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Essentialism and Selectionism

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Contingencies of selection, be they phylogenetic or ontogenetic, merely set boundaries on units; the do not provide blueprints. Thus, variability is fundamental to all units of selection. Skinner, by characterizing the units of analysis in behavior as generic in nature. established his science squarely within the selectionist paradigm. thereby avoiding the tendency. common throughout psychology, to slip into essentialist analyses. The distinction between essentialism and selectionism is refined in this article, and prominent examples of essentialism in associationism and even in behavior analysis are identified.

In this article we import a distinction from evolutionary biology—that between selectionism and essentialism to discuss contrasting trends in cognitive science. Largely because of the prestige of Darwin's theory, essentialism is out of fashion as an explicit doctrine in science. However, one can pay lip service to selectionism and still subscribe to essentialist assumptions, employ essentialist locutions, define essentialist units of analysis, and worse, pursue research guided by these assumptions, units, and locutions. In contrast to most of his contemporaries, B. F. Skinner consistently repudiated essentialism (although he never used the term) both in his science and in his writings. Of particular significance, we argue, was Skinner's early methodological claim that the appropriate units of analysis in a science of behavior are to be defined empirically, rather than a priori (Skinner, 1935, 1938). By putting this claim into practice, Skinner set the stage for a thoroughgoing selectionist science and so avoided the fruitless inquiry engendered by implicit essentialist assumptions. The field of behavior analysis has generally, although not always, hewn to Skinner's precepts and remains psychology's most consistently selectionist enterprise.

Essentialism and Selectionism Contrasted

Darwin's theory of evolution by natural selection is still hotly disputed in some quarters, but there are few in the scientific community that doubt that the theory, at least in its broad outlines, accounts for the extraordinary complexity and diversity of living things. The elegance of the theory lies in the unparalleled simplification that it achieved; countless acts of special creation were replaced by the repeated action, over the eons, of a relatively few, elementary processes. Although selective breeding had been practiced for millennia, the power of selection to explain adaptive complexity in nature was not cogently argued until Darwin unveiled *The Origin of Species* in 1859. Despite its elegance, Darwin's theory languished without widespread acclaim until its synthesis with population genetics in the 1930s. If selection was slow to be recognized for its role in phylogeny, it was slower still to be recognized as a nonteleological explanation for a wide variety of other complex phenomena, including operant conditioning (e.g., Thorndike, 1898), the immune response (Jerne, 1955), problem solving and the acquisition of knowledge (e.g., Campbell, 1960, 1974; Popper, 1972), cultural practices (e.g., Campbell, 1975; Skinner, 1948, 1971, 1981), perception (Campbell, 1956b), neural networks (e.g., Edelman, 1987), and technological innovations (e.g., Basalla, 1988; Root-Bernstein, 1989). Selectionist interpretations have been provided for phenomena as disparate as the locomotion of protozoa (Baldwin, 1895; Campbell, 1956a) and the orderly orbits of planetary bodies (Donahoe, Burgos, & Palmer, 1993).

As in the case of speciation, even when underlying mechanisms are poorly understood, selectionist accounts are appealing because of their simplicity and power.

Selectionism is gradually replacing what the evolutionary biologist Ernst Mayr called *essentialist thinking,* the tendency to view categorical phenomena in nature as reflections of universal, enduring qualities intrinsic to each class or unit (Mayr, 1976, 1982, 1988).[1] In Darwin's day, the dominant view of living things was essentialist in this sense; species and other classes of organisms were seen as collections of individuals that all shared some essential property that defined the group, and taxonomy was largely a matter of identifying these essential properties. Individuals within a group might vary widely, but they were all seen as variants of a single template. Individual variability could be explained as the outcome of less fundamental factors-crossbreeding, environmental stress, accident, or other vicissitudes.

Of course this position leaves unexplained the origin of the templates. It is characteristic of essentialism that phenomena are said to reflect some ideal, some essence, or some template that in itself remains unexplained. The devout presumably attribute the origin of templates to a deity. Certainly, much of the bitterness with which Darwin was attacked arose from the implication that natural phenomena could be explained without reference to a designer. Nevertheless, one does not have to be a deist to believe that species have essential properties; the skeptic merely adds the origin of templates to the list of natural phenomena that we accept as unexplained, perhaps even as unexplainable.

