



Does Explaining Past Success Require (Enough) Retention? The Case of Ptolemaic Astronomy

José Díez¹ · Gonzalo Recio² · Christian Carman³

Accepted: 22 September 2021 / Published online: 11 July 2022
© The Author(s) 2022

Abstract

According to selective, retentive, scientific realism, past empirical success may be explained only by the parts of past theories that are responsible of their successful predictions being approximately true, and thus theoretically retained, or approximated, by the parts of posterior theories responsible of the same successful predictions. In this article, we present as case study the transit from Ptolemy's to Kepler's astronomy, and their successful predictions for Mars' orbit. We present an account of Ptolemy's successful prediction of Mars' orbit from Kepler's perspective, and scrutinize whether the theoretical elements responsible for Ptolemy's empirical success are approximately retained in Kepler. In order to give to the realist the best chances, we try different strategies. We conclude that all fail and thereby this case constitutes a *prima facie* strong anomaly for selective retentive realism. Structural realists may call preservation of structure to the rescue, but the existing notions of structure do not work. In absence of a new notion that works, the burden of the proof lies on the realist side.

Keywords Selective scientific realism · Ptolemy · Kepler · Past empirical success

1 Selective, Retentive Realism and Explanation of Past Success

Selective scientific realism is currently accepted as the most, or even the only, plausible version of scientific realism about unobservables. Scientific realism about unobservables (SRU) claims that the non-observational content of our successful/justified empirical theories is true, or approximately true. Empiricist strictures recommend us to confine

✉ José Díez
diez.ja@gmail.com

Gonzalo Recio
gonzalorecio@hotmail.com

Christian Carman
ccarman@gmail.com

¹ LOGOS, BIAP, Univ. Barcelona, Barcelona, Spain

² UNQ, UNIPE, Buenos Aires, Argentina

³ UNQ, CONICET, Buenos Aires, Argentina

our beliefs to what our observations justify. As is well known, although some empiricist such as van Fraassen take actual observations to justify only that the theory is empirically adequate (which means that its observational content is true), realists think that successful observed predictions justify our believe in unobservable content as well. Today the best argument to justify such a step from what is observed to what is unobservable, is taken to be the no miracles argument (NMA): the predictive success of a theory that makes *novel* observational predictions making use of non-observational content/posits would be inexplicable, miraculous, unless such non-observational content approximately corresponds to the world “out there”. In short, SRU provides the best (/only?) explanation for the empirical success of predictively successful theories. Here the “novel” restriction, meaning that the theory was not cooked for predicting the known fact, is crucial, since the best explanation of a non-novel prediction is of course that the theory was so cooked.¹ According to realists, other alleged explanations of novel empirical success, such as van Fraassen’s (1980) constructive empiricism, or Wray’s (2018) selectionism, are not as good, or are not explanations at all.²

What is currently considered as the greatest challenge to NMA is the—already advanced by Poincaré and brought into this debate by Laudan (1981)—Pessimistic(meta-)Induction (PI): the history of science offers plenty of cases of predictively successful yet (according to many) radically false theories. Laudan’s challenge was resisted in different ways, among which the one consisting in reducing his list arguing that many items were not cases of mature science or of novel predictions. Yet not all cases could plausibly be so dismissed and realists accepted that in at least some cases (e.g. caloric, ether, ...) the successful novel predictions were made by positing unobservables (the caloric fluid, the mechanical ether) that according to the superseding theories do not exist *at all*. Here is where *selective*, or *partial*, realism—inspired by Poincaré and Duhem—comes to the rescue: when a theory makes a novel, successful prediction, the part of its non-observational content really used, i.e. that is indispensable for fueling the prediction, need not be the whole non-observational content. Indeed, usually it is only a proper part of the theory’s non-observational content that is essential for the novel prediction, and according to NMA it is just the approximate truth of such part what explains the observational success and what must, accordingly, be believed as approximately true. This is the core of the *divide et impera* move (Psillos 1999): divide the non-observational content into, on the one hand, what is necessary for fueling novel predictions and then approximately true and approximately retained by posterior, superseding theories; and, on the other hand, what is just *idle* and may simply be radically wrong and very likely abandoned in the future.

Selective Scientific Realism (SSR) can then be summarized as follows³: when a theory T makes a successful novel prediction P, there is a part Tr of T’s non-observational content that is: (i) responsible for the successful prediction P, (ii) approximately true, and (iii) approximately preserved by posterior theories which, if more successful, are more truth-like. According to selective realists, (ii) explains synchronic empirical success, and (iii) explains diachronic preservation of empirical success and, crucially, makes the realist position empirically/historically testable: without (iii) SSR would be a (meta)empirical

¹ This is compatible with non-novel predictions having other value, for instance explanatory, but NMA is not based on explanatory virtues (other arguments are).

² See e.g. Psillos (2020), Vickers (2020), French (2020), for recent rejoins of this latter selectionist antirealist strategy).

³ We borrow this specific formulation from Carman and Díez (2015)

assertion inaccessible to (meta)empirical assessment. In the above mentioned cases, for instance, SSR claims that although caloric and ether theories are false, they are *not totally* false; each theory has a non-observational part/content that is responsible for the relevant novel successful predictions which is approximately true and (as predicted by SSRiii) has actually been approximately retained by its successor(s).

Realists may differ on how to identify Tr (e.g. *structurally*—Worrall 1989, Ladyman 1998; French 2006—, through “*detection*” *properties*—Chakravartty 1998—or *case by case*—Psillos 1999; Votsis 2011; Peters 2014; Vickers 2017) but, as many emphasize, (Stanford 2006; Votsis 2011; Peters 2014; Vickers 2017) Tr cannot be identified/defined backwards, i.e. just looking from the posterior theory and construing/defining Tr of the past theory as what was *actually preserved* by the posterior theory. For one thing, nothing guarantees that there is anything substantially theoretical preserved (besides, say, logic or general mathematics, more on this below). For another thing, when something substantive is preserved, nothing guarantees that it is sufficient for fueling P; and in case it is not, then it does not serve for NMA. For still another thing, nothing precludes that posterior theories also preserve idle parts, which so not belong to Tr. But the main reason is that this backward identification of Tr would undermine the empirical character of SSR making (iii) unfalsifiable (and thus trivializing SSR). Selective realists trust that (iii) is and will be historically true, but they accept that it is fallible. This required fallibility is what makes the mentioned post-hoc strategy unacceptable (“the benefit of hindsight”, in Psillos 1999 words). Tr must be identified *as if we were living at T’s times*. It is precisely this fallibility of retention what drives the way in which the current case by case debate proceeds. Realists defend that going selective, scientific realism has solved its alleged anomalies (caloric, ether, phlogiston, ...). Critics argue that they have not, that what is retained does not suffice for fueling the relevant predictions, and also put new alleged anomalies on the table (see e.g. Lyons 2006 and Vickers 2013 for candidates).

