*Behavior and Philosophy*, *38*, 61-81 (2010). © 2010 Cambridge Center for Behavioral Studies

# REPLY TO COMMENTARIES ON FIELD & HINELINE'S "DISPOSITIONING AND THE OBSCURED ROLES OF TIME IN PSYCHOLOGICAL EXPLANATIONS"

Philip N. Hineline
*Temple University*

ABSTRACT: Six of the seven commentaries expressed basic agreement with our characterization of dispositioning as a typically unacknowledged, pervasive, and often problematic explanatory practice. One of these (Glenn) situated our own interpretive activity within the interpretation itself. Two others (Ruiz and Gergen) advocated expanding the scope of our analysis—Ruiz addressing a dilemma that confronts feminist advocacy, and Gergen surveying a broader range of societal issues. Petrusz and Turvey, as well as Gergen, delineated affinities between our behavior-analytic stance and their own preferred viewpoints (ecological perception theory and social constructionism, respectively). Palmer and Shimp focused on our discussion of temporal scales of analysis, both favoring reductionistic accounts anchored upon the time-scale of direct observation/experience. In contrast, Field and I argued that phenomena on particular scales, whether small or large, can potentially stand on their own; even constraint by process at one scale upon process at another does not mean that explanation in terms of the constraining process can be elaborated to account for the constrained one. The lone thoroughgoing dissenters with our essay (Fletcher and Kerr) found nothing wrong with pervasive dispositioning, nor did they recognize what we found to be problematic constraints of language patterns, including the reification of dispositional terms.
*Key words:* dispositioning, attribution theory, scales of analysis, bipolar constraints in language, feminist theory, social constructionism, generative theory, ecological psychology, dynamical systems theory, molar theory, nested relationships, Whorf

## Background and Summary of Our Essay

The inspiration for our essay was direct experience with discourse that involves conflicting psychological viewpoints, as well as with introducing students and colleagues to behavior analysis as an interpretive system. For some, it immediately "clicks," and they readily speak in ways that seamlessly incorporate behavior-analytic concepts and principles. These are in the minority, however. More commonly, students will struggle by attempting to translate those concepts into more familiar terms and phrasing, with transmogrifying results. Colleagues often will resist the approach as uncongenial: On one occasion when a person told

AUTHOR'S NOTE: Please address correspondence to Philip N. Hineline, Department of Psychology, Weiss Hall, Temple University, Philadelphia, PA 19122; Email: hineline@temple.edu

me he found a behavioral approach to be simplistic, I prevailed upon him to listen to a synopsis of a few foundational concepts. It wasn't long until he deemed it "too complicated." "Complicated instead of simple" didn't change his stance, however, and thus the roots of such disagreements between viewpoints are other than what is ostensibly argued about. Something unrecognized and interesting is likely to be involved.

For my own part, I had found some success in addressing the problem by characterizing behavior-analytic talk as a distinct dialect of English (Hineline, 1980), and then a social psychologist colleague, Louise Kidder, introduced me to Jones & Nisbett's (1971) seminal article on attribution theory. This subsequently provided a foothold for examining the early evolution of B. F. Skinner's prose, and thus the origins of the behavior-analytic dialect (Hineline, 1990). Attribution theory was clearly within the ballpark of my concern, but the manner in which it was typically discussed seemed curiously off the mark until it dawned on me that when attribution theorists identified a bias as "the fundamental attribution error," the accompanying theoretical prose typically illustrated that bias even when discussing it! It would have been a fairly straightforward matter to write an essay that simply identified and documented that ironic fact, but a more constructive approach concerned the question: How could that happen? How could the pervasive, biased pattern, unconcealed before the eyes of anyone reading the literature, go unnoticed as present in the prose discussing that kind of prose? That led Doug Field and me to attempt placing the phenomenon in the broader context of interpretive language, irrespective of particular academic disciplines, and the role of time in explanation became a coordinate theme throughout our essay. The problem is not about attribution theorists; after all, to say so would be to engage in dispositioning. Besides, the problem is more subtle and general than that would imply.

Our essay began by explicitly situating our own interpreting within the interpretation—giving a nod to philosophical traditions of explanation as well as to a few empirical studies of interpretive talk, several of which identify contiguity between events as a strong determinant of peoples' judgments of causation. This was followed by a brief survey of struggles to accommodate remote causation within the natural sciences, with a few illustrative quotations, thus making the point that our issues of concern are not peculiar to the behavioral sciences. Then, with a brief sketch of the history of attribution theory, delineating some ways in which the situation/disposition distinction evolved, our attempt at a substantive contribution began with an introduction of *dispositioning*:

> Dispositional explanations of behavior can be occasioned by clearly and directly identifiable conditions within, or properties of, the behaving individual. We have no argument with these, provided they are not identified only through the behavior they are said to explain—but dispositional terms are often invoked when such conditions do not obtain. In situations that lack obvious contiguous causal events it is common practice to invoke verbal constructs that infer or assert the presence of reified entities within the acting persons and to assign causality to those entities. For these practices we have coined the term

> *dispositioning*, defined as the imputing (or more explicitly, hypothesizing) of an entity that is either given explicit status of causing behavior or that easily acquires such status by virtue of its patterns of use. The origins of the entity are usually not specified, thus lending it the character of an autonomous agent. (Field & Hineline, 2008, p. 13)

We then drew upon several sources as well as our own observations to identify types of occasions where dispositioning typically occurs: consistency across individuals, consistency across situations, implicit historical origins, and implicit future consequences. Noting that those various occasions of dispositioning involve temporal dispersion of functionally related actions and/or events (functional, in the sense of $y = f(x)$), we proposed three categories:

**Integrative dispositioning**, which occurs when temporally extended or dispersed events are collapsed into a single locus by invoking a reified entity, which then functions in explanations as an aspect of an organism that seems present at every moment (e.g., personality labels).

**Mediational dispositioning** occurs when inferred entities are explicitly characterized as bridging temporal gaps that are discerned when remote events clearly affect behavior (e.g., memory traces, moods).

**Teleological dispositioning** occurs when an action is interpreted by identifying its likely, often remote, consequences. We identified two distinct types:

In many cases, past consequences of the individual's similar actions affect the present action (e.g., desires, intentions).

In other cases, the action is an explicit reaction to verbal statement and not to the individual's having previously engaged in the behavior of concern nor experienced its putative consequences (e.g., avoiding depletion of the ozone layer).

Next came our own, non-dispositional account of dispositioning itself, and of scientific explaining more generally (for a more comprehensive version see Hineline, 1992).

