Studies in History and Philosophy of Science 52 (2015) 20–34

Contents lists available at ScienceDirect

Studies in History and Philosophy of Science

journal homepage: www.elsevier.com/locate/shpsa

Did Ptolemy make novel predictions? Launching Ptolemaic astronomy into the scientific realism debate☆

Christián Carman [a], José Díez [b,*]

[a] National University of Quilmes/CONICET, Argentina
[b] University of Barcelona/LOGOS, Spain

ARTICLE INFO

Article history:
Received 24 December 2014
Received in revised form
12 April 2015
Available online

Keywords:
Scientific realism;
Ptolemy;
Geocentrism;
Non-miracle argument

ABSTRACT

The goal of this paper, both historical and philosophical, is to launch a new case into the scientific realism debate: geocentric astronomy. Scientific realism about unobservables claims that the non-observational content of our successful/justified empirical theories is true, or approximately true. The argument that is currently considered the best in favor of scientific realism is the No Miracles Argument: the predictive success of a theory that makes (novel) observational predictions while making use of non-observational content would be inexplicable unless such non-observational content approximately corresponds to the world "out there". Laudan's pessimistic meta-induction challenged this argument, and realists reacted by moving to a "selective" version of realism: the approximately true part of the theory is not its full non-observational content but only the part of it that is responsible for the novel, successful observational predictions. Selective scientific realism has been tested against some of the theories in Laudan's list, but the first member of this list, geocentric astronomy, has been traditionally ignored. Our goal here is to defend that Ptolemy's Geocentrism deserves attention and poses a prima facie strong case against selective realism, since it made several successful, novel predictions based on theoretical hypotheses that do not seem to be retained, not even approximately, by posterior theories. Here, though, we confine our work just to the detailed reconstruction of what we take to be the main novel, successful Ptolemaic predictions, leaving the full analysis and assessment of their significance for the realist thesis to future works.

When citing this paper, please use the full journal title *Studies in History and Philosophy of Science*

1. Introduction: selective scientific realism as a meta-empirical, testable thesis

Scientific realism (SR) about unobservables claims that the non-observational content of our successful/justified empirical theories is true, or approximately true. As is well known, the argument that is currently considered the best in favor of SR is a kind of abduction or inference to the best explanation, dubbed the No Miracles Argument (NMA). NMA states that the predictive success of a theory that makes (novel) observational predictions while making use of non-observational content/posits would be inexplicable, miraculous, unless such non-observational content approximately corresponded to the world "out there". In short: SR provides the best explanation for the empirical success of predictively successful theories. Empiricists such as Van Fraassen have argued that NMA is question begging, or simply has false premises, for there is another (at least equally good, according to them) explanation of empirical success, namely empirical adequacy. Yet, most realists feel comfortable replying that empirical adequacy provides no explanation at all, or at best an explanation that is inferior to (approximate) truth.

This comfortable position enters into crisis when Laudan (1981) brings pessimistic meta-induction back into the debate (brings it back, as one may trace pessimistic induction back to at least

☆ Research for this work has been funded by the research projects FFI2012-37354/CONSOLIDER INGENIO CSD2009-0056 (Spain), and PICT2010 No. 319 (ANP-CyT, Argentina)/PIP No. 114-201101-00112 (CONICET, Argentina). We want to thank L. Laudan, S. French, B. van Fraassen and two anonimous referees for comments and criticisms to previous versions of this paper.

* Corresponding author.
E-mail addresses: ccarman@gmail.com (C. Carman), jose.diez@ub.edu (J. Díez).

http://dx.doi.org/10.1016/j.shpsa.2015.04.002
0039-3681/© 2015 Published by Elsevier Ltd.

Poincaré). Laudan reminds us that the history of science offers many cases of predictively successful yet (according to him) totally false theories, and provides a long list of alleged cases. Laudan's confutation, which is not a direct argument for antirealism but rather a rejoinder to NMA, is contested in different ways, among them that his list contains many cases in which the theory at issue was not really a piece of mature science or that it was fudged to make successful predictions. But not all cases could be so contested and realists acknowledged that in at least two important cases, the caloric and ether theories, we had successful *and novel* predictions made with theoretical apparatus that posits non-observable entities (the caloric fluid, the mechanical ether) which, according to the later theories that superseded them, do not exist at all, not even approximately. Realists accept that they must accommodate such cases and the dominant strategy for doing so is to become *selective*: when a theory makes a novel, successful prediction, the part of its non-observational content responsible for such a prediction need not always be the whole non-observational content. Indeed, many times it is only *part* of the non-observational content that is essential for the novel prediction, and it is *only* the approximate truth of *this* part that explains the observational success (some versions of selective realism may be traced back to Poincaré and Duhem).

We can summarize Selective Scientific Realism (SSR) thus: in really successful predictive theories (i.e. that make novel predictions) a part of the non-observational content, the part responsible for their successful predictions, is (a) approximately true and (b) approximately preserved by posterior theories which, if more successful, are more truth-like. SSR(a) explains synchronic empirical success and SSR(b) diachronic preservation (and growth) of empirical success. Importantly, SSR(b) makes the realist position empirically/historically testable; without something like SSR(b), SR would be merely testimonial: an assertion inaccessible to material assessment.

Different selective realists disagree on how to identify such realist parts, but this does not matter for our concerns here. What does matter is that, in order to be genuinely realist, any version of the SSR thesis must preserve its (meta-)empirical character: SSR is a (meta-)empirical thesis, i.e. an empirical thesis that is designed to explain a (meta-)empirical fact, namely the predictive success of science. No acceptable construal of the SSR thesis can make the realist claim a priori or conceptually true: SSR must be fallible, otherwise it would make justification and truth conceptually inseparable, thus becoming a form of antirealism. The selective realism claim is that, though fallible, both SSR(a) and SSR(b) are true. Since we do not have independent, non-observational direct access to the world to test SSR(a), the claim that is relevant for testing SSR as a meta-empirical thesis is SSR(b); and selective realists claim that the history of science confirms SSR(b). They maintain that the historical cases that count as confutations of, or anomalies for, plain, non-qualified realism are actually confirmative instances of its more sophisticated, selective reformulation SSR. Although caloric and ether theories are false, they are *not completely* false; each theory has a non-observational part that is responsible for the relevant novel successful predictions which (is approximately true and) has actually been approximately retained by its successor theory/theories. Thus, according to them history confirms SSR(b), the only testable part of SSR. Therefore, defenders of SSR conclude, SSR is an empirical thesis that, though fallible, is historically well confirmed.

This is the way in which SSR is committed to fixing any alleged anomaly. Confronted with an alleged case of a theory that made novel, successful predictions but—the opponent of SSR argues—whose non-observable content is not retained by the superseding theory, the selective realist must find a part of its non-observational content that is both: (i) sufficient for the relevant prediction, and (ii) approximately retained by the superseding theory. As an empirical thesis, SSR may face possible anomalies and the way it must fix them is always through this *divide et impera* move (Psillos, 1999). According to some (e.g. Chakravartty, 1998; Psillos, 1999; Worrall, 1989), SSR successfully fixed the caloric and ether anomalies, while according to others (e.g. Chang, 2003; Laudan, 1981) it has not done so (not yet, or not fully). The debate continues, and other anomalies are presented and discussed. For instance, the phlogiston case, initially dismissed as a pseudo-case but later acknowledged by some as a real, troublesome case and faced down in a similar SSR-friendly manner (Ladyman, 2011).

Our goal here is to launch a new case into the debate: geocentric astronomy. It was another item on Laudan's list (actually, the first one on his list), though it is often dismissed as not really making novel predictions, just accommodating known facts (e.g. Psillos, 1999: 105). We argue that this is not so. The no-novel-predictions tag attached to Ptolemy's astronomy is a consequence of the mere epicycle-plus-deferent accommodating mechanism‖ reading of the theory; a myth that, like all myths, is both popular and false. We find this case particularly useful because it is relatively easy to find the parts responsible for the predictions. In other cases, such as the caloric or ether cases, much of the discussion and disagreement between realists and their opponents concerns whether some non-observational part of the theory was really necessary for the relevant prediction. Was the solid, mechanical substance with orthogonal vibrations necessary to derive Fresnel's laws, from which the white spot prediction follows? Realists say "no" (to the mechanical substance); opponents say "yes". Was the material fluidity of caloric essential for Laplace's derivation of the speed of sound in air? Realists say "no"; opponents say "yes". And one finds similar controversies in other cases. In the case of Ptolemy, however, the contents responsible for the predictions are relatively easy to identify.

We take the Ptolemy case to be not only especially manageable, but also especially interesting. For here, the SSR strategy consisting of trying to find in the superseding theory a part that approximately retains the parts of the superseded theory responsible for the prediction seems prima facie particularly difficult, if not unpromising. Contrary to other cases (such as the caloric and ether cases) in which the contenders agree that *some* part is retained and the disagreement focuses on whether that part suffices for the relevant predictions, in this case it is hard to find any relevant retained part, thus making the realist case particularly contentious.

A detailed discussion and assessment of the significance of the geocentric predictions for the SSR debate is beyond the scope of this paper, however. Although in every case we eventually discuss some criticisms that might be addressed against it, and in the last section we briefly mention some immediate general criticisms that the realist might raise, we are not able to analyze and assess now in detail the different strategies that realists might try in order to overcome the difficulties that, at least prima facie, these cases pose for SSR. Nor we can analyze here the possible application to our case of some general realist strategies against alleged counterexamples (such as some suggested in Vickers, 2013). This goes beyond the limits of this paper and is left for future work. We confine our goals here to a more limited scope: merely to reconstruct in detail the Ptolemaic predictions and launch them into the scientific realism arena showing that this case deserves, at least prima facie, close attention. Although geocentric astronomy has often been referred to in the SR literature, to the best of our knowledge its alleged novel predictions have never been presented and analyzed in detail, not even by Laudan himself who, as we mentioned, puts it as the first item in his list (the theory is not even mentioned in the, taken together, quite comprehensive lists of alleged counterexamples for selective realism in Lyons, 2006; Vickers, 2013).

Let us now proceed to Ptolemy's novel predictions. We present some of what we think are the best candidates for novel, successful predictions and for each one discuss whether it really qualifies as a prima facie case that SSR should deal with.

2. Venus and Mercury, and only they, produce transits[1]

The successful and novel prediction involved in this first case asserts that Mars, Jupiter and Saturn are always beyond the Sun (i.e. they are the "outer planets"). This does not mean that you would never find any outer planet closer to the Earth than the Sun. That is also implied by Ptolemy's theory but it is not true; during retrograde motion, Mars is closer to the Earth than the Sun. It means that whenever Mars, Jupiter or Saturn are in conjunction with the Sun, they are beyond the Sun. This implies a prediction that could be confirmed by observation (but not observed without a telescope): when, during conjunction, the planet is as close to the ecliptic as to be eclipsed by or to eclipse the Sun, the Sun will eclipse the planet and not the other way around. I.e. contrary to what happens with Venus and Mercury, the outer planets do not produce *transits*. The most correct description of this prediction is, thus, that only Venus and Mercury produce transits.

