The indispensability of laws in cognitive science

La indispensabilidad de las leyes en ciencias cognitivas

Sergio Daniel Barberis Almirón[*]
University of Buenos Aires, Buenos Aires, Argentina
sbarberis@filo.uba.ar
Orcid code: https://orcid.org/0000-0002-5381-3772

Abstract

Endorsing the philosophical distinction between laws of science and laws of nature, the present paper advocates for the explanatory indispensability of the laws of science in the field of the cognitive sciences. It is argued here that laws of science play an indispensable epistemic role both for functional analyses and mechanistic explanations of cognitive capacities. In this way, the paper provides a plausible explication of the explanatory power of the cognitive sciences while wisely bracketing the controversial metaphysical status of natural laws. It is argued that both the advocates and the detractors of intentional causal laws presuppose that those laws contribute neither to functional nor mechanistic explanations of target phenomena. However, the present paper shows, first, that functional analysis requires the specification of non-causal, scientific laws, and second, that the precise scientific representation of the activities and the dynamical organization of some mechanism is generally deployed, in the context of a mechanistic model, by specifying scientific laws. The conclusion is that the laws of science (but not necessarily the laws of nature) play an indispensable role in cognitive scientific explanations.

Keywords

Cognitivism, explanation, law, mechanism, dynamics, function.

* Doctor of Philosophy (University of Buenos Aires). Assistant Researcher of the National Council for Scientific and Technical Research (CONICET), at the Institute of Studies on Science and Technology, Department of Social Sciences, National University of Quilmes. Head of Practical Works of the subjects 'Philosophy of Sciences', 'History of Science', 'Metaphysics' and 'Philosophy of the mind' in the Department of Philosophy (UBA). Director of the UBACyT project: 'Conceptual reconstructions in biological sciences: didactic applications and social implications'. Author of publications in specialized journals on the philosophy of cognitive science and philosophy and history of neuroscience.

Resumen

Partiendo de la distinción filosófica entre las leyes de la ciencia y las leyes de la naturaleza, en el presente artículo se defiende la indispensabilidad explicativa de las leyes de la ciencia en el campo de las ciencias cognitivas. Se sostiene que las leyes de la ciencia desempeñan un papel epistémico indispensable tanto en el análisis funcional como en la explicación mecanicista de las capacidades cognitivas. De esta manera, se ofrece una elucidación plausible del poder explicativo de las ciencias cognitivas en términos del papel epistémico de las leyes de la ciencia, suspendiendo el juicio, de manera prudente, respecto del controvertido estatus metafísico de las leyes naturales. Se pone en evidencia que tanto quienes defienden como quienes rechazan el compromiso ontológico con leyes causales intencionales presuponen que esas leyes no contribuyen a la explicación funcionalista ni mecanicista de los fenómenos que describen. Sin embargo, en el presente trabajo se argumenta, primero, que el análisis funcional requiere la especificación de leyes científicas no causales y, segundo, que la representación científica precisa de las actividades y de la organización dinámica de un mecanismo se despliega mayoritariamente, en el contexto de un modelo mecanicista, mediante la especificación de leyes científicas. La conclusión es que las leyes científicas (aunque no necesariamente las leyes de la naturaleza) desempeñan un papel indispensable en la explicación en ciencias cognitivas.

Palabras clave

Cognitivismo, explicación, ley, mecanismo, dinámica, función.

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Introduction

Starting from the philosophical distinction between the laws of science and the laws of nature, this article defends the explanatory requisiteness of the laws of science in the particular field of cognitive sciences. According to van Fraassen, (1989), Swartz (1995), and Giere (1999), there is a very important conceptual distinction, often overlooked, between the 'laws of nature' or 'physical laws', on the one hand, and the 'laws of science' or 'laws of models', on the other. The laws of nature are those empirical regularities that govern the natural world, regardless of the scientific representations that are made of them; the laws of science are those basic principles that are constitutive of the theories of which they are part and that structure a good part of scientific practice, as described by Lorenzano (2007). In this article, an elucidation of the explanatory power of the cognitive sciences in terms of the laws of science is defended, maintaining certain neutrality regarding the relationship between the latter and the laws of nature. In other words, it is proposed to approach the explanation in cognitive sciences as an epistemic phenomenon, suspending the judgment regarding the metaphysical status of natural laws.

The idea of the explanatory indispensability of laws is classical in the philosophy of science, it belongs to the canon of logical empiricism. With the resurgence of mechanistic philosophy, in the 2000s, several mechanistic philosophers (or close to mechanistic philosophy) rejected the classic canon of the epistemic indispensableness of the laws of science.

Authors such as Bechtel and Abrahamsen (2005), Craver (2007), Kaplan and Craver (2011) have affirmed, with greater or lesser vehemence, that scientific laws do not play any explanatory role in the functional/mechanistic models of a certain cognitive capacity.

The arguments, if successful, show that there are no laws of nature in the biological and cognitive realms since in these realms there are only contingent and mechanistically fragile regularities. Mechanistic philosophers such as Machamer, Darden, and Craver (2000), Bechtel and Abrahamsen (2005), and Craver (2007) oppose the thesis according to which scientific explanation requires the postulation of laws of nature governing each of the activities and the organization inside a mechanism. However, these arguments leave open the possibility that the precise scientific representation of the activities and of the dynamic organization of a mechanism, in the context of a mechanistic model, is deployed mainly through the specification of scientific principles or laws and that, furthermore, it is the latter that bear a large part of the explanatory burden.

This document is structured in three sections. The first section reconstructs the debate in the philosophy of mind and the cognitive sciences about the existence and nature of intentional causal laws. The second section defends the thesis that functional analysis, as an explanatory guideline, is only incompatible with the relevance of causal laws, but not with the relevance of scientific laws in general. In the third section, an argument is developed for the epistemic requisiteness of scientific laws in mechanistic explanation. In the end, the main conclusions of the research are presented.

The debate on intentional causal laws

In the 1990s, the debate about the status of laws in cognitive science was framed in a broader metaphysical discussion about the plausibility of 'intentional realism'. Intentional realism sets out to reconcile two metaphysical theses in the philosophy of mind. On the one hand, intentional realists affirm that there are mental states with semantically evaluable content; that these mental states are semantically evaluable means that they have conditions of satisfaction. For example, the content of Sofia's belief that snow is white will be true or false according to the color of snow in the world. On the other hand, as Skidelsky (2003) points out, the intentional realist holds the thesis that such intentional states have a causal role in the production of behavior and other intentional states.

Jerry Fodor (1991; 1994), in his claim to intentional realism, argues for the causal relevance of intentional states by defending the thesis that there are 'causal intentional laws' that subsume intentional properties (i.e., be semantically evaluable) such as:

> Typically, intentional generalizations could be of the form: "If you want to, and you think you can't unless you do, then, *ceteris paribus*, you will try to do." For example, if you want to make an omelet, and you think you can't do it unless you break some eggs, then, all things being equal, you will try to do an action that is breaking the eggs (Fodor, 1994, p. 4).

