

The natural selection: behavior analysis as a natural science

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Most behavior analysts emphasize that a science of behavior must be a *natural* science as opposed to a *social* science or any other such description. But what does this mean and why is it important? I explore these questions by attempting to characterize the designation “natural science” and briefly surveying what behavior analysis has in common with other putative natural sciences. Included in the discussion are problems of agency, background assumptions, mechanism, and some examples of shared problems and issues with other natural sciences. Special focus is placed on behavior analysis as a biological science and the implications of that status.

Keywords: natural science, natural selection, agency, realism, pragmatism, contextualism, mechanism, dynamics.

Introduction

In understanding behavior analysis as a natural science, we need to examine ties between behavior analysis and other natural sciences—this is my overall theme.

Is It Natural?

“There can be a science of behavior” Baum (2005) asserts in his book, *Understanding Behaviorism*, echoing Skinner’s (1953) earlier call in *Science and Human Behavior*. Moreover, this science must be a *natural science*, presumably placed alongside physics, chemistry, meteorology, and biology, just to name a few examples. This assertion begs the question of what it means to talk about a natural science. The old and unresolved demarcation problem addressing the question of what makes for a science

as opposed to a non-science is now proposed within science itself. The label “natural” is presumably placed in opposition to some other specific label, for example, “social” as in social science, but not in opposition to “unnatural” or “supernatural”—we could not understand the meaning of a science of the “unnatural”. Surely however, this does not help us to distinguish a natural science from, say, a social science. The latter, by some academic categorizations, typically includes history, political science, sociology, cultural anthropology, economics, and, often, psychology. These might be identified as “human sciences”—concerned with human affairs broadly considered, investigated by a wide range of methods, many, if not most of which, are distinct from those in the natural sciences, and with the possible exception of economics, the social sciences appear to lack any coherent, consistent theoretical and empirical framework.

A natural science is about the natural world—its discovery, careful description, and essentially how it works. But again, you ask, what does “natural” mean? The problem here is essentially historical as well as conceptual. Though the words “science” and “scientist” only entered the English vocabulary in the 19th

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century, systematic explorations of the world in biology, physics, astronomy, chemistry, and the like greatly antedate this terminology from at least Aristotle's time. Under the distant influence of Aristotle, Thomas Aquinas along with other scholastics in the eleventh and twelfth centuries insisted on distinguishing the truths gained through reason and systematic observation applied to nature from those of faith gained through revelation. Indeed, contrary to common belief, this period in the late middle ages saw the true birth of experimental science. "Nature" then, for at least the last 2500 years or so, is seen basically as what we engage with in our environment—trees, stars, rain, tides, fire, birds, bees, birth, death, and decay. Of course, we know these sorts of nominally observable entities and events in no way exhaust nature's storehouse of phenomena—much of interest in natural science lies outside anything like ordinary observation.

But what about *behavior* as a phenomenon to be explored as one might study, say, motion, electromagnetics, or chemical reactions? Here the story naturally becomes complicated. Prediction and control of behavior must have origins in prehistory—their survival value is obvious. But not until very recently in human history was the ability to predict and control behavior the outcome of a systematic, that is, scientific study. Rather, prediction and control were gained through personal experience and its cultural transmission. This was, of course, mostly true of other natural phenomena as well. How to build a house, fashion a weapon, construct an aqueduct, predict the seasons, and many other useful activities also depended on extensive experience that ultimately became "rule-governed", as we say, and thus formed the bases for development of a proper science in addition to engineering. Much of this "proto-science/engineering" was based upon discoveries and inventions made through various means, including trial and error and just plain chance. The key idea here is *discovery*. Human behavior (and, perhaps to some extent, the behavior of other animals) was not a discovery in the usual sense of discovery like developing a new alloy, finding another moon of Saturn, or conceiving a

keystone to hold up an arch—behavior, including our own, is all around us; we're immersed in it as are fish in the sea. Even today, at least some philosophers would assert that there can be no real science of behavior because, among other things, there is nothing to discover about behavior we don't already know. The issue for them is presumably just how best to *talk* about it. Moreover, some assert that even granting there can be something like a science of, say, social behavior, there can be no *laws* of behavior as one finds in physics or chemistry for example. The first of these assertions, that there is nothing to discover about our behavior is, I think, simply a confusion; the second one about laws, or the lack of them, is more challenging to address.

I'll return to these issues subsequently, but, before I undertake that task, there is another unique aspect of behavior as seen historically and conceptually that militates against any possible natural scientific treatment—however one defines "natural". There is much complexity here, but the essential problem is with the notion of *agency*. No object or phenomenon of study in any natural science is considered to be an agent unto itself, never mind endowed with some quality deemed "free will". If we mean by free will that at least some of our actions are *independent of any controlling variables*, then any scientific account is, by definition, impossible. Matters of definition are not trivial here, but I think most people believe that at least some of their choices are "free" and somehow emanate from within in ways not accountable for by science. As behavior analysts, we recognize the problem of choice not as how an agent comes to "choose", but as one of understanding *how* behaviors may be distributed across available alternatives and *what* variables control such distributions. If these distributions were independent of any controlling variables, then attempts to understand behavior would, at best, reside in another domain outside of the interests, concerns, and abilities of science. Billiard balls, stars, DNA molecules, electrons, *E-coli*, and even the weather are not considered to have a will of their own—however complex their actions. In any case, such examples and countless others are considered to be subject

to scientific analysis in that their activities and functions are at least potentially understandable via systematic investigations and invocation of principles, laws, and models allowing effective description and prediction, if not control. And, in fact, the attribution of free agency to human behavior contradicts actual experience, given that our individual and cultural survival, as well as survival as a species, depends on effective prediction and control of behavior. “Effective” here obviously doesn’t mean to any arbitrary degree of precision—like natural selection, prediction and control only have to be good enough most of the time. That we may fail in our predictions in quite significant cases appears to give some credence to the free-agent point of view and perhaps those who hold such a view, if charitable enough, would see a putative science of behavior as a kind of promissory note on which its practitioners must make good. But, one just as well demand the same of other complex phenomena like the weather. The topic of *determinism* is obviously relevant here and I’ll return briefly to this issue subsequently.

