Responding to Ptolemy versus Copernicus: equivalence and inequivalence in the development of scientific methodology

Katherine Brading
University of Notre Dame
kbrading@nd.edu
March 29, 2015

Abstract. I use the case of Ptolemy versus Copernicus to distinguish observational, empirical, modal, and virtue (in)equivalence, and to show that during the two hundred years between Copernicus's *De Revolutionibus* and Du Chatelet's *Institutions de Physique* we learned a great deal 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 stable and likely to persist through theory change.

What follows is a written version of a talk given at the workshop on Equivalent Theories in Science and Metaphysics, held at Princeton on March 20-21, 2015, and is best read in conjunction with the slides that were used for this talk, posted separately.

Please cite this as a manuscript, thank you!

[slide 1, title slide]

Introduction

[slide 2]

I'm going to talk about an old and familiar example of equivalence between theories: Ptolemaic astronomy and Copernicus's astronomical system. I like this example because it's simple, it's familiar, and it's at the heart of what drives some really interesting developments in scientific methodology.

The key question – and it's one which was a major component of the epistemological crisis of the seventeenth century – 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. This question concerns whether or not we can have confidence that the choice we make (if we do pick one of the theories over the other) is going to survive the test of time, rather than turning out to be observationally equivalent to some other completely different theory that we end up picking in the future, so that the one we've chosen now ends up being rejected too. So our second question is this: "How (if at all) can we establish something in the sciences that is stable and likely to last?" And if you remember your Descartes, this is exactly the question that he poses at the start of his First Meditation.

The main point of my talk is going to be to make vivid that between 1543 (which is when Copernicus's De Revolutionibus was published) and 1740 (when we get Du Chatelet's *Institutions de Physique*), we learned a lot about how to respond to the epistemic challenges posed by equivalent theories. I will spell some of this out in detail.

[slide 3]

In section 1, I begin by setting up the basic equivalences and inequivalences between Ptolemaic and Copernicus's astronomical systems. In sections 2 and 3 I consider two different responses to the observational equivalence of these two systems, due to Kepler and Descartes respectively. In section 4 I skip forward a century to Du Chatelet, to draw together methodological lessons learned in the wake of Kepler, Descartes, Huygens, Leibniz and Newton. In section 5 I end with some concluding remarks on these methodological lessons, and on what we can expect theories developed within this framework to deliver. In particular, I suggest that scientific realism as formulated by van Fraassen attributes goals to science that are not aligned with this methodology.

[slide 4]

1.Equivalences and inequivalences in Ptolemy versus Copernicus

1.1 Geometrical equivalence and observational equivalence

The Ptolemaic 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*. In the Ptolemaic model, the Earth is situated at the center of the deferent of the planet in question, and then the planet moves on an epicycle which rotates as its center moves around the deferent. This can be seen in the lefthand diagram on the slide. The blue dot at the center is the Earth, the blue circle is the deferent, the red circle is the epicycle, centered on the deferent, and the red dot is the planet. The yellow line is the line of sight from the Earth through the planet to the background of the fixed stars. This is what we do to plot the motion of a planet: we plot its path relative to the background of the fixed stars. This is a two dimensional path, and is what is observable. In Copernicus's model, the Earth is on its own orbit, as it the planet. This is shown in the right hand diagram on the slide. The blue dot is again the Earth. The yellow dot is the Sun, and the red circle is the orbit of the Earth around the Sun. The red dot is the planet of interest, and the blue circle is now the orbit of that planet around the Sun. The yellow line is once again the line of sight from the Earth through the planet to the background of the fixed stars. There is geometrical equivalence between the model offered by Ptolemy and the model offered by Copernicus. If you hit "play" on these diagrams, you will see that they produce exactly the same observable motion of the planet with respect to the background of the fixed stars.

There are subtleties, arising from Copernicus's refusal to make use of Ptolemy's equant, for example, but the central epistemological challenge arises from the *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. This observational equivalence arises from the geometric equivalence of the basic models pictured on the slide.