The clause *ceteris paribus* that is mentioned in the quote is indispensable for the formulation of the law since, otherwise, the alleged law would be false. Returning to the example of Fodor, it could be the case that a person, due to some neuropsychological impediment in the integration of information, does not attempt any action that is the breaking of eggs even though he wants to make an omelet and believes that he cannot do it unless he breaks some eggs. All intentional generalizations allow exceptions of this kind. The incorporation of *ceteris paribus* clauses in the formulation of scientific laws, however, is a frequent source of philosophical malaise, as it threatens to trivialize the content of the law. Fodor considers that the inclusion of such clauses in the causal intentional laws (and in the laws of the special sciences in general) is not problematic, since it is possible to complete the antecedent of the statement of the law by specifying the perturbing conditions subsumed under the *ceteris paribus* clause using the concepts of some more basic theory:

> Exceptions to the generalizations of a special science are typically inexplicable from the point of view of (that is, in the vocabulary of) that science. That's one of the things that makes it a special science. But, of course, it may be perfectly possible to explain those exceptions in the vocabulary of some other science (…) or we [n]o have reason to doubt that [the clauses *ceteris paribus*] can be downloaded into the vocabulary of some lower-level science (let's say neurology, biochemistry, in the worst case, physics) (Fodor, 1987, p. 6).

An intentional causal law of the type envisioned by Fodor would have the form: "*Ms* cause *B ceteris paribus*," where *M* is a psychological, mental, or intentional property. The description of the antecedent *M* in the vocabulary of some relatively more basic theory is called by Fodor a 'realizer' of *M*. Since *M* is a (functional) psychological property, those who participate in the debate accept that it is a multiply realizable property: each functional property can be realized in different physical pro-

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perties $R_1, ..., R_n$. Each of these physical properties cannot cause B by itself; In each case, it is essential to add some physical condition C that, together with the first, is sufficient to produce B.

Fodor (1991) defines that condition C is a 'completer' relative to the realization of M through the physical property if and only if:

i. R_i and C are strictly sufficient for B;
ii. R_i, considered in isolation, it is not strictly sufficient for B;
iii. C, considered in isolation, is not strictly sufficient for B

Given this terminology, the law 'M cause B ceteris paribus' is true if and only if, for each performer R_i of M, there is a completer C_i such that R_i & C_i cause B. In frank opposition to Fodor's position, Schiffer (1991) considers that, even when statements of the type "The M cause B *ceteris paribus*" express true propositions, they do not refer to properly psychological natural laws. On the one hand, it is not about psychological laws because the right side of the biconditional: 'For each performer R_i of M, there is a completer C_i such that R_i & C_i cause B' implies that the condition & that does not require intentional or psychological vocabulary, is nomically sufficient for the occurrence of B. Furthermore, if the phenomenon of the multiple realizability of M is taken into account, it becomes clear that it is nomically possible that there is a R_i realizer of M who has no completer and therefore does not cause B. In other words, the intentional generalizations *ceteris paribus* admit absolute exceptions, so they cannot be true or, if they were, they would be trivially true.

In his response to Schiffer's objection, Fodor (1991) concedes, first of all, that it is nomically possible for a performer R_i of M not to cause B. However, if R_i it is indeed a realizer of M, it is conceptually impossible that it does not instantiate some of the laws of the causal network that define the functional property M. Although R_i it is an absolute exception to 'Los M cause B *ceteris paribus*', it cannot constitute an absolute exception to any law of the form 'Los M cause X *ceteris paribus*' that is part of the network that defines the causal role of property M. For Fodor, intentional causal laws are legitimate insofar as they do not have absolute exceptions for the entire network.

Diana Pérez (1995) objects to this argument of Fodor pointing out that, if it is admitted that 'The M cause B *ceteris paribus*' may have at least one absolute exception, that is, that it is nomically possible that a performer R_i does not enter into a causal connection with some instance of B, then it is not clear in what sense the properties M and B are cau-

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sally connected, since not only would there be no constant conjunction between the instances of *M* and *B* (and Fodor already admitted that they are not probabilistic laws) but that the presumed natural law would not offer support for counterfactual statements such as: 'If this were an *M*, it would cause a *B*'.

So far, a brief review has been offered of the problems plaguing the defense, by the staunch intentional realist, of the existence of *ceteris paribus* psychological laws. It should be noted that this is a primarily metaphysical debate about the status of natural psychological laws. Now, in the context of this article, a different question stands out: What epistemic role should be attributed to these intentional laws in the context of explanation in cognitive science?

Both Schiffer (1991) and Fodor (1991) seem interested in generalizations from common sense psychology. In the case of Schiffer (1991), he is interested in generalizations such as:

> If *x* wants *p* and believes that (*p* if *x* does *A*) and *x* has the correct beliefs about how to do *A* and *x* is capable of doing *A* and *x* has no stronger competing wish, then, *ceteris paribus*, *x* does *A*" (p. 11).

Fodor (1991) shares the same interest in generalizations from common sense psychology when he states that "intentional explanations/predictions of common sense are (at least implicitly) and at least occasionally, a kind of explanation/prediction by subsumption under laws" (p. 20).

However, as Fodor (1991) himself points out, the reconstruction of the explanatory guidelines of common-sense psychology is not the only philosophical project that is interested in psychological laws (nor the most important, I may add). When it comes to the structure of explanations in cognitive science, Fodor does not seem to assign intentional laws a fundamental role. I will allow myself to quote a key passage in this regard:

> It is a law that the moon appears larger to us on the horizon than above our heads, and it is an intentional law because it invokes inescapably relationships such as "appearing". Cognitive science seeks a computational explanation for this intentional law; seeks (e.g.) an algorithm that maps proximal visual arrangements and perceptual judgments, in such a way that the kinds of proximal visual arrangements that are caused by looking at the moon when it is close to the horizon reliably facilitate an overestimation of size (...) If there were no contingent and reliable generalizations about the relationships between proximal arrangements and perceptual size judgments - if they were not, in short, intentional laws - then computational models would have nothing to explain (Fodor 1991, p. 20).

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This example from Fodor illustrates, first, how the author oscillates between two different characterizations of the intentional law in question. According to the first, purely *folk* or common sense, characterization, the law establishes that the same objects appear to be larger on the horizon than on the head. The second characterization is cognitive (in particular, computational): the law links (causally) certain arrangements of proximal visual stimuli with certain perceptual estimates of the size of distal objects. It could be thought that they are two different formulations of the same natural law. However, there are good reasons to think that these are two different laws. The first law links certain spatial conditions with the perceptual judgments of an individual; the second law specifies an activity or causal interaction between the 'parts' of a computational subsystem of the individual. Thus, in the terms of Dennett (1969) and Skidelsky and Pérez (2005), while the first is a law at the personal level, the second is a law at the subpersonal level.

Secondly, the quotation highlights the role that Fodor attributes to intentional laws in explanatory patterns, not just common sense, but cognitive science. It is curious that while intentional laws are fundamental from a metaphysical point of view to realism about intentional states, they do not appear to be so from an explanatory point of view. Fodor (1991) states: "the (supposed) intentional laws provide the agenda for computational modeling" (p. 20). If this is the case, then psychological laws are rather the *explanandum* of an explanation in cognitive science, which must be realized and explained through the deployment of computational models.

