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Underdetermination and decomposition in Kepler's *Astronomia Nova*

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ABSTRACT

This paper examines the underdetermination between the Ptolemaic, Copernican, and the Tychonic theories of planetary motions and its attempted resolution by Kepler. I argue that past philosophical analyses of the problem of the planetary motions have not adequately grasped a method through which the underdetermination might have been resolved. This method involves a procedure of what I characterize as decomposition and identification. I show that this procedure is used by Kepler in the first half of the *Astronomia Nova*, where he ultimately claims to have refuted the Ptolemaic theory, thus partially overcoming the underdetermination. Finally, I compare this method with other views of scientific inference such as bootstrapping.

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1. Introduction

Perhaps the central problem of astronomy during the time from Copernicus to Kepler was the question of which was the true description of the system of the world—the Ptolemaic, the Copernican, or the Tychonic. In this paper, I will call this the *problem of the planetary motions*. At the core of this problem lay a methodological conundrum. It seemed that the observations available at the time would not be able to distinguish between the three world systems, as acknowledged by Kepler in the Introduction to the *Astronomia Nova*:

> "For each of these three opinions concerning the world there are several other peculiarities which themselves also serve to distinguish these schools, but these peculiarities can each be easily altered and amended in such a way that, so far as astronomy, or the celestial appearances, are concerned, the three opinions are for practical purposes equivalent to a hair's breadth, and produce the same results." (Kepler, 1992, pp. 47–48)

In modern terms, the question of which of the three world systems was the true system of the world was *underdetermined* by the available observations. Famously, this underdetermination was partially resolved when Galileo observed the phases of Venus in 1609, effectively eliminating the Ptolemaic theory as a possibility.[1]

Kepler wrote the *Astronomia Nova* before Galileo's discovery, yet by the end of this work, he believed he had emphatically refuted the Ptolemaic system. How, then, did Kepler think he had overcome the underdetermination that he himself mentioned in the Introduction? Although this question was made immediately irrelevant by Galileo's discovery, I think it is a question that is worth exploring for the sake of understanding scientific methodology, and in particular, ways in which underdetermination might potentially be resolved in certain cases.

The problem of the planetary motions, and its resolution, has been the subject of an extensive philosophical literature, particularly with regard to what reasons we could have had for accepting the Copernican theory over the Ptolemaic, prior to Galileo's discovery. I think that the reasons that have been proposed heretofore by various philosophers might support a slight preference for the Copernican theory, but they would not be enough to resolve the underdetermination—that is, they wouldn't be enough to convince a reasonable Ptolemaic theorist to switch to the Copernican theory. On the other hand, Kepler thought he had overwhelmingly strong

[1] The Tychonic theory remained as a possibility until at least the time of Newton, who starts off Book 3 of the *Principia* neutral between the Tychonic and Copernican theories.

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reasons for choosing the Copernican theory over the Ptolemaic. I believe that this is not mere rhetoric—Kepler's method would, if carried out in its entirety, have provided extremely strong support for the Copernican theory.

In order to understand Kepler's method, however, we must conceptualize the problem of the planetary motions in the right way. The typical way in which the problem is conceptualized is in terms of a familiar picture of theory and observation. We have a set of observations of the planetary motions as seen from the earth's surface, and the task of the planetary astronomer is to come up with a theory that can account for those motions. As Kepler points out, the Ptolemaic, Copernican, and Tychonic theories all account for these motions to about the same degree of accuracy, and moreover, with appropriate modifications, the three theories can be made empirically equivalent "to a hair's breadth". Seen in this way, the underdetermination looks more or less hopeless, although as we shall see, numerous attempts have been made to show how this underdetermination problem can be resolved.

I think, on the contrary, that we must conceptualize the problem in terms of *exploration*. And if we conceptualize the problem in this way, we will understand that Kepler attempted to resolve the problem using a process of *decomposition* and *identification* that I will describe in detail below—a process which, I think, can provide a powerful means to overcome underdetermination, at least in some cases. And the problem of the planetary motions is just such a case where the underdetermination can be resolved, as Kepler himself claims in the *Astronomia Nova*.

In Section 2, I will briefly examine the way in which the problem of the planetary motions has been treated in the past by philosophers, particularly John Worrall and Clark Glymour. I believe that the typical way in which the problem of the planetary motions has been conceived is not very helpful in considering how it might have gotten resolved. My alternative conception is not difficult to understand, but I have found that it is best to explain the conception using an example. Section 3 therefore contains an example of a problem that I believe is surprisingly similar in significant ways to the problem of planetary motions. In Section 4, I will examine what Kepler does in the *Astronomia Nova* leading up to his declaration that the Ptolemaic theory has been refuted. We will see that my alternative picture gives us a better grasp of what Kepler is trying to do. Finally, in Section 5, I will comment on some philosophical implications of my alternative picture.

