Structuralism as a Resource to Detect and Solve Some Current Philosophical Problems

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1. Introduction

The structuralist view of theories is a pertinent tool to address some current philosophical problems. I am not saying that the use of this metatheoretical perspective is indispensable for us to find all and every answer in the philosophy of science (not even for the ones I mention here), but rather that at least some important questions from that realm are perfectly manageable from this perspective, and even can be faced and resolved rather easily. Also, and given its inherent formalist standpoint, perhaps its approach to the matter is preferable to the alternatives, every time one values precision and clarity as virtues. Then, if I succeed, this article could be an incentive to dive into crystalline structuralist waters.[2]

Specifically, I briefly introduce three philosophical cases that can be successfully dealt with using structuralist tools. All of them can be initially addressed using only the rudiments of one (important) chapter from the structuralist manual (Balzer, Moulines, and Sneed 1987). Most assuredly, we would need more to get the complete picture and a definitive solution to each one of these problems, but I limit the study to that early phase of analysis, which I think would be enough to carry out my purpose.

These three philosophical problems are (1) the acknowledgment of rivalry between theories and within a particular theory; (2) some aspects of what has been called "revolutionary science"; and (3) the search and exclusion of circularity in empirical theories. Almost every case study we are about to consider stems from the evolutionary biology realm, but of course, it could be extended—*mutatis mutandis*—to many others.

The contribution is organized as follows: firstly (Section 2), I will succinctly introduce how our metatheoretical approach suggests the way scientific theories should determine some terms of its vocabulary following a procedure that, when seriously taken, allows us to detect and/or face

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[2]To this date, several works have been published—in articles and anthologies—that have to do with the fertility of the approach (see Diederich 1989; Diederich, Ibarra and Mormann 1989; 1994; Abreu, Lorenzano and Moulines 2013; Balzer and Moulines 1996; Balzer, Moulines and Sneed 2000; Díez and Lorenzano 2002; and also the 2014 volume 79 of number 8 of *Erkenntnis*). In writing this contribution, I had in mind a reader who is not necessarily an expert in that view at all.

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precisely the philosophical problems we are about to describe. In this very section, I will show the quotidian nature of this procedure while working under the program. Secondly (Section 3) I briefly introduce the mentioned problems and how the former procedure can help us to find their respective solutions/clarifications. The reader will notice that the first two situations are frequently referred to both by historians and philosophers of science. The third one, while not as common as the former ones, is very important insofar as it has put in check an entirely new discipline within evolutionary biology/systematics: cladistics. Finally, I offer the conclusions.

2. A quick look at structuralism

2.1. Structuralist normal science

Structuralism was born to explicate scientific theories from a semantic/model-theoretic stance using formal tools, initially through the work of one disciple of Patrick Suppes, Joseph Sneed (1979). These formal reconstructions of theories are seen by structuralists themselves from two perspectives:

1) The reconstruction is a goal in itself, maybe *the* goal. It is a response to the motto "what can be said, can be said with clarity"[3] that lies behind the elucidatory function of the philosophy of science. Elucidative tasks are especially relevant and necessary when one takes into account that few concepts are so misused in the philosophy of science (and in science) and at the same time so frequently employed than 'theory". Then, to clarify it is imperatively desirable. Structuralism takes a stand on what a scientific theory is, and the formal apparatus it provides can identify its components with singular precision. Also (and for reasons that exceed us), it reconciles two visions of scientific enterprises usually seen as incompatible: the quest for precision that distinguished logical positivism and, among others, the genidentical or historical changing character of theories so emphasized by Kuhnian theses (Kuhn 1962; Stegmüller 1981; 1983).
2) The second perspective emphatically claims that the usefulness of the reconstruction goes well beyond the formalization; though those additional benefits are invariably based on it. Therefore, and far from being a sterile ground in other respects, the formal reconstruction acts as a platform from where a multitude of problems in the philosophy of science can

