peirce (1911/1998) argued for the reality of what potentially *would* be:

In that second part [of “How to Make Our Ideas Clear”], I call “truth” the predestinate opinion, by which I ought to have meant that which *would* ultimately prevail if investigation were carried sufficiently far in that particular direction. . . . I . . . talked . . . as if . . . it was at least questionable whether any Real flower was ever “born to blush unseen, and waste its sweetness on the desert air.” But beyond question, such there are, which would have been found if inquiry could have been, and had been, sufficiently pushed in the right direction, although, in fact, it was not; and of things in which we rightly but vaguely believe, the immense majority are similarly unknown; and this majority grows relatively (and not merely numerically) larger the further inquiry is pushed, and we cannot, in any sense, look forward to a state of things in which such beliefs as that any stone let fall from the hand would drop to the earth are to be replaced by such a knowledge as that every stone that has been let loose has dropped. (p. 457)

For Peirce, meaning “does precisely lie in the conceivability, quite regardless of the practicability, of such applications” (p. 457).

Peirce (1931–1958) also disagreed with nominalists, such as Ernst Mach (Eisele, 1979), who took “the first impressions of sense,” as originally given elements:

The nominalists . . . understand experience . . . as the mere first impressions of sense. These “first impressions of sense” are hypothetical creations of nominalistic metaphysics: I for one deny their existence. But anyway even if they exist, it is not in them that experience consists. By experience must be understood the entire mental product. (6.492)

That product consisted of more than “the first impressions of sense” as a starting point:

There is but one state of mind from which you can “set out,” namely, the very state of mind in which you actually find yourself at the time you

do “set out”—a state in which you are laden with an immense mass of cognition already formed, of which you cannot divest yourself if you would. (5.416; also cf. 5.597)

From such a state of mind, there were no absolutely individual elements or atoms to begin with:

The logical atom, or term not capable of logical division . . . can be realized neither in thought nor in sense. . . . A logical atom, then, like a point in space, would involve for its precise determination an endless process. We can only say, in a general way, that a term, however determinate, may be made more determinate still, but not that it can be made absolutely determinate. Such a term as “the second Philip of Macedon” is still capable of logical division—into Philip drunk and Philip sober, for example; but we call it *absolutely* individual because that which is denoted by it is in only one place at one time. It is a term not indivisible, but indivisible as long as we neglect difference of time and the differences which accompany them. . . . The absolute individual can not only not be realized in sense or thought, but cannot exist, properly speaking. For whatever lasts for any time, however short, is capable of logical division, because in that time it will undergo some change in its relations. But what does not exist for any time, however short, does not exist at all. All, therefore, that we perceive or think, or that exists, is general. (3.93 and note 1)

Disputing the nominalistic focus on particulars as real, Peirce emphasized the importance of real generals and real relations. General properties like “hardness” were real (1.27), and “there are, besides, real *vagues*, and especially real possibilities” (5.453). Vague possibilities were not simply approximations to exact events. Rather, an exact statement for an empirical event was more an approximation to the real possibility:

Get rid thoughtful Reader, of the Ockhamistic prejudice . . . that in thought, in being, and in development the indefinite is due to a degeneration from a primary state of perfect definiteness. The truth is rather on the side of the scholastic realists that the unsettled is the primal state, and that definiteness and determinateness . . . are, in the large, approximations, developmentally, epistemologically, and metaphysically. (6.348)

Peirce believed such a position distinguished his views from those of modern philosophy and its rationalist and empiricist branches: “Thus, in one

word, all modern philosophy of every sect has been nominalistic” (1.19).

Commenting on nominalist claims for definiteness, the pragmatist Rorty (1961) said, “Nominalists thought . . . that whatever was real had sharp edges (like a sense datum or an atomic fact), and that whatever did not have sharp edges could be ‘reduced’ to things that did” (p. 199). Equating what Peirce called “nominalism” with what present-day philosophers call “reductionism,” Rorty said,

For Peirce, it is the nominalist and the reductionist who succumb to belief in metaphysical figments—namely the belief that beneath all the evident fuzziness, vagueness, and generality which we encounter in language (and, therefore, in all thought) there are nonfuzzy, particular, clearly intuitable reals. (p. 209)

For Peirce, “Nonfuzzy, particular, clearly intuitable reals” do not have a prior existence.

Coleman (1984) convincingly showed that Skinner also shifted from a nominalist position evident in 1931 to a realist position evident in 1935. At first, Skinner (1931/1972) had adopted a nominalist position when he defined the reflex as an observed instance of two particular events:

The synapse . . . is a construct. *It is the conceptual expression for the conditions of correlation of a stimulus and response, where the incidental conditions imposed by a particular stimulus and a particular response have been eliminated.* . . . A reflex is defined as an observed correlation of two events, a stimulus and response. (pp. 446–448)

Skinner rejected any need to assume that a reflex existed apart from its observation:

We have been proceeding, of course, upon an unnecessary assumption, namely, that there *is* a flexion reflex which exists independently of our observations and which our observations approximate. . . . A reflex, that is to say, has no scientific meaning apart from its definition in terms of such experimental operations as we have examined. (pp. 452–453)

Skinner thereby expressed the nominalist requirement for the direct observation of particulars. Later, Skinner (1935/1972) adopted a realist position.

