

*BEHAVIOR ANALYSIS AND ECOLOGICAL PSYCHOLOGY: PAST, PRESENT, AND FUTURE.  
A REVIEW OF HARRY HEFT'S ECOLOGICAL PSYCHOLOGY IN CONTEXT*

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Relations between behavior analysis and ecological psychology have been strained for years, notwithstanding the occasional comment on their affinities. Harry Heft's (2001) *Ecological Psychology in Context* provides an occasion for reviewing anew those relations and affinities. It describes the genesis of ecological psychology in James's radical empiricism; addresses Holt's neorealism and Gestalt psychology; and synthesizes Gibson's ecological psychology and Barker's ecobehavioral science as a means for understanding everyday human behavior. Although behavior analysis is excluded from this account, Heft's book warrants a review nonetheless: It describes ecological psychology in ways that are congruent and complementary with behavior analysis (e.g., nonmediational theorizing; the provinces of natural history and natural science). After introducing modern ecological psychology, I comment on (a) Heft's admirable, albeit selective, historiography; (b) his ecological psychology—past and present—as it relates to Skinner's science and system (e.g., affordances, molar behavior); (c) his misunderstandings of Skinner's behaviorism (e.g., reductionistic, mechanistic, molecular); and (d) the theoretical status of Heft's cognitive terms and talk (i.e., in ontology, epistemology, syntax). I conclude by considering the alliance and integration of ecological psychology and behavior analysis, and their implications for unifying and transforming psychology as a life science, albeit more for the future than at present.

*Key words:* behavior analysis, ecological psychology, Skinner, B. F., history of psychology, neorealism, Holt, E. B., Gibson, J. J., Barker, R. G.

In the behavioral, social, and cognitive sciences, the relations between behavior analysis and ecological psychology have been strained on principled, pragmatic, and political grounds. The grounds shift, though, depending on the behaviorism and the ecological psychology. They come in varieties—so many, in fact, that some behaviorisms have a closer affinity to some ecological psychologies than to each other. B. F. Skinner's radical behaviorism and James J. Gibson's ecological psychology are a case in point. They have a common lineage in Charles Darwin's evolutionary biology, John Dewey's functional psychology, and C. S.

Peirce's philosophical pragmatism, a lineage distinct from Isaac Newton's mechanics, stimulus–response psychology, and logical empiricism. The behavioral revolution, however, obscured this: Skinner's behaviorism became aligned, nominally, with neobehaviorisms that opposed the ecological tradition (e.g., mediational stimulus–response psychology), while several ecological psychologies opposed neobehaviorism (e.g., various Gestalt psychologies). Nonetheless, over the course of Skinner's career and afterward, colleagues and commentators occasionally noted affinities between his behaviorism and some of the ecological psychologies. Harry Heft's (2001) *Ecological Psychology in Context* provides an occasion for delving further into the affinities—historically and at present—and considering their implications for the future of psychological science. First, though, I offer some background.

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## BACKGROUND

On receiving Heft's (2001) *Ecological Psychology in Context*—subtitled, *James Gibson, Roger Barker, and the Legacy of William James's Radical Empiricism*—I turned to its synopsis on the back cover, where it reads as follows:

In this latest book in [Erlbaum's] Resources for Ecological Psychology Series, Harry Heft examines the historical and theoretical foundations of James J. Gibson's ecological psychology in twentieth century thought, and in turn, integrates ecological psychology and analyses of sociocultural processes. A thesis of the book is that knowing is rooted in the direct experience of meaningful environmental objects and events in individual-environment processes and at the level of collective, social settings.

In Heft's words, his aim is "to articulate an emerging conceptual foundation for an ecological approach in psychology" (p. xxv). His three goals are to (a) "cultivate a deeper appreciation for Gibson's approach by articulating its theoretical commitments, in part through philosophical and historical analysis" (p. xxvi); (b) "explicate a set of foundational ideas that can serve to draw together two of the major ecological programs in psychology—James Gibson's and Roger Barker's" (p. xxvii); and (c) "consider the place of ecological psychology in the discipline of psychology more generally" (p. xxvii). As for the last, Heft advances a synthesis of Gibson's and Barker's programs to address what he sees as experimental psychology's greatest deficiency: understanding everyday human behavior.

Given this aim and these goals, Heft's book was enticing. It would seemingly review and expand on material outside of behavior analysis, yet congruent with and complementary to it. The experimental analysis of behavior, for instance, has affinities with Gibson's (1979) theory of direct perception (Costall, 1984) and includes, as well, a field of human behavioral ecology that seeks to understand human behavior in biological and social context (Hackenberg, 1998).<sup>1</sup> In applied behavior analysis, Barker's (1968) eco-

logical psychology is among the foundations of ecobehavioral analysis (Morris & Midgley, 1990), one of the subdiscipline's variants (e.g., Lutzker & Campbell, 1994; Schroeder, 1990). In philosophical, theoretical, and conceptual analysis, ecological psychology's (a) subject matter is unmediated functional relations between organisms and their environments and (b) criterion of truth is pragmatic (Good, 2007; Noble, 1981), which they are, respectively, in behavior analysis (Krechevsky, 1939; Verplanck, 1954) and radical behaviorism (Moxley, 2001, 2002; Zuriff, 1980).

The prospects of reviewing Heft's book grew more enticing when I had the honor of meeting him at a reception hosted by Division 26 of the American Psychological Association—the Society for the History of Psychology. I discovered that he was a professor of psychology at Denison University (Granville, OH), from which department and university I had graduated. In my day, the department's curriculum was deeply informed by Skinner's science and system of behavior and J. R. Kantor's interbehavioral psychology. As a result, I thought Heft would appreciate Skinner's (1938, 1953) focus on contingencies, unmediated functional relations, and selection by consequences and Kantor's (1959, 1971) focus on history, context, and field theory.

I was wrong. Although Heft appreciates Kantor (e.g., overseeing Denison's Kantor Memorial Lecture), he neither cites nor references him because interbehavioral psychology was outside the purview of his book (H. Heft, personal communication, October 9, 2008). And, although Heft cites Ivan P. Pavlov, John B. Watson, and Clark L. Hull, he alludes to Skinner only once in passing, and otherwise relegates him to three footnotes, only two of them indexed. In each case, he misunderstands Skinner. Although Heft's history of ecological psychology and his consideration of various programs within it are excellent, his treatment of Skinner and, by association, contemporary behavior analysis led me to question whether the book warranted a review in this venue. It does, but a case has to be made.

#### WARRANTING A REVIEW

Behavior analysis has made unparalleled advances in elucidating basic behavioral principles that are general within species and

<sup>1</sup>According to Hackenberg (1998), human behavioral ecology integrates the experimental analysis of behavior with the field of behavioral ecology in order to establish "an empirically based set of methods, concepts, and interpretations [for] understanding human behavior in biological and social context" (p. 541). Unfortunately, as Hackenberg (1998) notes, the "field and laboratory approaches to understanding human behavior from an ecological perspective have to date developed almost entirely in parallel, with little or no recognition of each other's accomplishments" (p. 574). This is also largely true in the behavioral ecology of nonhuman behavior (but see Dall, Cuthill, Cook, & Morphet, 1997; Peden & Rohe, 1984).

across them in upward continuity (e.g., reinforcement; see Catania, 2007). The science of these principles is a natural science—a science of universal principles, albeit with individual and species differences in their parameters. This is what the natural sciences would expect of psychology if it were a natural science. In contrast, behavior analysis has made far fewer advances in understanding everyday human behavior, for instance, social, emotional, and cognitive behavior (Baron & Perone, 1982; Hake, 1982). The science of everyday human behavior is a natural history—a science of the situated products of behavior's history (see Gergen, 1973), not a science of history per se, except when it is (see Epstein, 1984; Pipkin & Vollmer, 2009; Wanchisen & Tatham, 1991). Everyday human behavior is what the culture expects psychology to explain.<sup>2</sup>

Understanding why behavior analysis has made fewer advances in natural history than in natural science would require that I address topics ranging from the sociology of science to research methodology. The former lies beyond the scope of my essay, but the latter is relevant where it concerns limitations in research practices and conclusions drawn from them (Baron & Perone, 1982). In the experimental analysis of nonhuman behavior, for instance, we sometimes simulate human natural history and its products (e.g., self-awareness, insight) using basic behavioral principles and processes (e.g., stimulus control, shaping; see Epstein, 1984; Keehn, 1986). Simulations, though, do not incorporate all the principles and processes involved in everyday human behavior or necessarily incorporate them in the same way (Bachrach, 1981). Likewise, in the experimental analysis of human behavior, we sometimes synthesize everyday behavior and its products (e.g., cooperation, creativity, trust), using the same and possibly additional principles and processes (Hake & Olivera, 1978; see also, e.g., Bruzek, Thompson, & Peters, in press). However, syntheses are not analyses and they suffer from the foregoing limitations in research practices and conclusions (see Whitehurst & Valdez-Menchaca, 1988).

<sup>2</sup>An analogous distinction in biology is, for instance, between the principles of molecular biology as natural science and products of evolutionary biology as natural history—both individuals and species—all the while both fields are constituents of biology as a “natural science.”

As for applied behavior analysis, while it analyzes everyday human behavior (e.g., Borrero & Borrero, 2008; see also Cooper, Heron, & Heward, 2007), its ultimate purpose is to improve the human condition (Baer, Wolf, & Risley, 1968), not understand it (but, see Bruzek & Thompson, 2007), even when conducting functional analyses (see Iwata, Dorsey, Slifer, Bauman, & Richman, 1982/1994). And, although behavioral interpretation offers plausible accounts of everyday human behavior (e.g., social, emotional, cognitive) based on and constrained by the basic behavioral principles and processes (Donahoe, 2004), its methods are not empirical (Galizio, 1987).

Perhaps the closest behavior analysis has come to analyzing human natural history is in the analysis of behavioral development, a field founded by Bijou and Baer (1961, 1965, 1978; Baer, 1970; Bijou, 1976), advanced by Schlinger (1995), and extended by Novak and Pelaez (2004) (see *The Behavioral Development Bulletin*, est. 1995). To date, though, this field has mainly drawn together the experimental analysis of human behavior, applied behavior analysis, and behavioral interpretation. It has yet to establish a body of sustained, programmatic research on the process or products of behavioral development much beyond the experimental analysis of child behavior. Not even the innovative concept of behavioral cusps (Rosales-Ruiz & Baer, 1997; see also Bosch & Fuqua, 2001; Bosch & Hixson, 2004) has yet yielded a substantive body of basic or applied research (but see Ingvarsson, Tiger, Hanley, & Stephenson, 2007).

