On Multiscaled and Unified

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My reaction to the papers by Baum (2013) and Shimp (2013) is best considered that of a rank-and-file experimental analyst of behavior who is familiar with the molecular–molar debate and recognizes its importance, but who is by no means an expert on it. As such, I hope (a) that I have not misrepresented any of Baum’s or Shimp’s views (and apologize if I have), and (b) that I give both of them some things to consider as they continue their attempts to disseminate their ideas to the behavior-analytic community and beyond.

There are certainly many ways to address the question “What counts as behavior?” This is an important question, for our answer to it (or at least our perspective on it) frames our research questions, our theories and models, and our interpretations. It comes as no surprise that the focus of this symposium was on molecular–molar issues; I suspect that is what the organizers of the conference had in mind when they invited Baum and Shimp to participate. If we consider our subject matter to be defined in terms of the functional relations by which “it” is characterized, then consideration of the levels at which the variables are expressed is critical.

I thank Linda Hayes, Maria Malott, and Jack Marr for organizing a most stimulating conference, and for inviting me to participate. I also thank Billy Baum and Charlie Shimp for their interesting and provocative papers, and the B. F. Skinner Foundation for permission to reprint the material shown in Figure 1. Finally, I thank Christine Hughes and Kim Sawrey for serving as sounding boards for some of my thoughts on the issues raised by these papers.

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I can think of no better contributors than Baum and Shimp. Self-reflection can be healthy, and for over four decades both have asked us to look beyond some of our traditional conceptualizations and reevaluate some of our assumptions. Their contributions, both empirical and theoretical, have helped define the molecular–molar debate and, in turn, shaped the field.

Self-reflection also can be painful. Unfortunately, like partisan politics, the debate between molecular and molar views can leave the impression that we are deeply polarized. To some degree, however, molecular versus molar is a false dichotomy. Questions of this sort are too complex to be characterized as either–or. Although many may lean one way or another, I get the sense that many of us believe that both approaches have something to contribute.

BAUM’S MULTISCALED APPROACH

Throughout his career, Baum has argued adamantly that we need to move beyond traditional conceptualizations based on temporal contiguity, and rethink some of our most cherished concepts (e.g., Baum, 1973, 1995, 2002, 2005, 2011, 2012). Regardless of one’s perspective on the molecular–molar debate, Baum reminds us that without considering relations extended in time, we can fall prey to bad habits (see also Field & Himeline, 2008; Himeline, 1990). Such bad habits are illustrated not only by the agent–action locations typical in psychology, but also by similar practices among behavior analysts (e.g., appealing to unobserved stimuli or responses, as in *automatic reinforcement* or *self-generated rules*, in an

attempt to account for otherwise unexplained behavior patterns observed under controlled conditions).

For those who describe themselves as radical behaviorists, Baum doesn't always make it easy. Unfortunately, I think some find it difficult to get past certain of his views and, thus, may not fully appreciate some of his points. I think most of us recognize that behavior analysis has advanced beyond the initial foundations laid by Pavlov and Skinner. For example, most probably accept that simple pairing accounts of conditioning are not sufficient (e.g., Rescorla, 1967) and that correlations between behavior and consequences extended in time can be important (e.g., Baum, 1973). However, one can accept that molar relations are important without accepting everything that Baum suggests goes along with it. Like Shimp, for example, I am not ready to call *molarism* a paradigm shift (Baum, 2002). And, despite some difficulties with it, I am not ready to abandon completely the practice of talking about reinforcement as strengthening (Baum, 2012); nor do I think that recognizing the importance of molar relations necessitates that we completely abandon Skinner's overall approach to private events (Baum, 2011).

In this paper, Baum makes several points early on that I think many will find palatable, regardless of their leanings on the molecular-molar issue. For example, he begins by making what I think is a key point: that any answer to the question "What counts as behavior?" necessarily is provisional. Given that function plays an important role, any definition of behavior will reflect the current state of knowledge regarding its controlling variables. As we refine our understanding of the nature of what we call contingencies of reinforcement, our conceptualization of what constitutes the independent and dependent variables of our science will change. He also makes

the important point that there is no role for the concept of agency in any meaningful conceptualization of behavior.