[3]More precisely: "Philosophy aims at the logical clarification of thoughts. Philosophy is not a body of doctrine but an activity. A philosophical work consists essentially of elucidations. Philosophy does not result in 'philosophical propositions', but rather in the clarification of propositions. Without philosophy, thoughts are, as it were, cloudy and indistinct: its task is to make them clear and to give them sharp boundaries. […] Everything that can be thought at all can be thought clearly. Everything that can be put into words can be put clearly" (Wittgenstein 2001, 4.112-4.116). As is well known, this was one of the main objectives of the Carnapian program. Carnap himself wrote in his *Aufbau*: "We too, have 'emotional needs' in philosophy, but they are filled by clarity of concepts, precision of methods, responsible theses, achievement through cooperation in which each individual plays his part. […] We feel that there is an inner kinship between the attitude on which our philosophical work is founded and the intellectual attitude which presently manifests itself in entirely different walks of life; we feel this orientation in artistic movements, especially in architecture […] We feel all around us the same basic orientation, the same style of thinking and doing. It is an orientation which demands clarity everywhere, but which realizes that the fabric of life can never quite be comprehended" (Carnap 2003, pp. XVII-XVIII).

be successfully addressed. As a result, even if at first glance you cannot see the additional advantages resulting from a formal reconstruction of a particular theory, you might discover what the approach has to say regarding many metatheoretical debates, including both generic and more local ones.

It is this second issue that I want to stress here. We are about to see how even what constitutes the first step in reconstruction is sufficiently powerful to deal with the problems that occupy us here. For now, the main point is that structuralism deals with them explicitly or implicitly in normal work. Again, and surprising as it might be, *it is routine*, it happens *every time* the approach is applied. Let us analyze what this means.

2.2. Structuralism and the empirical basis of theories

When Professor Pablo Lorenzano taught his students (myself one of them) about structuralism, he used to advise on what to do first when one is about to reconstruct a particular theory T. He often said, following Sneed (1979, p. 297), that pragmatically one starts where one can, and not always where one should. Without a doubt, he was right. However, ideally, the first thing one should do is to specify what T speaks about, and what its vocabulary is.

Then, in the second step of our reconstruction, if faced with an explanatory theory, it is necessary to identify what elements of that vocabulary help us to determine what T intends to explain. In empirical theories, these explanatory targets of T are chunks of reality, parts of nature. (As I shall show in the next subsection, this does not mean that a metaphysic alignment on the part of structuralism has taken place.) Often, this goes hand in hand with the reason why the theory was originally conceived, that is, as an answer to a problem in the realm of experience (Blanco 2011b). Structuralism claims that each scientific theory has something like "a world of its own". This "world" is characterized as a subset of terms within the vocabulary of T. This means that once you list the terms of T, it is possible to distinguish between two kinds of them. The structuralist criterion for this discrimination has to do with the answer you can give to the following question regarding every term in that vocabulary: "Is it possible to determine it without the application of T?"[4] Of course, there are two possible answers:

1) If it is not possible, then that term belongs to the set of what structuralists call "T-theoretical terms". It is so called precisely because of its dependence on T at the time of its determination: for every determination of the term, T and its laws are presupposed and applied. T is indispensable to do the job.
2) If it is possible, then, that term belongs to the set of what structuralists call " T-non-theoretical terms". T and its laws are not needed for its determination. T is dispensable to do the job (Kamlah 1976; Sneed 1979, p. 33; Balzer and Moulines 1980; Moulines 1985; Balzer, Moulines and Sneed 1987, Chapter II).[5]

[4] To determine a term is to assign one or more numbers, through a measuring method of calculus, if the term is quantitative; or if it is qualitative, with the application to an object.

[5] Between 1983 and 1986, Wolfgang Balzer developed a second criterion for theoreticity, the so-called "formal" one (Balzer 1985; 1986; Balzer, Moulines and Sneed 1987, pp. 73-78). This new criterion has no relation with what is presupposed by T and involves an existential and not a universal quantifier. Briefly, Balzer says that a term is T-non-theoretical *if there exists any* possibility for its determination that is independent of T. In this contribution, we do not follow Balzer on this.

In every empirical theory, what its users intend to explain, that is, the *explananda* of T, should be linked with this last set, and not with the former one. Notice that these *explananda* could be determined using the laws of T, but the fact that one can determine them without T is what makes its testing possible insofar while testing, you compare the results obtained thanks to the application of T with results obtained *from a different source* that is considered to be reliable by the users of T. If not, there would be no control for T, it would be controlling itself, and would be valid in every intended application for bad/vicious reasons. While dealing with explanatory scientific theories, we need to provide guarantees that this kind of problem is not present every time we test them.