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[1]Mayr (1976, 1982. 1988) borrowed the term *essentialism* from Popper (1957). Who coined it as an unambiguous substitute for the overworked term *realism*. The realists were realists were Scholastic philosophers who, quibbling over the exegesis of Aristotle, held that categories are defined by essential properties that transcend the specific members of the categories. The category of white things, for example, is defined as those elements that possess the property of "whiteness." Whiteness is an essence. a "thing" that we come to know in its own right through our experience with white objects. This position can be traced back in some form to Plato and Parmenides. In contrast, nominalists (chiefly following William of Occam) held that universal terms such as *white* were mere labels that we use for a collection of objects of a common color. They do not represent things with an existence or status independent of the set of white objects. Hobbes (1651/1968) captured the nominalist position in his discussion of the distinction between proper nouns and common nouns. Of the latter he noted, “Every one of which, though but one name, is nevertheless the name of diverse and particular things: in respect of all which together, it is called a *universal,* there being nothing in the world universal but names: for the things named are every one of them singular and universal. One universal name is imposed on many things for their similitude in some quality, or other accident" (p. 102). Locke (1690/1975), in his *Essay on Human Understanding,* foreshadowed much of the discussion in the present article by distinguishing between what he called “real essences" and “nominal essence,” that is, between properties that are intrinsic and those that are conventional. He held that only humanly defined categories such as *circle* and *Whig* could be said to have real essences. Natural categories such as *man* were conventional: he noted that occasionally infants were born so deformed that it was unclear whether they qualified as exemplars of the category *human.* Although circles have defining properties that, in a sense, precede any instance of a circle, natural categories such she*ep, tree, or man* do not have defining qualities, except by convention.

Darwin's (1859/1950) discussion of the term *species* is clearly at odds with the essentialist position. He noted,

> No one definition has satisfied all naturalists; yet every naturalist knows vaguely what he means when he speaks of a species. Generally the term includes the unknown element of a distant act of creation.. . . It is certain that many forms, considered by highly-competent judges to be varieties, resemble species so completely in character, that they have been thus ranked by other highly competent judges. But to discuss whether they ought to be called species or varieties, before any definition of these terms has been generally accepted, is vainly to beat the air.. . . It will be seen that I look at the term species as one arbitrarily given, for the sake of convenience, to a set of individuals closely re resembling each other, and that it does not essentially differ from the term variety, which is given to less distinct and more fluctuating forms. The term variety, again, in comparison with mere individual differences, is also applied arbitrarily, for "convenience" sake. (pp. 24-29)

Of course it was not Darwin's linguistic or philosophical predilections that aroused his contemporaries, but his exposition of selection as a non-teleological explanation of the diversity of life. Evolution requires only that there be heritable variation among individuals and contingencies of selection that operate over time. Selection is a process that necessarily takes time; hence if contingencies of selection are stable over the duration of one's observations, species may appear to have essential properties. But, Darwin showed that this appearance of stability is quite consistent with a selectionist account. When viewed over time, it is clear that the individual is a unique constellation of properties that can only be understood, not by considering one's group membership, but by considering, in detail, the environment-organism interactions of one's ancestors.

However, it means something to say, for example, that a fox is a fox and not a mouse. Presumably all foxes share a common ancestor, and as they breed with one another and not with dissimilar species, they have a strong family resemblance. In addition, all foxes are subject to similar contingencies of selection. Do not the contingencies of natural selection specify the essential properties of species? To argue so is to overlook a crucial feature of evolutionary phenomena. Foxes will vary from one another within bounds determined by the selection contingencies. Any variant that satisfies the contingencies can make a contribution to future generations of foxes. Moreover, although we speak of contingencies of natural selection in general terms, the contingencies are as individual as the organisms themselves. For example, we might observe that a changing climate favored foxes with heavier coats or that brilliant plumage in a species of bird was more effective in attracting mates. However, survival is a matter not of global contingencies but of the moment-to-moment contingencies of an individual's life. A light-coated fox might thrive in a region where food was abundant, and a modestly plumed bird might find a mate if competition were scarce. Although, on the average, it may be true that the race is to the swift and the battle to the strong, the average organism is an abstraction; only individuals exist, and time and chance happeneth to them all.