I find some of the other points Baum makes early on, however, somewhat less palatable. It certainly is hard to disagree with the view that any conceptualization of behavior should be consistent with evolutionary theory, and one cannot help but be disappointed (but certainly not surprised) by Levitis, Lidicker, and Freund's (2009) treatment. I find it easy to disagree, however, with Baum's contention that "with a few exceptions" behavior analysts "have ignored evolution" (p. 284). A number of behavior analysts beyond those Baum lists have discussed relations between behavior analysis and natural selection, and often extensively (e.g., Glenn 1991; Glenn & Field, 1994; Glenn & Madden, 1995; D. L. Hull, Langman, & Glenn, 2001; Skinner, 1966, 1981, 1984, 1986). And many behavior analysts I know teach a section on evolution by natural selection in their courses. I also find it hard to accept that the blame for our failure to connect with evolutionary biology lies squarely at the feet of a molecular view. Behavior analysts certainly could be better at establishing meaningful connections with biology (and with the neurosciences), and it is important that we do so (e.g., Schaal, 2013). In my view, however, Baum simply has not made a compelling case that molecularism stands as a major barrier to such connections.

Baum offers four principles to guide us when considering what counts as behavior. The implication is that these principles follow from a molar multiscale view of behavior. The first two of these (*only whole organisms behave* and *behavior is purposive*) likely would be included, at least in some way or another, even if one's perspective was more molecular. The notion that behavior is only meaningfully assigned to whole

organisms is important. I agree with Bennett and Hacker (2003) that it “transgresses the bounds of sense” (p. 78) to say, for example, that a frontal lobe “plans.”

Baum’s second principle, *behavior is purposive*, appears to incorporate the important characteristics of operant behavior (but I don’t understand why he chose *purposive* rather than *selected*). However, I find some of the points in this section confusing. The actual purposes served by the behaviors in the examples Baum provides are not particularly clear to me. Perhaps I am mistaken, but he seems to move somewhat haphazardly across different “levels of selection” (Glenn, 1991; Skinner, 1981); he seems to be saying that this purposive character applies only to the relevant contingencies of natural selection. Indeed, later he states that “all behavior serves only one ultimate purpose: reproductive success” (p. 291). Maybe he means that all behavioral *processes* serve only one ultimate purpose.

With his second principle, Baum brings in the notion of *phylogenetically important events* (PIEs; Baum, 2005, 2012) as an alternative way of talking about what most of us call primary reinforcers. I must admit, the benefits of PIEs escape me. Certainly, a number of fundamental difficulties remain in our field. For example, despite its appeal to many, the notion of *strength* remains problematic. As Shimp notes, it has been hard to untangle what are called *strengthening* and *selecting* effects of reinforcers. Furthermore, events we call reinforcers, and the contingencies they enter into, can function discriminatively (e.g., Boutros, Elliffe, & Davison, 2011; Davison & Jenkins, 1985; Lattal, 1975). And the concept of conditioned reinforcement continues to face considerable challenges (see Shahan, 2010, for a recent review). These and other issues continue to plague us, and I applaud Baum for addressing them head on.

And I agree that a multiscaled perspective might be beneficial. However, I just do not see how the notion of PIEs gets us closer to solving them. For example, I do not see how it helps us understand how various ontogenetic experiences determine the various behavioral functions that can be acquired by environmental events.

Baum’s third principle, *behavior takes time*, is a critical component of his position. As I noted earlier, I think many behavior analysts have embraced the view that our understanding of behavior is improved by considering correlations between behavior and consequences extended in time. I agree with Baum that early on, the field may have focused too strongly on momentary relations. Some of his arguments in this section, however, have the flavor of a straw man. For example, I am not sure anyone would argue that looking at a snapshot of behavior could ever provide an adequate characterization of it. I do wonder, however, what is lost by taking the other extreme; the implication that momentary events essentially are irrelevant (are myths). Consider, for example, the cumulative records shown Figure 1, which are reprinted from Figure 15 of *The Behavior of Organisms* (Skinner, 1938). Records A and B show lever pressing in a naive rat in the absence of any programmed contingencies (Record A is from a session prior to magazine training, and Record B is from a session afterward). Record C shows effects of a single response followed immediately by a single food presentation (three food pellets delivered consecutively). The first lever press in Record C was the only one in the history of the rat that was followed by food. Of course, the lever press in Record C was just the endpoint of a constellation of movements, and events leading up to it likely were relevant. Nevertheless, I am not sure how one could talk about the striking change in behavior