This is why the set of T-non-theoretical terms can count as the ontology for T, it is its empirical basis (a basis that is available before the discovery of T) and is considered as unproblematic data (or undisputed "facts") by the users of T, these being borrowed from the mentioned trustworthy or undisputed "different source" (we will return to this in the next subsection).

As can easily be inferred, this set of T-non-theoretical terms is closely related to the domain of application of T, which, as I mentioned, is what counts as "the first motor" that led scientists to invent/discover T. Sufficed to say that, for the Sneedian perspective, this pragmatic issue is as essential for the identity of T as its formal content.[6]

Therefore, the terms involved in the *explananda* of T should be T-non-theoretical. Usually (always?), the rest of the vocabulary (the T-theoretical terms) has explanatory power for those *explananda* (for more on this, see Bartelborth (1996; 2002), Díez (2002), Moulines (2004) and Lorenzano (2005)). Note that this distinction helps us to separate the problem to be attacked from what we use to solve it, and once this is done, the task can continue by exploring the following steps to attain the reconstruction of T. Let me say more about the origins of those T-non theoretical terms.

2.3. The sources of T-non theoretical terms

In the earlier subsection, I mentioned that what counts as data to be explained by any explanatory theory T, does not (should not) come from T, but from a "different" source. But, what is that "different" source? Is it the world itself? "Not necessarily", is the usual structuralist answer. Why? Because the T-non-theoretical terms probably are determined through the application of other previous-underlying theories.[7]

This leads to two new important issues within this approach. First, we can identify connections between different theories in science that help to transfer inter-theoretically information between each other. T inherits its T-non-theoretical terms from other theories.[8] In short, while determining

[6]Suppes' first doctoral student, Ernest Wilcox Adams (see Suppes 1994a, p. 201; 1994b, p. 5) suggested that introducing the entities to what scientists intend to apply a theory, should be included in the semantic reconstruction of theories. Surely a lot in the world might satisfy the demands of the formal axioms, but the users of a theory only have some portions of that great set in mind while applying it (Adams 1959, pp. 257-259). Sneed strictly followed Adams on this (Suppes did not).

[7]A lot of what counts as data for scientific theories is obtained through the use of instruments, and sometimes (not always, perhaps nor even in the majority of cases) these instruments are built following one or more theories. This has new philosophical consequences for some topics we address here (the relationship between incommensurability and relativism, for example). For space reasons, I sidestep this issue but see Bueno (2012), Díez (2012, pp. 104-113), Jaramillo Uribe (2012), and Lorenzano (2012).

[8]Even when it was first proposed by Sneed (1979), this idea was present in intuitions made by prior classic

the *explanandum* of T, we do not need to presuppose T, but we do presuppose the previous theories needed for their determination (see Moulines 1984; 1985; Diederich 1989, pp. 364-365).

Note that this introduces a connection between two metatheoretical realms: philosophy of science underlines the importance of the history of science insofar as any structuralist reconstruction leads us to look in history for one or more theories that are indispensable to determine the terms the theory under study explains. That is, this creates a meaningful link with the history of science that cannot be underestimated: as we will see, one can resolve a controversy on an alleged circular explanation by introducing an earlier theory that determines what T means to explain. (This relationship with history is not linear, but we can safely claim that if a set of terms was available before T; then we can consider it as T-non-theoretical.)[9] One can take this as a new convenience for their marriage (see Giere 1973; McMullin 1974) or for the consideration of both as "Siamese twins".

The second consequence becomes clear once we note that what *counts as* "the world" for T, might not be the same for another theory, T'. This is because what counts as a T-non-theoretical term in T could be T-theoretical in T' (see Sneed 1979, p. 252). This distinction is related to the theory we are dealing with. A term is not T-theoretical or T-non-theoretical in an absolute sense but in a particular context.

This gives us a clue to the answer we are looking for. What we can consider the world while dealing with a particular theory might well be loaded with another previous theory. Then, the primary source of T-non-theoretical terms (the uncontroversial data for T) is another different theory. If that can be extended to all the not-theoretical terms of every theory in science is something about which structuralists are, in principle, agnostics (see Díez 2012, pp. 110-113; Falguera 2003; 2012).