This is also the time (1935) that Skinner (1979/1984) claimed he was “abandoning” (p. 143) an S-R psychology. Inasmuch as Skinner continued to present his work in an S-R framework until 1945, it is perhaps more accurate to say that Skinner was abandoning a nominalist S-R psychology for a realist S-R psychology (until 1945):

By contrast, the B. F. Skinner of 1935 was not tied to particular events defined by specific, and apparently arbitrary, laboratory operations. A reflex is now to be defined as a correlation of stimulus and response *classes*: “The ‘stimulus’ and the ‘response’ entering into a given correlation are not to be identified with particular instances appearing upon some given occasion [as he had claimed in 1931] but with classes of such instances” (Skinner, 1935a, p. 57). (Coleman, 1984, p. 491)

The move from instances to classes established a realist position in which every instance did not have to be observed in order to exist.

In addition, Skinner (1957) considered knowledge as potential behavior that need not actually occur:

We may say that being told there is a fox in the neighborhood has the same effect as seeing one, just as being told that the telephone is out of order has the same effect as discovering that it is out of order in trying to use it. In both cases it is potential behavior which is called knowledge. (p. 363)

The potential behavior is behavior that would be the case, that would actually occur, in the envisioned circumstances.

Skinner’s rejection of nominalism for realism is further indicated in the notes of a student from his course in 1947. In those notes, Skinner explicitly used the term *nominalist* in rejecting that approach to verbal behavior (Hefferline, 1947, p. 53). Instead, Skinner affirmed the value of potential acts and a probabilistic conception of verbal behavior.

### OPPOSITION TO POSITIVISM

Consistent with his opposition to nominalism and his advocacy of real generals, Peirce (2.511) disagreed with

the positivist position that no hypothesis is admissible that is incapable of verification by direct observation:

[Comte] ought on [that] principle to forbid us to suppose that a fossil skeleton had ever belonged to a living ichthyosaurus. . . . The same doctrine would forbid us to believe in our memory of what happened at dinnertime today. . . . Of course with memory would have to go all opinions about everything not at this moment before our senses. You must not believe that you hear me speaking to you, but only that you hear certain sounds while you see before you a spot of black, white, and flesh color. . . . A man would have to devote years to training his mind to such habits of thought, and even then it is doubtful whether it would be possible. And what would be gained? . . . Comte, Poincaré, and Karl Pearson take what they consider to be the first impressions of sense, but which are really nothing of the sort, but are percepts that are products of psychical operation, and they separate these from all the intellectual part of our knowledge, and arbitrarily call the first *real* and the second *fictions*. (5.597)

As a more recent example, Russell (1927/1970, p. 48) said that we do not hear what a person says, we hear sounds with complicated connections to what a person says. Russell (1911/1959), who advanced views disputed by Peirce (Hawkins, 1997), claimed that “absolute, convincing certainty . . . belongs to particular experience. . . . It is our particular thoughts and feelings that have primitive certainty” and thus form “a solid basis from which to begin our pursuit of knowledge” (p. 19). In other words, a particular sense datum comes first, and collections of these basic units provide the foundation for further developments. From the nominalism of Russell’s logical atomism, it is not difficult to see the connection to “modern nominalists, the logical positivists” (Feibleman, 1971, p. 171; also cf. Carnap, 1963, pp. 50–67, for illustration and Kolakowski, 1972, on the relation between nominalism and positivism).

Presenting early sources of positivism, Abbagnano (1967) said, “The principal philosophical sources of positivism are the work of Francis Bacon, the English empiricists, and the philosophers of the Enlightenment” (p. 414); and Skinner (1979/1984, pp. 406–412)

was struck by his agreement with those sources, an agreement that was strong in his early work but only sporadically reasserted in his later work. Skinner explained this agreement by its origin in Bacon, whose views had an early and continuing influence on him. In his pre-1945 view, Skinner (1938/1966) said of his scientific method, "It is positivistic. It confines itself to description rather than explanation. Its concepts are defined in terms of immediate observations and are not given local or physiological properties" (p. 44). In addition, Skinner (1979/1984) had seen a close relation between behaviorism and logical positivism: "As far as I was concerned, there were only minor differences between behaviorism, operationism, and logical positivism" (p. 161).

However, in "The Operational Analysis of Psychological Terms," the seminal article for understanding his new views, Skinner (1945/1972) spoke against positivist positions. Skinner was interested in addressing "a wider range of phenomena than do current streamlined treatments, particularly those offered by logicians (e.g., Carnap) interested in a unified scientific vocabulary" (p. 372). Speaking of his 1945 paper, Skinner (Blanshard & Skinner, 1966–1967) said, "The physicalism of the logical positivist has never been good behaviorism, as I pointed out twenty years ago (Skinner, 1945)" (p. 325). Skinner is indicating his 1945 paper as the point for his rejection of "physicalism and logical positivism." In reference to the positivist Feigl, Skinner (1945/1972) also attacked the positivist reliance on rules or logic:

The psychologist . . . cannot . . . join the logician in defining a definition, for example, as a "rule for the use of a term" (Feigl); he must turn instead to the contingencies of reinforcement which account for the functional relation between a term, as a verbal response, and a given stimulus. (p. 380)

As verbal behavior, rules were subject to a probabilistic three-term contingen-

cy analysis. Rules did not come first, contingencies came first.

