# *Structure, equivalence, and modal realism: learning from Ptolemy versus Copernicus*

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**Abstract.** I use the case of Ptolemy versus Copernicus to distinguish observational, empirical, modal, and virtue (in)equivalence. Focussing on Kepler, I show that we learned a great deal from the resolution of this case about how to mobilize these different kinds of inequivalences in the process of physical theorizing, and to identify those elements of our theories that are likely to persist through theory change.

The elements most likely to survive through theory change are, I argue, modal dependence relations. If we try to fit our characterization of the aims of science to what the methodology has been developed to deliver, then perhaps we might try something like this: "An aim of science is to provide a set of modal dependencies adequate to the observable phenomena. Realism is a commitment to these modal dependencies as underlying the observable phenomena." Theories are sometimes a good tool for discovering and expressing these modal dependencies.

## Introduction

Ptolemy versus Copernicus presents us with an old and familiar example of equivalence between theories. The key question is this: "Given two observationally equivalent theories, how should we respond?" We can divide this into two questions. First, "How (if at all) do we decide between them?" This is the question that is immediate and pressing. Then, there is a second question, which arises on further reflection and which has implications for the longer term. "To what exent, if

at all, can we have confidence that the choice we make now will not turn out to be replaced later by a radically different, yet observationally equivalent, theory?"

I argue that we learned a lot from this case about how to respond to the epistemic challenges posed by equivalent theories, and I spell out some of the important features of this in detail. I begin by setting up the basic equivalences and inequivalences between Ptolemaic and Copernicus's astronomical systems (section 1), and then consider two different responses to the observational equivalence of these two systems, due to Kepler (section 2) and Descartes (section 3) respectively.

I end with some conclusions on the methodological lessons, and on what we can expect theories developed within this framework to deliver (section 4). In particular, I suggest that scientific realism as formulated by van Fraassen attributes goals to science that are not aligned with this methodology. Instead, we have good reason to think that the methodology delivers modal dependence relations (as I will explain), and these are what is most likely to survive theory-change.

## 1.Equivalences and inequivalences in Ptolemy versus Copernicus

### 1.1 Geometrical equivalence and observational equivalence

The Ptolemaic system and Copernicus's system of astronomy provide a model for each planet built from circular motion. Though the Ptolemaic model for a given planet is geocentric and Copernicus's heliocentric, these models for a given planet are *geometrically equivalent*. This is easiest to see for the basic case of a two-circle model, and are subtleties arising, for example, from Copernicus's refusal to make use of Ptolemy's equant, but as a consequence of the geometric equivalence the two systems produce exactly the same observable motion of the planet with respect to the background of the fixed stars. The central epistemological challenge arises from this *observational equivalence* of the geocentric Ptolemaic system and the heliocentric geometric system. By "observational equivalence" I mean that both theories are compatible with the actual observations.

## 1.2 Empirical (in)equivalence and modal (in)equivalence

Despite the geometrical and observational equivalence of the two systems, they are *empirically inequivalent*: once you nest the models for each planet together, into a planetary system, Copernicus's theory rules out possibilities that Ptolemy's theory does not.

For example, Ptolemy's system allows for the possibility that we could observe Mercury and Venus on the opposite side of the sky from the Sun. In fact, this is never observed, but Ptolemy's theory allows for it. In Copernicus's system, however, Mercury and Venus, as viewed from Earth, will always be within a small angular distance from the Sun, and it is *impossible* for them to be seen on the opposite side of the sky from the Sun. Therefore, the two theories are *empirically inequivalent*: while they are equally compatible with the actual observations (i.e. the are observationally equivalent), they differ over which observations are possible and impossible.

A second example is the ordering and spacing of the planets. In both systems, each planet is modelled individually using two circles: the deferent and epicycle in Ptolemy's system, and the planet's orbit plus the Earth's orbit in Copernicus's system. In Ptolemy's system, what matters is the relative size of the deferent to the epicycle, for each individual planet, but there is nothing to constrain the relative size of the deferent for one planet as compared to the deferent of another planet. As a result, Ptolemy's system does not determine the relative spacing between the planets, nor even the ordering of the planets. Things are different in Copernicus's system: the Earth's orbit appears in the model for each individual planet, and therefore provides a standard of size across the models for each planet. The consequence of this is that Copernicus's system determines a unique ordering of the planets and the relative distances between the planetary orbits. The spacing and distances were not, at the time, observable. What was observable was the two-dimensional trajectories of the planets against the background of the fixed stars. But, with space travel, for example, the spacing and distances would be observable, hence the empirical inequivalence.