But returning to the matter of discovery, is it the case that because human behavior is so familiar an encounter that we have nothing new to learn about it? I asserted earlier that this is a kind of confusion—why? There are at least three reasons that come to mind.

First of all, we may marvel at a writer’s skill in creating characters through literary techniques of refined description. Don Quixote, Hamlet, Ahab, Huckleberry Finn, and Leopold Bloom were created through adroit and refined descriptions of their actions, but the essential point is that these actions were *patterened*—patterns of behavior emerge to constitute the character—relatively consistent actions under different circumstances, what many might call “personality”. The emphasis here is on *description* as opposed to *analysis*; indeed, detailed analysis would typically detract from the literary achievement. But is description, however detailed, enough? B. F. Skinner, who began his career as a creative writer, ultimately felt he had nothing to say as a novelist. In part, he became disillusioned with the value of literature in understanding behavior and pursued its sci-

ence as opposed to its art. As he said, “A writer might portray human behavior accurately, but he did not therefore understand it” (Bjork, 1993, p. 57).

Second, consideration of the role of familiarity in discouraging discovery might be applied to motion, domestic animals, or, again, the weather. In all these cases, the *science* came late in human history. Though there were antecedents as early as the 14th century, the beginnings of an analytic description of motion based on empirical study did not truly occur until the 17th century with Galileo. It took a Darwin who by sifting and sorting massive quantities of evidence over many years to see that all the myriad species of organic beings could be understood as having a common origin and their variety emerging through mechanisms of natural selection. As for the weather, we are still learning much about its basic mechanisms, but meteorology draws from many well-established sciences: thermodynamics, fluid dynamics, geology, oceanography, botany, and others.

Third, reaching beyond description toward analysis, reasonably effective prediction and control of human behavior is essential to everyday social functioning, but this in no way implies that we must all be behavior scientists any more than a successful fisherman or farmer is thereby a biologist. As already mentioned, through experience we acquire a repertoire of descriptive attributes, rules of thumb, and vague intuitions about our own and others behavior that, for the most part, allow us to get through our day successfully. This repertoire is sometimes called “folk psychology” and incorporates concepts like beliefs, intentions, desires, reasons, mind, will, and the like to explain behaviors. At least some philosophical approaches to behavior also make considerable use of such concepts and much discussion goes on in the philosophical literature about the nature and role of beliefs, intentions, reasons, desires, and the like. In this domain, such concepts, however defined, serve as the basic means for understanding behavior. Among this group there seems to be little sympathy with, if not an outright denial of, the possibility that human behavior could be understood in any other ways—and especially

through a natural science of behavior. For example, Gordon Foxall, who in putting forth a view he calls “Intentional Behaviorism” (e.g., 2007), asserts that we are *compelled* to emulsify folk psychology and behavior analysis to effectively explain behavior; but, in my view, the two stances are immiscible, and insisting on the necessity of both takes the analysis of human behavior out of the domain of the natural sciences (Marr, 2008). Just where it puts it is not clear. A key issue here centers on just what counts as an explanation in the natural sciences—a very contentious issue I (and many others) have tried to address elsewhere (e.g., Marr, 2003c; 2008) and I’ll not pursue this issue here.

As I mentioned earlier, some philosophers may deny that there can be laws of behavior. In a larger context, this raises the contentious issue of just what counts as a law, a topic of much debate in the philosophy of science (e.g., Curd & Cover, 1998; Hitchcock, 2004; Machamer & Silberstein, 2002). Included in this debate is the question of whether laws are essential to every recognized natural science, biology, for example (Mayr, 1988; 2004; Sober, 2006). As with the term “natural”, the arguments depend largely on how the term “law” should be applied. To unwrap these debates, however interesting, would take far more space than I wish to devote here. Allow me simply to observe that if our concept of a law includes expression of a sufficiently general regularity in phenomena of interest that also plays a significant role in the explanatory schemes within the science, then even for a folk psychology to exist, there must be a recognizable *order* or pattern to behavior—a discernable order is the first requirement of any scientific approach. From systematic explorations of that order emerge principles and laws as well as reformulations of how we conceptualize behavior.

