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The case of early Copernicanism: epistemic luck *vs*. predictivist vindication

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Introduction: The epistemic luck thesis

From Copernicus himself up to Kepler and Galilei, Copernicans have been "right for the wrong reasons" (Finocchiaro 2010), because *there were* no epistemically compelling reasons objectively favoring the Copernican position at that stage – a good deal of research in the history and philosophy of science has converged on this claim. In the jargon of contemporary analytic epistemology, the situation of early Copernicans would then be regarded as one of *epistemic luck*. Roughly, epistemic luck characterizes an agent who happens to have a true belief without adequate justification.[1] The precise scope of the *epistemic luck thesis* about early Copernicanism may vary significantly. For our present purposes, it is safe to focus on a version of the thesis which appears particularly safe and popular. According to such version, Copernicanism has been a matter of epistemic luck *at least* from 1543 (the publication of Copernicus's *De Revolutionibus*) up to, say, 1600, namely a moment in which the Copernican allegiance of both Kepler and Galilei is already documented while their own scientific achievements in astronomy were yet to come. Some authors would be happy to say that Copernicanism eventually got to be vindicated with Newton, as it was subsumed under a more comprehensive theory of unrivalled success (e.g., Salmon 1990, p. 190). Others might want to insist that heliocentric astronomy remained ultimately unsteady until more "direct" and "physical" evidence of the Earth's motion became available in the XVIII and XIX centuries (see Graney 2015, ch. 10).

The epistemic luck thesis may perhaps look puzzling to those who are not much versed in the specialized scholarship. After all, its impact on textbooks, encyclopedias, and the general culture

[1] On the assumption that Copernicanism is fundamentally correct, the most relevant specification is probably *veritic* (epistemic) luck: "a person *S* is *veritically lucky* in believing that *p* in circumstances *C* iff, given *S*'s evidence for *p*, it is just a matter of luck that *S*'s belief that *p* is true in *C*" (Engel 2022, p. 36).

has been limited. The celebration of Copernicus, Kepler, and Galilei as founding figures for modern science has survived the Twentieth century largely unscathed and remains prevalent to this day. However, the presence of the epistemic luck thesis among experts actually has a sensible explanation. An advocate of the epistemic luck thesis would typically argue along the following lines, with a remarkable combination of historical and philosophical insights. First, they would note that each major phenomenon about the heavens that was known around 1600 and could be logically derived from the Copernican system could also be derived from an alternative, geostatic system, and viceversa (of course, with relevant background assumptions). They would go on to point out that, therefore, the Copernican view could not be distinguished from competing approaches on the basis of the available empirical evidence. As concerns further potentially discriminating criteria ("simplicity" is of course a recurrent example), they would deny that such criteria could have served as an effective basis to favor Copernicanism as objectively and epistemically superior to its competitors at least at the time when Kepler and Galilei decided to join the Copernican camp (pointing out, for instance, that minor epicycles remained a customary and pervasive device in Copernicus' work, or that other virtues would support geocentrism instead).

The textual evidence about the popularity of the epistemic luck thesis is sparse but consistent, spanning now more than a century. According to Pierre Duhem's thoughtful discussion in *To Save the Phenomena*, a considered attitude of antirealism fostered by the astronomical tradition led competent observers such as Andreas Osiander and Cardinal Bellarmine to duly appreciate that heliocentric and geocentric systems were empirically on a par at the time, and therefore scientifically on a par too. As Duhem famously and firmly concluded, we are "compelled to acknowledge and proclaim that logic sides with Osiander, Bellarmine, and Urban VIII, not with Kepler and Galilei – that the former had understood the exact scope of the experimental method and that, in this respect, Kepler and Galilei were mistaken" (Duhem 1908, p. 113). Fifty years on, another seminal reference is of course Thomas Kuhn. In a key passage of *The Copernican Revolution*, he notes that "each argument" originally put forward by Copernicus "cites an aspect of the appearances that can be explained by *either* the Ptolemaic *or* the Copernican system". The insistence of Copernicus on the greater "harmony" of heliocentrism, Kuhn points out, could only be appealing to a "limited and perhaps irrational subgroup of mathematical astronomers". Only in hindsight can one appreciate that some of them "fortunately" did follow their "Neoplatonic ear" (Kuhn 1957, p. 181). And a major theme of Kuhn's view of science is of course that one should strenuously resist turning the benefit of scientific hindsight into a form of hindsight bias in historical matters.

Notably, unlike other implications of Duhem's or Kuhn's work, the epistemic luck thesis about early Copernicanism does not seem to have lost ground over time.[2] As recently as 2011, historian Robert Westman introduced his impressive reconstruction of *The Copernican Question* noting that "Copernicus had opened a question […] which previously had not been seen to possess far-reaching consequences: how to choose between different models of heavenly motion *supported indifferently* by the same observational evidence" (Westman 2011, p. 5, emphasis added). Recent extensive work on anti-Copernican astronomy *after* Kepler and Galilei (especially the interesting case of Riccioli 1651) yielded even stronger claims, if anything. According to Graney, for instance, "in the middle of the seventeenth century […] science backed geocentrism" (Graney 2015, pp. 144-145; and also see Marcacci 2015). As for late Twentieth century philosophy of science, Wesley Salmon provides a striking example: "until Newton's dynamics came upon the scene, it seems to me, Thyco's [geostatic] system was clearly the best available theory" (Salmon 1990, p. 190). And physicists themselves are apparently no exception: according to Carlo Rovelli, for instance, "Kepler trusted Copernicus' theory *before* its predictions surpassed Ptolemy's" (Rovelli 2019, p. 120; also see Timberlake and Wallace 2019, pp. 144-145).

Imre Lakatos and Elie Zahar have featured in one relatively rare contemporary episode of sustained opposition to the epistemic luck thesis about early Copernicanism. Although known and appreciated in certain philosophical circles, it is fair to say that their attempt has remained quite unsuccessful. In essence, the goal of this paper is to revive it.