Ptolemy is able to calculate the distance of the Sun and the Moon from the Earth. Then, partially using (in addition to his Geocentrism) the traditional order of the planets (Mercury and Venus before Mars, Jupiter and Saturn), Ptolemy asserts that, given certain theoretical features of his geocentric system, in the gap left by the Moon and the Sun there is only room for Mercury and Venus: therefore, Mars and the rest of the planets must be located beyond the Sun and could not produce transits. We now briefly describe how Ptolemy calculates the Moon's and Sun's distances in the *Almagest*, then we approach the issue of the order of the planets and the criteria discussed by Ptolemy. Finally we present the calculation offered by Ptolemy in his *Planetary Hypothesis* of the planetary distance for Mercury and Venus, and show that, actually, given his geocentric system, there is no room for any of the outer planets between the Earth and the Sun.

2.1. The distance to the Moon and to the Sun

Ptolemy considers that the parallax of very distant bodies can be ignored, but that this is not the case for the Moon which, due to its proximity to the Earth, shows a non-negligible parallax. Therefore, the distance of the Moon can be calculated using parallax. And this is exactly what Ptolemy does (*Almagest* V: 13; Toomer, 1998: 247–251). His calculation presents some difficulties,[2] but the value he obtains for the distance of the Moon at syzygies (the generic name for both opposition and conjunction) is approximately correct, and this is the value relevant for calculating the distance of the Sun: Ptolemy obtains that the mean lunar distance at syzygy is 59 *tr* (terrestrial radii) and the maximum distance is 64.16 *tr*.

Though Ptolemy uses the parallax for calculating the Moon's distance, the instruments available at his time (and for many centuries to come) did not allow the parallax of the Sun to be measured; so Ptolemy used another procedure to calculate the Earth–Sun distance. In the *Almagest*, after calculating the lunar distance, he offers a calculation based on a diagram representing both solar and lunar eclipses at the same time. This method was used for the first time by Aristarchus (Heath, 1913), then by Hipparchus (Swerdlow, 1969; Toomer, 1974) and by many others after Ptolemy, including Copernicus (1543: IV, 18: 710–713). The method is independent of heliocentric or geocentric assumptions and uses only three data: the maximum lunar distance, the apparent lunar and solar radii when the Moon reaches its maximum distance (which are considered equal), and the radius of the Earth's shadow, also at the Moon's maximum distance (Perdersen, 2010: 203–214).

Ptolemy asserts that using a *dioptra*, he was able to conclude that the Moon and the Sun have the same apparent size when the Moon is at its maximum distance. He discards that instrument for measuring sizes, however, as it is too inaccurate and he decides to calculate the apparent sizes of the Moon and of the Earth's shadow in a very theoretical way using two very old eclipses (*Almagest* V: 14; Toomer, 1998:251–254). He obtains a lunar (and thus solar) apparent radius (β) of 15′ 40″ and calculates that the Earth's shadow (α) is 2.6 times greater, as in Fig. 1 (the center of the Moon is at M and the center of the Earth-shadow is at ES, therefore, β is the apparent radius of the Moon and α the apparent radius of the shadow-cone at the Moon–Earth distance).

We already knew that he obtained the maximum lunar distance at syzygy of 64.16 *tr*. Since it is not possible to go into all the details here, we only show how Ptolemy calculated the Sun's distance in a schematic way.

In Fig. 2, A is the center of the Sun, B is the center of the Earth and C is the center of the Moon. The line FK represents the size of the shadow of the Earth at maximum lunar distance. Fig. 1 (right) shows points F, C and K in a plane perpendicular to that of Fig. 2. While α represents the apparent radius of the Earth's shadow and β represents the apparent radius of the Moon and Sun (remember that they are equal). Then γ represents the horizontal parallax of the Moon and δ represents the solar horizontal parallax. The line BC represents maximum lunar distance at syzygy, which is 64.16 *tr*. Looking at the figure, it is easy to see that $\alpha + \beta + \varepsilon = 180^\circ$ and also that $\delta + \gamma + \varepsilon = 180^\circ$. Therefore $\alpha + \beta = \gamma + \delta$, i.e. the sum of the apparent lunar radius and the radius of the Earth's shadow is equal to the sum of the horizontal parallaxes of the Sun and the Moon. We know that β is equal to 15′ 40″ and α is 2.6 times β (since parallaxes

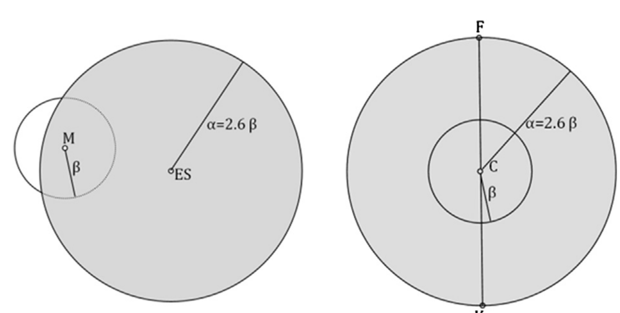

Fig. 1. M is the center of the Moon and ES is the center of the Earth's shadow at a lunar eclipse. β is the apparent diameter of the Moon and α is the apparent diameter of the shadow. In the left the moon is not totally eclipsed. In the right, the Moon is at the middle of a total eclipse, therefore both the center of the Moon and the center of the shadow are in C.

[1] This case elaborates an idea partially suggested by L. Laudan in personal communication.

[2] See Carman, 2009: 211–213 and the references at note 7, p. 210. Ptolemy arrives at the correct value by the "accidental interplay of a great number of different inaccuracies of empirical data and of computations that lead to nearly correct results" (Neugebauer, 1975: 106). Toomer (1998: 251, note 49) goes farther and asserts that Ptolemy altered the observational ad hoc. According to him, Ptolemy would have done this in order to obtain a mean lunar distance of 59 terrestrial radii, which would correspond to a minimum value that (according to a previous work, Toomer, 1974: 171) Hipparchus would have obtained. But the facts are that there is absolute no record of the value Toomer attributes to Hipparchus, and that there is no evidence at all that Ptolemy knew this alleged conjectured value and altered the data and calculations in order to obtained it. Without additional data, Toomer's claim is just a conjecture that may look plausible to him as the only explanation of a fact that, otherwise, looks like a miraculous coincidence.

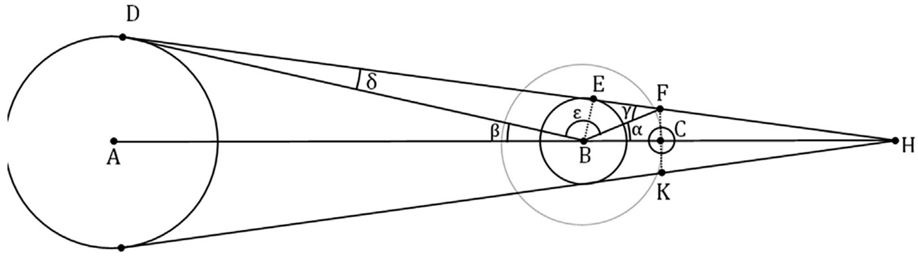

Fig. 2. Diagram of the calculation of the Sun distance according to Ptolemy. A is the center of the Sun, B is the center of the Earth and C is the center of the Moon. H is the vertex of the shadow cone.

are proportional to distances); consequently, $\alpha + \beta = 3.6$ $\beta = 56' 24''$. We also know that BC, the Moon's distance is 64.16 *tr* and this implies that γ is 53″35′. Therefore, the solar horizontal parallax, δ, is $(56' 24'' - 53' 35''=)$ $2' 49''$, which implies the Sun's distance is around 1220*tr*. Due to some rounding in the calculation, Ptolemy obtains 1210 *tr* (*Almagest* V: 15, Toomer, 1998: 257). In the *Almagest*, Ptolemy does not specify whether this value corresponds to the minimum, mean or maximum distance of the Sun, but as we see below, in the *Planetary Hypothesis* he says that it is the mean distance of the Sun. (Carman, 2009: 211; Goldstein, 1967: 7).

2.2. The order of the planets

The order of the planets was a very controversial issue in ancient times. The criterion expressed by Aristotle (De Caelo II: 10) was generally accepted: since the sphere of the fixed stars revolves fastest around the Earth and the Moon slowest, and the Moon is the closest celestial body, then the slower the body revolves around the Earth, the closer it must be. This criterion helps to order most, but not all, celestial bodies; for the mean longitudinal motion of the Sun, Mercury and Venus is the same. Therefore, applying the Aristotelian criterion, we know that the order is: 1) fixed stars, 2) Saturn, 3) Jupiter, 4) Mars, then the Sun, Mercury and Venus in an undetermined order, and finally 8) the Moon.[3]

In the *Almagest*, Ptolemy acknowledges these facts, including the underdetermined position of Mercury, Venus and the Sun: "but concerning the spheres of Venus and Mercury, we see that they are placed below the Sun's by the more ancient astronomers, but by some of their successors these too are placed above [the Sun's]" (*Almagest* IX: 1, Toomer, 1998: 419). Actually, we know that at least five of the six possible permutations of Mercury, Venus and the Sun were asserted in ancient times (Jones, 2006: 7, Neugebauer, 1975: 690–693). In the same passage, Ptolemy immediately says that those who assert that Venus and Mercury are farther than the Sun argue that the Sun has never been obscured by them, but Ptolemy discards this reasoning, arguing that the latitudinal motion of the planets could prevent the occurrence of transits, in the same way as most times the Moon is in conjunction, the Sun is not eclipsed because the Moon passes above or below the ecliptic. In the *Planetary Hypothesis* he offers an additional, stronger reason[4]: "If a body of such a small size [as a planet] were to occult a body of such large size and with so much light [as the Sun] it would necessarily be imperceptible, because of the smallness of the occulting body and the state of the parts of the Sun's body which remain uncovered". (Goldstein, 1967: 6; but we give the improved translation in Toomer, 1998: 419, note 2).

In the *Almagest*, Ptolemy continues by asserting that, "since there is no other way, either, to make progress in our knowledge of this matter, since none of the stars has a noticeable parallax [which is the only phenomenon from which the distance can be derived], the order assumed by the older [astronomers] appears the more plausible. For, by putting the Sun in the middle, it is more in accordance with the nature [of the bodies] in thus separating those which reach all possible distances from the Sun and those which do not do so, but always move in its vicinity" (*Almagest* IX: 1, Toomer, 1998: 419–420). So, in order to break the indetermination left by the Aristotelian criterion, Ptolemy adds a new one: it is more "natural" for the Sun to be in the middle of the two groups of planets, those which have any possible elongation and those who have a limited elongation.[5] However, in the *Planetary Hypotheses*, he adds a new argument: the closer the planet is to the Earth, the more complex its model must be since "the spheres nearest to the air move with many kinds of motion and resemble the nature of the element adjacent to them" (Goldstein, 1967: 7). Moreover, regarding simplicity, the simplest model is that of the Sun, for it has just one eccentric center; followed by that of Venus which has an eccentric center and a deferent; and then finally by that of Mercury which, being very similar to that of the Moon, has a rotating eccentric point. Therefore, the order is 5) the Sun, 6) Venus, 7) Mercury, 8) the Moon, in agreement with the dominant opinion but independently justified.

2.3. The distance to the planets

In his *Planetary Hypotheses*, Ptolemy devises a way to obtain the planetary distances. The calculation is briefly described by Thomas Kuhn (1957: 81–82), though he attributes it to the Arabian astronomers, because Ptolemy's authorship was not discovered until 1967 when the part of the *Planetary Hypotheses* dedicated to distance calculations was found and translated. In the *Almagest*, making use of his system of deferents and epicycles, Ptolemy was

[3] The stars rotate from east to west one turn in a bit less than a day. They are the fastest. The Sun, Moon and planets, move in their apparent motion with respect to the stars in the opposite direction (i.e., from west to east). Therefore, the apparent motion of the Sun, Moon and planets with respect to the stars must be subtracted. Consequently, if, for instance, Saturn moves slower in the zodiac than Jupiter, this means that Saturn is faster than Jupiter. The Moon loses one turn in around 28 days, that's why the Moon is the slowest. Saturn loses on turn in around 30 years, that's why Saturn is the fastest, after the fixed stars.