FIGURE 15⁽¹⁰⁾

EXTINCTION CURVE FOLLOWING THE REINFORCEMENT OF ONE RESPONSE

The first response in Curve C was the only response in the history of the organism to be reinforced.

Figure 1. Three cumulative records of lever pressing by a single rat. Record A was taken prior to training the rat to eat from the food magazine. Record B was taken after such training. During these sessions, lever presses had no programmed consequences. The first response in Record C was followed by presentation of three food pellets, after which the feeder was inoperative. Taken from Figure 15 of *The Behavior of Organisms* (Skinner, 1938). Reprinted with permission of the B. F. Skinner Foundation.

shown in Figure 1, which extends in time, without emphasizing the effect of a single response–event instance that took place within a brief period of time. This is not to say that operant repertoires necessarily are built entirely by *moments of truth* (or “slices of simultaneity” as Hineline, 2001, p. 343, has called them), nor it is to say that the broader context should be ignored. But it seems to me that an account of the effect shown in Figure 1 in terms of molar correlation would involve some of the same verbal practices (e.g., appealing to unmeasured responses and consequences) to which molarists object when criticizing molecular accounts. Although Skinner may have been a bit too narrow at times (e.g., “The reinforcement which

develops a skill must be immediate,” Skinner, 1953, p. 96), he also recognized that individuals who are “susceptible” to individual temporal conjunctions may have an evolutionary advantage over those who are not, or who are less so (e.g., Skinner, 1966, 1981). Interestingly, Field and Hineline (2008) suggested temporally and spatially contiguous relations may be of fundamental biological importance and, as such, may differentially affect our causal attributions (and lead us to ignore relations extended in time). I agree. If that is correct, however, then why shouldn’t contiguity hold an important place in our accounts of behavior? In any event, I cannot imagine how to conceptualize the acquisition of a highly skilled activity, or how one would go about shaping

one, without emphasizing momentary behavior–environment relations.

Baum contends that these sorts of issues are addressed by the multiscaled nature of the molar view (e.g., Field & Hiline, 2008; Hiline, 2001). For example, he states that “when one activity takes longer to occur than another activity, they differ in scale” (p. 289). Presumably, this holds for environment–behavior relations as well. Of course, levels of analysis, reductionism, and the related issue of behavioral units have been discussed at length (e.g., Catania, 1973; Marr, 1990, 2004; Morris, Higgins, & Bickel, 1982; Skinner, 1957). I think it is possible that the term *multiscale* may be a good way to frame these discussions. Baum, however, seems to consider *multiscale* to be synonymous with *molar*. As illustrated in my discussion of Figure 1 above, I am not as convinced as Baum that temporal contiguity is easily subsumed within a molar conceptualization (i.e., that temporal contiguity is best viewed as variance in correlation; Baum, 1973). Of course, as Baum and others (e.g., Hiline, 2001) have noted, what actually constitutes temporal contiguity is a legitimate question. But I think the same question can be asked of correlation. The words *contiguity* and *correlation* are emitted under conditions that vary along a number of dimensions. Following their extensive analyses of response-dependent and response-independent contingencies, Catania and Keller (1981) seem to me to strike a good balance:

Certainly single response–reinforcer contiguities can have behavioural effects. ... The account does suggest, however, that the range of conditions within which superstitious behaviour will be generated is narrower than was once supposed. (pp. 163–164)