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

[slide 5]

So, for each planet, Ptolemy's and Copernicus's systems are geometrically equivalent, and observationally equivalent. Nevertheless, 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. This is shown in the right-hand diagram on the slide. The Sun is over on one side of the sky, as viewed from Earth, and Mercury is on the opposite side of the sky. 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. This can be seen in the lefthand diagram on the slide. As viewed from Earth, the smaller orbits of Venus and Mercury keep them in close angular proximity to 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. As we saw ([slide 4]), 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. When it comes to the relative sizes of the deferents, and therefore the ordering and spacing of the planets, there is nothing in Ptolemy's system to allow such comparisons. In other words, 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 second circle (the red circle in the diagrams) that is the epicycle in Ptolemy's system, is the Earth's orbit in Copernicus's system (we are considering the outer planets). 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. Not only is the relative size of the blue and red circles fixed, but the red circle has the same size across the models for every 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. There was no way to get three-dimensional information on the order and spacing of the planets out of this two-dimensional information using Ptolemaic astronomy. (In practice, astronomers used the period of the planetary orbits as a guide to their order.) Copernicus's theory, on the other hand, provides a way of getting from two-dimensional information to three-dimensional information. This is unobservable unless we can get off the Earth and travel out among the planets (or, less extravagantly, make use of the relative enlargement of the planets provided by a given telescope, but the development of the telescope doesn't take place until the early 1600s), but is nevertheless an empirical inequivalence: Copernicus's system rules out relative spacings of the planets that Ptolemy's system allows, and these are in principle observable, so the theories are empirically inequivalent.

Empirical inequivalence – differing over which observations are possible and impossible – is a case of *modal inequivalence* of theories. What's epistemically interesting, I take it, is cases where we have theories that are *observationally equivalent but modally inequivalent*.

My empiricist tendencies make me inclined to say that, in the end, any interesting modal equivalence must be an empirical inequivalence, but we should not collapse the two. (And "in the end" here matters, as a warning not to be premature ruling out modal inequivalences that are not also empirical inequivalences, and as a methodological recommendation, that we should strive to turn modal inequivalences into empirical inequivalences.)

[slide 6]

Given the observational equivalence but modal inequivalence of Ptolemy versus Copernicus, is there an epistemically responsible way of deciding between them? In what follow, I will consider the responses of Kepler and Descartes.

[slide 7]

2. Kepler

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

Kepler came from a good family, but one which was no longer well-off. Kepler's grandfather ran the family, and his father wasn't around much, coming and going until Kepler was 16, at which point he left and never came back. Kepler was raised by his mother, Katharina, and he was her first child. He was small and sickly, but fortunately he survived, and he did well at school -- he was selected for an education designed for those going on to university. He was awarded a scholarship by the local Duke, and in 1589 at the age of 17 (rising 18) he went off to the University of Tübingen. The two areas that interested him most were mathematics (including astronomy) and theology. His mathematics and astronomy teacher was Michael Maestlin. At this time, where Copernicus's theory was taught, it was taught as mathematical astronomy, as a device for performing calculations, not as the truth about the structure of the cosmos. Maestlin was different. He was one of the first astronomers who took Copernicus's *cosmological* claims seriously, partly because of the 1577 comet. And so Kepler was taught astronomy by one of the few – perhaps the only – convinced Copernican teaching at a university in Europe at the time.

Kepler's first major astronomical publication, and the book which made his name, was *Mysterium Cosmographicum*, published in 1596. In this book, Kepler argues for the Copernican system, using a range of arguments, as we'll see. 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 what I shall call "*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. I will say more explictly what counts as a "virtue" once I have put more examples on the table.

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 great insight: in Ptolemy's system, the basic planetary motion is motion around the deferent, and in order to account for retrograde motion, and second circle must be added – the major epicycle – thereby complicating the model. In Copernicus's system, the basic planetary motion is again a single circular orbit, and *nothing* needs to be added to the model in order to recover retrograde motion: with the Earth in motion, the appearance of retrograde motion of a planet arises from the relative motion between the Earth (from where we are observing the planet of interest) and the planet we are observing. So, simply by putting the Earth into motion, Copernicus removes the need for any major epicycles, and thereby implements a dramatic simplification of mathematical astronomy. Kepler took this to count strongly in favor of Copernicus's system. However, this was also a well-known feature of Copernicus's system at the time, and few were persuaded by this argument from simplicity.

Kepler needed a stronger argument, and when he found it, he reportedly wept tears of joy.

