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BEHAVIORAL CAUSATION, CONTINUITY, AND NOVELTY

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ABSTRACT

In the target article, Smith (2019) tackles the problem of "the first instance" from a purely selectionist standpoint. He first shows that, intuitions to the contrary notwithstanding, natural selection can explain the occurrence of one particular type of novelty: the occurrence of novel magnitudes in the distribution of a quantitative phenotypic character. Extending this reasoning to the behavioral domain, Smith proposes that behaviorism may solve the problem of behavioral novelty in terms of operant reinforcement, behavioral atoms, and continuous response properties. Contrary to Smith, I argue that operant selectionism fails as an account of behavioral novelty even in simple cases, that selectionism is more crippling than helpful to behavior analysis, and that behaviorism in no way depends on the assumption of fundamentally continuous response dimensions.

Key terms: novelty, continuity, discreteness, behavior, stimuli, time

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As anyone who has read *Behavior and Its Causes* (Smith, 1994) would expect, the target article by Smith (2019a) is both thoughtful and provocative. In addition to his analysis of the foundations of behaviorism (e.g., Smith, 1988, 1994), Smith has expressed a long-standing interest in the analogy between operant reinforcement and natural selection (Smith, 1983, 2019b). Here he expands on his previous material by showing how Skinner's selectionist analogy might explain at least some cases of behavioral novelty.

Three parts of his argument can be distinguished. Smith starts with evolutionary biology, by showing that natural selection can produce novel magnitudes in the distribution of a quantitative character. He then extends this argument to the behavioral domain, suggesting that operant reinforcement can similarly account for the emergence of new responses, provided these responses involve continuous dimensions. He also discusses the possible role of reinforcement in complex cases of behavioral novelty, such as suicide — the sort of cases that featured prominently in Chomsky's (1959) critique of Skinnerian behaviorism. (Here Smith's discussion necessarily involves interpretation rather than experimental analysis.) He concludes with a deep and, if correct, revolutionary claim about the nature of behaviorism and cognitivism: whereas behavior analysis relies fundamentally on continuous behavioral properties, "cognitive science is based on a different metaphysics ... all of its basic categories are qualitative or discrete" (p. 13).

How much of this Smith actually endorses is unimportant: here and elsewhere (e.g., Smith, 1994, 2019b), it seems to me that he is more interested in examining different positions philosophically, and finding out where they lead, than in taking sides. In any case, in his article he formulates a set of beliefs about behaviorism that are worth examining, and I will proceed as if he endorsed all of them (even though this may not be the case). Imagine, then, that a majority of behavior analysts come to share Smith's beliefs about the analogy between operant reinforcement and natural selection, the role of reinforcement in behavioral novelty, and the primacy of quantitative behavioral properties over qualitative ones. Would behavior analysis actually make lasting progress on the problem of the "first instance?" And, more broadly, would behavior analysis be strengthened as a research program, or would it suffer from conceptual limitations that may even impede its further development as a science?

The answer to these questions depends in part on the correctness of the proposed explanations (can some cases of behavioral novelty actually be explained along the lines of what Smith suggests?), but the answer to the second question also depends on what one *means* by "behavior analysis." If the latter is stipulated to consist of Skinner's (say, post-1968) view of the field as operant "selection by consequences" (cf. Catania and Harnad, 1988), then, of course, behaviorists have no choice but to keep working on the development of the selectionist metaphor. But this self-imposed restriction would conflate the philosophical core of behavior analysis with a particular set of historical accidents, no matter how (supposedly) influential:

> Many of the contemporary features of behavior analysis ... were acquired through the contributions of individual scientists and reflect their own judgments and misjudgments. Thus, behavior analysts' views on the importance of reinforcement can and do vary (e.g., Malone, 1978), and one can propose alternatives to Skinnerian concepts while remaining a behavior analyst (e.g., Rachlin, 1973). There is one feature of behavior analysis, however, that seems indispensable. No researcher could stop studying relations between environment and behavior and still claim to be doing behavior analysis. Behavior analysis places no restrictions on how these relations can be studied or described, but it does prescribe that these