The role of genes—unknown to Darwin—does not affect our conclusion. Genes appear static, to be sure, but constellations of genes are variable within a species, even from parent to offspring, and they are continually subject to mutation. Indeed, without this variability, evolution would be impossible. Selection does not produce organisms stamped out of a common mold. To the contrary, a bizarre mutation that eliminated variability in a population would surely prove fatal when prevailing contingencies changed.

Thus, contingencies of selection do not yield rigid, static, or idealized species, nor do they select rigid, static, or idealized propert1es of species. The selected property, be it a morphological feature or a behavior, can vary in any arbitrary characteristic that is incidental to the contingency, but, more fundamentally, it can even vary along the dimensions that are defined by the contingency. A selection contingency merely sets minimum standards for a property; it does not provide a blueprint. Variation within the boundaries of the selection contingencies will be constrained only by those mechanisms that generate variability in the property. The critical difference between essentialism and selectionism, then, is that selectionism regards variability within classes of phenomena as fundamental, whereas essentialism regards it as a misleading irrelevance.

Selection and Behavior

The analogy between natural selection and learning has struck several observers, apparently independently (e.g., Baldwin, 1895, 1909/1980; Campbell, 1956a; Pringle, 1951; Skinner, 1953; Staddon, 1983). Thorndike (1898) provided the first systematic analysis of reinforcement: A cat in a puzzle box emits a wide variety of behaviors; some variants affect the apparatus in such a way that the door falls open, allowing the cat to escape; on later trials these successful variants. are more likely to occur. As in evolution, variable elements have different consequences; those with certain kinds of consequences are strengthened relative to the unsuccessful elements. Order emerges without appeal to a designer or to intentionality. Skinner (1938) noted that highly organized, complex behavior could be shaped from relatively undifferentiated baseline behavior by successive contingencies of reinforcement in which the selection criterion was gradually altered to more closely approximate some target behavior, just as gradually changing climate, habitat, competing species, and so on, presumably shaped highly organized, complex structures or innate behaviors in organisms (Skinner, 1966b). Thus, both evolution and reinforcement operate "by repeatedly selecting elements from a variable substrate" to produce orderly classes of phenomena. In behavior, as in morphology, variability is required if selection is to create new forms.

The behavioral repertoire of an organism effects both phylogenetic contingencies and contingencies of reinforcement. Consequently, variability is a fundamental characteristic of behavior, even under the most-restricted circumstances. Abstraction, idealization, categorization, and averaging cannot eliminate this variability; it can only mask it. We err in our science if we treat the variability of our subject matter as an annoying irrelevance that can be eradicated by such practices.

Implications for Units of Analysis in Cognitive Science

In the behavioral sciences no one has been a more thoroughgoing selectionist than Skinner, for he has interpreted all behavior—from lever presses to perception, verbal behavior, and thinking—in terms of principles of selection. But Skinner's place in the pantheon of selectionists does not rest primarily on his verbal interpretations, important though they are in showing the scope of these principles. Rather, his main contribution was the methodology that arose from his prescient grasp of the nature of his subject matter. Skinner recognized that variability was fundamental to behavior and fashioned his methodology accordingly. In particular, he realized that the appropriate units of analysis in a science of behavior are generic in nature. In this regard, so far as we know, he was unique.

Skinner's Empirical Units of Analysis

As Skinner (1935, 1938) observed, if we want our units of analysis to respect lines of fracture in nature, we must define them empirically. We cannot define them from our armchairs; rather, we must survey the variability of our subject matter and adopt working definitions according to the order we find. Skinner was not making a philosophical claim; he observed that the order that emerged in his investigations rested upon adopting empirically determined, generic units of analysis.