2.2 *Mysterium Cosmographicum*: Kepler's arguments from harmony and causality

[slide 8]

In his biography of Kepler, John Banville deliciously brings to life Kepler's moment of discovery as follows (Banville, 1981, p. 27):

> "The day was warm and bright. A fly buzzed in the tall window, a rhomb of sunlight lay at his [i.e. Kepler's] feet. His students, stunned with boredom, gazed over his head out of glazed eyes. He was demonstrating a theorem out of Euclid – afterwards, try as he might, he could not remember which – and had prepared on the blackboard an equilateral triangle. He took up the big wooden compass, and immediately, as it always contrived to do, the

> monstrous thing bit him. With his wounded thumb in his month he turned to the easel and began to trace two circles, one within the triangle touching it on its three sides, the second circumscribed and intersecting the vertices. He stepped back, into that box of dusty sunlight, and blinked, and suddenly something, his heart perhaps, dropped and bounced, like an athlete performing a miraculous feat upon a trampoline, and he thought, with rapturous inconsequence: I shall live forever. The ratio of the outer to the inner circle was identical with that of the orbits of Saturn and Jupiter, the furthermost planetsm and here, within these circles, determining that ration, was inscribed an equilateral triangle, the fundamental figure in geometry. Put therefore between the orbits of Jupiter and Mars a square, between Mars and earth a pentagon, between earth and Venus a … Yes, O yes. The diagram, the easel, the very walls of the room dissolved to a shimmering liquid, and young Master Kepler's lucky pupils were treated to the rare and gratifying spectacle of a teach swabbing tears from his eyes and trumpeting juicily into a dirty handkerchief."

What Kepler had done was to draw a circle with an equilateral triangle inscribed, and then another circle inscribed in the triangle. The ratio of the large circle to the small circle was about the ratio of Saturn's orbit to Jupiter's orbit.
Perhaps a square inside this circle, and then a circle in that square would give him Mars' orbit. This was what made him so excited: the prospect of uncovering a beautiful geometric harmony underlying the spacing of the planets in the Copernican system.

However, it didn't work. Depsite Kepler's best efforts, this first proposal with 2-dimensional circles, triangles and squares didn't work out. But the universe is 3-dimensional, not 2-dimensional! What if he used spheres instead of circles and 3-dimensional polygons instead of 2-dimensional shapes? This time, Kepler found what he was looking for.

There are exactly 5 regular polygons – no more, no less. 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. Here is how Kepler recalled his discovery, in his own words (for an animation, see slide 8, but note that this animation was desigend for a domed projection system and is less impressive when viewed on a 2-D screen!):

> The earth's circle is the measure of all things.
> Circumscribe a dodecahedron around it.
> The circle surrounding it will be Mars.
> Circumscribe a tetrahedron around Mars.
> The circle surrounding it will be Jupiter.
> Circumscribe a cube around Jupiter.
> The surrounding circle will be Saturn.
> Now, inscribe an icosahedron inside the earth.
> The circle inscribed in it will be Venus.
> Inscribe an octahedron inside Venus.
> The circle inscribed in it will be Mercury.

Mercury
octahedron
Venus
icosahedron
Earth
dodecahedron
Mars
tetrahedron
Jupiter
cube
Saturn

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 not undetermined in Ptolemy's system). In other words, hidden in Copernicus's orbits (unlike Ptolemy's) is a deep structure, an inner harmony. This is what Kepler has uncovered and laid before our eyes, thereby arguing for Copernicus's system on the basis of harmony.

Kepler's Platonic Solids model of the cosmos is the centerpiece of *Mysterium Cosmographicum*. With it, he offers a new argument for Copernicus's system, and one which stayed with Kepler for the rest of his life. However, despite the beauty of the model, few were persuaded by it.

[slide 9]

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.

So, to sum up where we are so far, having considered Kepler's *Mysterium Cosmographicum*. At this point in time, Kepler accepted the geometrical and observational equivalence of Ptolemy's and Copernicus's theories. He also accepted their instrumental inequivalence, but did not use this to argue for Copernicus's system. Instead, he first appealed the well-known empirical inequivalences, and to the virtue inequivalence of simplicity that was Copernicus's great insight, but these did not persuade many. Second, he developed new arguments, both based on virtue in equivalence, once of which appealed to harmony (his Platonic solids model) and the other of which appealed to causality (his attempted distance-speed relationships). Few were persuaded by these new arguments, either.

2.3 *Astronomia Nova*: from virtue inequivalence to observational inequivalence

[slide 10]

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, and so Kepler's version of the Copernican system and Ptolemy's system are observationally *in*equivalent.

