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COMMENTARY ON LEÃO AND CARVALHO NETO (2018): "SUCCESSIVE APPROXIMATIONS TO SELECTIONISM: SKINNER'S FRAMEWORK FOR BEHAVIOR IN THE 1930S AND 1940S"

COMENTARIO SOBRE LEÃO Y CARVALHO NETO (2018): "APROXIMACIONES SUCESIVAS AL SELECCIONISMO: EL MARCO DE SKINNER PARA EL COMPORTAMIENTO EN LAS DÉCADA S DE 1930 Y 1940"

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The authors have analyzed Skinner's early writings in an effort to identify sources of his thesis of selection by consequences as a causal mode in the analysis of behavior. The authors conclude that despite the presence of some aspects of selectionism in the 1930s and 1940s, remnants of antecedent, mechanistic causation still appear in much of his writing during these two decades. As a result, the authors point to the 1950s and beyond for the emergence of a reasonably mature version of selectionism in Skinner's writings.

I agree with much of the authors' scholarship and many of their conclusions, but I would place a slightly different spin on some of those conclusions, notably concerning variability and the generic basis of behavior. I expand on these matters

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a bit later. For now, let me start by summarizing what I take to be the background for selectionism, at least as represented in Skinner's work.

I'll begin by noting that in his reply to comments by Dahlbom in Catania and Harnad (1988, pp. 29-32), Skinner stated as follows:

> I had done research on the selection of behavior by consequences for many years before the similarity to natural selection suggested itself. (Skinner in Catania & Harnad, 1988, p. 32)

I am not sure about the precise number of years Skinner meant here when he said he had done research on the selection of behavior by its consequences "for many years." He clearly began research on operant behavior for his dissertation in 1930-1931, but as he himself notes, to suggest that he considered his dissertation research to be in the selectionist tradition is almost certainly premature. Thus, it seems to me that Skinner's own words here support the authors' conclusions. With respect to Skinner, worth noting is that Thorndike had done research on how the consequences of behavior affected its probability, and Thorndike used the term selection in connection with that research. When Skinner described his own research, he didn't often acknowledge Thorndike, perhaps because of Thorndike's appeal to emotional side effects of those consequences as the active ingredient that through temporal contiguity stamped in (through satisfaction) or stamping out (through discomfort or annoyance) the response during the learning process.

In one of Skinner's autobiographical statements, he mentioned how his "operational analysis" of the term reflex identified two distinct types of behavior, which he later termed respondent and operant (e.g., Skinner, 1978, p. 119; see also 1935b, 1937). These two types were distinguished by the environmental circumstances (e.g., operations) that produced them. Antecedent circumstances elicited respondents, whereas operants were emitted and then reinforced by their consequences. For operants, an antecedent stimulus set the occasion for the response, in a relation that superficially resembled that of conditioned respondents, but the critical relation was that the response produced a reinforcing consequence. The discriminative function of the antecedent stimulus was derived from the reinforcement relation, not the eliciting relation. In a relevant paragraph, Skinner (1978) then commented as follows:

> In my reply I used the term "operant" for the first time and applied "respondent" to the Pavlovian case. It would have been the right time to abandon "reflex," but I was still strongly under the control of Sherrington, Magnus, and Pavlov, and I continued to hold to the term doggedly when I wrote *The Behavior of Organisms* (1938). It took me several years to break free of my own stimulus control in the field of operant behavior. From this point on, however, I was clearly no longer a stimulus-response psychologist. (pp. 119-120)

Here, Skinner is writing about an earlier article in which he distinguished between two different types of behavior (Skinner, 1935b), then his reply to Konorski and Miller who challenged Skinner's distinction (Skinner, 1937). Once again, we can see that Skinner's usage of terms is somewhat inconsistent, much as the present authors point out.

It has always seemed to me that the problem with which Skinner was struggling can be traced to classical S-R behaviorism. Early classical behaviorists such as Watson sought to develop an effective alternative to the dominant concern with mental life as a subject matter for psychology and introspection as a method. The study of how behavior aided in adaptation was clearly such an alternative. However, for classical behaviorists there remained only one type of behavior. That type was caused by antecedent circumstances, in the fashion of the reflex arc. At issue was whether behavior was more flexible, variable, and spontaneous than strict reliance on only antecedent circumstances allowed. The sequential organization of behavior over time also presented a problem.