Fodor seems to be giving too much to the "enemy of psychological laws." In fact, the explanatory irrelevance of *ceteris paribus* laws for explanations in special sciences is part of the main objection that Schiffer (1991) raises against the existence of such laws:

> When I read biology [texts], I have a hard time finding something that looks like an explanation that invokes laws, and I think I know why. Suppose you invented a spring-activated mousetrap and had to explain how it worked. You would say that, when the machine works, it is because a mouse nibbles the cheese placed in a trigger mechanism; the movement caused by the nibbling triggers a bar attached to a stretched spring; etc. But I wouldn't mention any laws. Perhaps if the explanation were to continue through a long enough causal chain, it would arrive at laws; but they would be laws of physics, not laws about mousetrap theory. In the same way, most of biology is concerned with explaining how various mechanisms work —think of the photosynthesis explana-

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tion— and it seems that these explanations do not invoke biological laws, neither strict nor *ceteris paribus* (p. 16).

An unexpected consequence, then, of the controversy over intentional laws seems to be that these laws, even if they exist in the world, do not play any explanatory role in cognitive science, as Skidelsky (2003) points out. In the best of cases, they would offer a new description of the *explanandum* phenomenon. Of course, discovering these intentional laws would not be a trivial job, but even so, they would not carry substantive weight in explaining the phenomena that cognitive scientists set out to explain. This perspective on the explanatory irrelevance of laws in cognitive sciences crystallizes in the canonical conception of cognitivist explanation, namely, the *Functional Analysis* proposal of Robert Cummins (1975; 1983; 2000). This proposal is analyzed in the next section.

Functional analysis and the explanatory relevance of non-causal laws

Especially in the case of cognitive sciences, many philosophers accept Cummins's (2000) proposal according to which the explanatory pattern of cognitive sciences is functional analysis, and that this analysis does not require the identification of scientific laws in the *explanans*. In this section, it is shown that Cummins overestimates the scope of the thesis that he postulates, insofar as his results only apply to the irrelevance of the 'causal laws' for functional analysis. In fact, his more complete presentation of the structure of functional analysis reveals the indispensable (epistemic at least) of other types of 'non-causal scientific laws' for an explanation.

The conclusion of the previous section, largely implicit in the debate of the deliberate law, is explicitly defended by Cummins (2000). According to this philosopher, the explanatory guidelines of the cognitive sciences should not be interpreted in terms of the nomological-deductive conception of the explanation of Hempel (1965). The psychological causal laws do not perform the function of explaining the behavioral or psychological phenomena that they subsume, but, in any case, they redescribe those phenomena in a more general way.

> Does the Law of Effect explain why feeding a pigeon every time it pecks at a lever increases the frequency of pecking? Or does it just repeat the phenomenon in general terms? The correct moral of the story here is that the Law of Effect is an *explanandum*, not an *explanans* (…) In science when a law is thought of as an *explanandum*, it is called an "effect"

(…) Nobody thinks that the McGurk effect explains the data that it sub-sumes. No one who accepts the [nomological-deductive] model would suppose that it can be explained why someone hears a consonant like the one that appears to be pronounced by the mouth that is speaking, by appealing to the McGurk effect. This is the McGurk effect (Cummins, 2000, p. 119).

According to this author, when psychological causal laws are thought of as *explananda*, they are called effects. Just as, for Fodor (1991), intentional laws constituted what to explain through computational mo-dels, Cummins (2000) conceives psychological effects as in situ laws that specify the behavior patterns of different mechanisms. What does it mean that the effects are laws in situ? It is only another way of pointing out that they are not strict natural laws, i.e., laws that apply to all objects in all time and space but specify regular patterns of behavior that only apply to a special kind of system, due to the peculiar constitution and organiza-tion of that system.

According to Cummins (2000), in situ laws would not play an indispensable role in the specification of the *explanandum* either, since what is sought to be explained in cognitive sciences are not limited laws but rather specific capacities of a system: for example, the human capaci-ty to perceive depth, to learn a language, to plan, to predict the future, to understand mental states of others, etc. These capabilities can be thought of as complex dispositional properties of systems. These dispositional properties are usually specified by subjunctive conditional statements of the type: "*x* is soluble in water if and only if (if *x* were put in water, then, *ceteris paribus*, *x* would dissolve in water)" (Cummins 1983, p. 18). This analysis shows that, for a given system, having such or such a dispositio-nal property is satisfying this or that law in situ. It is in this sense that Cummins (2000) states that capabilities and effects are 'close relatives'. However, he is not prepared to argue that a capacity can always be spe-cified by a set of non-strict laws, since many of the regularities that we call 'effects' in psychology are, in fact, incidental to the exercise of some capacity, i.e., mere epiphenomena.

The problem with this argument from Cummins is obvious. The fact that there are laws in situ that describe effects incidental to the normal functioning of a capacity does not mean that those regularities that effec-tively govern normal functioning (and that, therefore, are part of the *ex-planandum* of a cognitive explanation) cannot be collected by laws in situ.

Like Schiffer (1991), Cummins reconstructs the explanatory pat-terns of the special sciences as consisting of the development of scientific

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theories or models about the functional and constitutive structure of the 'mechanisms' that underlie the effects or in situ laws of the systems that are part of the scope of each of these sciences. For this author, at least in his article from (2000), the functional analysis of a system would not require the description of any natural law.

What is the functional analysis of a capacity? According to Cummins (2000), the functional analysis consists of analyzing a complex arrangement of a system in a number of 'less problematic' arrangements, in such a way that the programmed manifestation of these analyzing provisions results in a manifestation of the analyzed arrangement. By 'programmed' he means "organized in such a way that it can be specified in a program or in a flow chart" (Cummins 2000, p. 125). Given this characterization, in principle, a functional analysis of any capacity can be offered. However, the explanatory interest of a functional analysis will be directly proportional to: (i) the degree to which the analytical capacities are less sophisticated than the analyzed capacity; (ii) the degree to which the analyzing capacities are of different types from the type of the analyzed capacity; (iii) the degree of the relative sophistication of the program being appealed, that is, the relative complexity of the organization of the components that are attributed to capacity.

In the simplest cases (for example, in the explanation of how a mounting tape works), the functional analysis of the global arrangement obviously accompanies the component analysis of the system. Functional analysis does not have to be componential; both the analyzed capacity and the analytical capacities can be attributed to the system as a whole. Component analysis, on the other hand, identifies the specific parts of the analyzed system, for example, those that perform the functions identified in the functional analysis. For Cummins (2000), this direct correspondence between concrete structures and functions is absent in the cases of relatively more complex systems, as are the majority of cognitive systems. In the latter, there is no direct correspondence between the identified functions, for example, through a computational model, and the specific parts of the brain studied by neurobiology (see Weiskopf, 2011). It is for this reason that Cummins considers it important to maintain functional analysis and componential analysis as being conceptually distinct.

