BEYOND TRIAL-AND-ERROR IN A SELECTIONIST PSYCHOLOGY

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ABSTRACT: A common criticism of a selectionist psychology is that the mechanism of overt behavioral variability and environmental selection cannot account for instances of seemingly novel, intelligent behavior. However, behavioral novelty is quite easily accounted for in much the same manner as it is in phylogenetic selection—through an appreciation of historicity. Nonetheless, the issue of novelty is closely related to another issue that is problematic for a selectionist psychology, namely the issue of what constitutes an ontogenetic adaptation. Ontogenetic adaptations simply cannot be defined purely by reference to behavioral units. A solution to this quandary emerges by considering how phylogenetic selection distinguishes between codical and material units. The former are inferred, relational, and persist; the latter are directly observed and temporary. A similar distinction in a selectionist psychology exists between associations and behaviors, and I argue that ontogenetic selection is for associations—not behaviors. Such a stance does not require a conceptual nervous system, although it does subsume operant and classical conditioning phenomena under a common conceptual umbrella. Throughout the paper, various analogies between phylogenetic and ontogenetic selection are considered: Codical units (e.g., genotypes, associations) vs. material units (e.g., phenotypes, behavior), preparedness vs. fitness, instinctive drift vs. allometry, associative drift vs. genetic drift, associants vs. alleles. The object of such comparisons is not to claim that they are accurate but rather to stretch and push the overall analogy between phylogenetic and ontogenetic selectionism so as to delineate the latter more accurately.

Key words: adaptations, behavioral units, historicity, insight, selectionism.

The issue of emergent, or novel, behavior has haunted selectionist approaches to psychology since their inception at the start of the 20th century. Certain animals, so the criticism goes, emit complex behavioral forms that could not possibly have been generated by an iterated, mechanistic process composed of variation and selection. Chimps will spontaneously break a stick from a bush in order to reach for an object outside an enclosure (Köhler, 1925). Hand-reared ravens will pull up a string, which is normally an aversive stimulus, in order to obtain food that is attached to its end (Heinrich, 2000). And, of course, many organisms navigate as if in reference to an internal cognitive map (wasps, Thorpe, 1950; honeybees, Gould & Gould, 1988; rats, Tolman & Honzik, 1930). The common feature in each of these examples is that complex behavior is displayed in the absence of explicit

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training under the presumed environmental contingencies that would gradually shape such behavior.

Results such as these have led some researchers and philosophers to conclude that selectionist principles—behavioral variability and environmental selection—are insufficient for a complete science of psychology (e.g., Dennett, 1995, 1996; Köhler, 1925; Maier, 1931). Daniel Dennett (1996) argues, for example, that in addition to the contrary experimental evidence, the functional disadvantages of a purely selectionist set of mechanisms (or what he terms “ABC learning” for Associationism, Behaviorism, and Connectionist) for generating behavior are simply too great for natural selection not to produce something better. He writes:

[Selectionist] conditioning is a good thing as long as you are not killed by one of your early errors. A better system involves preselection among all the possible behaviors or actions, so that the truly stupid moves are weeded out before they’re hazarded in “real life.” . . . There must be a filter, and any such filter must amount to a sort of inner environment. (Dennett, 1996, p. 88)

To Dennett and others, then, many of the behaviors of humans and nonhuman animals simply appear too adaptive, too intelligent, and too spontaneous for a set of selectionist principles, operating on overt behaviors, to generate. Of course, the sentiment that “something additional” is needed for a selectionist psychology to account for seemingly spontaneous, complex behaviors mirrors, quite closely, critiques of natural selection based on design. As Paley famously noted:

In crossing a heath, suppose I pitched my foot against a stone, and were asked how the stone came to be there; I might possibly answer, that, for anything I knew to the contrary, it had lain there for ever: nor would it perhaps be very easy to show the absurdity of this answer. But suppose I had found a watch upon the ground, and it should be inquired how the watch happened to be in that place; I should hardly think of the answer which I had before given, that for anything I knew, the watch might have always been there . . .

. . . [therefore] the watch must have had a maker: that there must have existed, at some time, and at some place or other, an artificer or artificers, who formed it for the purpose which we find it actually to answer; who comprehended its construction, and designed its use. (As quoted in Dawkins, 1986, p. 4)

In Paley’s passage one need only replace “watch” with “intelligent behavior,” “maker” with “consciousness,” and “place” with “internal environment” to match quite closely the Dennett quote given above. Despite their routing from biology, then, psychology still has its Paleys.

Or does it? Many instances of insightful behavior have in fact been shown to be reducible to principles of overt behavioral variation and environmental selection. As in phylogenetic selection, psychology’s Paleys seem to have under-appreciated the role that historicity (see Williams, 1992) plays in the creation of ontogenetic forms. Historicity or historicism
is the belief that an adequate understanding of the nature of any phenomenon and an adequate assessment of its value are to be gained through considering it in terms of the place which it occupied and the role it played within a process of development. (Mayr, 1982, pp. 129-130)

If, for example, Dennett is correct in assuming that some animals refer to an internal environment before acting in the external environment, we must still account for the development of this internal environment, and such an account will be the province of a selectionist psychology. And yet, I believe psychology’s Paleys, although somewhat misguided as to the flaws of a traditional selectionist psychology, are right to criticize it.

A traditional selectionist psychology is flawed, but at a deeper level than whether its general mechanism can in principle account for the emergence of novel, intelligent behavior. First, a selectionist psychology has often been held to be synonymous with operant conditioning (e.g., Hull, Langman, & Glenn, 2001; Skinner, 1981). This is an unnecessarily narrow and sterile characterization that does not benefit psychology as a whole. Secondly, and as a result of limiting a selectionist psychology to operant conditioning, the traditional units of a selectionist psychology are usually held to be behavioral, and one often reads that ontogenetic selection strengthens behaviors. Such statements are at best sloppy and at worst incorrect, and, I will argue, are derived from the failure of a selectionist psychology to appreciate the difficulty of defining ontogenetic adaptations. Finally, the lack of clarity regarding ontogenetic adaptation dooms a characterization of ontogenetic selection. After all, as in evolutionary biology, adaptations provide the data set that enables one to infer the properties of selection. Adaptations and the units of ontogenetic selection, then, provide the core muddle of a selectionist psychology, and psychology’s Paleys, to their credit, should not feel satisfied until this muddle is cleared up.

