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NOTES AND COMMENTS

Theoricity, observation and homology: a response to Pearson

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Abstract An interesting metatheoretical controversy took place during the 1980's and 1990's between pattern and phylogenetic cladists. What was always at stake in the discussion was not how work in systematics should be carried out, but rather how this practice should be metatheoretically interpreted. In this article, we criticize Pearson's account of the metatheoretical factors at play in this discussion. Following him, we focus on the issue of circularity, and on the role that phylogenetic hypotheses play in the determination of "primary homologies". Pearson argues that the recognition of primary homologies cannot be achieved without recourse to previous phylogenetic knowledge, and that to claim otherwise is to state that primary homologies are observable. To show why that view would be inadequate, he appeals to Hanson's views about theory-laden observation, alongside with a specific case study, which allegedly illustrates the more complex relation between observation and theory. We will argue that the pattern cladists' point (at least regarding the issue of homology) is better addressed by taking a quite different approach:

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instead of thinking in terms of observability, the topic can be tackled by paying attention to the way in which concepts are determined. We will take the notion of T-theoricity from metatheoretical structuralism and show that, once the issue is discussed with the appropriate metatheoretical framework, the alleged counterexample brought up by Pearson is not problematic at all for pattern cladism.

Keywords Cladistics · Pattern cladistics · Homology · T-theoricity · Metatheoretical structuralism · Evolutionary theory

1 Introduction

An interesting and important (though often misunderstood) metatheoretical controversy took place during the 1980's and 1990's between pattern and phylogenetic cladists. What was always at stake in the discussion was not how work in systematics should be carried out. After all, both positions agreed on how cladistic work ought to be done, and whichever position prevailed, the *practice* of phylogenetics and taxonomy would remain unchanged (cf. Platnick 1979, p. 538). However, they strongly differed in how to *interpret* this practice. The work of Pearson, which we discuss here, is valuable because it approaches the issue in the appropriate light, by defending the metatheoretical character of the discussion (Pearson 2010, pp. 479–480), instead of its metaphysical character (cf. Beatty 1982).

The pattern cladists' position involves several different theses, but Pearson focuses on the issue of circularity and the role that phylogenetic hypotheses play in the determination of "primary homologies" (de Pinna 1991). Pearson argues that the recognition of primary homologies cannot be achieved without recourse to previous phylogenetic knowledge, and that to claim otherwise is to state that primary homologies are observable (in the sense of the old theoretical-observational distinction). To show why that view would be inadequate, he appeals to Hanson's views about theory-laden observation, alongside with a specific case study (Smith and Wheeler 2006), which allegedly illustrates the more complex relation between observation and theory.

We will argue that the pattern cladists' point (at least regarding the issue of homology) is better addressed by taking a quite different approach: instead of thinking in terms of *observability*, the topic can be tackled paying attention to the way in which concepts are *determined*. Thus, the question to be answered is not "are primary homologies observable or theory-laden?", but "can primary homologies be determined independently of evolutionary biology?" We will take the notion of T-theoricity from metatheoretical structuralism and show that, once the issue is discussed with the appropriate metatheoretical framework, the alleged counterexample brought up by Pearson, is not problematic at all for pattern cladism.

2 Pattern cladism, homology and circularity

Pearson introduces the pattern cladist position as follows:

> Pattern cladists distinguish themselves from phylogenetic cladists by insisting that cladism does not require knowledge of the actual evolutionary histories of species to be classified. Instead, patterns of character traits by themselves are sufficient to determine the cladistic categorization of species, allowing the cladist to remain agnostic (at least in principle) on the particulars of evolutionary histories. (Pearson 2010, p. 476)

This characterization of pattern cladists is accurate. Again, the issue we will discuss is whether "primary homologies" (i.e. the construction of the data matrix) can be carried out without previous phylogenetic knowledge, independently of whether pattern cladism makes sensible claims about other (posterior) phases of cladistic analysis.[1]

Pearson also correctly recognized that having an adequate explication of "homology" is central for this debate:

> Pattern cladists will incline to emphasize a traditional approach to homology, solidified in the work of Richard Owen, who characterized homologues as "the same trait under a variety of forms and functions." […] Contrary to the traditional approach, much of contemporary biology, as well as philosophy of biology interprets homology as a historical concept. According to this approach, traits are homologous just in case they are derived from the same trait in a common ancestor. […] Much like the concept of "grandmother" or "adaptation," then, homology has an historical dimension to it. (*Ibíd*., p. 483-484)

The author also explains that the main reason to be careful with the concept of "homology" has to do with the recurrent problem of circularity.