Cummins (2000) characterization of functional analysis and his reasons for thinking that certain in situ laws can play the role of *explananda* in an explanation in cognitive science have been succinctly presented. Now, what reasons does Cummins offer for thinking that functional analysis does not require the identification of scientific laws?

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It is, at least, striking that, in his 2000 article, he simply takes it for granted that functional analysis does not need scientific laws. In the 1983 book, on the other hand, he is much more cautious and precise in formulating his theses. There he explicitly states that functional analysis does not require the specification of *causal laws*, for example, nomic correlations whose instances are cause-effect pairs (Cummins 1983). However, as he himself is in charge of emphasizing, not all scientific laws are causal laws.

Of particular interest are three types of laws that Cummins (1983) characterizes as non-causal. First, the 'laws of composition', which specify the analysis of a specific type of system; for example, water molecules are made up of two hydrogen atoms bonded to one oxygen atom and the double helix model of DNA. Second, the 'laws of instantiation', which specify how a property is instantiated in a specific type of system; an example of an instantiation law would be given by the Boyle-Mariotte law, according to which the temperature is instantiated in gas as the average kinetic energy of the gas molecules. Finally, the 'nomic attributions', which state that all *x's* have a certain property *P*; for example, the law of gravitation in the theory of general relativity.

Well, according to Cummins (1983), as long as it is clear that not every scientific law is a causal law, it is acceptable to represent the structure of functional analysis by means of a scheme in which non-causal laws essentially participate:

Functional Analysis [FA]

(1) Any system that has the components $C_1,...,C_n$ organized in the *O* manner–i.e., which has the analysis $[C_1,...,C_n,O]$– possesses the property *P*.
(2) *S* has the analysis $[C_1,...,C_n,O]$
(3) Por lo tanto, *S* tiene la propiedad *P*.

The conclusion (3) of the FA scheme is a nomic attribution. Premise (1) includes a law of instantiation (in the sense specified above), while premise (2) includes a law of composition. This alternative presentation of the structure of functional analysis shows that the thesis of the explanatory irrelevance of scientific laws in his article (2000) is presented, at least, in an ambiguous manner. All that the arguments developed there, show is that functional analysis does not require the identification of causal laws, although it does require the identification of (non-causal) laws of instantiation and composition.

What motivations does one have for thinking that the laws of composition are, indeed, scientific laws? A philosophical motivation behind this classification is the following. Complex systems for which a functional analysis can be informative are robust systems, that is, they are systems whose operation and organization is not completely ephemeral or circumstantial, but rather exhibits a characteristic regularity, not necessarily deterministic, which is crucial for the explanation. higher-level in situ laws (those that characterize the behavior of the system as a whole). Therefore, the specifications of the organization of the components of a system are, themselves, legaliformed, as Cummins (1983) points out:

> [A] successful analysis yields an explanatory gain when it allows us to realize that an object that has the specified class of components, organized in the specified way, is bound to have the property that it seeks to explain (p. 17)

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In the case of the status of the instantiation laws, the issue is more complex, according to Cummins, since these laws are derived principles that require, themselves, an explanation. This means that the $[C_1,...,C_n,O]$ analysis cannot provide, on its own, the complete (or definitive) explanation of the nomic attribution contained in FA's conclusion. Specifically, the capabilities $C_1,...,C_n$ must be explained, in turn, by the properties of the real parts of the system, those in which these capabilities are, in turn, instantiated. In other words, the only available guideline to explain an instantiation law is to 'derive' it from the nomic attributions that specify the properties of the components of the system, in terms of Cummins (1983). A full explanation of a capability should exhibit the details of the physical instantiation of the parsing capabilities in the system that contains them. In the special case of cognitive sciences, Cummins (2000) states that:

> Neuroscience enters this image [of explanation] as a source of evidence, arbitrating between [functional analyzes] in competition and, ultimately, as the source of an explanation of the biological realization of functionally described psychological systems (p 135).

In this way, it is seen how different types of non-causal scientific laws (nomic attributions, laws of composition, and laws of instantiation) play a fundamental role in the construction of a complete explanation of the psychological effects. Cummins' proposal does not explicitly distinguish between laws of nature and laws of science, but it does not seem problematic to formulate his thesis in epistemic terms, such as asserting

that certain (non-causal) types of laws or principles of science play an explanatory role in the context of functional analysis.

Mechanism and the Argument of the Epistemic Indispensability of Laws

Certainly, canonical mechanistic philosophers such as Machamer, Darden, and Craver (2000) and Craver (2007) do not consider that the traditional concept of 'strict scientific law' has any use in the reconstruction of explanatory patterns of neurobiology, neurosciences, or cognitive sciences. Very few, if any, of the generalizations that appear in the biological sciences are 'strict', in the same sense in which the laws of Newtonian mechanics were traditionally considered to be. Strict laws are universal, general, they are not vaguely true, they have no exceptions, they are projected and they are nomically necessary, as Leuridan (2010) summarizes. The generalizations of the biological sciences, on the other hand, are limited in scope, stochastic, mechanistically fragile, and historically contingent, a characterization in which Beatty (1995), Weber (2005), and Craver (2007) agree.

Craver (2007) illustrates these characteristics of biological generalizations with an example taken from neuroscience: the phenomenon of long-term potentation (LTP). The phenomenon of LTP can be characterized as an increase in synaptic efficiency in a population of postsynaptic neurons as a result of the application of a sequence of stimuli in presynaptic neurons. In particular, this phenomenon involves: (i) an increase in the amplitude of the excitatory potential of each postsynaptic neuron; (ii) an increase in the amplitude of the potentials of the population as a whole; (iii) a reduction in the latency of such potentials. Well, first of all, the LTP, qua natural regularity, has a limited scope. It is not a feature of all cells, nor of all chemical synapses; it changes from organism to organism, from one brain region to another, etc. Second, it is a stochastic generalization. Today, scientists are able to induce an LTP in about 50% of cases. Third, LTP is mechanistically fragile, its manifestation can vary and even be prevented due to alternations in the stimulus, in the background conditions, or in the components of the underlying mechanism. Lastly, the phenomenon of LTP is physically contingent. It is, as Beatty (1995) asserts, the product of contingencies in the evolutionary history of certain organisms. There was a time when no organism in the world manifested LTP and there is a physically possible world in which no organism ma-

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nifests LTP. Since the aforementioned characteristics are not unique to LTP, but are common to most biological generalizations, the conclusion reached by mechanists, such as Craver (2007), is that 'there are no natural biological laws ', in the sense that the generalizations of biology do not include strict regularities (that is, universal, general, without exceptions, physically necessary, etc.).

Many of the mechanistic philosophers accept this conclusion, namely that there are no natural biological laws. For the same reason, there are no natural cognitive laws either. Do mechanists further argue that generalizations from the biological sciences in general, and from the cognitive sciences in particular, play no central or indispensable epistemic/explanatory role in the scientific models in which they appear?