Relevant Relations and Issues

To return to the primary question of behavior analysis as a natural science, I have already argued that, in common with other natural phenomena, there is a discoverable order to behavior and that to transform that order into a natural science one must eliminate any ap-

peal to free agency. But these essentials are only a starting point for *any* science worthy of the name. My approach to the question is to examine various relations between behavior analysis and other, unquestioned, natural sciences like physics and biology. For example, what do we share and do not share with these sciences—in assumptions, concepts, content, methods, models, limitations, controversies, achievements, and goals? Obviously, I cannot in this paper give much detail on all these features and activities, but the following are some issues I think pertinent to relating behavior analysis to other natural sciences:

- *1. Ontology, Epistemology, and Patterns of Explanation
- *2. Behavior Analysis as a Biological Science
3. Contingency: The Fundamental Explanatory Concept
4. The Problem of Behavioral Units
5. The Role of Symmetry
6. Mathematical Models and Dynamical Systems
7. Problems of Reductionism
8. Scientific and Mathematical Verbal Behavior
9. Creativity in the Sciences and Mathematics

In this essay I’ll focus briefly and selectively only on the first two (starred) items in this paper, but the other items are equally important and reflective of problems shared by many, if not most, natural sciences. I have previously published some papers on certain aspects of all these topics (e.g., Marr, 1986, 1990, 1993, 1995, 1996, 2003a, 2003b, 2003c, 2004, 2006).

There are also numerous background issues here, that, while certainly important, will be treated only lightly in this essay. For example, there is much written and debated about putatively fundamental differences between various natural sciences—physics and biology being prime examples (e.g., Mayr, 2004). On the other hand, there are no clear dividing lines between scientific disciplines—a fact ever more obvious in any modern understanding of the world. Nature is certainly not fractured along the lines of any real or proposed academic categories—indeed, it’s not fractured at all.

Ontology and Epistemology

How shall we understand behavior? That is, if we grant that all phenomena we label “behavior” reflects the operation of natural forces in the same way we understand, say, motion, or physiology, or magnetism, or embryological development, then what other assumptions or commitments are entailed?

Radical behaviorism is said to be the philosophy of the science of behavior analysis. But what is modern radical behaviorism about and how is the *science* of behavior analysis related to this putative philosophical base? Behaviorism is still trying to sort itself out in quite fundamental ways (e.g., Baum, 2005; Chiesa, 1992; Moore, 2008; Rachlin, 1991, Staddon, 2001; Zuriff, 1985); and for at least the past couple of decades and continuing apace, many calling themselves radical behaviorists have debated endlessly on just what being such a behaviorist entails. The dimensions of these debates range widely but largely center on concerns of ontology and epistemology and are typically set up as putative polarities: *realism vs. pragmatism*, *description vs. explanation*, *molarism vs. molecularism*, *social constructivism vs. scientific empiricism*, *biology vs. physics as models*, *reductionism vs. holism*, and, of course, *mechanism vs. contextualism*. Based on these kinds of debates within the enterprise, “radical behaviorism” appears, at best, to be a family-resemblance term. Yet what is too often expressed is a kind of lust after a *Weltanschauung*—a single world view upon which to build the philosophical foundations for a science of behavior. Many, if not all, these polar distinctions occasioning the endless debates over conceptual issues are directly or indirectly tied to what it means to talk about *mechanism*. This term is, I believe, very much misunderstood by many behaviorists as I’ll subsequently attempt to demonstrate. But before I approach this thorny topic, let me begin with perhaps a more fundamental contentious “dichotomy”—realism versus pragmatism.

Really Successful

Realism versus pragmatism has been one of the more hotly debated issues in the philosophy

of science, but the nature of the debates with respect to, say, physics, are largely different than what is apparently contentious in behaviorism and behavior analysis (e.g., Devitt, 1997; Godfrey-Smith, 2003; Gregory, 1988; Laudan, 1990). Most any treatment of realism recognizes two essential dimensions: *existence* and *independence*. “Realism” in much of philosophy of science has mostly to do with asserting the existence of certain theoretical “unobservables” like electrons or quarks, and not that, say, the moon or Oslo is some sort of illusion. One very eminent behaviorist, Baum (2005), appears to believe *all* is illusion (or, at least, we can’t show that it is not!), but such a transcendental viewpoint is an odd foundational assumption for attempting to understand the world in any consistent, coherent, and comprehensible scientific sense.

In addition to asserting the existence of *something*, the scientific realist advocates the notion that science is about a world that can exist independently of our interests and concerns; and, moreover, that science can tell us something about this world that, by and large, is at least an approximation to what is the case—a kind of progress without, one should emphasize, necessarily attaining some ultimate Truth. That science demonstrates progress has been a controversial issue in the philosophy of science (Curd & Cover, 1998; Kitcher, 1993; Laudan, 1990), but much of the controversy seems to hinge on various uses of the term “progress”. Without pursuing this further here, surely no one would argue that say, Benjamin Franklin knew as much about electricity as we do today—there are *facts* of the matter.

Pragmatism, like realism, comes in various flavors, but, in general, is agnostic about just what the scientific realist might claim about the world, and evaluates scientific achievements on the bases of various measures of success—findings in agreement with theory, correct predictions, effective control of phenomena, etc. Of course, all scientists, realists and pragmatists, judge scientific success on these bases and this is, in part, why science itself is the most successful endeavor for discovering how the world works. But these “successful working” criteria beg the

question of just *why* some methods, theories, and so on, are more successful than others. From this point of view, pragmatism appears to be parasitic on some form of realism—successful working as an epistemological stance must be founded on a realist ontology. Regarding the success of scientific practices, even Larry Laudan, the most sophisticated pragmatist I know of admits:

...there must be something right about the rules...since a randomly selected set of rules for judging beliefs would not exhibit the striking success shown by the theories of the natural sciences. Unless the rules of scientific method reflect something about the 'facts' of the universe, scientific inquiry would be nothing like as successful as it is." (1990, p. 102).