We briefly described some notable prior attempts to explicitly encompass extended time relations within psychological interpretation: Bem (1967, 1972) in a theory of self-perception; Funder (1982) in distinguishing situational and dispositional attributions as concerning levels of analysis, but still engaging in what we call teleological dispositioning; Fiedler and colleagues (Fiedler et. al., 1989, Fiedler & Semin, 1992) in identifying linguistic practices as providing structural constraints, including the possibility "that some causal schemata and attributional biases may be located in language as a collective store outside the memories of individual people" (Fiedler & Semin, 1992, p. 81). We then turned to address the pervasiveness of dispositioning in formal psychological theory, which we often found to be even more rigidly dispositional than vernacular language. Fletcher (1995) discussed the overlap between folk psychology and scientific psychology, and advised caution in adopting features of the former into the latter. At the same time, we found that he seemed not to recognize his own dispositional bias (more on this below, since Fletcher and Kerr (2009) supplied commentary on our article). We then identified various examples, with quotations, of dispositioning within attribution theory. Especially notable among these is an article by Choi, Nisbett, and Norenzayan (1999), where differences found in a cross-cultural comparison are attributed to dispositions of people within the cultures rather than to the cultural practices themselves. Next, in asking whether dispositioning is truly an error, we identified some its adaptive functions in ordinary language, as well as maladaptive functions in both vernacular and scientific usage.

The final section of our essay concerned attempts to accommodate extended time within psychological interpretations, as well as attempts to break out of the bi-polar constraint (noun–verb, cause–effect, independent variable–dependent variable) of interpretive language, which forces a tension between situational and dispositional accounts. We noted that the problem of temporally-extended processes has been addressed by several behavior analysts, and also by proponents of Gibsonian "ecological perception theory." Funder (1982), mentioned above, also attempted to make multiply-scaled processes intuitively comprehensible by addressing spatial relationships in visual perception in a manner quite like our illustrative examples from physical mechanics. Mischel's research on "self-control" (e.g., Mischel, 1974) addressed extended temporal relations in a way that challenged the traditional interpretations of self-control as concerning "person characteristics," while his interpretations of that research still illustrated meditational and teleological dispositioning (Mischel, 1974, p. 288). More contemporary accounts continue to illustrate the tension between recognizing the interpenetrability of organism and environment (e.g., Lewontin, Rose, & Kamin, 1984), while failing to write in a manner that would be consistent with that interpenetrability.

As relevant to the language of causation, *per se*, we sketched the Whorfian position, especially Schultz's (1990) reanalysis, which focused upon the problem of arguing, within English, that there's something amiss with English. This frames our problem in a new light—that of Bakhtin's *heteroglossia*—cooked down in scientific language to bi-glossia, whereby members of the English-speaking community effortlessly shift back and forth between dispositional and situational interpretations. The tension therein arises over which locutions are appropriate to which occasions. Another whose work bears some affinity to our effort is Resnick (1997), who identified multiple scales as having more radical implications, suggesting an alternative worldview, a conception that embraces "decentralized interactions and feedback loops. . .new ways of viewing the world, new ways of thinking and new ways of knowing" (pp. 13 & 21). Ainslie's (2001) reconception of the self in *self-control* is radical in a different way, conceptualizing the self as explicitly multi-scaled and comprised of nested, nonlinear relationships that concern "interests" (i.e., consequences with varying values), "more like a population of bargaining agents than like a hierarchical command structure." Ainslie suggests that the unity of those agent/interests (combined actors/consequences) arises from the fact that they are, in effect, locked in a room together (2001, p. 43). And finally, although she appears a bit before the end of our essay, our heroine is Oyama (1985, 2001), a developmental biologist who, more than any other author we encountered, has consistently attempted to integrate environment-based and organism-based theorizing through her conception of developmental systems comprised of *interactants*, which are understood through multiple scales of analysis.

## The Commentaries

Several of the commentaries expressed substantial agreement with the main themes of our essay. **Sigrid Glenn ("On Explaining Behaving")** (2009) is a previous mentor of Doug Field and also is acquainted with an early draft that contained a good deal of Doug's unedited work. From that perspective, Glenn characterizes the extended trajectory of our writing project in a way that furthers our attempt to place our interpreting within the interpretation. She sees our essay as a complex interweaving of two themes—on one hand, the patterns and practices of dispositioning, and the roles of time in explanatory prose. On another hand it began as the balanced product of two authors' repertoires, each with its own history, but with that balance shifted by Doug's untimely death. Glenn's comments are especially well attuned to our agenda of portraying the problem inclusively, as a problem of getting beyond limitations of perception and of language that we all share.

**Maria Ruiz ("Beyond the Mirrored Space: Time and Resistance is Feminist Theory")** (2009) identifies specific practical and political implications of the dispositional bias that we focused upon. It had not occurred to us, but it is obvious once she points it out, that adopting the "Battered Woman Syndrome" as basis for a legal defense for women who kill their partners after years of abuse sets in motion a dispositonally-based precedent that ultimately diminishes the status of such women. Introducing the conception of a quasi-medical syndrome provides an accommodation to the legal requirement for "an immediate and unavoidable danger of death or grave bodily harm." However, as Ruiz details, the requirement for a contiguous cause justifying the woman's action not only "obscures the role of extended behavioral relations and temporally distant events related to the behavior being explained" (p. 142), it diminishes her status to that of a diseased person. This, then, is a telling example of the practice that we identified in general terms, that of blaming the victim.

**Kenneth Gergen ("Pragmatics and Pluralism in Explaining Human Action")** (2009) proposes expanding the implications of dispositioning and multi-scaled analyses well beyond the domains that we or Ruiz addressed. He advocates addressing the implications of our viewpoint for sustaining and transforming society, enhancing social justice, and enhancing the potentials for research and practice. To have done so would have required a book instead of our long essay. But if we had written that book, the chapters addressing those expanded priorities would have acknowledged the origins of environment-based interpretation in the work of B. F. Skinner, who devoted hundreds of pages to the implications of behavioral theory for social issues, including *Walden Two*, *Science and Human Behavior, Beyond Freedom and Dignity*, as well as essays such as "The Ethics of Helping People" and "What is Wrong with Daily Life in the Western World?"

(Skinner, 1948, 1953, 1971, 1975, 1986). Skinner's many critiques of "mentalism" can be understood as attempts to devalue the practices of dispositioning, thus to clear the field for widely ranging applications of environment-based theory and practice. Our analysis suggested why his attacks upon mentalism proved to be a largely unsuccessful strategy: With repertoires shaped since childhood by the surrounding culture, the vast majority of individuals, before they encounter formal psychological explanation, have already been taught extensive explanatory repertoires that make dispositional explanations "sound natural" even when close examination would reveal them to be vacuous.