Given these limitations in understanding everyday human behavior, we have sometimes turned to psychology on such topics as self-perception (e.g., Bem, 1967; Nisbett & Wilson, 1977), memory (e.g., Loftus, 1993; Watkins, 1990), and language (e.g., Berko, 1958; Moerk, 1980; see Catania, 2007; Malone, 2009). For the most part, though, psychology suffers from the foregoing limitations in research practices and conclusions, and worse—worse because its methods and measures are not designed to understand behavior as a subject matter in its own right. Behavior is but a basis for conjectures about hypothetical mental structures and functions (e.g., beliefs, memory, and aggression) that purportedly explain behavior (e.g., believing, remember-

ing, and aggressing). With these structures and functions as its purported subject matter, psychology is a science of its mini-sciences (e.g., social, cognitive, and personality psychology). Whether this is due to psychology's nature (Bower, 1993; Koch, 1981) or to its being preparadigmatic as a science (Observer, 1982; Staats, 1991) is a matter of considerable debate (Sternberg, 2004).

In behavior analysis, the terms for these structures and functions are either ordinary language terms for behavioral relations that are isomorphic with our basic principles (e.g., memory as stimulus control; Branch, 1977) or behavioral relations that are historically situated and context dependent (e.g., remembering as problem solving; D. C. Palmer, 1989). In the first case, psychology's subject matter is accounted for by behavior-analytic principles. In the second case, its mini-sciences are not natural sciences, but natural histories, accounted for by behavioral principles and processes in everyday context (see Day, 1969a; Deitz & Arrington, 1984).

For the foregoing reasons, psychology is unlikely to become either a natural science or a natural history. Behavior analysis, though, has claimed it is the former (see Skinner, 1983, 1985, 1987a, 1990), while Heft claims that ecological psychology is the latter. Psychology, however, needs both natural science and natural history—not just one or the other—and needs them to be congruent and complementary. Behavior analysis and ecological psychology are, respectively, a candidate natural science and a candidate natural history. First, given that ecological psychology seeks to understand behavior in terms of direct, unmediated relations between organisms and their environments, its subject matter is congruent with that of behavior analysis. As a result, their alliance and integration may offer a more plausible alternative to contemporary psychology than either alone. Second, the natural science of behavior-analytic principles and processes (with its emphasis on contingencies) complements ecological psychology's natural history of everyday human behavior (with its emphasis on context), and vice versa. As a result, behavior analysis may advance ecological psychology's understanding of human behavior by unifying psychology's mini-sciences with common principles and processes, while ecological psychology may advance our

understanding of human behavior in ways that support our nonhuman syntheses and human simulations, supplement applied behavior analysis, and add breadth and depth to behavioral interpretation.

In presenting an approach to human behavior that is congruent with and complementary to behavior analysis, Heft's book warrants a review. However, his misunderstandings of Skinner's science and system make my essay also a critique, as I address his implicit assumption that behavior analysis and ecological psychology are opposed. Before beginning, though, I introduce ecological psychology as articulated by Erlbaum's editors for its Resources for Ecological Psychology Series, and then by Heft.

#### ECOLOGICAL PSYCHOLOGY

##### *Erlbaum's Series Editors*

In their forward to Heft's book, Erlbaum's editors—Robert Shaw, William Mace, and Michael Turvey (2001)—write that it and the other volumes in Erlbaum's series are

dedicated to furthering the development of psychology as a branch of ecological science. In its broadest sense, ecology is a multidisciplinary approach to the study of living systems, their environments, and the reciprocity that has evolved between the two. Traditionally, ecological science emphasizes the study of the biological bases of *energy* transactions between animals and their physical environments across cellular, organismic, and population scales. Ecological psychology complements this traditional focus by emphasizing the study of *information* transactions between living systems and their environments, especially as they pertain to perceiving situations of significance to planning and execution of purposes activated in an environment. (p. xiii)

Bringing a behavioral interpretation to bear on the last sentence, it might read as follows: Ecological psychology emphasizes the functional relations between organisms and their environments, as well as changes in those relations, specifically, between (a) changes in the organism's response functions for the environment, whether unmediated (e.g., contingency-shaped) or "mediated" by other functional relations (e.g., precurrent behavior, but not hypothetical structures and processes), and (b) changes in the environment's stimulus functions for the organism, as their relations



are affected by context (e.g., conditional discriminative stimuli, motivating operations). This interpretation suggests that ecological psychology is congruent with behavior analysis, but much depends on the theoretical status of Shaw, Mace, and Turvey's (2001) cognitive terms and talk about "planning," "execution," and "purposes," that is, about whether the terms refer to explanatory constructs or descriptive concepts. They can be read either way—and are. If they are explanatory, though, then ecological psychology is as mentalistic as the experimental psychology it seeks to replace.

### *Heft's Ecological Psychology*

In arguing that experimental psychology should become ecological, Heft proceeds somewhat inductively. Indeed, beyond noting at the start that (a) ecological psychology "takes as its central tenet...the *dynamic interrelation* between a living thing and its environment" (p. xxiv) and (b) "the reciprocity of the environment and the person...is a central feature of the ecological approach" (p. 7), he does not fully characterize the field until his concluding chapter, from which I excerpt three paragraphs:

Ecological psychology's entry point for the examination of psychological issues is the dynamic, ongoing, environment–person relation. Identification of this relationship as the principle unit of analysis highlights the primacy of relational, temporally dependent phenomena of a psychological nature. These phenomena clearly reveal the fact that psychological processes are situated processes, never fully isolatable from their contexts.

Ecological psychology adopts the view held by other facets of the life sciences that natural processes are structured in a nested hierarchy of relations. Consequently, in addition to reciprocal influences within the psychological unit of analysis, also operative are between-level relations. Of these, the between-level relations of greatest interest to ecological psychology are influences from "above" that is, from circumjacent, extraindividual structures that create opportunities for individuals even as they constrain action. [The other between-level relation is from below: human biology.]

Ecological psychology assumes that what is most distinctive about human actions...is their efforts toward meaning. The environment is viewed as being rich in features with functional

significance, and a focal point in human evolution was the exploitation and subsequent elaboration of meaningful functional resources in the environment...Individuals function to discover and sometimes create these meanings, and they act in concert with these features...Beyond supporting individual actions, these resources permit individuals to participate in functionally meaningful activities that are socially distributed across social networks. (pp. 394–395)

The quotations from Heft's opening pages and the first two paragraphs above are also congruent with behavior analysis. His third paragraph, though, suggests an intentional stance (e.g., "efforts toward meaning"), as do his descriptions of organisms as "purposive" and "active agents" with "goals and interests" (see, e.g., p. xxiii). This stance, along with Heft's privileging the organism independent of the environment (e.g., individuals discover and create meanings in a permissive environment), is inconsistent with an ecological psychology whose subject matter is the dynamic, reciprocal relations between organisms and their environments, where those relations—not organisms—have the properties of being purposeful and intentional. I address these tensions in Heft's cognitive terms and talk later.

### *Ecological Psychology in Context*

As for Heft's book, it includes an introduction and a conclusion, and in between them nine chapters organized into three parts. The first part addresses ecological theory and philosophical realism in chapters on James's radical empiricism and Edwin B. Holt's philosophical behaviorism. The second describes the ecological approach and radical empiricism in chapters on perceiver–environment relations, direct perception, and the stream of experience. The third covers ecological psychology and the psychological field in chapters on Gestalt psychology and the ecological approach, ecobehavioral science, ecological psychology and ecobehavioral science, and ecological knowledge and sociocultural processes.

My essay, in turn, has five parts. First, I describe Heft's historiography of ecological psychology. Second, I review his ecological psychology—past and present—and relate it to Skinner's science and system. Third, I discuss

Heft's misunderstandings of Skinner. Fourth, I address the theoretical status of Heft's cognitive terms and talk. And fifth, I consider the possibility of the alliance and integration of ecological psychology and behavior analysis, and its implications for unifying and transforming psychology as a life science.

### HISTORIOGRAPHY

Heft's history of ecological psychology takes us through a neglected part of psychology's past to the present. Beginning with Descartes' mind-body dualism, he describes two long-standing, conflicting lineages: the psychology of the disembodied mind and the psychology of embodied behavior. In psychology today, these are, respectively, *cognitivism* and *behaviorism*. Unlike most psychologists, Heft takes neither side. Instead, he criticizes Cartesian metaphysics and Newtonian physics for besetting psychology with its present-day problems: experimental psychology's mentalism and cognitive neuroscience's reductionism. Mentalism is a problem because the disembodied mind is a myth, not a subject matter. Reductionism is a problem because embodied behavior is biology's subject matter, not psychology's. Heft offers ecological psychology as an alternative, but does not see behavior analysis as an alternative, too.

Ecological psychology did not, of course, emerge fully formed. It has a history, one version of which Heft uses to organize his book. He begins with James's (1912) radical empiricism, then turns to James's student, Holt (1915b, 1915c), who made radical empiricism into neorealism, and then to Holt's student, Gibson (1979), who made neorealism into direct realism. Heft then describes the relations between Gibson's and Barker's work and the Gestalt psychologies of Kurt Lewin (1946), Kurt Koffka (1935), and Fritz Heider (1926/1950), but ultimately sets Gestalt psychology aside. In the end, he synthesizes Gibson's ecological psychology with Barker's (1963, 1968) ecobehavioral science to advance theory and research on everyday individual and sociocultural behavior.

Heft offers an original, finely textured and thick description of this history, and thus an important contribution to the history of psychology. However, as a thick description, his history presents two problems. First, it is

thick in a nontechnical sense: It is a tough read. Its prose is sometimes difficult, its transitions occasionally unclear, and its terms and phrases now and then unwieldy, even, I suspect for Heft's intended audience—"scholars and students in the areas of ecological and environmental psychology, theoretical and historical psychology, cognitive science, developmental psychology, anthropology, and philosophy" (back cover). If Heft's writing were more accessible, his audience might better appreciate why his ecological psychology should become psychological science. The same can be said, though, of Kantor's writing style in advancing interbehavioral psychology as psychological science (Morris, 1982; see also Stevens & Stone, 1947).

Second, Heft's history is thick in a technical sense: It is thick with scholarship. This is, of course, a compliment, but in this context also a complaint. His scholarship is so thick that, when combined with his style, readers may not notice that he draws selectively from the history of ecological psychology to synthesize just two of its varieties for psychology's future. This is a concern—a concern with what Heft includes and excludes in his history, such that the lineage he describes converges on just his synthesis, not any others. Specifically, I am concerned with whether Heft is describing (a) the history of ecological psychology or (b) the historical roots of Gibson's and Barker's ecological psychologies. Although he claims to describe the latter (H. Heft, personal communication, October 9, 2008), his historiography reads like the former. As a result, it appears to be what historians call presentist (Samelson, 1974; Stocking, 1965).

### PRESENTISM

Presentism comes in two main forms. One is history that describes the past-to-the-present as a justification for today's apparently winning (or wished for) traditions. In Heft's case, his history can be read as a foretelling of just his synthesis of ecological psychology. To his credit, he admits to setting some ecological psychologies aside (e.g., Bronfenbrenner, 1979) and does so on principled grounds. He seeks "an ecological psychology that is broad in scope and, importantly, *theoretically coherent*" (p. xvii), not a collection of all that is called ecological psychology. Heft's point is well-taken, but my concern is not allayed if he

has selectively drawn continuities from what are but similarities in the ecological tradition (e.g., James, Gibson, Barker; see Costall, 2003).