Background and outline

Lakatos and Zahar (1975) carried out a comprehensive methodological assessment of the Copernican revolution. In their view, Copernicus' programme had a remarkable amount of "*immediate* support" from known phenomena that was not matched by the traditional geostatic approach, even if both parties were able to account somehow for all essential facts established in the late Sixteenth century. This showed that "there were good objective reasons for Kepler and Galilei to adopt the heliostatic assumption" (p. 188). Such claim, I submit, clashes with the epistemic luck thesis. According to Lakatos and Zahar's analysis, it is not the case that Kepler and Galilei (not even Copernicus, in fact) were "right for the wrong reasons", or at least not because good reasons were lacking. To this extent, given the information that was actually available in the relevant historical context, it was *not* just a matter of luck that their theoretical

[2] Swerdlow (2004) seems to offer a forceful but occasional exception: "there is altogether too much literature today — ultimately, I think, inspired by Duhem and his nonsense about 'saving the phenomena' — that holds that Copernicus had no good reasons to believe his theory to be a true description of the world. He had very good reasons and quite a lot of them." (p. 88)

allegiance turned out to be correct. It was instead a matter of plausible epistemic justification through sound scientific methodology. Let us call this the *vindication thesis*.

Lakatos and Zahar's analysis did not remain unchallenged, however. According to Thomason (1992), in particular, their methodological reconstruction can not justify the desired conclusion. Given that no articulated criticism of Thomason (1992) has emerged in more than thirty years, one is tempted to see such apparently old-fashioned controversy as settled, and perhaps too arcane to be reconsidered anyway. Yet this temptation should be resisted, I submit, if one is concerned about the epistemic luck thesis. In fact, Thomason did not challenge Lakatos and Zahar's dismissal of alternative philosophical reconstructions of the Copernican arguments. This, in turn, leaves the early Copernicans' choice with no plausible methodological justification. Moreover, there is at least one major reason why this case deserves a fresh look: in fact, the arguments in the debate may well need to be assessed anew as concerns their philosophical grounds. Let us briefly see why.

Both Lakatos and Zahar (1975) and Thomason (1992) relied on Lakatos's methodology of scientific research programmes as integrated by Zahar's "new conception" of "novel fact" (Lakatos and Zahar 1975, p. 185). Such conception had been put forward in Zahar (1973) as a solution to what arguably is the first clear statement of the "problem of old evidence", drawing from the now classic historical case of Einstein's general theory of relativity and the Mercury perihelion. But a good deal of additional work has been done meanwhile on this crucial topic (see Barnes 2022 for a valuable survey). This raises the question whether the integration of an updated analysis of the accommodation *vs*. prediction distinction may help revamp Lakatos and Zahar's original verdict and subvert the premises of the epistemic luck thesis, thus vindicating the vindication thesis.

The rest of this contribution is organized as follows. First, we will have to set the stage for an assessment of Lakatos and Zahar's line of argument in updated form. This will include a revised discussion of the use-novelty of empirical facts in science, which actually amounts to a relatively new tentative demarcation between empirical success and mere accomodation of known phenomena. The next section will lay out such proposal and also provide a characterization of the two contenders, namely, Copernicanism and Sixteenth century geocentrism. The methodological exercise will then ensue, outlining a replay of the original debate between Lakatos and Zahar and Thomason. The results will lead us to explore some implications of the vindication thesis and how it arguably sheds light on certain important methodological remarks to be found in the writings of leading figures of the Copernican controversy. In the last part of our discussion, however, we will see how the case of *ancient* heliocentrism may represent a significant, if indirect, challenge to vindicationism. This issue does not seem to have received due attention in the philosophical literature, but it may turn out to be no less serious and interesting than more familiar topics of contention.

Logical predictivism

Let *S* be a set of empirical findings established by scientific observation and let *T* be a theory (virtually *any* theory) postulating principles, structures, and processes underlying the "phenomena" encoded in *S*. As it turns out, it is a crucial fact of the philosophical analysis of science that, as a matter of logic, it will always be possible to derive all elements in *S* as consequences of a "theoretical cohort" integrating *T* with a relevant set of auxiliary assumptions. But this means that an alternative theory *T** could also be aligned with *S* in the same way, namely as embedded in a suitable theoretical cohort.[3] Duhem (1906) is of course a seminal source for this paramount methodological circumstance (see Laudan 1990, p. 274, for a more recent statement), which also serves as an undisputed starting point for Lakatos and Zahar (1975). As they say, "any two rival research programmes can be made observationally equivalent by producing observationally equivalent falsifiable versions of the two with the help of suitable *ad hoc* auxiliary hypotheses" (p. 180).[4] *Duhemian corollary* will work as a convenient shorthand for this statement. Zahar's "new conception" of "novel fact" was meant to go beyond this kind of "uninteresting" empirical equivalence and to specify how the same evidence may still give more support to one theory against another "depending on whether the evidence was, as it were, 'produced' by the theory or explained in an *ad hoc* way". In what follows, much in line with important work by Worrall (2002, 2006), I will employ a minimal implementation of use-novelty which — unlike Zahar's (1973) — squarely avoids reliance on dubious psychological and historical contingencies such as "the reasoning which [the scientist] used to arrive at a new theory" (p. 219). Consider the following, admittedly basic, characterization of an observable fact *F* as *strongly* confirming a scientific theory *T*:

(a) there exist other observable facts, *E*, such that *F* follows from *T* and *E*; but
(b) *F* does *not* follow from *T* alone; and
(c) *E* and *F* are logically independent.

[3] One such expanded set including theoretical principles and various auxiliary assumptions is sometimes just called a "system". "Theoretical cohort" is a nice terminological variant which I draw from Strevens (2020).

[4] Here, by "observationally equivalent" one should read "such that all *known* observable facts are accounted for by each theory as embedded in its own theoretical cohort".