Gergen embraces *social construction* as an approach to psychological explanation. As Ruiz (1995) has described in detail, that approach shares much with the behavior-analytic viewpoint. Indeed, Skinner's (1950) "Are Theories of Learning Necessary?," an early critique of business as usual in psychological theorizing, has much in common with critiques that advocate constructionist approaches. Both behavior analysis and social constructionism explicitly include the activity of interpreting within the interpretation (Hineline, 1992). While I have found some aspects of social constructionism to be congenial, I balk at its hints of "anyone's explanation is as good as anyone else's." Explaining is behavior, to be accounted for in terms that also account for behavior that is explained; one cannot stand outside the system. Nevertheless, a term such as "objectivity" need not be used in ways that are self-serving or meaningless. As I understand it, we agree that the solution to this is to be found among pragmatic criteria. Indeed, the anti-dispositional stance tends to be more pragmatic than the dispositional one, for while the fundamental question posed for organism-based theory is typically framed as "How does the organism do what it does?," the fundamental starting point for an environment-oriented approach is "What does the organism do under what circumstances?" Still, the range of pragmatic criteria to be entertained is itself a matter for some debate.

Despite a general stance of agreement, Gergen faults some hints of "an empiricist foundationalism" embedded within our essay. The examples that he identifies, such as our reference to "objectively gathered data," can be addressed within a behavior-analytic account (e.g., Hineline, 1992) without claiming to achieve context-free interpretations. Relatedly, many people (including some behavior analysts) understand an environment-based stance—or, even more broadly, a scientific stance—as necessarily deterministic. To be sure, we are appropriately known for criticizing explanations of human behavior that appeal to autonomous free will. However, a behavior analytic approach requires only the observation that we find ourselves in an orderly world—that behavior–environment relations are included in that order. Even the skill of behaving randomly has been demonstrated to be orderly and teachable (Neuringer, 2002). Furthermore, in striving toward a more humane society that supplies systematic consequences for peoples' actions one need not embrace a presumption that humans are autonomous agents. The common justification in terms of "holding people responsible," however humanistic it may sound, is a thinly veiled defense of punishment as a cultural practice. In contrast, while acknowledging punishment as a basic process that is integral to our biological adaptiveness, contemporary behavior analysts strive to devise effective alternatives, both at the individual level (e.g., functional analysis), and at the systems level (e.g., positive behavioral support). Thus, an emphasis on environmental determinants of behavior, with a de-emphasis on punishment, need not imply that "anything goes" (Hineline, 2004).

As a way forward, Gergen asserts the need for *generative theory*—"theory designed to challenge existing forms of intelligibility and to open alternative vistas for both research and practice" (p. 131). He includes Marx, Freud, and Skinner as salient among these, and includes our essay as well as his own work as contemporary efforts in a similar vein. His conception of *confluence theory* bears some similarity to the behavior-analytic conception of *the behavioral stream* (also advocated in Shimp's (2009) commentary, addressed below), especially if the latter is taken as incorporating our stance of including the interpreter within the interpretation. Gergen's proposal for *collaborative explanation*, however, carries the risk of reintroducing the dispositional bias, given the pervasiveness of that pattern among potential collaborators.

**Petrusz and Turvey ("Dispositioning and the Sciences of Complexity")** (2009) are advocates of the approach known as *ecological psychology* originated by J. J. Gibson (1979). As they point out, their approach "distances itself from 'dispositioning'. . .by virtue of its goals and its affinity with the sciences of complexity," and they aspire to "a methodology whose products will be more in line with the type of explanation Field and Hineline advocate" (pp. 138-139). This is clear in their emphasis upon organisms/environments as integral systems and their explicit emphases on multi-scaled analyses, adjusted by criteria of orderly relationships at nature's ecological scale, rather than by an ontology geared to physics.

Along with their indicating agreement with our reservations regarding the pervasiveness of dispositional bias and its accompanying implicit explanatory assumptions, Petrusz and Turvey provide some welcome additions by identifying theorists (besides the ones Field and I had discussed) who have attempted to craft alternative approaches to explanation that finesse the dispositional bias (Gottlieb, 1997 and Miller, 2009 on development, and Ulanowicz, 1997 on ecology). An additional cluster of work with a similar but limited affinity is that concerned with "self-organization" and "complexity." Petrusz and Turvey indicate that scientists working under these rubrics have generated

> . . .a convincing body of data and a range of practical applications, but the theories sitting behind the phenomena are still generally subject to comparison against conceptual frameworks built upon the assumptions criticized by Field and Hineline. (pp. 136-137)

By parsing organism and environment as a unitary entity, the language of "systems" succeeds in privileging neither one, for the implicit agency required by explanatory prose is then assigned to the system itself. A system is said to be "self-organizing," which can be read as *itself organizing* or as *organizing itself*—I prefer the former—with organizing as something that the system does, rather than something it does *to itself*. As Petrusz and Turvey characterize it, the organization of such a system arises from its characteristic dynamics rather than from outside control. The system will be affected by surrounding context, but nonlinearly, even to the extent of reorganization, especially at critical points that are characteristic of the system. Both the "characteristic dynamics" and "critical points" in this characterization suggest they are dispositioning the system itself, thus illustrating how difficult it is to break the constraining language patterns. Petrusz and Turvey do not succumb to that trap, however, for they enumerate numerous domains in which adherents of a complex systems approach encounter "a frontier between dispositional and non-dispositional strategies exists" (p.137):

> Crucially, what is missing from. . .(strategies that underlie dispositioning). . .is a concern for gaining knowledge about the contexts or histories of the components, or about the systems as a whole. All the emphasis is placed on knowledge of the properties possessed by the entities in question. . .rather than on dynamics and interactions. . . .The context in which the system is embedded is in some cases more valuable to explanation than knowledge of component properties. Most importantly, the weight of explanation is carried by the dynamics of the system's behavior rather than by the system's properties. (p. 138)

I shall return to dynamic systems later when discussing another commentary, but that can be facilitated by first addressing the commentary offered by **David Palmer ("How Shall We Account for Variance?")** (2009). Palmer details his agreement with much of our position, especially concerning the gratuitous reasoning (and unacknowledged assumptions) that frequently accompany dispositional explanations. His two main concerns are parsimony and pragmatism as criteria for evaluating explanations. We agree with those emphases, but find that they cut somewhat differently than he argues. Palmer proposes that, *in principle*, a molecular account of molar patterns is superior to an account at the molar level, in that it accounts for what is observed at both the molar and molecular levels (thus accounting for more of the total variance—an indirect way of saying the molecular account, when successful, is more parsimonious):

> . . .when a relationship at one level is indeed mediated by events at another level, we may not need to appeal to both levels under many conditions, but it is indeed an advance if we are able to integrate the analyses, for we will ultimately be able to account for more of the variance in the phenomena under consideration by a consideration of mediating events. (p. 154)

Agreed. When it works in also accounting for molar relationships, the molecular account is, indeed, more parsimonious. And, as Palmer notes, unlike the fictitious ether that in a bygone era was taken as accounting for the propagation of light through interstellar space, the nervous system surely has a tangible, verifiable role in the mediation of behavior. However, appeals to smaller-scale processes as accounting for larger-scales ones must be qualified by what one means by "working" and what kind of "in principle" is involved. Formal criteria have the appeal of a certain elegance, but accounts evaluated by those criteria often entail huge promissory debts. Thus, in addressing children's behavior, an account built upon fundamental neuroscience—the details of synaptic transmission, neurochemistry, and the like—may be seen by some as having a better claim to completeness in comparison with an account in terms of the whole child's actions and consequences. However, the neuroscientific account leaves unanswered the question of how best to organize and manage a day-care center, and even has little to say about the appropriate techniques for teaching an individual child. Thus, such an account, however elegant in principle, "doesn't work" pragmatically, either because pragmatically important details have not been worked out, or because even if they were, they would not translate readily into effective action.