A second form of presentism is to interpret history as a justification for today's apparently losing (or wished against) traditions. For example, Heft seemingly describes, by exclusion, the death of every behaviorism, as well as psychologies, old and new, that have an affinity with ecological psychology, from yesterday's act psychology (e.g., Brentano, 1874/1973; see also Day, 1980) to today's action psychoanalysis (e.g., Schafer, 1976; see also Delprato, 2006; Lee, 1988; Morris, 2003). These psychologies may have been outside the scope of his purview, but one of his responsibilities is to apprise readers of this. He did not. This concern notwithstanding, Heft's probing analysis of the ecological psychologies he covers substantially enriches our understanding of them. This may be the book's greatest contribution.

#### ECOLOGICAL PSYCHOLOGY

Although Heft makes Gibson the central figure in his ecological psychology, he begins by locating the foundations of Gibson's direct realism in James's (1912) radical empiricism, which had its foundations, too, notably in Darwin. Heft describes how Darwin's (1859) *Origin of Species* replaced the static great-chain-of-being with "a dynamic realm of thoroughly natural, co-evolved entities functioning in a web of environmental interdependencies. The structural and functional properties of natural entities, and the interdependencies they share, reflect their ongoing mutual history. This view underlies an *ecological perspective*..." (p. 13). This view also underlies the behavior-analytic perspective, whose origins lie in Darwin, too, specifically in the adaptation of organisms to their environments and vice versa (Costall, 2004).

Darwin's first influence on psychology, though, was through functionalism, whose founder, Dewey (1859–1952), was arguably the father of ecological psychology, too. Dewey's (1896, 1922) concept of the "transactional" relation between the organism and its environment was both Darwinian and ecological, and appeared later in George Herbert Mead's (1938) concept of their "mutuality"

(Jarvilehto, 2009; see Blackman, 1991, on Mead and Skinner). Dewey addressed this theme in a later collaboration with Arthur Bentley on *Knowing and the Known* (Dewey & Bentley, 1949; see also Bentley, 1941) when Bentley and Skinner were corresponding. Here is a telling comment from Bentley about material Skinner had sent him:

If I had known of it, there are certain things I have said in recent papers that I would have altered. This applies strikingly to the word "stimulus"...Your approach to reflex...is a joy to me. Your type of experimentation is something I understand 100%...Your treatment of operant behavior under reflex, your refusal to spread stimulus over everything, your manner of putting drive in its place, your manner of pushing away both the mechanical and the mental with one common slap – all are up my alley. (Skinner, 1979, p. 344)

Bentley saw the transactional nature of behavior in radical behaviorism and thus its ecological orientation. Heft might have seen this too, had he given Dewey more coverage or addressed Mead and Bentley. This exemplifies my concern about Heft's historiography.

In any event, when functionalism gave way to the behaviorisms of Pavlov, Watson, and Hull, the Darwinian perspective was, in Heft's view, eclipsed by reductionism, mechanism, and molecularism. Skinner's (1938) early implicit grounding in Dewey (Pronko & Herman, 1982), however, and his later explicit grounding in Darwin (Skinner, 1966; see Costall, 2004) made his characterization of behavior arguably (a) holistic: behavior can only be understood in relation to its environment, not in terms of responses alone; (b) contextual: the functions of responses and stimuli depend on time and place; and (c) molar: behavior is defined in terms of functionally-related classes of responses and stimuli, not relations among formal instances thereof (see Day, 1980; Krechevsky, 1939; Malone, 1990, pp. 221–275; Moore, 1983).<sup>3</sup> In Skinner's day, other referents for molar were dynamic, field-theoretic, and phenomenal (Littman & Rosen, 1950). I address a

<sup>3</sup> Many scientists are dismissive of philosophical "isms" and rightfully so, especially when they gloss over important distinctions in the behavior of scientists. However, they are ultimately just names for scientific values. That values have names is helpful.

different meaning of 'molar' later (see Baum, 2002).

Heft does not see these characteristics of Skinner's behaviorism, perhaps because it is associated with the behaviorisms mentioned above, which are incompatible with ecological psychology; because commonalities are difficult to discern in "great person" history, especially in psychology, whose scientists are trained to differentiate theories, not integrate them; because ecological psychology is allied with ethology and Gestalt psychology, both of which have had contentious relations with "behaviorism"; and because the natural home of Heft's cognitive terms and talk lies in cognitivism and teleological folk psychology, which are incompatible with behavior analysis. One of the difficulties in reading Heft's book was watching him struggle to describe a supposedly naturalistic ecological psychology in cognitive terms and talk.

#### ECOLOGICAL THEORY AND PHILOSOPHICAL REALISM

The first part of Heft's book addresses ecological theory and philosophical realism in two chapters, one each on James's radical empiricism and Holt's philosophical behaviorism.

#### *James and Radical Empiricism*

James is both a central and an enigmatic figure in psychology. Although a leading functionalist (James, 1890), his functionalism was initially dualistic: He wrote of the separable functions of mind and behavior. And, although a founder of pragmatism (James, 1907), his pragmatism sought truth more in personal coherence (interpretation) than in correspondence (prediction) or effective action (experimental control), the last of which was the criterion of truth advanced by Peirce (1878, 1905). Perhaps this is why Skinner, who followed Peirce in his pragmatism (Moxley, 2001, 2002), wrote so little about James. Indeed, the only substantive behavior-analytic treatment of James may be what Willard Day (1983) referred to as "a relevant, if angry reference...by Malone relating James's concept of interest to Skinner's concept of reinforcement" (p. 93; Malone, 1975; but see Bijou, 1969; Lindsley, 1969).

In the end, Heft finds the Jamesian foundations of ecological psychology not in this early

James, but in a later one who rejected structuralism's mind-body dualism. As an entity, consciousness was a fiction. James also rejected reductive physicalism (and maybe materialism). His alternative was radical empiricism: an ever-differentiating stream of consciousness between the knower and the known, whose relations are direct, that is, not mediated by mind-as-an-entity.

Monism and unmediated functional relations are also fundamental to behavior analysis, as were some of James's (1890) stances on neuroscience and mentalism (Malone, 1975). As for one of James's stances on neuroscience, Heft notes that:

an adequate psychology could only be one that included an analysis of the biological conditions that underlie psychological processes. It could be clear now that it is possible to make this argument...and at the same time reject a reductive materialism. To utilize a particular conceptual language to describe the underlying biological correlates of some psychological experience is not to offer an account that is identical with experience itself, only more precise. Moreover, from this viewpoint, it becomes apparent why a science properly called "psychology" can never be eliminated by a sufficiently complete neurophysiological analysis. [In fact], psychological considerations set the problems for a physiologically relevant neuroscience to address. (pp. 48-49)

Skinner made the same point on several occasions, for instance: "[A]s I said in *The Behavior of Organisms*...no physiological fact has told us anything about behavior that we did not already know...The helpful relation is the other way around: a behavioral analysis defines the task of the physiologist" (Skinner, 1978, p. 123). And, as he put it elsewhere: "A behavioral analysis is essentially a statement of the facts to be explained by studying the nervous system. It tells the physiologist what to look for" (Skinner, 1969b, p. 283).

As for James's stance on mentalism, Heft describes what James (1890) called the psychologist's fallacy: "taking what is the product of a psychological process for the process itself. It is a confusion of ends and means" (pp. 50-51; i.e., "where the products of our thoughts are taken to be the basis for our thoughts," p. 132). What James (1890) actually wrote, though, was this: "The great snare of the psychologist is the *confusion of his own stand-*



*point with that of the mental fact* about which he is making his report. I shall hereafter call this the ‘psychologist’s fallacy’ *par excellence*’ (p. 196; e.g., If Heft writes that, then he must think about psychology as I do—a fallacy). Heft’s point is well taken, but it is not the fallacy James described (Costall, 1986). In fact, Heft’s cognitive terms and talk may commit him to the very fallacy he criticizes.

Of course, fundamental differences do exist between James and Skinner, and between James and other ecological psychologies, some of them fatal to their alliance and integration. For example, if consciousness is the only reality, then this conflicts with behavior analysis and ecological psychologies whose monism is based in materialism, that is, in the world of unmediated functional relations among biology, environment, and behavior (e.g., Noble, 1991; see, e.g., Gibson, 1979; Wilcox & Katz, 1981).

#### *Holt and Neorealism*

In addition to James, Heft also locates the foundations of ecological psychology in Holt. For this, I begin with a quotation from the historian of psychology, Thomas H. Leahey (2004):

James’ challenge to the copy theory [of knowledge; i.e., representationalism] inspired a group of young American philosophers to propose a new form of perceptual realism at about the same time the Gestalt psychologists were reviving realism in Germany... They called themselves *neorealists*, asserting that there was a world of physical objects that we know directly, without the mediation of internal representations. (p. 356)

Holt made radical empiricism into neorealism in reaction to James’s idealism—consciousness is the only reality. As did James, though, Holt rejected dualism and reductive physicalism. In their place, he offered a molar behaviorism, but again, not today’s molar behaviorism.

*Molar behaviorism.* According to Holt, reductionistic, mechanistic, and molecular behaviorism could not account for the variability in everyday behavior (e.g., predicting a discrete response for every discrete stimulus and vice versa), much less for the intentional property of behavior in context. A quotation from Holt (1915a) is illustrative:

We may grant with Bethe [Albrecht Bethe (1872–1954), German physiologist] that the bee is only, in the last analysis, a reflex mechanism. But it is a very complex one, and when we are studying the bee’s behavior we are studying an organism which by means of integrated reflexes has become enabled to respond specifically to the objects of its environment. It may be doubted whether Bethe, or any other of the biologists, fully realizes the significance of this; fully realizes, that is, how completely in behavior the stimulus recedes from its former position of importance. To study the behavior of the bee is of course to put the question, “What is the bee doing?” This is a plain scientific question. Yet if we should put it thus to Bethe, his answer would probably be: “It is doing of course a great many things; now its visual organ is stimulated and it darts toward a flower; now its olfactory bulb is stimulated and it goes for a moment to rub antennae with another bee of its hive; and so forth.” But this is not an answer. We ask, “What is the bee doing?”... With a little persistence, we could probably get Bethe to say, “Why, the *bee* isn’t doing anything.” Whereas an unbiased observer can see plainly enough that “The bee is laying by honey in its home.” (p. 77; see Moore, 1983; on Holt and teleological behaviorism, see Tonneau, 2008, on Holt, 1915a, pp. 86–87)

For Holt, behavior was an “action-unit” bounded by an initiation and a cessation that defined an achievement (e.g., laying by honey; see Lee, 1995, 1998).