With regard to "successful working", I have emphasized order and prediction, the latter implicate in the former, as essential elements of a natural science, but that prediction is never to an arbitrary degree of precision. Thus determinism in science is not some metaphysical ideal, which at the least would have the scientist know the functional role of every possible controlling variable, including all relevant initial and boundary conditions, to infinite precision. Rather, prediction *is* a pragmatic achievement, always constrained by one or more of the following conditions: ignorance of relevant variables, errors in measurement, irreducible stochastic properties of the system of interest, and certain non-linearities which can produce super-sensitivities to initial conditions. For example, predicting the weather is limited by most of these conditions so that there is an inherent limitation on prediction beyond a few days. Without question, behavior is also in the category of constrained predictability, but as behavior analysts, unlike the meteorologist, we have the possibility of *control*, an advantage that allows us to test the predictability of our principles directly.

Without, I believe, agreeing with Baum that all is illusion, some behaviorists are anti-realists by virtue of questioning a world independent of us and express this stance as: *What is known is always a behavioral function* (Barnes-Holmes, 2000). Certainly, "knowing", in its many

manifestations, is trivially a behavioral function, but for at least some behavioral pragmatists *all* events are defined or known as behavioral functions instead of physical events that can exist independently of behavior. Thus, an apple, say, exists only in terms of our behavioral interactions with it; it has *no* existence independently of behavior. Thus we are truly the *only* measure of all things. That this strange principle also applies to scientific practice is apparently inspired by Skinner's (1957) emphasis on science as largely a verbal practice and thus the scientist is subject to contingencies and cannot stand outside the world under study. The non-realists take this to mean that the scientist (or behavioral pragmatist) has no special view point and thus can make no discoveries about the world as it is, but can only attempt to work successfully within the depths of the behavioral stream. To quote Barnes-Holmes:

From this perspective, the output pattern from a cumulative record...is not a representation of what the rat or pigeon 'really' did in the operant chamber. Instead, the pattern may be defined as a discriminative stimulus for a particular 'scientific' response...that has been differentially reinforced in the presence of that pattern." (2000, p. 198)

Why not *both*—what really happened *and* a discriminative stimulus? Even if one agreed with Barnes-Holmes, the whole approach is a complete distortion of Skinner's analysis of logical and scientific behavior, as well as his unachieved goal of developing a behavioral epistemology. To quote Skinner:

When a speaker *accurately* reports, identifies, or describes a given state of affairs, he increases the likelihood that a listener will act successfully with respect to it, and when the listener looks to the speaker for an extension of his own sensory capacities, or for contact with distant events, or for an *accurate* characterization of a puzzling situation, the speaker's behavior is most useful if the environmental control has not been disturbed by other variables (1957, p. 418, italics mine). And:

Differences in thought processes have been attributed to the apparent differences between the laws of religions or governments and the laws

of science. The first are said to be “made”, the second merely discovered, but the difference is not in the laws, but in the contingencies the laws describe. The laws of religions and governments codify contingencies of reinforcement maintained by social environments. The laws of science describe contingencies which prevail in the environment *quite apart from any deliberate human action* (1974, p. 124, italics mine).

When attempting to understand the current varieties of what might be called *pragmatic idealism*, particularly as applied to the behavior of the scientist, I get the impression that a number of behaviorists, in common with some other philosophers, see verbal behavior as a kind of bedrock foundation for approaching everything else as if such behavior was not also shaped by the world—sunshine, rain, stars, sex, water, food, predators, birds, falling rocks, death—all around before humans walked the earth and certainly independent of our individual existence. The sort of perspective that seems to emerge from idealistic pragmatism is a curious mixture of solipsism, radical skepticism, hopeless ignorance, and a notion that Nature is out to trick us into believing we know anything at all—an utter disconnect between the world and our descriptions of the world. But I say there are reasons *why* some things work and others don’t—as most compellingly demonstrated in the results of hundreds if not thousands of years of successful engineering and scientific efforts.

How Does It Work?

The *mechanism-contextualism* debate among behaviorists tends to get conflated with the realism-pragmatism argument and, in any case, has little, if any, correspondence in other sciences. Perhaps distantly related are certain discussions of putative distinctions between biology and physics (e.g., Mayr, 2004). The issues here largely involve problems of generality, reduction, and emergence, including the role (or non-role) of laws.

Developments and discussions over the years centering on what behaviorism should become when it grows up has continued to reveal not only considerable intellectual diversity but, I believe, unbridgeable chasms of opinion about

how to think about a science of behavior. I believe many, if not most of the distinctions are imbedded in the contextualist-mechanist debate. The avowed contextualists have set the agenda by asserting a distinct dichotomy between their positions and something they call “mechanism”. Contextualism (if one is forced into an “ism”!) can be said to emphasize the following:

1. The ongoing act in context as the unit of analysis.
2. Focus on the whole event.
3. Sensitivity to the role of context in understanding the event.
4. “Successful working” as a pragmatic truth criterion.

Now, for the most part, these criteria seem rather benign and a science of behavior would find some resonance in the study of ongoing acts in context, the role of the environment in the control and modification of behavior, and *properly formulated*, “successful working” as a means of evaluating research and application. Most of those identified as mechanists by contextualists would not fuss much about these criteria. But, as I have already indicated, the metaphysical fiddling doesn’t stop here. Inspired by Pepper (1942), “contextualism” had to be put in opposition to “mechanism”; each was saddled with characteristics to support that opposition and behaviorism was then shoe-horned into the contextualist camp. Clearly, such a divisive challenge could *not* have been initiated by folks calling themselves ‘mechanists’—no one in behavior analysis go around calling themselves such. In some context they might simply have said they were scientists trying to understand behavior. In this sense, to assert, never mind challenge, one of these folks by calling that person a “mechanist” is analogous to reminding the Pope that he’s a Catholic or my being told that what I was writing was prose.