[4] Neugebauer (1975): 227–230) shows that Ptolemy could predict transits of Mercury and Venus from his models and so he could also try to detect whether the planet was visible during transit or not.

[5] It is worth noting that this criterion does well because the limited maximum elongation of the inner planets is a consequence of the fact that their orbits are smaller than Earth's. But Ptolemy never gave much weight to this argument (which maybe had a merely heuristic role), probably because, as Copernicus (1543, I: 10) noted, this criterion fails with the Moon which is below the Sun but has all possible elongations. Anyway, there was no way for Ptolemy to link the limited elongation with the sizes of the orbits. For him, the limited elongation is "explained" by the fact that the center of the epicycle and the mean Sun are always aligned. And for this explanation it is irrelevant whether the planet is closer of further away than the Sun.

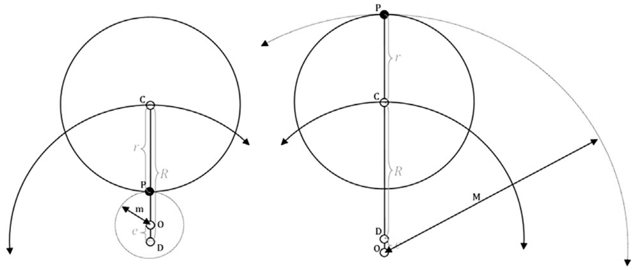

Fig. 3. The observer is located at Earth (O), the center of the deferent is D, the center of the epicycle is C and the planet is at P. Consequently, the distance OD is the eccentric (e), the distance DC is the radius of the deferent (R) and the distance CP is the radius of the epicycle (r). The minimum distance of a planet (m) is equal to $R - e - r$ (Fig. 1.left.) and the maximum distance (M) is equal to $R + e + r$ (Fig. 1.right.).

able to establish the proportion between the radius of the deferent and that of the epicycle. Were he also to consider the eccentric, he could calculate the proportion between a planet's maximum and minimum distances, expressed in parts, in exactly the same way as he did for the Moon, as we saw in the previous section. The maximum would, in most cases, be the sum of the three values, and the minimum could be obtained by subtracting the sum of the epicycle radius and the eccentric value from the deferent radius; see Fig. 3.

The calculation of the radii of the epicycles and deferent of both Mercury and Venus does not present any problem and rests on planetary observations.[6] If, as usual, we stipulate the unit of measure p as 1/60 the value of R, then the values for Venus are $e = 1.25\ p$ and $r = 43.17\ p$. Therefore, the maximum distance that Venus can reach is ($60\ p + 43.17\ p + 1.25\ p$) = $104.42\ p$, and its minimum distance is ($60\ p - 43.17\ p - 1.25\ p$) = $15.58\ p$. The proportion between these two distances is $^{104.42}/_{15.58}$, which Ptolemy rounds to $^{104}/_{16}$. In the case of Mercury, r will be $22.5\ p$; the distance from the center of the Earth to the point around which the orbit of the eccentric rotates will be $6\ p$; and finally the radius of that orbit will be $3\ p$. Therefore, Mercury's apogee can be calculated as $60\ p + 22.5\ p + 6\ p + 3\ p = 91.5\ p$. However, if as Ptolemy warned us, the perigee does not coincide with the position opposite the apogee, but occurs when the planet is 120° from its apogee, it will be necessary to calculate the distance at perigee independently (*Almagest* IX: 9, Toomer, 1998: 459–460). Doing so, Ptolemy obtains a result of $33.07\ p$. Therefore, Mercury's maximum distance is $91.5\ p$ and its minimum distance is $33.07\ p$. So the proportion is $^{91.5}/_{33.06}$ which, for unknown reasons, Ptolemy rounds to $^{88}/_{34}$ in the *Hypotheses*.

These calculations give us, of course, just the relative thickness of the sphere of each planet, not the absolute distances. Ptolemy goes further though, and says that since "it is not conceivable that there be in Nature a vacuum or any meaningless and useless thing" (Goldstein, 1967: 8), *the maximum distance of a planet corresponds to the minimum distance of the immediately superior planet*. Actually the *horror vacui* explains why there is no gap between an orbit and the following one, but not why they do not overlap, which is also a requirement if you want the limits of the orbits to be contiguous. Therefore, Ptolemy is also assuming that the planetary orbits are solid spheres (or some other hypothesis that explains why they cannot overlap); see Fig. 4. If he had one absolute distance and the order of the distances to the celestial bodies, then taking these proportions into account he could calculate the maximum, mean and minimum distances of each planet. We already know that in the *Almagest* (V: 13, Toomer, 1998: 247–251) Ptolemy calculated

[6] A reconstruction of the calculations can be found in Perdersen, 2010: 295–328.

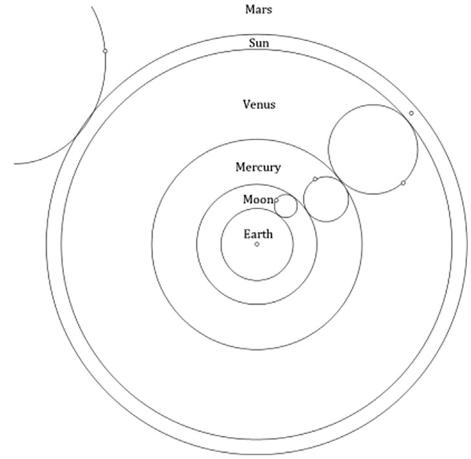

Fig. 4. The Ptolemaic nested spheres method: the maximum distance of a planet is equal to the minimum distance of the immediately superior planet. This is a simplified version, not to scale and ignoring eccentricities.

the Moon's maximum distance as 64.16 *tr*. In the *Planetary Hypotheses* (Goldstein, 1967: 7) he rounded the value to 64 *tr*. He therefore sets Mercury's minimum distance at 64 *tr* and, taking Mercury's proportion to be $^{88}/_{34}$, he calculates Mercury's maximum distance to be 166 *tr*, which then coincides with the minimum distance of the following planet, Venus. The proportion between Venus's distances is $^{104}/_{16}$, so Venus's maximum distance would be 1079 *tr*, and Venus's maximum distance should be equal to the Sun's minimum.

As we have seen, Ptolemy calculated the Sun's (mean) distance and obtained a value of 1210 *tr*. The Sun's eccentricity is just 1/24 and, therefore, the minimum distance to the Sun would be 1160 *tr*. This value does not coincide with the maximum distance for Venus obtained in the *Planetary Hypotheses* (1079 *tr*), but it is extraordinarily close. Ptolemy notices the discrepancy and claims that "since the least distance of the Sun is 1160 Earth radii, as we mentioned, there is a discrepancy between the two distances which we cannot account for, but we were led inescapably to the distances which we set down." (Goldstein, 1967: 7). Curiously enough, a great part of the discrepancy could be avoided had Ptolemy not rounded the proportions used and had not committed some arithmetical mistakes (Carman, 2009). What is relevant, however, is that between the maximum distance of Venus and the minimum distance of the Sun, there is not enough room for another planet; at least not for a planet with a sphere thickness similar to that of Mars, Jupiter or Saturn. Because the maximum distance of Venus is 1079 *tr* and the minimum distance of the Sun is 1160 *tr*, the thickness of the potential third planet would be equal to or smaller than 1079/1160 = 1.07. Ptolemy explicitly says that: "the remaining spheres cannot lie between the sphere of the Moon and the Sun, for even the sphere of Mars, which is the nearest to the Earth of the remaining spheres, and whose ratio of greatest to least distance is about 7:1, cannot be accommodated between the greatest distance of Venus and the last distance of the Sun" (Goldstein, 1967: 7). Since for Ptolemy there cannot be empty space, to do away with the gap Ptolemy proposes a slight modification of the distances of the Moon and the Sun (Goldstein, 1967: 7).

To conclude this case, we have seen that Ptolemy claims, by reasons that are independent of the Aristotelian criterion, that after the first celestial body, the Moon, come Mercury and then Venus, and that the Sun is farther than Venus. On the other hand, using the Aristotelian criterion, he accepts that Mars comes before Jupiter, which comes before Saturn. Then, calculating the absolute distance of the Moon and the thickness of the other bodies' spheres, and assuming that the spheres cannot overlap, since there is not enough room for Mars's sphere between Venus's superior limit and the Sun's inferior one, he concludes that Mars (and thereby Jupiter and Saturn) are beyond the Sun.[7] Summarizing, then, the novel successful prediction that only Mercury and Venus produce transits, follows from:

(1) the particular relative order, based on the Aristotelian criterion, for Mars, Jupiter and Saturn;
(2) the *Planetary Hypotheses* simplicity criterion, which implies the relative order of Mercury, Venus and the Sun;
(3) the Moon is the closest celestial body;
(4) the distances of the Moon and the Sun;
(5) the relative thickness of the spheres of Mercury, Venus and Mars;
(6) the hypothesis that the orbits of the planets are solid, impenetrable spheres (or some other hypothesis that explains why they cannot overlap).

As far as the theoretical hypotheses are concerned, (2) and (6), the prediction is based on totally false theoretical assumptions, from our heliocentric perspective. And they are *dramatically* false; none is retained by Heliocentrism, not even approximately. So we have a prima facie case of a true prediction made on the basis of false theoretical hypotheses, radically false ones, not approximately retained by superseding theories; that is, a case of successful, novel observable prediction that is not explained by unobservable truth, not even by approximate truth.

Note that all the premises, including (2) and (6), are necessary for Ptolemy's prediction, thus one cannot arrive at it by merely inferring from observations. Of course *these* premises are not necessary for the same prediction in a different (e.g. heliocentric) system, but this is irrelevant; for what matters for NMA is what is necessary relative to the *particular* derivation of the successful prediction in the superseded theory (otherwise nothing theoretical would ever be necessary, since it is always possible to derive a given prediction from different, incompatible premises).

What can selective realists say? They might try to reject that this prediction is a prediction of an observable fact, arguing that whether planets produce transits cannot count as directly observable. But we think this move is unpromising. It is true that if the predicted fact were simply a theoretical, non-observable consequence of the theory, then it could not count as an anomaly against selective realism. But transits are clearly observ*able* according to our current criteria. Observability is, of course, a matter of degree; and also changes with our instrumental capacities. So, even if the presence/absence of transits had not been observable before the use of telescopes (thus at Ptolemy's times), it would be an unfair strategy to dismiss this prediction as non-observable, for planetary transits are now uncontroversially taken as observable (and for some planets, observed) facts rather than unobservable theoretical hypotheses. But, moreover, it is not even true that transits could not be observed without telescopes, for at least Venus' transits are observable to the naked eye (Meeus, 1958: 101, Goldstein, 2007: 56, note 1). It is an extremely rare phenomenon, during the 6,000-year period from 2000 BCE to 4000 CE, a total of just 81 transits of Venus will occur (Espenak, 2014). There are some medieval reports of allegedly observed transits of Venus by Al-kindi, Avicenna, Averroes and Ibn-Bajja; although it seems that in these cases what they observed were just sun-spots, instead of Venus's transits (Goldstein, 2007), with the possible exception of Avicenna's observation (Kapoor, 2012).