Empirical inequivalence – differing over which observations are possible and impossible – is a case of *modal inequivalence* of theories. What is epistemically interesting, I take it, is cases where we have theories that are *observationally equivalent but modally inequivalent*. Given the observational equivalence but modal inequivalence of Ptolemy versus Copernicus, is there an epistemically responsible way of deciding between them? In what follows, I consider the responses of Kepler and Descartes.

## 2. Kepler

### 2.1 Instrumental (in)equivalence and virtue (in)equivalence

Kepler's first major astronomical publication was *Mysterium Cosmographicum*, of 1596. In this book, Kepler argued for the Copernican system, using a range of arguments. Kepler accepted the geometrical and observational equivalence of Ptolemy's and Copernicus's systems. He also accepted their *instrumental inequivalence*: by the late 1500s, many mathematical astronomers used Copernicus's system for the calculational advantages that it offered. But no-one (that I know of) saw in this a reason to prefer Copernicus's system as giving the structure of our planetary system, and Kepler did not use the instrumental inequivalence as the basis of an argument either.

Kepler did use *empirical inequivalence* to argue for Copernicus's system, and specifically the empirical inequivalences I mentioned above, concerning the observed position of Mercury and Venus in the sky relative to the Sun, and the determinate ordering and spacing of the planets that follows from Copernicus's system. However, these empirical inequivalences were well-known, and few at the time were persuaded by them.

Kepler also used "*virtue inequivalence*". In our current discussions of theory choice, we have become used to speaking about theoretical "virtues", and I shall use this as an umbrella term for a range of considerations that have been used to help guide theory development and theory choice. Kepler argued for Copernicus's system

on the basis of three virtues: simplicity, harmony and causality. In the case of simplicity, Kepler appealed to Copernicus's insight that, whereas in the Ptolemaic system an epicycle needs to be added in order to account for retrograde motion, with the Earth in motion *nothing* needs to be added to the circular motion of the planet in order to recover the appearance of retrograde motion. So, simply by putting the Earth into motion, Copernicus removed the need for any major epicycles, thereby implementing a dramatic simplification of mathematical astronomy. Kepler took this to count strongly in favor of Copernicus's system.

Kepler's Platonic solids model of the cosmos is the centerpiece of *Mysterium Cosmographicum*. There are exactly 5 regular polygons (the Platonic solids), and Kepler discovered that if you put the Sun at the center, and if you nest spheres among these regular polygons, you uncover a beautiful harmony in the arrangement of our planetary system. The number of shells gives the number of planets in Copernicus's system (and disagrees with the number in Ptolemy's), and the spacing of the shells gives the spacing of Copernicus's orbits (something undetermined in Ptolemy's system). In other words, hidden in Copernicus's orbits (unlike Ptolemy's) is a deep underlying geometrical structure, an inner harmony, thereby providing Kepler with a new argument for Copernicus's system.

The final argument offered by Kepler in *Mysterium Cosmographicum* is an argument from causality. Kepler went beyond Copernicus in his commitment to Copernicus's system, insisting that the Sun be located at the center of the planetary orbits, and arguing that rather than simply being the lamp of the cosmos (as Copernicus had argued) the Sun is the motor of the cosmos, driving the planets around in their orbits. Kepler believed he could support this claim if he could find a relationship between the distance of the planet from the Sun and the speed of that planet's orbital motion. As we saw above (section 1.2), Copernicus's system determines the relative radii of the planetary orbits, so if Kepler was able to find a relationship between the determined distance of the planet to the Sun and the speed of the planet (here the eccentricity of the orbits, which we have ignored so far, is important), this would enable him to provide a strong argument for Copernicus's

system. Kepler attempted this in *Mysterium Cosmographicum*, but he wasn't able to find a relationship that worked, so the argument remained incomplete.