*Recession of the stimulus.* In the foregoing quotation, Holt mentioned “how completely in behavior the stimulus recedes from its former position of importance.” This was his concept of the “recession of the stimulus” which he used to account for behavior that was unpredictable on the basis of proximal stimuli. Recession takes place over the course of an individual’s history, as proximal stimuli recede into the “stimulus array” or “structured environment,” to use Gibson’s (1979) terms.<sup>4</sup>

Holt’s concept may be understood in at least two ways. It may mean that, as the functional

<sup>4</sup>Contingencies are structured, too (see Thompson & Zeiler, 1986). Schedules of reinforcement have structures that affect not just individual responses, but patterns of responding over time (Ferster & Skinner, 1957; Zeiler, 1984). Stimulus control is also structured, for instance, in stimulus equivalence relations (Sidman, 1986; Zuriff, 1976) and relational frames (Hayes, Barnes-Holmes, & Roche, 2001).

relations among stimuli and responses grow in number, the prediction of one from the other becomes increasingly difficult. This was why Skinner (1935) defined behavior in terms of functional relations between classes of stimuli and responses, not their instances. Recession may also mean that the functions of proximal stimuli become conditional on the functions of other stimuli, which become conditional on the functions of still others. This was, perhaps, the basis of Sidman's (1986) point about behavior being embedded in *n*-term, higher-order contingencies. No matter how we understand Holt's concept, predicting and controlling behavior requires what he referred to as an appropriate "grain" of analysis, for instance, in the classes of stimuli and responses being analyzed. To use the microscope metaphor (Skinner, 1956), we have to adjust our level of resolution for the two classes in order to discern functional relations between them.

*The cognitive revolution.* As for the evolution of psychology as a science, Heft offers Holt's (1915a) prediction about psychology's future, and then laments:

By beginning the analysis with the assumption that "a total situation comprising both organism and environment is always involved," an account of cognition results that will avoid some of the problems typically accompanying mentalism. [Holt] proposed, "I venture to predict that behaviorism will be able to *give a complete account of cognition* without invoking the services of the metaphysical subject nor any one of its swarming progeny of Egos" (p. 177). Holt's prediction has yet to be realized. Although the language of psychology may have changed, the conceptual, if tacit, reliance on "inner mental states" continues. (pp. 89–90)

Heft's lament is that the cognitive revolution was only a change in the surface structure of psychology's terms and talk, not in the deep structure of its explanatory practices (see Costall, 2004; Greenwood, 1999; Leahey, 1992b; Moore, 1995; Morris, 2003).

Skinner (1979), by the way, read and admired Holt's (1915a), *The Freudian Wish and Its Place in Ethics*, but was critical of *Animal Drive and the Learning Process: An Essay Toward Radical Empiricism* (Holt, 1931). For instance, although Holt's definition of behavior was molar, Skinner criticized it for being a stimulus–response, initiation–cessation psy-

chology that was inadequate for explaining "higher mental achievements" (Skinner, 1979, p. 102). In addition, although Holt cited Moliere's example of the circularity in the "soporific virtues" of opium for sleep, which Skinner used on several occasions (e.g., Skinner, 1977), Skinner (1979, p. 166) detected circularity in Holt's physiologizing.

*Poverty of the stimulus.* Leahey (2004, pp. 392–400) is unsympathetic toward radical behaviorism, but he describes it accurately. However, in summarizing the fate of neorealism, though, he seemingly misunderstood neorealism in a way foretold by his description above of the neorealists' "world of physical objects that we know directly" (p. 356). Leahey's (2004) summary was this:

Neorealism did not last long as a philosophical movement. Its primary failing was epistemological: accounting for the problem of error. If we know objects directly and without mediation by ideas, how is it that we have mistaken perceptions? With a copy theory, error is easy to explain, by saying that the copies may not be accurate. Realism finds error difficult to account for. (p. 386)

This is a poverty-of-the-stimulus argument, one which Noam Chomsky (1959) made famous in his review of Skinner's (1957) *Verbal Behavior* (Schoneberger, 2005). According to Chomsky (1959, p. 54–58; 1965), children's linguistic environments are too impoverished for them to learn grammar, so grammar must be innate (Chomsky, 1965; contra. Pullam & Scholz, 2002).

In a form of this argument closer to Leahey's criticism of neorealism, Chomsky claimed that behavior was not lawful because stimuli—Leahey's "physical objects"—did not reliably affect behavior. His example was the unpredictability of what people might say on seeing a Dutch painting: "Dutch," "Tilted," "Beautiful," and "Remember our camping trip last summer" (Chomsky, 1959, pp. 31–32). According to Chomsky (1959), if a stimulus is not related to a response, then "the word 'stimulus' has lost all objectivity in this usage. Stimuli are no longer part of the outside physical world; they are driven back into the organism... [T]he talk of 'stimulus control' simply disguises a complete retreat to mentalistic psychology" (p. 32). In a structural account of behavior, stimuli are, indeed, too impoverished to explain individual differences

in responding. A functional account, however, enriches and transforms the stimulus's functions through history and context which allows functionalism to explain the differences. This enrichment and transformation is at the heart of both Skinner's behaviorism and the ecological perspective.

Just as Chomsky's (1959) criticism of Skinner is based in a structuralist perspective, so too is Leahey's (2004) criticism of neorealism, where he noted that the perception of "physical objects" varies unlawfully across individuals and can be mistaken. In a functional account, however, variability and mistaken perceptions are not unlawful or mistaken. As Skinner (1974) put it: "We are always 'dealing with reality,' although the term must be taken to include *more than a current presentation*. The important differences are among behaviors, and these in turn are explained by differences in past contingencies" (p. 89). Differences in past contingencies—history—account for differences in the present functions of physical objects, which are, in turn, a function of their context (see Knapp, 1986). This was also Holt's account of illusions (Holt, Marvin, Montague, Perry, Pitkin, & Spaulding, 1912; see Tonneau, 2008), which makes Skinner's account ecological, too.

#### THE ECOLOGICAL APPROACH AND RADICAL EMPIRICISM

The second part of Heft's book addresses the ecological approach and radical empiricism in three chapters, one each on perceiver–environment relations, direct perception, and the stream of experience. Gibson is the central figure.

##### *Perceiver–Environment Relations*

As for perceiver–environment relations, Heft mainly describes Gibson's concept of affordances (see Reed, 1991), for which Gibson advanced two programs. In a destructive program, he argued that organisms do not perceive the elements of a sensation and then represent and retrieve them as a whole in their minds, which supposedly explains perceptual regularities in an impoverished physical environment. In a constructive program, he argued that regularities in perception arise from properties of the environment he called

"affordances," as in what the environment "affords" behavior. Affordances are purportedly relative to the individual and multiple in possibilities. For example, as Heft noted: "[W]hen looking for something to hold down papers on a windy day, a book will serve that purpose; it will also play the role of a door-stop or a window prop, or as its initial intended function as a repository of information" (p. 131).

Affordances are akin to Kantor's (1959) concept of stimulus function in his natural history of human behavior (e.g., the graspability of a hammer). They are also akin to Skinner's concept of discriminative stimuli in his natural science of behavior (e.g., the discriminative function of a hammer for related operant behavior; see Costall, 1984). Stimulus functions and discriminative stimuli are likewise relative to the individual and multiple in possibilities, making Heft's example of a book's functions appropriate for both Kantor and Skinner. For the unprepared reader, though, the term 'stimulus' fails to capture the familiar, intuitive sense of a stimulus's reciprocity with behavior (see Verplanck, 1954), whereas terms such as 'affordance' and 'meaning' do (H. Heft, personal communication, October 9, 2008). Whether this is fatal to Skinner's behaviorism is a practical, not a principled matter.

Where Kantor and Skinner differ from Gibson regarding affordances was that he, Gibson, conceived of them as properties of the environment, independent of the organism. This committed him to a realist ontology of affordances and a correspondence theory of truth (Good, 2007), which is incompatible with behavior analysis, Peirce's pragmatism, and seemingly, contemporary ecological psychology (Costall, 1986, 2004). Heft addresses this issue, but does not resolve it (Chemero, 2003b). For an account of affordances as relations between organisms and their environments, not as the latter's properties, see Chemero (2003a).

Skinner's (1977, 1990) destructive and constructive programs for the mainstream psychology of perception were similar to Gibson's, but he did not acknowledge any commonalities in their programs or historical development. He mainly noted differences. For instance, in a letter to Fred Keller about a colloquium Lewin had given at the University

of Minnesota circa 1939, he wrote: "Skinner is sure we agree, but fundamentally there is the same old ghost of purpose standing between us. I am getting througher and througher with any talk about goal-directedness" (Skinner, 1979, p. 24). Moreover, Skinner misunderstood Gestalt psychology on at least one point. He thought that it posited perceptual "givens" in either the organism or the environment. This can be gleaned from a letter he wrote to Keller in 1940 about a conversation he had had with Kohler and Lewin:

K. was contending that a hell of a lot of things were "given" in perception. You can see aggression, friendliness, etc. I argued that the behavior of two or more people depended on one's experience—that you saw merely the behavior of two people and that you inferred the friendliness. And so on—well, anyway I suddenly asked him if, when you looked at a familiar picture, the familiarity was part of the perceptual pattern. He got pretty flustered for a moment, then rallied and said yes. When I contended the familiarity could not be in the stimulus but was obviously related to the past experience of the observer, he said, in plain language that surprised me, that he didn't care where the thing came from genetically [i.e., developmentally]. If this is what they've been saying all along I've had the wrong idea. (Skinner, 1979, p. 246; see Skinner, 1957, p. 136)

Misunderstanding occurs on both sides.

#### *Direct Perception*

According to Heft, "Gibson's claim that perception is direct and unmediated virtually stands alone in 20<sup>th</sup>-century psychology. All other recent theories assume that between stimulus input...and psychological outcome...are mediating processes that enrich or otherwise transform this input" (p. 154). What enriches and transforms the input in Gibson's account is its "context," that is, the relations among proximal and distal stimuli that lawfully sustain and constrain what the environment affords perception. For example, even though the retinal image of a nearby object is larger than its image at a distance, the object is perceived as retaining its size, that is, as being "invariant." In cognitivism, invariance is explained by mediating cognitive processes that purportedly enrich and transform the object. In Gibson's view, invariance is

explained by context, both historical and current. Context enriches and transforms the functions of objects through (a) perceptual learning about invariant relations in our physical world and (b) the relations in which the objects are embedded—the ambient optical array (e.g., light reflection), texture gradients, and multiple specifications across locations and movement.

Gibson's theory of direct perception complements the behavior-analytic account on this point. Although behavior analysis provides a natural science of perceiving in terms of stimulus control, stimulus control per se does not specify the history and contexts that account for what is perceived (e.g., invariance). Gibson, in contrast, did account for this. He specified the history and contexts, and offered an account of perception's natural history and a research program to support it. In a sense, he did for perception what Ernst Moerk (1980, 1992) did, in part, for Skinner's (1957) account of verbal behavior. Moerk specified the history and contexts for learning to speak grammatically and offered an account of the natural history of language acquisition and a research program to support it (Moerk, 1990).