2. Philosophical analyses of the problem of the planetary motions

Much of the literature on the problem of the planetary motions in the past few decades responds to Kuhn's account of the problem in *The Copernican Revolution* (1957), so I will start there. Kuhn argues that the Copernican theory was "neither more accurate nor significantly simpler than its Ptolemaic predecessors" (Kuhn, 1957, p. 171), and then states that the advantage of the heliocentric system was *aesthetic* in character. It could explain *qualitative* features of the planetary motions in a tidier and less ad hoc manner than could the Ptolemaic theory—e.g., retrograde motion, and the fact that Venus and Mercury are always in the vicinity of the sun. Another significant advantage was that the relative sizes of the planetary orbits could be determined, something that could not be done in the Ptolemaic theory. For any given planet, if you make the assumption that the orbits are roughly circular, you can determine, through triangulation, the ratio of the orbital radius of the earth to that planet. And since you can do the same with all the planets, you can determine the ratios of all the orbital radii to each other. Kuhn argues that these are all aesthetic considerations in favor of the Copernican theory.

Various philosophers have since attempted to show that the preference for the Copernican theory can be given a rational basis, not just an aesthetic one. This literature is extensive, and it would be beyond the scope of this paper to review all of it. On the other hand, I want to contrast my own approach to the problem to those of at least a few previous philosophers. I will therefore examine two other approaches—those of Worrall (2010) and Glymour (1980).[2]

I am examining Worrall (2010) because it is the latest version of perhaps the most influential attempt to respond to Kuhn's accounts, that of Lakatos and Zahar (1975). This approach is based on the intuition that the Copernican theory is to be preferred because the Ptolemaic theory is ad hoc, since the epicycles are introduced merely to account for retrograde motion, whereas the Copernican theory is not, because the retrograde motion drops naturally out of the heliocentric arrangement of the planets. An immediate problem for this view is that since both the Ptolemaic theory and the Copernican theory were constructed based on observations that had already been made, some account needs to be provided of why the Ptolemaic theory ends up being ad hoc while the Copernican theory does not. Worrall (2010) takes care of the problem by invoking the idea of *use-novelty*—in order to avoid being ad hoc, theories must make novel predictions, but such predictions are understood "not in the temporal sense but in the sense of 'falling out' of the theory without having had to be worked into that theory 'by hand'" (Worrall, 2010, p. 129).

Now, exactly what does it mean for a prediction to fall out of a theory, instead of being worked in by hand, and how do you tell the difference? Worrall later explains the difference for the case of the Ptolemaic and Copernican theories (p. 141). First, he distinguishes between a *general* geocentric theory of planetary motion, and a *particular* version of geocentrism. A general geocentric theory consists simply of the claim that the earth is at the center of the system of the world, whereas a particular version would be a geocentric theory with a particular number of epicycles, and with determinate values for the parameters of the epicycles (such as their sizes, locations of their centers, and so on), as well as other parameters of the theory. Worrall argues that agreement between the predictions made by a particular geocentric theory and observations might give you reason to believe in that particular geocentric theory. But there would be no reason to accept the underlying general geocentric theory, because we should not be surprised that the particular geocentric theory matches observations—the epicycles have been "put in by hand" to match the observations!

Worrall contrasts this with the Copernican theory, in which, he claims, the retrograde motions "drop out naturally from the heliocentric hypothesis" (p. 142). Now, one might complain that the parameters of the Copernican theory, such as the sizes of the planetary orbits, have been tuned to fit the observations, in the same way that the parameters of the Ptolemaic theory have. So one should not be surprised, on the Copernican theory as well, that there is a good match between the predictions and the observations. Worrall, therefore, is careful to state that he means the "qualitative phenomenon, not the quantitative details". But what is the qualitative phenomenon here? Presumably, it is the existence of the retrograde motion. Now, I think it's true that most of us feel intuitively that the Copernican theory does a tidier job than the Ptolemaic theory in accounting for the retrograde motion. But now

[2] Aside from the reasons offered within the text, I am examining Worrall (2010) partly in response to a very helpful set of comments from an anonymous referee, and I am examining Glymour (1980) partly in response to a question by Don Howard that was raised at the Athens conference about the connection between my view and bootstrapping.

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