Baum’s fourth principle, *behavior is choice*, follows from Herrnstein’s (1970) conceptualization and, in my view, represents one of the most important contributions of a molar

view. At the very least, it illustrates that it is impossible to fully understand any behavior–consequence relation without accounting for other behavioral opportunities present in the situation. (As an aside, I’ve always wondered if *choice* is the best word. Like *discriminate*, *prefer*, *attend*, and *discount*, it implies intervening behavior where none has been measured.) Baum uses this principle, in combination with his third one, to reiterate a point he has made several times: that time allocation is the fundamental dimension of behavior (e.g., Baum, 2002; Baum & Rachlin, 1969). Time allocation is for Baum what response rate was for Skinner. Unfortunately, like response rate, time allocation is not without its limitations. The *accounting problem* may be more serious than Baum seems to acknowledge. For example, as Shimp notes, time allocation in the Baum and Rachlin (1969) study was not independent of the probability of changing locations. And problems arise when allocations of individual activities sum to more (or less) than the total available time. As with molar accounts of the effects of Figure 1 discussed earlier, this seems to create some of the same sorts of conundrums claimed to be created by a molecular view. Presumably, activities occurring on the same scale must compete. When allocations of individual activities add up to more (or less) than the total time available, did some of them occur on different scales, was there rapid switching, or did some of them occur simultaneously (e.g., involve speaking or listening)? Or were some of the activities really components of larger, more complex activities? And if behavior always involves the whole organism, can we allocate time to two things at once?

SHIMP’S UNIFICATION

Like Baum, Shimp throughout his career has insisted that we move beyond some of our traditional

assumptions (e.g., Shimp, 1975, 1976, 1979, 1981, 1992, 2009). Shimp's approach has emphasized local patterns of behavior created by reinforcement, rather than molar correlations between responses and reinforcement.

I think that many of those who describe themselves as radical behaviorists find it as difficult to embrace some of Shimp's views as they do Baum's. Indeed, I cannot help but wonder if some aspects of his views may have helped to fuel claims that a molecular approach is, in essence, a form of mentalism (e.g., Baum, 2011). However, I think one can accept that reinforcement shapes extended patterns of behavior within a "behavioral stream" without accepting the necessity of constructs such as "memory" (e.g., Branch, 1977). Nevertheless, I think Shimp's writings do remind us that, even though there may be irreconcilable differences between behavior analysis and much of psychology (see McDowell, 1991), there can be value in looking beyond the confines of our discipline.

In a sort of Hegelian dialectic, Shimp identifies three categories of analysis. Categories 1 and 2 correspond to molecular and molar analyses, respectively, and Category 3 is a unification of these two approaches. One thing I like about Shimp's overall treatment here is that it contains an explicit recognition of what has been called *shaping* and *strengthening* effects of reinforcement. My first exposure to this distinction came from Morse's (1966) brilliant chapter in Honig's *Handbook of Operant Behavior*. Perhaps one source of tension between the molecular and molar views is our confusion with this distinction, and with difficulties in arranging conditions that clearly distinguish these effects. Although some may say that this confusion means that the distinction is useless, I am not sure I would agree. Many treatments of behavior acknowledge, in some fashion or another, that

reinforcement contingencies both identify responses (i.e., they differentially select some responses, or properties of behavior, over others) and increase the prevalence of those responses or properties (e.g., Killeen, 1994; Nevin & Grace, 2000; Shull, Gaynor, & Grimes, 2001). Despite the persistent difficulties with the notion of response *strength*, I have a hard time imagining extracting it from our vernacular. For example, I am not sure Skinner's (1957) treatment of verbal behavior would have been possible without it.

I suspect that Shimp is right when he cautions the reader that advocates of both the molecular and molar views might feel like Kennedy did during his presidential debate with Nixon. His Category 1 corresponds to his vision of molecular analyses, and reflects his empirical work on reinforcement contingencies and local patterns of behavior (e.g., Hawkes & Shimp, 1975; Shimp, 1966, 1969; Shimp, Sabulsky, & Childers, 1989) and his longstanding interest in structure (e.g., Shimp, 1975, 1976). He cites shaping as the prototypical example of a molecular analysis and uses Skinner's ping-pong playing pigeons as an illustration. Certainly, shaping can be considered the home territory of the molecular view (e.g., Galbicka, 1994), and I thought this section contained several interesting points. For example, I agree that a failure to recognize the power and scope of shaping has fueled a number of criticisms of behavior analysis. Indeed, it seems to me that the subtle and cumulative effects of shaping are for some psychologists what the subtle and cumulative effects of natural selection are for the proponents of intelligent design (e.g., Blumberg & Wasserman, 1995; Dawkins, 1986). I also agree that technological advances have increased the availability of a variety of behaviors to be shaped, and I think behavior analysis is starting to make use of these technologies (e.g., Dallery & Glenn,

2005); such advances may help connect us with other disciplines (Stephen Intille's B. F. Skinner Lecture at the 2012 ABAI convention serves as a nice example).

