Revista Mexicana de Análisis de la Conducta • Mexican Journal of Behavior Analysis
2018 | Núm. 2 • diciembre | No. 2 • December | Vol. 44, 261-269
http://dx.doi.org/10.5514/rmac.v44.i2.68545

SELECTION BY CONSEQUENCES, CAUSALITY AND ESSENTIALISM: COMMENTS ON LEÃO AND NETO

SELECCIÓN POR CONSECUENCIAS, CAUSALIDAD Y ESENCIALISMO: COMENTARIOS SOBRE LEÃO Y NETO

José E. Burgos
University of Guadalajara

The target paper (Leão & Neto, 2018; "L&N" henceforth), if I understood correctly, is a conceptual-historical analysis that downplays the possibility of an incoherence in Skinner's early thought between his view of causation and an implicit, incipient form of his thesis of selection by consequences (*SbC*) he allegedly held at the time. According to *SbC*, operant conditioning is literally a process of evolution by selection at the ontogenetic level[1]. L&N also present their analysis as an

Correspondence concerning this article should be addressed to Centro de Estudios e Investigaciones en Comportamiento, Universidad de Guadalajara. Av. Fco. De Quevedo 180, Arcos Vallarta, Guadalajara, Jalisco 44130, Mexico. jburgos@cucba.udg.mx, http://www.ceic.cucba.udg.mx/. I thank Kennon A. Lattal for his kind invitation to write this commentary.

[1] Contrary to appearances, I have never pursued *SbC*, as it was tailored for operant conditioning (indicated by the *C*, which refers to consequences of *responding*), Pavlovian conditioning be damned. This exclusion of Pavlovian conditioning is based on a sharp operant-Pavlovian separation, which was and still is untenable in view of ample evidence that both types of conditioning interact in significant ways. What I once pursued over 20 years ago for about a decade was an extension of *SbC* to Pavlovian conditioning called "selection by reinforcement" (*SbR*), where "reinforcement" referred to *both*, Pavlovian *and* operant conditioning. Not anymore. As I recount elsewhere (Burgos, in press), I realized that *SbR* suffered from the same flaws as *SbC*. I also realized that a neural-network model initially framed within *SbR* (Donahoe, Burgos, & Palmer, 1993) did not need *SbR* at all. I still pursue the model, sans *SbR*.

attempt to clarify the early development of Skinnerian psychology. The paper is thought-provoking and discussions of the topic can help clarify some issues.

The possibility of an incoherence, L&N assert, arises from the fact that Skinner (1931) defined the reflex as a *necessary* relation (p. 446), which allegedly carries a commitment to a mechanistic view of causality. This view, L&N and others claim, following Skinner (1981), is incompatible with the thesis of *SbC*. According to Skinner (1981), *SbC* "is a causal mode found only in living things" (p. 503) that "replaces explanations based on the causal modes of classical mechanics" (p. 501). At the time of his 1931, he also viewed the operant as a reflex (which, of course, he changed later on)[2]. All this leaves his early writings open to the accusation of incoherence, which seems to the main concern of the target paper.

No need to worry, though, at least about such an accusation, or so L&N argue. They provide textual evidence of a transition in Skinner's thought from a mechanistic to a selectionist view of causality. Scharff (1982) made a similar point in her analysis: Skinner shifted from "necessitarian" (mechanistic) to probabilistic causality, and abandoned the view of operants as reflexes (but see Note 2). These transitions suffice to refute, or at least downplay, any accusation of incoherence against Skinner's early thought. By definition, a transition is an indeterminate, noncommittal intermediate state that does not lend itself to a definite accusation of incoherence between strongly-held, well-defined positions.