Note also that realists could not dismiss this case as a case of non-risky prediction either. Relative to the Ptolemaic hypotheses that predict it, the fact is of course not risky but secure. But independently of such hypotheses the prediction is risky. On the one side, it is quite precise for what follows from the system is when and where the transits must occur. On the other side, no other hypothesis had the same consequence. Of course a posterior hypothesis, Copernicus', had the same consequence, but we now that this cannot disqualify the prediction as non-risky, for then no prediction (e.g. Levarrier's discovery of Neptune, Poisson's effect, …) made by a theory already superseded by other that preserves the prediction, could qualify as risky, which is something that realists don't want to endorse.

Other possible defensive move could be to argue that actually the only hypothesis needed is that Mercury and Venus are inner planets and the rest are outer, and that this distinction is approximately preserved in posterior astronomical theories. We do not think this move works either. First, it is true that the claim that Mercury and Venus are Ptolemaic-inner and the rest Ptolemaic-outer follows from (1)–(6) above and that it suffices for the prediction, but this does not make the theoretical hypothesis used in (1)–(6) for deriving the inner/outer extensions dispensable. What matters is *how Ptolemy* establishes this divide. Secondly, one cannot actually say that the divide "inner" and "outer" is the same, nor approximately the same, in both theories. "Inner" and "outer" mean different things, and denote different states of affairs, in Ptolemy and Copernicus: In Ptolemaic astronomy an inner planet is always located between the Earth and the Sun and an outer planet is always further than the Sun. This is not by chance, this is exactly what 'inner' and 'outer' mean for Ptolemy. But this criterion applied to the Copernican model implies critical problems. In the Copernican model, Venus and Mercury are sometimes between the Earth and the Sun (for example, at lower conjunction) but sometimes further than the sun (for example, at superior conjunction), therefore, Venus and Mercury would be sometimes inner and sometimes outer planets. Something similar happens with Mars which, according to Copernicus, is sometimes closer to the Earth than the Sun (during retrograde motion) and, therefore, Mars would be an inner planet in those moments but an outer during the rest of the time. Actually, the fact that inner and outer doesn't mean the same for Ptolemy and Copernicus is exactly the fact used by Galileo to refute Ptolemy, when he argues, showing the full phases of Venus, that Venus must be further than the Sun during superior conjunction. Note that this does not allow construing a minimal sense of 'inner' meaning simply "between the Sun and the Earth", and of 'outer' meaning its negation that applies to both the Copernican and Ptolemaic system and that could be adduced by the realist to be the minimal theoretical hypothesis retained: Mercury and Venus are always between the Sun and the Earth according to Ptolemy, but this is not the case according to Copernicus.

The only move that could seem plausible to us is to claim that, upon closer inspection, Ptolemy's prediction is false; for his prediction is not simply that there is no other planet between the Earth and the Sun, but rather that there *cannot be* a planet with an orbit thickness greater than 1.07. This prediction is false according to our lights, for recall that, after the discovery of Neptune, which explained Uranus's anomalies, astronomers postulated the existence of a new planet, Vulcan, between the Sun and Mercury in order to explain the anomalies of the latter, without being worried about its thickness. Of course Vulcan was not discovered, for it

[7] It is true that the Aristotelian criterion based on the angular speed already placed Mars beyond the Sun; but it is clear that, at least for the relative order of the Sun and Mars, Ptolemy did not trust the criterion. He explicitly asked whether Mars could be located before the Sun and decided that it comes after the Sun not because of the Aristotelian criterion, but because there is no enough room for Mars between Venus and the Sun, as we saw above.

simply does not exist, but its existence was compatible with Newtonian celestial mechanics, which therefore does not predict that there *cannot* be another inner planet. This is true: the prediction taken modally is false. But it is not at all clear that this is the correct reading of the prediction. First, it is one thing for the prediction to have modal force and other, quite different thing, is that what is predicted is a modal fact. If the latter were the case, then every prediction made using laws (i.e. all predictions in science), thereby using modal facts, should be read as a prediction of a modal fact, a reading that we are sure realists do not want to endorse either. But, secondly, even if someone wanted to endorse this reading, it cannot cancel the non-modal component: modal facts are not observable, and if this and other predictions must be read *only* as predictions of modal facts, there would be no observable predictions in science. Therefore, even if there were an admissible modal reading of this prediction, it could not be the only relevant reading. There must be a reading that makes this prediction (and all other alleged observable predictions in science) observable/detectable. Or put in simpler terms: even if one accepts that the modal fact is predicted, since the modal fact implies the actual fact, one has to accept that the actual fact is also predicted, and what count as observable.

To conclude this first prediction, then, we seem to face a case of an observable prediction that (i) is true, but (ii) follows from premises some of which are radically false and not even approximately retained by posterior theories.

3. Outer planets are never eclipsed by the Earth's shadow

In this brief section we will present other successful novel prediction that follows from the same set of theoretical claims that implies the prediction of Section 2. Therefore, it could be understood as a kind of corollary of the previous section. As we noted above, a lunar eclipse is produced when the Earth passes between the Sun and the Moon and therefore the Earth's shadow obscures the Moon; in Figs. 1 and 2 (right) the Moon (C) is eclipsed because it is inside the Earth's shadow cone. For this phenomenon to occur, the Moon must be so close to the Earth at opposition that it falls inside the Earth's shadow cone. In other words, the line BC (Fig. 2) must be smaller than BH. When Ptolemy calculates the solar distance in the *Almagest*, he also explicitly calculates the length of the Earth' shadow cone, i.e., distance BH. He asserted that this distance is 268 *tr* (*Almagest* V: 15, Toomer, 1998: 257). The value is approximately correct, because it depends only on the apparent size of the Sun, the Moon and the Earth's shadow. The inner planets could never be eclipsed by the Earth's shadow, not because they are not close enough (according to Ptolemy, actually they are) but because they are never at opposition. There is no epistemic merit in predicting that the inner planets will never be eclipsed by the Earth's shadow, because it necessarily follows from an observational fact: they are never at opposition. The situation is different, though, with respect to outer planets, because they can be at opposition. Therefore, if a superior planet were so close to the Earth that it fell into the Earth's shadow cone, it would be eclipsed. This does not happen, because the outer planets are far enough away not to fall in the shadow. This is correctly predicted by Ptolemy since the cone shadow is 268 *tr* high but, as we saw above, according to him Mars's minimum distance (which is equal to the Sun's maximum distance) is 1260 *tr*; and of course Jupiter and Saturn are even farther. Recall that Mars's distance depends on the planetary order assumed by Ptolemy, and we already showed that this order is based on false premises that are not retained. If the order of the planets had been different and, for example, Mars had been in the place of Mercury or Venus, Mars could have been eclipsed by the Earth's shadow. This counterfactual situation is represented in Fig. 5.

Therefore, the fact that Mars is never eclipsed by the Earth's shadow during retrograde motion is another successful Ptolemaic prediction; and a novel one, since the theory was not designed to accommodate this fact (Mars's distance was not settled to fit the absence of Martian eclipses and, of course, neither the size of the shadow's cone, which depends on apparent sizes of the Sun, Moon and Earth's shadow, that must be approximately what they are in order to correctly predict eclipses).

One could perhaps argue that if Mars did ever get close enough to the Earth to be eclipsed, we would notice it, so it's unclear in what sense we should qualify this prediction as novel. Well, the prediction is novel since, as we just said, the theory was not designed for accommodating the unobserved eclipse. On the other hand, it is simply not true that if Mars had ever been close enough to the Earth to be eclipsed we would have noticed it via some other direct observation. Actually, all (except of course it not being eclipsed) Mars *observations* at that time, *taken in isolation*, are compatible with it being so close; what makes it clearly not that close are not simply Mars observations, but Mars' theoretical role in the system, i.e. its position relative to other celestial bodies (together with other observations for these other bodies).

4. The changing phases of outer planets

It is well known that, contrary to Heliocentrism and Brahe's Geocentrism, Ptolemy's Geocentrism wrongly predicts that Venus does not show strong change in phases. The story is somewhat more complicated though, for although it is true that Ptolemy's system was not capable of predicting some of Venus's phases, it is also true that it does correctly predict other phases of the inner planets *and* all the phases of the outer ones. In particular, what constitutes a novel prediction is the fact that according to Ptolemy, contrary to what he supposed happens with inner planets, the outer planets' phases go from full to almost full and then to full again, without passing through, like inner planets, new phases (or even half phases).

Let us first analyze the causes of the phases of a planet; see Fig. 6. The phase of a planet depends on the Sun–planet–Earth angle (angle SPO; the Sun is the light source, the planet the reflector and the Earth is the point of observation). When SPO is 0°, both the Earth and the Sun are facing the same side of the planet and therefore the illuminated half of the planet is seen in its totality from the Earth: this is a full phase. When SPO is 90° or 270°, the side of the planet facing the Earth and the illuminated side are perpendicular to each other, therefore, from the Earth we see just

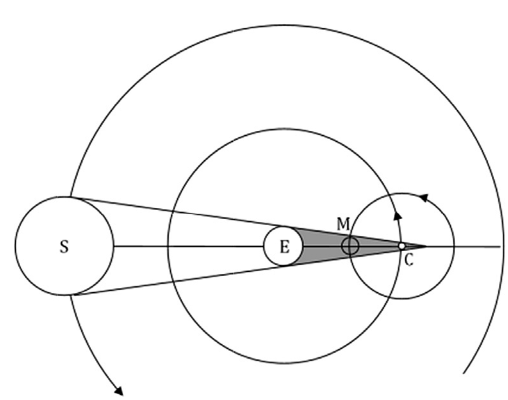

Fig. 5. Mars (M) is so close to the Earth during retrograde motion that it falls into the Earth's shadow cone.

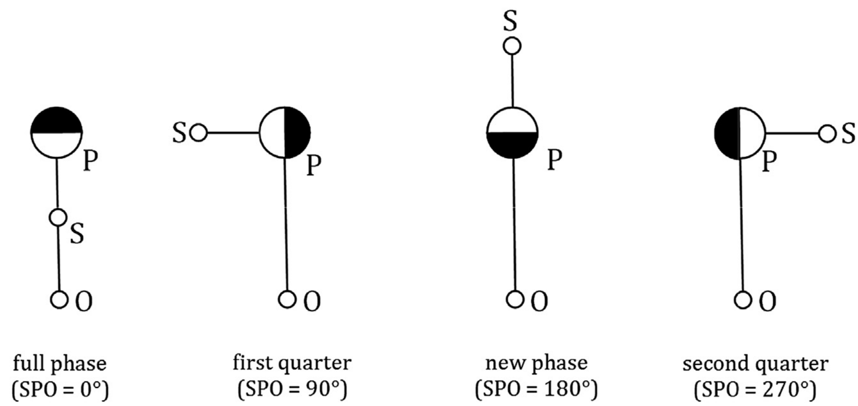

Fig. 6. Different possible phases of an inner planet. The planet is at P, the observer at O and the Sun at S.

half of the planet illuminated. Finally, when SPO is 180°, the illuminated side is invisible from the Earth; we see the non-illuminated side: this is a new phase.