### HOW BEHAVIOR IS STRENGTHENED

Peirce (1931–1958) said, "Habits have grades of strength varying from complete dissociation to inseparable association. . . . The habit change often consists in raising or lowering the strength of a habit"; and he referred to the strengthening as "reinforcement" (5.477). In Peirce's (1878/1992a) account,

When a nerve is stimulated, if the reflex activity is not at first of the right sort to remove the source of irritation, it will change its character again and again until the cause of irritation is removed, when the activity will quickly subside. When the nerve comes to be stimulated a second time in the same way, probably some of the other movements which had been made on the first occasion will be repeated; but, however this may be, one of them must ultimately be repeated, for the activity will continue until this does happen: I mean the movement which removes the source of irritation. . . . Of those which were repeated, some will probably be repeated again, and some not; but always there remains that one which must be repeated before the activity comes to an end. The ultimate effect of this will inevitably be that a habit gets established of at once reacting in the way which removes the source of irritation; for this habit alone will be strengthened at each repetition of the experiment, while every other will tend to become weakened at an accelerated rate. . . . Thus we see how these principles not only lead to the establishment of habits, but to habits directed to definite ends, namely the removal of sources of irritation. . . . The general formula of all our desires may be taken as this: to remove a stimulus. Every man is busily working to bring to an end that state of things which now excites him to work. (pp. 265–266)

Peirce cast habit in a way that resembled what came to be called *negative reinforcement*.

Although Skinner (e.g., 1968, p. 68) had used the term *negative reinforcement* as equivalent to *punishment*, in Skinner's (e.g., 1989, p. 127) later account, if the removal (indicated by the term *negative*) of a stimulus following a behavior results in the subsequent maintenance or increase of that behavior (*reinforcement*), then negative reinforcement has occurred and the removed stimulus was aversive. Peirce's

approach may be theoretically workable and applicable to what Skinner did in the laboratory. Skinner characteristically arranged for a deprived or aversive condition for his laboratory animals before reinforcing behavior. Skinner (1953) also presented situations in which the interpretation as to whether positive or negative reinforcement is operating appears arbitrary:

Suppose we have deprived a man of permission to leave a military camp until a certain task has been performed, and suppose that upon past occasions the performance of similar tasks has been followed by the restoration of this privilege. Have we generated a state of deprivation, in which behavior which has been reinforced by the return of privileges will be strong, or have we presented an aversive condition from which the individual can escape only by performing the required task? It is possible, of course, that we have done both. (p. 175)

And Skinner (1983/1984) gave at least one example of positive reinforcement that many would see as negative reinforcement (the removal of an aversive stimulus): “Desirable behavior is positively reinforced by a release from threat (absolution, being ‘saved’)” (p. 61).

There is, however, a practical difficulty with Peirce’s “negative reinforcement” account for behavior analysis. The removal of the stimulus may not always be easily observed (whereas the increase or decrease of the behavior is). It seems excessively indirect to construe positive reinforcement as occurring when the introduction of a consequent stimulus—which removed an irritation—is followed by an increase in the behavior (habit). It seems simpler and more practical to construe positive reinforcement as occurring when the observed introduction of a consequent stimulus is followed by an increase in the behavior (habit).

### COMMUNITIES OF SELVES

For Peirce (1992), reality “in the long run” could not be determined without a community for determining it:

The real, then is that which, sooner or later, information and reasoning would finally result in, and which is therefore independent of the vagaries of me and you. Thus, the very origin of the conception of reality shows that this conception essentially involves the notion of a COMMUNITY, without definite limits, and capable of an indefinite increase of knowledge. And so those two series of cognitions—the real and the unreal—consist of those which, at a time sufficiently future, the community will always continue to reaffirm; and those which, under the same conditions, will ever after be denied. (p. 52)

In addition, not only was there a multiplicity of selves in a community, but there was a multiplicity of selves within a person:

Two things here are all-important to assure oneself of and to remember. The first is that a person is not absolutely an individual. His thoughts are what he is “saying to himself,” that is, is saying to that other self that is just coming into life in the flow of time. When one reasons, it is that critical self that one is trying to persuade; and all thought whatsoever is a sign, and is mostly of the nature of language. The second thing to remember is that the man’s circle of society (however widely or narrowly this phrase may be understood) is a sort of loosely compacted person, in some respects of higher rank than the person of an individual organism. (Peirce, 1905/1998, p. 338)

Peirce was indicating a similarity between a person as a community of multiple selves and society as a loosely compacted person of multiple selves.

Drawing on Malinowski’s appendix to *The Meaning of Meaning* by Ogden and Richards (1923/1989), Skinner (1957) emphasized the role of the community in the development of verbal behavior. Speaking of the relations between speaker and listener, Skinner (1989) said, “We have been considering a kind of super-organism, the first half of which gains when the second half acts on the world, and the second half gains when the first half makes contact with that world” (p. 45). As for the selves of this superorganism or community, Skinner (1947) proposed “a new conception of the individual as the locus of a system of variables . . . it is quite clear that more than one person, in the sense of an integrated and organized system of responses, exists

within one skin" (p. 39). Selves may vary in complexity and contrast, but multiple selves were the rule: "Complex contingencies of reinforcement create complex repertoires, and, as we have seen, different contingencies create different persons within the same skin, of which so-called multiple personalities are only an extreme manifestation" (1974, pp. 167–168). For Skinner (1986), an analysis of behavior "defines self as a behavioral repertoire that results from a particular set of contingencies of reinforcement. Most people have a great many selves in that sense" (p. 716; also cf. 1974, pp. 149–150; 1989, p. 28).