According to SSR, the retentive clause (iii) is essential for its explanatory virtues, for SSR does explain not only the success of current theories but also the success of the previous theories when successful, and the degree to which they were successful. For instance, the retention by Maxwell of Fresnel’s equations explains the novel successful predictions of Fresnel’s theory. The parts of Newton’s theory retained by Einstein, explain Newton’s successful predictions and (in this approximative case) the degree to which he was successful, or so realists claim. Explanation of past observational success then depends on the (approximate) preservation of the relevant non-observational part of the former theory by the latter.⁴

The claim that when there is success there is (an approximate) posterior preservation of the relevant parts, makes SSR (meta-)empirically testable and constitutes one of its main advantages (compared to merely testimonial realism, e.g. Popper’s), but also one of its main risks. It is this claim that we want to scrutinize here. Our case study aims at showing that it is possible to understand why a previous theory succeeded, and the degree in which it succeeded, and it failed, even in cases in which what is retained of the superseded theory

⁴ To be more precise, what explains past theories’ success is, as always according to (selective) realism, their (partial) approximate truth; what retention of the relevant parts makes, is to explain past theories’ success relative to the (alleged) partial approximate truth of the posterior theories. This is how, as said above, the retention meta-prediction makes SSR historically testable, for it *implies* retention. The retentivist realist might try to weaken this retention demand while still accommodating NMA, but as far as we know no such retentivist version has been offered (of course there are non-retentivist options, which we briefly mention below and whose scrutiny goes beyond the limits of this paper),

by the superseding theory clearly does not suffice for fueling the successful predictions of the superseded one. We will proceed in two steps. First (Sect. 2), we show how we can derive Ptolemy's prediction of Mars' observable celestial longitude (i.e. its angle on the plane of the ecliptic with respect to the position that the Sun has at the autumnal equinox), and the degree of this success, from the perspective of Kepler's theory. Secondly (Sect. 3), we will discuss what theoretical parts of Ptolemy's theory can be considered retained by Kepler's, and whether they suffice to explain the observable successful prediction. In case they do not suffice, this case study would undermine one of the alleged virtues of SSR: even if in some cases previous success is explained by something retained, there are cases, such as this one, in which past success would be explained without enough retention.⁵

The case we have reconstructed in detail is, as just mentioned, Mars' theoretical model for accounting for Mars' celestial longitude, that is, Mars' spatiotemporal observed trajectory (including speed) in the visual sky. Mars' observed trajectory, though, is not a case of a novel prediction, since Ptolemy's model for Mars is built to account for its known seeming trajectory (including speed). Nevertheless, this does not mean that Ptolemy's theoretical model for Mars is merely accommodative. To start with, Ptolemy's astronomy is not, in general, merely accommodative, for the geometrical models he proposes to account for the observed longitudes predict other phenomena which constitute novel predictions, (cf. Carman and Díez 2015; Carman 2015). With respect to the specific model for Mars, although the qualitative model is built from known observed trajectory-data, its quantitative tuning is adjusted when new, more precise data are measured, and the fact that the qualitative model may be quantitatively tuned for new data is an epistemic value that also needs a non-trivial explanation. One of the aspects in which we believe this case study is important is that we are able to derive in full detail the degree of (small) errors in Ptolemy's prediction.⁶

Secondly, we claim that retention in the explanation of past success is important also in some cases independently of the presence or absence of novel predictions. The best explanation that a known data is predicted by a theory that is designed to square with/predict such data is not of course that it is approximately true but that it has been so cocked up. *But* if a posterior, superseding theory makes the same successful prediction with a new theoretical apparatus, then realists must call for (approximate) preservation also here. Newton's theory was designed using Keplerian known planetary trajectories. Einstein's theory makes the same successful planetary predictions (and improves some previous unsuccessful ones, such as Mercury's trajectory), and it would be a problem for the realist if the theoretical part of Newton's theory used for fueling such predictions were not (approximately) retained by Einstein's. Likewise in our case. Some might argue that the two cases are different in that Newton's theory was designed using known planetary trajectories, but it was

⁵ One might suggest that NMA-realists may account for successive success without retention, when the same fact is explained by the new theory "changing perspective", for instance when somebody explains an accident appealing to the slippery road and other to, e.g., that the driver was sleepy (we thank an anonymous reviewer for this suggestion). We agree that there exist such cases in ordinary explanations, but in such cases the new explanans is not alternative/incompatible to the first one but complementary; one explanans focusses on one part of a causal chain while other changes the perspective and focusses on a different part of the (assumed) same causal chain. Nothing like this happens in our case study, or in any other similar intertheoretical change (e.g. phlogiston to oxygen case, Newton to Einstein case, ...) in history of science.

⁶ In this regard, our approach substantially improves what, as far as we know, is the only other philosophical analysis in the current realism debate of the transition from geocentrism to heliocentrism, Wray 2018, which is merely qualitative and barely enters in astronomical details; the same applies to the brief reference made by Niiniluoto (1999).

not in general a merely accommodative theory, nor a “merely saving appearances” tool. But the same happens with Ptolemy’s which, as just said, *did* make novel predictions; and it was not a “merely saving appearances” tool either for it was conceived as describing real physical mechanisms and structures of the heavens (Lloyd 1978).

Finally, and really crucial, although Mars’ model does not do novel predictions *with respect to an observed spatiotemporal trajectory*, the model *does* novel predictions with respect other observational phenomena. In particular, one of the Ptolemy’s novel predictions presented by Carman and Díez (2015, 29–32) is a novel observational consequence of Mars’ theoretical model, namely the increasing apparent size/brightness during the retrograde motion of Mars:

As explained in this work, the spatiotemporal trajectory of retrograde observed motion, excluding the changes in speed, is compatible with the theoretical direction in the epicycle being either direct or inverted direction. The known changes in the observed speed during retrogradations is what made Ptolemy opt for the direct direction (Carman and Díez 2015, 31–32). But this choice, based on the changes in observed speed, has novel observational consequences. In particular, the direct direction has as a theoretical consequence that Mars is closer to Earth at opposition, which in turn has the observational consequence that in such parts of the retrogradation Mars is seen as first increasing and after decreasing in apparent size/brightness (with auxiliaries such as that the apparent size of the same planet depends only on the distance to the observer), prediction which is both risky and successful. And, as argued in Carman and Díez (2015), historical records support the claim that this observable consequence was not used to construct the model. Moreover, this prediction is even more accurate and risky than what Carman and Díez indicate, since in Ptolemy’s model the variation in Mars’ apparent size/brightness not only depends on its position on the epicycle, but also on the position of the center of the epicycle on the deferent. Given that the deferent is not centered on the Earth (i.e. the deferent is eccentric), the center of the epicycle varies its geocentric distance, and with it the whole epicycle on which the planet moves changes its distance to the Earth. So, there are two theoretical periods on which the Mars-Earth distance depends: the revolution of Mars on its epicycle, and the revolution of the center of the epicycle on the eccentric deferent. An opposition marks the moment of maximum closeness for the first period, and the location of the center of the epicycle on the perigee of the deferent marks the maximum closeness for the second. When the two moments coincide, we obtain the minimum distance between Mars and the Earth, with the corresponding consequences in apparent size/brightness. And, importantly, unlike what happens with the variation in brightness between opposition and any other elongation, the variation in brightness between oppositions is not perceptible with the naked eye but needs modern telescopes, so it is certain that it was not used as an input in building the model (time-novelty guarantees use-novelty). Thus, with this more precise description, the successful observable prediction in point is even riskier and uncontroversially novel.

One might argue that, even if we succeed, this case is not crucial against SSR, for Ptolemy’s successes are not sufficiently numerous and varied.⁷ If correct, this criticism would rely on the alleged *immaturity* of Ptolemy’s astronomy. We, though, do not think this is correct. Ptolemy’s successful predictions reported by Carman and Díez (2015) are enough numerous and varied, and we take that Ptolemy’s astronomy is a mature science (yet of course within the limits of ancient science) according to any non question-begging

⁷ We thank an anonymous reviewer for this objection.

standard. Philosophers and historians such as Kuhn, Hanson, Lloyd and even Duhem consider it as mature science, and as far as we are aware, all parties in the realist debate treat Ptolemy's astronomy as a piece of mature science.