Consider, for example, the behavior of a rat in a water maze. A rat placed in a water maze at a fixed position will quickly learn the direct path to a submerged platform that is maintained at a constant position. If we compared the paths traveled by the rat across trials we would typically observe a decrease in total distance traveled and a decrease in the number of turns. We might, therefore, conclude that the behavior has been directly acted upon by a selective process. However, is such a statement correct? Is the behavior of the rat in the water maze an ontogenetic adaptation? Such a question appears absurd until we perform a simple transfer experiment. That is, when the rat is placed in a novel starting position, it will still swim directly to the platform (Eichenbaum, Stewart, & Morris, 1990). The overt behavior of swimming to the platform during initial training, then, cannot be said to have been the locus of selection. Therefore, and counter intuitively, we cannot state with confidence that the efficient change in overt behavior by the rat, that is, its learning curve, was even an ontogenetic adaptation. Indeed, models of such behavior (e.g., Schmajuk & Blair, 1993 for a neural network account; Reid & Staddon, 1998 for a generalization account) are less concerned with the overt behavior than the processes which generate this behavior. Thus, one might reasonably conclude that the behavior exhibited by our
rat in the maze is not itself an adaptation but merely the output of an adaptation—phylogenetic or ontogenetic—existing elsewhere. That is, the result of selection by reinforcement, like selection by differential reproduction, is not “behavior” (or a phenotype) but a process for producing behavior (i.e., a genotype).

The following paper begins with what I will term the “shallow analogy” between a selectionist psychology and natural selection—that “selection by consequences” and “survival of the fittest” are more or less synonymous (Skinner, 1981). This analogy rests on the concept of historicity. This is followed by discussions of ontogenetic adaptations and units. My discussion is purposefully broad in scope, at times both general and specific. My reasoning, however, is that dogmatism, for which psychology has a propensity, is best met by eclecticism.

**Historicity**

Few would question that overt behavioral variability and subsequent environmental selection can produce an almost infinite number of learned stimulus-response and/or operant repertoires, given a sufficient amount of time. As such, the criticism of a selectionist psychology based upon the seemingly spontaneous emergence of “intelligent” behaviors is quite weak.

In order to illustrate this point, consider, an experiment by Köhler (1925) in which a piece of food is put outside an enclosure that houses a chimpanzee. The food is placed beyond the reach of the animal, but within the enclosure is a stick of sufficient length to reach the food. Köhler reports that after first attempting to reach directly for the food, the chimpanzee suddenly grabs the stick and in one smooth motion pulls the food to within reach. In short, the chimpanzee’s behavior appears to be spontaneous, intelligent, and devoid of any overt trial-and-error learning. However, a series of experiments has shown that at the very least this insightful behavior by chimpanzees is dependent upon maturational factors (Jackson, 1942; Schiller, 1952). Older chimps are much more likely to spontaneously use a stick to reach for food. Further, even in mature chimps some general, and possibly quite specific, experience in handling stick-like objects is necessary for the behavior to emerge (Birch, 1945; Schiller, 1952; for similar results with Rhesus monkeys see Shurcliff, Brown, & Stollnitz, 1971).

The insightful behavior observed by Köhler, therefore, becomes the end product of his chimp’s history of trial-and-error—a history that he did not directly observe. One approach of a selectionist psychology, then, when confronted with a complex, novel behavior is to construct a reinforcement history (e.g., a causal chain) that could generate the observed performance. Consider, for example, a scenario for which Dennett claims a selectionist psychology would be inadequate:

Suppose, to borrow one of Skinner’s examples, I am held up and asked for my wallet. This has never happened to me before, so the correct response cannot

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This paper will not attempt to locate these models within the organism. Indeed, I suggest later in the paper that a selectionist psychology benefits from a certain ambivalence toward mapping its units onto, for example, the brain.
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have been “reinforced” for me, yet I do the smart thing: I hand over my wallet.
(Dennett, 1978, p. 66)

Dennett suggests that such smart behavior results from trial and error operating within a hypothetical mental environment. Dennett further claims that such an “ability” defines a class of organisms which he terms “Popperian” (Dennett, 1995). Whereas rats, for instance, might learn a discrimination via the overt selection of “hypotheses” (Krechevsky, 1938; see Köhler, 1959 for a similar view), Popperian creatures test their hypotheses within a mental environment. However, such reasoning fails to take into account the historicity of ontogenetic selection. Certainly, Dennett in his past has been asked by an authority figure to surrender something of value—a mother or father, for instance, demanding the handing over of a toy. Perhaps he played cops-and-robbers as a child. Maybe he has handed over his wallet to a friend or a child. Maybe he has read a story or watched a television program in which an individual handed over their wallet to a thief. Within such reinforcement histories would lurk the pieces selected by present circumstances. We need only suggest a mechanism such as that proposed by Estes (1950, 1953) in which a pool of stimulus-response elements are “drawn upon” by the current environment. Even if Dennett had never explicitly been rewarded for handing his wallet over to a mugger, he might have been “preadapted” by his reinforcement history to do so.

A preadaptation, or more accurately an exaptation (Gould & Vrba, 1982), is defined as a feature, selected explicitly for one purpose/function, being “coopted” for another function. For example, tail feathers in birds are thought to have evolved primarily for flight, but in some species have been exapted for sexual display. Within evolutionary theory there is considerable controversy over the utility of such a distinction (e.g., Buss, Haselton, Shackleford, Bleske, & Wakefield, 1998; Gould, 1991; Reeve & Sherman, 1993). After all, if life began, say, with single-celled organisms, then all adaptations are ultimately exaptations. Within a selectionist psychology, however, the term possesses more utility (see Delius & Sieman, 1998 for an application to the psychological phenomenon of transitive inference). The seemingly sudden emergence of any given behavior need not be explained by reference to a specific, directed reinforcement history. Rather, the novel behavior in question might consist of a cooption of pre-existing elements within the behavioral repertoire of the organism. Indeed, the distinction between exaptations and adaptations mirrors quite closely the distinction between rule-governed behavior and contingency-shaped behavior within a selectionist psychology.

Just as many critics of a selectionist psychology make the mistake that every overt behavior requires a unique reinforcement history, much talk of “filters” and “inner environments” originates from the mistaken assumption that ontogenetic selection operates solely on overt behavior. This will be considered in more detail in a later section of this paper. However, suffice it to say, ontogenetic adaptations are only made evident under the appropriate context, and hence, the sudden appearance of a behavior does not rule out a selectionist origin. For example,
pigeons do not show any overt changes in behavior to a tone that has been paired with food. However, if the tone is next presented without food, and is preceded by a localized light, pigeons will approach and peck the light. On one level, this observed pecking is an instance of novel behavior. The birds never showed pecking behavior in the presence of the tone, and the light is never followed by food. But to focus on the behavior’s novelty distracts from the obvious explanation, which is that the birds had indeed learned that the tone preceded food, despite no behavioral correlate. This learning, then, becomes evident when the tone is preceded by a light, thus evoking a classically conditioned approach and peck response to the light.

Of course, unlike phylogenetic “just-so stories,” one can, to some degree, experimentally instantiate hypothetical reinforcement histories by providing animals with reinforcement histories, which have an impact on their ability to produce solutions to various problem-solving tasks. So, for example, pigeons explicitly trained to peck a plastic banana, push boxes, and step up on boxes “solved” an analogy of Köhler’s insight problem in which chimps were placed in an enclosure that contained a box and a banana, suspended out of reach (Epstein, Kirshnit, Lanza, & Rubin, 1984). Pigeons trained to push blocks with their heads will solve obstacle problems in which access to a key is impeded by a block (Nakajima & Sato 1993), and Rhesus monkeys will solve stick-reaching problems after specific training regimens (Shurcliff et al., 1971). The purpose of such experiments is not to claim that a given specific set of reinforcement histories is the cause of a given novel, adaptive behavior observed outside the laboratory. Rather, the value of such experiments, as with just-so stories in evolutionary biology, is to illuminate mechanistic, historical processes, which might or might not act to create an adaptation.

