Behavior and Philosophy, *48*, 25-33 (2020). © 2020 Cambridge Center for Behavioral Studies

FIRST INSTANCES IN PHYLOGENIC AND ONTOGENIC SELECTION AS CAPTURED BY THE VERBAL BEHAVIOR OF SCIENTISTS AND PHILOSOPHERS OF SCIENCE

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ABSTRACT

Selectionist sciences such as evolutionary biology and behavior analysis depend on variations. Variations must emerge before environments can act upon them. Yet if first instances in ontogeny are not products of ontogenic selection they are prerequisites for, but not instances of, selection. They count as behavior but not as operant behavior. When Smith (2019) examines how Skinner treats these issues, he relies on snapshots of Skinner's writings over decades, during which Skinner's approach evolved from one anchored in physics as a model science to one increasingly aligned with biology. Skinner's early treatments of the problems of ontogeny and phylogeny differed from his later formulations. Accounts of scientific behavior based only on verbal samples from an evolving scientific corpus typically omit both their antecedents in the laboratory and the research consequences that follow. Furthermore, behavior analytic research has a long history of exploring the sources of novel behavior. Thus, we need not defer to cognitivist views regarding the Problem of the First Instance.

Keywords: B. F. Skinner; evolution; novelty; selection; verbal behavior

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We have no conflicts of interest to disclose.
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As exemplified in Terry L. Smith's (2019) article, the philosopher of science attends to the structure, logic, and internal consistency of the written products of a scientist's behavior. In an effective science, these products will be related in some systematic way to the natural events the scientist studies. But doing science differs from the philosophy of doing science, and one main difference lies in how verbal behavior works in each. When behavior analysts attend to the relation between natural events and the verbal behavior occasioned by them, they begin by characterizing the antecedents: the written science is verbal behavior occasioned by data. Once it has been created, subsequent verbal behavior is shaped by its consequences. Science doesn't produce a fixed or final written product. Instead, it designates forms of verbal behavior that interact with events. In other words, scientific vocabularies and usages evolve.

Behavior analysts are perhaps unique in turning their analyses on their own behavior. They sometimes examine its origins, as when they consider how new vocabularies evolve along with new discriminations that may be formed during interactions with behavior in the laboratory. For example, Skinner observed the evolving lever pressing of rats as it interacted with various antecedent and consequent stimuli. He learned to discriminate among different aspects of the rats' performances, but the vocabulary then available to him, primarily Pavlovian, depended on other dimensions of the laboratory environment than those that had become salient to Skinner. As he described in his autobiography (Skinner, 1979), he had to free himself from the language of the conditioned reflex as he created a new vocabulary of operant behavior. When confronted with questions raised by philosophers of science, behavior analysts must ask whether the evolved verbal products of their science can adequately be treated without considering their origins and the different audiences they address.

We will argue that Smith's paper bases its general statements on a too-limited sample of quotations drawn from different contexts at different times across Skinner's work over decades and that it makes insufficient contact with the contemporary literature on novelty in ontogeny, which bears significantly on the Problem of the First Instance. Like an explanation of evolution that relies solely upon natural selection, any account of behavior that relies solely on selection is bound to be incomplete. Smith suggests that this presents a problem for Skinner and for behavior analysts, but does it? In particular, does the Problem of the First Instance justify alternative approaches to the study of behavior?