Before starting with the analysis of this case in the next section, let us conclude this one emphasizing that our case study represents a challenge exclusively for the *retentive* version of (selective) scientific realism, which, as just said, we take to be the best (or even the only plausible) realist option, the main reason being that it is the only one that is (meta)empirically testable through the history of science, thus not being *merely testimonial*. There are recent non-retentive reformulations of SR (e.g. Doppelt 2011; 2014, *best theory* realism; Mizrahi 2013 *relative* realism; Saatsi 2016 *minimal* realism; Vickers 2019 *qualified* realism), which therefore are not directly affected by our case study, although they have their own—according to us unsurmountable—difficulties (some of them can hardly be qualified as proper realist accounts⁸). We cannot, though, discuss them here, so what follows is addressed to challenge exclusively the retentivist family of selective realists. In the next section we present the main elements of the relevant prediction about Mars and reconstruct its transit from Ptolemy to Kepler, and in Sect. 3 we analyze whether accounting for Ptolemy's success requires enough theoretical retention and its consequences for retentivist realism.

2 The Ptolemaic Prediction of Mar's Orbit from Kepler's System

The motion of the planets has been one of the major problems in astronomy since its beginning. The irregular paths they describe, retrograding through the Zodiac against the “fixed” stars, presented an extremely difficult and, at the same time, alluring challenge to the astronomers of all times who tried to reduce them to hidden regularities (the motion of the planet through the Zodiac is measured in degrees, the 0° point being the place where the Sun locates on the Zodiac at the vernal equinox). Starting with Eudoxus and Callipus, whose systems of homocentric spheres can be found in Aristotle's *Metaphysics* (1073a15–1074b15), up to the complex models Ptolemy devised in his *Almagest*, Greek astronomers slowly built a comprehensive corpus of theories that allowed for the accurate prediction of planetary positions at any given moment.

A very important step was the incorporation, around the third century BCE, of the epicycle and deferent model in which the planet M moves on an epicycle with center P, which in turn is moving on a geocentric deferent. In such a model, though, the retrograde loops are always equally sized and separated, and cannot then account for the fact that retrogradations are not evenly placed throughout the Zodiac, nor are their amplitudes—the apparent size of their loops—always equal. To solve this problem, the next step was the introduction of an eccentric deferent: the deferent is no longer geocentric, but instead its center is displaced from the center of the Earth. The retrograde loops are now not equally separated nor equally sized from the observer's point of view: instead the loops located at the part of the deferent which is nearer to Earth—its perigee—will be seen as being both more separated and larger than the ones located in the opposite part of the deferent, the one farther away from Earth—its apogee—(Fig. 1).

⁸ This is argued, for instance, in Alai (2016) and Díez (2018) about Doppelt's, and in Rowbottom (2019) about Saatsi's.

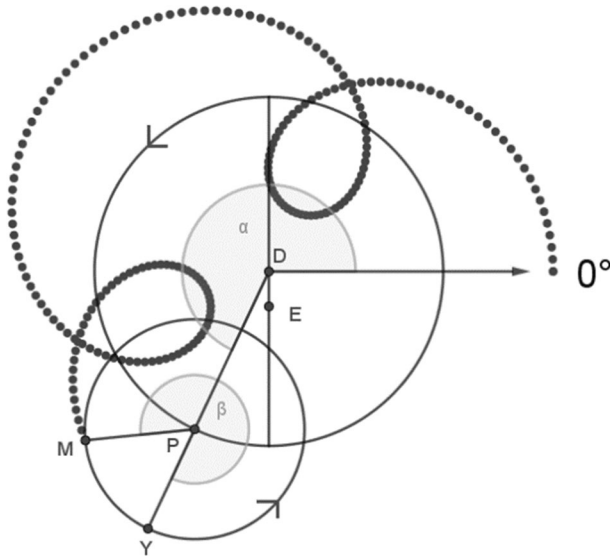
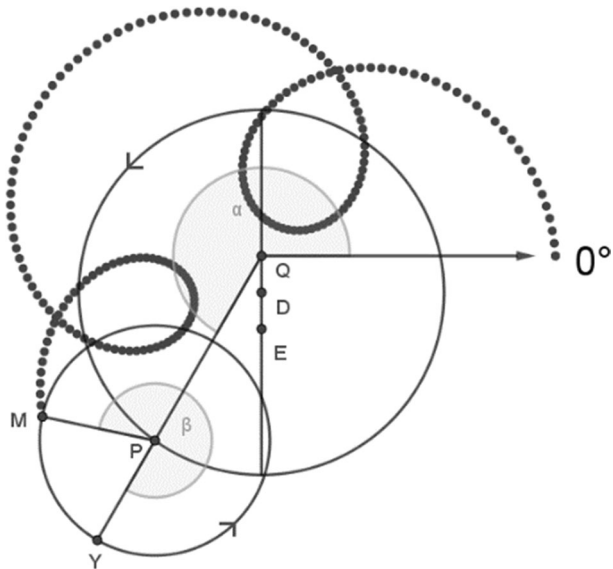


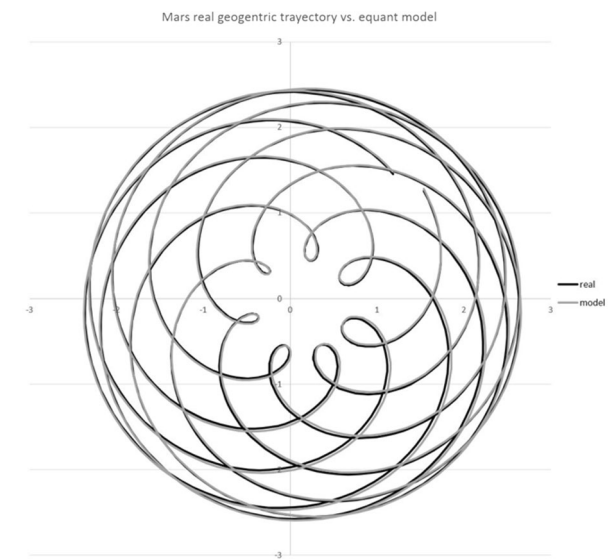
Fig. 1 The epicycle-deferent model with eccentric D, in which, seen from the Earth E, the retrogradation loops which are farther from the Earth are seen as closer to each other, while the ones closest to it are seen as more separated

Fig. 2 A full Ptolemaic planetary model, with the Earth at point E, the equant Q, and the center of the deferent bisecting EQ, at D. The motion of the center of the epicycle P, represented by α , is uniform as seen from Q, and the prolongation of the line QP determines the mean apogee of the epicycle Y, from where the uniform motion of the planet M, represented by β , is measured



The innovation allowed astronomers to correctly predict the moment and location of the retrogradation, but not the amplitude of the retrogradation. This anomaly remained until the beginning of the second century AD, when Ptolemy composed his *Almagest* (Ptolemy 1984), in which the planetary model includes a double eccentricity with respect to Earth E: one corresponding to the center D of the deferent, and one

Fig. 3 Representation of a comparison between the retrograde motions of Mars actually observed and the ones predicted by a full Ptolemaic model. While the eccentric model shows errors of several degrees near the apogee and perigee of the deferent, Ptolemy's equant model solves these discrepancies



corresponding to the center of uniform motion Q , the point from which the motion of the center of the epicycle E is perceived as uniform (Fig. 2). They are related so that the second one is always exactly in the same direction of the first one as seen from the Earth, and the center of the deferent D precisely bisects distance E - Q .