> relations are studied. ... behavior analysis embodies a mode of explanation that appeals fundamentally to the environment. (Tonneau, 2011, p. 34)

Adherence to what Hineline (1990) has called, environment-based psychological theory, certainly does not require any commitment to operant selectionist notions; neither, by the way, does it require a Machian or Humean philosophy of science. As I have argued elsewhere in detail (Tonneau, 2013), the best justification for behavior analysis may simply be the view that environment-behavior relations are *constitutive* of psychological phenomena — an ontological thesis about the nature of the mind, if you will (cf. Rachlin, 1994; Tonneau, 2013). It is this indigenous philosophical thesis that may ultimately justify the focus on the environment that is evident in behavior analysis. Although neither prohibited nor required, will the analogy between operant reinforcement and natural selection prove a valid contribution to environment-based psychological theory?

I doubt it. The validity of the analogy is debatable (see, e.g., the commentaries and replies in Hull et al., 2001), and Skinner's version of it especially so, considering its narrow focus on selection to the detriment of variation processes (cf. Staddon, 2014; Staddon & Simmelhag, 1971). In support of the pure-selection viewpoint, Smith puts forward a case of evolutionary change that does not require any new mutation to proceed. The example is correct, but highly restricted. It may be the *only* case of evolutionary novelty to fulfill the pure-selection scenario. In all other cases I can think of, and excluding strictly population-level changes such as migration and drift (which do not involve new mutations, but do not involve natural selection either), mutation is involved. Furthermore, as Smith is well aware, the fact that pure selection can explain a special case of evolutionary change does not entail that the changes actually observed under directional selection are due to selection alone. Examination of real cases often reveal values of phenotypic variance that are inconsistent with pure selection models (see, e.g., Falconer & Mackay, 1996).

Thus, if behavior analysis is to embrace some version of the analogy between reinforcement and natural selection, the biological evidence would seem to favor a version of the analogy closer to Staddon's (2014) than Skinner's. Of course, one could resist this conclusion by maintaining that in the behavioral domain all variation is random, not only in the sense of being independent of its later effectiveness (as non-directed mutations are in biology), but also in the sense of being unrelated to present and past environments. And this, indeed, seems to have been Skinner's position, as expressed by his well-known comparison of operant behavior to a lump of clay (Smith, 2019a). The problem with a version of the selectionist analogy devoid of variation processes, however, is that it conflicts with the very facts that Chomsky (1959) hammered at behaviorism and that Smith wishes to address: the existence of numerous cases of behavior (for example, linguistic behavior) that are emitted without having been reinforced previously, yet depend on previous environmental inputs.

How do Smith's (2019a) attempts at explaining behavioral novelty fare in this respect? A close look at his argument reveals difficulties even when addressing a (simple?) motor act such as lever pressing. In the biological example that Smith discusses, the alleles that replicate differentially are causal antecedents, not components, of the target phenotypic character (R). Each of these alleles, either present or absent at any given locus, acts in conjunction with the others to determine the magnitude of R via complex pathways of gene expression. Because some of the alleles may have opposing effects, it is possible, in breeding experiments with downward lines, to select organisms with magnitudes of the target phenotypic character that are *lower* than in the parent generation. The alleles with a higher reproductive success end up being *more* prevalent in the population (in terms of probability of occupancy at any given locus), yet the associated phenotypic trait is of lower magnitude on the average.

On the operant side of the analogy, however, each of the elementary replicators on which selection works is said to be "a sort of behavioral atom ... the essential ingredient or *component* of all observed instances" (Skinner, 1953, as quoted by Smith, 2019a, p. 8; emphasis mine). How, then, is it possible to reinforce behavior differentially so that responses with higher forces, durations, or magnitudes, are emitted at *lower* rates than the others (e.g., Hunter & Davison, 1982)? How is it even possible for response duration to *decrease* while responding is being reinforced, so that not only the whole instances (R), but also, of necessity, their components, become more prevalent over time (Margulies, 1961)? How can a *higher* number of responses emitted over time coexist with a *lower* number of atoms on each response emission? Are not the atoms the true replicators under reinforcement, according to Skinner (1953)?