On the evolution of Skinner's verbal behavior

Skinner's verbal behavior evolved over decades. In his early career, Skinner wrote of behavior in terms rooted in classical mechanics and in terms established by his predecessors (e.g., reflexes, habits). Over time, however, his approach changed from one in which physics served as the model science to one in which biology served that purpose (e.g., Catania, 2013; Leão & Neto, 2018; Moxley, 1997; but see also Palmer & Donohoe, 1992). Skinner's verbal behavior evolved as he watched behavior in the laboratory. What did he see, and why was the vocabulary available to him at the time inadequate? An analysis of his verbal behavior depends on identifying its antecedents and consequences and not on determining whether some statements formally align with others. As Day wrote,

> There is little need for Skinner explicitly to repudiate the troublesome sections of *The Behavior of Organisms*, such, for example, as the discussion of static and dynamic laws. His verbal behavior is simply different now from what it was in 1938, the difference being

> undoubtedly accountable for in terms of his more extensive experience in the laboratory [and] his having worked through some of the implications of the analysis of verbal behavior. (1969, p. 490)

Smith quotes many samples of Skinner's verbal behavior, but usually without considering their sources or the later forms that may have evolved from them. For example, he cites Skinner's (1989) comments on *habit* as if they should be consistent with what he wrote more than fifty years earlier (Skinner, 1935). He also writes about Skinner's (1953) comments on the *elements* of behavior as if they were a central component of Skinner's conceptual apparatus, even though Skinner explicitly presented the term as one directly adopted from what a traditional critic might have deployed — instead of being his own term, it belongs to a hypothetical competitor. Arguing that elements are a crucial feature of Skinner's position gives unnecessary weight to a rhetorical flourish. The term *elements*, as pertaining to components of behavior, appears rarely in Skinner's corpus.

On the distinction between qualitative and quantitative characteristics

Smith's abstract reads, "Skinner limits the analogy between phylogeny and ontogeny... Although qualitative characters exist in phylogeny, Skinner does not think they exist in ontogeny" (p. 1). Skinner, however, didn't invoke the distinction between qualitative and quantitative traits in his accounts of phylogeny and ontogeny. Rather, he emphasized continuity of characteristics in all selectionist systems (e.g., Skinner, 1966, 1975, 1981). Darwin wrote,

> ... there is no fundamental difference of this kind. We must also admit that there is a much wider interval in mental power between one of the lowest fishes … and one of the higher apes, than between an ape and man; yet this immense interval is filled up by numberless gradations. (1871, pp. 34-35)

We could imagine Skinner making a similar point regarding the continuum from lever presses and key pecks to human verbal behavior (Skinner, 1957).

Smith distinguishes between *qualitative* and *quantitative* traits, but what is the significance of this difference? He describes smooth versus wrinkled peas as an example of "natural selection of a binary character controlled at a single locus" (p. 3) and goes on to argue that, though the populations of both kinds of peas may vary continuously in smoothness, they remain separate and thus smoothness is a qualitative rather than a quantitative trait. Here the discrete or qualitative dimension must be attributed to the genotype, whereas the phenotype can vary continuously or quantitatively. Subpopulations may be characterized by distinct genetic characters, but it does not follow that the relevant phenotypic traits are categorical.

It may be instructive to expand on the analogy between phylogenic and ontogenic selection, as operants and species are classes that have many features in common (Catania, 1978). Smith's example presumably derives from Mendelian genetics though he does not cite it. Mendelian genetics, however, is not relevant to Darwinian selection at the level of species and phylogeny. Darwin's death, after which Mendelian genetics provided the main account of descent, began a period some have called the Eclipse of Darwinism. Darwinism was said to be on its deathbed (Bowler, 1983; Catania, 1987), because Mendelian genetics could not provide the prerequisite variations for evolution by natural selection. The eclipse ended with the modern synthesis in biology in the 1920s, when geneticists combined mutation with Mendelian genetics to provide the variations demanded by Darwin's selectionism (Huxley, 1942).

Qualitative versus quantitative characteristics in ontogeny

Qualitative traits are no more an issue in ontogenic selection than they are in phylogenic selection. Operants and species are classes that have many features in common (Catania, 1978), but they are not the only kinds of classes. In biology, for example, classes of organ systems cut across species boundaries. We aren't sure from Smith's account what dimensions of behavior might count as qualitative, but it's our guess that though they might count as behavior they would not count as *operant* behavior. Thus, based on what we know about selectionist systems, they no more pose a fundamental problem for Skinner's selectionism than Mendelian selection does for Darwin's. Skinner had no objection to units of behavior that might not be operant: "In such a bird as the pigeon, pecking has a certain genetic unity" (Ferster & Skinner, 1957, p. 7).