It is at the level of historicity, then, that the analogy between a selectionist psychology and Darwinian selection appears most robust (e.g., Hull et al., 2001; Skinner, 1981). For instance, one might easily imagine a selectionist psychologist writing:

So what would otherwise be a vast dollop of luck is smeared out into acceptably probable portions. And natural selection not only seizes on each of these chance advantages but also preserves them cumulatively, conserving them one after another throughout a vast series, until they gradually build up into the intricacy and diversity of adaptation that can move us to awed admiration. Natural selection’s power, then, lies in randomly generated diversity that is pulled into line and shaped over vast periods of time by a selective force that is both opportunistic and conserving. (Cronin, 1991, p. 8)

Of course, Cronin is writing about natural selection, but surely the same logic applies to a selectionist psychology.

However, if this is so, why do many psychologists and philosophers argue that the traditional selectionist psychology advocated by Pavlov, Thorndike, Hull, and Skinner (i.e., “Behaviorism”—a poorly defined “ism” if ever there was one) is profoundly a-historical? For example, Plotkin (1997, pp. 27-30) suggests that
behaviorism was and still is concerned solely with what he terms, “the framework of now-and-forever causation.” For Plotkin, a traditional selectionist psychology is more accurately described by the fixed, static laws of Physics and Chemistry than by the historical dependency of Darwinian selection. Dennett (1983), too, argues that the ontogenetic “story-telling” of Skinner, with its emphasis on hypothetical reinforcement histories, is in fact qualitatively different from the adaptionist “just-so” stories found in evolutionary psychology. Where the latter offers a strong coupling of optimality and design analyses with concepts of fitness, the former, according to Dennett, answers the question of, “What has happened?” with the response, “Something or other” (Dennett, 1983, p. 387).

The tired and worn riposte to Plotkin and Dennett, is, of course, to claim that their opinions, that is to say their “behavior,” is merely a result of individual reinforcement histories. Perhaps in the current scientific Zeitgeist they are reinforced by making such outrageous claims. Perhaps their verbal behavior is under the control of a verbal community that is blind to the analogy between contingencies of reinforcement and Darwinian selection. But a selectionist psychology gains nothing through such a subterfuge. There is a legitimacy to Plotkin’s and Dennett’s criticisms which needs to be addressed. Ontogenetic and phylogenetic selectionism are not identical, and there is, indeed, a profound difference between explanations based upon ontogenetic and phylogenetic historicity. This difference becomes evident when one turns to the concept of adaptation.

**Ontogenetic Adaptations**

Evolutionary biologists and philosophers have expended considerable effort in methods for distinguishing adaptive features from nonadaptive features (Brandon, 1990; Dennett, 1995; Lewontin, 1978; Thornhill, 1990; Williams, 1992). And for good reason. Phylogenetic adaptations—changes in the beaks of Galapagos finches, a peacock’s tail, bipedalism—provide the primary information about how evolutionary processes, and hence natural selection, work. However, unlike evolutionary biologists, psychologists have not, in general, concerned themselves with the problem of whether a given behavioral change is an ontogenetic adaptation or not. This absence of conceptual rigor, I believe, is a clue that will lead us to the heart of the selectionist muddle.

First, the very meaning of an “ontogenetic adaptation” is unclear. For example, according to Skinner (1966) phylogenetic adaptations are to natural selection as ontogenetic adaptations are to operant conditioning. He writes: “Successful responses are selected in both cases [i.e., phylogenetic and ontogenetic], and the result is adaptation” (p. 1212). But “successful” is, of course, the crucial adjective. Biologists have long recognized that phylogenetic adaptations cannot merely be defined by “fitness,” that is to say, reproductive success. Such a stance leads to the circular relativism inherent in the phrase “survival of the fittest.” If asked why the beak of a finch is a particular shape, answers such as “because that is the shape that survived” or a detailed list of causal pathways (e.g.,
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a reinforcement history) extending backward in time, although perhaps true, are ultimately vacuous. Such answers do nothing to highlight the specific interaction between an organism’s phylogenetic history and its environment; they do not tell us why a particular form survived, they merely specify that it did. However, for the most part these are the sorts of answers blithely given by radical behaviorists in the guise of a selectionist psychology. Why, for example do artists paint pictures?

Artists paint pictures because of the consequences, and people look at pictures because of the consequences . . . if we can discover the reinforcers which are contingent upon the artist’s behavior when he paints a picture, and upon the behavior of others when they look at a picture, we can not only explain their behavior but also use our knowledge to give art a more important role in our culture. (Skinner, 1999, pp. 380-381)

Dennett is quite right to characterize such answers as being equivalent to the exchange “What happened? Oh, something or another” (see Dennett, 1983, p. 387).

The problem may be restated as follows. If all behavior is the result of selection by consequences (acting on both the phylogenetic and ontogenetic scale), and if selection by consequences produces adaptations, then all behaviors are adaptations. From this perspective, ontogenetic “adaptations” experience a similar fate as such mentalistic terms as “beliefs” and “desires.” That is, they become superfluous. A “real” account of behavior isn’t to be had through reference to “beliefs” but by detailing the reinforcement history of the organism. Likewise, a “real” account of behavior isn’t to be had through references to “adaptations” but by detailing the reinforcement history of the organism. It is for this reason that Dennett (1983) likens the phylogenetic strategy of optimality analyses (i.e., “the Panglossian paradigm”) with his own ontogenetic strategy of “intentionality theory.” According to Dennett, just as evolutionary biologists find utility in asking, “What has this phenotypic trait evolved for?” so can selectionist psychologists benefit by asking, “What was the intention of that behavior?” He writes

. . . when a supportable rationale for . . . an act is found, it is a mistake—an anachronistic misapplication of positivism—to insist that “the real reason” for the act must be stated in terms that make no allusion to this rationale. A purely causal explanation, of the act, at the microphysical level, say, is not in competition with the rationale-giving explanation. (Dennett, 1983, p. 354)

Whether one agrees with Dennett or not, the questions facing a selectionist psychology remain: does it make sense to speak of ontogenetic adaptations, and are ontogenetic adaptations by necessity “mentalistic” constructs? In what

2 It hasn’t escaped my notice that the issue of ontogenetic adaptations for a selectionist psychology shares many of the same problems as a radical behaviorist epistemology. That is, a prevalent view of science is that it is an enterprise which over time produces a more accurate (i.e., “true”) representation of a given domain (i.e., reality). And yet, from the standpoint of radical behaviorism there cannot be a representation, only a history of contingencies (see Ringen, 1999).
follows I present several possible conceptualizations of what might be meant by “ontogenetic adaptations” primarily by analogy to what is meant by phylogenetic adaptations. However, as we shall see, each of these conceptualizations is problematic.