In his discussion of Category 1, Shimp suggests that "shaping creates new, unified, and hierarchically organized temporal patterns of behavior" (p. 296). Shimp has long suggested that behavior analysis should pay more attention to the structure of operant behavior, particularly its temporal structure (e.g., interresponse times). Of course, effective shaping requires some understanding of the current structure of the behavior being shaped. I doubt, however, that the fundamental principles that allow shaping to occur differ across different structures (topographies). At any rate, I was struck by the emphasis here on hierarchical structure. Certainly, hierarchical patterns of behavior abound, and they play an important role in many conceptualizations of both human (e.g., Anderson & Bower, 1973) and nonhuman (e.g., Timberlake & Lucas, 1989) behavior. Furthermore, functional analyses have revealed what appears to be hierarchical organization of problem behavior in applied settings (e.g., Lalli, Mace, Wohn, & Livezey, 1995). Although understanding complex behaviors extended in time likely requires more than strict temporal contiguity and chaining, is hierarchical organization a fundamental outcome of shaping? Are all extended patterns of behavior necessarily hierarchically organized? It seems to me that shaping can create operants of any variety of structures, even those that can be described as random (e.g., Neuringer, 2002). Furthermore, I am not sure what it means to say that "a hierarchically organized pattern is a unitized pattern, the whole of which controls its component parts," and that "all the different movements [are] under control of the larger movements" (p. 297). Similarly, I don't quite grasp the utility of the

metaphor of a "plan." My confusion here may result from the fact I know very little about theories of associative memory, or about research on motor control, feedback systems, and robotics; thus, perhaps I simply do not understand Shimp's writings.

The discussion of shaping and molecular analyses raises the issue of identifying behavioral units. This is a complex issue, one that certainly is relevant to the question of what counts as behavior (but one I might be well advised to avoid). As Marr (2004) points out, "The term 'unit' in behavior has taken on a variety of meanings, not all of which may be useful or even coherent" (p. 151). However, just as I see no reason to assume there is a "true" fundamental unit (see Branch, 1977), I see no reason to assume there is a "true" fundamental structure of units, which itself then determines the nature of the control over smaller components.

Shimp's points about average rate of responding, steady-state methodology, and the shift away from moment-to-moment analyses are all well taken. The general character of modern quantitative models of operant behavior likely is a reflection of free-operant methodology. I cannot agree, however, with Shimp's speculation that the development of free-operant methodology substantially hindered our understanding of shaping, and that it "drove behavior analysis toward quantification of things easily quantified" (p. 299). It seems to me, rather, that Skinner's emphasis on shaping and the development of the free-operant preparation went hand in hand. Furthermore, plenty of quantitative models of behavior have emerged over the years from other methodologies (e.g., C. L. Hull, 1943; Rescorla & Wagner, 1972).

I have just a few, relatively scattered, reactions to Shimp's Category 2. I think molarists indeed might react to this discussion much as Kennedy did to Nixon. The molecular leaning

likely will be sympathetic to Shimp's views about Baum and Rachlin's (1969) time-allocation data, his concerns about the conceptual clarity of molar behaviorism, and his contention that molar analyses confound shaping and strengthening effects. The molar leaning likely will say that adopting a molecular view does not solve these problems. I found the distinction between (behavior stream) dependent and independent molar analyses interesting, but somewhat difficult to conceptualize, perhaps because I am unfamiliar with Wilson and Herrnstein's (1985) book (but am now inclined to read it). This distinction reminded me a little of that between contingencies and metacontingencies (e.g., Glenn, 1988, 1991).