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 this to construct arguments in support of his preferred option. In the end, after much hard labor, use of his causal story led him to develop a new version of Copernicus's theory that was observationally inequivalent to Ptolemy's theory.

2.4 Lessons from Kepler

[slide 11]

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 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. Assume, for the sake of argument, that both Ptolemy and Copernicus allow that Venus may be an extended body, and that it may have no light of its own (reflecting light from the Sun). Ptolemy's and Copernicus's systems make different predictions concerning the possible 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: How (if at all) can we establsih something in the sciences stable and likely to last? Or, focusing specifically on the epistemic challenge from observational equivalence: 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*:

[slide 12]

> "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:

[slide 13]

> "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."

[slide 14]

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. In these details, we find a further lesson from Kepler: It's these aspects of a theory -- the 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". Using this for our question about long-term stability of theories, the proposal is that if we think of theories in this way, then developments at the level of species do not threaten the genus.

[slides 15 and 16]

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, irrelevant to the observational success of Ptolemaic astronomy. Having granted this (and set to one side the geocentric/heliocentric difference in species), 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. And this is very informative, as we will now see.

[slide 17]

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 wil 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.

[slide 18]

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

Kepler's first law: The orbits of the planets are ellipses with the Sun at one focus.
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.

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, let's 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.

As you can see, 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. To see this, look at the animation on slide 19.

[slide 19]

The red circle is the orbit of the planet. The Sun is the yellow dot, and the purple dot (in the yellow circle) is the equant – located at an equal distance on the opposite side of the center of the circle from the Sun. Set the animation into motion. The pale blue dot is the position of the planet according to Kepler's laws, to first order approximation in eccentricity). The dark blue dot is the position of the planet according to Ptolemy II's laws. 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 you can see, the predicted positions of the planets deviate only slightly from one another. In fact, the eccentricity shown in this diagram is much greater than that of the planets, and as you reduce the eccentricity, and the Sun and equant move closer to the center of the circle, the differences between the predictions of the two systems decreases. For the actual eccentricities of the planets, the differences are less than observational accuracy prior to Tycho, and they are therefore observationally equivalent up to the observational accuracy of the time.

[slides 20 and 21]

In sum, the features of Ptolemy II's theory responsible for its observational success concern first the shape of the orbits, and for Ptolemy II these are eccentric circles, and second the speeds of the angular planets in their orbits, and for Ptolemy II these are determined according to equal angles in equal times about the equant. To first-order in eccentricity, which is observational accuracy prior to Tycho, Kepler and Ptolemy II are observationally equivalent. Only when second order effects (ellipticity) become observable, do the two come apart observationally.

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.

[slide 22]

What happened was that, 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. (See Julian Barbour, Absolute or relative motion?)

In light of this epistemic lesson, we need to modify what we had learned thusfar from Kepler.

[slide 23]

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.

This is a methodological lesson that was brought to rich fruition by Newton, as has been beautifully argued by George Smith (see his Closing the Loop), and is central to the discussions of Newton's *Principia* by both George Smith and Bill Harper. But that is to get ahead.

3. Descartes

[slide 24]

In Part III of Descartes's *Principles of Philosophy*, Descartes considers the three basic systems of mathematical astronomy available at the time: Ptolemaic, Tyychonic, 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.

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*.

[slide 25]

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 (this point comes out in 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. As notes above, this is an argument 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.

[slide 26]

For our purposes, it is important to note the the criterion of success here involves *qualitative* (or at most approximate quantitative) consistency between the theory and the observed phenomena. Descartes claims in *Principles* Part III that all the observations of the planetary motions are consistent with his vortex theory, including that the shapes of the orbits are not perfectly circular (III.34), and the latitudinal and longitudinal variations (III.35&36), and concludes (III.37) that "all the phenomena of the Planets can be explained by the hypothesis proposed here." In other words, qualitative consistency with the observed phenomena is sufficient for acceptance of the theory.

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. The fine-grained motions arise from innumerable collisions, and there is no good reason to think that they follow any precise mathematical laws. Moreover, the coarse-grained motions are likely to be temporary, and subject to change over time. 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 (see George Smith's Closing the Loop), 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.