Randomness

First and foremost as with the case of phylogenetic adaptations, an ontogenetic adaptation must arise nonrandomly. Obviously, if an organism shows immediate changes in behavior as a result of head trauma, then we would not speak of such behavior as an ontogenetic adaptation. Similarly, if it were shown that random fluctuations in neural connections resulted in changes in overt behavior, then we could not speak of these behavioral changes as adaptations. Biologists have long been aware that changes in species attributes can result from essentially random processes. Genetic drift, for example, refers to random changes in the frequencies of alleles within a population. In small populations genetic drift can result in the elimination of a phenotypic traits, not because the traits had a negative effect on fitness, but simply because of the random nature of gene flow. In a selectionist psychology, therefore, it might make sense to utilize the term associative drift in an analogous fashion. Certainly there are circumstances in which neuronal death (e.g., Parkinson’s disease, Alzheimer’s disease, etc.) affects behavior. Presumably, too, there are circumstances in which random fluctuations in synaptic efficacy affect behavior. Therefore, the possibility exists that observed changes in behavior might have an essentially random, nonselective, basis.

The similarity of the term “instinctive drift” to genetic drift and its psychological analog, “associative drift” is unfortunate. Where the latter concerns more-or-less random changes in unit frequencies, instinctive drift refers to a nonrandom change in behavioral frequencies. However, instinctive drift, like associative drift, refers to behavioral changes that we would not term ontogenetic adaptations. Specifically, instinctive drift refers to the phenomenon of a reinforced response being replaced by a nonreinforced, yet phylogenetic, adaptive response. For example, a hungry pig might cease to place a coin in a cup and instead begin to drop the coin and engage in rooting behavior (Breland & Breland, 1961). A more accurate term than instinctive drift would be allometric behavior.

Within evolutionary biology allometric relations refer to nonproportional changes in body structures. For example, as humans grow, all body-parts do not show proportional changes in size. The legs show more rapid growth and the head less rapid growth than the body as a whole (Sinclair, 1969). Selection (artificial or natural) which targets a specific physical trait might, therefore, produce changes in other traits, and conceivably these changes could be proportionally greater than the specifically selected trait. However, as with associative drift, it would be misleading to characterize allometric behavior as an ontogenetic adaptation.
Design Principles

From the perspective of evolutionary biology, an adaptation is a “purposeful phenotypic feature” (Thornhill, 1990) in the sense that it is nonrandom and solves the problem of maximizing the organism’s relative fitness, given a set of environmental variables (the organism’s niche). In the same manner, a selectionist psychologist might say that an ontogenetic adaptation is intelligent or rational or predictive (e.g., Dennett, 1983). These are, after all, merely descriptive words that one uses to characterize behaviors. They are the psychological equivalent of *a priori* design considerations, and they need not specify any particular mechanism or mentalist possession, such as consciousness, on the part of the behaving animal. The movement of a dog across a shuttle box prior to the onset of an electric shock, a bee ceasing to visit a food patch which no longer delivers food, a chimpanzee gazing away from a food item when in the presence of another chimp of a higher rank—we sense that these are ontogenetic adaptations, because they are clearly nonrandom and because they are intuitively “intelligent,” “rational,” and one might even say “purposeful.” In applying these adjectives we are no more placing them within the “mind” of the organism than an evolutionary biologist places “optimality” into a species. And just as a biologist doesn’t test optimality, per se, a psychologist doesn’t test intelligence. Rather, one uses it as a means to understand the ontogenetic and phylogenetic history of an individual organism.

Of course, the strategy of resorting to design considerations in order to distinguish adaptations from nonadaptive behavioral changes is far from sufficient. What if a behavior appears less than intelligent? Does this mean that it is not the result of learning—that it is not an adaptation? Köhler (1925), for example, described the following error made by a chimp named Sultan. After recording the success of the chimp with several box-stacking problems Köhler moved the suspended food to a new position in the enclosure. On the first trial with the food in a new location Sultan did stack the boxes but under the former food position. Here we have an example of adaptive behavior, box stacking in order to obtain a food item, which appears to be less than intelligent. Evolutionary biology, of course, faces a similar dilemma. Given the lag between environmental change and phenotypic evolution, one should encounter features that do not appear to be adaptive in the current environment but which nonetheless are adaptations. Consider a dog turning in circles on the living room carpet before settling down to sleep. Further, under some circumstances a “dumb” behavior, such as that exhibited by Sultan, might be very powerful evidence for an ontogenetic, selectionist process just as the poor design of, say, the mammalian eye and the human sperm duct provide evidence that they are adaptations.

Conversely, the intelligence or rationality exhibited by a particular behavior within a particular environment does not guarantee that learning or ontogenetic selection, per se, has taken place either. The observed fit might be more aptly described as a consequence of maturation, that is, phylogenetic selection, rather than learning. For example, just because a particular schedule of reinforcement produces a highly predictable pattern of responding, it does not follow that this
pattern of responding is an ontogenetic adaptation (i.e., the result of learning). For instance, in rats there is a positive correlation between the number of responses required to gain access to food (i.e., “procurement costs”) and the meal duration once food has been obtained. Conversely, there is a negative relation between procurement costs and meal frequencies (Collier, Hirsch, & Hamlin, 1972). Further, these are steady-state results, which might lead one to conclude that they should be termed an ontogenetic adaptation. However, Staddon and Zanutto (1998) have shown that a relatively simple, motivational model can account for Collier et al.’s data and a range of other eating phenomena.

Emergent regularities in behavior that are a function of regularities in outcomes, therefore, are not sufficient to define an ontogenetic adaptation. As another example, as noted earlier chimps require experience handling sticks in order to solve insight problems which involve using a stick in order to extend reach (Jackson, 1942). Given an equal amount of time with sticks, however, young chimpanzees still fail such problems more often than older chimps. Schiller (1952) hypothesized that this difference was due to the manner in which young and old chimpanzees normally utilize sticks. Young chimps in his study tended to chew, scratch, and rest on sticks while older chimps tended to use sticks in order to poke or batter other objects. That is, we cannot forget that the emergence of specific behaviors might be more accurately described as a phylogenetic adaptation rather than as an ontogenetic adaptation.

**Blank-Slate Reasoning**

One could argue, though, that the age-differences observed by Schiller depended upon previous, specific ontogenetic selection. For example, research on song learning in birds has shown that the critical periods during which song learning is possible can be lengthened or shortened depending upon the quality of the song stimuli, social stimuli, and so forth (e.g., Baptista & Petrinovich, 1984, 1986). Feeding behavior, mate-partner preferences, care for young—it is well established that learning processes play an important role in establishing each of these “behaviors.” That is, one can always attempt to “write” a hypothetical, ontogenetic adaptationist story in place of a maturational, phylogenetic story. Skinner’s causal mode of “selection by consequences” (e.g., Skinner, 1981) is an extreme version of such reasoning. “Selection by consequences” essentially denies any difference between phylogenetic processes, maturational processes, and learning processes. All three involve the alteration of an “operand” by its consequences.