It is time now to see how this simple tool can bring light to common issues in the philosophy of science.

3. Dealing with philosophical problems in science

3.1. On rivalry in science

The first example has to do with empirical commensurability between and within theories (Kuhn 1962; Feyerabend 1962), and in the following, we will address briefly both kinds of cases (see Stegmüller 1975; 1976).

3.1.1. Rivalry between different theories

It is possible to identify competition between any two theories, by the comparison of what counts as the "part of the world" each one of them intends to explain. You have genuine rivalry if the

philosophers of science (see Hempel 1970; Lewis 1970). Note the curious coincidence in the date of these sources. While visiting Argentina in March 2009, Sneed was asked by José Díez whether he was aware of Hempel or Lewis's papers (both appeared in 1970, while he was working on his book published in 1971). Sneed's answer was "not at all".

[9] Even when the independent determination of any given term of T does not imply its historical precedence, a previous determination for that term does imply its independence from T. (There is an interesting conversing case regarding mass and Classical Particle Mechanics, see Moulines (1991, pp. 236-238)).

intersection of both sets is not empty. The bigger the extension of that intersection, the more ferocious the competition would be. Note that, paradoxically, the core of the competition is an agreement about what we have in "the world" for each of them to explain. The greater the agreement, the more difficult their coexistence would be. There is an intentional component here: to have a rivalry, the users of both theories must intend to apply them to the same domain (the very same elements belong to both sets, being then mutually co-extensive), or at least that there is overlapping, that is, that the intersection of both sets of intended applications is not empty. In the first case, we have total empirical equivalence between both; in the second, only partial empirical equivalence.

For example, it has been repeatedly defended that the theory of natural selection is a rival of natural theology in the sense that the respective users of both intend to explain the same portion of nature: adaptations. However, it is perhaps true that the former theory forbids the existence of organic structures that would be perfectly accepted in the latter. To use Darwin's example, traits that while useful for a different species, result to be harmful for the one that bears them (Darwin 1859, p. 159). Still, the intersection of the two sets of terms involved in the determination of the *explananda* of both theories is not empty, though probably not co-extensive, and then we have genuine rivalry between them (see Ospovat 1980a; 1980b; Blanco 2008; 2011; Caponi 2011a; Ginnobili 2014).

Following with one additional Darwinian case, it has been underscored that the antagonism between the theory of the *Vertebrata* Archetype coined by Richard Owen and the theory of common descent has to do with their coincidence on what they want to explain: the presence of homologies (see Blanco and Ginnobili 2020). Here, there are reasons to think that their respective sets of intended applications are coextensive with each other, given the fact that Darwin did not question any of the procedures proposed by Owen to detect homologies. On the contrary, he used them. I will return to this point in Subsections 3.2 and 3.3.[10]

A third case of this kind of rivalry can be found in the scope of the domain of application of the theory of natural selection between Alfred Wallace and Darwin. The second one, more pluralist, opened the door, between others, to one Lamarckian theory to explain the origin of some features, while the first one used to be skeptical of that, remaining "loyal" to a more universal application of the theory of natural selection in nature. While Darwin himself intended to limit the set of applications of the theory of natural selection, Wallace intended to broaden it. A debate between both scientists was the obvious result. Note that this discussion takes place within the intentional realm. One should not confuse an intended application with a successful one, the latter being decided in the empirical arena.[11] Of course, and it happens very often in scientific practice, one

[10] Using present standards, it would be hard to consider nineteenth-century natural theology (and its inherent creationism) as a scientific theory. Perhaps we should consider it as a proto-theory or something of the sort. However, it would be anachronistic to do so, insofar as Darwin himself considered it as a scientific attempt (Darwin 1859, pp. 194, 203, 355, 356, 372, 393, 471, 473, 474, 478). Interestingly enough, Darwin does not say the same about Owen's theory of archetypes, objecting to its scientific status because of its link with religion (Darwin 1872, p. 383).