#### *The Stream of Experience*

Heft next turns to one of "the most distinctive and central features of James's psychology": the stream of consciousness, that is, "the temporally extended character of experience" (p. 173). Here, he addresses the ontological primacy of ongoing change in organism-environment interactions; how an organism's movements influence that change and are influenced by it; the functional (not temporal) distinctions among remembering, perceiving, and imagining; and the problem of error, in which direct perception is not synonymous with veridical perception, even as perception is lawful. These are distinctive features of behavior analysis, too, but difficult to identify in Heft's thick descriptions.

#### ECOLOGICAL PSYCHOLOGY AND THE ECOLOGICAL FIELD

In the third part of his book, Heft addresses ecological psychology and the psychological field in four chapters on Gestalt psychology and the ecological approach, Barker's ecobe-



havioral science, Gibson's ecological psychology and ecobehavioral science, and ecological knowledge and sociocultural processes.

#### *Gestalt Psychology*

In moving from Gibson to Barker, Heft addresses the influence of Gestalt psychology on them, specifically, Lewin's (1946) concept of an object's valence in an individual's lifespace, Koffka's (1935) inclusion of both a geographical and a behavioral environment, and Heider's (1926/1950) distinction between things and their medium. Although Gibson and Barker were attracted to Gestalt psychology, they ultimately set it aside: Gestalt psychology was dualistic and rooted in physics, whereas ecological psychology was monistic and rooted in biology.

#### *Ecobehavioral Science*

In introducing Barker's ecobehavioral science, Heft addresses cognitive neuroscience's reductionism and ecological psychology's alternative to it. The alternative is to consider all natural processes as "structured in a nested hierarchy of relations" (p. 394). In this view, "natural processes" are lawful at their respective levels of analysis and not reducible to processes at other levels, even as the products of the processes at one level participate in and influence those at another level, that is, sustain and constrain them. In ecological psychology, the levels are (a) biological—the biological organism interacting with its physical world; (b) psychological—the psychological organism interacting with its ecological world; and (c) social—the social organism interacting with its sociocultural world. Heft aligns Darwin with the first level, Gibson with the second, and Barker with the third. Behavior analysts might nominate, respectively, Darwin, Skinner, and Marvin Harris (Harris, 1977, 1979, 2007; see Glenn, 1988; Lloyd, 1985; Malagodi, 1986; Vargas, 1985).

Barker thought psychology had failed to understand everyday human behavior because it had not begun as natural history. A natural history of psychology would provide taxonomic data on what it sought to explain—everyday human behavior—frequency data on behavior; its incidence; and correlational data on the circumstances under which it occurred—putative functional relations. To ground psycholo-

gy in natural history, Barker and his colleagues established the Midwest Field Station in Oskaloosa, Kansas, which facilitated "the study of human behavior and its environment in situ by bringing to psychological science the kind of opportunity long available to biologists: easy access to phenomena of psychological science unaltered by the selection and preparation that occur in laboratories" (Barker, 1968, p. 1).

In *One Boy's Day*, Barker and Wright (1951) provided narrative accounts of the behavior of individual children over the course of their daily activities (Barker, 1967). Although some putative functional relations were discernable in the children's "behavior streams," Barker and Wright could not predict behavior very well on the basis of proximal stimuli alone. Their insight was to see that the grain of their analysis was wrong:

...some attributes of behavior varied less across children within settings than across settings within children. We found, in short, that we could predict some aspects of children's behavior more adequately from knowledge of the behavior characteristics of the drugs stores, arithmetic classes, and baseball games they inhabited than from knowledge of behavior tendencies of particular children. (Barker, 1968, p. 4)

These higher-order "behavior settings" were naturally occurring sociocultural structures: the actions of groups of individuals in specific locations within temporal boundaries that were discriminable and quasi-stable, yet independent of specific individuals, even as individuals participated within them (Barker & Gump, 1964). In this, Barker established an "ecobehavioral science"—a science at an "extraindividual" level—which Heft sees as complementing Gibson's ecological psychology at the individual level (see, respectively, Glenn, 2004, on metacontingencies, and Skinner, 1969a, on contingencies). Even at the extraindividual level, variability remains to be accounted for. For this, Heft eschews explanatory systems that appeal to cognitive representations that individuals bring to their settings (e.g., scripts) and turns instead to Gibson's nonmediational ecological psychology.

Although Heft does not note any relations between Gibson and Skinner, he mentions operant theorists in the context of Barker's

ecobehavioral science in his unindexed footnote: "Barker...has been embraced erroneously on occasion by operant theorists who greatly simplify the operations of behavior settings by viewing the coercion of action as operating through contingencies of reinforcement" (p. 280). Setting aside whatever coercion may mean, Heft correctly points out that current, proximal contingencies alone are not sufficient to account for everyday human behavior; history and context are also necessary (e.g., biological and behavioral ontogeny, concurrent operants, motivating operations). This is not a fatal criticism of behavior analysis, of course, because the field acknowledges these factors or controls for them when analyzing contingencies (Morris, 1992).

Although Heft did not support his claim about the operant theorists' simplification of behavior settings with any citations, his claim was not about Rogers-Warren and Warren's (1977) book, *Ecobehavioral Perspectives on Behavior Analysis* (H. Heft, personal communication, October 9, 2008), in which Barker is cited widely. The book was based on a 1976 conference prompted by an exchange in the *Journal of Applied Behavior Analysis* between the ecological psychologist, Edwin Willems (1974), and the behavior analyst, Don Baer (1974). The exchange arose over Willems's arguments that applied behavior analysis did not sufficiently account for interdependencies within behavior (e.g., side-effects), across settings (e.g., individual differences), or over time (e.g., history effects), which could lead to undesirable outcomes (e.g., Sajwaj, Twardosz, & Burke, 1972; see also Martens & Witt, 1988; O'Connor & Lubin, 1984).

The conference brought together ecological psychologists and applied behavior analysts in a dialog over their similarities and differences, for instance, in their goals, methods, and ethics. Some of the applied behavior analysts, however, were already ecologically-oriented. Todd Risley and Bob Wahler, for instance, presented research that went beyond contingencies to include contextual factors in, for instance, daycare settings (e.g., incidental teaching; Hart & Risley, 1975; see also Risley, 1977) and social systems (e.g., setting events; Wahler & Fox, 1981; see also Wahler, Berland, Coe, & Leske, 1977).

Behavior analysts, however, have engaged Barker critically and for reason. Take Bijou's

response to Len Krasner's (1977) question, "What do you think of the work of Roger Barker?":

I had hoped that Barker would deliver what he promised: a collection of objective information about behavior in relation to the environment. But he didn't. His data are full of preconceptions, interpretations, and inferences about goals. Not only do Barker and his colleagues read intent into the behavior of their subjects, but they rehash their data before they write up the interactional episodes. Barker claimed that his accounts would be as objective and neutral as a collection of rocks in a museum. It didn't turn out that way. You can't use Barker's data unless you accept his theory of human behavior with all of its inner determinants, and his method of observation. They claim that they can't avoid introducing mentalistic terms like intentions and cognitions. We behavior analysts have found that we must repudiate much of the observational method of people such as Barker and the Whittings because of its subjectivity [e.g., Whiting & Whiting, 1975]. To be consistent with the behavior analysts' core method, we have to decide to watch the individual and count selected instances of occurrences. It paid off [see Bijou, Peterson, & Ault, 1968; Bijou, Peterson, Harris, Allen, & Johnston, 1969]. (pp. 597-598)

Bijou's concern with Barker's ordinary language is my concern with Heft's cognitive terms and talk. At issue is whether their theoretical status is explanatory or descriptive. In Bijou's view, their status was explanatory and, hence, the terms and talk were mentalistic.

#### *Ecological Psychology and Ecobehavioral Science*

Heft next delves further into how Gibson's account of individual behavior complements Barker's account of extraindividual behavior, which makes their synthesis possible. This is his book's major creative contribution. It also addresses material that speaks to the modern meaning of molar (see Baum, 2002), how Aristotle's four causes map onto behavior (see Moore, 2002), a dynamical systems perspective (see the *Journal of the Experimental Analysis of Behavior*, 1992, 57[3]), and the contextualism-mechanism debate (see Morris, 1993a, 1993b).

#### *Ecological Knowledge and Sociocultural Processes*

In the final chapter in this part of the book, Heft addresses current issues in the psycho-

logical and social sciences. Cognitivists will find it critical of their mediational and representational theories, while behavior analysts may find it difficult to follow because of Heft's terms and talk, and the topics he covers. The topics lie largely outside mainstream psychology, albeit seemingly consistent with behavior analysis. Among them are the role cultural structures play in human evolution, the cumulative growth and enrichment of those structures for human knowledge—not in one's head, but in one's actions—through tools and artifacts (e.g., texts), and the socially distributed nature of knowledge.

According to Heft, knowledge is "best understood as a functional relation between the knower and the known. Rather than viewing knowing as an intra-individual process as something primarily occurring 'in' the private theaters of individuals' consciousness, it [is] seen, most basically, as action-in-context" (p. xxxiv). Or, as he writes of Holt: "cognition is a functional relation between action and referent" (p. 98). Moreover, knowledge is not only behaving effectively with respect to the physical world, but also with respect to the sociocultural world. That is, social knowing is an unmediated functional relation between an organism and its social environment. As such, we know another person's facial expressions, personality, intentions, and mind – that is, their affordances – directly (Good, 2007; McArthur & Baron, 1983; e.g., Berry & McArthur, 1986). This is a behavior-analytic theory of mind (Hackenberg, 1988; Harzem & Miles, 1978; Hineline, 1980, 1983, 2003; Schlinger, in press; Spradlin & Brady, 2008; see Leuder & Costall, 2004; Ryle, 1949).

In his concluding chapter, Heft summarizes his case that psychology must become ecological in order to account for everyday human behavior. It bears reading, but not my reviewing it. Other concerns remain, the first being Heft's misunderstanding of Skinner.

#### SKINNER'S BEHAVIORISM

As noted, Heft does not cite Skinner in his text, referring to him only by allusion, this with respect to radical behaviorism's "extreme environmentalist position" (p. xxx), which is a misunderstanding (Skinner, 1966, 1981; see also Morris, Lazo, & Smith, 2004). He other-

wise relegates Skinner to the three footnotes mentioned earlier. In the first of the indexed footnotes, indexed under "Skinner's operant psychology," Heft addresses Skinner's research preparation: "In view of the fact that action is intentional, what an individual actually will do is not something that can be predicted, except in those exceedingly rare conditions where the possibilities for meaningful actions (i.e., actions the lead to some outcome) are extremely narrow (as in the case of an operant chamber) or specific motivations are artificially elevated (e.g., through deprivation)" (p. 383).

Whether Heft means that everyday behavior cannot be predicted in principle or in practice, both meanings are problematic. The in-principle meaning rules out the possibility of a science of behavior, either as a natural science or a natural history, as well as, presumably, the possibility of a naturalized ecological psychology. The in-practice meaning is an empirical question which the success of applied behavior analysis has, in part, answered (see Cooper et al., 2007).