But isn’t this behaviorism at its worst: the confounding of ontogeny and phylogeny, which is simply another form of blank-slate reasoning? One answer to such an accusation is that much can be learned by pushing a particular view until it breaks down. By asking how much of an organism’s behavior can be explained in principle through purely ontogenetic selectionist processes, one avoids theoretical complacency. Clark Hull, for instance, was quite adept at using stimulus-response principles to account for insight learning (1935) and stimulus equivalence (1939),
and there can be no doubt that these successes caused the more “cognitive” psychologists of the time to refine their experiments and terms. However, if we assert that there is no qualitative differences between phylogenetic and ontogenetic selection, that all explanations of behavior boil down to “selection by consequences,” then we are left with the conclusion that all behaviors are adaptations, and therefore there is no such thing as an ontogenetic adaptation (nor a phylogenetic adaptation, for that matter).

Imagine the following. A cliff stands at the top of a sandy slope. The slope is concave, such that the grade of the slope is less steep at the top than at the bottom. Occasionally a rock dislodges from the cliff and comes to rest on the slope. When this happens we take a stopper full of water and squirt it at the sand in front of the rock and note that the rock tumbles some distance down the slope. In addition, we note that the more we squirt water in front of the rock, the more probable its tumbling behavior becomes. On the other hand, if instead of using water we use tar, we will then note that the tumbling behavior of the rock becomes less probable. The question facing a selectionist psychology is whether the tumbling behavior of a rock should be considered an ontogenetic adaptation. Certainly the shape of the rock will affect its ability to tumble, but this I would suggest is what is meant by adaptation in the phylogenetic sense.

**Units of Ontogenetic Selection**

Perhaps, though, the real issue is one of units. Perhaps an ontogenetic adaptation is not to be found in behavior itself but in the inferred units and mechanisms that produce behavior. How can this be? After all, the usual analogy between natural selection and ontogenetic selection begins with the law-of-effect and its notions of overt behavioral variability and environmental selection. The traditional analogy between learning and natural selection, though, is wrong. Behaviors are not analogous to genes, and the ontogenetic environment does not select behavior. Rather, if a selectionist psychology, like evolutionary biology, distinguishes between two selective domains: the material and the codical (Williams, 1992), then the obvious unit of selection is the “association,” and ontogenetic selection becomes not so much selective as constructive.

**Codical and Material Domains**

Williams (1992) distinguishes between two domains upon which natural selection operates: the material and the codical or informational (see Table 1). He writes, “it is the information [evolutionary biologists] are concerned with, and the DNA is the medium, not the message. A gene is not a DNA molecule; it is the transcribable information coded by the molecule” (Williams, 1992, p 11). That is, the ultimate units of natural selection exist in the codical domain, because it is information—genes, genotypes, gene pools—which persist over evolutionary time, not the physical instantiations of these units. This is, of course, a reformulation of Heraclitus’ “you can never step in the same river twice.” Rivers do not persist, but
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**Codex (Informational) Domain**

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Table 1. An illustration of how ontogenetic and phylogenetic units might be placed within codical and material domains. Units increase in complexity from top to bottom. The left-hand column is adapted from Williams (1992). Note that depending on one’s emphasis, the operant can be considered an informational or a physical unit.

the rules that define rivers do. Similarly, an individual organism does not persist from one moment to the next. Cells are replaced, blood is pumped, neurons fire. However, the pattern of phylogenetic information contained within that individual—it’s genotype—does persist from one moment to the next. From this perspective a gene is a rule, which relates an environmental input to a phenotypic output. Genes are not bits of DNA. A gene is an interaction or relation between

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bits of DNA (or proteins) and a cellular environment. Ultimately, natural selection concerns the changes in frequency of units in the codical domain, input-output relations, even though on a moment-to-moment basis it is the material unit, for example, an individual, which is impacted by the environment.

Is a behavior, then, analogous to a gene? No. As just noted, a gene is a codical unit. A behavior, however, is a material unit. A gene is characterized by dimensions of “bits, redundancy, fidelity, and meaning” (Williams, 1992, p. 10), whereas behaviors are characterized by dimensions of force, duration, momentum, frequency, and so forth. This argument applies whether the behavior in question is “emitted” or “respondent.” Operant behavior, for example, is an element of an ontogenetic phenotype and is analogous not to genes but to the soma. However, what the psychologist infers from the study of operant behavior is a relationship, or rule between a behavior and an outcome. That is, operant psychologists study not so much behavior (units in the material domain) as the relations between behaviors and outcomes (i.e., units in the codical domain). When framed in this manner, as an informational unit, though, the operant clearly becomes merely a type of relation, that existing between a behavioral class and an outcome, or if framed in terms of a three-term contingency, that existing between an occasion setter, a response, and an outcome. Similarly, Pavlovian conditioning emphasizes another type of relation, that existing between stimuli and outcomes. In other words, the fundamental units of a selectionist psychology are not behaviors but associations.

The Gene-Association Analogy

The notion that associations, not behaviors, are the fundamental units of a selectionist psychology—a stance that I will term associationism—is not a radical proposition, but its centrality to a selectionist psychology requires clarification. Donahoe and Palmer (1994), for example, make a cogent argument that ontogenetic selection never affects behavior alone but also changes the environmental control of behavior. More specifically, they suggest that reinforcers operate on what they term “environmental-behavior relations.” Their insistence on environmental-behavior relations stems from attempts to produce dynamic, molecular accounts of behavioral change, which in turn force one to consider the effects of a single reinforcing event.

If an organism is continuously immersed in an environment and is continuously behaving in that environment, then both stimulus and response events necessarily precede and, hence, are potentially affected by the occurrence of a reinforcer regardless of the contingency according to which the reinforcer occurs. (Donahoe, Palmer, & Burgos, 1997, p. 199)

In what follows I will emphasize just three points. First, associationism does not imply essentialism (e.g., a conceptual nervous system, Skinner, 1950). Second, associationism permits a more accurate correspondence between ontogenetic and phylogenetic selection. Third, I will argue that associationism permits a more inclusive selectionist psychology.
The claim that associations are the primary unit of ontogenetic selection does not require that one embrace mentalism or essentialism (for an explanation of the latter term, see Palmer & Donahoe, 1992). For example, consider the hypothesis that operant conditioning involves the formation/selection of response-outcome (R-O) associations (Colwill & Rescorla, 1985, 1990; Rescorla, 1993b, 1995). A typical experimental result used to support this claim involves the training of two responses, lever pressing and chain pulling, say, via two outcomes, for example, sucrose and food. After establishing both responses one of the outcomes is then “revalued” by giving subjects free access to either of the two outcomes followed by an injection of LiCl. Finally, when the subjects are presented with both response manipulanda, they show a strong preference for the response that was not previously reinforced by the devalued outcome. What, though, is meant when one makes the claim that such a revaluation procedure exposes a R-O association? Donahoe and Burgos, for example, state that

Revaluation encompasses an important set of phenomena, but associations cannot provide a basis for interpreting them in behavior analysis. Associations are inferred processes and, as such, cannot be manipulated or measured. That is, they are not subject to direct experimental analysis. Associationism makes use of a conceptual approach that Skinner labeled a conceptual nervous system and, as such, is particularly susceptible to the twin temptations of the nominal fallacy and logical circularity. (2000, p. 332)

Donahoe and Burgos are correct that associations are inferred units, just like genes are inferred units. However, they are incorrect when they claim that an association is a process. Just as a gene is not a process, an association too is not a process. Similarly, an inferred unit can indeed be manipulated. Associations may be eliminated, strengthened, or weakened just as genes may be multiplied, decreased, or eliminated within a population. None of these statements imply a conceptual nervous system, although of course they imply mechanisms of some sort or another. In fact, Donahoe and Burgos (2000) propose just such an ingenious (and inferred) mechanism that would produce the R-O association revealed by revaluation experiments.