Leuridan (2010) interprets mechanists as holding that neither scientific laws nor natural regularities explain why a certain phenomenon occurs. There is no lack of textual evidence for this interpretation. In the founding article on mechanism, Machamer Darden and Craver (2000) argue, not only that the notion of strict law of nature has no application in biology, but that the concept of 'activity' can perform all epistemic/explanatory functions traditionally attributed to scientific laws. In the proposed analysis of these authors, a mechanism is a complex of entities and activities organized in such a way that they are productive of regular changes from starting conditions to termination conditions (Machamer, Darden, and Craver, 2000). Entities are the parts or components in mechanisms. They have properties that allow them to be involved in a variety of activities; Typically, such entities possess location, size, structure, and orientation. In molecular biology and neurobiology, the hierarchy of mechanisms 'bottoms out' in describing the activities of entities such as macromolecules, smaller molecules, and ions. Activities are the causal components in mechanisms. The activities in which the hierarchy of mechanisms bottoms out can be classified, according to Machamer Darden and Craver (2000), into at least four types: geometric-mechanical, electrochemical, energetic (thermodynamic), and electromagnetic activities. Finally, the entities and the activities of the mechanisms present a certain spatial and dynamic organization that guarantees the productive continuity of the mechanism. At this point, the authors ask themselves: is the specification of scientific laws indispensable for the characterization of the activities of a mechanism? The answer that Machamer Darden and Craver (2000) offer in this text is clearly negative:

> Sometimes the regularities of activities can be described by laws. Other times they can't. For example, Ohm's law can be used to describe aspects

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of the activities in the neurotransmission mechanism. But there are no laws that describe the regularities of binding of proteins to regions of DNA. However, the notion of activity carries some of the characteristic features associated with laws. Laws are considered as determinate regularities. They describe something that acts in the same way under the same conditions: i.e., same cause, same effect. The same can be said of the mechanisms and their activities. A mechanism is a series of entity activities that produce termination conditions on a regular basis. These regularities are not accidental and support counterfactuals insofar as they describe activities (...) There is no philosophical advance postulating an additional entity, a law, as underlying the productivity of the activities (p. 7-8).

Apparently, for these authors, not only the notion of strict natural law, but also the more general notion of a scientific law is dispensable in the characterization of mechanistic models. In the first place, they seem to suggest that every scientific law describes the activity or capacity of some kind of entity, whether of a system or a component of a system. Second, not every activity has a corresponding description in terms of some scientific law (the example they provide is that of the linkage process between DNA and a protein). Third, all those traits or virtues traditionally attributed to scientific laws —in particular, natural necessity and the support of counterfactuals— can be directly attributed to the activities coincidentally constitutive of a mechanism. Therefore, it is the activities of the component parts of the mechanism that carry the genuine explanatory weight. Such statements appear even in more recent contributions to the mechanistic literature. Thus, Kaplan and Craver (2011) emphasize that:

> (…) Mechanism models frequently involve mathematical descriptions of the causal relationships in a mechanism. This gives the erroneous impression that the explanation in such cases involves subsumption under generalizations and that it is the generalization that is performing the explanatory task. In fact, however, the generalizations are explanatory because they describe the causal relationships that produce, underlie, or maintain the *explanandum* phenomenon (p. 612).

Next, it is argued that, from the mechanistic philosopher's own point of view, there are good reasons to reject the thesis of the epistemic/explanatory irrelevance of scientific laws in the characterization of mechanisms. An argument is put forward that stresses two crucial premises based, first, on the criteria for distinguishing between relatively unproblematic descriptions of activities, on the one hand, and so-called 'filler terms', on the other, and, second, in the demand for quantification of the

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dynamic principles of the organization of mechanisms. This argument can be outlined as follows:

Argument of epistemic necessity [AEN]

1. A complete mechanistic model requires the specification of the activities and the organization of the mechanism that underlies this phenomenon.
2. Scientific laws are indispensable for the specification of the nonproblematic activities of a mechanism.
3. Scientific laws are indispensable for the specification of the dynamic organization of a mechanism.
4. Therefore, scientific laws are indispensable for the specification of a complete mechanistic model.

The AEN-2 premise is based on the conceptual distinction between 'sketches' of mechanisms, at one extreme, and models that offer an ideally complete description of a mechanism, at the other extreme, as elucidated by Craver (2006; 2007) and Bechtel and Abrahamsen (2005). A sketch of a mechanism is an incomplete model of a mechanism. While some of the terms or parameters that appear in the model characterize specific parts of the mechanism or their corresponding activities, others are mere 'filler terms'. Some of the most common filler terms for activities are cause', 'activate', 'encode', 'inhibit' or 'render'. These terms indicate some part or activity postulated in the mechanism, but "do not offer any details about how that activity is carried out" (Craver, 2006, p. 360). The limit case of mechanism sketch is constituted by the 'phenomenal models'. In a phenomenal model, no parameter represents some part or activity of the mechanism underlying the phenomenon. At the other pole of the classification are ideally complete descriptions of a mechanism. In these models, the terms or parameters represent all and only the parts and activities that are explanatory relevant for each aspect of the *explanandum* phenomenon. Of course, the everyday life of modeling in science occurs at the intermediate points of this continuum; there are the 'mechanism schemes', for example, those models that represent some components of the mechanism, but omit other components, either because they are irrelevant in the context of certain specific explanatory demands, or whose features are unknown structural, functional or dynamic.

On the one hand, for mechanists such as Machamer, Darden, and Craver (2000) and Craver and Darden (2001) there is a set of types of geometric-mechanical, electrochemical, thermodynamic, and electro-

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magnetic activities in which the mechanistic explanation in biology hits bottom. This means that the parameters that represent these types of activities cannot be considered *prima facie* as filler terms, but are part of the theoretical body available for scientific modeling. Some examples of these relatively fundamental activities are the rotation of the alpha-helix in sodium ion channels (mechanical activity), the formation of a covalent bond between amino acids in a protein (electrochemistry), the diffusion of certain ions through the cell membrane (thermodynamic) or the conduction of electrical impulses through nerves (electromagnetic). On the other hand, concepts or terms of activities such as 'cause', 'encode', 'inhibit' are exemplary of filler terms, for example, terms that are in place of some activity with respect to which we do not know exactly how it is carried out. Now, what is it that distinguishes the terms for relatively fundamental, or unproblematic, activities from the filler terms? Why is "ionic diffusion" an activity that is part of the stock of concepts at hand for mechanistic modeling, but 'storage of representations' is not?

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The common thread of the answer to these questions can be found in Weber (2005). This author focuses on a specific process that is part of the action potential mechanism: the passive transport of ions through the membrane. This process is the result of the action of two different types of forces. First, there is an electromagnetic force. The fact that the positive and negative charges are unevenly distributed on both sides of the membrane creates a net Coulomb force that moves the sodium ions through the membrane. The second force is osmotic. The ions are always in thermal random movement, constantly bouncing off the lipid membrane. Therefore, more ions will cross the membrane from the side where the ionic concentration is higher. There is a state of equilibrium in which these two forces cancel and which is given by the Nernst equation:

$$[1]\, E = \frac{RT}{zF} \ln \frac{[X]_o}{[X]_i}$$

In this equation, R is the gas constant, T is temperature (in degrees Kelvin), z is the valence of the ion, F is the Faraday constant, and $[X]_o$ and $[X]_i$ are the ionic concentrations outside and inside the cell membrane. If the ion concentrations are not in equilibrium with the membrane potential, then there is a net force that is proportional to the potential difference between the membrane voltage and the equilibrium voltage for that type of ion.