### RECURSIVE METHODS

Peirce and Skinner both developed recursive approaches to research that can be traced to Darwin. A distinguishing characteristic of Darwin's method was his readiness to formulate and reformulate hypotheses. In his autobiography, Darwin (Barlow, 1958) said he had an ubiquitous readiness to form an hypothesis: "I cannot resist forming one on every subject" (p. 141); but he was also ready to abandon and reformulate them: "I have steadily endeavored . . . to give up any hypothesis, however much beloved . . . as soon as facts are shown to be opposed to it" (p. 141); and rejection or modification was normally the case: "With the exception of the Coral Reefs, I cannot remember a single first-formed hypothesis which had not after a time to be given up or greatly modified" (p. 141). Darwin's method had its impact on some philosophers of science, such as Stanley Jevons and Charles Peirce. Both men had met and corresponded. Peirce's views are considered to be more developed (cf. Medawar, 1963/1982, p. 131).

For Peirce, after encountering some surprising or interesting fact (or facts), an abduction is made to an hypothesis (or conjecture) which would account for the facts (unlike the situation with induction, some of the facts may be

unlike one another and even in conflict). This process may be conscious and public as it was at least partially with Mendeleev's hypothesis of a periodic table for the elements (Peirce, 1903/1997, p. 283), an hypothesis with a checkered history that became increasingly confirmed with further experimentations, corrections, and the discovery of new elements (cf. Petryanov & Trifonov, 1984). But the abduction process need not be conscious or entirely conscious and deliberate (cf. Peirce, 1929). With the abduction in hand, deductions can be made to consequences that would follow from the hypothesis:

That which is to be done with the hypothesis is to trace out its consequences by deduction, to compare them with results of experiment by induction, and to discard the hypothesis, and try another, as soon as the first has been refuted, as it presumably will be. How long it will be before we light upon the hypothesis which shall resist all tests we cannot tell; but we hope we shall do so, at last. (Peirce, 1901/1998, p. 109; also cf. p. 95)

In other words, the outcomes of tested deductions produce facts from which inductions may be made that affect the viability (rejecting, altering, supporting) of the original hypothesis. New facts, including the unanticipated consequences of testing the deductions, and previous facts may enter into the next cycle of abduction, deduction, and induction. Practical empirical truth, in its highest probabilistic extent, is a function of this recursive process: "The only method of ascertaining the truth is to repeat this trio of operations: conjecture; deductions of predictions from the conjecture; testing the predictions by experimentation (not necessarily what is technically so called, but essentially the same thing,—trial)" (Peirce, 1931–1958, 7.672). This gives a three-way relation of abduction, deduction, and induction among three categories of events: perceptions and facts, conjectures and hypotheses, and consequences. The inductions at the end of one cycle enter into and become



part of the perceptions and facts of the next cycle.

The reasons for selecting one hypothesis rather than another may be fairly extensive, and a convincing case for starting with a particular hypothesis may be made before collecting further data (Fann, 1970; Kapitan, 1997, p. 484). However, whether deliberated at length or not, hypotheses would be forthcoming. Peirce (cited in Brent, 1998) said, "I perform an abduction when I [do so much] as express in a sentence anything I see. The truth is that the whole fabric of our knowledge is one matted felt of pure hypothesis confirmed and refined by induction" (p. 72). In other words, Peirce regarded hypotheses as ubiquitous—on a continuum from relatively formal deliberations to informal unconscious perceptions.

For Peirce, the cyclical nature of this process gave it strength, not any single sequence: The "reasoning should not form a chain which is no stronger than its weakest link, but a cable whose fibres may be ever so slender, provided they are sufficiently numerous and intimately connected" (Peirce, 1868/1992, p. 29). Commenting on this metaphor, Bernstein (1983) said,

Peirce criticizes the picture of scientific reasoning that represents it as a linear movement from premises to conclusions or from individual "facts" to generalizations. In its place he emphasizes the multiple strands and diverse types of evidence, data, hunches, and arguments used to support a scientific hypothesis or theory. Any one of these strands may be weak in itself and insufficient to support the proposed theory, but collectively they provide a stronger warrant for rational belief than any single line of argument—like a strong cable that is made up of multiple strands. (p. 69)

Speaking of such a process, Medawar (1963/1982), who won the Nobel Prize for Medicine in 1969, said,

The regulation and control of hypotheses is more usefully described as a *cybernetic* than as a logical process: the adjustment and reformulation of hypotheses through an examination of their deductive consequences is simply another setting for the ubiquitous phenomenon of negative feedback. The purely logical element in scientific discovery is a comparatively small one,

and the idea of a *logic* of scientific discovery is acceptable only in an older and wider use of "logic" than is current among formal logicians today. (p. 135)

For Medawar, this is the "general stratagem that underlies almost all regulative processes or processes of continuous control, namely *feedback*, the control of performance by the consequences of the act performed" (p. 107). This process is cyclical and becomes increasingly convincing, but "no hypothesis in science and no scientific theory ever achieves apodictic certainty—never achieves a degree of certainty beyond the reach of criticism or the possibility of modification" (1979, p. 87).

In adopting a realist position, Skinner (1935/1972) held that any terms for the units of behavior must be class concepts, which meant that the consequences in Skinner's 1945 formulation for the three-term contingency entailed a collection of consequences, not a singular instance. Skinner accepted an indefinite extension of consequences without limits to their recursiveness in consequence classes, in shaping behavior, and in changing any behavior over time. In his ubiquitous graphs of organism performance, each point of recorded behavior offered an opportunity for one of Peirce's abductive cycles of abduction, deduction, and induction even if none were consciously made until after several data points had been recorded.