The confusion surrounding essentialism and associations has also hindered the appreciation of one aspect of the analogy between genes and associations. Namely, just as the fundamental nature of the gene is given by its persistence while material units such as individuals come and go, so do associations persist while responses come and go. Again, the argument that associations persist while behaviors do not is not an essentialist argument, nor is it a restatement of the learning-performance distinction. Consider the phenomenon of extinction. In operant psychology, extinction refers to the decrease in frequency of a response due to a cessation of contingent reinforcement. However, some evidence suggests that the association between the stimulus, response, and outcome is not eliminated but is rather overlaid with an inhibitory association (Bouton, 1991; Rescorla 1993a, 1993b, 1995). Others (e.g., Dragoi & Staddon, 1999) have interpreted extinction as involving a comparison between R-O associations, which accumulate and dissipate
at different time rates. Whether one accepts such descriptions of extinction, the deeper point is that any description of extinction specifically and ontogenetic selection in general absolutely requires the retention of some unit across time. Further, given that this unit does not depend on the presence of individual behaviors, we must assert that what persists in a selectionist psychology is a codical unit, (e.g., the association or an “internal state”). Finally, given that codical units, such as associations, must be instantiated somewhere, one can only conclude that a selectionist psychology does, in fact, require a distinction between learning and performance. As Staddon writes

The phenotype-genotype distinction is uncomfortably similar to the neo-behaviorist distinction between performance and learning, which Skinner had always rejected. . . . Only some things that animals do can be selected through operant reinforcement. Distinguishing those that can from those that cannot requires an understanding of the relations between the internal processes that generate overt actions and the actions themselves. . . . It also requires an understanding of the differences between those internal states that have successors across time, i.e., memories, [his italics] and those internal states that leave no imprint on the future. (Staddon, 2001, pp. 74-75)

Staddon’s point, and the point being made here, is that if one insists on defining a selectionist psychology purely in terms of material units, one is left with an extremely restricted conceptual base which actually has very little in common with phylogenetic selectionism.

By recognizing the association as the primary unit of ontogenetic selection one not only creates a more accurate analogy between phylogenetic and ontogenetic selection, one also forms a more inclusive selectionist psychology—a psychology which is open to neuroscience and which subsumes operant and classical conditioning under a common explanatory framework. Despite some perhaps unintended suggestions to the contrary (e.g., Hull, Langman, & Glenn, 2001), a selectionist psychology is not restricted to operant conditioning. It is unclear, for instance, how the principles of overt behavioral variation and environmental selection aid a researcher who investigates classical conditioning phenomena. Perhaps, if classical conditioning simply consisted of the transfer of a response from the unconditioned stimulus to a neutral stimulus, we might speak of behavior as a selected unit in such procedures. Stimulus-response theorists did indeed rather cleverly think in terms of drive stimuli (e.g., hunger), which selected the responses elicited by the US (e.g., the S0 of Hull). However, as is now known, Pavlovian behavior is as much a function of the CS as the US (Holland, 1984). Nonetheless, all of the phenomena which embody both Classical and Operant conditioning may be described in terms of inferred relations/associations among

3 An additional point, which relates to the essentialism some psychologists condemn in the performance-learning distinction, concerns what is often referred to as “constraints on learning.” Constraints, of course, imply an ideal. However, when considered in terms of associations, constraints are better termed “barriers” in the same sense that “barriers” exist between species at the phylogenetic level. Associationism, then, actually “saves” constraints on learning from essentialism.
stimuli, outcomes, and responses. By way of analogy consider that although a geneticist uses slightly different procedures to select eye color, say, and wing shape in fruit flies, the different contingencies embodied in these procedures do not imply fundamentally different selectionist processes. In both cases the researcher merely refers to selection acting among a set of common codical units, that is, genes. Similarly, the differences between classical and operant conditioning procedures exist primarily in the methods applied by the experimenter. Codical units, ironically, force one to move beyond procedural differences and focus instead on selection processes operating on a moment-to-moment basis on the experimental subject. When framed in terms of associations and the selection processes that modify associations, classical and operant conditioning become in a sense indistinguishable (see Donahoe et al., 1997). Further, this consistency of units permits one to speak in terms of a universal principle of selection, which applies to both types of conditioning (Donahoe, Burgos, & Palmer, 1993).

Regarding neuroscience, all psychologists should hope to maintain a scientific unit, which they can eventually translate into neurological principles. The concept of an association dovetails quite nicely with the opinion that neural computation refers to a changing pattern of connections among a group of neurons. Further, an association at the psychological level might very well arise from selective and/or constructive processes at the neural level, although the concept is not dependent on either of these lower-level processes. This neutrality is, itself, beneficial. For example, Purves (e.g., Purves, White, & Riddle, 1996) has argued that development and learning at the neural level consist primarily of an active interaction between neuronal growth (e.g., dendritic growth, synaptic development) and environmental inputs. This constructivist theory of neural development emphasizes the notion that a more or less structured environment guides neuronal growth. Others have argued, equally forcefully, that learning at the neural level consists primarily of overlapping waves of excess growth followed by selective “pruning” (e.g., Changeux & Dehaene, 1989; Edelman, 1987). The debate between neuroscientists who favor a hypothesis of neuronal constructivism and those who favor a selectionist hypothesis is on-going (see Quartz & Sejnowski, 1997). However, the codical units of a selectionist psychology are not dependent upon either neural selectionism or neural constructivism. Associations, for example, may be instantiated through either, both, or neither of these processes.

Further Implications of the Gene-Association Analogy

If one accepts the distinction between material and codical units, and the resultant analogy between genes and associations, then several further analogies between a selectionist psychology and natural selection become possible. Table 1 illustrates one way in which phylogenetic and ontogenetic units may be divided among material and codical domains, beginning with the gene/association and moving upward to higher “levels of selection.” For the sake of brevity I will consider here only two cells from the table: a psychological equivalent to fitness—preparedness, and a psychological equivalent to the allele—what I term the
“associant.” Finally, I will briefly remind the reader that although higher-order units, such as individuals, species, and molar behavioral patterns may be constructed from genes and associations, the question of whether these units contain group-level properties which are exposed to selection remains controversial.