However, since we are not on the planet to measure the angle SPO, we must analyze the phases relative to the elongation of the planet from the Sun seen from the Earth, i.e., from angle POS. At the first and second quarters, the angle POS will depend on the proportion between the sides PO and PS, i.e., on the proportion between the distance from the Sun to the planet and that from the Earth to the planet, and it will always be greater than 0° (it would only be 0° if SP were also 0) and less than 90° (it would only be 90° if SP were infinite). The new phase is observed when POS is 0°; but the full phase will also be produced when POS is 0°. So, to distinguish between the full and new phase, the conjunction of the planet with the Sun (POS = 0°) is not enough; it also depends on another feature, namely the proportion of the distances. If at conjunction the Sun is farther than the planet from the Earth, then it will be a new phase; but if the planet is farther than the Sun, it will be a full phase. These are then the four cases shown in Fig. 6. There still remains, nevertheless, one case to be analyzed: what phase corresponds to an opposition, i.e., to SOP = 180°? This case is not represented in Fig. 6 because it presupposes that the planet is always closer to the Sun than to the Earth and opposition is only produced when the Earth is between the planet and the Sun and, therefore, the planet is closer to the Earth than to the Sun; as represented in Fig. 7. It is easy to see that at opposition the planet will also be in a full phase.

Summarizing these results: *if the planet is at opposition, we necessarily see a full phase* (case 1); but *if the planet is at conjunction*, it depends also on the proportion between the Sun–planet and Earth–planet distances. *If the planet is farther away than the Sun, we will observe a full phase* (case 2), but *if the planet is closer than the Sun, we will observe a new phase* (case 3).

Let us now analyze the conjunction and opposition of the planets according to the Ptolemaic model, distinguishing what happens in the case of inner and outer planets. When we discussed the *Almagest* criterion for locating the Sun in the middle of the outer and inner planets, we mentioned that, while the superior planets can have any possible elongation, the inner ones have a limited elongation: they always move "in its [the Sun's] vicinity", as Ptolemy said. In order to maintain the small elongations of the inner planets always, Ptolemy set the center of the epicycle of the inner planets so that it was always aligned with the Sun. Therefore, the inner planets, Venus and Mercury, do not have oppositions, but have two different conjunctions with the Sun during their synodic period: one when the planet is moving with retrograde motion (called inferior conjunction) and the other when the planet moves in the same direction as the Sun (superior conjunction). Fig. 8 represents both an inferior conjunction (V_1) and a superior one (V_2) at the same time.

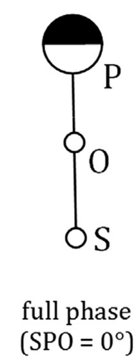

Fig. 7. Full phase possible only when the elongation of the planet from the Sun, POS, is 180°. The planet is at P, the observer at O and the Sun at S.

The outer planets, in contrast, have just one conjunction and one opposition (produced when the planet is in the middle of its retrograde motion). In Fig. 9, M_1 represents the planet at conjunction and M_2 the planet in the middle of its retrograde motion at opposition.

What phases correspond to each conjunction and opposition? Starting with inner planets, the Ptolemaic version of Venus's superior conjunction is represented in Fig. 10(A), which can be compared with the Copernican view in Fig. 10(B). In the Ptolemaic model, the planet is closer to the Earth than the Sun is, therefore it is case 3 (new phase at opposition); while in the Copernican model, the Sun is closer than the planet and,

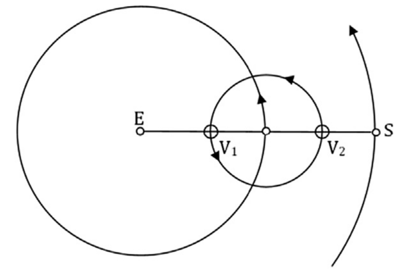

Fig. 8. Two possible conjunctions of an inner planet. The earth is at E, the inner planet is at V1 (inner conjunction) and at V2 (superior conjunction). The Sun is at S.

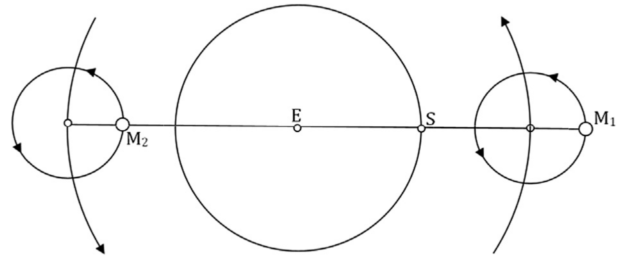

Fig. 9. Conjunction and opposition of an outer planet. The Earth is at E and the Sun is at S. M1 represents the planet at conjunction and M2 the planet in the middle of its retrograde motion at opposition.

therefore, it is case 2 (full phase at opposition). As is well known, observation through a telescope of the full phase of Venus at superior conjunction was one of the most important refutations/anomalies of Ptolemaic theory.

Now, the Ptolemaic and Copernican versions of the inferior conjunction are represented in Fig. 10(C) and (D), respectively. In both cases the planet is closer than the Sun and therefore both are case 3 (new phase at opposition). So, Ptolemy correctly predicts the phase at inferior conjunction.

Let us now move on to the phases of superior planets. Fig. 11(C) and (D) represent the phase of a superior planet at opposition in the Ptolemaic and Copernican models, respectively. In both cases the phase is new and in both cases the planet is closer to the Earth than to the Sun. Therefore, both are case 1. We already saw that at opposition, the full phase depends exclusively on the fact of being at opposition, which is an observable fact. The order of the planets, i.e., the fact that the planet is inner or outer, does not play any role in this case. So, even if the prediction is correct, it is not based on theoretical parts of the Ptolemaic system and, therefore, does not qualify as a novel prediction.

Fig. 11(A) and (B) represents the conjunction of the planet and the Sun in the Ptolemaic (A) and the Copernican (B) systems. In both cases the phase is full and in both cases the Sun is closer to the Earth than the planet. Both cases belong, therefore, to case 2. And we know that Ptolemy decided that Mars is beyond the Sun based on highly theoretical and false reasons that have nothing to do with

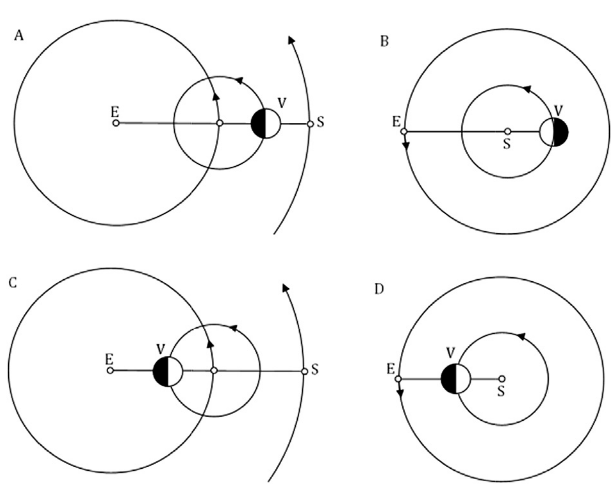

Fig. 10. Inferior and superior conjunction of an inner planet in both Ptolemaic and Copernican models. The Earth is at E, the Sun at S and the planet at V. Superior conjunction is represented in the upper figures, A represents the Ptolemaic version and B, the Copernican version. Inferior conjunction is represented in the lower figures. C represents the Ptolemaic version and D the Copernican version.

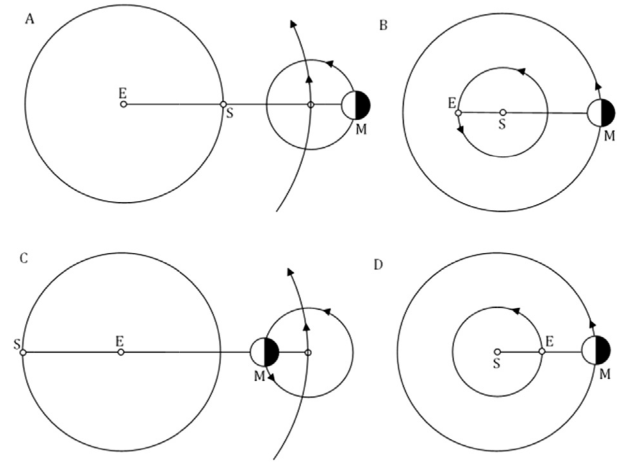

Fig. 11. Outer planet at conjunction (upper figures) and opposition (lower figures). The Earth is at E, the Sun at S and the planet at M. At the right is represented the Ptolemaic version and at the left the Copernican version.

the planetary phases. Now, if the outer planet has full phase at both conjunction and opposition, then (contrary to what, according to Ptolemy, happens with inner planets) the phases of the planet go from full to full, reaching just almost-full phases between opposition and conjunction. This pattern of phases depends on the fact that the planet is beyond the Sun at conjunction, which has been obtained by highly theoretical and false reasons. Therefore, the correct prediction of the pattern of the phases of the outer planets is a successful novel prediction of the Ptolemaic system. Actually, it is a very successful one. According to modern calculations, the illuminated part of Mars's surface could never be smaller than 0.838; of Jupiter's, smaller than 0.989; and of Saturn's 0.997 (Meeus, 1998, 284). And according to Ptolemy the minimum of the illuminated surface for Mars is 0.958, for Jupiter 0.996 and for Saturn, 0.998.[8]

To summarize, the inner planets have two conjunctions but no oppositions. The phase at superior conjunction was wrongly predicted by Ptolemy and this was used against the Ptolemaic system by Galileo. The outer planets have one conjunction and one opposition; Ptolemy predicted correctly the full phase of both. In the prediction of the full phase at opposition only observational premises are used, no *unobservable hypothesis* is used, therefore it is not relevant in our debate. With regard to the correct prediction of the full phase at conjunction, however, Ptolemy has to assume that the Sun is closer to the Earth than to the planet, an assumption which is based on theoretical and false reasons; therefore, it constitutes a novel prediction. Taken together, the novel prediction asserts, therefore, that the outer planet phases go from full to almost-full and then back to full, which is certainly true, and surprising for independently of his system (and of course of the posterior superseding theories) there was no reason to expect that.

Note that the realist *cannot* dismiss this case by arguing that these were not predictions really made by Ptolemy because he was not aware of the planetary phases, a phenomenon not accessible to

[8] These values have been calculated using the formula 42.1 in Meeus, 1998:283. The formula uses as input data the Earth–Sun distance, the Earth-planet distance and the planet-Sun distance. These distances have been calculated using trigonometric functions using the values in terrestrial radii of the epicycle and deferent radii offered by Ptolemy's values in his *Planetary Hypotheses* (cf. Van Helden, 1985: 27).

observation at the time.[9] It is true that phases were not observed until the 17th century, but this is no reason not to take them into account. If the implication of Venus's new phase at superior conjunction was legitimately considered a wrong prediction, why should the other implications not count as correct predictions (and novel ones, since the theory was not fudged to allow for them, and they do not follow simply from observations)? The fact that the theory's creator does not make the derivation himself, cannot convert the prediction into irrelevant. Poisson's white spot, the realist's most famous example, would not otherwise count as a successful novel prediction of Fresnel's theory, for it was not Fresnel but Poisson who derived it (as, according to him, an absurd consequence of Fresnel's theory). What is relevant for SSR is whether unobservable theoretical hypotheses make successful novel predictions, and this is completely independent of whether the proponents of the system are aware of such predictions. Thus, if Ptolemy's successful prediction does not count because he was not aware of it, Fresnel's white spot should not count either (of course the lapse of time between the formulation of the theory and the derivation of the prediction, and its eventual observation, is also completely irrelevant).