[11] Typically, structuralism associates certain empirical claims to every element of the set of the intended application, such as (for every x belonging to that set) "x behaves as the laws of T predict". That sentence can be true or false, but that does not mean that the theory is true or false, but only that that part of the world ("x") does or does not satisfy the laws of T. When the sentence is true, we are in front of a successful application of T

can explain the failure through several recourses, preserving the applicative intentions about the seemingly failed application, despite the evidence against it.

As a result, when reconstructing any two theories, we can see the coincidence (or not) between their domain of application and infer from it the level of their competition. Surely, it is not always the case that these disputes end in an all-or-nothing verdict, which would mean a total replacement of one theory for the other. Sometimes, what happens is a mere redistribution of the domain of application of those theories. As it is determined intentionally, the domain of application of any theory is not fixed but is an open set, where elements can enter or leave through time.[12]

Depending both on the context in which this rivalry takes place and the caliber of the theories at stake, the result can be one of what Kuhn called "scientific revolution" on the one hand (I will deal with this in Section 3.2), or an "internal" debate between each other, which we shall see next.

3.1.2. Rivalry within one and the same theory

A less obvious kind of rivalry can also perfectly be explicated using structuralist tools: the competition between heterogeneous theoretical elements of the same theory. This is possible thanks to the hierarchical perspective that arranges those elements in a multi-level array. As we go down the hierarchy, through the elements located in the lower stratified portions of the theoretical structure, typically, we find that their respective domain of application gets increasingly more restricted, since the theory gains in empirical content.[13]

The more important difference with the former kind of rivalry is that it explicates the discussion between scientists under *the same* research program. Even when the resolution of that discussion might be challenging and complex, the triumph always takes place within one single theory/paradigm/program and does not lead to any revolutionary event at all. On the contrary, in the long run, the program as a whole is strengthening. If this ends up in change, it is an intra-theoretical change.

We find an example of this in another well-known debate between Darwin and Wallace regarding the application of a different portion of (explanatory theoretical terms within) the theory of natural selection: how pervasive is sexual selection in the history of life? Is it the theoretical element of the theory of sexual selection or another one (such as the one related to survival) that should be applied to explain the evolution of this or that trait? Whatever the answer to that question, the theory of natural selection wins insofar as we have found a successful application for it whether by the theory of sexual selection or by the theory of natural selection related to an improvement in

in the sense that "x" does behave as T predicts. Even when closely related, one should not confuse the set of intended applications of T with the empirical claims associated with T. We have three sets: the set of potential applications, the set of intended applications, and the set of successful applications. Note how an intended application might well be not successful, and a potentially successful application might not be intended.

[12]Structuralists claim that the intended applications are an essential part of a particular theory. However, this change does not necessarily lead to repetitive revolutions provided that some particular elements remain, that is, its paradigmatic exemplars. This is another way in which a theory might change without a modification in its identity.

[13]For a complete understanding of these issues, it might be necessary to explicate both what a theoretical element is and what lies behind this arrangement. Unfortunately, we leave this aside for space reasons (see Balzer, Moulines, and Sneed, Chapter II).

survival (or any other one).[14]

Again, and as we can see from the last example, it is not an all-or-nothing debate, but it often ends with a change of belonging of elements in the respective sets of the intended application of those theoretical elements, which are portions of the same gathering theory. As we take the intended applications as part of the identity of theories, this kind of accidental, peripheral change can capture the development of a theory in what Kuhn called "normal science" (see Kuhn 1962; 1976).

Finally, this approach is also relevant to the historiographical issue of priority between Wallace and Darwin, because this kind of application allows us to see what was what both of them discovered with precision, what concepts both contributions share, and what the novelties between both versions are (see Blanco 2016; Ginnobili and Blanco 2019). Note that a conclusion based on this systematic approach can be made independently of both chronological issues and even the opinions of Darwin and Wallace themselves (Wallace 1908, pp. 5-7; Darwin 1958, p. 21).