> For pattern cladists, the source of the mistake in making homology an historical concept lies in its trading in the possibility of a theory-independent empirical basis for taxonomic classification for a theory circular classification. The theory circularity arises because homology is supposed to be evidence for certain evolutionary relationships between taxa. But, if homology conceptually packs in the historical relationship between taxa and their traits, the evidence that homology provides for evolutionary relationships is circular. (*Ibíd*., p. 484)

A similar point has been raised regarding "adaptation." Succinctly, if by "trait *x* is an adaptation" we understand "*x* is a product of natural selection," then natural selection theory cannot hope to non-circularly *explain* the presence of adaptations (i.e. natural selection theory would explain the possession of traits that are products of

[1] For example, there is also a discussion about whether or not the dichotomies on the optimal cladogram should be considered to represent speciation events. Although that is also an interesting side of the debate, our focus will be the issue of homology, which is the one Pearson centers on.

natural selection). Thus, as two concepts of homology are sometimes distinguished (Blanco 2012; de Pinna 1991), some authors (Gould and Vrba 1982) have also tried to distinguish between two concepts of adaptation.

Note how relevant the distinction between *explanation* and *determination* turns out to be (cf. Pearson 2010, p. 477). Pattern cladists would thus sustain that the kind of homology that functions as an input to cladistics (i.e. the characters in the data matrix) are *determined* or *identified* independently of evolutionary theory, although their presence can later on be *explained* by reference to evolution and common origin. Up to this point, we agree with basically everything that Pearson has stated. Our concern lies with another one of his contributions.

According to Pearson, the claim that characters and character states (i.e. primary homologies) are determined independently of phylogenetics means that pattern cladists are compromised with the view that "homology" is an *observational* concept, in the sense of the old theoretical-observational distinction. Thus, he claims that:

> Pattern cladists' aim to avoid circularity in their system of classification can hardly be faulted, and their turn to theory-neutrality to avoid circularity is certainly sensible. Yet, the view of scientific practice as being theory-neutral, secured by direct observation is at best a minority view within contemporary philosophy of science. In order to appreciate this point, it is worth making explicit that pattern cladists' view of science conforms neatly to core elements in positivist and early post-positivist characterizations of science. (*Ibíd.*, p. 485)

Then, he introduces the usual Hansonian arguments against the idea that there can be purely observational statements, and thus a firmly established empirical basis for scientific theories. He also attempts to illustrate this more complex relationship between theory and observation with a specific case study (Smith and Wheeler 2006), in which previous knowledge of the phylogeny of organisms led the authors to recognize certain traits as primarily homologous.

In the next section we argue, against Pearson, that to claim that a concept can be determined independently of a given theory does not amount to claiming that said concept is observational (that is, not loaded with *any* theory). To accomplish this, we will follow the way structuralism deals with the old problem of theoretical terms.

3 Structuralism and the T-theoricity criterion

Following the demise of logical empiricism, there have been some attempts to reformulate the theoricity criterion they had proposed. The structuralists (Balzer et al. 1987; Sneed 1971) have come up with a notion of theoricity that rests on three fundamental points.

First, for several reasons (cf. Bar-Hillel 1970; Hempel 1970; Lewis 1970; Putnam 1962) they reject the distinction between theoretical and observational concepts, and adopt the theoretical and non-theoretical one, leaving aside observation as a criterion for this demarcation.

Second, this new theoretical/non-theoretical distinction is not absolute, but relativized to particular theories. That is, a given concept can be theoretical in one theory and non-theoretical in another theory.

Third, structuralists say that a concept C is T-theoretical (theoretical for theory T) if and only if every way of determining C presupposes T; and it is T-non-theoretical if and only if it is not T-theoretical (i.e. if there exists a way of determining C that is independent of T). Determining a concept means finding its value (if the concept is quantitative) or its extension (if it is qualitative). A determination of a concept C presupposes a theory T if and only if one applies the laws of T to find the value (or the extension) of C.