Still, there are good reasons to believe no incoherence existed *to begin with* in Skinner's early thought between a full (even if momentary) commitment to mechanistic causality and any implicit, incipient form of the thesis of *SbC* he might have held at the time. Moreover, Skinner's shift to probabilistic causation pointed out by Scharff (1982) was not required to ensure coherence with the idea of *SbC*. I am saying that the claim of an incompatibility between mechanistic causality, the

---

[2] In speaking of "the reflex," Skinner (1931) was ambiguous. The definition of the reflex as a necessary relation refers to *unconditioned* reflexes. It would be patently mistaken, almost oxymoronic, to define the *conditioned* reflex as a necessary relation. However, in viewing the operant as a reflex, Skinner must have meant that the operant was a conditioned reflex, even if of a different kind than the classical, Pavlovian conditioned reflex. If this interpretation is correct, his early commitment to a mechanistic view of causality was specifically about unconditioned reflexes, not the operant as a conditioned reflex (or even classical Pavlovian conditioned reflexes). The implication is that operant conditioning calls for a different (e.g., probabilistic) view of causality (but so does Pavlovian conditioning). No incoherence there.

Darwinian theory of phylogenetic evolution by natural selection and, to this extent, operant conditioning viewed as *SbC*, is mistaken. Let me explain.

As I said in the concluding remarks somewhere else (Burgos, in press), no defender of *SbC* has clarified exactly what is the causal mode of *SbC* or classical mechanics, or why the latter is inadequate for *SbC*. In what follows, I assume that by "classical" Skinner meant "Newtonian" (if he did not, I have no idea what he meant, in which case, I have nothing else to say about the matter; if he meant "Cartesian," his rejection of the causal mode in "classical" mechanics was vastly outdated). I also assume that he accepted the conventional (but, as I argue below, mistaken) wisdom according to which Newtonian mechanics commits us to the causal mode of *efficient* causation. If both assumptions are correct, the case for *SbC* hinges on a rejection of Newtonian mechanics as inadequate for a science of behavior (and evolutionary biology), because the former commits us to efficient causation and this is an inadequate causal mode for a science of behavior (or evolutionary theory). As usual, however, the matter is much more complicated.

For a start, Newtonian mechanics is entirely compatible with probability theory[3]. The combination of the two with classical thermodynamics gave rise to (non-quantum) statistical mechanics, a foundation of modern physics. Thus, there is no such opposition between mechanistic and probabilistic causality. Let us not confuse mechanistic with strictly deterministic causality where the cause *always* brings about the effect. The existence of probabilistic machines clearly illustrates the compatibility: Something can be a machine without being strictly deterministic. The statistical nature of evolution by natural selection and operant conditioning, then, is no excuse to repudiate explanations of them by mechanistic causality.

An example of a Newtonian-mechanistic explanation of natural selection with probability considerations is Sober's (1984) interpretation of natural selection as a Newtonian *force* that acts upon evolution, viewed as a change in the gene frequencies of a population[4]. Mutation, migration (both analogous to gravity), and genetic

3 Newton clearly thought much about probability, as shown by his various writings on the topic, including his famous correspondence in 1693 with Samuel Pepys on the Newton-Pepys problem. Stigler (2006) has interpreted Newton's proposed solution as indicating a probabilistic view of nature (see also Sheinyn, 1971).

4 Sober takes this view more as broad, useful practical guideline than a strong theoretical principle. He admits the possibility of evolution without change in gene frequencies (e.g., purely phenotypic and cultural evolution due only to environmental changes). Hence, changes in gene

drift (analogous to Brownian motion) are others. Sober (1984) thus proposed that the theory of evolution consisted of three kinds of laws: A zero-force law (which describes how evolution occurs in the absence of any forces, natural selection included), source laws (which describes how the forces of evolution, e.g., natural selection, are produced), and consequence laws (which describe how forces, if present, drive evolution, i.e., the effects of the forces).

According to Sober (1984), the Hardy-Weinberg equilibrium is the zero-law of the theory of evolution, as it describes (final, steady-state) gene frequencies as a result only of allele frequencies, without the effect of any force[5]. The law can be adjusted to take into account the presence of these forces, either in isolation, or in composition. Sober (1984) formulates all this purely in terms of Newtonian force vectors, where such forces push gene frequencies into particular directions, very much in the same way that forces in Newtonian mechanics push particles and deviates them from a straight line. The interpretation is coherent, very clear and precise, and allows for a more rigorous articulation of the theory of evolution.