Well, as Weber (2005) points out, what this brief explanation shows is that the passive transport of ions is explained in terms of at least two scientific laws: Coulomb's law, which specifies the force with the which charged bodies attract or repel each other, and the Nernst equation, which specifies the equilibrium state for passive ion transport. Weber's conclusion is that, at least in some specific cases, it appears that laws of nature are required to understand how certain activities arise and develop. However, Weber (2005) concedes that this conclusion can be reformulated in terms of the laws of science, in such a way that "we could use the so-called semantic conception of theories to say that the model [of the action potential] is a physical theory -chemistry that incorporates some biological information" (p. 27)

The example of passive diffusion shows that the possibility of making explicit an acceptable mechanistic criterion to draw the distinction between the concepts of 'non-problematic activities' and 'filler terms'. In the context of scientific modeling, certain activity concepts are considered relatively unproblematic (i.e., as those in which mechanistic research stops) only if such processes or activities can be subsumed under some confirmedscientific principles or laws. Given the classification of fundamental activities by Machamer, Darden, and Craver, the relevant scientific laws for mechanism are those of the theories of mechanics, chemistry, thermodynamics, electrodynamics (and the various theories between those fields). Since the well-confirmed scientific laws that would specify activity terms such as 'memory storage' or 'rotate visual representation' are supposedly unknown, the latter cannot be considered relatively unproblematic in the construction of a complete mechanistic description.

Another important point is that the example of passive transport makes it possible to question the (perhaps implicit) thesis of mechanist according to which the scientific laws involved in mechanistic models can only, —their only function is— specify the activities or capacities of the parts of a mechanism. It could be argued, following Cartwright (1983) that Coulomb's law only describes the 'ability' of charged bodies to be moved by other charged bodies under certain conditions. However, this defense does not apply to the Nernst equation. Since it is a thermodynamic law, the equation specifies the behavior of a piece of matter without paying attention to the causal details of the processes involved; *a fortiori*, thermodynamic laws do not refer to capacities possessed by individual objects.

In the context of the Nernst equation, although ions have an 'ability' to move or be moved by other ions, what they do not have is the ability to try to equilibrate across the membrane. Perhaps the electrochemical gra-

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dient across the membrane has this capacity, but it is not a component part of the mechanism, it is not even a material object, as Weber (2005) points out. This example shows that, at least in the case of some scientific laws involved in mechanistic models, the function of such laws is not linked to the specification of the activities carried out by the parties. In sum, it is clear that the thesis of the epistemic/explanatory irrelevance of scientific laws cannot be accepted by the mechanistic philosopher. Rather, this thesis is required by a good elucidation of the mechanistic distinction between sketches, diagrams, and ideally complete descriptions of mechanisms.

The AEN-3 premise focuses on the indispensability of scientific laws in specifying the dynamic organization of a mechanism. This indispensability can be illustrated with the paradigmatic example of a successful mechanistic explanation proposed by Craver (2007), the explanation of the action potential using the ion channel theory. From a Hempelian, nomological-deductivist perspective, it could be thought that the electrophysiological model of Hodgkin and Huxley (1952) of the 'action potential' explained the development of the nerve impulse through the neuronal membrane by means of certain principles or dynamic laws, collected in the Differential equations of the model, which essentially represented the changes in the selective permeability of the membrane. For mechanistic philosophers, on the other hand, the Hodgkin and Huxley model is a phenomenological model or, at best, the incomplete sketch of the mechanism of the action potential. The main reason is that the Hodgkin and Huxley model includes filler terms. The 'activation' and 'inactivation' of certain 'active transport particles' of ions were activities (and parts) postulated by the model, but for whose existence there was no evidence of any kind. In fact, that initial hypothesis turned out to be empirically inadequate and was consequently rejected. According to Craver (2007):

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> C.M. Armstrong (1981) and Bert Hille (1992), among others, elevated the discourse of specific ion channels above the status of filler terms. In Hille's model, now part of neuroscience textbooks, conductance changes in action potentials are explained by the temporally coordinated opening and closing of channels through the membrane (p. 116).

In Hille's theory of 'ion channels', above a certain threshold of depolarization of the cell, a large number of sensitive channels Na^+ open, which dramatically increase the conductance through the cell membrane allowing the entry of ions of the Na^+ intracellular medium. This flux of sodium ions drives the cell voltage to approximately the equilibrium

potential of Na^+ and accounts for the rising phase of the action potential. The depolarization of the membrane produces the inactivation of specific channels of Na^+; In addition, it activates another type of ionic channels that are specifically sensitive to potassium ions K^+: these then diffuse from the intracellular fluid to the extracellular environment. The diffusion of potassium ions returns the potential of the membrane to its equilibrium potential. Since these de K^+ channels take a relatively long time to return to their closed state, the membrane enters a hyperpolarization phase, during which it is less excitable. Despite the fact that Armstrong and Hille's explanation pattern was clearly mechanistic, tending to identify the specific parts and activities responsible for the action potential phenomenon, Craver (2007) maintains that the proposal of these researchers constituted a mechanism scheme. There was still, according to Craver, filler terms; in particular, the question of how the channels were 'activated' and 'deactivated' was still pending.

Craver focuses on reconstructing the empirical investigations of these activities postulated for the case of specific channels of Na^+ (see Hille, 1992). In modifying the example slightly and reviewing the empirical research on the way in which the activation of potassium channels K^+ takes place. This shift in focus makes it possible to highlight the importance of the specification of certain scientific laws in a relatively more complete description of the activities of a mechanism, i.e., one that replaces padding terms with relatively unproblematic concepts.

The fundamental leap in the scientific understanding of the structure of K^+ channels is relatively recent. In Doyle et al. (1998), a disciple of Hille, Rod Mackinnon, and members of his laboratory, succeeded in applying experimental X-ray diffraction crystallography techniques to reconstruct the three-dimensional structure, at the atomic level, of channel $KcsA$ —a channel of potassium from the bacterium *Streptomyces lividans*. The $KcsA$ structure consists of 396 amino acid residues (or 3504 atoms). The channel is constructed of four subunits of a peptide chain tetramer, each consisting of an outer helix, an inner helix, a pore helix, and a selectivity filter. The protein atoms form a central pore through these subunits, as reported by Chung and Kuyucak (2002) (see Figure 1).

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Figure 1
The sculpture 'The Birth of an Idea' by Julian Voss-Andreae
based on the KcsA potassium channel

Source: Photo by Dan Kvitka. Sculpture commissioned and owned by Roderick MacKinnon.