This modification brought a stunning improvement in the precision of the planetary models. One of the most striking examples can be found in the case of Mars (see Fig. 3), whose big eccentricity and closeness to the Earth caused some of the most challenging irregularities of all the planets.

The Ptolemaic model for Mars became the standard for astronomical inquiry until the times of Brahe and Kepler, more than 15 centuries later. It was not until Kepler (2015) that a new level of accuracy could be achieved, thanks to his own efforts to finally demolish the remaining vestiges of Hellenistic astronomy: circularity in the shape of the orbits, and angular uniformity in its motions. Although theoretically monumental, the Keplerian shift did not bring a great advance in precision, at least not if it is compared to the advancements produced by the *Almagest* in its own time. As he himself tells us, his *Astronomia Nova* implied a betterment of only 8 min of arc in heliocentric terms. But, to gain those 8 min, the destruction of Ptolemaic astronomy was a necessary step.

What was the cause of Ptolemy's so great a success, specially about the challenging Martian orbit? How could an astronomical system which was born under such different—and, ultimately, false—theoretical s so similar results as the ones derived from Kepler's own theories? In order to answer this question, a careful evaluation of the relation between Ptolemy's and Kepler's models must be carried out. For the technical analysis of Ptolemy's models for Mars' seen from Kepler's we summarize in what follows the reverse step-by-step analysis made by Carman and Recio (2019).

In the Keplerian model for Earth-Sun-Mars system (Fig. 4), Mars (M) moves on an ellipse with the Sun (S) at one of the foci. The other focus is F_M , and D_M is the center of the ellipse. Angle β grows in such a way that line SM sweeps equal areas in equal times (Kepler's second law). The Earth also moves on an ellipse with S at one focus, with an

Fig. 4 Keplerian model of the Earth-Sun-Mars system (not to scale)

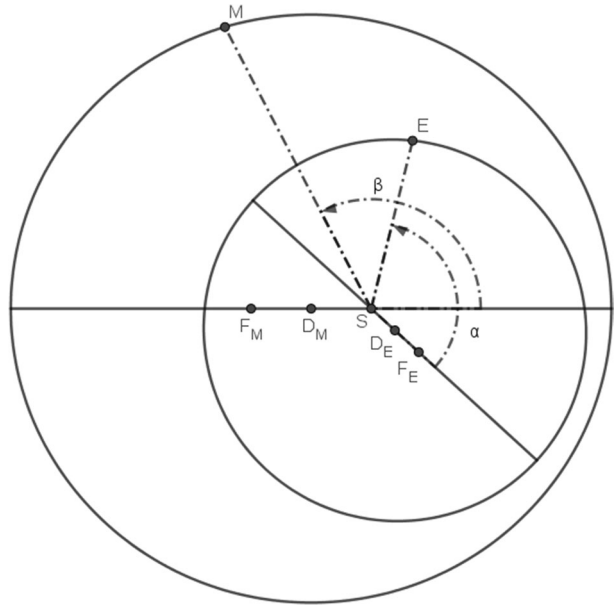
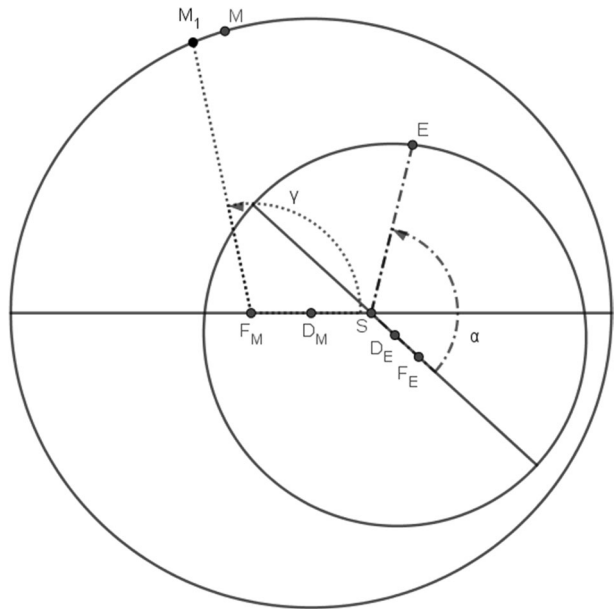


Fig. 5 Keplerian model where Mars moves not according to Kepler's second law, but according to the equant law, in which angle γ grows in such a way that Mars moves with uniform angular motion around the empty focus F_M . This means that the predicted position will differ from the previous Keplerian model, locating Mars not at M , but at M_1



empty focus F_E , and an ellipse center D_E . Angle α grows also in accordance with Kepler's second law.

The first step back consists in replacing line SM sweeping equal areas in equal times by the Martian equant F_M with uniform angular motion of angle γ determined by lines $F_M M_1$ (M_1 being the new position of Mars) and $F_M S$ (Fig. 5).

Fig. 6 In this step the planet does not move on an ellipse—here represented with a dashed line—any more, but on a circle with center at D_M , the center of the previous ellipse, and a radius equal to the semi-major axis of the previous ellipse. This means that the predicted location of the planet will change with respect to the one of the previous model—except at perihelion and aphelion—: instead of being located at M_1 , it will be located at M_2

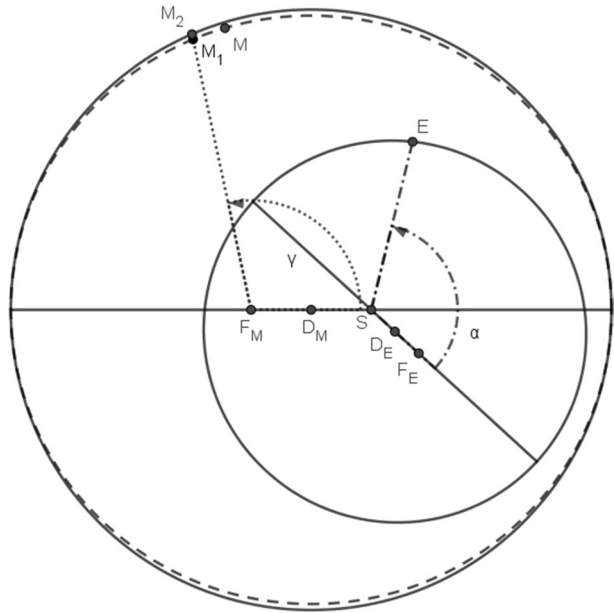
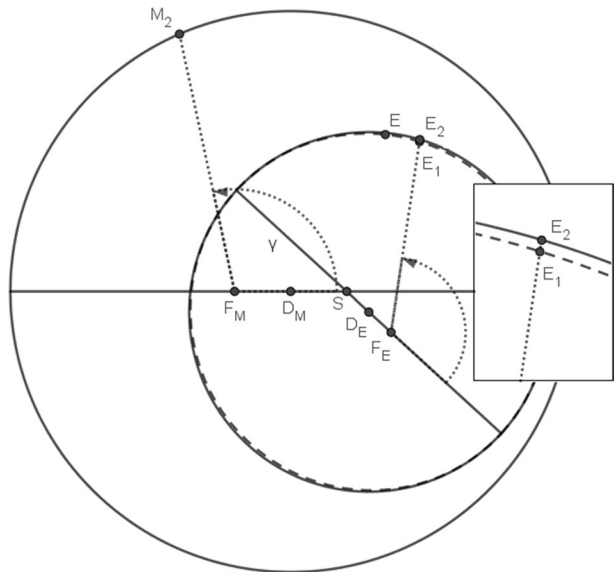
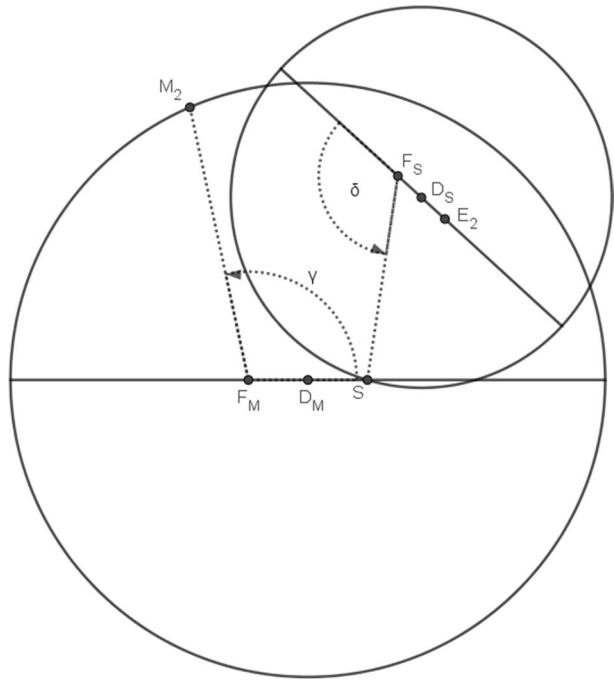