Equally erroneous is the suggestion that operant conditioning is unexplainable by mechanistic causality. The theory of behavioral momentum (Nevin, Mandel, & Atak, 1983) uses Newtonian mechanics to explain resistance to change in operant conditioning in terms of momentum as the product of mass and velocity. Killeen (1992) has also explained operant conditioning in terms of Newtonian forces, within a behavioral selectionist framework! Such efforts show that operant conditioning qua *SbC* is explainable in terms of Newtonian mechanics. This outcome is at odds with Skinner's (1981) recommendation that the causal mode of *SbC* replaces the causal mode of Newtonian mechanics, a recommendation that suggests an incompatibility between the two, but perhaps I am missing something, so I must delve deeper.

As I mentioned, conventional wisdom has it that the causal mode of Newtonian mechanics is efficient causation, the mode that comes to mind most naturally in relation to motion by direct collision. Efficient causation is incompatible with Skinner's (1981) thesis of *SbC* in that efficient causation is a strictly deterministic and, hence, necessary relation, of the sort observed in unconditioned reflexes, but

---

frequencies are not necessary for evolution, although they may well be sufficient (if gene frequencies change, then there is evolution).

5 There can thus be changes in gene frequencies and, to this extent, evolution, without selection. Selection, therefore, is not necessary for evolution. Whether selection is necessary for evolution without changes in gene frequencies is less obvious.

not in emitted responding in operant conditioning, the focus of *SbC* (see Note 2). However, this interpretation of the causal mode of Newtonian mechanics as efficient causation is moot. There is no evidence that Newton reduced all Aristotelian causes to efficient, and some have argued that he did not. According to Faulconer (1995), Newton, influenced by Francis Bacon (1561–1626), viewed the objects of science as *formal* causes:

> The essence of the two causes Newton accepts, mechanical and first, is that they are both formal causes: each is the law of a science, a formal description of the origin of motions. ... In being mathematical, the mechanical is formal. Even though the word *mechanical* suggests to us strongly that Newton's physics is a matter of materialistic determination, that suggestion is misleading. Newton's physics is mechanical because it deals with force; the word *mechanical* is a technical term in Newtonian science: to be a mechanical cause is to be a force that is mathematically describable. ... Though it is common to think of Newtonian causation as efficient causation and to say that modern science reduced Aristotle's four causes to two, material and efficient, it is more accurate to say that Newton reduces Aristotle's four causes to one, formal cause (p. 81).

This interpretation could still support Skinner's (1981) suggestion that the causal mode of classical mechanics is incompatible with the causal mode of *SbC*, but in a different way. This possibility leads to the topic of essentialism, over which there is much confusion in the behavioral selectionism literature. Unfortunately, L&N perpetuate the confusion.

The possibility in question arises from interpreting Aristotelian formal causes as *essences* (a standard albeit not the only interpretation), which makes Newtonian mechanics essentialistic. According to Palmer and Donahoe (1992), however, whom L&N cite to support their analysis, the thesis of *SbC*, as a form of selectionism, is anti-essentialistic. This position would seem to be at odds with a Newtonian treatments of evolution and conditioning. Fortunately, the opposition is only apparent and can be easily dispelled in favor of my initial conclusion that Newtonian mechanics is entirely compatible with *SbC*.

One key clarification is that the only essentialistic thesis rejected in population thinking in evolutionary theory is the Aristotelian (typological) concept of species, according to which they themselves *are* essences (e.g., Mayr, 2001). The problem with this concept is clear: Essences do not change[6], but species do; therefore, species cannot *be* essences. However, Sober's (1984) Newtonian-mechanistic reconstruction of evolutionary theory in no way implies that concept of species. The former is entirely compatible with a rejection of the latter.

Still, some might retort that the essentialistic character of Newtonian mechanics prevents it from describing change, which is key (essential?) to evolution and operant conditioning. Such an objection, however, would be equally mistaken. Newtonian mechanics is all about change. Its method of fluxions and fluents (nowadays known as calculus) was designed to describe change mathematically. A dynamical function *f* is a mathematical description of how a certain magnitude *y* (the dependent variable) changes in time (*x*, the independent variable). The derivative of a dynamical function ($\frac{dx}{dt}$), defined as the slope of the tangent line at a point of the function, is a measure of the instantaneous rate of change at that point.