[slide 27]

There is another lesson from Descartes that is important for our topic of (in)equivalent theories. The upshot of Descartes's methodology is radical underdetermination. In *Principles* Part IV, paragraph 204, he writes: "it suffices if I have explained what imperceptible things may be like, even if perhaps they are not so." And he then elaborates as follows:

> "For just as the same artisan can make two clocks which indicate the hours equally well and are exactly similar externally, but are internally composed of an entirely dissimilar combination of small wheels: so there is no doubt that the greatest Artificer of things could have made all those things which we see in many diverse ways."

In other words, the same observable phenomena are compatible with many different accounts of the arrangements and motions of the imperceptible parts: we have a proliferation of observationally equivalent theories, and no means to decide between them. The lesson is clear: the principle of clear and distinct ideas, plus causality and unification, by themselves lack the epistemic purchase we are looking for if we are to address the problem of observationally equivalent theories.

4. . Lessons in methodology: Du Châtelet, *Institutions de Physique* (1740)

[slide 28]

I am now going to skip ahead 100 years, to the methdological lessons learned in the wake of the geocentric/heliocentric dispute, or the dispute over the "system of the world". Book 3 of Newton's *Principia* is entitled "The System of the World", and the entire *Principia* is directed towards the resolution of this problem. Even a generation after this, not everyone took the dispute to have been settled, but enormous progress had been made on methodology. There is a great deal that I could (and should) say about the methodological developments wrought by Newton himself, but I don't have the time (in my talk) or the space (in this written version) to discuss both Newton and Du Châtelet, and Newton's methodology has been the subject of much excellent recent scholarship, so I will skip forwards to Du Châtelet.

Du Châtelet was writing in France at a time when Cartesian physics is dominant. Du Châtelet begins her *Institutions de Physique* by praising Descartes's many important contributions to physics, but then turns her attentions to failures of his method. By the time she was writing, there had been a proliferation of Cartesian hypotheses put forward to explain all manner of phenomena. Du Châtelet writes (*Institutions*, Chapter 4, para 55) that "the books of philosophy, which should have been collections of truths, were filled with fables and reveries". So the question is pressing: How can we break the equivalence between all these hypotheses, and arrive at theories that are stable and likely to last?

[slide 29]

Du Châtelet insists that an improved methodology is needed. Specifically, we must reject "clear and distinct ideas" as the principle of intelligibiity by which to judge hypotheses, not least because this principle is too subjective. She argues that we should adopt the principle of sufficient reason (and especially the principle of continuity, which she argues is a corollary of the principle of sufficient reason) instead (see *Institutions* 1.8). She also argues that we should reject "mere compatibility" with what has already been observed as the empirical standard by which to judge hypotheses: hypotheses must have testable observational consequences, and we must pursue all the observational consequences of a hypothesis.

[slides 30 and 31]

Du Châtelet advocates strengthening our use of both (i) virtue inequivalence and (ii) observational and empirical inequivalence.

In the case of virtiue in equivalence, her primary emphasis is on the principle of sufficient reason. According to Du Châtelet, the principle of continuity follows from the principle of sufficient reason, and in her hands both these principles receive a causal interpretation, concerning the relationship between successive states of a system. (Her elaboration of this is super fascinating, and has a long-lasting impact, with her discussion of continuity having been quoted in the Encyclopedie of Diderot and D'Alembert, and her powerful anticipation of what became known as Laplace's demon.)

A second methodologically important type of virtue inequivalence for Du Châtelet is simplicity. She discusses at length the constraints that we need to place on our hypothesizing. She writes that "it is necessary… that the phenomenon result necessarily, and without the obligation to make new suppositions", and goes on: "When the necessary consequences do not follow from it, and to explain the phenomenon, a new hypothesis must be created in order to use the first, this hypothesis is only a fiction unworthy of a philosopher." (*Institutions* 4.69) In this way, she argues for simplicity and against ad hoc hypotheses.

A third methodologically important virtue emphasized by Du Châtelet is unification. We have seen that this was important for Descartes, and it was also important for Newton, whom Du Châtelet studied carefull but whom we have not discussed. Du Châtelet insists from the outset on the Cartesian ideal of a single matter thoery by which to account for all observable phenomena, both animate and inanimate, and boh celestial and terrestrial.

[slide 32]

Turning to observational and empirical inequivalence, Du Châtelet discusses in detail the ways in which the observational consequences of a theory must be worked out in every detail and tested empirically. Empirical tests are a stern master, and empirical falsification a powerful tool. She writes (*Institutions*, 4.64):

> "One experiment is not enough for a hypothesis to be accepted, but a single one suffices to reject it when it is contrary to it."