The realist might perhaps argue that she accepts the successful prediction as novel in some sense, but not in the relevant sense since interesting novel predictions must be "risky", "surprising" or "improbable", and this is not: since Ptolemy makes predictions along the whole orbits it is not surprising that some just get correct by chance. There are two readings of this objection. One is absolutely general and would apply to all Ptolemy's predictions. We will deal with this general objection in the last section. The second, more restricted reading says that this specific prediction, the changing phases of outer planets is particularly non-risky. We strongly disagree. Upon any non-question begging notion of "surprising", this prediction is surprising, for without taking into account Ptolemy's theory (and of course posterior theories), the fact that the illuminated part of a planet suffers such *specific* (full-almost full—full again-…) variations is quite improbable per se, i.e. relative only to the facts already *observed* at the time (or other background beliefs not dependent on the Ptolemaic system). Put in a different way, it is as surprising as Copernicus' prediction of Venus phases at second conjunction, which was observed by Galileo in 1611 and taken not only as a refutation of Ptolemaic Geocentrism, but also as a confirmation of Heliocentrism (if we ignore in that context Brahe's Geocentrism that implied it as well; if we do not ignore it, the prediction confirms in that context neither theory in particular, but rather their disjunction). There is no non-question begging notion of surprise that makes Copernicus' prediction surprising but Ptolemy's one not.

The only position we think that the selective realist could try to adopt is to reject that in this case there is no retention: the posterior theory retains the *relative positions* of the Earth, the Sun and the planet. That is, if Ptolemy correctly predicts a certain phase, which is also predicted by Copernicus, then the relative positions of the light source (Sun), the reflector (planet) and the observer (Earth) are preserved (see Figs. 6 and 7 above); and actually *must* be preserved. This is true, but this last emphasis also shows the problem in this possible realist answer. For the relative positions are not only preserved but *must* be preserved *because* such relative positions are a priori/conceptually implied by the phenomenon: if a planet illuminated by the Sun shows phases seen from Earth, then (given the laws of reflection) it is a priori true that the relative positions are such as they are. Therefore, the only "theoretical content" in Ptolemy's system retained by the successor is useless for the selective realist: the claim that with regard to the observed phases the relative Earth—Sun—planet position in Ptolemy's system are retained by Copernicus' system is a priori true (modulo the shared laws of reflection, which are totally independent of the astronomical theories) and thus useless for a retentionist thesis which must be, in every specific case, fallible. Put it otherwise, if the only non-directly observable content that is preserved is something that a priori (thus infallibly) follows from the observable prediction (and the background laws of reflection), the realist retention thesis loses all its interest. If (given background uncontroversial assumptions A) P is predicted by H, which in turn is implied by P, then (given A) P and H are logically equivalent. The retention becomes trivial and NMA uninteresting. If the realist replies that at least background assumptions A are non a priori retained, it is again an irrelevant retention, now because (as laws of reflection in our case) A have nothing to do with the theories/hypotheses that are into question. On the other hand, it is even unclear that relative positions (H) are unobservable. Actually, given the equivalence (modulo A) between P and H, the phases are precisely a way of observing these relative positions!

So, we seem to have another prima facie case of successful observable prediction not explained by theoretical truth. The theoretical hypotheses on which the correct prediction relies are not preserved, not even approximately, by the superseding theory.

5. The increasing brightness during the retrograde motion for Mars

It is usually claimed that the main reason for the rejection of Eudoxus' homocentric spheres in favor of the epicycle and deferent system was that the former cannot explain the patent increase in planetary brightness during retrograde motion. If the brightness of the source is taken as constant and variation in brightness depends only on distance, then an increase in brightness is obviously interpreted as the planet approaching the Earth. In the Eudoxian proposal, all the spheres are centered on the Earth and consequently the planets never change their distance from it; thus the system was incapable of explaining changes in brightness. This is the *official story*. For example, in his *Copernican Revolution*, Kuhn (1957: 58—59) claims that "…all homocentric systems have one severe drawback which in antiquity led to their early demise. Since Eudoxus' theory places each planet on a sphere concentric with the Earth, the distance between a planet and the Earth cannot vary. But planets appear brighter, and therefore seem closer to the Earth, when they retrogress. During antiquity the homocentric system was frequently criticized for its failure to explain this variation in planetary brilliance, and the system was abandoned by most astronomers almost as soon as a more adequate explanation of the appearances was proposed." The main candidate for this more adequate explanation was the epicycle and deferent system, in which the planet's distance to the Earth changes; increasing during retrograde motion, as shown in Fig. 10(C)—for inner planets—and Fig. 11(C)—for outer planets.

If the epicycle and deferent system had been designed to explain the increase in brightness of the planets during retrograde motion, such an increase in brightness could not be presented as a novel prediction of Ptolemaic astronomy. Nevertheless, we see no evidence that the epicycle and deferent system was conceived of for that reason. Moreover, that reason

[9] This does not mean that phases were not mentioned at all. Recall that Ptolemy discusses the transit of the inner planets and says that they do not obscure the Sun because they are very small compared to the Sun, thus assuming that during conjunction, they are in new phases (otherwise they would not "obscure" the Sun). Also, this was an explicit problem in medieval Ptolemaic astronomy: the fact that phases of Venus were not observed was an anomaly that they tried to solve (some astronomers postulated, for example, that Venus has its own light and that is why it does not show phases). See Goldstein, 1996.

played no role in the initial acceptance of the planetary model. We have two main reasons to maintain this view. First, only for Mars is the change in brightness perceivable to the naked eye. Thus, the change to epicycles and deferent would have been designed only for Mars, and not for all the planets (though of course, for the sake of coherence it could be transferred to the other planets). Second, the correct increase in brightness only occurs if the epicycle and deferent rotate in the same direction; but we have plenty of historical evidence of epicycle and deferent systems that rotate *both* in the direct sense, as Ptolemy's does, *and* in the inverted sense of rotation, for which, during retrograde motion, the planet is farther from rather than closer to the Earth. When one analyses the specific reasons that Ptolemy adduces in the *Almagest* for selecting the right-direction model, the change in brightness plays no role at all. All in all, we think it is highly doubtful to say the least that the direct-direction epicycle–deferent system applied to all the planets was designed and selected to explain the change in brightness. If we are right, then it may count as a case of a novel prediction.

5.1. The brightness of the planets and the postulation of the model

It is important to recall that ancient astronomers do not talk about brightness but about apparent size. The distinction between apparent size and brightness does not exist before the telescope (Goldstein, 1996: 1). So, it is natural to interpret a change of brightness, i.e. of apparent size, as a change in distance.

As we already mentioned, the thickness of Mars's sphere is around 7:1, which means that in the middle of its retrograde motion, Mars is seven times closer to the Earth than at conjunction. Also as we mention above, in the middle of its retrograde motion Mars's phase is full. Therefore, Mars appears much brighter at opposition than at conjunction, going from a magnitude of −1.5 to +1.8 (negative magnitudes mean that the planet is brighter). The change, therefore, is of more than three magnitudes and clearly perceivable. The situation is different for the other two superior planets though. Even if they also have a full phase at the middle of their retrograde motion, since their epicycles are very small compared to the deferent, their distance from the Earth varies very little and the change in brightness is hardly detectable to the naked eye, if at all. Saturn's magnitude changes from 0.1 to 0.6, with a difference of just half a magnitude; and Jupiter's from −2.7 to −1.8, with a magnitude difference of 0.9. There are no references to ancient detections of changes in Jupiter's or Saturn's brightness. Actually, as we will see, we do not have any ancient reference to changes in Mars's either.

In the case of inner planets, we have to take into account that during retrograde motion, when the elongation is very small, they are hidden by the Sunlight and therefore they are not visible. This is particularly true for Mercury. According to Ptolemy, the first visibility of Mercury as an evening star is at 11.5° (*Almagest* XIII: 7, Toomer, 1998: 638) and the maximum elongation from the Sun is around 28.5° (*Almagest* XII: 19, Toomer, 1998:596). It is only visible around greatest elongation so there is not much variation in its distance during the intervals of visibility. Moreover, the small apparent size of Mercury prevents the detection of any variation in its apparent size. As for Venus, since it is much brighter than Mercury, it is visible even when it is very close to the Sun. According to Ptolemy, the first visibility of Venus as an evening star is at 5 2/3° (*Almagest* XIII: 7, Toomer, 1998: 638). Because of the relatively large size of the epicycle, its distance from the Earth changes significantly. We already saw that the thickness of its orbit is $^{104}/_{16}$, i.e., 6.5. Therefore, its apparent size should increase 6.5 times during one synodic period. Despite this, its brightness shows relatively little change because the enormous variation in its distance from the Earth is largely canceled by the effect of the phases: the more Venus's distance decreases, the more it wanes, arriving at its new phase at its minimum distance. Moreover, according to modern observations, the maximum brightness of Venus occurs at about 39° of elongation from the Sun (Goldstein, 1996: 1) and therefore very far from the middle of its retrograde motion; actually closer to maximum elongation.

So, it is far from clear that the epicycle and deferent system was favored because it could explain the change in brightness. The only ancient text that mentions changes in brightness of the planets is by Simplicius. In his commentary on Aristotle's *De Caelo*, he claims that the observed change in brightness of Venus and Mars was one of the reasons for the rejection of the homocentric spheres in favor of epicycle models (*De Caelo* 2.12, Bowen, 2013:165). Of course, Simplicius is right about Mars, but wrong about Venus: it is impossible to see Venus in the middle of its retrograde motion, and even if we could see it, it would not change its size, for the reason already mentioned. Therefore, as Goldstein (1996:4) says, "this observational claim is to be understood as a 'reconstruction' based on a consequence of Ptolemaic theory". In fact, Mars and Venus are the planets that should change size most, because they are those with the greatest differences between maximum and minimum distance. Bowen goes further and says that "no one before Ptolemy appears to have paid any attention to the fact that the stars (both fixed and wandering) differ in size (brightness), if they noticed it at all" (Bowen, 2013:289) and concludes that "it is difficult, then, to hold that prior to the second century CE there was any real concern with the apparent size (brightness) of the five planets, though this claim is essential to Simplicius' history" (Bowen, 2013). So, Simplicius' text is actually inferring the observation from the theory and not justifying a theory from an independently noticed observation.

We really do not know how the epicycle and deferent system was postulated; mainly because we do not know much about pre-Ptolemaic astronomy. Some authors place its origins in Plato's time (van der Waerden, 1955, 1974) but it is usually asserted that it originates with Apollonius and is improved by Hipparchus. It is possible that the model was first conceived for the inner planets, because for them, since the Sun is aligned with the center of the epicycle, it may seem more natural that they rotate around a fixed point that, aligned with the Sun, in turn rotates around the Earth. If this is the case, then the system was not proposed to explain the change in brightness, which is not observed in inner planets. It has recently been proposed that the epicycle system could have been inspired in mechanical representations of Babylonian planetary periods, such as the gears of the Antikythera Mechanism (Evans and Carman, 2013). If this is the case, again, the origin of the system has nothing to do with changes in brightness. In any case, there is no evidence whatsoever, besides the Simplicius text, that the model as applied to all planets was postulated to explain the change in brightness, which was clearly only reported for Mars.