How can Newtonian mechanics describe change *and* be essentialistic, if essences are unchangeable? The answer is that what is unchangeable in Newtonian mechanics are the *laws* of motion, which constitute the essence of motion. The Second Law asserts that $F = ma$ (F denotes "force," m "constant mass," and a "acceleration"). This law is unchangeable, even if it describes change (a is a measure of the rate of change of velocity, the second derivative of position as a function of time). This essentialistic character of Newtoninan mechanics is thus entirely compatible with change.

An equation that describes changes in gene frequencies from one generation to the next due to the effects of migration, mutation, and/or selection is unchangeable in the same sense: The equation *itself* is does not change, even if it describes change. The very *same* equation is used to calculate different rates of change, which implies that the same *law* is at work: The law *itself* (just like the three laws of motion in Newtonian mechanics) does not change. In the case of operant conditioning, a mathematical description of resistance to change in the behavioral momentum theory is unchangeable in the same sense: The description itself does not change; what changes is its various numerical solutions. And so on.

This form of essentialistic thinking, where immutable laws are viewed as essential to dynamical phenomena, is fully compatible with change and population thinking in evolutionary theory. There is no incoherence in speaking of the immu-

6 Such unchangeable character has less to do with their being essences and more with their being *types* or *kinds*, more traditionally known as universals. Redness (exemplified or possessed by red things) has been, is, and will always be redness, regardless of whether it is essential or accidental.

tability of laws that determine rates of change as essential to dynamical phenomena or processes such as evolution and operant conditioning qua *SbC*. These forms of essentialistic thinking do not entail the essentialistic (typological, Aristotelian) concept of species.

There is yet another form of essentialistic thinking more directly related to the concept of species, but does not imply the typological concept of species either and, hence, is compatible with evolutionary theory: The notion that species *have* essences or essential properties[7]. One thing is to say that species *are* essences (the typological view, untenable under evolutionary theory), quite another that species *have* essences. Having essential properties is entirely compatible with change in ways that satisfy the theory of evolution by natural selection. In particular, the formation of a new species, known as speciation, is the coming into existence of an individual with certain essential characteristics that remain fixed while the individual changes.

All of this allows for meaningful talk of the *same* species through time, underlying uses of biological taxonomy. *Homo sapiens sapiens* has been the same species since it formed about 315,000 years ago, even if it can be said to have evolved and still is evolving in a standard sense (e.g., changing of gene frequencies of its population). We do not say that it is a different species just because it has evolved. Only in speciation can we correctly say that a new species evolved, but this is not the only sense in which species can be said to evolve. What allows such talk to make sense is the assumption that our species has some essential properties that remain (roughly) fixed through time, and which define it. Exactly what they are, of course, remains a matter of debate, but many biologists consider bipedalism, opposable thumbs, an articulate language (often viewed as an *exaptation*), and a certain genotype as defining (most likely fuzzily) of our species.

All in all, then, the only form of essentialistic thinking that is incompatible with evolutionary theory is the typological concept of species. Other, more widespread forms (viewing laws as essential to phenomena, defining species in terms of essential properties) are compatible with evolutionary theory. I thus stand by my initial

---

7 This notion is compatible with the view of species as *individuals*, very popular in the philosophy of biology (e.g., Ghiselin, 1974; Hull, 1978) and among behavioral selectionists who have extended to operants (e.g., Baum, 2017; Glenn, Ellis, & Greenspoon, 1992). The technical clarification here is that an essential property of a species as an individual is not a kind but a *haecceity*, the "thisness" or quiddity that makes a particular species the unique individual it is.

conclusion: Skinner (1981) was mistaken in suggesting that the causal mode of classical mechanics was incompatible with (to be replaced by) the causal mode of *SbC*.

But all this is much ado about nothing if intended to support *SbC*. The target paper, albeit not strictly a defense of *SbC*, has not changed my mind about *SbC* (see Note 1). Nor has a recent defense (Stahlman & Leising, in press). If I am right in my rejection, L&N's analysis is about how a blunder came to be, an important contribution. Conceptual-historical analyses of the phlogiston, the luminiferous aether, geocentrism, and flatearthism can be very valuable, especially to identify where the faulty steps were taken, as morals to prevent us from repeating them.