According to Skinner, the search for orderly units of behavior and environment begins arbitrarily. The experimenter has a hunch, follows a precedent, or picks defining properties of his units at random. If the dynamic properties of behavior are not orderly when the units are so defined, the experimenter systematically restricts or modifies the definitions until orderly relationships between variables emerge. For example, in the study of the flexion reflex in a spinal preparation, Skinner (1938) wrote,

> If we are measuring fatigue, for example, we shall not obtain too smooth a curve if our stimulus varies in such a way as to produce at one time one direction of flexion and at another time another; but as we restrict the stimulus to obtain a less variable response, the smoothness of the curve increases. (p. 36)

The process of modifying these definitions can continue to the point at which both environmental and behavioral units are completely restricted. Here we choose to count only those stimulus and response events that meet very narrow definitions. For example, we might define the response in terms of specific effectors, precise location, force, latency, and so on. However, Skinner noted that nothing is gained by continuing to restrict these definitions past a certain point:

> The generic nature of the concepts of stimulus and response is demonstrated by the fact that complete induction obtains (and the dynamic changes therefore reach an optimal uniformity) *before* all the properties of stimulus and response have been fully specified in the description and respected in each elicitation. (p. 37)

Extending the analysis to operant behavior, Skinner noted that, in the process of restricting our definitions, an inflection point is reached at which our data are most orderly. Continued restriction actually leads to a deterioration in the orderliness of the data. Consider bar pressing in the rat: Before we can see precisely what a given act consists of, we must examine the changes it undergoes in strength. Here again we merely specify what is to be counted as a response and refuse to accept instances not coming up to the specification. A specification is successful if the entity which it describes gives smooth curves for the dynamic laws.

> . . . The number of distinguishable responses on the part of the rat that will give the required movement of the lever is indefinite and very large. They constitute a class which is sufficiently well defined by the phrase 'pressing the lever.' . . . The members of the class are quantitatively mutually replaceable in spite of their differences. If only such responses as had been made in a very special way were counted (that is, if the response had been restricted through

> further specification), the smoothness of the resulting curves would have been decreased. The curves would have been destroyed through the elimination of many responses that contributed to them
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> . . . A respondent, then, regarded as a correlation of a stimulus and a response and an operant regarded as a functional part of behavior are defined at levels of specification marked by the orderliness of dynamic changes. (pp. 37-40)

Note that Skinner was not recommending that we simply manipulate our independent variable until order emerges; he was recommending that we modify our *definitions* until order emerges. That is, a single set of observations might yield either orderly or disorderly relationships, depending on what we choose to count as stimulus and response events.

Skinner explicitly rejected the practice of ad hoc categorizing stimulus and response units. When a boy hides from a dog, it is a mistake, he averred, to assume, uncritically that the dog is a stimulus or that hiding is a response. Such practices may be useful in interpreting behavior outside the laboratory but should be avoided in the basic science. Skinner also rejected Watson's (1930) definition of a response as "anything the animal does, such as turning toward or away from a light, jumping at a sound, and more highly organized activities such as building a skyscraper, drawing plans, having babies, writing books and the like" (cited in Skinner, 1938, p. 42). Clearly, the latter activities do not share the orderly properties of bar pressing or turning toward a light.

Skinner justified his position on pragmatic rounds, declining to speculate whether methodological advances will permit fruitful analyses of fully restricted units. However, one might have justified it on principled grounds as well: Generic units of analysis follow from a commitment to selectionism. As noted earlier, contingencies of selection cannot yield idealized units; variability is fundamental. Selection contingencies merely set bounds on what is possible; they do not prescribe designs.

The effectors that enable a rat to press a bar were presumably selected for their contribution to foraging, climbing, running, and so on. A particular act, say, extension of the forepaw, contributes to each of these activities, and the structures participating in the act can be explained in part by reference to contingencies of natural selection. Not only do these contingencies tolerate variability in form from one instance to the next, but such variability is actually necessary if reinforcement contingencies are to shape effective locomotion in an uneven environment. Variability of response topography is not only unavoidable, it is adaptive. Skinner's generic units, then, are analogous to species: The units are orderly but are neither arbitrary nor invariant.