Notwithstanding this characterization of Skinner's science, laboratory research is widespread in ecological psychology. As the human behavioral ecologist, Bruce Winterhalder (1981), observed: "The problem is to simplify complex adaptive systems so that they retain essential and interesting (i.e., nontrivial) features, but at the same time become analytically tractable" (p. 18). Or, as the ecological cognitive psychologist, Ulric Neisser (1985) noted, ecological psychology is not inimical to laboratory research as long as it "maintain[s] the integrity of the variables that matter in natural settings" (p. 25). In fact, most of the research published in the journal, *Ecological Psychology*, is laboratory-based.<sup>5</sup>

Heft's second indexed footnote to Skinner appears in a discussion of Sigmund Koch's (1999a) high regard for Gibson's "ontology-revealing framework," to wit: "Koch (1999b) cited Tolman's purposive behaviorism, with its phenomenal characteristics, as an 'ontology-respecting framework,' and Skinner's program, with its highly restricted language base, as an example of [an] 'ontology-distorting frame-

<sup>5</sup>For laboratory syntheses of natural forging in behavioral ecology, see Fantino and Abarca (1985) and Kamil, Yoerg, and Clements (1988). On such syntheses, see Catania (2007, pp. 190–192).

work' (pp. 187–188)" (p. 392). Whether Skinner's program is ontology-distorting, compared to the phenomenal program, is another empirical question. In physics and astronomy, the phenomenal characteristics of our physical world—for example, the striking beauty of a setting sun—explain little about sunsets in comparison to physical principles and processes (e.g., wavelength, hue, saturation, brightness, light dispersion). Moreover, astronomy teaches us that the phenomenal "sunset" is ontology-distorting: The sun does not set—the earth turns. In the end, technical and phenomenological terms and talk need not be antithetical. They may serve different goals: some scientific and logical, others social and emotional. Behavior-analytic and phenomenological terms and talk, however, remain antithetical because their ontologies differ so fundamentally. Other natural sciences have largely overcome this problem (but see Medin & Atran, 1999).

#### MOLAR BEHAVIOR, MOLAR EXPLANATIONS

As mentioned earlier, Heft criticizes Pavlov, Watson, and Hull for being reductionistic, mechanistic, and molecular. Presumably, he is similarly critical of Skinner, but as I noted, Skinner's science was not aligned with these isms. Moreover, Skinner's (1935) unit of analysis belies these characterizations. It was nonarbitrary and unique. It was nonarbitrary in that it was not identified structurally, a priori, or theoretically, but in terms of the lawfulness of the subject matter. It was unique in that its lawfulness was identified by the relations between the classes of responses and stimuli. Under- or over-restricting the classes reduced prediction and control: Under-restricting them led to the inclusion of responses and stimuli that were not functionally related (i.e., that had no defining properties), while over-restricting them led to the exclusion of responses and stimuli that were functionally related (i.e., that did have defining properties).

As a result, Skinner characterized behavior as molar, not molecular, as the terms were used in his day (Littman & Rosen, 1950; see also Baum, 1995; Day, 1980; Malone, 1990, pp. 221–275; Moore, 1983). As the terms are used in our day, Skinner's characterization of behavior is still molar, but his explanation of behavior is molecular, not molar (Baum, 2002,

2004a), if the two can be parsed (Baum, 2004b). In both cases, though, molar is ecological in perspective. I begin with Skinner.

#### *Molar Behavior*

E. G. Boring was likely the first to point out an affinity between Skinner's behaviorism and Gestalt psychology. His observation, though, was critical. It was critical of Skinner's (1931) definition of behavior in the theoretical portion of his doctoral thesis: "You have given a very broad, strange, almost bizarre meaning to the word *reflex*. You have taken it away from the constrained anatomical reflex-arc meaning, and you have equated it to the concept of the psychological fact-as-relational-correlation... And if you succeeded you would have merely the equivalent for Gestalt with a special epistemology back of it" (Skinner, 1979, p. 73).

Boring was astute, but whether Skinner saw the equivalence or was informed by it is another matter. At the time, though, he was interested in Gestalt phenomena (e.g., perception, insight; e.g., Skinner, 1932; see Skinner, 1979, pp. 9–10, 21, 30–31).

Soon thereafter, Isadore Krechevsky (later, David Krech) (1939) noted the Gestalt-like nature of Skinner's unit of analysis in his review of *The Behavior of Organisms* (Skinner, 1938):

Skinner's reflex is *not* a "molecular" unit, as understood and criticized by Gestalt psychologists. Gestalt psychologists never have objected to "psychological units" and to analysis. That is a misunderstanding all too common among American critics. What the Gestalt psychologists *have* criticized is the type of unit used and the degree of analysis indulged in, but certainly not the fact of analysis. They have maintained that each science must determine for itself what its fundamental unit is to be, and that the appropriate unit need not always be the smallest discernible "molecule." (p. 406)

Krechevsky continued:

Skinner himself follows this fundamental position of the Gestalt psychologist...Skinner's reflex...is a *molar* one; the physiologist's, a "molecular" one (relative to Skinner's). Again we find Skinner following the best traditions of Tolman, Lewin, Koffka, *et al.*; for if he stops with his process of analysis at the point beyond which no *psychological* sense can result, he is not differing from the Gestaltist's concept of what is proper analysis. (pp. 406–407; see Skinner, 1935)



In the foregoing accounts, Skinner's unit of analysis was molar in three senses: (a) behavior was defined as the lawful conjunction of classes of responses and stimuli, where differences in their instances were largely irrelevant (e.g., how a lever was pressed or a step was taken); (b) more complex behavior was not necessarily a concatenation of simpler, more elementary responses—it was lawful in its own right (e.g., operant stimulus control, walking to the post office); and (c) behavior was not identifiable in terms of responses per se—it was a property of their functional relations with stimuli (e.g., reinforcers, mail).

Although Skinner's unit of analysis was molar, his explanations of behavior were molecular in today's molar–molecular distinction (Baum, 2001, 2002, 2004a). This is a paradigmatic distinction between (a) molecular accounts of behavior that appeal to contiguity between instances of responses and stimuli *in* time, and (b) molar accounts that appeal to correlations between classes of responses and stimuli *over* time (see Rachlin, 1992, 1994). Skinner's account is molecular because, although responses and stimuli were class concepts, their functional relations were accounted for by the contiguity of their instances (e.g., response-reinforcer contiguity; Baum, 1995, 2002, 2004a).<sup>6</sup>

### *Molar Explanations*

Just as Skinner's molar definition of behavior was ecological, so too is the meaning of molar in the new paradigm (Baum, 2004a, b). According to Baum (2004b), "The molar paradigm...goes back at least to 1896 when John Dewey published a famous article criticizing the reflex and advocating instead for a view of behavior as composed of 'coordinations' that are continuous" (pp. 117–118; see also Baum, 2004a, p. 352). Baum (2004a) also cites Holt's (1915a) and Koffka's (1935) criticisms of the molecular definition of behavior. However, these are the same points Skinner (1931, 1935) made in proposing a molar definition of behavior (see Baum, 1995; Pronko & Herman, 1982); they are not about

molar explanations. One modern molar concept, though, may uniquely align with the ecological perspective: nesting.

*Nesting.* Heft mainly discusses nesting in the hierarchical—that is, nested—levels of biological, psychological, and sociocultural systems (e.g., biological processes participate in all behavior, but are not its "cause"). His within-level analyses mainly address sociocultural systems. For example, "one boy's day" is nested in sociocultural patterns of activities within the nuclear family over more extended periods of time, which in turn are nested in even more temporally extended sociocultural patterns of community practices (see Good, 2007; for a behavior-analytic account, see Glenn, 2004). Although similar, discussions of nesting in today's molar explanation mainly address the psychological system—individual behavior extended in time (Baum, 1995, 2002, 2004a; Rachlin, 1992, 1994). For instance, warming up, running, and cooling down is a pattern of activity that is nested in the broader pattern of engaging in aerobic exercise that is nested the broader pattern of living healthfully (Baum, 1995). Another example is getting to work, working, holding a job, and making a living (Baum, 2002; see Baum, 2004a, for an example of a pigeon on a concurrent schedule of reinforcement; see also Vyse, 2004). These patterns of activity—from the briefest to the most extended—are lawful in their own right, maintained by their correlated contingencies and metacontingencies of reinforcement.

Nesting may not be unique to today's molar account; it may be found in Skinner's molecular explanation too, but not in those terms. See, for example, Skinner's (1938, pp. 102–108) analysis of the functional unity of the individual "members" of the chain of responses involved in a rat's approaching a bar, pressing it, and eating the food that is produced. Each member is a pattern of activity nested in a more extended pattern of activity. In any event, more conceptual clarification is needed concerning the concept of nesting. It may be another significant point of congruence between behavior analysis and ecological psychology.

### MOLAR BEHAVIOR AGAIN

Returning to Krechevesky's insights about *The Behavior of Organisms*, we find that the book's other reviewers did not see any

<sup>6</sup>In today's molar–molecular distinction, the molar and molecular accounts need not be mutually exclusive (Moore, 1983; Morris, Higgins, & Bickel, 1982). Behavior may be lawful and orderly on multiple scales of analysis (Hineline, 2001).

relations between it and the precursors of ecological psychology. As for why, Bill Verplanck (1954) offered the following observation:

Because of the existence in Sherrington and Pavlov of sets of data of the kind [Skinner] believes are needed [for his system], he has adopted many of their terms [e.g., reflex, stimulus, response] and applied some of their laws in defining his area [e.g., respondent behavior]. As a consequence, he has been misinterpreted. In his choice of terminology, Skinner has assured that his works and those of his followers will be read easily by the followers of Hull and Guthrie and only with emotion, if not with difficulty, by those who have selected the organismic-field-Gestalt-force family of words to work with. (p. 307)<sup>7</sup>

This may be another source of Heft's misunderstanding of Skinner, but misunderstanding runs in both directions. Behavior analysts have had difficulty with "the organismic-field-Gestalt-force family of words," leading us to interpret ecological psychology's cognitive terms and talk as being mentalistic, when perhaps they are not.

Not all ecological psychologists misunderstand Skinner on these points, notable among them, Alan Costall (2004). Costall has pointed out that Skinner respects Darwin's emphasis on the mutual adaptation and coordination between the organism and its environment, citing the following passage from *The Behavior of Organisms* (Skinner, 1938):

Behavior is what an organism is doing—or more accurately what it is observed by another organism to be doing. But to say that a given sample of activity falls within the field of behavior simply because it normally comes under observation would misrepresent the significance of this property. It is more to the point to say that behavior is that part of the functioning of an organism which is engaged in acting upon the environment or having *commerce* [emphasis added] with the outside world. (p. 6)

Perhaps this is what Heft means by an active organism. Costall (2004) continued: "In addition, Skinner's theoretical terms such as operant and reinforcer embody relational

thinking because they are defined reciprocally and functionally. Perhaps Skinner's clearest statement of the 'internal' relation between animals and their environments occurs in his accounts of his own scientific activity. The so-called Skinner boxes were designed, in effect, to 'reflect' the animals they were to contain:

"A laboratory for the study of behavior contains many devices for controlling the environment and for recording and analyzing the behavior of organisms. With the help of these devices and their associated techniques, we change the behavior of an organism in various ways, with considerable precision. But note that the apparatus was designed by the organism we study, for it was the organism which led us to choose a particular manipulandum, particular categories of stimulation, particular modes of reinforcement, and so on, and to record particular aspects of its behavior (Skinner, 1961, p. 543)." (p. 6; see also Costall, 2006)

Some other ecological psychologists see these affinities, too, but do not acknowledge them. For instance, in preparing this essay, I described the affinities to an ecological psychologist who agreed with them, but said he could not say so publicly. Presumably, he would lose credibility among colleagues for whom Skinner is the straw person in their destructive programs for advancing ecological psychology. This is a political constraint on the relations between the two fields.