As for mechanism, the term has many meanings but those calling themselves contextualists seem to focus on examples like levers, billiard balls, old wind-up clocks, and such—a kind of toy or grease-monkey mechanism. Beyond the extraordinarily rich and sophisticated fields of classical, relativistic,

and quantum mechanics, which the orthodox contextualists apparently ignored, real and proposed mechanisms are, of course, part of *all* natural sciences—we invoke mechanisms for photosynthesis, genetic expression, natural selection, solar energy, magnetic induction, crystal formation, rain, stimulus control, schedule performance, choice, and even relational frames. David Palmer, in responding to two well-known contextualists, Hayes and Barnes-Holmes, said it best:

In preparing for my book review I struggled to understand relational frame theory, and I was puzzled by the magnitude of that struggle. Why should I have so much trouble understanding the prose of other behavior analysts? In their vigorous rejoinder to my review Hayes and Barnes-Holmes...ask the same question and offer the following suggestion: Together with other critical reviewers...I represent a *mechanistic philosophical tradition*, incompatible with the authors' contextualistic stance. The term *mechanistic* is used pejoratively, but I don't understand why. *I just want to know how the world works.* (2004, p. 225, my italics)

Hayes and Barnes-Holmes would assert this to be a meaningless aspiration—the rest of science would, I think, profoundly disagree.

If you want to know what mechanism in our field is about, just look at what good behavior analysts—basic and applied—*do*. Without question, the fundamental explanatory device in behavior analysis is *contingency*, that is, the relations between events, for example, situations, behaviors, and consequences. Our field explores environment-behavior relations to develop principles accounting for the acquisition, maintenance, and dynamic changes in patterns of behavior—in other words, *mechanisms* of contingencies. These principles, in turn, allow for achieving prediction and control of behavior in the same way principles in physics, chemistry, and physiology can achieve these same goals in their respective fields. In these other sciences, one is not abashed at a question such as, “What is the mechanism of this phenomenon?” In Clark Maxwell's childhood expression of wonder: “What is the go of that?”

What kinds of mechanisms?

The Rebel with a Cause

While behavior analysis emerged largely from academic psychology, its methodology derived from classical physiology and pharmacology—the methods of these, in turn, can be traced back to experimental physics, for example, as first conducted by Galileo. Given this powerful methodological legacy and its unique perspectives on how to understand behavior, behavior analysis has never been comfortable in the bosom of psychology and through the years has grown even further from it. There is little need to review this rather grim history except to say that the insistence by behavior analysis on being recognized as a coherent, if not comprehensive, natural science has also played a major role in its estrangement from main-stream psychology—much of which seems remote from anything like a natural science, in content and training. More than ever, the field of psychology reminds me of The Yellow Pages, but at least in that bulky source there is some organization—the arrangement of topics is alphabetical! Introductory psychology texts are a cruel joke—dumbed-down, timid, eclectic picture-books bearing no resemblance whatever to any serious introductory books in any other natural science, biology included. *If one can open the book to any chapter and start there without having mastered the earlier chapters, then you know you're not dealing with anything coherent, much less any enterprise worthy of the title of natural science.* Unfortunately, behavior analysis must pay dearly by its isolation and its viability as an independent field is, at best, uncertain.

Behavior Analysis as a Biological Science

But championing behavior analysis as an independent field when viewed in a wider scientific context is far too provincial a perspective. Although a significant number of behavior analysts would disagree, I strongly side with Skinner that behavior analysis is a branch of biology; after all, behavior is a biological function of organisms—what else could it possibly be? (For similar views, see, e.g., Donahoe,

1996; Donahoe & Palmer, 2004) Aristotle recognized that behavior is the first criterion that one is dealing with a living being—action essentially defines life and is a major factor in biological adaptation. All of us view operant behavior as dynamically adaptive. Indeed, the very possibility of behavior-consequence interactions yielding new behaviors must have emerged from evolutionary mechanisms. More specifically, for example, contingency effects in engendering consistent patterns of behavior show a species generality that rivals the tricarboxylic acid (TCA) cycle. Moreover, operant conditioning can be demonstrated down to the cellular level—we must be tapping into some quite fundamental biological processes for these to be the case (see, e.g., Byrne & Roberts, 2009). In any case, behavior is but one class of phenomena that collectively may be described as a general fund of biological explanation:

1. Molecular (biochemical, biophysical)
2. Cellular functions
3. Tissue/organ functions
4. Morphogenic/developmental
5. Behavioral/environmental
6. Species adaptation/evolution

While behavior is clearly worthy of study in its own right (like, say, metabolism), to provide a fuller account, at some point we need to understand how various biological functions interrelate. Behavior as a biological property of organisms naturally raises questions of relations between behavior and other biological functions at different levels of analysis, including the role of neural processes in behavior—an aspect of reductionism that must be treated seriously in any account of behavior analysis as a natural science, but I'll not deal with this very complex issue in this paper (but see Marr, 1990).