MacKinnon won the Nobel Prize in 2004 for this description of the atomic structure of the $KcsA$ channel. However, the same researcher considers that the task is far from being concluded and states:

> Many questions remain unanswered. I suspect that the ions in the pore interact with each other through the structure of the protein. To test this idea, however, higher resolution data on selectivity filter chemistry are required, and perhaps protein dynamics studies (Hille, Armstrong, & MacKinnon, 1999, p. 1109).

He is not wrong. Most recent reviews of advances in the field of ion channels, such as Kuyucak and Bastug (2003), point out that the discovery of the atomic structure of channels like $KcsA$ has changed the fo-

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cus of theoretical studies in the area, from qualitative models to quantitative models in which it is intended to specify the functional and dynamic aspects of the permeability of the channels, starting from the information available about the molecular structure. In this sense, computational modeling provides a complementary source of understanding regarding crystallographic experiments. As Chung and Kuyucak (2002) point out:

> During the last few years, there have been enormous advances in our understanding of structure-function relationships in biological ion channels. The sudden advance has been the product of the combined effort of experimental and computational biophysicists, who have brought to light the working principles of these exquisitely designed biological macromolecules that regulate ionic gradients across the living membrane (...) Many aspects of Observed macroscopic properties of ion channels are being considered by stochastic dynamics and molecular simulations. Intuitive and claimed explanations of ion permeability and selectivity are beginning to be replaced by quantitative statements based on rigorous physical laws (p. 268).

The contemporaneously accepted explanation of the action potential basically consists of the ion channel scheme proposed forty years ago by Hille. This scheme proposes certain component parts or entities and activities, for example, the activation and inactivation of ion channels. Recently, MacKinnon has identified, with an atomic level of detail, the structure of these channels, in particular, of the potassium channels in certain bacteria. However, this theoretical and experimental achievement, rather than constituting a 'resting point' for the research, rather implied the launching of a set of computational modeling works aimed at quantitatively representing the activities and the dynamic organization of the concrete components, which guarantee the productive continuity of the mechanism. The need for such models should be clear at this point in the argument: without such quantitative understanding and in accordance with well-confirmed scientific laws, representations of ion channel activities are mere filler terms, which cannot be considered as relatively nonproblematic and well understood.

Considering that it is a paradigmatic exemplar of mechanistic explanation, the requisiteness of dynamic principles in explaining the action potential strongly suggests that such principles will be equally indispensable in the construction of many other, if not most, of the mechanistic models in biology and cognitive sciences.

This long argument in favor of the necessity of the laws of science for the specification of mechanistic models should not be read as a criti-

que of mechanism. According to a charitable interpretation, it is notable that, in key passages of their texts, mechanists kindly accept the indispensable nature of this type of scientific principle, both for the fundamental description of activities (AEN-2), and for the description of the organization. Mechanism dynamics (AEN-3).

Regarding the general thesis of epistemic indispensableness, Craver and Kaiser (2013) state, against Leuridan, that:

> In short, no mechanist denies that biologists look for regularities and routinely formulate [non-strict] generalizations that can be used for prediction, explanation, and control of phenomena. In fact, it is difficult to see how any significant human activity can be carried out without discovering and representing (in some sense) such regularities (p. 130).

Following Bogen (2005), Craver and Kaiser emphasize the variety of epistemic roles that the principles of scientific models can play in the search for mechanisms, among others, describing the phenomenon to be explained, describing some restrictions on acceptable mechanistic models, calculate quantitative parameters relevant to the mechanism and simulate the behavior of the mechanism. For their part, Bechtel and Richardson (2010) suggest, perhaps surprisingly, that to the extent that they emphasize the study of scientific models over the study of general theories, their mechanistic proposal is not incompatible with the family of semantic conceptions. of theories. In most semantic conceptions, such as that of Giere (1988) mentioned by the authors, certain principles (or 'laws of science') are indispensable for the specification of scientific models.

The mechanistic acceptance of the necessity of the laws of science is even clearer if we focus on the premise AEN-3. Thus, for example, in the context of a review of certain overly strong statements made by Machamer, Darden, and Craver, Kaplan and Craver (2011) argue the following:

> Frequently, the features of the spatial and temporal or dynamic organization of the components and their activities are explanatory relevant and are included in [mechanistic] models (...) Mechanisms are frequently described using equations that represent how the values of the component variables change with each other. Mathematical description, although not essential for all mechanistic explanations, is certainly a useful tool for characterizing complex interactions between components, even in moderately complicated mechanisms (p. 606).

Thus, for these authors, scientific principles can be 'useful tools' in the adequate representation of the dynamic organization of mechanisms, as long as they reach a relatively low threshold of complexity. Although

useful, such principles appear to be optional. Now, this excessive preven-
tion of Kaplan and Craver can be questioned, since what alternative do
we have to the use of such dynamic principles in the representation of
complex systems? On this point, I agree with mechanists such as Bechtel
and Richardson (2010), and Bechtel and Abrahamsen (2010), who point
out that the 'complexity threshold' for the use of dynamic equations is
quickly exceeded in most —if not all— biological systems studied by
neurobiology or cognitive sciences. Bechtel and Richardson (2010) even
go so far as to formulate the very thesis of requisiteness, according to
which the specification of dynamic laws is a necessary requirement for
the construction of complete mechanistic explanations:

> As the number and importance of interactions increases, so does the
> complexity of explanatory problems. The task of constructing an ex-
> planation for a given domain can be seen as the task of finding a suffi-
> cient number of variables, of constraints on the possible values of those
> variables, and the dynamic laws that are functions of those variables.
> These laws make it possible to use the model to predict future states of
> affairs from descriptions of preceding states (p. 21).

Given this textual evidence, it is prudent to reconstruct the me-
chanistic position as accepting the thesis of the explanatory requisiteness
of scientific laws (not so of natural laws, whether strict or not). If the
opposite interpretation on which Leuridan's (2010) argument is based
was chosen, then the argument presented in this section would constitute
a critique of the mechanistic conception. The point is that this interpre-
tation of Leuridan runs the risk of attacking a 'straw man': a thesis not
defended by anyone and rejected by all the participants in the debate.
Therefore, it can be concluded that, under a charitable interpretation of
the mechanistic conception, it commits itself, like the functional analysis
of Cummins (1983), to the thesis of the epistemic/explanatory indispen-
sability of the principles or laws of science, at least for the case of mecha-
nistic models in biological or cognitive sciences.

Conclusions

This article defends the indispensability of scientific laws in the field of
cognitive sciences.

The distinction between the laws of science and the laws of nature
was introduced.

It is evident that both those who defend and those who reject the existence of intentional causal laws assume that these laws do not contribute to the functionalist or mechanistic explanation of the phenomena they describe.

It is argued that functional analysis requires the specification of non-causal scientific laws.

An argument is made in favor of the epistemic necessity of scientific laws for mechanistic explanation.

Scientific laws (though not necessarily the laws of nature) play an indispensable epistemic role in explanation in cognitive science.