A data-based example that challenges Palmer's stance is provided by Vollmer & Bourret's (2000) application of the matching law to the behavior of professional basketball players. From their abstract:

> For players with substantial playing time, results showed that the overall distribution of two- and three-point shots [1] was predicted by the matching equation. Game-by-game shot distribution was variable, but the cumulative proportion of shots taken from three-point range as the season progressed was predicted almost perfectly on a player-by-player basis for both male and female basketball players. (p. 137)

To be sure, one can do an analysis of concurrent dynamic processes, some of which can be understood in terms of moment-to-moment relationships: the mechanics of running, and of simultaneously dribbling the ball, adjusting to movements of opposing players, all organized in relation to lines painted on the floor and a hoop suspended above it. However, analyses of these would be unlikely to yield the orderly patterns at the much expanded scale that Vollmer and Bourret identified.

Palmer acknowledges—even asserts—this approvingly when quoting B. F. Skinner with respect to identifying the appropriate unit for behavioral analysis by examining progressively greater degrees of precision (smaller and smaller scales):

> . . .when we have reached the point at which orderly. . .changes appear, we cannot go beyond it with further [precision in the definitions of the terms of our analysis] without destroying the desired result. (p. 152)

Baum (2010) makes a similar point specifically regarding specific data that reveal the applicability of the matching law to an organism's choices between alternatives:

> In the examples we have considered so far, all choice dynamics were examined at time scales that permitted calculation of a behavior ratio in each unit. If, as analyses moved to smaller and smaller time scales, one could still see the matching relation holding, one might be tempted to conclude that matching holds all the way down. . . .When the same relation applies at every time scale,

[1] That is to say, attempts from distances that could yield two vs. three points.

> that is *self-similarity* (also known as scale invariance). Suppose, however, that local (small-scale) processes do not reflect the extended regularity—that is, suppose the extended relation is not evident in the local processes. If such small-scale regularities exist, then matching might be derivable from them, but those small-scale regularities might not be derivable from the matching relation. (Baum, 2010, p. 167)

Vollmer & Bourret's basketball example, sketched above, provides an example supporting Baum's argument. In his conclusions based upon a detailed multi-scaled analysis of data that preserved moment-to-moment as well as longer-term relationships, Baum concludes:

> Self-similarity, or scale invariance, may occur across all time scales or across all but the smallest. Reduction occurs as one examines the dynamics of choice at the smallest time scales. One might be tempted to consider the relations at the smallest time scale to be somehow "fundamental," and the traditional molecular view of behavior would privilege a small scale that seemed to coincide with its assumptions of discrete momentary events and contiguity (Baum, 2002, 2004). The molar view, however,[2] considers the regularities at all time scales to be relevant and valid. None is "fundamental," because any may be interesting or useful. Practical considerations or apparatus limitations might favor research or application at any time scale. In applied behavior analysis, in particular, regularities on the smallest time scale may be of little use or interest (Baum, 2003). (Baum, 2010, pp. 173-174)

Balancing this is a key point that Field and I attempted to clarify, but which apparently was missed by some of the commentators, even though it is at least partially supportive of their positions. To wit: even when larger-scale relationships cannot be derived from the smaller-scales ones, the larger-scale relationships cannot violate at least some of the smaller-scale relationships embedded (nested) within them. Returning to the basketball example: At the same time (literally, because these are temporally overlapping, nested relations), each attempted shot that contributed to the matching relationship would have been compromised if the component molecular relationships were changed—by a slippery floor, a deflated ball, sweat in a player's eyes, or poison in the drinking fountain.

In our essay we illustrated this point by analogy with the physical properties of a steel bridge. Palmer rejects the relevance of our discussion of triangle structures and the dynamic principle of resonance, as applying to the independent validity of intermediate-scale relations that might be observed in behavior—stating that "The terms *triangle* and *resonance* are analytical terms inherent in a particular level of analysis, but they are not intrinsic to the phenomena to be explained" (Palmer, 2009, p. 153). I agree that in any particular case they are inherent in a particular level of analysis—that's the point! But in differing cases the relevant scales can vary from molecular to astronomical. Since it entails behavior patterns,

[2] Baum's term. In the Field and Hineline essay it was called the multi-scaled view, for "molar view" implies that larger-scaled analyses are to be favored over smaller-scale ones.

the basketball example described above may be more pertinent than our bridge example. However, Palmer seems to be advancing substantially the same argument, replacing our bridge with his see-saw—our triangular truss structures with his Type-1 lever; our rusted rivets with his termite-infested board. In both cases, relationships at a higher level cannot occur if those at a lower level are compromised—as in melting the steel of the bridge or chewing up the fibers in the seesaw.

In some cases these kinds of relationships might appropriately be treated as "parameters" whose values need to be included, perhaps as modulating variables, to account for more of the observed variance, as Palmer suggests. Within limits, some of them may amplify or compress the larger-scale phenomena. Still, while they may *constrain* the larger-scale configuration, these are not the constraints that *configure* the larger scale relationships. As applied to the properties of Palmer's seesaw, the tensile strength of the board does not specify the properties of a Type-1 lever, although the lever's properties depend upon that tensile strength. Meanwhile, the limiting characteristics at the molecular scale (the steel in the beams has melted; the plank has become dust) have characteristics of strong interaction, and attempting to add the variances that arise at different scales becomes untenable.