3.2. The nature of scientific revolutions

The empirical commensurability between competing theories that I mentioned above helps us when thinking about the nature of scientific revolutions. How much of the outgoing paradigm is displaced? How much of the incoming paradigm involves an inheritance of the displaced one? As Kuhnian revolutions take place within a discipline, it is difficult to find one without com- mensurability of some sort between the old paradigm and the new one.[15] Again, it is this commensurability itself is what ignites the very rivalry that ends up in the disruptive event. The first intuition here is that even when surely revolutions have to do with novelties, sometimes they are also linked with the rearrangement of remaining "old" data. Therefore, the notion of revolution o of change of theory/paradigm is not incompatible with some kind of preservation. If we keep in mind the structuralist distinction between T-theoretical terms and T-non theoretical terms, two successive theories might be partially commensurable not because part of their vocabulary is theory-neutral as a whole (they might well be loaded with a preceding theory), but because that part of their vocabulary is not loaded with those two theories (see Kuhn 1962; Lorenzano 2012; Díez 2012). It is this kind of neutrality what allow the inter-theoretical preservation of data in the empirical realm (see Kuhn 1983), even when you can have novelty in the explanatory realm. Let us see two examples.

The first case has to do with the so-called "Copernican revolution" (by the way, one of the exemplars used by Kuhn himself). In dealing with the rivalry between ancient astronomy, the

[14]Many authors, including those that do not share semantic perspectives when addressing scientific theories, have realized that some apparent flaws of the theory of natural selection, such as its alleged tautologicity, are related to the layering of a hierarchical structure (see Díez and Lorenzano 2013). Once they took that into account, they were able to see that the theory of artificial selection, the theory of sexual selection, and the theory of natural selection related to survival are indeed theoretical elements of the same theory (see Endler 1986, pp. 3-15; Tuomi 1981; Brandon 1996, p. 51; Lerner 1959; Gould 1976; Wassermann 1978; Tuomi and Haukioja 1979a; 1979b; Naylor and Handford 1985; Castrodeza 1988, pp. 182-183; Ginnobili and Blanco 2019). Then, a rivalry between any of these three theories is an internal debate that would not end in a revolutionary event.

[15]To talk about incommensurability between theories that have nothing in common is not sound. In such a case, we do have incommensurability, but it is not epistemologically meaningful (see Díez 2012, pp. 72-73). It is trivially true that Chomskian linguistics is incommensurable concerning quantum mechanics.

Tychonic version of the cosmos, and the Copernican point of view, a discussion has taken place regarding the concept of "planet" and how it could concern the revolutionary character of the rise of modern astronomy. However, it is undeniable that here we have partial commensurability; once we notice that *the same* data (astronomical tables for the apparent trajectory of each "planet") are explained by these three different theories with similar success (see Díez 2012, p. 92).

Severinus Longomontanus, a former assistant of Tycho, and a great defender of his system, wrote a manual in which he introduces not only Tycho's model (its main goal), but also the equivalence in precision to give account of *the same* available data while using any of the three systems (for Saturn's apparent movement, for example; see Longomontanus 1622, pp. 324-326). That means that "the world" was in some genuine sense the same for these theories, whatever the very important novelties introduced by Tycho or Copernicus might be concerning Ptolemy: three mutually exclusive theories share the same phenomena to be explained and this coincidence is the very reason why that (or any other theoretical) competition starts in the first place. The structuralist pro- gram would easily see this partial compatibility of these systems, shedding light on what Kuhn meant when he recognized a revolution here.[16]

The second case has to do, once again, with the link between the theory of common descent and the theory of the Archetype. There is a debt of the Darwinian Theory to Owen concerning the determination of homologies that Owen explained using an ideal archetype. Virtually, there is no change at all between the set of intended applications in both theories and even it is possible to detect the same paradigmatic exemplars, such as the mandibles of insects (Owen 1843, p. 215; Darwin 1872, p. 383) or the different forms of limbs in the group *Vertebrata* (1847, p. 127; 1849, pp. 3-9; Darwin, 1859, p. 200, 434, 479; 1872, p. 383). An exhaustive description of this is beyond our goals (see Blanco and Ginnobili 2020), but more light can be found in this very case, as we shall see in the next section.

As a corollary, probably every so-called revolutionary event in the history of a discipline does not change everything we can find in the old research paradigm (see Laudan 1984).