5.2. The inverted-direction models

In the Ptolemaic models for the five planets, the epicycle and the deferent rotate in the same direction and therefore the retrograde motion is produced when the planet is within path of the deferent: when the tangential speeds of epicycle and deferent are in opposite directions. It is also possible to produce retrograde motion by inverting the direction of the motion of the planet on its epicycle. In that case, the retrograde motion is produced when the planet is at its maximum distance from the center of the deferent. Fig. 12 (left) represents the direct-direction epicycle system: the retrograde motion is produced at the minimum distance; Fig. 12 (right) represents the inverted-direction epicycle system: the retrograde motion is produced at the maximum distance.

The different shape of the loops should not invite the reader to ask why it was not possible to decide between the models just by

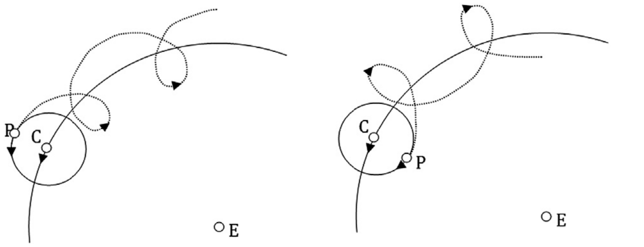

Fig. 12. Direct-direction (left) and inverted-direction (right) epicycle systems. The Earth is at E, the planet at P. C is the center of the epicycle.

observing how the planet moves in the sky, for the figure represents the motion as seen from far away from the Earth. Because the planet (P), the center of the epicycle (C) and the Earth (E) are in the same plane, from Earth the planet is simply seen going forward and backward: the looping movement in and out is not perceived. The inverted-direction model is not just a geometrical possibility; there is strong evidence that it was proposed in ancient times. The *Keskintos Inscription* (Jones, 2006), a fragmentary astronomical inscription found on the island of Rhodes and now housed in the *Staaliche Museen* of Berlin, is dated from about 100 BCE and contains a set of numerical data with some astronomical explanation that can only be understood if we assume the inverted-direction model (Jones, 2006: 28, Neugebauer, 1975: 702–704). The inverted-direction model is also clearly referred to in a papyrus known as Papyrus Michigan 3.149, from the second century CE (and so, contemporary with Ptolemy) in which the direct-direction model is attributed to the inner planets and the inverted-direction model to the outer planets (Robbins, 1936; Neugebauer, 1972). This is especially relevant to our topic, for it shows that the change in brightness is not related to the selection of the epicycle direction. The inner planets do not show a change in brightness and nevertheless are carried on a direct-direction epicycle; the outer planets are carried in the inverted-direction, even if at least one of them shows a clear increase in brightness during retrograde motion.

It is also the case that Pliny (1962), a contemporary of Ptolemy, in his *Natural History* (II, 12) clearly refers to the inverted-direction model, although his discussion of planetary motion is very muddled and it is questionable how much he really understood. In particular, Pliny asserts that when the planets are at opposition and therefore in the middle of retrograde motion, "they are observed as smallest since they are away at the furthest height and travel with least motion". So, while Simplicius, who defended the direct-direction model, claimed that he observed Venus to be bigger in the middle of its retrograde motion (which is patently false), Pliny, commenting on the inverted-direction model, asserted that he observed the outer planets to be smaller during retrograde motion (which is also patently false).

5.3. *The reasons adduced by Ptolemy for choosing the direct-direction model*

When in the *Almagest* Ptolemy introduces the planetary models, he explicitly says that there are two possibilities for explaining the retrograde motion (i.e., the inverted- and direct-direction models). He chooses the direct-direction model and makes his reasons explicit, among which there is no mention of the change in brightness at all. For a correct understanding of Ptolemy's crucial passage, it is important to realize that when he talks about the eccentric hypothesis he is also talking about the inverted-direction model, because they are equivalent; and when he talks about the epicyclical hypothesis, he is talking about the direct-direction model:

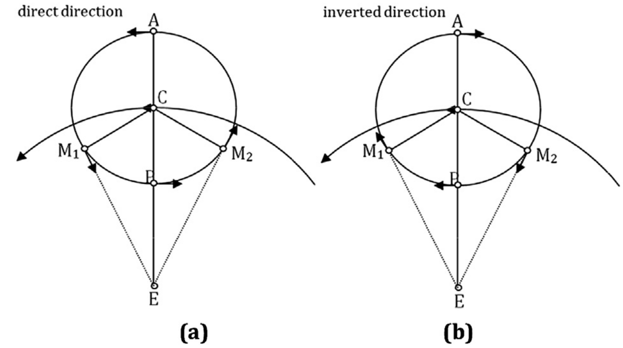

Fig. 13. The maximum and minimum speed at both directed-direction (a) and inverted-direction (b) of the epicycles models. The Earth is at E, the Planet is at P in the perigee, at A in the apogee and at M1 and M2 when it speed is the mean speed. C is the center of the epicycle.

> "For [the retrograde motion] we find… that in the case of the five planets the time from greatest speed to mean is always greater than the time from mean speed to least. Now this feature cannot be a consequence of the eccentric hypothesis [i.e. inverted-direction model], in which exactly the opposite occurs, since the greatest speed takes place at the perigee in the eccentric hypothesis [i.e. inverted-direction model], while the arc from the perigee to the point of mean speed is less than the arc from the latter to the apogee in both [i.e. inverted and direct direction] hypotheses. But it can occur as a consequence of the epicyclical hypothesis [i.e. direct-direction model], however, only when the greatest speed occurs not at the perigee, as in the case of the Moon, but at the apogee; that is to say, when the planet, starting from the apogee, moves, not as the Moon does, in advance [with respect to the motion] of the universe, but instead towards the rear. Hence we use the epicyclical hypothesis [i.e. direct-direction model] to represent this kind of anomaly" (*Almagest* IX: 5, Toomer, 1998: 442).

Fig. 13(a) and (b) helps to understand this passage. In both situations point A represents the apogee, i.e., the maximum distance from the Earth, and point P the perigee, i.e., the minimum distance. At points M_1 and M_2 the planet moves with its mean motion, i.e., the motion of the deferent because the epicycle tangential speed is perpendicular to the speed of the deferent. The planet moves with constant angular velocity around the epicycle, therefore it takes more time to go from A to M_1 than from M_1 to P. Fig. 13(a) represents the direct-direction model: the maximum speed of the planet is reached at A, where the tangential speeds of the epicycle and deferent are in the same direction. At P, the tangential speeds go in opposite directions and therefore P represents the minimum speed. In Fig. 13(b), the maximum and minimum speeds are inverted: minimum speed is at apogee (A) and maximum at perigee (P). Ptolemy says that he observed that for the planets, it takes more time to go from maximum speed to mean, than from mean to maximum. Therefore, the direct-direction model must be chosen.[10]

[10] Moreover, it should be noted that the inverted-direction model is unable to make Venus and Mars move in retrograde motion. As Asger Aaboe (1963) shows, the inverted-direction model does not produce retrograde motion when the speed of the deferent is greater than the speed of the epicycle, i.e. if for a certain period of time the planet makes more turns in longitude than retrogradations. If you also assumes (as it actually was assumed) that r is smaller than R, then you will not have retrograde motion if the speed of the epicycle is less than double the speed of the deferent. However, this is precisely what happens with Venus (720 turns of the epicycle in 1151 turns of the deferent) and Mars (133 turns of the epicycle in 151 turns of the deferent). Therefore, the inverted-direction model could not be used to predict the motion of Mars or Venus.

To summarize, only Mars's change in brightness was sufficiently clear to the naked eye; there is no ancient reference to changes in brightness of the other planets. There were two options to combine the epicycle and deferent directions in each planet: the direct-direction model, Ptolemy's, and the inverted-direction model. This generates three possible models for the whole system: one in which all the planets follow the direct direction, the other in which all the planets follow the inverted direction and also a mix-model that applied direct direction to inner planets, which do not show a change in brightness, and inverted direction to outer planets, one of which had a clear change in brightness. Finally, Ptolemy's explicit reasons in favor of the direct-direction model do not mention brightness at all. All this makes it highly implausible that the Ptolemaic system was designed to explain Mars's change in brightness, and accordingly makes it very plausible that the phenomenon was another successful *and* novel prediction of his system that the selective realist has to deal with. It is worth noting that the novel and successful prediction is not restricted just to Mars. Ptolemaic theory also successfully predicts that Jupiter and Saturn have a change of brightness but so small that it is not observable by the naked eye (even if it is observable, and observed, by simple instruments, like telescopes).

In order to face it down, the selective realist needs to find a theoretical part in Ptolemy's system that is both sufficient for predicting Mars's change of brightness *and* approximately retained in Geocentrism. We simply do not see any such theoretical content. Of course, the realist could say that there is something *structural* retained, namely the variation in distances, common to both models. Yet, in the first place this does not look very theoretical; and secondly, and more importantly, again (given certain background conditions such as brightness depending only on distance) this "retained part" a priori follows from the phenomenon, and we saw above that a retention claim that is a priori true does not serve the realist's goals. The only possible realist reaction seems to be to dismiss this case as not novel, claiming that the historical data we offer does not conclusively show that Ptolemy did not design his system for accommodating change in brightness. Yet we think that, though not totally conclusive, the data provided suffice at least to put the burden of the proof on the realist side: according to the reasons given, the hypothesis that Ptolemy did not design his system to accommodate this fact is extremely more plausible than its negation. Thus, in absence of new historical data to the contrary, we have yet another prima facie case of novel, successful prediction implied by theoretical content not preserved, even approximately, by posterior theories.

6. Some objections

Ancient geocentric astronomy has generally been ignored in the ongoing scientific realism debate. We have argued here that Ptolemaic astronomy presents at least four predictions that are both successful *and* novel, in the sense that the theory was not designed to accommodate them, thus qualifying them as cases that deserve attention in the SR debate. Moreover, the fact that in these cases there seems to be almost nothing preserved at the theoretical level by the superseding heliocentric theory that preserves the observational predictions, makes them especially challenging for the selective realist.

Although our main goal here was to present a detailed reconstruction of these Ptolemaic predictions, leaving an overall discussion and assessment for future work, before concluding we want at least to take into consideration what we take to be the most immediate objections that the realist could raise. The first one is that this is a case of immature science, or simply too old, and thus does not deserve attention.

First, antiquity: is Ptolemaic astronomy too old to deserve attention? No; truth, and realism, are not time-dependent. If a (mature) theory makes successful novel predictions, it poses a case for SSR; no matter how old that theory is. If (approximate) truth and (approximate) retention are indispensable for explaining the success of modern science, they must also be indispensable for explaining the success of ancient science: NMA is not, and cannot be, time indexed.

Second, maturity. Is Ptolemaic astronomy too immature to deserve attention? No. As Laudan (1981) and Worrall (1989) emphasize in different manners, novel, i.e. not merely accommodative, success should *always* deserve attention, for NMA assumes that non-accommodative success, without partial approximate non-observable truth, would always be miraculous. Secondly, any non-question-begging notion of maturity qualifies Ptolemy's astronomy as mature science. For instance, according to Psillos (1999, 102) maturity is characterized by "the presence of a body of well-entrenched background beliefs about the domain of inquiry which, in effect, delineate the boundaries of that domain, inform theoretical research and constrain the proposal of theories and hypotheses", and all these features can be found in Ptolemy's theory (cf. e.g. Hanson, 1973; Kuhn, 1957).