An example may help to clarify this point. The concept of "acceleration" can be determined independently of Classical Particle Mechanics (CPM hereafter), and hence, is CPM-non theoretical. This is so because one can measure accelerations without utilizing the laws of CPM. Such independent determinations of accelerations are what allow us to *test* CPM. A test of a theory consists, precisely, in testing whether a theoretical determination of a concept (for instance, by finding the values for the net force and mass of a particle and solving for a in $F=ma$) is equal—or coincides to some approximate degree—to the value obtained via its non-theoretical determination. For example, suppose that the laws of CPM predict that a given particle will accelerate at 2 m/s^2 downwards (a CPM-theoretical determination of acceleration). If we measure the acceleration of the particle independently of these laws (but perhaps not from *every* law) and find that it effectively moves with that acceleration, then we have a case that confirms CPM.

In this sense, it can be said that the concept of acceleration belongs to the "empirical basis" of CPM (note that the empirical basis is not necessarily describable only by observational concepts). The concepts that form the empirical basis of any theory are those that are non-theoretical for that theory. Since they can be determined independently of it, they can be used to test it. This does not mean that they are *absolutely* theory-free, but that they are free from the theory under test. Thus, they can be "loaded with theory," but only with *other* theory/ies. Furthermore, the structuralists have argued (and illustrated, with multiple case-examples) that the concepts that figure in the *explanandum* of any given theory are always non-theoretical for that particular theory, thus avoiding the self-justification problem that Hanson posed.

In order to discuss Pearson's point, it is important to remark, once again, that T-non-theoretical concepts can, many times, *also* be determined theoretically (as illustrated above, this is just what happens when a theory is tested). Furthermore, that a concept is T-non-theoretical does not imply that said concept can be determined independently of T *in every application* of that theory. There are particular applications in which an independent determination is impossible (i.e. not every application of a theory allows us to test that theory). Following up on the previous example, in some cases, it may be impossible to determine the acceleration of a specific particle independently of CPM. In that case, if one knows the mass and net force to which the particle is subjected (for example, from a previous application of the theory in question), its acceleration can only be found by the application of CPM.

Thus, the pattern cladists' thesis (regarding homology) is equivalent to the claim that the concept of 'character state distribution' (i.e. a set of primary homologies) is cladistics/evolutionary-non-theoretical. And in claiming this, we believe that they are obviously right. In the vast majority of systematic studies, the data matrix is constructed independently of the analysis of those data. If the characters in question are morphological, the criteria effectively used are those that authors such as Saint-Hilaire and Owen proposed, and that Remane (1952) synthesized, namely: topology, special quality or composition, "intermediate" forms, etc. If the characters are molecular (genetic or aminoacid), then various forms of alignment of sequences are used (which might perhaps be thought of as cases of Remane's criteria), again, *prior* to the realization of the phylogenetic analysis itself.

To be fair, Pearson does claim, later on (though *after* giving the Hansonian argument cited before), that:

> [Another misunderstanding] concerns the potential philosophical dismissal of pattern cladism on the basis of an otherwise unspecified claim that observation is always theory-laden. [… T]his idea is superficial to the point of making a strawman of pattern cladism. Pattern cladists need not feel threatened by arguments showing that observation is theory-laden, since it is not theory in general that pattern cladists typically seek to purge from cladistic taxonomy, but only evolutionary theory.[… W]ith respect to our central issue of homology, pattern cladists will likely embrace thinking of their observations of traits as informed by theoretical considerations from, say, functional morphology. Patterns in nature will be recognized as patterns only if the observer is armed with the relevant theory to recognize them as patterns. […] What is at issue, then, is not whether observations regarding patterns that determine homology are theory laden, but whether observations of homology are inextricably connected to evolutionary theory in particular (*Ibíd*, p. 486)

In his reply to this point, Pearson offers an example in which previous (partial) knowledge of the phylogeny of the terminal taxa at stake was relevant for the establishment that certain trait is (primarily) homologous to certain others. The example consists in a case in which the knowledge of the phylogeny allowed the researchers to identify certain structure as a venom gland, in a group of fish in which the venom gland seemed to be absent.[2] That is, it allowed them to apply the concept of "primary homology" via prior knowledge of the phylogeny. However, in light of the distinction presented in this section, we may now see that this reply is irrelevant to the truth or falsity of the claims made by pattern cladists. This is so because nothing prohibits that a non-theoretical concept is determined theoretically in some cases.