3.3. On circular explanation

Although the specific problem I now address is philosophical in nature, it has been initially stressed by experts in the scientific discipline in question itself: cladistics (Hennig 1966). Shortly, cladistics is an approach to systematics in biology that the majority of contemporary scientists consider to be the best one for several reasons. One of them is its efficiency regarding the reconstruction of the parental lines through grouping biodiversity, the two realms (systematics and parenthood) that Darwin was convinced should be tied to each other (Darwin 1872, p. 369). Therefore, they believe that the ideal outcome of cladistics is a hypothetical tree that intends to be the actual reconstruction of the familiar tree between the involved groups (potentially, all the groups of all living beings that live or ever lived on our planet). In a nutshell, the final and herculean task of the community of cladists is to offer the ultimate tree of life that, if correct, reflects—no more, no less—the history of life. Again, classification and the specification of parenthood become two sides of the same coin, and this is a tenet for some important portion of this community. From this perspective, the application of cladistics methods results to be an explicit recognition of the relevance of Darwin's

[16] Note another related issue: just as incommensurability can be used against realism, data preservation in revolutionary events seems to support it. I thank C. Carman for providing Longomontanus' reference.

thoughts on the topic, and the materialization of its ideal of what biological classification should be. However, the relation between cladograms and the actual evolutionary history is precisely what is at stake in an internal debate within the community. The problem we are about to introduce is not but a derivative of that controversy.

That being said, what is the problem? As I mentioned, it is a byproduct of a more general debate regarding the role evolution plays in the analysis, and it is related to how one can recognize/determine homologies in nature. How cladists do their (often, very complex) task is beyond my goals. However, it is necessary to deal with some key parts of the initial procedure to introduce what the problem is about.

To take into account that the theory of common descent, so clearly linked to cladistics, is today the usual/natural explanation for the occurrence of homologies, leads us to demand that those determinations should be done without the assistance of the theory of common descent. This has been stressed by several authors, including those who were not fond of cladistics methods, such as Ernst Mayr. He, together with Peter Ashlock, wrote:

> Relationship among species and higher taxa is indicated by the existence of homologous characters, but there is considerable uncertainty about what homology is and how it can be established […] When Darwin discovered common descent as the cause of homology, it became possible to adopt a more rigorous definition than the ones suggested by [his] forerunners, and yet, 12 years after the publication of the Origin, *there is still considerable argument over the definition of homology. The problem is how to avoid a circular definition*. […] Simpson's analysis of the problem (1961, pp. 69-93) is particularly enlightening. He points out […] the phenomenon of identical twins. Two siblings are not identical twins because they are so similar; rather, they are so similar because they are identical for having been derived from a single egg cell. The establishment of an unambiguous definition of homology is thus the first step in the analysis. (Mayr and Ashlock 1991, pp. 142- 143, my emphasis. See also Wood 1995; Padian 2007, p. LXXXVIIII; Griffith 2007; Rosenberg and Neander 2009, p. 309.)

However, together with the triumph of cladism as *the* systematic methodology, most of the enormous literature that discusses this topic has seen the light in the context of cladistics analysis. The intuition is nonetheless the same: the historical explanation (common descent) cannot be what is at stake at the time of the determination of similitudes that are explained by that historical circumstance.

There is not a homogenous position within cladists on how genuine (or serious) this problem is. Two groups can be identified in this community: those who consider it as a valid restriction that should be respected (pattern cladists), and those who do not. Again, space reasons prevent us from elaborating on this.[17]

[17] The problem of circular explanation in evolutionary biology, specifically in the theory of common descent has bogged down philosophers (and scientists interested in philosophy of science) for several recent decades, and the sources are too numerous to be quoted here. I offer a short list: Roffé (2020), Brower (2000; 2019), Pearson (2018; 2010), Roffé, Ginnobili and Blanco (2018), Lorenzo (2015), Wagner (1989; 2014), Pearson (2010), Rupke (2009), Amundson (2005), de Pinna (1991), Aboitiz (1988) and Brady (1985).

However, structuralism leaves us no way out, and I think it is quite simple to see what position it should take in that internal debate. If we determine the *explanandum* of T only by the application of T, how could it be possible to do non-circular testing of T? If that is the case, we lack the needed guarantee for genuine testing. Structuralism is not exactly a straight solution to the problem, but it offers what I esteem to be the clearer available view on the topic as long as it helps us to see a problem where not everybody sees it, and, as I shall show, gives the key clue about the direction where we should be looking for a solution. So far, the main point is to recognize that if the only method to determine any two traits as homologies is using the theory we then apply to explain the occurrence of those specific traits, then, we end up in the undesirable scene of circular (vicious) explanation. Therefore, our verdict is that the problem is real, and requires a solution.