Each one of clauses (a)-(c) should be meant to apply on the background of further contextually unchallenged assumptions.[5] On this basis, there are two key scenarios in which a researcher will be able to conclude that T is strongly confirmed by F. One amounts to purely temporal novelty: the elements in E happen to be already known at a given moment, F is logically derived and *then* established by observation. (In an experimental setting, for instance, the facts in E will typically reflect certain conditions that have been purposely designed and realized in order to check for the occurrence of F, which is expected under those conditions on the basis of T, and ideally not otherwise.) But a situation in which *both* E and F happen to be known is just as much compatible with the fulfilment of (a)-(c), and it arguably captures the idea of so-called *use-novelty*.[6] In Zahar's original cornerstone case, for instance, observationally established facts about the solar system turn out to be sufficient and non-redundant to derive from Einstein's theory of general relativity the already known and otherwise independent fact of Mercury's precessing perihelion and its observable consequences. As all three clauses above are satisfied in this case, evidence about Mercury's perihelion qualifies as an empirical success of the theory regardless of whether Einstein himself may have hoped or even planned to address that problem better than it was handled by classical Newtonian means (see Earman and Janssen 1993 for a thorough reconstruction). Another related way to look at clauses (a)-(c) is to see them as implying $T \vDash E \supset F$ but ruling out each of $\vDash E \supset F$, $T \vDash E$, and $T \vDash F$. This may be regarded as a situation in which the *connection itself* between E and F is made sense of by T, not the brute fact of their joint occurrence.[7]

[5] The historical evidence in the philosophy of science suggests that a definition of this kind must be liable to charges of triviality. What if T amounts to the combination of $E \supset X$ and $X \supset F$ for arbitrary X, for instance? Here I will not try to develop a formal treatment to neutralize all such frivolous counterexamples (although a subtle potential triviality objection raised by Jason Alexander helped me with the formulation of clause (c)). They will be of no consequence for the subsequent discussion, however. In all cases of interest for us, T will include categorical and unverifiable claims about the world (such as "the Earth revolves around the Sun") that are relevant in the derivation of F from T and E. See Lange's (2004, p. 208) objection to Myrvold (2003) for a related debate.

[6] As far as I can tell, a confirmation theorist who relies on (a)-(c) will elude all troubles raised by Votsis (2014) for "incidental predictivists". Consider the potentially problematic hypothetical case of two scientists A and B such that A derives known fact X from T and known fact Y whereas B derives known fact Y from T and X. If clauses (a)-(c) are satisfied in both cases, my proposal implies that *both X and Y* strongly confirm T. So Votsis's objections do not seem to apply here (2014, pp. 75-76).

[7] As concerns clauses (a)-(c) themselves, I'm really not claiming much originality. In Niiniluoto's (2016) terminology, for instance, the fulfilment of (a)-(c) implies that T achieves "deductive systematization" or complies with a "linking up" variant of the notion of "unification" with regards to E and F. Similar conditions have been also employed to explicate Whewell's celebrated idea of "consilience": see McGrew (2003) and Myrvold (2003). Also see Alai (2014) for a related discussion and proposal.

To be sure, this characterization is fully consistent with the Duhemian point that virtually any theory can be tailored and refined to recover known phenomena such as *E* and *F* (see Crupi 2021), and it is also consistent with the idea that verified observable consequences, even if merely accommodated, can still provide *weak* support for a theory. However, the fact that a key piece of theory (roughly, a Lakatosian hard core, or part thereof) enables the derivation of some of the available evidence from other independent parts of it is arguably contingent on what the theory actually says and is taken as a distinctive element of empirical success. An analogy with evidential reasoning in statistical settings may be helpful. Surely a good measure of fit between, say, a linear model and a relevant data set speaks in favor of a linear interpretation of the underlying process at least to some extent. However, the more stringent demand of so-called *cross-validation* is routinely applied to guard against "overfitting", namely to go beyond the limited support that mere accommodation can provide. If a subset of the data constrains a specification of the model parameters which in turn fares well on a separate subset, the support achieved is taken as clearly stronger (see Schurz 2014, p. 92, for a similar remark).

A reconstruction reconstructed

An updated account of use-novelty is the first step in my project to recast Lakatos and Zahar's (1975) analysis in a new form, and to counter Thomason's (1992) criticism. The second step needed is of course a characterization of the theories to be compared. Here, the heliocentric "rough model" or framework (the Lakatosian core of Copernicanism, as it were) will be meant as implying the following claims:[8]

(i) the Sun is stationary;
(ii) the sphere of the fixed stars, centered (approximately) in the Sun, is at rest;
(iii) the Earth revolves around the Sun;
(iv) the Earth rotates around its own axis;
(v) the Moon orbits the Earth (closely);
(vi) planets other than the Earth also revolve around the Sun;
(vii) planets are ordered from the center outward by (strictly) increasing revolution periods.

[8] My reconstruction here is largely consistent with Lakatos and Zahar's (1975) and similar to Carman's (2018). Point (vii), in particular, is explicitly stated early on in the *Commentariolus* as a basic feature of the heliocentric system. In *De Revolutionibus* (Book I, Chapter X), it is presented as the consequence of more fundamental assumptions that are shared through the astronomical tradition (also see Lakatos and Zahar 1975, p. 185). This elucidation was prompted by a remark from John Worrall.

As concerns the core commitments of the Ptolemaic approach, here is a fitting list for our purposes:

(i*) the Earth is stationary;
(ii*) the sphere of the fixed stars revolves around the central Earth;
(iii*) the Sun revolves around the Earth;
(iv*) all planets (including the Moon) revolve around the Earth with a combination of (few) circular motions;
(v*) heavenly bodies are ordered from the outer sphere inward by decreasing overall rotating speed.

Of course, (i*)-(v*) are all consequences of the *full* Ptolemaic theory that was taught in the schools in Copernicus's time including the sophisticated machinery of deferents and epicycles as appropriately specified.

A key complaint by Thomason (1992) is that Lakatos and Zahar (1975) "ignored Tycho Brahe" (p. 161). The claim is not unfounded at first sight, for in Lakatos and Zahar (1975) Brahe's case is taken on in the criticism of competing methodological reconstructions (especially the falsificationist and simplicist), but otherwise relegated in minor footnote remarks. Indeed, a reference to Tycho Brahe's theory occurs almost invariably whenever the epistemic luck thesis arises. It is therefore crucial to emphasize that in the current context Brahe's model is nothing but a specification of the core claims (i*)-(v*) above and indeed a rather minor variant of the more traditional, full Ptolemaic system. In fact, for *any* "planet", the *actual trajectory* postulated by Brahe *around the (stationary) Earth* is demonstrably identical to the corresponding Ptolemaic trajectory. The only caveat is that the Sun is not always further away than Mercury and Venus, but rather at the center of their epicycles. This difference is of course interesting but immaterial for all astronomical evidence available between *De Revolutionibus* and Galilei's discovery of Venus's phases (1610), and thus immaterial for our purposes too. In this perspective, to describe the Thyconic system as a "mixed" model, "combining" Ptolemy and Copernicus is quite misleading. At least in terms of Lakatos and Zahar's methodological question about "immediate support" favoring Copernicus's theory, one could just as well regard Brahe's theory as "Ptolemaic" throughout – or, equivalently, consider Brahe's theory as *the* key geocentric counterpart of Copernicanism at least by the time of Kepler and Galilei.[9] In any event, the post-Lakatosian

[9] See Margolis (1991) for a revealing independent argument supporting this move on purely historical grounds.

reconstruction outlined above thoroughly includes the Tychonic system as a specific model entailing the pillars of Ptolemaic geocentrism (i*)-(v*).[10] Let us now check the implications.