Fitness and Preparedness

Darwinian fitness refers to the relative change in frequency of a codical unit (e.g., a genotype at a single locus), where that change can be shown to be the result of selection. A selectionist psychology speaks more of changes in relative associative strength, of which changes in response frequency are one measure. Is there a psychological term that describes relative associative strength? The notion of an operant contains the idea of nonrandom, selected changes in unit frequency, but operants are taxonomic units that refer to a specific type of association. No, the closest a selectionist psychology comes to the concept of fitness is in the concept of preparedness (related to Thorndike’s “readiness,” see Malone, 1991). Preparedness describes the finding that some stimuli/responses appear to be optimized for specific types of associations. (Delius & Emmerton, 1978; Garcia & Koelling, 1966; Shettleworth, 1975). Delius and Emmerton, for example, found that pigeons fail to learn visual discriminations when stimuli are followed by differential shock outcomes but learn the same discriminations quite quickly when differential food outcomes are used. Conversely, shock is successful in training pigeons in auditory discriminations while differential food outcomes fail to yield this performance. One might therefore argue that in the “shock environment,” auditory stimuli are more fit than visual stimuli. Further, the concept of preparedness is implicitly contained within the Rescorla-Wagner model (Rescorla & Wagner, 1972) and its many derivatives. The $\alpha$-parameter of the Rescorla-Wagner model is essentially a fitness parameter.

Alleles and Associants

A second analogy that may be drawn between evolutionary theory and a selectionist psychology is that existing between the allele and what I term the associant. An allele is the term evolutionary biologists use for a type of particulate variability. Specifically, the term refers to one of several forms of a given gene. As such it is an informational unit, but one which in the physical domain of DNA can be segregated into a discrete Mendelian factor. Eye color within a human population or within a population of fruit flies, for example, is a phenotypic trait whose variability corresponds to multiple alleles in the gene pool. Natural selection, then, involves changes in the frequency of these alleles. Dawkins

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1 It is impossible to define fitness in a single sentence. As Dawkins (1982, pp. 179-194) has pointed out, there are at least five definitions of fitness in the evolutionary literature. The definition I am using here is his “Fit the Second,” which is commonly used by population geneticists.
(Dawkins, 1989, p. 38) provides a nice analogy between alleles and the potential rowers who can occupy a given seat (i.e., locus) of an 8-person scull (i.e., genotype). Processes such as crossing over and sexual selection serve to select, say, an individual (an allele) from the set of all first-seat rowers.

Within a traditional selectionist psychology the temptation would be to equate alleles with operants. After all, just as an 8-person scull, in an attempt to cross the finish line, can use Tom, Dick, or Harry in the first-seat position, a rat may use its left paw, right paw, or head to press a lever in order to operate a feeder. Further, as with alleles, operants are primarily defined through their effects—the manner in which they operate upon the environment. Behaviors which alter the environment in a similar manner, as with the just mentioned rat with a lever to press, might be thought of as competing with one another for representation within the eventual, steady-state behavior of the animal.

However, the concept of an operant, as stressed earlier, is overly specific as it is primarily concerned with associations between a response and an outcome. A more general analog for the allele, of which the operant is but a subset, emerges from the just-discussed concept of preparedness. That is, in a selectionist psychology the equivalent of alleles are the members of the subset of all possible behaviors (the behavioral repertoire) and all possible stimuli (the perceptual repertoire), which can in principle enter into an association with a given outcome. The term that I will suggest for the members of this set is associants.

Associants are first and foremost defined by the principle of preparedness outlined above. As such they are inferred units that, like the allele, exist in the informational domain. Associants are the set of all stimuli and responses with a preparedness greater than zero. Shettleworth (1975) found, for example, that in golden hamsters digging behavior would increase in frequency following food reinforcement. Scratching behavior, though, did not increase in frequency following food reinforcement. We might say that digging is operant behavior amenable to food reinforcement, but more generally we could also state that in golden hamsters digging is an associant of food reinforcement. Similarly, in pigeons we might say that acoustic stimuli are associants of shock reinforcement (Delius & Emmerton, 1978) and that in rats tastes and odors are associants of nausea (Garcia & Koelling, 1966). Note, too, that the term reinforcement should be used in an extremely general sense. In the above examples the reinforcer is merely any stimulus/response whose occurrence is contingent upon the associant. Hence, we might also say that in rats a light is an associant for a tone (e.g., sensory preconditioning, Rizley & Rescorla, 1972; second-order conditioning, Holland, 1985) and that in pigeons peck-rate is an associant for visual stimuli (Cleaveland, 1998; Urcuioli, 1985).

**Higher-Level Units**

The associant, like its phylogenetic counterpart, the allele, is a rather modest higher-order unit. Indeed, alleles and associants are more aptly described as “classes” rather than as units. That is to say, selection does not operate on the class,
but on the members of the class. However, does it therefore follow that all higher-order units beyond the gene and the association are likewise not “exposed” to selection? Suffice it to say, the issue is a controversial one.

Perhaps contrary to dominant perceptions outside the field, evolutionary biologists have not concluded that the gene is the only, or even the primary unit of selection. Williams (1992), for example, suggests that one may speak of a higher-order unit of selection to the extent that such units are sorted at that level by differences in the rate of increase of units at that level. As an example, he imagines two populations of closely related moths whose larvae specialize, respectively, on two food sources: black oak and white oak (1992, p. 28). If, after many generations, a biologist discovers that the black oak specialists still survive, and have given rise to a daughter population specializing on another food source, and if that biologist can also show that specializing on white oak as a food source tends to cause a population to go extinct, then a form of group selection has taken place. That is, the biologist has shown a nonrandom selective change in unit frequencies where the units, in this case, are clades. Others, though, have argued that higher-order phylogenetic units are only real to the extent that the observed changes in the frequency of these units depend upon characteristics unique to that level (e.g., Vrba & Eldredge, 1984). Age distributions and sex ratios, for example, are characteristics inherent to a clade but not an individual. If differences in these characteristics led to frequency differences of clades, then one might speak of the clade as a unit of selection.

A selectionist psychology encounters a similar problem. That is, are units beyond the single association more than a taxonomic convenience? Table 1 provides several instances of higher-order units both in the codical and the material domain. Can these higher-order units be selected? From the perspective of a traditional selectionist psychology, the answer has been largely negative.

Traditionally, all stimulus and response units are viewed as the temporary constructs of lower-level associations. First, the gradual nature of learning curves supports this view. When confronted with the regular, sudden decreases in solution latencies obtained by insight psychologists, Thorndike countered that these results were simply evidence of previous step-by-step learning. The gestalten that insight psychologists freely evoked were viewed by Thorndike as having no intrinsic properties beyond those of the lower-level stimulus-response associations of which they were presumably composed. It would seem that Skinner, on the other hand, in defining the operant, did permit what one might term higher-order units (Skinner, 1935, 1938; Staddon, 1967). Writing a poem could be a response just as much as twitching a finger. All that mattered was whether the arbitrarily defined behavior became more frequent when positively correlated with the arbitrarily defined outcome. This is essentially Williams’ approach to higher-order phylogenetic units, as described above. However, like Thorndike, Skinner held that operants were constructed (shaped/chained) via a bottom-up process (what Donahoe et al., 1997 refer to as Skinner’s preference for moment-to-moment analyses). An operant response, such as writing a paper, which is composed of several lower-level operants can be reinforced to the extent that the “selection pressures” on each
lower-level operant are in agreement. That is, like Thorndike and contrary to the Gestalt psychologists, Skinner held that a higher-level operant exerted no influence or control over its lower level elements.