[2] As Pearson explains (p. 487), the goal of Smith and Wheeler's paper was not to identify homologies, but rather to use a molecular phylogeny as a predictive guide to the number and characteristics of venomous fishes (we thank an anonymous reviewer for pointing this out to us). Furthermore, the venom gland and delivery-system was not homologous among every taxon they studied. The passage that Pearson quotes, and that we discuss here, refers only to three genera, and is a side-remark made by the original authors.

Consider an imaginary situation with two groups of physicists: “Pattern mechanicists” and “Mechanical mechanicists.” Pattern mechanicists argue that the concept of “acceleration” is “free from CPM,” while mechanical mechanicists argue that this is not the case. The second group presents as an argument against the first a case study in which accelerations are determined using the laws of classical mechanics. But this kind of argumentation is inadequate. If the argument of the pattern mechanicists made any sense, their claim that accelerations are “free from mechanical theory” was not about the *impossibility* of determining accelerations *with* the resources of classical mechanics. Rather, it was about the *possibility* of determining them *without* those resources. The fact that this can be done is what allows classical mechanics to explain the accelerations of particles in a non-circular way. Similarly, the fact that distributions of primarily homologous characters can be determined independently of phylogenetics is what allows phylogenetics (i.e. the reconstruction of the diversification pattern of species) to explain the distribution of “observed” traits without the risk of circularity.

In consequence, *at best*, the example Pearson brought up would be nothing more than one in which a concept (primary homology) was determined cladistics/evolutionary-theoretically (and in which, at best, the concept could not have been determined otherwise). As said before, this would be of no help when trying to decide if such concept is cladistics/evolutionary-non-theoretical, i.e. if the concept is independent of cladistic and/or evolutionary analysis, in a sense relevant for our discussion.

However, we think that Pearson’s example does not even amount to this. Strictly speaking, the example he introduced does not count as a cladistics/evolutionary-theoretical determination of a column in the data matrix.[3] In this case, knowledge of the phylogeny of the fish in question led scientists to a more careful application of the classical criteria for identifying primary homologies. That is, the knowledge of phylogeny of these fishes fulfilled a heuristic function. What finally counted for the determination of homology was a tissue (i.e. composition) criterion, a non-evolutionary dependent one (cf. Owen 1849):

> Our examination of six species in these three genera indicates that anterolateral grooves are present in all six species, but conspicuous venom glands associated with these grooves are lacking. However, the caudal margin of their fin spines have conspicuous glandular tissue […] that differs significantly from the typical muscle tissue found on the posterior margin of the spines in most nonvenomous spiny-rayed fishes […]. We tentatively identify this structure as a venom gland, pending further study. (Smith and Wheeler 2006, p. 213)

[3] Perhaps a more convincing example of a cladistics-theoretical determination of part of a data matrix could come from direct optimization/dynamic homology approaches (Wheeler, 1996; Ramírez, 2007 presents a similar framework for morphological characters). In Wheeler’s approach, the sequences given to the phylogenetic analysis are not *a priori* aligned; rather, alignment occurs within the cladistic analysis itself. In Ramirez’s case, cladistic analysis is what allows the researcher to decide between different possible primary homology schemes. Even then, as noted above, cases such as these would not count against the non-theoreticity of the concept of “character in a data matrix.”.

4 Conclusion

Even if Pearson's approach is correct when treating this interesting debate from a metatheoretical standpoint, his reference to Hanson does not shed light on the debate. The pattern cladists' proposal of the independence of the notion of primary homology from cladistics has more to do with how concepts are determined than with issues related to observation. When the topic is considered with the right meta-theoretical lens, the alleged counterexample to the pattern cladists' view proposed by Pearson weakens, and the point defended by the pattern cladists is strengthened. To better evaluate the adequacy of their claims (regarding homology), the views of the pattern cladists should be re-described in terms of their cladistics/evolutionary-theoreticity.

More generally, it is crucial for the empirical status of any theory that its *explanandum* can be determined without the application of its own laws. Of course, a different theory might well be involved in that determination, but that does not lead to an epistemological problem. Thus, and as a corollary for our particular case, it is of the highest importance to justify that pattern cladists are correct regarding the alleged evolution/cladistics independence of the determination of (primary) homologies.

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