Of course, early S-R behaviorists had answers to these challenges. For example, perhaps there were unrecognized or at least underappreciated antecedent eliciting circumstances that afforded what only appeared to be flexibility, variability, and spontaneity. When it came to the organization of behavior over time, early behaviorists relied on concatenation of S-R units based on kinesthetic feedback, like links in a chain. A rat learned to run a maze when kinesthetic feedback linked the steps together—the feedback from steps with the right paw elicited taking steps with the left paw, and so on.

These answers failed to convince many researchers and theorists. To use examples in traditional learning theory, thousands of experiments were conducted on place versus response learning, latent learning, and so on. Early on, E. B. Holt (1914) had anticipated the concept of purposive behavior as an alternative to mechanistic S-R formulations, and in the 1920s McDougall challenged Watson on similar

grounds. The result was that by the 1930s, researchers and theorists had begun to insert a hypothetical variable inside the organism in some sense that mediated the relation between stimulus and response. The result was mediational S-O-R neobehaviorism. Skinner (1978) cited Tolman as representative:

> What I had called a "third variable" Tolman called "intervening." For me the observable operations in conditioning, drive, and emotion lay *outside* the organism, but Tolman put them inside, as replacements for if not simply redefinitions of, mental processes, and that is where they still are in cognitive psychology today. Ironically, the arrangements are much closer than mine to the traditional reflex arc. (p. 118)

Prime among the overarching concepts that knit the mediating variables together was purpose.

Skinner embraced little of this whole orientation to psychological theorizing from the beginning. His operational distinction between operants and respondents meant that he did not need to embrace mediational neobehaviorism. I say this guardedly because he did appeal to the mediating variable of the reflex reserve (Skinner, 1938), but he eventually abandoned it. However, he still needed to account for flexibility and variability of operant behavior. He did so relatively early, in terms of the generic nature of stimulus and response (Skinner, 1935a). This notion gave him the degrees of freedom to avoid a commitment to antecedent mechanistic control. In the case of operants, the class was defined in terms of the contingency, not the antecedent stimulus that elicited the response. Yes, individual instances of a response class may have different properties. The rat may press the lever with greater or lesser force, with its left or right paw, at one end or the other of the lever, but if the contingency didn't specify any of these properties, rather only that the lever be pressed, these properties were incidental and varied from instance to instance. Perhaps it is only a matter of emphasis in comparison with the authors' analysis, but I have always thought Palmer and Donahoe (1992) emphasized that Skinner accommodating variability through the generic nature of behavior, and the generic nature of behavior established the necessary foundation for selectionism, but was not selectionism per se. To be sure, research on the property of variability in operant behavior continues.

It has always seemed to me that the formal emergence of selectionism is in *Science and Human Behavior* (Skinner, 1953):

> In both operant conditioning and the evolutionary selection of behavioral characteristics, consequences alter future probability. Reflexes and other innate patterns of behavior evolve because they increase the chances of survival of the species. Operants grow strong because they are followed by important consequences in the life of the individual. Both processes raise the question of purpose for the same reason, and in both the appeal to a final cause may be rejected in the same way. (p. 90)

And again:

> We have seen that in certain respects operant reinforcement resembles the natural selection of evolutionary theory. Just as genetic characteristics which arise as mutations are selected or discarded by their consequences, so novel forms of behavior are selected or discarded through reinforcement. There is still a third kind of selection which applies to cultural practices. A group adopts a given practice— a custom, a manner, a controlling device—either by design or through some event which, so far as its effect upon the group is concerned, may be wholly accidental. As a characteristic of the social environment this practice modifies the behavior of members of the group. The resulting behavior may affect the success of the group in competition with other groups or with the nonsocial environment. Cultural practices which are advantageous will tend to be characteristic of the groups which survive and which therefore perpetuate those practices. Some cultural practices may therefore be said to have survival value, while others are lethal in the genetic sense….
>
> The evolution of cultures appears to follow the pattern of the evolution of species. The many different forms of culture which arise correspond to the "mutations" of genetic theory. Some forms prove to be effective under prevailing circumstances and others not, and the perpetuation of the culture is determined accordingly. When we engage in the deliberate design of a culture, we are, so to speak, generating "mutations" which may speed up the evolutionary process. The effect could be random, but there is also the possibility that such mutations may be especially adapted to survival. (pp. 430, 434)

The hint of selectionism at the social-cultural level that the authors note in *Walden Two* is interesting but not quite as developed as in Skinner (1953).

