

A predictivist vindication of early Copernicanism

June 2022

In a classic paper, Lakatos and Zahar (1975) carried out a comprehensive methodological assessment of the Copernican revolution. In their view, Copernicus's programme had a remarkable amount of "*immediate support*" (p. 188) from known phenomena that was not matched by the traditional geostatic approach, even if both systems were able to account somehow for all essential facts established in the late Sixteenth century. This showed, Lakatos and Zahar submitted, that "there were good objective reasons for Kepler and Galilei to adopt the heliostatic assumption". This point was meant to crucially refute a contrary claim to be found in Kuhn (1957, p. 181).

Lakatos and Zahar's analysis did not remain unchallenged, however. According to Thomason (1992), in particular, Lakatos's methodology can not support the desired conclusion, not even with the addition of Zahar's criterion for *novel fact*. I put forward three reasons why it makes sense to go back to this apparently old-fashioned controversy.

1. A key complaint by Thomason (1992) is that Lakatos and Zahar (1975) "ignored Tycho Brahe" (p. 161). The claim is not unfounded, for in Lakatos and Zahar (1975) Tycho's case is taken on in the criticism of competing methodological reconstructions (especially the falsificationist and simplicist), but otherwise suspiciously relegated in minor footnote remarks. This raises a first question: *can Lakatos and Zahar's argumentative strategy survive due consideration of the Tychonic system?*
2. Both Lakatos and Zahar (1975) and Thomason (1992) relied on Zahar's (1973) early characterization of how certain known phenomena could provide important and "immediate support" to a new theory. But a good deal of additional work has been done meanwhile on this crucial topic — see Myrvold (2003), Lipton (2005), Worrall (2002, 2006), and Votsis (2014). This raises a second question: *would the integration of a more developed analysis of the accommodation vs. prediction distinction help vindicate Lakatos and Zahar's original verdict?*
3. Thomason (1992) did not challenge Lakatos and Zahar's (1975) dismissal of alternative philosophical reconstructions of the Copernican arguments. This seems to leave the early

Copernicans' choice with no plausible methodological justification. Already championed by Riccioli (1651), such view has been revived by Kuhn's work, then fueled by Feyerabend (1975), and has remained influential in the scholarship thereafter (see Graney 2015 and Marcacci 2015; Finocchiaro 2010 is an interesting exception). Meanwhile, no criticism of Thomason (1992) has appeared in thirty years. This raises a third, and final, question: *is there any serious chance left for one to resist Kuhn's (1957) charge of "irrationality" on early Copernicans?*

In my contribution, I plan to recast Lakatos and Zahar's (1975) analysis in a new form, and defend it from Thomason's (1992) criticism. My argument will proceed in *three* steps.

First, I will define a coherent variation of the Lakatosian reconstruction which thoroughly includes the Tychonic system (see Margolis 1991 for a revealing independent argument supporting this move on purely historical grounds). A crucial point of my proposal is that Lakatos and Zahar's (1975) reference to the heliocentric "rough model" be taken very seriously as indicating a key unit of methodological assessment. Also, broadly following 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. Briefly put, I will consider a known fact F as use-novel (indeed, a successful prediction, and therefore confirming evidence) for a core theory T in case there exists another known fact E such that F follows from T and E but *not* from T alone or E alone (of course, on the background of further contextually unchallenged assumptions).¹ To be sure, this characterization fully integrates the Duhemian point that a theory can always be tailored to recover known phenomena, here E and F (see Crupi 2021). By contrast, 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 parts of it is taken as a distinctive element of empirical success.² As a classic example, posit $E =$ Kepler's first law, $T =$ Newton's theory, and $F =$ Kepler's second law. Also, so-called *cross-validation* techniques can be seen as a routine application of the same idea in statistical settings (as pointed out by Schurz 2014, p. 92).

Second, I will establish a conditional statement: *if* my reconstruction holds, then positive answers ensue for questions (1)-(3) above. Relevant cases involved in the controversy between Lakatos and Zahar (1975) and Thomason (1992) will be systematically revisited in this perspective.

¹ To defuse the logical trickery of positing $T = E \supset F$ and claim confirmation for such degenerate T one should add that T itself must not follow from F . But this proviso is obviously satisfied by real scientific hypotheses, models, or theories: these are not meant to be logically *provable* by the phenomena.

² Formally, one way to see this analysis is as a special (non-probabilistic) case of Myrvold's (2003) account of the evidential value of "unification" via the increase of "informational relevance" between known phenomena. The connection with use-novelty, however, does not play any role in Myrvold's discussion.

As a cornerstone illustration, consider retrograde motion: as it happens, my basic analysis of use-novelty implies that retrograde motion counts as an impressive piece of confirmation for the heliocentric “rough model” but *not* for the Tychonic specification of geostatic astronomy.

As a *final* step, I will assess the antecedent of the conditional above, with an attempt at detaching the consequent, and thus providing a full methodological vindication of early Copernicanism. On the one hand, I will defend my variant of a predictivist view of confirmation: not only is my interpretation of use-novelty in the spirit of the original Lakatos / Zahar / Worrall tradition, I will submit; it also arguably receives indirect theoretical support from a variety of widespread informal remarks, including reference to (no) double-counting, predictive risk, unification, and explanation. Moreover, my reconstruction is consistent with the historical record in the case-study at issue — so I will argue — and allows for an insightful recapitulation of the rather untidy debate on whether the Copernican theory was effectively best off when people like Kepler and Galilei made up their mind in its favour.

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