Fig. 7 Model for the Earth-Sun-Mars system when Kepler's first two laws are replaced by equants and circular orbits



The second step consists in substituting the Keplerian Mars' ellipse by the Ptolemaic Mars' circle with the same center D_M (dashed and solid lines in Fig. 6, respectively). Given that the equant F_M still governs the motion of the planet, the direction of the line joining the equant and Mars will remain the same, so the only change in location is Mars moving from M_1 to M_2 .

Fig. 8 The Earth E_2 becomes stationary, and the Sun moves around it. The center of the orbit of the Sun is D_S , with a center of uniform motion at F_S . The motion of Mars remains referenced to the Sun S . Thus, the apsidal line F_MS moves with the Sun, as does the orbit of Mars

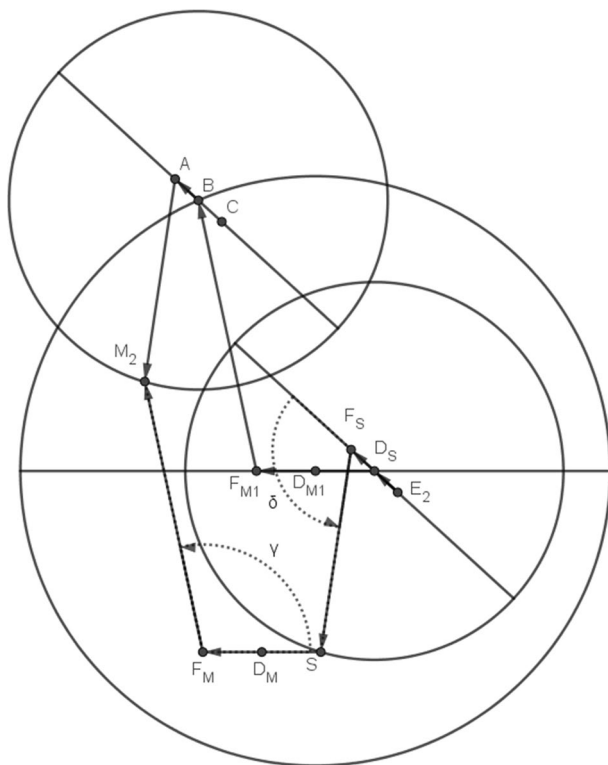


The next step consists in applying similar changes to the Earth's orbit (Fig. 7). On the one hand, we get a new position of the Earth, in E_1 instead of E , because of the incorporation of uniform angular velocity from the equant point F_E instead of Kepler's area law. On the other, the orbit of the Earth becomes a circle with center at D_E —the previous center of the ellipse—, and with a radius equal to the semi-major axis of the previous ellipse. This will change the predicted position of the Earth, from E_1 on the ellipse—here represented with a dashed line—to E_2 on the circle.

These four changes imply differences in the predicted geocentric position of Mars: if we take the Sun as the reference, we have changed both the Earth's and Mars' positions, in a way that does not preserve the relative positions between those two bodies. Now, in the next step, we change the system of reference, moving from a heliocentric to a geocentric model, with a stationary Earth, and the Sun moving around it, carrying with it Mars' orbit (Fig. 8). The Earth is stationary at E_2 and so the Sun S moves on a circular orbit with center at D_S . The eccentricity E_2D_S is the same as the previous SD_E , but the direction is inverted. The center of uniform motion of the Sun will be F_S . Analogously, the eccentricity E_2F_S will be the same as the previous SF_E but, also, with inverted direction. The angular motion of S with respect to F_S , here represented by δ , will be the same as the previous angular velocity of the Earth with respect to F_E . Mars model remains the same, but now with a revolving Sun. It is a model analogous to that of Tycho (but with eccentrics and equants).

The next step involves a new way of decomposing the relative motions between the Earth and Mars into circles. We transform the model from one with an eccentric Martian orbit, into one with an epicycle. This means that we leave a stationary deferent for Mars—instead of the heliocentrically referenced orbit we had until now. To account for the Earth's motion, though, we incorporate an epicycle in Mars' system. This can be easily understood as a vectorial equivalence (Fig. 9). The relative position between the Earth E_2 and Mars

Fig. 9 There is a new change in the geometrical representation of the apparent motion of Mars. While in the previous step Mars was moving on an orbit which in turn moved around the Earth on a small circle—the orbit of the Sun—the orbit of the Sun, now it is moving on a small circle with center B, equal to the orbit of the Sun, and uniformly to the corresponding center of uniform motion A. This small circle is in turn moving on an orbit whose apsidal line now passes through D_S

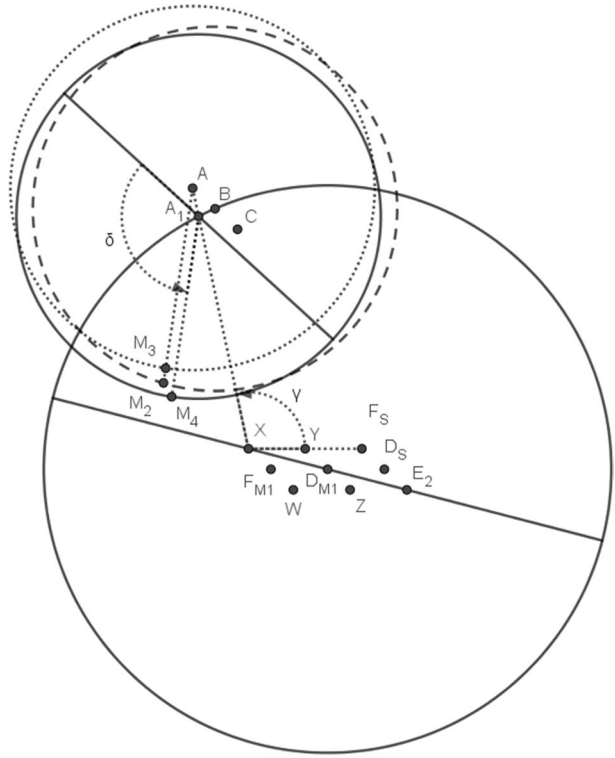


M_2 can be expressed as the addition of vectors $E_2D_S + D_SF_S + F_SS + SF_M + F_MM_2$ —as Figure—, but also as $E_2D_S + D_SF_{M1} + F_{M1}B + BA + AM_2$. Given that $D_SF_S = BA$; $F_SS = AM_2$; $SF_M = D_SF_{M1}$; and $F_MM_2 = F_{M1}B$, then both additions give the same relative position between E_2 and M_2 . It is important to note that these last changes do not involve any difference in the predicted geocentric positions for Mars, since they can be understood as merely changes in the way the relative motions between the bodies are analysed.