A possible ontogenic analog to smooth versus wrinkly peas can be found in phonemic units of speech. A child raised in an English-speaking environment discriminates between the consonants *b* and *p* but a child from an Arabic-speaking environment does not. A discontinuity or qualitative difference appears for some listeners but not for others. Risley (1977) provides one of several accounts of how these discontinuities can arise automatically through shaping based on the consequences of hearing both one's own voice and the voices of caregivers. Adult English speakers don't report intermediate phonemes between *b* and *p*, even when the stimulus varies continuously between the two (Harnad, 2003). Hence, an apparent categorical distinction emerges from a continuous function (see also Catania, 2017, p. 209). Categories obscure the underlying continuous functions from which they have been derived. The contingencies of speech produce the behavioral units we call phonemes, just as other contingencies produce the units we call operants.

Smith argues that Skinner's behavior analysis cannot deal with categorically distinct ontogenic behavior. This assumes relevant discontinuities in ontogeny, but in the absence of other evidence too much hinges on a short paragraph near the end of his paper:

> Novel behavior thus poses a conceptual challenge for selection by consequences only in the case of a truly qualitative response. This is where (as we have already seen) selection is at a pronounced disadvantage. And this is why examples of syntactically novel utterances offer what appear to be examples of the limitations of operant selection. They purport to be qualitatively discrete responses, and thus beyond the reach of shaping. With this type of response, the problem of the First Instance looms large (p. 11).

With regard to novelty, deciding whether a difference is qualitative is not the only problem. Another difficulty is in deciding which properties of utterances are indeed novel. As Skinner (1953, p. 94) wrote: “We divide behavior into hard and fast units and are then surprised to find that the organism disregards the boundaries we have set.” Reinforcement does not operate on single responses and it does not respect structural boundaries. Reinforcement strengthens all responses that share critical dimensions of the behavior that preceded the reinforcer. The task may then be to find two behaviors in ontogeny that share no dimensions. Surely this would imply “qualitatively discrete” behaviors. Skinner (1953, p. 94) notes, however, this may not be possible: “It is difficult to conceive of two responses which do not have something in common."

We should seek out the novelty of *properties* of the utterance. Linguists such as Chomsky have made much of children speaking sentences that they have neither heard nor spoken before. But those utterances are composed of words and sentence structures that have been frequent in the child's verbal history. The only novel feature is their new combination. As MacCorquodale wrote,

> That a child learns certain orders… on the basis of a relatively small sampling from the enormous universe of such instances shows simply that a child is able to make complex abstractions and to generalize from them to diverse new instances. A parameter value may surprise us, but it does not prove that the processes of stimulus generalization and response induction are not applicable. (1970, p. 93-4)

If such processes are indeed applicable, an apparently novel qualitative response is instead likely to be only quantitatively novel.

Are phylogeny and ontogeny analogous?

Behavior analysts have not ignored the sources of adaptive novel behavior, but their substantial literature relevant to the sources of novel behavior is too often unrecognized. Consider just a few: Neuringer and Jensen's (2013) review of cases in which novelty in operant behavior is reliably engendered by reinforcement procedures; research by Blough (1966) that anticipated later demonstrations of the reinforcement of variability as a dimension of responding (e.g., Page & Neuringer, 1985; Neuringer, 2004); Pryor et al. (1969) on novel behavior in the porpoise; the Columban Simulations in Skinner's laboratory in the early 1980's (e.g., Epstein et al., 1981, 1984); Sidman's (1994) research on equivalence classes; and the emergence of novel operant behavior along multiple stimulus dimensions in what has been called adduction (e.g., Catania, 2017, Chapter 17; Catania et al., 2000).