For our purposes, what is important is that Kepler was using causality – specifically, the idea that the Sun is the physical cause of the motions of the planets – as a tool for trying to develop Copernicus's theory. The *physical inequivalence* between the Aristotelian cosmology, with which Ptolemaic astronomy was associated, and whatever cosmology might be developed to accompany Copernicus's mathematical astronomy, was of course one of the major objections to Copernicus's astronomy. One of Kepler's driving motivations was to develop a physical cosmology consistent with the new astronomy, and his starting point was according a causal role to the Sun in the motions of the planets. Kepler was using causality as a means of guiding and constraining his theory development. According to the taxonomy I am developing here, this use of causality – which does not, in itself, involve observational, empirical, or modal inequivalences, though it may lead to these as consequences – falls under the umbrella of virtue inequivalences.

### 2.2 *Astronomia Nova*: from virtue inequivalence to observational inequivalence

Following a long struggle, Kepler published his first two laws of planetary motion in his *Astronomia Nova* of 1609. These two laws say:

1. The orbits of the planets are ellipses with the Sun at one focus.
2. The speeds of the planets vary according to equal areas in equal times (or, more carefully: the radial segment from the Sun to the planet sweeps out equal areas in equal times).

These two laws modify Copernicus's system, changing the shape of the orbits from the planets (from circles to ellipses) and the speeds of the planets (from equal angles in equal times (i.e. uniform circular motion) to equal areas in equal times). Thus, they attribute a different shape to the orbits of the planets from Ptolemy, and different speeds, yielding observational *in*equivalence between Kepler's version of the Copernican system versus Ptolemy's system.

The process by which Kepler broke the observational equivalence between Ptolemaic astronomy and the Copernican system is worth noting explicitly. Kepler started with observationally equivalent theories. These theories were nevertheless empirically inequivalent, and Kepler took aspects of this to favor one theory over the other. They were also virtue inequivalent (simplicity, harmony, causality), and he used these inequivalences to construct arguments in support of his preferred option. In the end, after much hard labor, use of the causal inequivalence led him to develop a new version of Copernicus's theory that was observationally inequivalent to Ptolemy's theory.

### 2.3 Lessons from Kepler

Recall our original question: Given two observationally equivalent theories, how should we respond? We broke this into two, the first of which is: How (if at all) do we decide between them? Kepler's response to this first question focuses our attention on the diachronic process by which other kinds of inequivalence between two theories can be mobilized in theory development, with the upshot in this case being the breaking of observational equivalence. In the case we have looked at, Kepler used virtue inequivalence. Another familiar example is Galileo's telescope and the phases of Venus. Prior to the development of the telescope, any such phases were unobservable, and therefore this difference is an empirical inequivalence between the theories but not an observational inequivalence. However, with the development of the telescope, the phases became observable, and in 1613 Galileo published his observations of the phases of Venus. Thus, an empirical inequivalence becomes an observational inequivalence, and as it turns out the observed phases of Venus are incompatible with the Ptolemaic system but compatible with Copernicus's system. Galileo used this to argue in favor of Copernicus's system.

This attention to the diachonic process by which different kinds of equivalence and inequivalence may be mobilized in theory choice leads us directly into consideration of our second question: To what exent, if at all, can we have confidence that the choice we make now will not turn out to be replaced later by a

radically different, yet observationally equivalent, theory? That is, given the occurrence of observational equivalence, why should we have confidence in the stability of the theory we now accept?

Kepler makes some remarks that can be used to address this question. He raises the following worry, in *Mysterium Cosmographicum*:

> "But you may object that it can to some extent still be said, and to some extent could once have been said about the old tables and hypotheses, that they satisfy the appearances, yet they are rejected by Copernicus as false; and that by the same logic the reply could be made to Copernicus that although he gives an excellent explanation for what is observed, yet he is wrong in his hypothesis." (Kepler, 1596)

He then offers the following, as a response to this worry:

> "For it can happen that the same conclusion follows from two suppositions which are different in species, because they are both included in the same genus, and the point in question is a consequence of the genus."

Familiar in this is the suggestion of "selective realism" as a means of responding to worries arising from theory change. In current philosophy of science, this is one route by which scientific realists attempt to respond to the pessimistic meta-induction. In Kepler's terminology, the proposal is that there can be continuity in the genus despite changes in species.