In addition, Skinner was opposed to the formal hypothetico-deductive model that employed hypotheses, deduction, and testing in a single, linear sequence that required a heavy dose of statistics to reach its conclusion. Skinner (1988, p. 115), for example, welcomed Marriott's (1988) criticism of null hypothesis significance testing, which had little recursiveness even though subsequent experiments might be recommended. Instead, Skinner (1956/1972) had presented "A Case History in Scientific Method" which—while seeking an increase in control and predictability—traced the variabil-

ity of interest over time in an individual organism without adhering to a formal preset plan: "When you run onto something interesting, drop everything else and study it" (p. 104). Skinner (1938/1966, p. 437; cf. 1988, p. 89) also claimed that formal preset hypotheses were subordinate or unnecessary in a descriptive science such as he was advancing. Skinner, of course, was not speaking of Peirce's informal hypotheses that were inherent in every fact. The idea that a one-way, one-time, two-part hypothetico-deductive model could suffice as a research study would have been as repugnant to Peirce as it was to Skinner.

### TRUTH

For Peirce and Skinner, an investigation into truth begins with an investigation into the meaning of truth, and they both shared a similar concept of meaning in terms of the consequences, as well as the contexts, for the use of the term (Moxley, 2001a, 2001b). Their close agreement is indicated by Skinner's (1979) citation of Peirce:

[Peirce's] method . . . was to consider all the effects a concept might conceivably have on practical matters. The whole of our conception of an object or event is our conception of effects. That is very close, I think, to an operant analysis of the way in which we respond to stimuli. (p. 48)

This "very close" approach "to an operant analysis" indicated a further agreement on how the meaning of anyone's concept of the term *truth* could be explicated: "When someone says he can see the meaning of a response, he means that he can infer some of the variables of which the response is usually a function" (Skinner, 1957, p. 14). The meaning of *truth* lies in the functional contingencies of the speaker's use of, or the listener's response to, the term *truth*. A detailed analysis of the meaning of *truth* would proceed like any other analysis of the contingencies for behavior: "One begins wherever possible and proceeds as soon as possible to a more and more adequate account—which, of course, will never be

complete" (Skinner, 1988, p. 380). This leaves everyone's use or understanding of the term *truth* in an incomplete or probabilistic state. *Truth* was an ascription of verbal behavior, and like all behavior it was fundamentally probabilistic: "There is no way in which a verbal description of a setting can be absolutely true" (Skinner, 1974, p. 136). Both Peirce and Skinner shared the conviction that absolutely certain, apodictic truth does not apply to our everyday lives. From there they take some different departures primarily for what they chose to emphasize about truth. Their respective approaches indicate what truth means for them without claiming any exact and enduring certainty for that meaning.

For Peirce (1903/1998), truth exists: "Every man is fully satisfied that there is such a thing as truth, or he would not ask any question. *That* truth consists in a conformity to something *independent of his thinking it to be so*, or of any man's opinion on that subject" (p. 240). However, we cannot know what it is with complete and enduring assurance, or know that we know what it is, except "in the long run." Peirce (1911/1998) said, "I call 'truth' the predestinate opinion, by which I ought to have meant that which *would* ultimately prevail if investigation were carried sufficiently far in that particular direction" (p. 457). Until that time, Peirce used the term *truth* in a probabilistic way to indicate the best approximation: "The only method of ascertaining the truth is to repeat this trio of operations: conjecture; deductions of predictions from the conjecture; testing the predictions by experimentation (not necessarily what is technically so called, but essentially the same thing,—trial)" (Peirce, 1931–1958, 7.672). Until "the long run" arrives, this method may give a better approximation and perhaps even a final determination of truth, but we can never know this with absolute certainty. In our everyday lives, empirical truth cannot be absolutely certain or exact: "In truth, positive certainty is

unattainable in man" (Peirce, 1998, p. 26, also cf. 1903/1998, p. 236).

Peirce consistently fell back on truth "in the long run": "Truth is that concordance of an abstract statement with the ideal limit towards which endless investigation would tend to bring scientific belief" (1931–1958, 5.565). Responding to those who claimed that pragmatic truth meant whatever you were currently satisfied to accept, Peirce said, "If truth consists in satisfaction, it cannot be any *actual* satisfaction, but must be the satisfaction which *would* ultimately be found if the inquiry were pushed to its ultimate and indefeasible issue" (p. 450; also cf. 1931–1958, 5.555–564). Truth "in the long run" distinguishes Peirce's view:

Foundationalist theories need not but often have conveyed the idea that foundational principles possess not only logical but temporal priority. With regard to Peirce, it is the idea of the final opinion in the infinite *future* and with a future community, that is so strikingly different. (Thayer, 1996, p. 5)

In other words, everyday truth could only be regarded at best as a successive approximation (however precise the approximation) toward whatever "the long run" brings. This does not prevent practical action on whatever is not doubted, on whatever truth is not seriously brought into question. This is mostly what people mean by what is true.