#### SKINNER'S BEHAVIORISM IN ECOLOGICAL COGNITIVE PSYCHOLOGY

Heft is not alone in misunderstanding Skinner's behaviorism, of course. He is in good company, as the following anecdote relates. The third edition of Leahey's (1992a) *History of Psychology* included a section titled "The Strange Death of Radical Behaviorism" (pp. 464–486). The death was strange, Leahey noted, because radical behaviorism had not died. This section also contained a subsection, titled "Critique of Cognitive Science" (pp. 467–468), which reviewed Skinner's (1985) article, "Cognitive Science and Behaviorism," which criticized cognitive science and offered radical behaviorism in its place. Here, Leahey observed:

Skinner's plea may be answered, for there appear to be grounds for reconciliation between one form of cognitive science, Neisser's (1984 [actually, 1985]) "ecological psychology," and radical behaviorism. Like the radical

<sup>7</sup>This may not be fair to Guthrie. He distinguished between bodily *movements* and behavioral *acts* (Guthrie, 1935, pp. 27–28; 1940).

behaviorist the “ecological cognitive psychologist [begins]...with a careful description of the environment and people’s ordinary activities within it.” Like the radical behaviorist, the ecological cognitive scientist “take[s] the environment seriously.” Like the radical behaviorist, “ecological psychologists are generally reluctant to construct models or to postulate hypothetical mental events.” Like the radical behaviorist, the ecological psychologist believes that “introspection does not reveal [the] structures” in the environment to which people respond. Neisser does not discuss the parallels between his description of ecological psychology and radical behaviorism, but they are there. (p. 468)

This was astute. The passage from Neisser (1985) that was the basis, in part, for Leahey’s observation, addressed the ecological approach more focally:

The ecological approach is the one [as opposed to the information-processing approach to cognition] that begins by taking the environment seriously, focusing on cognition in ordinary settings. To study concept formation, one begins with an analysis of everyday concepts; to study perception, one begins with visual control of action in cluttered environments; to study memory, one begins with the kinds of things people ordinarily remember. Such an approach usually forces the researcher to look at temporally extended stimulus variables and behavior that occurs over time, rather than at the brief flashes and momentary responses popular in information processing research. It also implies a concern with cognitive development and cognitive change, including both the changes due to age and those that come with the acquisition of skill—i.e., with learning itself. Most important, perhaps, is that ecological psychologists are generally reluctant to construct models or postulate hypothetical mental events. Too often, they believe, those hypotheses have substituted for careful analysis of the real environment and the real events that occur in it. (p. 21)

Neisser did not explicitly embrace a mutualist perspective or entirely rule out mediational theorizing, but we can see a likeness among his ecological approach, Heft’s (2001) ecological psychology, and Skinner’s (1977, 1985, 1990) radical behaviorism.

The next two editions of Leahey’s book also addressed the strange death of radical behaviorism (Leahey, 1997, pp. 454–457; 2000,

pp. 528–531), but omitted the passage concerning Neisser (1985). This may have been due to my pointing it out to Neisser when I invited him to participate in a 1995 APA symposium titled, “Theories of Direct Action: Alternative Foundations for Cognition, Development, and Behavior,” which Gregory Kimble chaired. The symposium included papers by Costall (1995) on Gibson’s theory of direct perception, Michael Watkins (1995) on his own direct memory perspective, Susan Oyama (1995) on developmental systems, and me on behavior analysis (Morris, 1995). Leahey was the discussant.<sup>8</sup> Neisser had declined my invitation to participate, writing:

Thanks, but no thanks. Despite what Leahey says, my views are really far removed from those of “behavior analysts.” Although I do think that *perception*—well, some forms of perception—can occur without mediating mental representations, I am sure that such representations do exist and play a role in many other mental activities—from recognition to recall to imagery to thought... For me these are open empirical questions, rather than matters to be settled by Skinner’s a priori methodological fiat. There are other differences too—deep ones, decades old—that would take a long letter to explain. Anyway, I must decline your kind invitation. Good luck with the symposium. (Ulrich Neisser, personal communication, October 20, 1994; see Brewer, 2001, on the development of Neisser’s thinking and the influence of Gibson on it.)

There are also decades-old points of congruence and complementarity that take a long time (and a long review essay) to explain. The latest edition of Leahey’s (2004) book also does not help. It no longer addresses the strange death of radical behaviorism, which suggests that Leahey thinks it is dead, even as it continues to prosper on many measures (see Malott, 2008; Roediger, 2004).

#### PROVINCES OF KNOWLEDGE

I conclude this section with a final observation about Heft’s misunderstanding of Skinner and perhaps ours of ecological psychology. The misunderstandings might arise because

<sup>8</sup>The symposium was a disappointment. The audience was intensely interested in the material on perception, memory, and developmental systems, but dismissive of behavior analysis, even as I presented it, as a generic version of direct realism.

Skinner and ecological psychology address, in part, different provinces of knowledge. Skinner largely addressed the effects of contingencies (e.g., the three-term contingencies) and in the process controlled for context (e.g., history, motivating operations). Ecological psychology, in contrast, largely addresses the effects of context and in the process controls for or overlooks contingencies. As a result, Skinner's science and ecological psychology appear to dismiss each other's province of knowledge, yet of course they do not. In controlling for each other's province, they implicitly acknowledge that it plays a role in behavior. In behavior analysis, context is today increasingly analyzed in theory and research on behavior's phylogenetic and ontogenic history (e.g., Pipkin & Vollmer, 2009; Schneider, 2007; Skinner, 1966; Wanchisen & Tatham, 1991) and current context (e.g., motivating operations, conditional stimulus control, transformations of function; see, e.g., Hayes, Barnes-Holmes, & Roche, 2001; Michael, 1993; Sidman, 1994). Ecological psychology might consider contingencies more seriously.

#### COGNITIVE TERMS AND TALK: SELECTIVITY

I now turn to my concern about Heft's cognitive terms and talk. The problem, of course, is not with the terms and talk per se, but with Heft's wavering on their status as explanatory constructs or descriptive concepts. His term *selectivity* provides a useful example: In his ecological psychology, selectivity is an attribute of the organism, whereas in Skinner's (1966, 1981) science, it is an attribute of the environment. The differences in the two uses, however, may be more apparent than real in their ontologies and epistemologies, and in the syntax by which they describe their subject matter.

#### ONTOLOGY, EPISTEMOLOGY, AND SYNTAX

##### *Ontology*

As Heft uses selectivity, it is seemingly an organism-based process and, as such, is mentalistic (or mentalistic-sounding). As behavior analysts use selection, it is seemingly an environment-based process and as such, is mechanistic (or mechanistic-sounding to Heft). Neither mentalism nor mechanism,

however, necessarily follows from these uses. In ecological psychology, organisms are not self-actional agents that freely select their environments. Instead, they select environments that have been prepared to be selected by species phylogeny and individual biological and behavioral ontogeny. The environment's history with the organism is determinant, not the organism's selectivity. This history is the source of an organism's response functions for the environment, for example, the function of running, as in running for a train or training for a run (i.e., different operants).

Similarly, in behavior analysis, the environment does not mechanically select behavior. It selects behavior that has been prepared to be selected by species phylogeny and individual biological and behavioral ontogeny. Behavior's history with the environment is determinant, not the environment's mechanical action. This history is the source of the environment's stimulus functions for behavior, as in Heft's example of the functions of a book as a paperweight or doorstop (i.e., different reinforcing or discriminative functions). As Skinner (1974) noted, "...perception is in a sense purposive or intentional [i.e., selective]. A person is not an indifferent spectator soaking up the world like a sponge" (p. 82; see also pp. 61-63).

Heft's stance on the theoretical status of selectivity actually wavers. On the one hand, he describes organisms as "purposive" and "active agents" with "goals and interests," as though his terms and talk were explanatory. On the other hand, he writes that "action... cannot be studied without reference to the environment" (p. xiv), that ecological psychology offers "an account of the functional relation between the properties of the environment and an individual's actions" (p. 7), and that it looks at "the traditional psychological topics of perceiving, acting, and knowing as activities of ecosystems rather than of isolated animals" (p. xiv). Here, his cognitive terms and talk are used in a descriptive, not an explanatory, "language game" (Wittgenstein, 1953, 1958; see Deitz & Arrington, 1984). In fact, given Heft's nonmediational stance, selectivity cannot be an organism-based process. It is, instead, a property of the *relation* between an organism and its environment, which is also the behavior-analytic stance on psychological terms and talk (Skinner, 1945, 1974). In the end, Heft's and Skinner's



ontologies may be compatible, depending on how we read Heft (and how we read Skinner).

### *Epistemology*

If their ontologies are compatible, then their disagreements may be epistemological, with Heft focusing on the organism's history with the environment (i.e., organisms select) and Skinner focusing on the environment's history with the organism (i.e., environments select). The organism's history and the environment's history, however, are not different histories. They are the same history viewed from different vantage points. Heft begins with an organism that selects the environment, whereas Skinner began with an environment that selected behavior (see Catania, 2007, p. 38). Sometimes, though, Skinner began with the organism's selectivity, as in, "Man acts upon the world and changes it, and is changed in turn by the consequences of his actions" (Skinner, 1957, p. 1) and "Individuals shape each other's behavior by arranging contingencies of reinforcement, and what contingencies they arrange and hence what behavior they shape, are determined by the evolving social environment, or culture, responsible for their behavior" (Skinner, 1957, p. 48). When analyzing behavior structurally, we might meaningfully ask which comes first—the organism or the environment. The question may depend on our goals.

When analyzing behavior functionally, though, the question of which comes first—the organism or the environment—is meaningless. Neither the ecological nor the behavior-analytic unit of analysis is reducible to its participants acting independently and mechanically on each other. The unit is a transactional, mutual, or functional relation that evolves over time (D. K. Palmer, 2003, 2004). In this account, the terms and talk of selectivity are ontology-distorting if they imply uncaused causes in either the organism or the environment. Moreover, functional relations are not due to one participant independent of the other, but are the result of their history in an ever-evolving stream of behavior. Given a sympathetic reading of Heft's from-the-organism terms and talk, his epistemology may be congruent with Skinner's, given a sympathetic reading of Skinner's from-the-environment terms and talk.