One way out of the problem would be to assume that the number of atoms within the time frame of a single response (R) is the only direct measure of their fitness (their overall free-operant rate, and that of the response itself, being by-products) and to stipulate two kinds of atoms: the "positive" atoms, which contribute to an increased response magnitude, and the "negative" atoms, which contribute to a decreased response magnitude. These positive and negative atoms could consist, for example, of flexion versus extension movements, or of keeping a lever down versus releasing it, and they would be analogous to alleles of different types. But now we face another issue, which is of accounting for what Skinner (1953, as quoted by Smith, 2019a), called the "functional unity" of an operant response such as lever pressing. The spatial integration of typical operant responses (a spatial integration that obtains for good biological reasons) seems incompatible with a collection of independent, positive and negative atoms, each emitted at its own rate or with its own probability. If we want to account for the shape of a distribution of response durations under differential reinforcement (e.g., Kuch, 1974), for example, we will need to abandon the assumption of independence of atomic components and restrict the points of occurrence of negative atoms at certain times, so that response instances terminate at the right moments. This may make it possible to accommodate the functional unity of operant responses but dropping the assumption of atomic independence runs counter to Smith's selectionist explanation of novel magnitudes.[2]

The selectionist explanation of behavioral novelty fails at a deeper level when we move from a rat pressing a lever to a complex act such as suicide. This becomes more evident if we slightly change Smith's example of the brook to imagine suicide from a bridge with parapets. The people who climb over a parapet to commit suicide execute movements with their legs that they would never emit when throwing middle-size objects into the water. Thus, even if an object's vanishing below the surface reinforces components of the arm movements of throwing (which basically is Smith's hypothesis), in this case a history of selection of throwing cannot explain how suicide is executed. Note that the selectionist hypothesis cannot be rescued by imagining an

[2] A note to avoid possible confusions. Here I have not tried to *prove* that a pure-selection model of novel operant magnitudes is untenable. Perhaps Smith's proposal about magnitudes will eventually be made to work in the behavioral domain, although I doubt it. What I have been arguing instead is that explaining novel magnitudes of response characters along the lines of the target article faces serious difficulties. In response, operant selectionists can of course always take the neural route: if they stop assuming that the operant replicators are behavioral, and assimilate them instead to neural elements, distinct from behavior in the same sense that alleles are distinct from phenotypic characters, then the objections I have raised no longer apply (cf. Tonneau, 2016). The operant replicators are no longer *parts,* as opposed to causal antecedents, of behavior being reinforced. The question that remains at this stage is whether behavior analysts prefer models that are selectionist but not behavioral to models that are behavioral but not selectionist.

alternative history of reinforcement for leg movements, because the latter are never reinforced by the vanishing of objects. When climbing over an obstacle, we actually become closer to, not farther from, the objects in front of us.

One of the fundamental reasons for the failure of Smith's model lies in its focus on responding at the expense of *stimuli*. Operant selectionism tends to neglect stimuli because the environmental events and properties that guide behavior are not the sort of things that could repeat over time ("reproduce") the way a reinforced response does.[3] Although we are very far from understanding a behavioral episode as complex as suicide, in the case that Smith discusses we may intuitively assume that having one's body under water is a current "goal" of the suicidal person — a goal that was "made important" by networks of connected "beliefs" and that now attracts behavior through whatever movements are fitting. Now, a well-known line of arguments in philosophy might lead us to conclude that intentional idioms such as "beliefs" and "goals" are beyond the bounds of environment-based psychological theory (see Foxall, 2007), but I remain unconvinced (Tonneau, 2007). It seems to me that progress can be made toward a stimulus-based account of beliefs and goals (cf. Tonneau, 2007, 2011, 2013), provided the stimuli in question are both remote in time from current behavior and scattered over widely distributed time frames.[4]