Fact 1: *Stations and retrogressions are observed for each of Mercury, Venus, Mars, Jupiter, and Saturn*. Thomason (1992) questions that this major point from Lakatos and Zahar (1975, p. 185) may strongly support the Copernican framework on the grounds that, historically and psychologically, Fact 1 (a "*dominant* problem in Western astronomy", p. 182) was something that Copernicus definitely *did* want to account for when devising his theory. By our criterion of empirical success (as distinct from accommodation), this is irrelevant, however. Logically, as soon as observational evidence *E* indicates the non-redundant fact of the very existence of a (Copernican) planet (i.e., a major heavenly body other than Moon, Sun, and fixed stars), the Copernican framework (i)-(vii) immediately entails Fact 1 as concerns that object. On the other hand, Fact 1 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with *E* or any other independent observable fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 1 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated. As a consequence, Fact 1 does provide strong and immediate support to the Copernican position against the Ptolemaic approach in our revised reconstruction.[11]

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[10] One objection here might be that the Tychonic model cannot entail (iv*) just because for Tycho the Sun is the center of simple circular epicycles for each planet. I take this to be an inconsequential semantic issue, however. In both (i)-(vii) and (i*)-(v*), I employ "to revolve" to denote a periodic motion around a stationary center. This is quite consistent with the planets "orbiting" the Sun for Tycho, much as the Moon orbits the Earth for Copernicus. Once this innocent stipulation is clarified, I submit that the traditional Tychonic model does verify (i*)-(v*). In any event, regardless of the terminological preferences, the fact remains that one can turn a full classical Ptolemaic model into a full Tychonic model by just moving the Sun to the center of Mercury's and Venus's (embedded) epicycles, and leaving all actual trajectories otherwise unaltered. (I thank José Díez for pressing me on this point.) Note that such variation is no larger departure from Ptolemy's original full theory than the "inverted-direction epicycle" approach, which is thoughtfully discussed by Carman and Díez (2015, pp. 30-31) and explicitly and sensibly taken to belong squarely to the Ptolemaic tradition.

[11] Fact 1 is a qualitative statement. However, in an insightful footnote (n. 19, p. 181), Thomason (1992) makes a striking observation concerning a more quantitative aspect of these phenomena: in the Copernican approach, the appearance of retrograde motion for superior planets such as Saturn can be large enough to be easily detected only in presence of a "considerable gap" with the fixed stars. The fascinating implication is that, conversely, the observable amplitude of the retrogressions of superior planets may be a basis for a Copernican to infer a large distance of the fixed stars. This in turn would potentially make an empirical

Fact 2: *Mercury and Venus are never seen to go in opposition*. Thomason does not address this point from Lakatos and Zahar (1975, p. 186), but he could have easily objected that, here again, Fact 2 was an established phenomenon that Copernicus did want to account for when devising his theory. Yet Fact 2 is entailed by the Copernican framework (i)-(vii) along with observational evidence *E* such as a small observed interval between two successive conjunctions (less than a year) for Mercury and Venus, implying the non-redundant statement that both planets are internal. On the other hand, Fact 2 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with *E* or any other independent observable fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 2 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated. As a consequence, Fact 2 does provide strong and immediate support to the Copernican position against the Ptolemaic approach in our revised reconstruction.

Fact 3: *Mercury's retrogressions are seen to be more frequent than Venus's*. Thomason (1992) addresses a closely related point from Lakatos and Zahar (1975, p. 186) and questions that it may strongly support the Copernican approach for "it seems plausible to hold that [it] played some role guiding Copernicus to the view that the Sun was in the center of the planets' orbits" (p. 185). Yet Fact 3 is entailed by the Copernican framework (i)-(vii) along with evidence *E* such as a smaller observed interval between two successive conjunctions for Mercury than for Venus, implying the non-redundant statement that the former must be the innermost internal planet. On the other hand, Fact 3 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with *E* or any other independent observable fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 3 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated. As a consequence, Fact 3 does provide strong and immediate support to the Copernican position against the Ptolemaic approach in our revised reconstruction.

Fact 4: *Intervals between successive conjunctions are smaller for Mercury than for Venus.* This point is not addressed by either Lakatos and Zahar (1975) or Thomason (1992), but it is of interest in our perspective. We have seen that observational information about successive conjunctions can complement the Copernican framework (i)-(vii) entailing the ordering of internal planets, by which Fact 3 can then be derived. In addition, this situation is largely symmetric: indeed, Fact 4 is entailed by the Copernican framework (i)-(vii) along with *E* now meant as known observable facts mentioned above. More precisely, because Mercury and Venus are never seen to go in opposition (Fact 2), the theory entails that they must be internal planets, and because retrogressions are seen

success of a fact that no vindicationist seems to have ever dared to classify as more than a (reasonable) accommodation, namely the failed detection of stellar parallax (see, e.g., Worrall 2002, p. 198).

to be less frequent for Venus than for Mercury (Fact 3), the latter must be the innermost, with a shorter orbital period and thus more frequent conjunctions. On the other hand, Fact 4 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with either *E* or any other independent fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 4 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated.

Fact 5: *The length of Venus's retrograde arc is seen to be greater than Mercury's*. This is a case that Thomason himself allows as use-novel for Copernicus (from *De Revolutionibus*, Book I, Chapter X) because, although of course known, it does "not seem obviously relevant to the structure of the cosmos" (1992, p. 188), and thus to the guiding explanatory aims of Copernicus' inquiry. In our perspecrive, Fact 5 is entailed by the Copernican framework (i)-(vii) along with observational evidence *E* such as the interval between two successive conjunctions and relevant angular measurements implying a non-redundant assessment of the magnitude and period of Mercury's and Venus's motion as referred to the Sun. On the other hand, Fact 5 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with *E* or any other independent observable fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 5 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated.