Placing behavior analysis in the bosom of biology constrains the kinds of mechanisms one might invoke. A popular root metaphor for operant conditioning, if I may steal that phrase from the contextualists' hero, Pepper (1942), has been "selection", as in Darwin's natural selection. I think Skinner (1953) first suggested this in *Science and Human Behavior*

more than a half-century ago and the idea has continued to flourish. In conformity with the great evolutionary biologist Ernst Mayr, a few behaviorists have emphasized the special character of biological explanation in contrast with those in the physical sciences. Ernst Mayr (1988, 2004) has discussed several characteristics he believes distinguishes the biological from the physical sciences. In a paper (1996) published a while back titled *The Mingled Yarn*, I tried to show that many of these could be placed within an integrated approach of *dynamical systems*, thus relating a number of natural sciences together with a common quantitative perspective. I'll say a bit more about this shortly.

In the spirit of Mayr's distinctions, Baum (2004), Chiesa, (1994), and Donahoe & Palmer (2004) among others, have emphasized the role of what might be deemed "historical causation" shared by natural selection and the selection of operant behavior. To be sure, behavior analysis often appeals to history in attempts to understand current behavior; presumably, this avoids talking about "memory" and the surrogate becomes "action-at a temporal distance" (see Marr, 2008, for a critique of this misguided tactic). But historical causation as selection in conjunction with variation acting over time is not typically viewed as "mechanical." The reason given is that "mechanical" refers to immediate and reversible (e.g., "billiard ball"?) causation, setting it apart from history. Once again, a "straw mechanism" is invoked (see, e.g., Marr, 1993, 1996). Moreover, I find it curious that behavior analysts often emphasize the necessity of immediate delivery of consequences for behavior in order to effect appropriate changes in the subsequent probability of that behavior. Presumably "immediate causation", at least, has an important standing in the selection and modification of behavior. Perhaps more fundamentally, just how much time is needed to invoke "historical" as opposed to "immediate" causation?

Now, here's a case of an unambiguous *mechanical* selection process yielding an outcome familiar to biologists (From Weaver, 1982, p. 261):

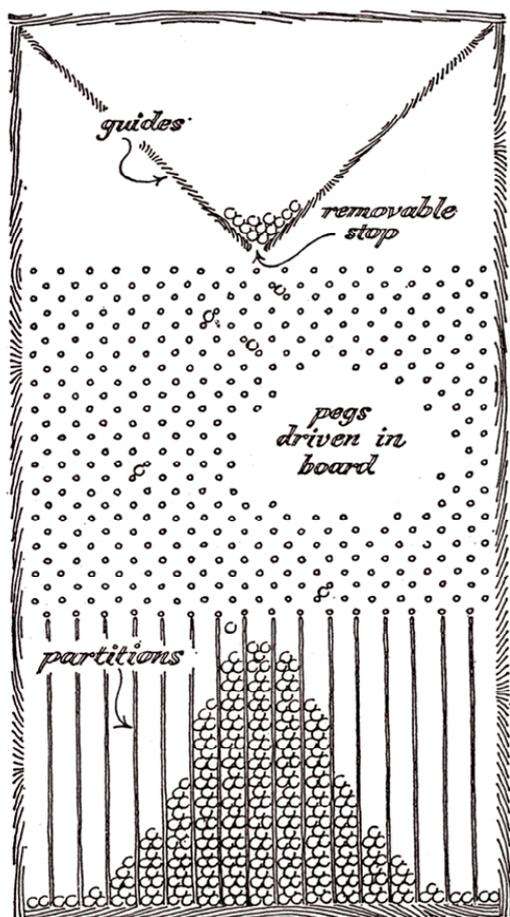


FIG. 42 *The Quincunx*

This is called a *quincunx* and is a kind of concatenated binomial selector yielding a distribution approaching the normal density function. Of course, one could change the distribution in innumerable ways by, for example, bending or removing pegs, changing the number or even the shape of the balls; in other words, changing both the environment and the “organism”. Of course, we could imagine the balls entering this device say, at the rate of one per day. Over the course of history, these individual and surely mechanical events would emerge as a distribution through selection. *History is just mechanism over time.* We could, for example, generate a mathematical model of biological selection (and there are many) that would capture certain common *dynamics* of interacting variation and

selection, as in this simple case. Finally, even Mayr characterized natural selection as follows:

Darwin’s natural selection was his most daring, and most novel, theory. It dealt with the *mechanism* of evolutionary change and, more particularly how this *mechanism* could account for the seeming harmony and adaptation of the organic world. It attempted to provide a natural explanation in place of the supernatural one of natural theology. Darwin’s theory for this natural selection was unique. There was nothing like it in the whole philosophical literature from the pre-Socratics to Descartes, Leibnitz, Hume, or Kant. *It replaced teleology in nature with an essentially mechanical explanation* (2004, p. 109, italics mine).

The metaphor of natural selection applied to operant behavior is based on three aspects—variation, selection, and retention. This seems to be as far as the metaphor can go. The actual processes involved in micro- and macro-evolution, including speciation, are immensely varied and complex, and, in many cases controversial, or simply unknown.

Sources of Variation

Variation, of course, is the *Anlage* from which all else emerges, given a changing environment. But here are only some of the possible sources of biological variation that one way or another can contribute to species development. Some of these lie outside considerations of selection (for example, genetic drift), at least as the behavior analyst might typically view it.