The last step can be understood, in theoretical terms, as a change in the position of the apsidal line, which now passes through the geocentrically fixed center of the orbit of the Sun D_S rather than the moving Sun S (Fig. 10). Thus, the equant of Mars, F_M , is now replaced by the geocentrically fixed F_{M1} , the equant of the center of the epicycle, B , and the center of Mars' orbit, D_M , is replaced by D_{M1} , the center of the deferent that carries the center of the epicycle, B . Mars, in turn, now moves on a small circle, like an epicycle, with center B around a center of uniform motion A , with an angular velocity equal to that of the Sun around F_S .

Regarding the properties of the system, Ptolemy never realized that the epicycle had two relevant points, one which determines the center of uniform motion, and one which determines the center of the epicycle itself—points A and B respectively, Fig. 10. This lack of distinction in the model caused further mistakes regarding the position of the deferent. Given that he was using the mean Sun as the main marker for determining the position of the center of the epicycle, he concluded that the center of the epicycle was indeed the same as its center of uniform motion. Thus, he thought that the center of the epicycle was in the

Fig. 10 Given that the new epicycle is responsible for representing the annual motion of the Earth, it too has three points, A, B and C, two of which are relevant to our discussion: A as the center of uniform motion, and B as the center of the epicycle itself. And each of them has a corresponding apsidal line, which determines their centers of uniform motion, and the centers of the circles on which they move. Apsidal line WZE_2 corresponds to C, $F_{M1}D_{M1}D_S$ to B, and XYF_S to A



direction of point A, yielding the dotted epicycle, instead of the dashed epicycle centered on B, and a new position M_3 for Mars. But the use of the mean Sun would also lead him to determine an equant X, and a center D_{M1} for the Martian deferent, ultimately resulting in a center A_1 for the solid epicycle, and also for the point of uniform motion, which makes δ to grow uniformly. This gave him a final position M_4 for Mars.

Summing up: we have (1) replaced the Martian law of areas with an equant, (2) replaced Mars' elliptic orbit with a circle, (3) applied these two modifications to Earth's orbit as well, (4) changed the system of reference to a heliocentric one, (5) analysed the relative motions between the Earth, the Sun, and Mars, in such a way that we obtain an epicyclic model, and finally (6), simplified the epicycle in the same way Ptolemy did, retaining only one relevant point as the center of the epicycle and of Mars' uniform motion on it, with the corresponding modification in the position of the apsidal line. Steps (1)–(3) and (6) involve a modification of the geocentric position of Mars, while steps (4) and (5) only imply a different, though observationally equivalent, geometrical representation of their relative motions.

3 Accounting for Past Success Without Enough Retention?

The observational machinery in Ptolemy and Kepler is the same: apparent trajectories in the sky. More precisely, the angle in the plane of the ecliptic, usually obtained directly by an instrument measuring the angle between a star of known longitude and Mars. The

unobservable, theoretical explanatory apparatus is nevertheless substantively different. As we have seen in detail, Ptolemy postulates circles, eccentrics, equant points and uniform theoretical angular speeds, while Kepler uses ellipses (first law) and neither linear nor angular constant real speeds, but, in any case, a speed determined by the conservation of the area sweep by the line joining the planet and the Sun in equal times (second law). Is there nevertheless theoretical content in Ptolemy retained by Kepler, even approximately, sufficient for the relevant predictions?

Before starting with the astronomical analysis, it is worth facing two possible objections that, if correct, would undermine the significance of our case study.⁹ First, we have just assumed that “apparent” angular positions in the sky are observable, but “real” geo-epicyclical or helio-elliptical are unobservable in a sense relevant for the realist debate. Somebody, though, could object to this and claim that everything in both systems, i.e. seeming as well as real trajectories, is observable. Of course, if this were the case, we would accept that our case study would be irrelevant to selective retentive realism. Nevertheless, we do not think this position is tenable. Of course, one may take that there is a sense in which the epicycle-deferent movements are observable, for if planets move in this manner what we see in the sky are the planets moving in this manner. But this is one thing, and quite another thing is to claim that *what we see* is *that* planets move in this manner. In the same vein, one could say that in a sense when we see bodies up and down in a pan balance we observe bodies being heavier than others, but this does not mean that when we look at the balance we *visually-see that* one body is heavier than other. In any event, we are not alone here, discussants of the Ptolemaic case in the realist debate also assume that the postulated “real” trajectories count as non-observable, theoretical, content (Niiniluoto 1999; Wray 2018).

The second objection sustains that Ptolemaic astronomy was not taken by Ptolemy himself, or other relevant posterior astronomers, realistically as a physical description of the heavens, and that his system was rather generally considered merely as a mathematical instrument “to save the phenomena”; and, the objection continues, if this is so then this case poses no threat for (retentive) realism. We do not think this objection works either. First, this Duhemian view on Ptolemy’s astronomy has been very influential, but more recent historiography has convincingly argued against it (cf. e.g. Loyd 1978; Musgrave 1991). In fact, there are good reasons to believe that Ptolemy had a realist stance about his system. For instance, in the *Almagest* (247) he uses his lunar model to account for lunar longitudes against the fixed stars, and the epicycle of the model implies that Moon’s real, theoretical distance to the Earth changes, change in distance that Ptolemy takes realistically and tries to square with parallax observations. Or, in his *Planetary Hypotheses* (Goldstein 1967), he clearly interprets the circles in his planetary models as physically real, for following the physical principles that nothing in nature is void of purpose, and that planets’ trajectories cannot overlap, he imposes the constraint that the farthest distance one body can reach is the closest the next one has. Also, in the *Almagest* he devotes an entire paragraph to a philosophical justification of the complexity of his theory of planetary latitudes, (600–601), claiming that the heavens are simple but their simplicity should not be equated to our ideas of simplicity. These three examples, and others, make little sense if it were true that Ptolemy did not take his model of the heavens realistically but rather just as a mathematical device for observable predictions. Ptolemy’s attitude towards his astronomical system can hardly qualify as instrumentalist.

⁹ We thank two anonymous reviewers for these comments.

But second, and more importantly, we believe that this is not what really matters. Be the dominant interpretation of Ptolemaic astronomy as it may, *if* we accept that the theory, or the model for Mars in particular, makes novel, risky and successful predictions—and we think we have provided uncontroversial evidence that this is so—, then a NMA-realist is committed to accept that what fuels such predictions is approximately true and (if she is of the retentivist kind we focus on here) approximately retained by posterior superseding theories that preserve the predictions. This is so *independently* of the specific historical interpretation that the theory has received. The NMA-realist is committed to the motto “(non-miraculous) empirical novel success implies approximate truth”, there is no conditional clause of the kind “only if the theory was taken realistically by its creators/commentators”. It is true that the realist may accept that she has no realist interpretation for the success of a particular theory and therefore accept that it is a miraculous case. And maybe the realist is ready to accept that this is so in our case study (as we showed above, the “Ptolemaic astronomy is not a mature science” strategy does not work). We should then assess how this affects the tenability of her *general* NMA-realist position (is one counter-example enough problematic?, more than one is needed?, how many would be fatal?). But without an additional argument that, as far as we know, has not been yet provided, nothing of this hinges on how was the theory interpreted by its users.