As in the commentary of Petrusz and Turvey, invocation of system dynamics is a core feature of the commentary provided by **Charles Shimp ("Behavior Streams" vs. "Behavior Extended in Time")** (2009). Given that he, too, works within the behavior-analytic tradition, there are several points of agreement, especially concerning distortions arising from vernacular language, the importance of discussing the role of the interpreter in relation to the interpretation (to which we devoted several pages, rather than the "few passing references" (p. 157) which he suggests), the advantage of dynamic rather than static conceptions of behavior, and the important role of time within explanatory systems. Those notwithstanding, Shimp's commentary continues a repartee that he and I have engaged in for more than three decades, concerning whether there is a best scale for analysis of behavior, and if so, which scale that should be. In his commentary, Shimp stakes a claim for such a best scale, whereas Field and I did not. This, as well as other points of misunderstanding or disagreement, can be captured by re-presenting a portion of the second paragraph of his commentary (pp. 157-158), indented and in italics, with my replies or related observations inserted within square brackets:

> Field and Hineline advocate for a multi-level analysis of behavior, one level of which would involve the temporal dynamics of moment-to-moment behavior in real time, which I will refer to in my comments as "real-time behavior."

[With this label, Shimp attempts to privilege the scale that is accessible via direct perception, which implies that present reality spans only a few seconds at most. As sketched by Moore (1981), however, even when the phenomenon of concern is seemingly encompassed by direct perception, the definition of a singular scale of measurement and interpretation can be elusive:

> The molar-molecular distinction is, of course a time–honored distinction in experimental psychology. Yet, its current treatment suggests certain ironies. Consider Shimp's position, sometimes called molecular, and that of the Harvard group, sometimes called molar. For Shimp, the independent variable is the momentary probability of reinforcement, indeed a molecular concept. However, the dependent variable is the pattern of responding as revealed by IRT analysis[3], which seems ironically a molar concept. In contrast, for the Harvard group, the independent variable is rate of reinforcement, a molar concept, but the response unit is the instantaneous response—although it is condensed to a rate measure. This response unit ironically seems quite molecular. (Moore, personal communication based upon the notes he used when presenting his talk.)

The claim to a special reality of patterns that we can directly see is adroitly placed in perspective by Rachlin and Frankel (2009), who provide an ingenious illustration of simultaneous realities on multiple, overlapping time scales:

> Suppose a group of schoolchildren is visiting the barn where locomotives are kept. Two locomotives are sitting there, not moving at all. The engineer showing the children around points to one of them and says, "This locomotive is going 100,000 miles per year at present." Then he points to the other and says, "This one is a little older and we use it less; it's only going 50,000 miles per year at present." (Rachlin & Frankel, 2009, p. 131)

Their ensuing discussion focuses on the fact that "at present" properly applies not only to those two statements, but also to a child's observation that *at present* neither one is moving at all. The choice of scales for analysis is a pragmatic one—whether one is concerned as a person standing in front of a locomotive, or whether one is concerned with scheduling its maintenance. Both the negligible velocity at the moment and the 50,000 miles per year are equally real, and equally present. Recognizing them and choosing between them is not as difficult as Shimp suggests.]

> Field and Hineline propose a method to conceptually integrate real-time behavior and "behavior extended in time."

[For us, *all* behavior is extended in time, and behavior on larger time scales is just as real as that which can be perceived moment-to-moment.]

> I predict, however, that the method would actually have the *opposite* effect. Field and Hineline's method is integration through separation. . . . It would grant independence to the level of "behavior extended in time" from the level of continuous, observable, real, moment-to-moment behaving.

[We addressed this via the limited linkages when discussing the concept of emergence at particular scales, both in the original essay and in the present replies

---

[3] IRT = inter-response time. Thus, in IRT analysis the analytic unit includes both a response and the time since the most recent occurrence of a similar response.

to Palmer's commentary. To reiterate: *All* behavior is extended in time to a greater or lesser degree, and emergence of some pattern at a particular scale does not mean total independence from relationships at smaller scales. Rather, our point is that the configuration at one scale may not be *predictable from* organization at another scale—whether from small to large or from large to small].

> The paper needs to explain how behavior extended in time, and therefore presumably occurring somehow *in* time at *some* time or times, can be independent of moment-to-moment behavior. A host of questions immediately arise, such as whether behaviors at different levels occur simultaneously. (p. 158)

[We addressed this explicitly, for example, in the opening paragraph under "Multi-Scaled Process":

> In exchanges with behavior analysts who adhere to contiguous causation it often is evident that our rejection of contiguity *as necessary* (emphasis added) for causation is misunderstood as advocating privileged status for interpretation in terms of distal or extended ("molar") relations. To the contrary, we recognize that some relations for which causal terms are appropriately invoked do involve contiguous events. But we also assert the need to understand processes as simultaneously occurring on multiple time scales. That is, the theorist must be sensitive to the possibility of ongoing orderly relations between events that are occurring simultaneously on widely disparate scales. These overlapping scales of process can involve both spatial and temporal dimensions, with some processes embedded within others. Distinct phenomena may emerge at each scale, analogous to the multiple spatial scales that one might study with various magnifications of a microscope. In part the problem is to achieve an intuitive understanding of behavioral/psychological processes in this way. (p. 44)

We attempted to illustrate the above points not only via tangible examples from physical mechanics, and also with a more abstract passage from Warren & Shaw (1985):

> There can be no fixed unit of change, or fixed spatio-temporal scale, over which all events are defined (their p. 8; our p. 45). . .events of different periods may overlap within the same region of space-time, that is, natural events come *nested*, like the scenes and acts of a play. . .we must recognize that events of importance for perceivers are defined at ecologically appropriate scales, or levels of nesting. The relevant level of nesting is determined by the significance of events at that level for the needs and activities of the perceiving animal. . .The nesting of simpler events may give rise to complex events not necessarily reducible to their simpler elements—although in some trivial cases they may be. (their p. 9; our p. 45)

In this case, "the animal" that Warren & Shaw refer to is the scientist.]

In his defense of relatively small-scale analyses, Shimp appeals to some of Skinner's brilliant/ground-breaking research—the precise shaping of pigeons' behavior in both basic and applied research, as well as his introduction of the cumulative record as analytic technique. It is worth pointing out that a most potent feature of cumulative records for portraying patterns of intermittently reinforced behavior is its enabling simultaneous examination at both fine-trained and intermediate dimensions. The loss of graphical techniques, regarding which I share Shimp's lament, are a likely casualty of the shift to computer-controlled experimentation and increased emphases upon primarily quantitative analyses rather than evaluating behavior change via direct perception of data plots. I also agree that narrowly defined and precisely executed contingencies can be exceedingly important. However, there are also situations in which excess precision can be dysfunctional, either through expense in time and effort or through the impairment of functional variability (Hineline, 2005).

Shimp seems to miss the point that context, as well as behavior within the context, can be multiply scaled. Molecular contextual relations are often characterized in terms conditional discriminations, whereas molar contextual relations are more commonly characterized as parametric effects. Also, Shimp understands us as advocating different explanatory principles for different time scales. While, to be sure, in particular cases that is likely to be true, our assumption is that no behavioral principle is confined to a particular scale. Thus, a track-and-field athlete's running a hurdle race is likely to be maintained by positive reinforcement, while some fine-grained features of topography in clearing individual hurdles clearly are shaped by differential punishment (clipping a hurdle with one's ankle is quite painful). In other circumstances, agreeing to run a race could be punished, while fine-grained features of technique are positively reinforced. The same applies to the scientist/interpreter's discriminations.