Fact 6: *Mars, Jupiter, and Saturn are all seen to always retrogress at opposition*. Fact 6 is considered but dismissed by Thomason (1992, p. 188). Yet Fact 6 is entailed by the Copernican framework (i)-(vii) along with known evidence *E* such as the observation of a quadrature for each of Mars, Jupiter, and Saturn implying the non-redundant fact that all three planets are external. On the other hand, Fact 6 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with *E* or any other independent observable fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 6 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated.

Fact 7: *Jupiter's retrogressions are seen to be more frequent than Mars's, and Saturn's more frequent than Jupiter's*. This point (from *De Revolutionibus*, Book I, Chapter X) is not addressed by either Lakatos and Zahar (1975) or Thomason (1992). Fact 7 is entailed by the Copernican framework (i)-(vii) along with evidence *E* such as a larger observed interval between two successive conjunctions for Mars than for Jupiter, and for Jupiter than for Saturn (all of which greater than a year), implying the non-redundant statement that Mars must be the innermost external planet, and Saturn the outermost. On the other hand, Fact 7 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with *E* or any other independent observable fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 7 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated.

Fact 8: *Intervals between successive conjunctions are smaller for Saturn than for Jupiter, and smaller for Jupiter than for Mars.* This point is not addressed by either Lakatos and Zahar (1975) or Thomason (1992), but it is of interest in our perspective. We have seen that observational information about successive conjunctions can complement the Copernican framework (i)-(vii) entailing the ordering of external planets, by which Fact 7 can then be derived. In addition, this situation is largely symmetric: indeed, Fact 8 is entailed by the Copernican framework (i)-(vii) along with *E* now meant as known observable facts mentioned above. More precisely, because Mars, Jupiter, and Saturn are all seen to go in opposition (Fact 6), the theory entails that they must be external planets, and because retrogressions are seen to be less frequent for Mars than for Jupiter, and for Jupiter than for Saturn (Fact 7), the former must be the innermost and the latter the outermost, with decraeasing orbital periods and thus increasingly frequent conjunctions. On the other hand, Fact 8 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with either *E* or any other independent fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 8 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated.

Fact 9: *The length of Mars' retrograde arc is seen to be greater than Jupiter's*, *which is seen to be greater than Saturn's*. Thomason pairs this with Fact 5 as use-novel for Copernicus (1992, p. 188). In our perspective, Fact 9 is entailed by the Copernican framework (i)-(vii) along with observational evidence *E* such as the interval between two successive conjunctions and relevant angular measurements implying a non-redundant assessment of the magnitude and period of Mars's, Jupiter's, and Saturn's motion as referred to the Sun. On the other hand, Fact 9 does *not* follow from core Ptolemaic assumptions (i*)-(v*) as conjoined with *E* or any other independent observable fact, so in this case clause (a) is violated. Of course, according to the Duhemian corollary, Fact 9 follows from a full Ptolemaic theoretical cohort (Brahe's is one example), but then clause (b) above is violated.

Although surely incomplete, the reconstruction above concerning facts (1)-(9) is sufficient to license a key conclusion for our purposes: according to our characterization of empirical success (which recovers Zahar's original motivation, as illustrated by the Einstein/Mercury example), and despite the uncontested truth of the Duhemian corollary, the Copernican view was indeed "immediately supported" by various known facts which did not support geocentric competitors in the same way.[12] It should be clear – but it's worth emphasizing – that this conclusion relies on a broadly Lakatosian distinction between core *vs*. full models.[13] Again following Lakatos and Zahar, I'm not committed to deny the ("uninteresting") traditional remark that, unlike core models, full models of either strain (heliocentric or geocentric) with all their parameter values specified end up being empirically indistinguishable around 1600. In particular, one can see that, for all of them, clause (b) of my criterion of strong support is invariably violated.

Methodological issues in Clavius *vs*. Kepler and Galilei

Strictly speaking, Lakatos and Zahar (1975) did not want to commit to any specific account of "why Kepler and Galilei actually became 'Copernicans'" (p. 188, footnote 1). This notwithstanding, it seems clear that at least some methodological issues did play a role in the arguments and choices of major figures in the Copernican controversy. Consider for instance the following important quote from a most distinguished post-Copernican astronomer of geocentric allegiance, German Jesuit Christopher Clavius (1538-1612):

> That Copernicus should have succeeded in saving the phenomena in a different way is not at all surprising. The motions of the eccentrics and epicycles taught him the times, the magnitudes, and the quality of appearances, future as well as past. Since he was exceedingly ingenious, he was able to conjure up a new method, in his opinion more convenient, of saving the appearances. […] Just as, when we know a correct conclusion, we can construct a chain of syllogisms which derive that conclusion from false premises. […] All that can be concluded from Copernicus' assumption is that it is not absolutely certain that the eccentrics and epicycles are arranged as Ptolemy thought, since a large number of phenomena can be defended by a different method. (Clavius 1581, quoted in Duhem 1908)

[12] One may wonder whether my approach leaves *any* room for strong support in favor of the geocentric position. A fascinating example can be drawn from Carman and Díez (2015, pp. 26-28) and concerns a pattern of phases for a superior planet such as Mars. In our terms, from the observationally established fact that Mars is sometimes found at opposition, one can infer by *either* the heliocentric postulates (i)-(viii) *or* the geocentric postulates (i*)-(v*) the observation of a waxing *vs*. waning gibbous disk before and after opposition, respectively. In this sense, my reconstruction converges with Carman and Díez's (2015) point that a geocentric system gets strong empirical success in this case, even if the phenomenon happened to be unobserved before modern times.

[13] In a similar fashion, Myrvold's (2003) assessment of the Copernican controversy relied on the contrast of "a bare-bones Ptolemaic hypothesis with a bare-bones Copernican hypothesis" rather than the corresponding "fully specified models of the heavens, with all parameters filled in". A Lakatosian approach, equipped with a core / programme distinction, can provide a motivation for this move.

Clavius' sources and references may be remote, but his logical and methodological insight is neat. He knows from the astronomical tradition that alternative models can account for the same phenomena. The implications of this fact converge with a sound principle of (Aristotelian) logic, namely that true (observational) statements can logically follow from false (theoretical) claims. And Clavius goes on to note that, for someone who wants to devise a theoretical system or model by which a known conclusion follows, the task is virtually only a matter of ingenuity and dedication. The latter point essentially reflects the Duhemian corollary, and it is not by chance, therefore, that Duhem himself cites and much appreciates this passage in *To save the phenomena*.