References

Burgos, J. E. (in press; online first publication, February 3, 2018). Selection by reinforcement: A critical reappraisal. *Behavioural Processes*. Retrieved from https://doi.org/10.1016/j.beproc.2018.01.019

Baum, W. M. (2017). Behavior analysis, Darwinian evolutionary processes, and the diversity of human behavior. In M. Tibayrenc & F. J. Ayala (Eds.), *On Human Nature: Biology, Psychology, Ethics, Politics, and Religion* (pp. 397–415). London: Academic Press. Retrieved from https://doi.org/10.1016/B978-0-12-420190-3.00024-7

Donahoe, J. W., Burgos, J. E., Palmer, D. C. (1993). A selectionist approach to reinforcement. *Journal of the Experimental Analysis of Behavior*, *60*, 17–40. Retrieved from http://dx.doi.org/10.1901/jeab.1993.60-17

Faulconer, J. E. (1995). Newton, science, and causation. *The Journal of Mind and Behavior*, *16*, 77 – 86. Retrieved from https://www.jstor.org/stable/43853669

Glenn, S. S., Ellis, J., & Greenspoon, J. (1992). On the revolutionary nature of the operant as a unit of behavioral selection. *American Psychologist, 47*(11), 1329-1336. Retrieved from http://dx.doi.org/10.1037/0003-066X.47.11.1329

Ghiselin, M. (1974). A radical solution to the species problem. *Systematic Zoology*, *23*, 536–554. Retrived from https://www.jstor.org/stable/2412471

Hull, D. L. (1978). A matter of individuality. *Philosophy of Science*, *45*, 335–360. Retrieved from https://doi.org/10.1086/288811

Killeen, P. R. (1992). Mechanics of the animate. *Journal of the Experimental Analysis of Behavior*, *57*, 429–463. Retrieved from https://dx.doi.org/10.1901%2Fjeab.1992.57-429

Leão, M. F. F. C., & Neto, M. B. C. (2018). Successive approximations to selectionism: Skinner's framework for behavior in the 1930s and 1940s. *Mexican Journal of Behavior Analysis, 44,* 1-24.

Mayr, E. (2001). *What evolution is.* London: Phoenix.

Nevin, J. A., Mandell, C., & Atak, J. R. (1983). The analysis of behavioral momentum. *Journal of the Experimental Analysis of Behavior, 39,* 49–59. Retrieved from https://doi.org/10.1901/jeab.1983.39-49

Palmer, D. C., & Donahoe, J. W. (1992). Essentialism and selectionism in cognitive science and behavior analysis. *American Psychologist, 47,* 1344–1358. Retrieved from http://psycnet.apa.org/doi/10.1037/0003-066X.47.11.1344

Rachlin, H. (1002). Teleological behaviorism. *American Psychologist, 47,* 1371–1382. Retrieved from http://psycnet.apa.org/doi/10.1037/0003-066X.47.11.1371

Scharff, J. L. (1982). Skinner's concept of the operant: From necessitarian to probabilistic causality. *Behaviorism, 10,* 45–54. Retrieved from https://www.jstor.org/stable/27758994

Skinner, B. F. (1931). The concept of the reflex in the description of behavior. *The Journal of General Psychology, 5,* 427–445. Retrieved from https://doi.org/10.1080/00221309.1931.9918416

Skinner, B. F (1981). Selection by consequences. *Science, 213,* 501–504. Retrieved from http://dx.doi.org/10.1126/science.7244649

Sober, E. (1984). *The nature of selection.* Chicago, IL: The University of Chicago Press.

Stahlman, W. D., & Leising, K. J. (in press; online first publication, March 19, 2018). The coelacanth still lives: Bringing selection back to the fore in a science of behavior. *American Psychologist.* Retrieved from http://dx.doi.org/10.1037/amp0000261.

Sheynin, O. B. (1971). Newton and the classical theory of probability. *Archive for History of Exact Sciences, 7,* 217–243. Retrieved from https://doi.org/10.1007/BF00357217

Stigler, S. M. (2006). Newton as a probabilist. *Statistical Science, 21,* 400–403. Retrieved from https://projecteuclid.org/euclid.ss/1166642443