Returning to Shimp's paragraph, repeating the sentence where I left off:

> [If behaviors at different levels occur simultaneously] how can we tell which behavior is which? At which moment do we know which behavior is which? Or do we know only in some extended-in-time sense? (p. 158)

[This issue is well handled by Rachlin and Frankel's (2009) example, described above, regarding the two locomotives.]

Shimp criticizes our use of analogies with physics. First, it should be clarified that our use of analogy was an attempted didactic device to aid the reader's intuitive understanding—not to accrue prestige for psychology by borrowing from another science. Our multiple analogies also illustrated that multi-level organization is not peculiar to any one of them, but instead is evident in a broad range of phenomena. The point was not to argue for any particular psychological principle—other than that of multiple scalability—as applicable to behavioral process (although I do find intriguing the possibility that the principle of resonance might capture some of the features of behavior patterns that interact with periodically occurring environmental events; Hineline, 1986). I should point out that the kind of dynamic modeling that Shimp clearly favors, that is, an interpretive approach based upon modeling of dynamic systems with input variables specified at molecular scales, is also a kind of analogizing. While this indeed can be a useful and potent approach, I should emphasize that even if a computer-based model generates output patterns that closely match data obtained from living organisms, that does not *necessarily* mean that the program is modeling the principles that underlie the data generated by the living organism. Also, Shimp's reference to "plans" and "hierarchical structures" as involved in such modeling suggests the smuggling of some molar relationships into an ostensibly molecular account. This is no embarrassment to the practice of real-time modeling, *per se*, for that, too, can be accomplished on multiple scales.

And finally, Shimp proposes that instead of indulging in the speculative analogizing of this target article we should do research focused upon "determining empirically and conceptually whether there *are* independent levels" (p. 161). Indeed, I have expended substantial efforts in exploring multi-scaled analyses, revealing circumstances where molar relationships prevail in conflict with molecular ones, and *vice-versa*, as well as apparently proceeding independently (e.g., Andrzejewski et. al., 2005; Ennis-Soreth & Hineline, 2009; Field, Tonneau, Ahearn, & Hineline, 1996; Hackenberg & Hineline 1987, 1992; Herrnstein & Hineline, 1966; Hineline, 1970, 1972; Hineline & Sodetz, 1987; Lambert, Bersh, Hineline, & Smith, 1973; Mellitz, Hineline, Whitehouse, & Laurence, 1983; Wanchisen, Tatham, & Hineline, 1988).

As stated in their abstract, **Fletcher and Kerr ("Why Dispositioning Won't Go Away")** (2009) find no faults or limitations in the dispositional explanatory pattern—even as explicit strategy. They do find fault with our use of physical analogies. . .and much else.

First, they take exception regarding our brief and selective excursion into the history of physics. We included that partly to make the point that not only psychologists have struggled with how to conceptualize action at a distance. We began with separations in space because the issues there are intuitively more obvious, whereas separations in time are our primary concern. We addressed the latter by sketching some phenomena in other sciences that resemble the types of situations that we later identify as occasioning integrative and teleological dispositioning. Fletcher and Kerr focus on the spatial issues, offering several examples indicating that adherence to contiguous causation is alive and well in contemporary physics. They assert further, that "the ability to provide plausible mechanisms that link such putative causes and effects, is one key criterion in distinguishing sciences from pseudosciences" (p. 121). As Palmer notes in his commentary, we all are confident that there is a physical nervous system present within a person during the interval between an environmental event and a person's action in relation to that event. That does not lead us to accept phrenology as a science, however plausible it might seem.[4] Our issues concern when and in what

[4] However, Uttal (2001) has argued that interpretations of contemporary brain-imaging studies often risk the pitfalls identified with phrenology.

manner it is appropriate to appeal to that substrate as accounting for the environment–behavior relation, and we observe that most forms of dispositioning are not linked in a rigorous way to known details of neural process. An illustrative case is the construct of "schemata," which Fletcher has frequently invoked as an explanatory construct (Fletcher, 1984, 1995), referring to schemata as *things* that a person uses. Despite their salience within cognitivist theory, the purely hypothetical status of schemata is seldom acknowledged, and descriptions of their proposed instantiations within nervous systems are even scarcer.

Regarding "the bipolar constraints of ordinary language" (p. 121), Fletcher and Kerr challenge our saying "Given the bipolar constraint of explanatory language, one cannot simultaneously state dispositional and situational interpretations within the same sentence—indeed they do not readily cohabit the same paragraph or essay" (p. 47). Their counterexamples (p. 121):

> James is an anxious person who fell apart when under pressure in the job interview.
>
> Mary is insecure, and when threatened verbally lashed out.
>
> Tom got an A because he is smart and the teacher liked him.

The third is actually about two peoples' behavior, with dispositional/organism-based formats regarding each. The first two are more descriptive than explanatory, and they entail separate clauses; perhaps we should have said "clauses" rather than "sentences." Regarding those two: in more detailed expositions elsewhere (Hineline, 1986, 1990) I have noted that both organism-based and environment-based explanations acknowledge an immediate situation as context, but typically not as the anchoring source of the action. Thus, "when under pressure" and "when threatened" provide such contexts for James's anxiety and Mary's insecurity as internal states presumed to initiate actions.

As to paragraphs, Fletcher and Kerr assert that

> . . .a glance at any developmental or social psychological journal will similarly reveal that hypotheses and claims (typically expressed in the same sentence or paragraph) *embodying interactions between dispositional and situational causes* are commonplace. (p. 121, italics added)

But, as our essay documented with several cases, when it comes to the bottom line, dispositional language pervades the authors' interpretations, as when Jones and Nisbett's (1971) appealed to "powerful cognitive forces" (p. 2) in accounting for the actor/observer difference and in Fletcher's (1995) appeal to "complex attributional schemata" as producing attributional judgments (p. 75; see also Fletcher, 1984). We noted Choi, Nisbett, and Norenzayan's (1999) frequent pattern of dispositioning, and included an Appendix containing twelve examples with commentary—notable since their dispositional assertions were offered as explaining cross-cultural differences regarding commission/non-commission of the fundamental attribution error (i.e., dispositioning) itself.