There is one subtle but crucial step in which Clavius' line of argument that is flawed, however. Granted, that one may come up with *some* novel theory saving all known phenomena is indeed "not at all surprising" for an "exceedingly ingenious" scholar such as Copernicus. This is a purely existential claim, though. It does not imply in any way that ingenuity and dedication are enough to generate a theory *such as Copernicus'*, namely such that its core claims disclose substantial logical connections between already known facts. While ingenuity and dedication *alone* do plausibly account for the accommodation of known observational facts, they do not account for crucial additional features that a theory may have, such as the successful derivation of use-novel data from further and otherwise independent evidence, as it happens with Facts (1)-(9) above. In modern terms, the idea of the comparative strength of the Copernican approach does not arise from a neglect of the Duhemian corollary (which Clavius essentially, and appropriately, endorses and emphasizes) but from the methodological relevance of the prediction / accommodation distinction (which Clavius apparently disregards).

Let us imagine a late Sixteenth Century astronomer who thinks that Clavius is unduly dismissive of Copernicus' achievement precisely because in her/his view facts such as (1)-(9) do provide strong and selective support to heliocentrism. How would such a scholar phrase her/his position? "Use-novely" is of course an esoteric term of art of contemporary philosophy of science and "prediction" would be too much of a stretch of ordinary parlance, for virtually all relevant observations describe long known phenomena. In such a predicament, our target methodological point would have to be glimpsed through periphrases, metaphors, and tentative terminology, such as a reference to "explanation": use-novel facts would be regarded as "explained" by the theory in a way that merely accommodated phenomena can not attain. Arguably, Kepler was precisely one such astronomer, and the opening remarks of his first important work, the *Mysterium* (1596, Chapter I), look strikingly like a direct response to Clavius:

> I have never been able to agree with those who rely on the model of accidental proof, which infers a true conclusion from false premises by the logic of the syllogism. Relying […] on this model they argue that it was possible for the hypotheses of Copernicus to be false and yet for the true

> phenomena to follow from them as if from authentic principles. (Kepler 1981, p. 75, translation slightly adapted)

By contrast, the most important point by which his confidence in Copernicus' theory was established, Kepler says here, is a matter of explanation: "for the things at which from others we learn to wonder, only Copernicus magnificently gives the explanation [*rationem reddit*], and removes the cause of wonder, which is not knowing causes". In a laborious but forceful attempt to motivate such claim, Kepler goes on to mention a series of manoeuvres that only a Copernican ("someone who places the Sun at the center") can perform:

> If you tell him to derive from the hypothesis […] any of the phenomena which are actually observed in the heavens, to argue backwards, to argue forward, *to infer one phenomenon from another* [*unum ex alio colligere*], and to perform *anything that the truth permits* [*quae veritas rerum patitur*], he will have no difficulty with any point [emphasis added].

"To infer one phenomenon from another" – in informal terms, this is pretty much as close as one can get to what we see with Facts (1)-(9) above. And this, according to Kepler, is something that "the truth permits", a sign of the status of the theory itself, not just of its inventor.

Soon after receiving Kepler's *Mysterium*, Galilei himself replied with a famous letter. "I have adopted Copernicus' opinion many years ago", Galilei writes to Kepler, "and from that I've been able to find the causes of many natural effects, which are doubtless inexplicable [*inexplicabiles*] by the conventional hypotheses" (Galilei 1890-1909, vol. X, p. 68). Much has been written about which "effects" Galilei may be referring to in this important but elusive passage. A recurrent conjecture (starting from Kepler himself) is that he might already have been thinking about his later (and mistaken) argument that tides prove the Earth's motion (see Voelkel 2001, pp. 71-72). But even if tides are included in Galilei's "many effects", his claim may well have a wider scope and resonate with Kepler's own remarks on explanatory success: Copernicus' theory had distinctive support from the start on the basis of long known phenomena that geocentric approaches could not explain (only accommodate). One crucial point should be emphasized here. It may appear that these remarks attribute epistemological or methodological significance to metaempirical virtues ("explanatory power", one might say). But this would be a rather misleading impression, in my reading. To the extent that "explanation" really amounts to a paraphrase to capture and convey the idea of support from use-novel evidence as contrasted with plain accommodation, the arguments at issue are fully reducible to a straight empiricist methodology. After all, according to the spirit of predictivism, verified use-novel consequences provide nothing but supporting empirical evidence, and the distinction between use-novel facts and facts that are temporally novel is meant to be a largely inconsequential contingency in methodological terms. It is just because some known facts are still “predicted” in this broad sense that one is led to regard them as “explained” in a distinctive way.[14]

In a later exchange with Balliani, the missing pieces of Galilei’s view seem to emerge much more explicitly: the variety of reasons favoring heliocentrism is unpacked, including both new *and old* items; no less important, Thycho’s system is said to face virtually the same hurdles that plague Ptolemy’s original approach:

> As for Copernicus’ opinion, truly I take it as certain, and not only for the observations of Venus, of sunspots, and of the Medicean moons, but for *his own other reasons*, as well as for many more particular reasons of mine which I regard as conclusive […] In Thyco’s opinion there remain, I find, those utmost difficulties which lead one to depart from Ptolemy, whereas in Copernicus I have nothing at all to raise the slightest qualm. (Galilei 1890-1909, vol. XII, pp. 34-35, emphasis added.)

It is quite difficult to make sense of this passage (from 1614) unless it implies that facts such as (1)-(9) above (Copernicus’ “own other reasons”) do carry substantial evidential weight, favoring heliocentrism against geocentrism in either Ptolemy’s *or Thyco’s* variants.

In principle, even if *there were* good reasons to prefer the heliocentric system to its geocentric competitors at the end of the Sixteenth Century, they might have played no role in the scientific choices of early Copernicans. This does not seem to be the case, however. The textual evidence provides at least some significant hints that Kepler and Galilei, unlike their opponents, did appreciate the relevance of use-novel data as distinctively supporting heliocentrism.