However, this does not entail that a settlement on questions of truth will eventually be forthcoming. Although one of Peirce's (1931–1958) speculations on "the long run" stated, "The world becomes an absolutely perfect, rational, and symmetrical system in which mind is at last crystallized in the infinitely distant future" (6.33), he also thought he could offer no more than a modest hope for what "the long run" might bring:

We cannot be quite sure that the community ever will settle down to an unalterable conclusion upon any given question. Even if they do so for the most part, we have no reason to think the unanimity will be quite complete, nor can we rationally presume any overwhelming *consensus*

of opinion will be reached upon every question. All that we are entitled to assume is in the form of a *hope* that such conclusion may be substantially reached concerning the particular questions with which our inquiries are busied. (6.610; also cf. 5.609)

What happens "in the long run" was an untestable speculation, and Peirce (1931–1958, 6.524) himself questioned the value of such speculations. In an exact sense, truth was inherently unsettled. In a probabilistic sense, truth might be considered settled for the time being. The only exception that Peirce (1898/1998) allowed for certain and exact truth was for pure mathematics: "We . . . know . . . only in an uncertain and inexact way. In favor of pure mathematics we must, indeed, make an exception. . . . Pure mathematics, however, is no science of existing things" (p. 51). Peirce's pure mathematics was a tautology without empirical content.

By contrast, Skinner does little speculation on "the long run," although he (1979) acknowledges a role for "ultimate consequences" (p. 48). In terms of what we can know at this time and in the foreseeable future of our lives, what is called "truth" may become increasingly probable but it never attains apodictic certainty. All "sentences about nature range from highly probable 'facts' to sheer guesses" (Skinner, 1955–1956/1972, pp. 5–6); and no formulation of scientific law and no formulation of logic completely escapes the probabilistic origins of verbal behavior because "The contingencies always come first" (Skinner, 1989, p. 44). Accordingly, in the usage of truth as what is certainly real, Skinner (1979/1984) finds the question of truth irrelevant: "Have I told you the truth? How can I tell? A science of verbal behavior makes no provision for truth or certainty" (p. 336). Science is not about this kind of truth: "So far as I am concerned, science does not establish truth or falsity; it seeks the most effective way of dealing with subject matters" (Skinner, 1988, p. 241); and Skinner (1979/1984) cautioned against

accepting any practice or statement as permanently settled: "Regard no practice as immutable. Change and be ready to change again. Accept no eternal verity. Experiment" (p. 346).

For Skinner, *truth* never escapes probability. Unlike Peirce, Skinner does not encourage hope for a settlement of truth "in the long run." Like Peirce, Skinner's (1974) only concession to a conceivable role for absolute truth is in verbal formulations that may disregard empirical reality: "Absolute truth can be found, if at all, only in rules derived from rules, and here it is mere tautology" (p. 136).

### CONCLUSION

In addition to advancing a probabilistic interpretation of behavior, private events, and consequences in a three-term contingency, Peirce and Skinner were both advocates of experimental science, espoused a behavioral psychology, shifted from nominalism to realism, opposed positivism, had a similar account for how behavior was strengthened, advocated the importance of a community of selves, had recursive approaches to method, and shared a belief in the probabilistic nature of truth. In addition, in response to the question, "Do you see operant conditioning as close to any existing philosophical system?" Skinner identified pragmatism and noted the similarity between Peirce's views and his own. Skinner's (1979) claim that this "is very close . . . to an operant analysis" (p. 48) is apt if Skinner had read and thought about Peirce's (1992b) entire essay "How to Make Our Ideas Clear" (p. 41), in which Peirce presented a probabilistic three-term contingency for habits as well as an explication of private events. And Skinner's claim is even more apt if he had read further from Peirce. Whether or not Skinner was influenced by other readings or discussions of Peirce's views, Skinner's radical behaviorism may be seen as a continuation and extension of those views.

### REFERENCES

- Abbagnano, N. (1967). Positivism. In P. Edward (Ed.), *The encyclopedia of philosophy* (Vol. 5, pp. 414-419). New York: Macmillan.
- Barlow, N. (Ed.). (1958). *The autobiography of Charles Darwin 1809-1882*. New York: Norton. (Original work published 1882)
- Behrens, P. J. (1993). Peirce's psychophysics then and now. In E. C. Moore (Ed.), *Charles S. Peirce and the philosophy of science: Papers from the Harvard Sesquicentennial Congress* (pp. 309-318). Tuscaloosa: University of Alabama Press.
- Bernstein, R. J. (1983). *Beyond objectivism and relativism*. Philadelphia: University of Pennsylvania Press.
- Blanshard, B., & Skinner, B. F. (1966-1967). The problem of consciousness—A debate. *Philosophy and Phenomenological Research*, 27, 317-337.
- Brent, J. (1998). *Charles Sanders Peirce: A life*. Bloomington: Indiana University Press.
- Cadwallader, T. C. (1974). Charles S. Peirce (1839-1914): The first American experimental psychologist. *Journal of the History of the Behavioral Sciences*, 10, 291-298.
- Carnap, R. (1963). In P. A. Schilpp (Ed.), *The philosophy of Rudolph Carnap*. La Salle, IL: Open Court.
- Coleman, S. R. (1984). Background and change in B. F. Skinner's metatheory from 1930 to 1938. *Journal of Mind and Behavior*, 5, 471-500.
- Eisele, C. (1979). The scientific philosophy of Ernst Mach. In C. Eisele (Ed.), *Studies in the scientific and mathematical philosophy of Charles S. Peirce* (pp. 228-235). The Hague, The Netherlands: Mouton.
- Fann, K. T. (1970). *Peirce's theory of abduction*. The Hague: Martinus Nijhoff.
- Feibleman, J. (1971). A reply to Bertrand Russell's introduction to the second edition of *The Principles of Mathematics*. In P. A. Schilpp (Ed.), *The philosophy of Bertrand Russell* (pp. 155-174). La Salle, IL: Open Court.
- Fisch, M. H. (1969). Vico and pragmatism. In G. Tagliacozzo (Ed.), *Giambattista Vico: An international symposium* (pp. 401-424). Baltimore: The Johns Hopkins Press.
- Fisch, M. H. (1986). In K. L. Ketner & C. J. W. Kloesel (Eds.), *Peirce, semeiotic, and pragmatism: Essays by Max H. Fisch*. Bloomington: Indiana University Press.
- Hawkins, B. S., Jr. (1997). Peirce and Russell: The history of a neglected "controversy." In N. Houser, D. D. Roberts, & J. Van Evra (Eds.), *Studies in the logic of Charles Sanders Peirce* (pp. 111-146). Bloomington: Indiana University Press.
- Hefferline, R. (1947). *A psychological analysis of verbal behavior: Class notes made by R. Hefferline, in a course at Columbia University, given by B. F. Skinner* (HUG FP 60.50). Cambridge, MA: Harvard University Archives.