Kepler spells out his proposal in more detail as follows. The observed motions of stars depend on two parameters (among others): the relative motion of the Earth and the stars, and the relative size of the Earth's orbit to the distance of the fixed stars. This dependency is present in both Ptolemy's and Copernicus's system, and produces predictions that are equivalent up to the accuracy of observations available at the time. It is these quantitatively precise dependencies of the observables on the variations of particular parameters that need to be the focus of

our attention when we're looking at equivalence across "theory change". The proposal is that if we think of theories in this way then developments at the level of species do not threaten the genus.

Is this answer any good? Well, in order for the genus/species argument to work, it must be the case that all the observational successes of Ptolemaic astronomy are due to the genus rather than the species. In order to assess whether or not this is the case, let's start by granting Kepler's claim (from *Mysterium Cosmographicum*, above) that geocentrism versus heliocentrism is a difference in species. Having granted this, we can then examine whether *all* the observational successes of Ptolemaic astronomy arise from the remaining *genus* (shared with the Copernican system), or whether there are *other* differences in *species* that play a role in the observational success of Ptolemaic astronomy.

Kepler formulated his genus-species argument in *Mysterium Cosmographicum*, prior to his development of his laws of planetary motion. It turns out to be instructive to consider his genus-species argument in the light of these laws of planetary motion. Kepler's laws and Ptolemy's system are *not* geometrically equivalent to one another, so the first question is whether we can make the same genus-species argument there. It turns out that there are aspects where we can, but there is also a very important aspect where we can't.

In order to set the geocentric/heliocentric difference in species to one side, so that we can see whether there are remaining significant differences in species, we will consider the fictitious character Ptolemy II. Ptolemy II is Ptolemy's younger brother, who copied everything that his brother did, except that he was a bit wilder and even more uncoventional than his brother: he was a heliocentrist. In other words, he used all the same mathematical devices as Ptolemy, but used them in a heliocentric system.

Now we can compare two heliocentric systems – Ptolemy II's with Kepler's Copernican system – in order to determine whether the observational successes of the two theories arise from their common genus, or whether there are important differences in species. We will do this in two steps. First, we will identify those features of Ptolemy II's system that are responsible for its observational success, as

compared to Kepler's system. Then, we will ask whether these features belong to the genus (as they need to in order for Kepler's genus-species argument to work) or to the species.

First, we formulate the relevant parts of Ptolemy II's system in an analogous manner to Kepler's laws:

> Ptolemy II's first law: The orbits of the planets are circles with the Sun offset from the center.
> Ptolemy II's second law: The planets move with equal areas in equal times about the equant.

These laws produce observationally inequivalent planetary motions at the level of accuracy achieved by Tycho Brahe. Prior to Brahe, they could not have been distinguished observationally. In order to understand the features of Ptolemy II's theory responsible for its success, we first consider the approximation of both theories that is zeroth order in eccentricity. Since ellipticity depends on ecccentricity, this means zero ellipticity as well as zero eccentricity. In this case, the laws become:

> **Zeroth order in eccentricity approximation:**
>
> Kepler's first law: The orbits of the planets are circles with the Sun at the center.
> Kepler's second law: The planets move with equal areas in equal times about the Sun.
>
> Ptolemy II's first law: The orbits of the planets are circles with the Sun at the center.
> Ptolemy II's second law: The planets move with equal areas in equal times about the center.

Thus, the first laws coincide exactly. Moreover, since the Sun is at the center, and since in the case of a circle a radial segment sweeping out equal angles in equal times will also sweep out equal areas in equal times, the second laws also coincide exactly.

However, the eccentricity of the planetary orbits is observable with naked eye observations, and was known in ancient astronomy. So the zeroth order approximation will not do. Let's move to the first order approximation in eccentricity. Since ellipticity depends on eccentricity squared, this first order approximation will include ellipticity but *not* eccentricity. (The eccentricity of an ellipse is a measure of the *distance* between the two focii, whereas its ellipticity concerns its shape – it is an *area* effect, depending on the *square* of the distance.) The laws then become:

**First order in eccentricity approximation:**

Kepler's first law: The orbits of the planets are circles with the Sun offset from the center.
Kepler's second law: The planets move with equal areas in equal times about the Sun.