Bibliography

BEATTY, John
    1995   The Evolutionary Contingency Thesis. En Gereon Wolters, James Lennon y Peter McLaughlin (Eds.), *Concepts, Theories and Rationality in the Biological Sciences* (pp. 45-81), Pittsburgh, University of Pittsburgh Press.
BECHTEL, William & ABRAHAMSEN, Adele
    2005   Explanation: A Mechanist Alternative. *Studies in the History and Philosophy of Biological and Biomedical Sciences, 36*, 421-441. https://doi.org/10.1016/j.shpsc.2005.03.010
    2010   Dynamic Mechanistic Explanation: Computational Modeling of Circadian Rhythms as an Exemplar for Cognitive Science. *Studies in History and Philosophy of Science, 41*, 321-333. https://doi.org/10.1016/j.shpsa.2010.07.003
BECHTEL, William & RICHARDSON, Robert
    2010   *Discovering Complexity*. Cambridge, MIT Press.
BOGEN, James
    2005   Regularities and Causality. Generalizations and Causal Explanations. *Studies in History of Philosophy of Biology and Biomedical Sciences, 36,* 397-420. https://doi.org/10.1016/j.shpsc.2005.03.009
CARTWRIGHT, Nancy
    1983   *How The Laws of Physics Lie.* Oxford, Clarendon Press.
CHUNG, Shin-Ho & KUYUCAK, Serdar
    2002   Ion Channels: Recent Progress and Prospects. *European Byophysics Journal, 31,* 283-293. https://doi.org/10.1007/s00249-002-0216-4
CRAVER, Carl
    2006   When Mechanistic Models Explain. *Synthese, 155,* 355-376. https://doi.org/10.1007/s11229-006-9097-x
    2007   *Explaning the Brain: Mechanisms and the Mosaic Unity of Neuroscience,* Oxford, Clarendon Press.
CRAVER, Carl & DARDEN, L.
    2001   *Discovering Mechanisms in Neurobiology: The Case of Spatial Memory*. En Peter Machamer, Robert Grush y Peter McLaughlin (Eds.), *Theory and Method in Neuroscience* (pp.112-137), Pittsburgh, University of Pittsburgh Press.

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Φ

CRAVER, Carl & KAISER, Marie

    2013   Mechanisms and Laws: Clarifying the Debate. En Hsiang-Ke Chao, Szu-Ting Chen y Roberta Millstein (Eds.), *Mechanism and Causality in Biology and Economics* (pp. 125-145), Dordrecht, Springer.

CUMMINS, Robert

    1975   Functional Analysis. *Journal of Philosophy, 72*(20), 741-765. https://bit.ly/2LPROEb

    1983   *The Nature of Psychological Explanation.* Cambridge, MIT Press.

    2000   'How Does It Work?' Vs. 'What Are The Laws': Two Conceptions of Psychological Explanation. En Robert Cummins, *The World in the Head* (pp. 282-310). Oxford, Oxford University Press.

DENNETT, Daniel

    1969   *Content and consciousness.* Londres, Humanities Press.

PÉREZ, Diana

    1995   ¿Leyes causales intencionales? *Revista Latinoamericana de Filosofía, 20*(1), 67-81.

DOYLE, Declan, CABRAL, João, PFUETZNER, Richard, KUO, Anling, GULBIS, Jacqueline, COHEN, Steven, CHAIT, Brian & MACKINNON, Robert

    1998   The Structure of the Potassium Channel: Molecular Basis of K^+ Conduction and Selectivity. *Science, 280,* 69-77. https://doi.org/10.1126/science.280.5360.69

FODOR, Jerry

    1987   *Psychosemantics.* Cambridge, MIT Press.

    1991   You can fool some of the people all of the time, everything else being equal; Hedged laws and psychological explanations. *Mind, 100*(1), 19-34. https://doi.org/10.1093/mind/C.397.19

    1994   *The Elm and the Expert. Mentalese and its Semantics.* Cambridge, MIT Press.

GIERE, Ronald

    1988   *Explaining Science.* Chicago, University of Chicago Press.

    1999   *Science without Laws.* Chicago, University of Chicago Press.

HILLE, Bert

    1992   *Ions Channels of Excitable Membranes,* Sunderland, Sinauer.

HILLE, Bert, ARMSTRONG, Clay & MACKINNON, Robert

    1999   Ion Channels: From Idea to Reality. *Nature Medicine, 5*, 1105-1109. https://doi.org/10.1038/13415

HODGKIN, Alan & HUXLEY, Andrew

    1952   Quantitative Description of Membrane Current and Its Application to Conduction and Excitation in Nerve. *Journal of Physiology, 117,* 500-544. https://doi.org/10.1113/jphysiol.1952.sp004764

KAPLAN, David & CRAVER, Carl

    2011   The Explanatory Force of Dynamical and Mathematical Models in Neuroscience: A Mechanistic Perspective. *Philosophy of Science, 78,* 601-627. https://doi.org/10.1086/661755

KUYUCAK, Serdar & BASTUG, Turgut

    2003   Physics of Ion Channels. *Journal of Biological Physics, 29*, 429-446. https://doi.org/10.1023/A:1027309113522

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Sophia 30: 2021.

© Universidad Politécnica Salesiana del Ecuador

Print ISSN: 1390-3861 / Electronic ISSN: 1390-8626, pp. 93-119.

LEURIDAN, Bert
    2010   Can Mechanisms Really Replace Laws of Nature. *Philosophy of Science, 77,* 317-340. https://doi.org/10.1086/652959

LORENZANO, Pablo
    2007   Leyes Fundamentales y Leyes de la Biología. *Scientia Studia, 5*(2), 185- 224. https://doi.org/10.1590/S1678-31662007000200004

MACHAMER, Peter, DARDEN, Lindley & CRAVER, Carl
    2000   Thinking About Mechanisms", *Philosophy of Science, 67,* 1-25. https://doi.org/10.1086/392759

SCHIFFER, Stephen
    1991   Ceteris Paribus Laws. *Mind, 100,* 1-17. https://doi.org/10.1093/mind/C.397.1

SKIDELSKY, Liza & PÉREZ, Diana
    2005   La distinción personal-subpersonal y la autonomía de la explicación de nivel personal en Dennett. *Manuscrito*, *28*(1), 77-112. Recuperado de: https://bit.ly/3r6NznQ

SKIDELSKY, Liza
    2003   Realismo Intencional, Eliminativismo y Psicología Científica", *Análisis Filosófico, 23*(1), 13-39. Recuperado de: https://bit.ly/37tHBpc

SWARTZ, Norman
    1995   *A Neo-Humean Perspective: Laws as Regularities.* En Friedel Weinert (Ed.), Laws of Nature: Essays on the Philosophical, Scientific and Historical Dimensions (pp. 67-91), New York, de Gruyter.

VAN FRAASSEN, Bas
    1989   *Laws and Symmetry.* Oxford, Clarendon Press.

WEBER, Marcel
    2005   *Philosophy of experimental biology.* Cambridge University Press.

Weiskopf, Daniel
    2011   Models and Mechanisms in Psychological Explanation. *Synthese, 183,* 313-338. Recuperado de: https://bit.ly/37xNZM4

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