In a footnote, Smith acknowledges the well-established finding that novel behavior can be reinforced. He claims this still fails to explain the origins of novel responses and suggests that the selection of novel behavior is irrelevant to the Problem of the First Instance unless it can account for "qualitatively novel responses that are appropriate to the context" (p. 12). He continues: "Unless one is willing to argue that qualitatively novel but appropriate human action is an illusion — that is, that such actions occur in the same 'random' way observed in phylogeny—ontogeny faces a problem that phylogeny does not face."

Phylogenic traits are not adaptive on their first occurrence; they become so over time as they adapt to their environments. The same is necessarily the case for emergent behavior in ontogeny. But we should not expect to find first occurrences in higher-order classes of behavior, such as the syntactic units of human verbal behavior, if we cannot identify them in the more fundamental classes that provide the nonverbal scaffolding that supports verbal behavior. We've already seen how continua can be reduced to ostensibly qualitative categories, but even aside from the issue of what would qualify as a qualitative trait reason for doubt remains. The term *action* is drawn from colloquial vocabulary, and for that and other reasons actions are not equivalent to instances of an operant class. We are still left with the problem of how we decide whether differences are qualitative or quantitative. Categorization does not provide us with a sufficient condition.

Does qualitatively novel behavior justify alternative approaches?

Smith (2019) is sympathetic to the formalist's position. His paper implies that cognitive science can deal with qualitatively novel behavior where behavior analysis cannot — indeed, that cognitive science "specializes" in this approach: "all of its basic categories are qualitative or discrete" (Smith, p. 13). This seems a *non sequitur*. That cognitive science comprises qualitative

categorizations (are there any other kinds?) is unrelated to the question at hand, which is about the provenance of ontogenic behavior. As Smith notes, one issue is whether categories reflect "the natural lines along which behavior and environment actually break" (Skinner, 1972, p. 458). Another is whether categories are explanatory. But the verbal categories in cognitive science are typically tautologies (e.g., Skinner, 1977) and that makes them unacceptable as explanations. Thus, whether cognitive science specializes in qualitative characters is scarcely relevant to the Problem of the First Instance.

Smith notes that the problem of the First Instance applies in all selectionist systems, including those of phylogeny and ontogeny. In deferring to the formalist, Smith inadvertently takes a classical position that opposes evolutionary biology. If qualitatively novel behavior in ontogeny lends credence to cognitive science, then qualitatively novel characteristics in phylogeny lend credence to creation science, in competition with evolutionary biology. The problem vanishes, however, if we recognize instead that selectionism, though crucial, is not exhaustive. It doesn't explain everything. Here is how Charlesworth et al. put it with respect to phylogenic selection:

> We have focused our discussion on the sources of the variability used in *adaptive* evolution. However, it is important to understand that contemporary evolutionary biology does not take a dogmatically adaptationist or pan-selectionist view of the evolutionary causes of all characteristics of living organisms. This is especially true for properties of the genome itself, many of which must involve interactions between the effects of mutational processes, selection and genetic drift. (2017, pp. 9-10).

Skinner too did not rule out different categories of causes. For example, in discussing whether private events can function as causes he wrote that:

> ... they may be called causes, but not initiating causes. ... We do engage in productive private verbal behavior in which some initiation certainly occurs, if that term means anything, but if my analysis is correct, public versions must have been established first. In that case, the initiation passes to the environment. (p. 719, in Catania & Harnad, 1984).

In a characteristic Skinnerian move, he locates the ultimate causes in the environment. In this context it is fitting that he regarded the difference between public and private behavior not as a qualitative one but rather as extremes on a continuum: "The skin is not that important as a boundary" (p. 617 in Catania & Harnad, 1984).

To whom does the Problem of the First Instance belong?