Fletcher and Kerr agree with our assertion that the privileging of dispositional explanation can have pernicious effects, but they assert that "this does not rule out of court the merits of using dispositional attributions in scientific models. *Rather, it implies that they need to be used judiciously and with due caution*" (p. 122, italics added). If this were typically done, we would not have criticized the common practice—indeed, we would not have written the essay. Clearly, the pervasiveness of dispositional explanations of the fundamental attribution error, which we documented to some degree, indicates a lack of judiciousness and due caution. It was not we who introduced the term "fundamental attribution error" as applying to the interpretive patterns that we have called "dispositioning." The key point to be reiterated is the observation that theorists who coined the term "error"—and the extensive community of researchers who have discussed it without appearing to question the "error" label—exemplify a consistent bias favoring dispositional explanations, even while discussing them as errors. Also, we attempted to consistently discuss it as a set of interpretive practices and not as a characteristic of the interpreters, thus demonstrating alternatives to dispositional explanation. Still, our main agenda was to illustrate how subtle and pervasive the dispositioning bias is within the interpretive prose of mainstream psychology.

Not surprisingly, Fletcher and Kerr take issue with our asserting that Fletcher failed to recognize the extent of dispositional bias within his own prose. They assert that, instead, in Fletcher's (1985) book that we cited as well as in the Fletcher (1984) article which they cite in their commentary, he explicitly endorsed the usefulness of dispositional explanation in scientific accounts. To be sure, dispositional explanations are endorsed therein—however, I found no evidence that he entertained non-dispositional alternatives to any appreciable degree.

Fletcher and Kerr offer their own explanations as to why dispositional explanations are so pervasive:

> First, personal dispositions (of various kinds) do exert causal influence over behavior. This is not a hypothesis or a speculative claim, but a fact about the world. . .humans have evolved to perceive and explain human behavior in this way. (p. 123)

This is a rather bald example of reification. Causes are not things; causal prose is a kind of talking and writing whose characteristics are very much a cultural product (e.g., see Deutscher, 2010). Fletcher and Kerr go on to defend dispositioning by appeal to current fashions in psychological research (theory of mind), which to me is an over-interpretation of most data taken as supporting it, and concepts of memory and schemas which, in my view, are typically invoked vaguely and with few, if any, rigorous accounts that translate the dispositional constructs into actions. And:

> In psychology, accounting for how information is represented in the organism in some way, by altering or creating a disposition of some sort, is a standard strategy for explaining how exposure to an event at time one can influence behavior months or years later at time two. (p. 121)

While Fletcher and Kerr clearly find this a benign state of affairs, Deutscher (2010), in his recent updating of what might be called the "Whorfian issues," or the influences of language on thinking, describes compelling data from a diverse set of languages and accompanying practices, then provides a summing up that applies equally well to the organism-based vs. environment-based patterns within English that we have been discussing:

> The demonstrable impact of language on thinking is very different from what was touted in the past. In particular, no evidence has come to light that our mother tongue imposes limits on our intellectual horizons and constrains our ability to understand concepts or distinctions used in other languages. The real effects of the mother tongue are rather the habits that develop through the *frequent use* of certain ways of expression. The concepts we are trained to treat as distinct, the information our mother tongue continuously forces us to specify, the details it requires us to be attentive to, and the repeated associations it imposes on us—all these habits of speech can create habits of mind that affect more than merely the knowledge of language itself. (p. 234)

Just so.

## References

Andrzejewski, M. E., Cardinal, C. D., Field, D. P., Flannery, B. A., Johnson, M., Bailey, K., & Hineline, P. N. (2005). Pigeons' choices between FI and RI schedules: Utility of variability? *Journal of the Experimental Analysis of Behavior*, *83*, 129-145.

Baum, W. M. (2002). From molecular to molar: A paradigm shift in behavior analysis. *Journal of the Experimental Analysis of Behavior*, *78*, 95-116.

Baum, W. M. (2003). The molar view of behavior and its usefulness in behavior analysis. *The Behavior Analyst Today*, *4*, 78-81.

Baum, W. M. (2004). Molar and molecular views of choice. *Behavioural Processes*, *66*, 349-359.

Baum, W. M. (2010). Dynamics of choice: A tutorial. *Journal of the Experimental Analysis of Behavior*, *94*, 161-174.

Bem, D. (1967). Self-perception: An alternative interpretation of cognitive dissonance phenomena. *Psychological Review*, *74*, 183-200.

Bem, D. (1972). Self-perception theory. In L. Berkowitz (Ed.), *Advances in experimental social psychology, Vol. 6* (pp. 1-62). New York: Academic Press.

Choi, I., Nisbett, R. E., & Norenzayan, A. (1999). Causal attribution across cultures: Variation and universality. *Psychological Bulletin*, *125*, 47-63.

Deutscher, G. (2010). *Through the language glass: Why the world looks different in other languages*. New York: Henry Holt & Co.

Ennis-Soreth, M., & Hineline, P. N. (2009). The probability of small schedule values and preference for random-interval schedules. *Journal of the Experimental Analysis of Behavior*, *91*, 89-103.

Field, D. P. & Hineline, P. N. (2008). Dispositioning and the obscured roles of time in psychological explanations. *Behavior and Philosophy*, *36*, 5-69.

Field, D. P., Tonneau, F., Ahearn, W., & Hineline, P. N. (1996). Preference between variable-ratio and fixed-ratio schedules: Local and extended relations. *Journal of the Experimental Analysis Behavior*, *66*, 283-295.
Fiedler, K., & Semin, G. R. (1992). Attribution language as a socio-cognitive environment. In G. R. Semin & K. Fielder (Eds.), *Language, interaction, and social cognition* (pp. 79-101). London: Sage Publications.
Fiedler, K., Semin, G. R., & Bolten, S. (1989). Language use and reification of social information: Top-down and bottom-up processing in person cognition. *European Journal of Social Psychology*, *19*, 271-295.
Fletcher, G. J. O. (1984). Psychology and common sense. *American Psychologist*, *39*, 203-213.
Fletcher, G. J. O. (1995). *The scientific credibility of folk psychology*. Mahwah, NJ: Erlbaum.
Fletcher, G. J. O., & Kerr, P. S. G. (2009). Why dispositions won't go away. *Behavior and Philosophy*, *37*, 119-125.
Funder, D. C. (1982). On the accuracy of dispositional vs. situational attributions. *Social Cognition*, *1*, 205-222.
Gergen, K. J. (2009). Pragmatics and pluralism in explaining human action. *Behavior and Philosophy*, *37*, 127-133.
Gibson, J. J. (1979). *The ecological approach to visual perception*. Boston: Houghton Mifflin.
Glenn, S. S. (2009). On explaining behavior. *Behavior and Philosophy, 37,* 149-150.
Gottlieb, G. (1997). *Synthesizing nature–nurture.* Mahwah, NJ: Lawrence Erlbaum Associates.
Hackenberg, T. D., & Hineline, P. N. (1987). Remote effects of aversive contingencies: Disruption of appetitive behavior by adjacent avoidance sessions. *Journal of the Experimental Analysis of Behavior*, *48*, 161-173.
Hackenberg, T. D., & Hineline, P. N. (1992). Choice in situations of time-based diminishing returns: Immediate vs. delayed consequences of action. *Journal of the Experimental Analysis of Behavior*, *57*, 67-80.
Herrnstein, R. J., & Hineline, P. N. (1966). Negative reinforcement as shock-frequency reduction. *Journal of the Experimental Analysis of Behavior*, *9*, 421-430.
Hineline, P. N. (1970). Negative reinforcement without shock reduction. *Journal of the Experimental Analysis of Behavior*, *14*, 259-268.
Hineline, P. N. (1972). Avoidance sessions as aversive events. *Science*, *176*, 430-432.
Hineline, P. N. (1980). The language of behavior analysis: Its community, its function, and its limitations. *Behaviorism*, *8*, 67-86.
Hineline, P. N. (1986). Re-tuning the operant-respondent distinction. In T. Thompson & M. D. Zeiler (Eds.), *Analysis and integration of behavioral units: A festchrift in honor of Kenneth MacCorquodale* (pp. 55-79). Hillsdale, NJ: Earlbaum.
Hineline, P. N. (1990). The origins of environment-based psychological theory. *Journal of the Experimental Analysis of Behavior*, *53*, 305-320.
Hineline, P. N. (1992). A self-interpretive behavior analysis. *American Psychologist*, *47*, 1274-1286.
Hineline, P. N. (2004). When we speak of intentions. In K. A. Lattal & P. N. Chase (Eds.), *Behavior theory and philosophy* (pp. 203-221). New York: Kluwer Academic/Plenum Publishing.
Hineline, P. N. (2005). The aesthetics of behavioral arrangements. *The Behavior Analyst*, *28*, 15-28.