Ptolemy II's first law: The orbits of the planets are circles with the Sun offset from the center.
Ptolemy II's second law: The planets move with equal areas in equal times about the equant.

For Ptolemy II's laws, this first order "approximation" has no effect: the two sets of laws are identical. Moreover, Ptolemy II's laws and Kepler's laws to first order give rise to very similar predictions. In Kepler's case, the line from the Sun to the planet sweeps out equal areas in equal times. In Ptolemy II's case, the line from the equant to the planet sweeps out equal angles in equal times. As it turns out, the predicted positions of the planets deviate only slightly from one another, and as you reduce

the eccentricity (so that the Sun and equant move closer to the center of the circle), the differences between the predictions of the two systems decreases, such that for the actual eccentricities of the planets, the differences are less than observational accuracy prior to Tycho. The two theories are therefore observationally equivalent up to the observational accuracy of the time.

With this in mind, we can now turn our attention to our second, and crucial, question: Are the features that are responsible for the observational success of Ptolemy II's theory, as compared to Kepler's theory, features of the genus (as is needed for Kepler's argument to go through) or features of the species?

Consider first shape. The eccentric circle is an approximation of an ellipse and a focus, so here Ptolemy II and Kepler share the same genus.

Now consider speed. In this case, Ptolemy II's theory correlates different parameters with the observations (angles to time times about the equant) as compared to Kepler (areas to times about the Sun). This is a difference in species. The two *happen* to coincide to a high level of approximation for *small eccentricity*, but diverge as the eccentricity increases.

For a very long time in the history of astronomy we were tricked by an accident of our local circumstances. Had the eccentricity of the orbits of the planets in our planetary system been greater, the two possibilities under consideration (equal angles in equal times about the equant, and equal areas in equal times about the Sun), would have come apart observationally. But the orbits of our planets have very low eccentricity, and it takes very high eccentricity to make the two observationally inequivalent. We have here a very important epistemic lesson: a misleading situation in our local circumstances can leave us with an error that can persist for a very long time. In light of this epistemic lesson, we need to modify what we had learned thusfar from Kepler.

Here is where we are. Modal dependencies among the parameters and observables of a theory are what are stable and likely to last; i.e. the modal structure underlying the observable phenomena. We can use this as a guide to what will survive theory change (genus versus species). Nevertheless, this methodological proposal is not infallible. As we have seen, our local circumstances may be

misleading, producing long-term observational equivalence despite modal inequivalence (with an unconceived alternative, perhaps). In light of this hugely important realization comes the recognition of one reason why observational precision and quantitative details in the phenomena matter: mere approximate consistency with the observable phenomena doesn't protect us against this type of error – it doesn't have enough epistemic bite – what led to the discovery of the "equant error" was high precision observations and careful attention to these observational details in theorizing.[1]

## 3. Descartes

In Part III of Descartes's *Principles of Philosophy*, Descartes considers the three basic systems of mathematical astronomy available at the time: Ptolemaic, Tychonic, and Copernican. The *Principles* was published in 1644, long after Galileo's 1613 publication of his observations of the phases of Venus. The observed phases are inconsistent with Ptolemy's system, but they are consistent with Tycho's and Copernicus's, so Descartes rejects Ptolemy's system (Principles III.16) and takes the dispute over the system of the world to be between Tycho's geocentric system and Copernicus's heliocentric system.

Descartes accepted both the geometrical and the observational equivalence of these two systems. Notice that this is more than 30 years after the publication of Kepler's *Astronomia Nova*, in which Kepler offered a version of the Copernican system that is *observationally inequivalent* to Tycho's. Descartes did not consider that this offered good grounds for favoring the Copernican system (for reasons I will come back to below), and this is important for our purposes, because it reminds us that observational *in*equivalence is not (always) sufficient for deciding in favor of one theory over another.

[1] This is a methodological lesson that was brought to rich fruition by Newton, as has been argued by George Smith (2014) and Bill Harper (2011).