Many once believed that Skinner claimed to explain everything with reinforcement (perhaps some still do). That was why the so-called circularity of the Law of Effect troubled so many (e.g., Postman, 1947). This is less of an issue these days but, as exemplified by Smith's critique, the problem seems to have moved elsewhere. Now some seem to believe that Skinner claimed that the operant explains everything. Another misunderstanding is that a reinforcer generates new responses that are like the one that produced it. But it must generate variants of that response, not just replications. Otherwise it would be no more effective in allowing operant selection in ontogeny than Mendelian genetics was in allowing Darwinian selection in phylogeny.

Any particular first instance may not be explicable in selectionist terms. This arises not just in theory, but in practice:

> … the first instance can be encouraged by arranging the student's environment and history of reinforcement to improve the likelihood that the desired entity will emerge. Its occurrence, however, cannot be guaranteed. … Applied practitioners have recourse to few options when they are faced with what Skinner called 'the problem of the first instance.' (Touchette & Howard, 1984, p. 176).

In such circumstances it is pragmatic to focus on the consequences of behavior rather than their antecedents.

Smith (p. 2) states that Skinner, in the *Behavioral and Brain Sciences* treatment of some of his classic papers (Catania & Harnad, 1984), demurred rather than addressing the question of the origins of behavior. We must challenge that interpretation. In the passage Smith cites, Skinner writes that a human "possesses a large pool of uncommitted behavior available for quick shaping" (p. 609). The origins of that pool are rooted in evolutionary history. Skinner notes that a pigeon's behavior may be shaped as a sculptor shapes clay, but also that the pigeon's peck often spontaneously emerges as a distinct unit; it does not need to be shaped by successive approximations to its final form. He accounts for the discontinuity by appealing to phylogeny:

> The pecking movement usually emerges as an obviously preformed unit. … It is possible, however, that there is a genetic discontinuity, and that in a bird such as the pigeon the pecking response has a special strength and a special coherence as a form of species behavior. ... Continuity with other behavior must be sought in the evolutionary process. (Skinner, 1953, p. 93)

Skinner later extended his answer to this issue by adding cultural or sociogenic selection to the phylogenic and ontogenic varieties of selection by consequences:

> How and why do responses occur before they have been reinforced? The problem was solved in part by the evolution of processes through which individuals take advantage of behavior already acquired by others. Imitation is an example. It often brings the imitator into contact with the reinforcing consequences responsible for the behavior imitated. The behavior of the imitator is "primed" in the sense of made to occur for the first time and usually when it is likely to be reinforced. (1990, p. 1206)

Elsewhere he wrote, "Imitation and modeling are not foolproof behavioral processes, but they were the best natural selection could do" (in Catania & Harnad, 1984, p, 718).

The origins of imitation remain unexplained. But how much should this matter to the behavior analyst? Evolutionary biologists may be interested in the origins of life on Earth, but they seldom work on the problem (and when they do, they are not doing evolutionary biology). The modern evolutionary synthesis says nothing about how life came to be but only about what happened once it did. Life's genesis is a matter for other scientists. Likewise, however much behavior analysts may be interested in the origins of operant behavior, they seldom work on the problem. They leave it to other scientists (though typically not cognitive ones). As behavior analysts we may well identify conditions under which novel variations are likely to emerge, but because they are novel, we won't be able to say ahead of time what they will be.

A similar case can be made with regard to seeking the origins of behavior in the nervous system. When asked about this issue (in Catania & Harnad, 1984), Skinner wrote:

> Introspective mentalists simply put the neurologist on the wrong track, and so I believe do cognitive psychologists. It is the function of a science of behavior at the present time to give neurologists their assignments, as it was the function of genetics prior to the discovery of DNA to give modern geneticists their assignment with respect to the gene. (p. 507)

Let's leave Skinner with the last word here. He later added that "the 'reduction' of behavioral facts to neurological facts [will not] be helpful until the behavioral facts are correct. The phylogeny and ontogeny of behavior are subjects in their own right" (p. 707).

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