Skinner's analysis of behavioral units establishes his science squarely within a selectionist paradigm. Moreover, without his methodological precepts there could be no justified inductions in the analysis of behavior and hence no science of behavior; nor could there be plausible interpretations of complex phenomena based on that science. Despite the importance of Skinner's analysis, his views on the subject are virtually unknown outside the field of behavior analysis.

A Priori Units of Analysis

Some concepts have formal definitions, presumably proposed by one individual and adopted, without complaint, by others. There is no requirement that a concept with a formal definition map onto the dusty and spotted stuff of the real world. To the contrary, concepts with formal definitions *do not* map onto the natural kinds of the real world. We treat some phenomena categorically before we try to define them—garnet crystals, for example. In such a case we may try to define the class, but our attempt requires not *imposing* a definition but *discovering* one. We must study crystals, and we may, at length, settle on a definition that seems to embrace an appropriate group. Nevertheless, on further study we are sure to find a crystal that seems as though it *ought* to qualify as a garnet but that just fails to meet our definition. The crystal has some impurity or irregularity that we had not anticipated. We may modify our definition, or we may just decide that, although imperfect, it is good enough. Our empirical definition will have some slack in it, uncharacteristic of a priori formal definitions. Thus, the problem for the scientist is to determine the natural lines of fracture of the phenomena under study.

The task of determining these lines of fracture is the task of finding order in our subject matter. Order is partly, of course, in the eye of the beholder, thanks to a particular genetic makeup, a particular personal history, or a particular practical contingency—all of which involve contingencies of selection. Although garnet crystals may seem an obvious and well-established category to a mineralogist, they surely do not to a sea slug or to many an unschooled human, for that matter. A chicken will see little to choose between a fox and a coyote but will make much of the difference between a worm and a caterpillar. Honeybees will swarm to buckwheat in the morning but forage elsewhere in the afternoon, bewildering the farmer, who sees no difference. On the other hand, regularities in the world presumably do not wait upon an observer; the apple would still have fallen, even if Newton had overslept. But how these regularities affect the observer is a matter of contingencies of selection.

Categories with formal definitions or categories defined a priori can be said to have essential properties, properties that, in a sense, precede any example of the category. We are free to define such categories as we please, and they need not reflect distinctions in nature. If we choose to define human beings as featherless bipeds, then we cannot object to including *Tyrannosaurus rex.* As it is considered good scientific practice to define one's terms, researchers may be tempted to define their terms before the data are in, and, in effect, provide a formal definition for a concept that is more appropriately defined empirically. Doing so may be harmless, but it invites essentialist thinking into a domain shaped, at least in part, by contingencies of selection. The problem is not unique to cognitive science. The evolutionary biologist Benjamin Burma (1949) voiced the following lament about the species concept:

> In some respects it is extremely unfortunate that names ever get attached to ideas or objects. The false attachment of names to ideas or objects similar but not identical with the original can work harm far exceeding the benefits conferred by having a convenient label. The name "species" has come to such a state. (p. 369)

In biology and psychology, orderly classes of phenomena will have all the variability characteristic of products of selection. Such categories, then, will be fluid both because the exemplars of a category will have varying properties and because the boundaries of the category will depend upon the demands and characteristics of the observer. But it is a rare cognitive scientist who defines his terms empirically. In its flight from the restrictions of behaviorism, cognitive science has abandoned this important methodological constraint. Consequently many cognitive concepts retain a strong essentialist flavor.

Some Difficulties With Essentialist Units of Analysis

It is our thesis that essentialist thinking is no more appropriate in behavioral or cognitive science than it is in evolutionary science. Selection is the only natural process yet proposed for explaining adaptive complexity in nature (Dawkins, 1986), and we argue that this is as true for behavior as it is for morphology. (Note that this is not to claim that *all* structures or behaviors have been specifically selected. As we have argued, selection is a blunt tool that permits, and therefore ensures, variability.) An analysis of a behavioral phenomenon in essentialist terms may well be better than nothing, but it can be more profitably recast in selectionist terms. Moreover, essentialist analyses are prone to the following problems.