### *Syntax*

This leads to a third reason why the differences in Heft's and Skinner's accounts of selectivity may be more apparent than real: Their terms and talk are distorted by the causal implications of their syntax (see Heline, 1980, 1983, 2003). That is, their differences may lie more in the surface structure of their descriptions of the relations between organisms and their environments than in the deep structure of their explanations of those relations. Even though both may concede that neither the organism nor the environment is a selective thing-in-itself, they are nonetheless enmeshed in syntax that leads them to parse the relation as being from one or the other. Two quotations from Skinner illustrate the issue: (a) "...a person does not act upon the environment, perceiving it and deciding what to do about it; the environment acts upon him, determining that he will perceive it and act in special ways" (Skinner, 1972, p. 352) and (b) "The difference between the cognitive and behavioral approaches is perhaps best seen in the field of perception. For cognitive science, the direction of action is from organism to environment...In a behavioral analysis, the direction is reversed. At issue is not what an organism sees but how stimuli alter the probability of its behavior" (Skinner, 1985, p. 95). However, as noted above Skinner (1957, pp. 1, 18), himself, engaged in from-the-organism terms and talk.

In the end, Heft's and Skinner's syntactical practices are ontology- and epistemology-distorting. Heft's from-the-organism syntax makes ecological psychology seem organismically-based, context-independent, and mentalistic to behavior analysts, while Skinner's from-the-environment syntax makes behavior analysis seem environmentally-based, context independent, and mechanistic to Heft. Both Heft's and Skinner's syntax distort their ontologies and epistemologies, making differences in their explanations more apparent than real.<sup>9</sup>

<sup>9</sup>Another source of these difficulties may be that ecological psychology's subject matter is expressed in terms of functional relations between *organisms* and their environments, perhaps stemming from the field's origins in evolutionary biology, whereas the behavior-analytic subject matter is expressed in terms of functional relations between *responses* and stimuli, perhaps stemming from its origins in experimental physiology.

## TOLMAN, PURPOSE, AND SELECTIVITY

Let us not forget Edward C. Tolman, another of Holt's students. Although Tolman was not a source of ecological psychology, Heft refers to his purposive behaviorism for correct thinking about selectivity. Tolman's thinking, however, wavered, which may be another source of Heft's wavering in his cognitive terms and talk. In the 1920s, purpose—another word for selectivity—was a descriptive concept for Tolman, that is, an intervening variable in MacCorquodale and Meehl's (1948) terms. It was an ordinary-language description of molar behavior (Tolman, 1925; see also Good & Still, 1986). By the 1930s, however, Tolman began to waver over whether purpose was descriptive or explanatory, the latter of which made it a hypothetical construct. Skinner (1987b) noted this in his review of Smith's (1986) *Logical Positivism and Behaviorism: A Reassessment of the Alliance*, as follows:

Neorealism, as Smith says, left Tolman in an embarrassing position. Could purposes and cognitions be seen in the behavior of another organism or were they internal "determiners" of behavior? His equivocation was clear in *Purposive Behavior in Animals and Men* [Tolman 1932]. "Within a single paragraph," Smith points out, "he describes purposes and cognitions as 'immanent' in behavior...[intervening variables] and on the other hand as 'determinants' or 'causes' of behavior that are 'invented' or 'inferred' by observers' [hypothetical constructs]. (p. 207; Smith, 1986, p. 90)

In the end, Tolman did not equivocate. Purpose became a hypothetical construct (e.g., Tolman, 1949; see also Amundson, 1983), one of whose legacies is today's cognitivism (Morris, Higgins, & Bickel, 1982) which, ironically, is what Heft seeks to replace with ecological psychology.

## CONCLUSION

In this essay, I have reviewed some past and present relations between ecological psychology and Skinner's behaviorism, and their promise for the future of psychological science. I did so mainly in the context of Heft's *Ecological Psychology in Context*, even if it is not the best means to this end. His history and system of ecological psychology are seemingly presentist and selective, and he misunder-

stands Skinner's science and system. Even so, his book provides an occasion for comparing and contrasting the two fields and noting some complementarities.

## COMPARISONS, CONTRASTS, AND COMPLEMENTARITIES

The systems are alike in their nonmediational and nonrepresentational accounts of the relations between organisms and their environments; in their opposition to physiological reductionism; and in their molar perspectives on behavior. More subtle affinities may also be found in their forms of phenomenology (e.g., Giorgi, 1970; Merleau-Ponty, 1962; see also Day, 1969b; Delprato, 2006; Giorgi, 1975; Kvale & Grenness, 1967); appreciation of developmental systems theory (e.g., Moore, 2001; Oyama, 2000; see also Lickliter, 2000; Midgley & Morris, 1992; Schneider, 2007); focus on action as their units of analysis (i.e., on what gets done or accomplished; see Lee, 1995, 1998); and a systems or integrated-field perspective (see Delprato, 1987; Midgley & Morris, 1988; Timberlake, 1993).

Not only are behavior analysis and ecological psychology alike in these regards, but they are also complementary (a) as a natural science and a natural history of behavior and (b) in their provinces of knowledge—contingencies and context. What behavior analysis offers are principles applicable to all human behavior everywhere. Yet, when considered independent of human history, they are closer to biological processes than to psychological ones. In turn, what ecological psychology offers are historically situated accounts of everyday human behavior. Yet, when considered independent of natural science, they are closer to humanistic explanations than to scientific ones. Integrating and allying behavior analysis and ecological psychology may strengthen them individually, as well as unify and transform psychology as a life science. As a life science, psychology could then address, for example, not only conditioned reinforcement as a principle in a natural science of behavior, but also values and aesthetics as the situated products of human natural history – biological, behavioral, and cultural.

Where behavior analysis and ecological psychology contrast most is in their terms and talk. Heft's seem mentalistic, but this is inconsistent with ecological psychology's non-

mediational theorizing. Even if not mentalistic, they privilege the organism, which is inconsistent with ecological psychology's organism-environment mutualism. In comparison, Skinner's terms and talk seem mechanistic, which is inconsistent with radical behaviorism's antiessentialism. Even if not mechanistic, his terms and talk privilege the environment, which is inconsistent with radical behaviorism's focus on functional relations. Stimuli, for instance, are defined in terms of their functions for responses (e.g., eliciting, discriminative, reinforcing, evoking), not independently of them.

In any event, a science of everyday human behavior requires both natural history and natural science. With both, an alliance and integration of behavior analysis and ecological psychology may promote psychology's unification and transformation.

#### ALLIANCE AND INTEGRATION

These possibilities notwithstanding, behavior analysis and ecological psychology are not prepared, as yet, for alliance or integration, much less to unify psychology or transform it into a life science. As for their alliance, the fields are barely cognizant of each other (e.g., cross-citations are rare, but see Hackenberg, 1998; Turvey, Solomon, & Burton, 1989). Moreover, each likely views itself as ultimately sufficient for understanding everyday human behavior, dismissing the contributions of the other and any complementary relations between them.

As for their integration, ecological psychology has not evolved to the point where this is likely, even if possible (see Krantz, 1977). Heft's vision for the field is only a possibility, not an achievement. Ecological psychology remains underdeveloped (Good, 2007). For instance, a cursory review of the past 20 volumes of *Ecological Psychology* shows that its research is restricted largely to perception, especially to visual perception. Little attention has been paid to the other domains of human natural history (e.g., social, emotional). Moreover, even where these domains share fundamental conceptual commitments (e.g., mutualism), they seem to lack unity in their terms and talk, and thus appear to be minisciences unto their own, as they are in psychology. Finally, ecological psychology's research practices differ little from those of experimental

cognitive psychology, with its contrived laboratory methods for understanding human natural history. Its participants observe, make judgments about, and respond to scenes and events, but do not participate in them (Neisser, 1980; A. Costall, personal communication, August 22, 2008). The question is whether behavior analysis has evolved to the point that its integration with ecological psychology is possible. Its conceptual commitments suggest this may be the case, at least in principle; however, it will have to look beyond ecological psychology's terms and talk, as well as research methods, which may be difficult in practice.

#### UNIFICATION AND TRANSFORMATION

Even if behavior analysis and ecological psychology were to become allied and integrated, their ability to unify and transform psychology into a life science is severely challenged by their numbers and orientation. In number, the Association for Behavior Analysis International (est. 1975) has about 6,000 members, while the International Society for Ecological Psychology (est. 1981), which publishes *Ecological Psychology*, has fewer than 350.<sup>10</sup> In contrast, the American Psychological Association (est. 1892) has nearly 150,000 members and the Association for Psychological Science (est. 1988) has about 20,000.

In orientation, experimental psychology is largely what Heft calls it—mentalistic and reductionistic—in contrast to the naturalism of ecological psychology and behavior analysis. As such, psychology will remain deeply resistant to both fields (Good, 2007; Skinner, 1974, 1990). However, the two fields are not alone in their critiques of cognitivism or as alternatives to it, as related, respectively, in Costall and Still's (1987) *Cognitive Psychology in Question* (see Morris, 1989) and Still and Costall's (1991) *Against Cognitivism: Alternative Foundations for Cognitive Psychology* (see Leudar & Costall, 2008; Osbeck, 2009). These alternatives are attracting increasing interest in the

<sup>10</sup>For a related perspective, see the interdisciplinary, but eclectic field of environmental psychology (Stokols & Altman, 1987) and the *Journal of Environmental Psychology* (est. 1980). Ecological psychology and environmental psychology, however, do not engage in much cross-citation (but see Reser's 2007 review of Heft's book). For a behavior-analytic perspective on environmental psychology, see Geller (1987).

behavioral, social, and cognitive sciences, while still other alternatives are emerging (Delprato, 2006; Morris, 2003; see also Malone, 2009). This family of alternatives may eventually become a basis for a real revolution in psychology (see Moore, 1995). As such, where behavior analysis addresses human natural history as a subject matter, it might keep an eye on these alternatives—ecological psychology among them.

For a revolution to occur, though, these alternatives may have to keep more than just an eye on each other. They may also have to engage each other at their shared boundaries. One of the boundaries is research on everyday human behavior, where the strategies and tactics of human behavioral research (Johnston & Pennypacker, 2008), especially those in applied behavior analysis (Cooper et al., 2007), provide a basis for reliable, direct observations of behavior in naturalistic settings, as well as functional analyses of the relations between behavior and its environment (Bijou, et al, 1968; Iwata et al., 1982/1994). The shared boundaries of these fields may be where interdisciplinary research is most progressive. As Frances Mechner (2008) noted in his review of Eric Kandel's (2006) *In Search of Memory: The Emergence of a New Science of Mind*, research at shared boundaries

makes sense when we consider that demarcations and boundaries of scientific disciplines do not exist in nature – they just reflect primitive efforts to categorize a natural universe we have barely begun to understand. Since there is only a single natural universe for scientific disciplines to explore, the expansion of their domains within this universe must inevitably bring them into increasing contact. (p. 236)

Allying and integrating behavior analysis and ecological psychology may not only expand both fields, but also encourage a unification and transformation of psychology that stands a chance of accounting for the richness and nuance of everyday human behavior.

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