Sneed and his colleagues have seen that these problems are a serious threat to the empirical nature of theories, and the same tool we are describing serves as a way to detect the presence of this circularity problem.[18] Also, structuralism leads us to look at previous theories, in this case, in the realm of comparative anatomy that were useful for the diagnosis of homologies by the time *On the Origin of Species* was first published (Darwin 1859).

The way Darwin established parental links was by taking into account what has been known since the works of Owen as "homologies" (1843; 1847; 1849), a set of (some kind of) similarities between living beings that can be found even in very different living forms within *Vertebrata*. Since 1859, the theory of common descent has been considered the best available scientific explanation for the presence of those similarities, but, previously, Owen himself had provided one with his inventive theory of the Archetype.

Santiago Ginnobili, Ariel Roffé, and I have recently entered into a discussion using these tools (Blanco 2012; Roffé, Ginnobili and Blanco 2018; Blanco, Roffé and Ginnobili 2020; Roffé 2020b). We showed both the relevance of structuralism in dealing with this issue and why the theory of common descent as it was introduced in Darwin's main work is not circular.[19] However, Owen's procedures for the diagnosis of homologies are not enough today (see Remane 1952). But that does not prevent us from taking into account that whatever other protocol is used in making these determinations, it should not be loaded with the explanatory theory (common ancestry) for their occurrence. Even in the case that we need to apply a theory to get the determination of homologies, that theory is neither the theory of common ancestry nor the Archetype one.

For that reason, the normal practice of structuralism helps us to avoid making mistakes that might lead to this kind of circularity, and this is the case whether there is an explicit an ongoing discussion on the topic or not regarding the theory in question. In other words, to have at least this portion of structuralism in mind from the starting point can genuinely help to do better science.[20]

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[18] I think we can consider it as a by-product of Sneed taking distance ourselves from the classical observational-theoretical distinction (see Putnam 1962; Bar-Hillel 1970; Suppe 1979, pp. 68-69; Stegmüller 1981, p. 92, 94, 106; 1984, pp. 235-236).

[19] This is true at least for this reason. For discussions about tautologicity (to which structuralism can also be of help), see the sources mentioned above, in n. 14.

[20] I strongly believe that the teaching of scientific theories and the way textbooks are written would also benefit from this approach.

4. Conclusions

1) Structuralism has been conceived as a formal tool for the explication of scientific theories from a semantic point of view. Even when it comes to satisfying an important necessity in the philosophy of science (what is the nature of scientific theories, what are their structures, etc.), its fertility can be seen in the fact that it provides us with a platform from which one can address significant philosophical problems. Rivalry within and between theories, the nature of scientific revolutions, and circular explanations are only three of them.

2) As I succinctly showed, the T-theoretical and T-non-theoretical distinction together with a hierarchical view of theories help to shed light on specific philosophical issues that ignited specific debates within the community, such as:

– What do two rivalry theories (e.g., the theory of common ancestor and the theory of the archetype; ancient astronomy and the Tychonic or Copernican Systems) have in common?

– Can some particular type of rivalry between theories (e.g., the theory of natural selection related to an improvement in survival and the theory of sexual selection) be seen as a competition within the same theory (e.g., the theory of natural selection)?

– What is the nature and scope of scientific revolutions (e.g., how much of Owen's comparative anatomy works remains in Darwin's theory of common descent)?

– How does our conception of revolution in science affect our position regarding scientific realism?

– Are these two scientists (e.g., Darwin and Wallace) the co-discoverers of that theory (e.g., the theory of natural selection)?

– Does this theory (e.g., the theory of common descent) offer a circular explanation?

– From which previous theory does this one (e.g., the theory of common descent) get the data (e.g., the determination of homologies) it is about to explain?

Both the elucidation of these problems and the provision of a compass that leads to their solution are side effects of the routine application of this approach.

3) For those interested in formal approaches as well as for those interested in pragmatic issues within science, to dive into crystalline structuralist waters is worth the effort.

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