She elaborates further in the ensuing paragraphs. For example (4.66):

> "Thus, in making a hypothesis one must deduce all the consequences that can legitimately be deduced, and next compare them, with experiment; for should all these consequences by confirmed by experiments, the probability would be greatest. But if there is a single one contrary to them, either the entire hypothesis must be rejected, if this consequence follows from the entire hypothesis, or that part of the hypothesis from which it necessarily follows."

As we see even from these brief extracts, Du Châtelet discusses falsification, conditions for acceptance of theories, and is explicit that falsification may be selective, applying to only parts of the theory (this echoing the genus/species distinction that we saw in Kepler).

(As Jim Weatherall pointed out in discussion after my talk, this is an early text – 1740 – in which to find discussion of raising the probability of a hypothesis through experimental confirmation of the consequences of the hypothesis.)

[slide 33]

Jointly, the use of theoretical virtues and observations of empirical consequences of hypotheses, push the development of scientific theories forwards in a way that achieves two important methodological goals. First, the proposed methodology keeps the competition manageable, by ruling out (fallibly) any (unconceived) alternatives that don't satisfy the principles of intelligibility. (Here it would be important to compare Du Châtelet with Newton's 4th rule of reasoning, but I have no time or space to do that here.) For example, the resources of Newton's *Principia* rule out the Tychonic system only by means of the rule "no forces without sources", which is a causal principle with its roots in Kepler (as we saw above). In other words, we need to appeal to virtue inequivalence to decide whether the Sun or the Earth is more nearly at rest. (See also George Smith's discussion of a manuscript in which Newton wrestles with ruling out the Tychonic system.)

Second, the methodology insists that we actively seek observational inequivalence between alternative theories. Once again, there are no guarantees that we will be successful, but we are required to try. An example of this is Du Châtelet's discussion of Huygens and Newton on gravitation, in which she makes vivid that tiny quantitative differences can make all the difference. I will end my discussion of Du Châtelet by spelling out this example in more detail.

[slide 34]

Du Châtelet discusses Huygens' vortex theory of gravitation, which contains no particle-to-particle gravitational interaction, and Newton's theory of universal gravitation, and the question of how we are to decide between these two theories.

Back in his *Principia*, Newton had argued that no vortex theory could reproduce the trajectories of the planets (call these the "bulk" motions), thereby arguing for the observational inequivalence of his theory as compared to vortext theories (see *Principia*, Book 2, Section 9, Scholium to Proposition 53). However, this argument depends on assumptions and idealizations about fluids. Huygens, Bernoulli and others rejected these assumptions and developed vortext theories intended to recover the observed bulk motions. If correct, the upshot would be *observational equivalence* with Newton's theory, at least for bulk motions.

[slide 35]

Given observational equivalence for bulk motions, how should we decide between these theories. As we have seen, Du Châtelet insists on including both observational and virtue considerations in theorizing, so perhaps the place to turn is to seek virtue inequivalence. According to Du Châtelet, Huygens' theory satisfies the principle of sufficient reason, whereas Nweton's theory (as an action-at-a-distance theory) does not. Thus, virtue inequivalence favors Huygens's theory.

However, according to Du Châtelet's methodological considerations, we must not stop there. Rather, we must seek empirical equivalences and try to turn them into observational equivalences.

[slide 36]

Du Châtelet is very clear about exactly where those empirical inequivalences lie, and she is completely up-to-date on the observational evidence relating to these empirical inequivalences. Recall the difference between Newton's and Huygens' theories mentioned above. According to Newton, gravitation acts from particle to particle, including on and between the interior particles of a given body: "Therefore the gravity toward the whole planet arises from and is compounded of the gravity toward the individual parts." (Newton, *Principia*, Book 3, Proposition 7, Corollary 1) This is universal gravitation. Huygens responded to this by rejecting the step in

Newton’s argument that takes him from the motions of the planets (“bulk motions”) to “universal gravitation”. Thus, for Huygens, if we can recover the bulk motions, the challenge from Newtonian gravitation has been met. The key question for us is whether, in this difference over universal gravitation, we can find the resources to generate an empirical inequivalence.