Temptation to circular reasoning. In the worst case, essentialism attempts to explain a behavioral phenomenon by inventing a property of the organism responsible for the phenomenon. Because behavioral phenomena are presumably, at least in part, a function of properties of the organism, this seems an innocent step. However, the hypothetical property often is later invoked as an explanation for the phenomenon; it transubstantiates from a tautological construct to an essence with causal status (cf. Skinner, 1963). Of course, this is circular, and it is rare scientist who deliberately engages in circular reasoning. However, it is often easier to ridicule circular reasoning than to avoid it. William James (1907) noted that although no one seriously thinks that wealth is an explanation for having money, it is not uncommon to attribute sickliness to poor health, or muscular feats to great strength, or problem-solving skills to great intelligence. In cognitive science it is not uncommon to attribute language acquisition to linguistic competence, or ineffective performance on a recall task to limited capacity. Patently circular usage is typically avoided by the original proponents of a concept. Circularity often emerges, however, when a concept becomes familiar, it tends to become reified, especially by subsequent researchers, students, and writers of secondary texts. (Indeed, Medin & Ortony, 1989, have suggested that such reification may be fundamental to cognition.)

Curtailment of inquiry. A second shortcoming of essentialist explanations of phenomena is that they tend to cut off inquiry prematurely (Skinner, 1950). Essential properties are treated as givens; they need no further explanation. Note that attributing a behavioral phenomenon to an innate structure of the organism is not essentialistic for innate structures are presumably selected by evolutionary processes. However, the structure will not be ideal, universal, or fixed, but will reveal the variability characteristic of all physical features that are the products of selection. As noted, natural selection permits variability and, indeed, may select for variability. Far from being ideal, structures are often opportunistically cobbled together from the available "raw materials" of ancestral species (Gould, 1980). Investigating the structure, its variability, and its evolutionary origins enriches our understanding of the behavioral phenomenon.

However, the widespread practice of gratuitously invoking the genetic endowment is essentialism in selectionist's garb. To attribute a behavioral phenomenon to an invented property of the organism and then to "explain" the property by casually alluding to the genetic endowment is vacuous unless the origin of the property can plausibly be identified and discussed in selectionist terms. The short-term memory register, for example, is a metaphor, not a structure. As a metaphor it may be useful, but it has no explanatory force, for it is accorded just those properties necessary to explain the disparate phenomena grouped under the heading *short-term memory* (cf. Crowder, 1982). To suggest that it is innate is a statement of faith, not a serious proposal. At best, the suggestion that it is innate is the beginning of inquiry, not its end, for we now have assumed responsibility for accounting for the natural selection of short-term memory. It is most often treated, not as a variable outcome of selection, but as a fixed property of the organism. No attempt is made to treat it as a product of contingencies of selection. .

Unparsimonious explanations. A third drawback of essentialist concepts, and the last that we shall mention here, is that they all require separate explanations. Darwin's theory achieved a dramatic simplification, as many apparent acts of creation were shown to be the product of a few common processes, and seemingly unrelated phenomena were shown to be intimately related to one another. Unconstrained by selectionist thinking, the structures postulated within cognitive science may proliferate in an unconstrained manner. For example, when the data obtained in memory experiments are not readily accommodated by the distinction between short-term and long-term memory, an intermediate memory may be proposed. Similarly, when not all data obtained with a procedure used to study long-term memory can be accommodated by a single set of processes or structures, different subtypes of long-term memory—semantic versus episodic or declarative versus nondeclarative—may be proposed. These proposed subdivisions of memory may begin as convenient shorthand descriptions of observed differences, but become reified into "types" of memory whose characteristics are just those required to encompass the data that were the impetus for their postulation.

Often, such distinctions lead to pointless debate over whether a given phenomenon is really evidence for a given concept or whether it is better taken as evidence of some sense to ask whether *Archaeopteryx* is really a bird or a reptile, there is little to be gained by asking whether a given datum in a memory experiment is really from long-term rather than short-term memory or from episodic rather than semantic memory. Observations of intermediate forms and intermediate memories may both be of great value because of what they reveal about the course of selection, but not because of what they purport to reveal about membership in one or another essentialist "type." Problems with lack of parsimony, as with circularity and curtailment of inquiry, are not necessary consequences of assigning names to classes of observations, but they are often the essentialist accompaniments of such a practice.

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