The Aristarchus puzzle

Our line of argument so far was meant to divert certain objections to vindicationism about early Copernicanism along the lines of Lakatos and Zahar (1975) and thereby to challenge the popular epistemic luck thesis. Apparently, an updated and sharpened construal of Lakatos and Zahar’s basic approach can survive Thomason’s (1992) criticism and even enlighten certain plausible underlying motivations for Kepler’s and Galilei’s scientific engagement in heliocentric astronomy. This section will be devoted to the reconstruction of a rather different source of concern for vindicationists. It may be called the *Aristarchus puzzle*, and it arises from the following argument.

1. Facts such as (1)-(9) above indicate that Copernicus’ heliocentrism was better than post-Ptolemaic geocentrism.
2. *Ceteris paribus*, working scientists have accepted a theory *X* as better than a competing theory *Y* if and only if *X* was better than *Y* given the available evidence.

[14] A similar reductionist strategy may well be pursued for other alleged “theoretical” virtues. As for “simplicity”, for instance, Sober (2015) offers much relevant material (see pp. 12-21).

3. Facts (1)-(9) above were available evidence in Aristarchus' time (third century BC).
4. Pre-Ptolemaic geocentrism (from Eudoxus) is no better than post-Ptolemaic geocentrism given the available evidence in Aristarchus' time.
5. The *ceteris paribus* clause in 2. is fulfilled in Aristarchus' case.
6. Copernicus' heliocentrism was essentially the same theory as Aristarchus'.
7. Aristarchus' theory was scientifically unpopular in his time.
8. Copernican heliocentrism is better than post-Ptolemaic geocentrism even only by the available evidence in Aristarchus' time [from 1 and 3].
9. Aristarchus' heliocentrism is better than post-Ptolemaic geocentrism even only by the available evidence in Aristarchus' time [from 6 and 8].
10. Aristarchus' heliocentrism was better than pre-Ptolemaic geocentrism given the available evidence in Aristarchus' time [from 4, 9, and the transitivity of "being a better theory than"].
11. Working scientists have accepted Aristarchus' theory as better than pre-Ptolemaic geocentrism if and only if Aristarchus' theory was better than pre-Ptolemaic geocentrism given the available evidence in Aristarchus' time [from 2 and 5].
12. Aristarchus' heliocentrism was *not* better than pre-Ptolemaic geocentrism given the available evidence in Aristarchus' time [from 7 and 11].
13. Contradiction [from 10 and 12].

Although informal, I will take the above argument is essentially valid. Indeed, trying to retain all the premises 1 to 7 and reject the conclusions seems an unlikely and contrived manoeuvre. Accordingly, some of the premises must be given up. The problem then is that the argument can be legitimately regarded as a *reductio* of vindicationism, and especially of a Lakatosian strain. To clarify this, let us consider the premises more closely.

Premise 1 is a straightforward implication of both Lakatos and Zahar's (1975) and our methodological analysis. The overarching principle stated in premise 2 is taken almost literally from Worrall (1976, pp. 164-165), where it features as "both a clarification of, and an improvement on, the account already given by Lakatos" of "how methodologies can be tested using history of science" (p. 168). Premise 3 is an historical truism: as early as the fourth century BC, Eudoxus' model must have integrated basic observations such as (1)-(9). Premise 4 seems a methodological assessment which none of the parties would challenge (see Lakatos and Zahar 1975, p. 180, and Thomason 1992, p. 191, for instance), while rejecting premise 5 would commit one to invoke a rather massive interference of extra-scientific factors in the development of early Hellenistic culture, for which no historical evidence seems in sight. This survey leaves the (post)Lakatosian vindicationist in a quite uncomfortable position, for Lakatos and Zahar (1975) also apparently accept premise 6 (p. 188), while premise 7 is the traditional consensus view in the history of ancient science.

The Aristarchus puzzle is a neglected but serious difficulty for vindicationists. We know that the Greeks developed astronomy in a mathematical and empirically testable form. According to vindicationists, Copernicus and early Copernicans had strong empirical arguments for heliocentrism. But if heliocentrism was already devised by Aristarchus in presence of at least some of the same crucial empirical evidence, then why it was not accepted in ancient times? [15]

According to a Lakatosian view as developed by Worrall (1976), methodology is regarded as testable against the history of science. Ideally, in a successful methodological programme difficulties are addressed in a way that gets independent support by historical research. Arguably, the most appealing solution of the Aristarchus puzzle for a Lakatosian methodologist would be to find good reasons to reject premise 7. Consider Lakatos and Zahar's (1975, p. 181) insistence against Kuhn's remark that "there were no good reasons for taking Aristarchus seriously" (Kuhn 1962/70, p. 75) and their explicit statement that "Copernicus' (*and indeed, Aristarchus'*) rough model had excessive predictive power over its Ptolemaic rival" (p. 188, emphasis added). In light of the Aristarchus puzzle, these claims surely are a source of discomfort for a (post-)Lakatosian vindicationist. However, if one is willing to entertain the hypothetical scenario that heliocentrism *was* well received after all (or even actively developed) after Aristarchus, this would offer a valid way out of the difficulty. Such scenario is just too good to be true, though.

Or is it?

Forgotten heliocentrism?

There is at least one fascinating and independent strain of recent historical research indicating a clear solution of the Aristarchus puzzle for vindicationists. In a series of contributions from the

[15] Beyond both Aristarchus and Ptolemy, historical research over the last decades has addressed developments leading to Copernicus across the high and late medieval period, with a special emphasis on the role of Arabic astronomy (see Swerdlow and Neugebauer 1984 for a key discussion). Surely Ptolemy was known and much criticised by Arabic intellectuals not only for philosophical reasons, but also on scientific grounds, some of which feature prominently in Copernicus' work too. Some Arabic astronomic treatises include non-Ptolemaic technical solutions which also appear in Copernicus with no known documentation in other earlier sources, either ancient or Christian, and even the Earth's motion is occasionally discussed as a scientific possibility (Ragep 2007). As the evidence for actual transmission to Copernicus is so far inconclusive, however, the implications of these facts remain a matter of controversy (see Blåsjö 2018). Moreover, whereas advanced non-geocentric astronomical approaches in antiquity are ostensibly acknowledged by Copernicus, it is not clear to what extent they were appreciated in the Arabic tradition.

1990s on, physicist and historian Lucio Russo has advocated a highly innovative view of the development of science in the Hellenistic period in which astronomy plays an important role (Russo 1994, 2004). This approach implies, among other things, that premise 7 of the Aristarchus puzzle above needs to be radically revised: indeed, it is submitted, third century BC heliocentrism was not discarded at all, but rather seriously considered by contemporaries of Aristarchus such as Archimedes (287-212) and Erathostenes (276-194), and actively endorsed and developed by major later figures including Seleucus (born 190) and above all Hipparchus (190-120). But how can this amazing claim be supported?