1. MEIOSIS PROCESSES (e.g., recombination, linkage distance)
2. SEGREGATION (e.g., independent assortment, dominance, incomplete dominance, epistasis, pleiotropy)
3. NON-MENDELIAN PROCESSES (e.g., cytoplasmic inheritance, dependent assortment)
4. CHROMOSOMAL VARIATIONS (e.g., polyploidy, deletions, duplications, inversions, translocations)
5. MUTATIONS (e.g., transitions, transversions, tautomeric, regulatory effects)
6. QUANTITATIVE (e.g., polygenic expression, genetic drift, gene-environment)

interaction)

7. DEVELOPMENTAL DYNAMICS (e.g., “Evo-Devo”)
8. *IN UTERO* HISTORY
9. ALLOPATRIC, PARAPATRIC, AND SYMPATRIC ISOLATION
10. STOCHASTIC / CHAOTIC PHYSIOLOGICAL PROCESSES

Note that only a few of these are directly related to behavioral mechanisms, and even fewer are of much interest to behaviorists. Of course, some of these sources may contribute to behavioral variation in organisms, but here are a few, more proximal, sources of behavioral variation.

1. REFLEX PATTERNS AND THEIR THRESHOLDS
2. SPECIES-SPECIFIC SENSORY / MOTOR PROGRAMS
3. CONTINGENCIES AND DIFFERENTIAL SENSITIVITY TO THEM
4. VARIATION AS A RESPONSE CLASS
5. SELF-ORGANIZATIONAL PROCESSES: EMERGENCE
SOCIAL/CULTURAL DYNAMICS
6. A HOST OF INDIVIDUAL DIFFERENCES RELATED TO ALL THE ABOVE PLUS MUCH MORE

Note there is not much analogical overlap here with the many biological sources already presented. But more importantly, we understand this list of sources contributing to behavioral variation far less than biologists understand most of those earlier presented. Regarding the variation aspect of the selection metaphor, unlike many examples from genetics and developmental biology, behavior analysts have little to say about *sources* of variation in behavior, or even the moment-to-moment occurrences of behavior. Operants are said to be simply “emitted”. Once a behavior occurs, of course, we can do many things with it, including the shaping of variation itself. On the surface, we appear to be more like old-time selective breeders—producing a fatter pig, a more productive cow, or a friendlier dog—we kind of know what to do, but not very much about how it works. Certainly, we have to have a pig, cow, or dog to start with.

Shaping and Selection

Much effort has been placed on a putative analogy between operant class and biological species. But biologists have focused immense conceptual energies on how the term “species” might best be defined. An informative book on speciation by Coyne & Orr (2004) asserts that more than 25 different formulations or definitions of “species” can be found in the literature and the list is apparently still growing. This begs the question of just what analogy relates operant class to species. To make matters more misty, behavior analysts also struggle with how best to conceptualize “operant class” (e.g., Baum, 2005; Catania, 2007, Donahoe & Palmer, 2004; Glenn, 2003, Hull, Langman & Glenn, 2001). Glenn, who has written much on the topic of selection (e.g., 1988, 2003, 2004), characterizes the operant in terms of classes of lineages perhaps in analogy with a couple of species perspectives treated by Coyle and Orr (2004) that key into lineages. If I understand her formulation (and I may well not), one begins with a “behavioral event” treated as an entity or individual with multiple properties. A lineage is created through selection by consequences acting on properties of the behavioral event so that an operant is a lineage of behavioral occurrences. This has some dynamic appeal, but leaves open some questions: As stated, I don’t see a place for what some behavior analysts call a “generalized operant class” (or even “higher-order operant class”). If “operant class” is not a clear category, surely higher-order operant class is even more vague (see, e.g., Palmer, 2004), but let us assume there is such a thing (presumably we would have to extend the analogy from the species to at least the genus level!). If the lineage is based on differential selection of current properties of an individual “behavioral event” how would so-called operant classes such as generalized imitation emerge when there may be few, if any, shared properties among individual behavioral events? I assume one might see these as multiple lineages—a class, as it were, but how would that class be defined—can a class itself be a lineage? Perhaps more problematical for me is a behavioral event, typically a continuum, essentially made up of “parts” to be differentially

acted upon by some selection mechanism. Are these parts assumed to be independent of each other? If not, what then does it mean to talk about the properties comprising the behavioral event? One may find it relatively easy to characterize say, a pigeon's key peck into differentiable features, but even in this example, complexities abound. I suppose one could *define* the properties on the basis of what is modifiable through selection, but then how would you know what to select? And, could you say that *only* that property was modified? Perhaps that putative property was embedded in some other property or is co-extensive with it.

Yes, analogies can be made along the lines of variation, selection, and retention. But behavior analysts seem to have only some reasonable grasp of selection; for the most part, variation is taken as a given (and its sources obscure); and, as for retention, by and large, we leave that to the cognitive psychologists. For those behaviorists who are committed to "action-at-a-temporal-distance", the retention aspect of the analogy is simply assumed or otherwise ignored.

I guess my basic question is: What value is found in pursuing the analogy between natural selection and the modification of operant behavior? Does this effort help us to understand either process better; does it help us conceptualize and design better experiments; does it help us to explain behavior analysis to those in other fields? In any case, outside of biologists, most scientists are not that familiar with evolutionary mechanisms. Conversely, are we wasting our conceptual energies in trying to massage this metaphor beyond all possibility of relevance?

Certainly, the analogy illustrates how a retentive selection process acting on variation can lead to complexity—we *know* this with evolutionary biology—the evidence from many sources at every level of biological function is beyond question. But can the promise of the role of selection in behavior analysis in understanding complex behavior be compellingly and convincingly fulfilled? That remains an open question, but only the science of behavior analysis can achieve this goal by itself; it cannot be attained by seeking analogies from another science. I'm not saying that there has been no

value whatever in playing with the analogy, but the more we come to understand the dynamics of behavior, particularly the development of complex performances, the less we will need to pursue analogies from any other sources.