- Kapitan, T. (1997). Peirce and the structure of abductive inference. In N. Houser, D. D. Roberts, & J. Van Evra (Eds.), *Studies in the logic of Charles Sanders Peirce* (pp. 477–496). Bloomington: Indiana University Press.
- Kolakowski, L. (1972). *Positivist philosophy*. Harmondsworth, England: Pelican.
- Marriott, F. H. C. (1988). The role of the statistician in psychology. In A. C. Catania & S. Harnad (Eds.), *The selection of behavior: The operant behaviorism of B. F. Skinner: Comments and consequences* (pp. 114–115). Cambridge, MA: Cambridge University Press.
- Medawar, P. (1979). *Advice to a young scientist*. New York: Basic Books.
- Medawar, P. (1982). Hypothesis and imagination. In *Pluto's republic* (pp. 115–135). Oxford, England: Oxford University Press. (Original work published 1963)
- Moore, E. C. (1961). *American pragmatism: Peirce, James, & Dewey*. New York: Columbia University Press.
- Moore, E. C. (1993). Introduction: Charles S. Peirce and the philosophy of science. In E. C. Moore (Ed.), *Charles S. Peirce and the philosophy of science: Papers from the Harvard sesquicentennial congress* (pp. 1–13). Tuscaloosa: University of Alabama Press.
- Moxley, R. A. (1989). Some historical relationships between science and technology with implications for behavior analysis. *The Behavior Analyst*, 12, 45–57.
- Moxley, R. A. (2001a). The modern/postmodern context of Skinner's selectionist turn in 1945. *Behavior and Philosophy*, 29, 121–153.
- Moxley, R. A. (2001b). Sources of Skinner's pragmatic selectionist in 1945. *The Behavior Analyst*, 24, 201–212.
- Ogden, C. K., & Richards, I. A. (1989). *The meaning of meaning*. New York: Harcourt Brace Jovanovich. (Original work published 1923)
- Peirce, C. S. (1929). Guessing. *The Hound & Horn*, 2, 267–282.
- Peirce, C. S. (1931–1958). In C. Hartshorne, P. Weiss, & A. Burks (Eds.), *Collected papers of Charles Sanders Peirce* (Vols. 1–8). Cambridge, MA: Belknap Press.
- Peirce, C. S. (1992). Some consequences of four incompatibilities. In N. Houser & C. Kloesel (Eds.), *The essential Peirce: Selected philosophical writings* (Vol. 1, 1867–1893, pp. 28–55). Bloomington: Indiana University Press. (Original work published 1868)
- Peirce, C. S. (1992a). A guess at the riddle. In N. Houser & C. J. W. Kloesel (Eds.), *The essential Peirce: Selected philosophical writings* (Vol. 1, 1867–1893, pp. 245–279). Bloomington: Indiana University Press. (Original work published 1878)
- Peirce, C. S. (1992b). How to make our ideas clear. In N. Houser & C. J. W. Kloesel (Eds.), *The essential Peirce: Selected philosophical writings* (Vol. 1, 1867–1893, pp. 124–141). Bloomington: Indiana University Press. (Original work published 1878)
- Peirce, C. S. (1997). In P. A. Turrissi (Ed.), *Pragmatism as a principle and method of right thinking: The 1903 Harvard lectures on pragmatism*. Albany: State University of New York. (Original work published 1903)
- Peirce, C. S. (1998). Of reasoning in general. In N. Houser, J. R. Eller, A. C. Lewis, A. D. Tienne, C. L. Clark, & D. B. Davis (Eds.), *The essential Peirce: Selected philosophical writings* (Vol. 2, 1893–1913, pp. 11–26). Bloomington: Indiana University Press.
- Peirce, C. S. (1998). On the logic of drawing history from ancient documents, especially from testimonies. In N. Houser, J. R. Eller, A. C. Lewis, A. D. Tienne, C. L. Clark, & D. B. Davis (Eds.), *The essential Peirce: Selected philosophical writings* (Vol. 2, 1893–1913, pp. 75–114). Bloomington: Indiana University Press. (Original work published 1901)
- Peirce, C. S. (1998). Pragmatism as the logic of abduction. In N. Houser, J. R. Eller, A. C. Lewis, A. D. Tienne, C. L. Clark, & D. B. Davis (Eds.), *The essential Peirce: Selected philosophical writings* (Vol. 2, 1893–1913, pp. 226–241). Bloomington: Indiana University Press. (Original work published 1903)
- Peirce, C. S. (1998). What pragmatism is. In N. Houser, J. R. Eller, A. C. Lewis, A. D. Tienne, C. L. Clark, & D. B. Davis (Eds.), *The essential Peirce: Selected philosophical writings* (Vol. 2, 1893–1913, pp. 331–345). Bloomington: Indiana University Press. (Original work published 1905)
- Peirce, C. S. (1998). A sketch of logical critics. In N. Houser, J. R. Eller, A. C. Lewis, A. D. Tienne, C. L. Clark, & D. B. Davis (Eds.), *The essential Peirce: Selected philosophical writings* (Vol. 2, 1893–1913, pp. 451–462). Bloomington: Indiana University Press. (Original work published 1911)
- Petryanov, I. V., & Trifonov, D. N. (1984). *Elementary order: Mendeleev's periodic system* (N. Weinstein, Trans.). Moscow: MIR Publishers.
- Rorty, R. (1961). Pragmatism, categories, and language. *The Philosophical Review*, 70, 197–223.
- Russell, B. (1970). *An outline of philosophy*. London: George Allen & Unwin. (Original work published 1927)
- Schneider, S. M. (1997). Back to our philosophical roots: A journal review of *Transactions of the Charles S. Peirce Society*. *The Behavior Analyst*, 20, 17–23.
- Skinner, B. F. (1947). Experimental psychology. In W. Dennis, B. F. Skinner, R. R. Sears, E. L. Kelly, C. Rogers, J. C. Flanagan, C. T. Morgan, & R. Likert (Eds.), *Current trends in psychology* (pp. 16–49). Pittsburgh, PA: University of Pittsburgh Press.
- Skinner, B. F. (1953). *Science and behavior*. New York: Macmillan.
- Skinner, B. F. (1957). *Verbal behavior*. New York: Appleton-Century-Crofts.
- Skinner, B. F. (1963). Behaviorism at fifty. *Science*, 140, 951–958.