Structuralists could perhaps call for a more strict notion of maturity that involves certain structural complexity, and then complain that Ptolemy's astronomy is structurally not enough interesting.[11] But this goes beyond NMA in assuming that not all novel successful predictions deserve attention, only those made by structurally enough complex theories do. We accept that this might be a possible strategy to overcome these alleged anomalies (strategy that would deserve close inspection anyway), but we do not think it is fair to *start* dismissing alleged counterexamples because they don't satisfy additional substantive constraints that go far beyond novelty and rely on a very specific and demanding notion of structure that might leave out theories (such as Natural Selection), that one does want to include (recall that in a just general notion of structure, Ptolemy's theory is quite mathematically complex, it has a complex mathematical structure).

A possible objection that a realist could rise without relying on additional (structuralist or other) conditions is related with the Venus' phases case but generalizes. We saw that Ptolemy predicts well some phases of inner planets and all phases of outer ones. But we also recalled the well-known fact that he wrongly predicts a new phase of inner planets at superior conjunction when it is actually full. This is a major failure and, as we also recalled, was used by Galileo against Ptolemaic geocentrism. Then, the objection goes, if a theory makes a so major failure, and not only this one but many other -for as we now know that it actually makes a great number of other dramatically wrong predictions-, can it count as empirically successful and its few novel, successful predictions deserve attention? A full response to this objection exceeds the limits of this paper, but a short answer will do for our present concerns: the NMA does not depend on the global balance between successful and unsuccessful (novel) predictions. It says that empirical success without partial true and approx retention is factually impossible, so single cases of alleged counterexamples are relevant for the NMA-based debate. Of course the more the cases the greater the relevance of the theory, and our claim is that Ptolemaic astronomy offers a sufficient number of cases to deserve attention. On the other side, all once-accepted and then-superseded theories, have been superseded due to major failures they have suffered. Dramatic anomalies are not strict refutations but put theories into Kuhnian crisis. This is true of Ptolemaic

[11] We thank Steven French for this comment (personal communication).

astronomy, but also of Fresnel's light theory, of Newton's mechanics, and all others. If a single impressive successful prediction by Fresnel's credits the theory as a relevant case (which squares with the realist retentivist thesis), regardless some other major failures, Why several novel successful predictions by Ptolemy's (that do not square with the retentivist thesis) should not? We believe that until the realist offers a non-question begging answer, the burden of the proof lies on her side.

To conclude, let us quickly comment a possible, general criticism mentioned in Section 4, namely that the successful predictions presented here should not worry the realist, and do not deserve special attention in the realist debate, since they are not really risky: the theory made numerous predictions and it is not surprising that some turn out to be true. An extreme version of this charge is to say that the theory also predicts things such as that Mars and Saturn do not collide, and that nobody would consider that this deserves any attention. With regard this last, extreme version of the criticism we do not think it is fair. That Mars and Saturn do not collide (and other predictions of this same kind) are not relevant for they are not relevantly novel/surprising. The theory predicts this fact, true, but the fact was well known and taken into account in designing the theory (is not a use-novel fact, in the sense of Psillos, 1999) for the theory was designed to accommodate, among other things (e.g. the retrograde apparent motion) that planets do not collide.

Now the not extreme version: Why should we pay attention to these predictions if the theory makes so many predictions that for sure some, among them the ones referred here, will turn out to be true? There are two readings of this objection, and we believe none works. According to the first, this means that the theory makes so numerous predictions that, although many fail, of course some will succeed just by chance. On this reading, the theory fails massively, but eventually gets right sometimes, thus this success should not worry the realist. Well, upon this reading the objection is clearly untenable: Ptolemy's theory does not massively fail. Actually, until the observation in 1610 of Venus' phases at second conjunction, all *observed* predictions were correct.[12] One might rejoin that what matters is not just observed (at a time) but *observable* predictions, and that we *now know* that Ptolemy's theory implies numerous facts that we can now observe do not obtain (for instance by sending a satellite). But then, by the same line of reasoning we should not take the Newtonian prediction of the discovery of Uranus as relevant for the realist debate since we now know that Newtonian mechanics makes numerous failing predictions.[13] And the same applies to any other superseded theory.

In a second reading the objection says that the theory simply makes such numerous predictions that it may get things right in some of them just by chance (it does not matter now how many or whether more predictions fail than succeed). But this literally applies to all superseded theories, so in order for this criticism not to kill as relevant cases Newton and Fresnel together with Ptolemy, there should be a feature that applies to Newton and Fresnel but not to Ptolemy and that makes the successful predictions of the former relevant while successful predictions of the latter irrelevant. The only one we can come up with is that in the Newton and Fresnel cases the numerous predictions are made using theoretical tools that are (approximately) retained by superseding theories, while in Ptolemy's case this is not so. But of course this would be question begging: to impose the retentive condition as conceptually necessary for considering a successful prediction relevant for the realism debate trivializes the realist position, SSR would just be a conceptual truism. And other features, different from retention, that one can consider apply to Newton, Fresnel and other never-questioned cases, also apply Ptolemy. Systematicity/maturity applies to all them, as we have seen. The condition (much weaker than retention) that from the perspective of the superseding theory one can understand why the superseded theory gets the right/wrong predictions it gets, also applies to Ptolemy. Antiquity (as opposed to immaturity) is, as we have just seen, irrelevant. And other, more substantive conditions, such as structural richness, either also apply, in a neutral reading, to our theory, or, in a more stringent reading, go far beyond NMA and put the burden of the proof on the realist side.

In any event, until the realist provides a relevant distinguishing feature that serves to ignore Ptolemy's predictions, the burden of the proof lies on her side. The selective realist may, on a closer analysis, come up with a non-question begging manner of disqualifying Ptolemy's novel, successful predictions as relevant for the debate. Or accept them but, contrary to appearances, find a theoretical content that is both responsible for these predictions and approximately preserved by posterior astronomic theories (and whose retention is still fallible, i.e. not a priori implied by the prediction in point, cf. Section 4). This is precisely part of the discussion we wanted to open by presenting these predictions in enough detail and launching Ptolemy's case into the realist debate. Unfortunately this second task goes beyond the limits of this paper; the detailed analysis and assessment of these anomalies and their consequences for selective scientific realism are left for future work.

[12] Except for the apparent size of the Moon at quadratures that he predicted to be by far bigger than observed but, in this case the exception confirms the rule.

[13] The retentivist way in which the realist solves the Newtonian case is not relevant here: first we have the successful novel predictions of a false theory, then we explore a selective realist solution, if possible (our point is precisely that this retentivist strategy does not seem to work in our case).

References

Aaboe, A. (1963). On a Greek qualitative planetary model of the epicyclical variety. *Centaurs, 9*, 1-10.

Bowen, A. C. (2013). Simplicius on the planets and their motions. In *Defense of a Heresy*. Leiden-Boston: Brill.

Carman, C. (2009). Rounding numbers: Ptolemy's calculation of the Earth-Sun's distance. *Archive for History of Exact Sciences, 63*(2), 205-242.

Chakravartty, A. (1998). Semirealism. *Studies in History and Philosophy of Science, 29*, 391-408.

Chang, H. (2003). Preservative realism and its discontents: Revisiting caloric. *Philosophy of Science, 70*(December 2003), 902-912.

Copernicus, N. (1543). *De revolutionibus orbium cœlestium* (Nuremberg, 1543)

Espenak, F. (2014). *The 2012 transit of Venus*. Internet access (date June 12, 2014) http://eclipse.gsfc.nasa.gov/OH/transit12.html

Evans, J., & Carman, C. (2013). Mechanical astronomy: a route to the ancient discovery of epicycles and eccentrics. In N. Sidoli, & G. van Brummelen (Eds.), *From Alexandria, through Baghdad: Surveys and studies in the Ancient Greek and Medieval Islamic mathematical sciences in honor of J.L. Berggren* (pp. 145-174). Springer.

Goldstein, B. R. (1967). The Arabic version of Ptolemy's planetary hypotheses. *Transactions of the American Philosophical Society, New Series, 57*(4).

Goldstein, B. R. (1996). The pre-telescopic treatment of the phases and apparent size of Venus. *Journal for History of Astronomy, XXVII*, 1-12.

Goldstein, B. R. (2007). Some medieval reports of Venus and mercury transits. *Centaurus, 14*(1), 49-59.

Hanson, N. R. (1973). *Constellations and conjectures*. Dordrecht: Reidl.

Heath, Th. ([1913] 1997) *Aristarchus of Samos. The ancient Copernicus. A history of Greek astronomy to Aristarchus together with Aristarchus' treatise on the sizes and distances of the Sun and Moon*. (First edition: Oxford: Oxford University Press) Oxford: Oxford and Clarendon University Press.

Jones, A. (2006). The Keskintos astronomical inscription: Text and interpretations. *SCIAMVS, 7*, 3-41.

Kapoor, R. C. (2012). *Did IbnSina observe the transit of Venus of 1032 CE?*. ICHA Science Meetings at the IAU XXVIII General Assembly.

Kuhn, T. (1957). *The copernican revolution*. New York: Vintage Books.

Ladyman, J. (2011). Structural realism versus standard scientific realism: The case of phlogiston and dephlogisticated air. *Synthese, 180*(2), 87-101.

Laudan, L. (1981). A confutation of convergent realism. *Philosophy of Science, 48*, 19-48.

Lyons, T. (2006). Scientific realism and the Stratagema de Divide et Impera. *The British Journal for the Philosophy of Science, 57/3*, 537-560.

Meeus, J. (1958). The transits of Venus from 3000 B.C. to A.D. 3000. *Journal of the British Astronomical Association, 68*, 98-108.

Meeus, J. (1998). *Astronomical algorithms* (2nd ed.). Virginia: Willmann-Bell.

Neugebauer, O. (1972). Planetary motion in p. Mich. 149. *The Bulletin of the American Society of Papyrologists, 9*(1–2), 19-22.

Neugebauer, O. (1975). A history of ancient mathematical astronomy. In *Studies in the history of mathematics and physical sciences 1, 3 Vols.* Berlin: Springer.

Perdersen, O. (2010). *A survey of the Almagest. With annotation and new commentary by Alexander Jones*. New York: Springer.

Pliny. (1962). Natural history in ten volumes. In *Praefatio, libri I, II with an English translation by H. Rarckham* (Vol. I) Cambridge: Harvard University Press. The Loeb Classical Library.

Psillos, S. (1999). *Scientific realism: How science tracks truth*. Routledge.

Robbins, F. E. (1936) *Michigan Papyri*, Vol. III, J. G. Winter (Ed.), Ann Arbor: 62–117.

Swerdlow, N. (1969). Hipparchus on the distance of the Sun. *Centaurus, 14*(1), 287-305.

Toomer, G. J. (1974). Hipparchus on the distances of the Sun and Moon. *Archive for History of Exact Sciences, 14*, 126-142.

Toomer, G. J. (1998). *Ptolemy's almagest* (First Edition: London: Durkworth, 1984). Princeton: Princeton University Press

Van Helden, A. (1985). *Measuring the universe: Cosmic dimensions from Aristarchus to Halley*. Chicago, Ill: Chicago University Press.

Vickers, P. (2013). A confrontation of convergent realism. *Philosophy of Science, 80*(2), 189-211.

van der Waerden, B. L. (1955). *Die Astronomie der Pythagoreer*. Amsterdam: Verhandelingen Kon. Akad, 1st series, xx, no. 1.

van der Waerden, B. L. (1974). The earliest form of the epicycle theory. *Journal for the History of Astronomy*, 175-185.

Worrall, J. (1989). Structural realism: The best of both worlds? *Dialectica, 43*, 99-124.