Let us start now with the specific theoretical analysis. We begin by considering some possible realist strategies for defending the existence of relevant preservation in this case by appealing to very general features. A first general option could be to claim that there obviously is something not directly observable and sufficient for the prediction that is approximately preserved, namely the *real relative positions* of Earth, Mars, Sun and the fixed stars, which seen by an observer on Earth produce the apparent observable movements. Yet, first, it is not clear that these real relative positions are something astronomical-theoretical in a substantive sense rather than, for instance, merely optical-theoretical. But, secondly and more important,¹⁰ these real relative positions, even if considered astronomical-theoretical in some substantive sense, would be of little help for the realist, for real relative positions are just implied by the observations (given a background common geometry). According to this proposal, the Ptolemaic Tr and its associate retentive claim would read thus: “The theoretical content responsible in the Ptolemaic system of the relevant prediction about Mars is the real relative positions between Mars, Earth, Sun and the fixed stars, and our historical (meta-)prediction is that these relative positions are going to be approximately preserved”. Since these real positions are implied by apparent trajectories observed by an observer on Earth, it is a priori, infallible, that they are going to be (approximately) retained by any posterior theory that makes (approximately) the same observable predictions. We do not think this serves to realist’s goals.

Another general option might be to claim that there is no doubt that there is something that is retained and not implied by the common observations, namely the geometrical structure of the space–time manifold, undoubtedly shared by Ptolemy and Kepler.¹¹ True, but insufficient. If this is the proposed Ptolemaic Tr, and leaving aside that this is dubiously substantially *astronomical*, there is no doubt that it is not *sufficient* to fuel the relevant prediction, the first condition that Tr must satisfy for it to serve NMA aims.

¹⁰ We follow here Carman and Díez (2015), who entertain a similar objection with respect to their own examples.

¹¹ We thank an anonymous reviewer for this comment.

A more astronomically specific, but still general, consideration could be to argue that, actually, a circle is an ellipse with zero eccentricity, and when the eccentricity is small, the ellipse approximates a circle. Kepler's ellipses would then approximately retain Ptolemy's circles, which would suffice for explaining Ptolemy's success. We do not think so. The Ptolemaic explanatory apparatus is not a single circle, but a complex system consisting in a deferent, an epicycle and a motion combination of speeds and centers of motion by eccentrics and equants. We do not think there is a non- ad hoc sense in which this theoretical system/mechanism as postulated of the real world is *on overall* an approximation of Kepler's theoretical ellipses (Kepler's explanation of Mars needs two ellipses, Mars' and Earth's). Below we will check whether this proposal, combined with other about more specific theoretical posits, may be effective to retentive realist goals.

This last consideration makes it worthy to emphasize the following warning: given two elements x in Ptolemy and y in Kepler, one cannot adduce that y retains x *based exclusively* on the fact that Ptolemy uses x in predicting P , Kepler uses y in predicting (an improved version of) P , and there exist some kind of transformation from x to y . For instance, we know that a Fourier series is able to embed any closed, periodical trajectory. Suppose now that there had been an ancient astronomer (with modern mathematical skills), say Pfourier, that postulated that the planets (actually any celestial body) "really" are (meta)/physically driven by a specific Fourier-series kind of "action" or "force" (divine, natural, or with whatever metaphysical nature you like). Obviously, one could explain Pfourier's observational success from Kepler's astronomical system viewpoint, using some transformations between Fourier series and the Keplerian system. *But*, and this is our warning, this *alone* would *not* justify the claim that Pfourier's astronomical series are approximately retained by Kepler, neither in the metaphysical sense, *nor* in the structural sense either: although Kepler's elliptical orbits are easily "transformable" from/to Fourier series, their mathematical structure substantially differ.

Let us now check more specific possible options. Since these general strategies for finding a global retention between Ptolemy and Kepler do not then seem promising, if there is something retained that serves realist goals, it must be something more detailed that calls for a more piecemealing scrutiny. In this vein, when one inspects both theories' predictive tools for this prediction, the following features seem to be (almost exactly) preserved:

- The values of the Keplerian heliocentric mean motions of Mars (Λ_m) and the Earth (Λ_e) are retained, but now (a) Λ_m is the mean motion of the center of the epicycle and (b) Λ_e is the mean motion of Mars around the epicycle.

- The values of the mean radii of the elliptical orbits of Mars (rm) and of the Earth (re) are retained, but now (c) rm is the radius of the circular deferent and (d) re is the radius of the circular epicycle.

- In Kepler, the apparent/observed motion of Mars is not uniform seen from the Sun. This is due to two factors: (e) because the motion is physically not uniform in itself as ruled by the second law (i.e. its vectorial speed changes), and (f) because of the optical effect given the particular location of the observer in Mars's orbit (with the Sun in one of the foci). In the same way, in Ptolemy, the apparent motion of the center of the epicycle is not uniform seen from the Earth. This is due to two factors: (g) because the motion is not uniform due to equant rule, and (h) because of the particular location of the observer in Mars' deferent (the Earth is ecentered). Now, the distance between the Earth and the center of the deferent is (proportionally) the same as the distance between the Sun and the center of Mars' elliptical orbit, meaning that the effect is explained exactly in the same way by both theories. If a planet moves according to the second law around the Sun, the empty focus works approximately as an equant point.

Let us check these retentions in turn to see whether there can be interpreted as preservation of “natures” (“metaphysical” features), or at least as preservation of “structures” (“structural” features),¹² and combined all together they suffice for accounting for Ptolemy’s successful prediction.

- Mars’ deferent speed in Ptolemy equals Mars’ mean speed in Kepler. This is due to the fact that Mars’ deferent speed in Mars accounts for the observed Mars’s mean orbital speed. Nevertheless, this is not strictly implied by observations, for observations might be squared by other (more complicated) mechanism. So, this could count as a theoretical preserved fact, with some approximations given the difference between the constant speed of a circular movement and the mean speed of a non-constant elliptic movement.

- Mars’s epicycle speed in Ptolemy equals Earth’s mean speed in Kepler. Here, despite the fact that the numerical value is the same, which of course has some function in explaining Ptolemy’s success from Keplerian perspective, we do not see in which sense there is some theoretical fact, either metaphysical or merely structural,¹³ that is the same, or approximately the same, in both theories. Contrary to the previous feature, here such nearly identical values correspond to speeds of *different planets*. To defend that, although this is not a metaphysical preservation it must count as structural preservation, is according to us being too flexible in the notion of (astronomic) structure for the very objects with the same value are different. If both systems attribute the same value of a given property, say speed, to the same object, but as kinematical effects of dynamical actions of different metaphysical nature (say divine in one case and natural in the other, or corpuscular in one case and field-like in the other, or...) system, we agree that this cannot count as metaphysical preservation but that it might count as structural preservation. But that if the objects with the same magnitude are different, Mars in one case and the Earth in the other, we do not think a structure is preserved. There is no unique understanding of “structure” in the structuralist family (Frigg and Votsis 2011), sometimes it refers to mathematical equations, other times to the Ramsey sentence, still other times somehow unspecifically just to “relational” facts. To us, none of these senses of “structure” makes Mars’ speed around the center of its epicycle (which orbits around the Earth) being similar to Earth’s mean elliptical speed around the Sun, a case of (approximate) identity of structures. The only sense of “structure” that could perhaps be claimed that is retained here is the above commented “real relative positions (and velocities) of Mars, Earth, Sun and fixed stars”, but we already saw that it does not serve realist goals. In the absence of a valid already existing notion, the realist must provide in this case a clear, new and non ad-hoc, notion of structure with her desired consequence. Meanwhile the burden of the proof lies on her side.