- Skinner, B. F. (1966). *The behavior of organisms*. Englewood Cliffs, NJ: Prentice Hall. (Original work published 1938)
- Skinner, B. F. (1968). *The technology of teaching*. Englewood Cliffs, NJ: Prentice Hall.
- Skinner, B. F. (1972). Some relations between behavior modification and basic research. In *Cumulative record* (3rd ed., pp. 276–282). New York: Apple-Century-Crofts.
- Skinner, B. F. (1972). The concept of the reflex in the description of behavior. In *Cumulative record* (3rd ed., pp. 429–457). New York: Appleton-Century-Crofts. (Original work published 1931)
- Skinner, B. F. (1972). The generic nature of the concepts of stimulus and response. In *Cumulative record* (3rd ed., pp. 458–478). New York: Appleton-Century-Crofts. (Original work published 1935)
- Skinner, B. F. (1972). The operational analysis of psychological term. In *Cumulative record* (3rd ed., pp. 370–384). New York: Appleton-Century-Crofts. (Original work published 1945)
- Skinner, B. F. (1972). Freedom and the control of men. In *Cumulative record* (3rd ed., pp. 3–18). New York: Appleton-Century-Crofts. (Original work published 1955–1956)
- Skinner, B. F. (1972). A case history in scientific method. In his *Cumulative record* (3rd ed., pp. 101–124). New York: Appleton-Century-Crofts. (Original work published 1956)
- Skinner, B. F. (1972). Flight from the laboratory. In *Cumulative record* (3rd ed., pp. 314–330). New York: Appleton-Century-Crofts. (Original work published 1961)
- Skinner, B. F. (1974). *About behaviorism*. New York: Knopf.
- Skinner, B. F. (1979). Interview with B. F. Skinner (J. Morrow, Ed.). *Behaviorists for Social Action*, 2, 47–52.
- Skinner, B. F. (1980). *Notebooks*. Englewood Cliffs, NJ: Prentice Hall.
- Skinner, B. F. (1981). Selection by consequences. *Science*, 213, 501–504.
- Skinner, B. F. (1984). *The shaping of a behaviorist*. Washington Square: New York University Press. (Original work published 1979)
- Skinner, B. F. (1984). *A matter of consequences*. New York: New York University Press. (Original work published 1983)
- Skinner, B. F. (1986). Is it behaviorism? *Behavioral and Brain Sciences*, 9, 716.
- Skinner, B. F. (1987). *Upon further reflection*. Englewood Cliffs, NJ: Prentice Hall.
- Skinner, B. F. (1988). In A. C. Catania & S. Harnad (Eds.), *The selection of behavior: The operant behaviorism of B. F. Skinner: Comments and consequences*. Cambridge, MA: Cambridge University Press.
- Skinner, B. F. (1989). *Recent issues in the analysis of behavior*. Columbus, OH: Merrill.
- Staddon, J. (2001). *The new behaviorism: Mind, mechanism, and society*. Philadelphia: Psychology Press.
- Thayer, H. S. (1996). Peirce and truth: Some reflections. *Transactions of the Charles S. Peirce Society*, 32, 1–10.