Metaphors as Models

But there is an exception—what might be called *analytic similarity*. Without question, the specific mechanisms involved in natural selection and in behavioral selection are very different. Moreover, there are far more processes operating in biological selection than anything we understand in behavioral selection. The three-step aspects of variation, selection, and retention are more than fairly general and, and in my view, in order for an analogy to be useful, something more would have to be elaborated—for example, analytic similarities. What this involves is looking for *shared dynamic processes with a common mathematical structure*. For example, there are certain analytic similarities between mechanical and electrical systems, so we can explore with profit electrical circuits that mimic mechanical systems. In the domain of selection dynamics, a major example is the application of *genetic algorithms*.

Jack McDowell and his colleagues (2004, 2008) have ingeniously applied genetic algorithms to simulate the effects of certain schedules of reinforcement. The results are astonishing and even mysterious in reproducing many features of schedule performance well-known from laboratory studies. The work is brilliant by any measure, but in no way *had* to depend specifically on the natural selection metaphor pursued by behaviorists. For example, genetic algorithms are applied fruitfully to many problems in engineering without any consideration of analogs with natural selection. They are but one of several analytic methods for finding optimal solutions to some very complex problems, for example, antenna design. Genetic algorithms are thus exemplars of a whole set of computational models that include dynamical programming, simulated annealing, neural networks, and the like (see, e.g., Camazine, et al., 2001; Hannon & Ruth, 1997; Nowak, 2006; Solé & Goodwin, 2000; Stewart, 1998).

In my “Mingled Yarn” paper (1996), I tried to show that all the *qualitative* distinctions Mayr made between physics and biology could be dissolved into *quantitative* ones by consideration of dynamical system approaches. Self-organization, patterning, regulation, development, selection, emergence, irreversibility, meta-stability, population dynamics, predator-prey interaction, and many other biological phenomena can be demonstrated and treated by dynamical modeling. In recent years, these methods have been increasingly applied to behavioral questions, as McDowell’s papers and those of many others illustrate.

There are many other expressions of mechanism in our field, but I want to say that mechanism, as practiced by contemporary behavior analysts is achieving its highest standard in the form of mathematical models of behavior. In other natural sciences mathematical methods are the most powerful ways of figuring out how the world works and behavior analysis is happily joining that good company.

Coda

I have already indicated that the issue of behavior analysis as a natural science is multifaceted and in my essay I’ve only touched on a very few aspects. Assuming you have read this far, I should conclude with a question you may have raised—so what? Why might this issue of behavior analysis as a natural science be important? I confess my interest in this topic has its origin in my earlier training in physics, engineering, and mathematics and later in physiology and pharmacology. My initial attraction to behavior analysis was seeing a cumulative record; the orderliness and beauty of the patterns reminded me of what one can see in the physics lab. That behavior could be explored and revealed with such precision was immensely seductive. Perhaps some of you have had a similar experience. But aside from any personal investment in my topic, there are much more significant reasons.

Perhaps most salient are the methodological and theoretical contributions of other natural sciences to behavior analysis and of behavior

analysis to them. The former include a host of quantitative methods and models including control theory, dynamical systems and complexity theory, genetic algorithms, and connectionist models. Behavior analysis has also been enriched by such fields as evolutionary and ecological biology, neurophysiology and pharmacology. In turn, behavior analysis has given back through its founding of behavioral pharmacology and through countless methodological and conceptual contributions to neurobiology, behavioral ecology, and economics.

There is at least one less obvious contribution, derived in part from a theoretical analysis of scientific and mathematical behavior—what Skinner deemed *behavioral epistemology*—namely effective instructional design in science and mathematics education. I have been privileged to be involved with a National Science Foundation supported effort to improve instruction in calculus-based engineering physics and some of our methods are variations on precision teaching developed by applied behavior analysts (Marr, et al, 1999).

In addition, there are other fields where behavior analysis has had some indirect influence, but could contribute a great deal more. Consider the following book titles: *Consequence-Driven Systems* (Bozinovski, 1995) and *Reinforcement Learning* (Sutton & Barto, 1998). The authors’ names will probably be unfamiliar to you (as our names would be to them), because these books are about *robotic design* where the rudiments of operant behavior have been put to use in designing automatic adaptive systems. A sophisticated as these devices may be, I think they have much to learn from us—and we from them in terms of instantiating models of behavior.

Finally, behavior analysis and its place among the natural sciences have powerful implications for the training of our students and thus for the future of the field. If behavior analysis is really a natural science, then, as with other natural sciences like physics, chemistry, and biology, its academic training should reflect a proper level and breadth of analytic and experimental rigor. With very few exceptions (at least in the USA) students in basic and applied programs in behavior analysis, undergraduate and graduate,

are woefully ignorant of the basic sciences and mathematics. We are all too aware that many students choose fields like psychology precisely because they wish to avoid rigorous science and mathematics courses. As behavior analysts, we need to establish a climate where, for example, quantitative approaches to behavior as well as a thorough grounding in the natural sciences, especially biology, are an inherent part of the curriculum at all levels. As for the common query, if not complaint, of students asking why they need such courses, I'm reminded of Ernst Mach who taught physics to medical students. When asked by these students, "How much physics will a doctor need?" Mach replied, "A doctor always needs exactly as much physics as he knows. Knowledge one does not possess one cannot use".

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