[slide 37]

As Du Châtelet discusses, by the 1730s an empirical difference had been uncovered between the two theories, and this was fast becoming an observational inequivalence. In Chapter 15, para 379, of the *Institutions*, Du Châtelet states:

> “M. Huygens believed the gravity to be the same everywhere [because it pertains to the body considered as a whole], and Newton assumed it to be different in different places on earth and dependent on the mutual attraction of the parts of matter: the only difference between them is the shape they attribute to the earth – since from M. Newton’s theory arises a greater flattening than from that of M. Huygens.”

Thus, Du Châtelet is very clear about the difference between the two approaches being due to the disagreement over universal gravitation (i.e. whether it is particle to particle or not), and on where the observational consequences differ. The empirical inequivalence, and its source, between the two theories is clearly articulated.

[slide 38]

Moreover, Du Châtelet is up to date with the efforts to measure the shape of the Earth, and is awaiting further results that will help to determine the question between Huygens and Newton. She says that initial results from the measurements taken on the expedition to the pole led by Maupertuis favor Newton:

> “The one that comes from the measurements at the Pole is approximately as the one that M. Newton had determined with his theory. Thus, it is true to say

> that M. Newton made great discoveries owing to the measurements and observations of the French and that he will most likely receive confirmation." (*Institutions*, 15.384)

This makes it look as though observation is decisive.

[slide 39]

However, recall that Du Châtelet's methodology requires the pursuit (perhaps indefinitely, she suggests, given our epistemic capacities) of an interplay between the observational and the virtues. Du Châtelet's retains a hope that a PSR-satisfying theory might one day be found, and so she cautions against throwing out the genus for the species. She writes that it remains "to be examined if some subtle matter is not the cause of this phenomenon… perhaps a time will come when we will explain in detail the directions, movements, and combinations of fluids that operate the phenomena that the Newtonians explain by attraction, and that is an investigation with which the physicians must occupy themselves." In other words, since our theory does not satisfy our theoretical virtues, we cannot consider out work done. Rather, we *must* press forwards in seeking to develop our theory in line with both our observational and our virtue criteria.

[slide 40]

Let's sum up where we are by 1740, the date of Du Châtelet's *Institutions*. First of all, in order to constrain the multiplication of observationally equivalent theories, we need to use *theoretical virtues* as well as observational evidence. *Observational inequivalence* in theory choice is necessary *but not sufficient*. It is central to Du Châtelet's methodology that we *actively seek* observational inequivalence, and especially precise, quantitative observational inequivalence. Moreover, observational inequivalence is an important criterion of theory choice (recall her emphasis on falsification). Nevertheless, even if a theory meets the stringent observational tests discussed by Du Châtelet, we must not rest there if our theory does not also satisfy our principles (our virtues). These work in tandem with the observational criteria, and have a powerful epistemic role in Du Châtelet's methodology. Moreover, observational evidence must be handled with care, and I particular we must be careful not to throw out the genus for faults of the species. Finally, as Du Châtelet emphasizes, all of this is fallible.

So, given this methodology, which developed to a great extent in response to the epistemic challenge posed by the observational equivalence of Ptolemy versus Copernicus, what (if anything) is stable and likely to last? I will address that question as part of my closing remarks, in the final section.

5. Concluding remarks

[slide 41]

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 "How (if at all) can we establish something in the sciences that is stable and likely to last?". 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 an emerging consensus that (contra Descartes) achieving observational inequivalence is necessary (though not sufficient) for making a decision.

[slide 42]

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; principles of intelligibility such as Descartes's clear and distinct ideas and the principle of sufficient reason; and the principle of continuity. Another important example that came to the fore in the seventeenth century is that of conservation laws. An example from the twentieth century is symmetry principles.

[slide 43]

The second question asks about what elements of our theories, if any, are stable and likely to last. On the basis of the methodology that we saw developing 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, and in light of what we have seen, it is clear that 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 (reflected in Du Châtelet's discussion), 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.

[slide 44]

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 circularity challenge (we can discover that we've been systematically misled, recall Ptolemy II).

In sum, by 1740 we have a methodology 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.

[slide 45]

Conclusion: We have the resources to address the epistemic challenge from observationally equivalent theories.

Thank you.

My talk was followed by a most enjoyable discussion period. My thanks to all those who made such interesting points and asked such excellent questions. Thanks also to the organizers of the event, and especially the local organizer Hans Halvorsen.