Descartes argued for Copernicus's system of the basis of *virtue inequivalence*, and in his case he appealed to the virtues of *causality* and *unification*. Descartes's argument for the Copernican system, briefly summarized, runs as follows. He begins from his theory of matter, which is in turn based on his criterion of clear and distinct ideas (his principle of intelligibility). This tells us that the principal attribute of matter is extension, and that the world is made of little bits of matter in relative motion with respect to one another. According to this theory of matter, there is matter in motion everywhere. Descartes claims that this matter in the heavens must be fluid (III.24) and that this fluid is everywhere and carries the bodies (the planets) along with it (III.25). With this background in place, Descartes then argues as follows. Since Tycho's model involves intersecting orbits, no such fluid account can be consistent with it (III.39), but Copernicus's model does not involve intersecting orbits, so a fluid account is not automatically inconsistent with it. Therefore, we should reject Tycho's system and accept Copernicus's. This argument is based on causality and unification. First, it sets up a causal account of planetary motion, based on a background matter theory. Then, it seeks to embed each of the available theories of mathematical astronomy within that causal account, and shows that only one of the two theories can be so embedded (the Copernican theory). Thus, we can have a successful *unification* between planetary motion and our broader physics if we adopt Copernican theory over Tychonic theory.

For our purposes, it is important to note that Descartes's criterion of success here involves *qualitative* (or at most approximate quantitative) consistency between the theory and the observed phenomena. This goes against our methodological lesson from Kepler, but Descartes had good reason for it. According to his matter theory, the planets' motion arises from constant collisions, and it is only the coarse-grained motions that are potentially informative. Therefore, from the perspective of Descartes's matter theory, the details of the planetary motions are epistemically impotent. Descartes could have been right about this. It turns out that he wasn't, but this has to do with special features of our planetary system and of gravitation, which we have come to understand only much later. So we must introduce another note of caution to the lessons taken away from our discussion of

Kepler, in section 2 , above. It may turn out that not all systems (perhaps certain kinds of nonlinear systems, for example) will be appropriately tackled by the methodology suggested there.

## 4. Concluding remarks

I began with the question "Given two observationally equivalent theories, how should we respond?", and separated this into two questions: "How (if at all) do we decide between them?", and "To what exent, if at all, can we have confidence that the choice we make now will not turn out to be replaced later by a radically different, yet observationally equivalent, theory?". What have we learned?

With respect to the first question, in the case of Ptolemy versus Copernicus we saw a diachronic process of mobilizing different forms of inequivalence towards deciding between them, and the eighteenth century saw an emerging consensus that (contra Descartes) achieving observational inequivalence is necessary (though not sufficient) for making a decision. The main kinds of (in)equivalences that we saw doing work were: observational; empirical; modal; and virtue. Under "virtue", I included such considerations as simplicity, harmony, causality, and unification.

The second question asks about what elements of our theories, if any, are likely to last. On the basis of the methodology that developed during this period, we can conclude that certainly it is not "theories as descriptively true stories about the world". This is from van Fraassen's highly influential characterization of scientific realism, but adopting such an aim for science would result in a mismatch between the methodology and the stated aim. In particular, this characterization of the aim of science has no sensitivity to Kepler's genus/species distinction, nor to the central role of modal dependencies among the parameters and observables (what are the observationally relevant parameters, and if I fiddle with *this* by exactly this amount, what precisely happens to *that*?) in enabling scientific theories to do the work that they do. If we try to fit our characterization of the aims of science to what the methodology has been developed to deliver, then perhaps we might try something like this:

> "An aim of science is to provide a set of modal dependencies adequate to the observable phenomena. Realism is a commitment to these modal dependencies as underlying the observable phenomena."

Theories are sometimes a good tool for discovering and expressing these modal dependencies.

One reason I like this is because it evades Kyle Stanford's (2006) circularity challenge (we can discover that we've been systematically misled, recall Ptolemy II).

In sum, the Ptolemy versus Copernicus crisis led to methodoloical developments by which we know what we need to do to decide between observationally equivalent theories. In addition, we have no good reason to think that the results that this methodology delivers (the modal dependencies) are unlikely to survive theory-change. Moreover, if we are feeling in a realist frame of mind, we can note that our commitment to these results is a commitment beyond the phenomena, to the underlying modal dependencies, and so is a form of realism. We have, I maintain, the resources to address the epistemic challenge from observationally equivalent theories.

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