Hineline, P. N., & Sodetz, F. J. (1987). Appetitive and aversive schedule preferences: Schedule transitions as intervening events. In M. L. Commons, H. Rachlin, & J. Mazur (Eds.), *Quantitative analyses of behavior, Vol. V: Reinforcement value—The effects of delay and intervening events* (pp. 141-157). Hillsdale, NJ: Erlbaum.

Jones, E. E., & Nisbett, R. E. (1971). The actor and the observer: Divergent perceptions of the causes of behavior. In E. E. Jones, D. E. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, & B. Weiner (Eds.), *Attribution: Perceiving the causes of behavior* (pp. 1-16). Morristown, NJ: General Learning Press.

Lambert, J. V., Bersh, P. J., Hineline, P. N., & Smith, G.. D. (1973). Avoidance conditioning with shock contingent upon the avoidance response. *Journal of the Experimental Analysis of Behavior*, *19*, 361-367.

Lewontin, R. C., Rose, S., & Kamin, L. J. (1984). *Not in our genes*. New York: Pantheon.

Miller, D. (2009). The provenance and control of behavior: Simplistic answers are doomed to fail. *Ecological Psychology*, *21*, 131-137.

Mischel, W. (1974). Processes in delay of gratification. In L. Berkowitz (Ed.), *Advances in experimental social psychology (Vol. 7)*. New York: Academic Press.

Moore, J. (1981). On molar and molecular analyses of behavior. Invited Address presented at the meeting of the Association for Behavior Analysis; Milwaukee, Wisconsin; May 28, 198l.

Neuringer, A. (2002). Operant variability: Evidence, functions, and theory. *Psychonomic Bulletin & Review*, *9*, 672-705.

Mellitz, M., Hineline, P. N. Whitehouse, W. G.,. & Laurence, M. T. (1983). Duration-reduction of avoidance sessions as negative reinforcement. *Journal of the Experimental Analysis of Behavior*, *40*, 57-67.

Oyama, S. (1985). *The ontogeny of information: Developmental systems and evolution*. Cambridge: Cambridge University Press.

Oyama, S. (2001). Terms in tension: What do you do when all the good words are taken? In S. Oyama, P. E. Griffiths, & R. D. Gray (Eds.), *Cycles of contingency: Developmental systems and evolution* (pp. 177-193). Cambridge, MA: MIT Press.

Palmer, D. C. (2009). How shall we account for variance? *Behavior and Philosophy*, *37*, 151-155.

Petrusz, S. C., & Turvey, M. T. (2009). Dispositioning and the sciences of complexity. *Behavior and Philosophy*, *37*, 135-140.

Rachlin, H., & Frankel, M. (2009). Taking pragmatism seriously: A review of Baum's *Understanding behaviorism: Behavior, culture, and evolution* (2nd ed.). *Journal of the Experimental Analysis of Behavior*, *92*, 131-137.

Resnick, M. (1997). *Turtles, termites, and traffic jams*. Cambridge, MA: MIT Press.

Ruiz, M. R. (1995). B. F. Skinner's radical behaviorism: Historical misconstructions and grounds for feminist reconstructions. *Psychology of Women Quarterly*, *19*, 161-179.

Ruiz, M. R. (2009). Beyond the mirrored space: Time and resistance in feminist theory. *Behavior and Philosophy*, *37*, 141-147.

Schultz, E. A. (1990). *Dialogue at the margins*. Madison, WI: University of Wisconsin Press.

Shimp, C. P. (2009). "Behavior streams" versus "behavior extended in time." *Behavior and Philosophy*, *37*, 157-163.

Skinner, B. F. (1948). *Walden two*. New York: Macmillan.

Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review*, *57*, 193-216.

Skinner, B. F. (1953). *Science and human behavior*. New York: Macmillan.

Skinner, B. F. (1971). *Beyond freedom and dignity*. New York: Knopf.

Skinner, B. F. (1975). The ethics of helping people. *Criminal Law Bulletin*, *11*, 623-636.
Skinner, B. F. (1986). What is wrong with daily life in the western world? *American Psychologist*, *41*, 586-574.
Ulanowicz, R. E. (1997). *Ecology: The ascendent perspective*. New York: Columbia University Press.
Uttal, W. R. (2001). *The new phrenology*. Cambridge, MA: MIT Press.
Vollmer, T. A., & Bourret, J. (2000). An application of the matching law to evaluate the allocations of two- and three-point shots by college basketball players. *Journal of Applied Behavior Analysis*, *33*, 137-150.
Wanchisen, B. A., Tatham, T. A., & Hineline, P. N. (1988). Pigeons' choices in situations of diminishing returns: Fixed- vs. progressive- ratio schedules. *Journal of the Experimental Analysis of Behavior*, *50*, 375-394.
Warren, W. W., & Shaw, R. E. (1985). Events and encounters as units of analysis for ecological psychology. In R. E. Shaw, W. M. Mace, & M. T. Turvey (Eds.), *Resources for ecological psychology* (pp. 1-27). Hillsdale, NJ: Erlbaum.