The awful methodological hurdle here is the painful lack of a firm textual basis for a scholar of our age. With the exception of two minor writings by Aristarchus and Hipparchus themselves, respectively, *no* scientific work of astronomical content has reached us from the timeframe between these two prominent figures. Under this grim predicament, the task of collecting evidence to assess Russo's hypothesis is bound to follow a rather thin indirect route, namely, to rely on passages of scientific relevance in non-scientific pre-Ptolemaic sources on the plausible assumption that they may bear traces of proper scientific work which is nowadays inaccessible. For our purposes, a valuable illustration comes from a passage in Seneca's *Naturales Quaestiones* (mid-first century AC), which is thoroughly analysed by Russo (1994, pp. 221-223). Here Seneca — a distinguished Roman intellectual with no direct involvement in scientific research — is reporting the position of "some people" as concerns "the five planets" by which one can understand "why they move backward" (*quare agantur retro*):

> [They] would say to us: You are wrong if you judge that any star either stops or alters its orbit. It is not possible for celestial bodies to stand still or turn away. They all move forward. […] What is the reason, then, that some celestial bodies appear [*videantur*] to move backward? The encounter with the Sun imposes upon them the appearance of slowness, as well as the nature of their paths and their orbits which are so placed that at a fixed period they deceive [*fallant*] observers. In the same way ships seem to be standing still even though they are moving under full sail.

That retrogressions occur at inferior conjunctions (e.g., for Venus) or oppositions (e.g., for Mars), namely at a certain kind of "encounter with the Sun" is of course a common notion across geocetric *and* heliocentric accounts of the phenomena. Only a heliocentric theorist, however, would go on making a sharp distinction between real motion along the orbits and purely apparent motion "backward", insisting that retrogressions must be explained as the effect of the illusory perception of a moving observer as one who sees a sailing ship as apparently still. A bold but disregarded theoretical proposal would have hardly found its way from an isolated early Hellenistic scholar on the Eastern side of the Hegean Sea to a Hispanic playwright working in Imperial Rome three centuries later unless some influential figure in between had accepted and reported it as a successful account of the relevant phenomena. Overall, Russo's comprehensive reconstruction spans a couple of centuries, including texts from Vitruvius, Lucretius, Cicero, Manilius, Pliny, and Plutarch, beyond Seneca. Each piece of evidence is only circumstantial by itself, but a systematic pattern is clearly discernible: while active astronomical research remained ostensibly silent through all this period (Russo 1994, p. 211), earlier Hellenistric doctrines appear to have occasionally surfaced as pieces of shallow erudition in loosely related contexts. In both Pliny and Cicero, for instance, one finds the casual but explicit statement that "planet" is a misleading label, for so-called "wandering stars" are in fact not wandering in any literal sense (p. 225).

Currently, Russo's position remains way out of mainstream perspectives in the history of science.[16] Moreover, a historical resolution of the Aristarchus puzzle is bound to trigger a "revenge" problem, namely to raise a counterpart "Ptolemy puzzle": assuming that Aristarchus' insight was indeed successfully developed in Hellenistic science, then why did *Ptolemy* not recover heliocentrism in the second century AC? (Statement 9 would then become the pillar of the new *reductio* argument.) The externalist strategy is comparatively stronger in this case, though. It seems established that Ptolemy had to refound the astronomical tradition in Alexandria without the benefit of a continuous intellectual lineage from his Hellenistic predecessors and indeed after a rather dramatic and durable halt. Russo also makes a case that Ptolemy's sources were themselves incomplete, not including the latest and more advanced fruits of Hellenistic science (Russo 1994, pp. 210-213). Indeed, he finds it "not too surprising if traces of some of Hipparchus' ideas might be found more easily in the Rome of the first century BC […] than in the Alexandria of the second century AC" (p. 232).

Logically, rejecting premise 7 above surely is one way to meet the challenge of the Aristarchus puzzle. For a predictivist like Lakatos and Zahar (1975), given all other plausible premises 1-6, Aristarchus' theory should not have been unpopular in his time. At least if Russo is right, this is just what observant historical inquiry reveals: "Aristarchus' heliocentric model had been given up not in the period between Aristarchus and Hipparchus, but […] during the long interruption of the scientific activity that occurred between Hipparchus and Ptolemy" (Russo 1994, p. 238).[17]

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[16] Scholz's (2023) fascinating discussion is consistent with some of the unconventional claims by Russo. In Klagh (2007, pp. 24-28), however, one finds the traditional view that "Aristarchus' heliocentric system was not considered a serious rival to the geocentric models and soon went into oblivion", including a reference to the much discussed accusation of impiety from the Stoic philosopher Cleanthes (which is questioned in Russo and Medaglia 1996 as the misguided effect of a philological accident).

[17] While rejecting premise 7 happens to be my favourite post-Lakatosian rescue strategy from the Aristarchus' puzzle, it is not the only logical possibility that can be pursued on historical grounds. A very different (and indeed quite incompatible) alternative might be to give up premise 6. According to Carman (2018, p. 1), for instance, "given that ancient astronomers were perfectly capable of understanding the great

Concluding remarks

In an effort to bring together the threads of our discussion, one is tempted to consider that critics of Lakatos and Zahar's (1975) predictivist vindication of early Copernicanism may themselves be right for the wrong reasons. In fact, while the limitations of the original methodological reconstruction can be amended to counter the epistemic luck thesis, the challenge of the Aristarchus puzzle may still undermine a post-Lakatosian account in a way that seems to have largely eluded the scope of the most lively philosophical debates. To this extent, Thomason's (1992, p. 198) remark that "the Copernican Revolution […] should remain the touchstone for evaluating any philosophy of science claiming to be historically relevant" may well be integrated by an additional suggestion, namely that the interaction between philosophy and history of science should perhaps be more thoroughly explored even beyond the limits of the Modern Age.

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advantages of heliocentrism over geocentrism […], it seems difficult to explain why heliocentrism did not triumph over geocentrism or even compete significantly with it before Copernicus". Carman's remarkable solution is that "the first Copernican was Copernicus".

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