Unification has played a key role in Shimp's approach over the years. For example, he has attempted to provide a unified account of data from discrete-trials and free-operant procedures (e.g., see Shimp, 1975), and has advocated for a unification of features of radical behaviorism and cognitive psychology into a more "liberal behaviorism" (e.g., see Shimp, 1975, 1976). As I noted earlier, one thing that appeals to me about Shimp's approach is an explicit recognition of the distinction between shaping and strengthening effects of reinforcement. As I understand Category 3, shaping builds hierarchically organized units that are extended in time (the molecular part), and aggregates of these units are then expressed against relevant parameters of reinforcement to generate strength functions (the molar part). The implications of this unification for understanding the complexities of behavioral repertoires can then be explored using computational modeling and modern technology. Computational modeling has long been an important feature of Shimp's conceptualizations (e.g., Shimp, 1984, 1992, 1994), and, as he notes, a number of interesting, real-time computational models have emerged in behavior

analysis recently. Although I am probably less sanguine than Shimp about the potential of "behaving theories," I think they are worth pursuing. For example, they can suggest experiments, particularly those aimed at investigating behavioral dynamics. Plus, they seem like they are fun (and one can do an entire "experiment" in a relatively short period of time).

Shimp suggests that Category 3 incorporates the strengths of both the molecular (shaping) and molar (behavior extended in time) approaches, and states that the molar functions

serve as test beds for theory development. The empirical functions are not the be all and end all, and are not "basic" in any important sense, but they serve admirably as constraints on theory development. In an iterative process, we first guess how a theory we define might behave, we simulate the theory and find out how well it does or does not work, revise the theory to make it correspond more closely to data, and so on. (p. 309)

I have a hard time deciding if the unification offered by Category 3 is meaningful or superficial. Again, perhaps I am confused, but as I understand it, for any given "behaving theory," the presumably hierarchical structure of Category 1 is adjusted (presumably by adjusting the "plan") until the molar functions of Category 2 resemble those obtained empirically with key pecks and lever presses (and with a variety of other responses). This seems to resemble the empirical strategy outlined by Skinner (1935), one it appears we have embraced as a field by showing orderly relations with key pecks and lever presses. Category 3, however, extends this analysis, via simulation, to units that have structure. For Shimp, capturing the structure of our behavioral units seems vital. It seems to me, however, this strategy in the context of Category 3 (comparing the functions generated by behaving models with known empirical functions) treats those momentary, mythical events as the very

standard against which “behaving theories” are judged. What, then, is added by Category 3? Perhaps one can say that although a particular structure does not define an operant, operants do have structure; and it may be, for example, that structural features are important for how operants interact. There is a growing interest in bringing behavior analysis to bear on more complex human activities. Sidman (1986) suggests that, although cognitive science may have been too impatient,

Behavior analysis ... has not been impatient enough to attempt all of the syntheses of which it is capable. ... Behavior analysts themselves have not continued to examine the units of their own science in sufficient depth to appreciate whether and how they might be able to account for just those phenomena that concern cognitive scientists. (p. 328)

As we continue to explore higher order behavioral classes and emergent phenomena (e.g., Catania, 1995; Sidman, 1994), perhaps Shimp’s Category 3 will prove useful.

SUMMARY AND CONCLUSIONS

The ability to polarize is testimony to the importance of the molecular–molar issue. Can we begin to circle the wagons around the question of “What counts as behavior?” At first glance, it appears that Baum’s multiscale molarism and Shimp’s Category 3 might be viewed as olive branches. Indeed, Shimp noted at the conference that he “was pleased to hear Baum say that his position is very similar to mine” (p. 305). But are their positions really all that that similar? Although he has added the *multiscale* modifier, Baum’s basic view seems relatively unchanged. Although Shimp’s Category 3 appears to incorporate the strengths of both molecular and molar approaches, he still seems quite focused on the structure of operant behavior; the molar functions are not basic in any sense. In any event, whether or not their positions

really are similar after all, the field is indebted to Baum and Shimp for their willingness to tackle tough questions. Besides, it’s fun to argue.

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