From this perspective, neither one's leg movements while climbing over a parapet nor any of their components need to ever have been *reinforced* in one's past. Climbing over obstacles, however, has been consistently *correlated* with coming closer to an object visible in the forward direction, an object that now functions as a behavioral facilitator because of its inclusion in molar patterns of stimuli that include past glimpses of water, disappearing objects, and the like. Of course, no such account has ever been fleshed in, but contrary to Smith's operant selectionism, at least this does not seem barred from succeeding. Developing an account along these lines, however, will require abandoning what Bindra (1978) has called the "response-reinforcement framework"[5] in favor of a theoretical approach in which operant performance is guided, rather than reinforced, by antecedent-stimulus correlations (cf., e.g., Baum, 2012; Cowie, 2019; Cowie & Davison, 2016; Davison, 2017; Shahan, 2017).

Emphasizing the role of stimulus correlations in guiding performance does not mean neglecting other historical influences on behavior, such as the emission of previous responses in different contexts (see, e.g., the concepts of transformation function in Epstein, 1985, and transformation rule in Fischer, 1980). On the contrary, once liberated from the assumptions of operant selectionism, we can explore a fuller range of determinants of behavior while leaving the issue of continuity vs. discreteness of responding (Smith, 2019a) completely open. In Catania and Cerrutti's (1986) demonstration of the emergence of new response forms through the recombination of controlling stimulus properties (circle → peck on the left, triangle → peck on the right, green color → fast pecking, red color → slow pecking), for example, why insist that

[3] Operant selectionists may try to incorporate stimuli in their account by hypothesizing that operant reinforcement selects stimulus-response relations instead of mere responses. This won't work, however, for reasons discussed by Tonneau and Sokolowski (2000, p. 170).

[4] This assumption is necessary to find an environmental substitute for a "non-existent state of affairs" such as one's drowning (in the case of somebody who never committed suicide). What makes the state "non-existent" is the very fact that its stimulus components are temporally disconnected from one another. For further discussion see Tonneau (2011, 2013).

[5] One does not need to agree with Bindra's (1978) own theoretical outlook to appreciate the validity of his criticisms. They are highly relevant to the target article, and more generally to the issue of novel, adaptive performance.

performance is fundamentally continuous? Why not respect the functional unity of behavior (Smith, 2019a, p. 6) and the spatial discreteness (left vs. right) of the stimulus objects that guided key pecking? Similarly, in Covarrubias and Tonneau's (2016) study of spatial behavior in the A-no-B sandbox task, why should behavior analysts disregard *a priori* a model of performance in terms of discrete, probabilistic response mixtures?

The main merit of the target article (and this seems to have been Smith's purpose) is to clarify where the assumptions of behavioral selectionism lead. When looking at the results, however, I find them more crippling than helpful to behavior analysis. In the case of cognitivism, it seems to me that Smith conflates historical accidents, however influential, with essence. It is true that Chomsky's program, for example, relied fundamentally on discrete units, presumably because of his inspiration in computability and automaton theory (e.g., Chomsky, 1956). A similar orientation can be found in the language-of-thought hypothesis (e.g., Fodor, 1975) and in information-processing models of cognition. But, just as behavior analysts have no reason to limit themselves to continuous properties of behavior, cognitivists have no reason to limit themselves to discrete cognitive units. And, indeed, a Gestalt theorist none other than Köhler (1940) could freely invoke isomorphisms between continuous fields of neural activity and perception without turning into a closet behaviorist. As Scheerer (1994) remarked:

> In many ways, Gestalt theory is directly opposed to the current "computational theory of mind" (CTM) espoused most forcefully by Fodor (1975). The biggest difference is that under the CTM representations must be composed of syntactically structured symbols, while under Gestalt theory mental representations are realized by cortical fields and thus must be analogue. (p. 191)

To put it simply: if one is willing to admit "representations" in one's theories, why not *continuous* representations?

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