However, the issue of whether it makes sense to speak of higher-order units of selection in psychology remains as controversial as in evolutionary theory. For example, teleological behaviorism (Rachlin, 1992, 1995), in a sense merges aspects of radical behaviorism with gestalt psychology. From this perspective an ontogenetic unit becomes not a discrete act followed by a single consequence but a pattern of acts and their pattern of outcomes, with these global patterns constraining their constituent parts. As a thought experiment, Rachlin (1995) has us imagine a view into a passing train in which we see a man swinging a hammer. Such an act might be similar to a rat pressing a lever, but is it the unit of selection? Rachlin would answer yes and no. If we expand our view of the behavior, swinging the hammer does have an immediate consequence, perhaps by successfully hitting a nail. But other equally legitimate units might be any one of the following: “joining one piece of wood to another; building floor; building house; providing shelter for his family; supporting his family; being a good husband and father; being a good person” (p. 115).

A similar conceptual problem can be found in the equivalence class literature. Is an equivalence class a unit or a category? For instance, suppose we define two groups of stimuli for a subject, where group membership is given by a specific outcome, Reward 1 or Reward 2. Suppose further, that we next train a unique response, Response 1 or Response 2, to one member from each group. If we found that this response spontaneously transferred to all other members of a given group, then we would have to conclude that each group was a higher-order unit of selection. Experiments with humans have, indeed, shown that responses will transfer among the members of an equivalence class (e.g., Barnes, Brown, Smeets, & Roche, 1995) and that the equivalence relation can incorporate the conditioned blocking phenomenon (Rehfeldt, Dixon, Hayes, & Steele, 1998). The abstract claim that one might draw from such research is that category membership imposes constraints on each member of the category, or, stated differently, higher-level psychological units, once constructed, may “insulate” their lower-level constituents from specific, targeted selection pressures. This is essentially the claim made by Rachlin (1995) when he writes, “In summary, a problem of self-control arises where we find a pattern in behavior, particular components of which are dispreferred relative to alternatives that are inconsistent with that pattern” (p. 116).

**Ontogenetic Adaptations Reconsidered**

The issue of higher-order units of selection returns us to the issue of adaptations raised earlier. At that time I considered two questions: first, in what manner does it make sense to speak of ontogenetic adaptations, and are ontogenetic adaptations by necessity “mentalistic” constructs? For instance, are molar regularities in choice behavior ontogenetic adaptations or merely a class of
lower-level adaptations? It should now be apparent that this question cannot be answered as long as a selectionist psychology assumes that behaviors are the fundamental unit of selection. Ontogenetic adaptation does not refer to behavior but to an inferred relation, which one can term an association. Ontogenetic selection, like phylogenetic selection, does not select material units such as behaviors but selects associations which are codical units. Or stated differently, ontogenetic selection selects for particular associations which result in the selection of particular behavior patterns (see Donahoe et al., 1997, p. 201). Taxes, such as the orientation by certain insects toward or away from sources of light (Staddon, 1983), although certainly an example of “selection by consequences” are not ontogenetic adaptations. Similarly, as Staddon and Zanutto (1998) have shown, a correlation between behavioral patterns and schedules of reinforcement are also not necessarily evidence of ontogenetic adaptations.

Let us reconsider the example of a rat’s behavior in a water maze with which this paper began. An ontogenetic adaptation is not revealed by a decrease in the amount of time it takes subjects to locate the underwater platform. Rather, an ontogenetic adaptation is inferred when we place the rat in a novel starting location, and it swims directly toward the platform, or when we rotate the external environmental cues in the room and observe that the rat swims to a coordinate defined by these cues. Such tests reveal an association between environmental stimuli and the animal’s behavior. It is the formation of such an association that defines an ontogenetic adaptation.

**Conclusion**

This paper has striven to clarify several conceptual issues surrounding a selectionist psychology, notably the concepts of ontogenetic adaptations and ontogenetic units. A selectionist psychology is often assumed to rely on two principles: overt behavioral variability and environmental selection. However, responses are not discrete organisms. They do not reproduce and they do not persist. By insisting on a rather unimaginative interpretation of the law-of-effect a selectionist psychology has overly confined itself to operant conditioning, and even a rather constrained form of operant conditioning, at that.

Throughout the paper the analogy between Darwinian selection and ontogenetic selection has been considered. I make no assertions that the many analogies I’ve drawn between the two domains are correct. However, I would hope that psychology’s “Paleys,” alluded to in the Introduction, will realize that a selectionist psychology contains a richness of which they were unaware. Further, I hope that the comparisons offered here between phylogenetic selectionism and ontogenetic selectionism will inhibit casual statements, which assert that the two are more or less the same. If this were so, one might just as reasonably ask why Darwinian selection is not phrased in terms of associations and reinforcement. The reason is, of course, because selection has a specific meaning, which is appropriate to the evolutionary biology data set. Similarly, principles of reinforcement and associationism are defined by the psychological data set. The two sets of principles
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are conceptually similar, but this should not blind us to their differences. Indeed the very term “selectionist psychology” might be misleading. Ironically, an expression which Dennett (1995, 1996) rather derogatorily applied to selectionist approaches to behavior, namely “ABC learning,” embodies quite nicely the principles advocated here—associationism, behaviorism and connectionism. These principles account for the very criticisms he and others have leveled against a selectionist psychology.

In conclusion, given the complexity of the subject matter and the youth of the science, it is perhaps understandable that Psychology is replete with analogies. The brain is like a switchboard. The brain is like a computer. Memory is like gravity—an event exerting an effect at a temporal distance. Behaviors are like individual animals existing in an environment “red in tooth and claw.” Further, the use of analogies is not necessarily detrimental to a science. The equations underpinning Maxwell’s theory of electromagnetism originated through analogies made to rotating tubes and fluid dynamics. However, attempts to maintain an analogy despite evidence that it no longer holds can only lead to dogmatism. In a letter Clark Hull wrote to Spence, Hull writes in reference to Gestalt psychology,

Of course what they say about our postulates is true in a certain sense. For example, when we take the principles of generalization and association, those principles naturally do not apply to the solar system and to astrophysics and to other cosmic processes. . . . Now of course I at least would have no objection to having postulates that would apply to the solar system if at the same time they really applied to the running of rats in a maze. (Amsel & Rashotte, 1984, p. 58)

If the principles that form a selectionist psychology explain a rat in a maze, then yes, psychologists should embrace its principles. If not, though, new principles are called for.

References


BEYOND TRIAL-AND-ERROR