In between *Science and Human Behavior* in 1953 and "Selection by Consequences" in 1981 were several articles on the biological context of behavior, notably how behavior was a subject matter in its own right that could be studied for its adaptive contributions. Of particular relevance in these articles was selection of innate behavior at the phylogenic level, certainly dispelling the common notion that behaviorists argued all behavior was learned. A review of research activity at the time in the experimental analysis of behavior suggest an upsurge in interest in biological constraints on conditioning. This research challenged the contention that general processes underlay learning, that these processes were recruited purely on the basis of temporal contiguity, and that the specific combinations of stimuli and responses in these instances were not critical. These challenges were widely taken to invalidate any form of a behavioristic approach to psychology. Skinner (1969) commented critically about these challenges as follows:

> No reputable student of animal behavior has ever taken the position "that the animal comes to the laboratory as a virtual *tabula rasa*, and that species differences are insignificant, and that all responses are about equally conditionable to all stimuli." (p. 173)

About Behaviorism in 1974 formalized contingencies of survival at the phylogenic as distinct from contingencies of reinforcement at the ontogenic level, anticipated in Skinner's earlier writings. *Beyond Freedom and Dignity* in 1971 expanded evolution of cultures beyond *Science and Human Behavior*. All culminated in "Selection by Consequences" in 1981.

I am less concerned than the present authors about the role of temporal contiguity and the role of necessity in contingencies. I note that Moxley wrote a series of articles about certain features of Skinner's approach here, one of which the authors cite: Moxley (1999). I believe Moxley's other articles would prove relevant to the authors' argument.

Why then is selectionism important in behavior analysis? I would say for both pragmatic and theoretical matters. From the pragmatic point of view, behavior analysts are interested in the provenance of behavior as well as the variables currently in control of that behavior (e.g., Skinner, 1969, p. 199). More simply, behavior analysts are interested in prediction and control of behavior. If we know the variables that are responsible for the provenance of the behavior in question as well as those currently in control, we are in a position to alter its probability. Consider aggres-

Table 1

Level	Variation	Interaction	Replication
Phylogenic	Genetic	Survival	Transmission via genetics
Ontogenic	Random, uncommitted movements	Reinforcement	Transmission via modified nervous system
Cultural	Cultural practices	Solve problems	Transmission via language

sion. An instance could be a function of relations at any of the three levels. Knowing where to look for relevant variables gives us some leverage to alter its probability.

As an aside, suppose we show a video clip of a pigeon in an operant chamber. The key is dark, but then is illuminated by a green light for 6 s. The pigeon pecks furiously at the key, and when 6 s elapse, we see that food is presented in the hopper below the key. At issue is whether pecking is respondent ("autoshaping"), elicited in virtue of the relation between keylight and food as that relation became relevant at the phylogenic level, or operant, selected in virtue of a fixed-interval 6-s schedule of reinforcement at the ontogenic level. On the basis of only the video clip, it is impossible to tell. One needs to know the provenance of the pecking, which is to say which operations engender the response. It could be engendered by either (a) a stimulus presentation operation in conjunction with a signaling operation, in which case the pecking is respondent; or (b) a consequential operation in conjunction with a signaling operation, in which case the pecking is operant. Skinner's point regarding the operations responsible for the provenance of behavior is critical.

From a theoretical point of view, I turn once again to Skinner's words:

> As a causal mode, selection by consequences was discovered very late in the history of science—indeed, less than a century and a half ago—and it is still not fully recognized or understood…. The facts for which it is responsible have been forced into the causal pattern of classical mechanics, and many of the explanatory schemes elaborated in the process must now be discarded. (Skinner in Catania & Harnad, 1988, p. 15)

The important words in this passage are "the causal pattern of classical mechanics." Here, it seems to me that Skinner explicitly rejected the notion of antecedent mechanistic causation, in favor of selection. As noted earlier in the present comments, ironically Tolman's use of intervening variables (or perhaps more accurately, hypothetical constructs) in his explanatory schemes constituted a commitment to antecedent mechanistic causation. As a result, neobehaviorists institutionalized antecedent mental causes as they sought to explain the behavior of their subjects and participants, and an epistemological dualism as they sought to explain their own scientific behavior. This tradition clearly persists in contemporary mentalistic psychology.

In summary, Skinner suggested selection consists of an ongoing repetitive cycle of variation, interaction with the environment, and replication, understood as transmission to and expression in the future. The cycle plays out at three levels: phylogenic, ontogenic, and cultural. The table below summarizes these relations.

I agree with the authors that the form of selectionism we now attribute to Skinner emerged over time rather than early in Skinner's writing, although much of Skinner's early writing did lay a foundation for selectionism as a causal mode in the analysis of behavior.

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