- Mars and Earth mean radius ratio in Kepler equals Mars’ deferent and epicycle radius ratio in Ptolemy (c and d). As before, there is a common value with some function in the explanation of past success, but we do not see a sense in which the same theoretical fact, natural or structural, is preserved, for, again, the same value does not correspond to the same objects.

¹² We follow the standard nature vs structure way of contrasting standard versus structural (epistemic) realisms (see e.g. Frigg and Votsis 2011 comprehensive review).

¹³ By ‘metaphysical’ we mean “concerning the nature of things”, and with ‘structural’, “concerning its structure”, following the nature/structure divide between standard and structural realists. So when we use ‘metaphysical’ below we refer to something that is in the theoretical content of the theory, not to anything transcending to, or underdetermined by, theory’s content.

- The distance from the observer on Earth to the center of Mars' orbit is the same in both cases. This is preserved, and not implied by observations since again a different and much more complex spatial distribution could also observationally work. So, we have other candidate for theoretical preservation.

- The distance between the Earth and the center of the deferent is (proportionally) the same as the distance between the Sun and the center of Mars elliptical orbit; and, if a planet moves according to the second law from the Sun, the empty focus works kinematically approximately as an equant point. True. But note, first, that the identical distances are again between different objects. And, second, in Ptolemy the angular speed is regular with respect to the equant, but while in Kepler the angular speed of the empty focus is approximately constant, what is strictly constant is the areal speed with respect to the focus occupied by the Sun. We do not see then which theoretical explanatory mechanism would be preserved here (though of course the closer the foci the more similar angular and areal speeds are).

Summing up, we have some theoretical elements clearly (approximately) preserved, *but* it is uncontroversial that they alone are by no means sufficient. If these elements are all what is preserved from Ptolemy to Kepler, this by no means suffices for fueling Ptolemaic correct observable predictions regarding Mars' longitude evolution around its orbit, as the realist would need. The provisional conclusion then is that, though some part of the theoretical machinery is preserved, it is by no means sufficient for fueling the predictions in point, which undermines the NMA basis of SSR in this case. If this is all, we seem to have a case in which one is able to account in extremely detail former predictive success without *enough* theoretical retention.

The realist might complain that in this last strategy we payed attention only to elements (distances and speeds, for example) almost exactly preserved and considered one by one, and isolated from each other. Let us conclude with a "mixt" strategy, one that consists in defending that a combination of elements in Ptolemy is on overall at least structurally retained in, or structurally enough similar to, Kepler. In order to give to this strategy the highest chances, let us present it by imagining, following the steps in the previous section, a succession of small theoretical transitions from Ptolemy to Kepler, made if you want by a succession of different theories proposed by a succession of astronomers between Ptolemy and Kepler:

- (i) Suppose that, after Ptolemy, an astronomer named Ptolomer decided to modify Ptolemy's model for Mars introducing an eccentricity and equant point in the epicycle (step 7, above), and consequently changing the direction of the apsidal line (step 8). The predictions of this new proposal are slightly better than Ptolemy's ones.
- (ii) Ptolomer had a naughty disciple, named Ptoplomer, who decided to invert the orbits in the case of the outer planets (but not in the inner ones), keeping all the mean motions, equant points and eccentricities untouched, but changing the epicycle and deferent model by a moving eccenter model. In this way, Mars revolves around the Sun (the Sun is located at the opposite side of the equant point), and the Sun, the equant point, and the center of Mars orbit revolve around the Earth (step 6 above). This proposal is similar to that of Tycho Brahe, but with eccentrics and equants. Ptoplomer's proposal is empirically equivalent with Ptolomer's theory.
- (iii) A new astronomer, named Ptopler, decides to put the Earth to revolve around the Sun, just as Mars already did, making the Sun fixed. This step is very similar to Copernicus's

- one (step 5, backwards). Just as Tycho and Copernicus are empirically equivalent, so Ptolomer and Ptopler are. The change is just a change in the system of reference.
- (iv) After Ptopler, a new astronomer, Ptepler, proposed a new theory that is exactly like Ptopler's but now with elliptical orbits. He keeps the Sun at the position at which it was in Ptopler's theory and the motion is uniform from the equant point, but now, the trajectory followed by each planet, including Mars (step 2 above) and the Earth (step 4) is elliptical instead of circular. The semi-major axes of Ptepler's orbits are equal to the radii of Ptopler's orbits. Ptopler's and Ptepler's theories are not empirically equivalent, but the difference is really small.
 - (v) Finally, a German astronomer, named Kepler replaced the equant point by the second law applied from the focus at which the Sun is located. The second law is applied to all the planets, including Mars (step 1) and the Earth (step 3). Kepler's and Ptepler's theory are not observational equivalent, but the difference is again pretty small.

To conclude, let us assess this succession of theoretical transits (we label now these theories by their author's names):

- We agree that Ptolemy and Ptolomer are approx. empirically equivalent and also that the former is on overall approximately metaphysically preserved by the latter. Changes are metaphysically inert, just a matter of approximation.

- What about Ptolomer? The situation here seems different. Ptolomer and Ptolomer are empirically equivalent, but it is far from clear that the former is (approx.) theoretically preserved by the latter in all the aspects relevant for the prediction. First, outer planets, Mars included, invert the orbit. Second, Mars changes from orbiting around a point (which is not the Sun) that orbits around Earth, to orbiting around the Sun. We think it is clear that there is no astronomical preservation here, the "nature" of the astronomical system is not the same. The two systems substantially or radically differ in ways they claim the world is. The realist might perhaps argue that there is at least structural preservation, but is this so? Is the world structurally the same if Mars orbits around the Sun instead of around of a point (different from the Sun) that orbits around the Earth? This depends on how we understand "structure" here, but the mathematical equations have dramatically changed, the corresponding Ramsey sentences as well, and in general the relations between objects have also changed. If the structural realist defends structural preservation here, she needs to specify a clear and non-ad-hoc notion of structure that does not require at least preservation of relations. We cannot prove there cannot exist such a notion, but meanwhile the structural realist does not provide it this case remains a challenge and the burden of the proof is on her side.¹⁴

- As for the step from Ptolomer to Ptopler, though they are again empirically equivalent, we take it that there is an essential theoretical non-preservation since (as between Tycho and Copernicus) in one case the Earth is metaphysically at rest and in the other the Sun is. Again, whether there nevertheless is structural preservation depends on the reading

¹⁴ A reviewer objects (if we understand well) that we are inverting the burden of the proof and that it is us who have to prove that there is nothing structural retained. We think we have done it *to the extent one can do it*. We have shown that the existing notions of structure that are minimally precise (mathematical equations, Ramsey sentence) do not work. A totally imprecise notion of "structure" or "something structural" is impossible to assess with respect to its alleged retention in this case. The structural realist has not given any other precise notion we could check. Our conclusion is *not* that there does not exist a notion that works here, but simply that until the structuralist does not provide it the burden of the proof lies on her side (and we cannot be guilty of not being able to come out with other, still precise notion of "structure" that fit better this case).

of “structural”, but if the only difference here is “where we fix the center of reference”, it may be argued that there is no structurally relevant difference.

- Next step is from Ptolemy to Ptolemy, in which the latter is a (better) empirical approximation of the former. And we accept that, modulo approximation, there also is metaphysical retention as well, to the extent that circles may be considered metaphysical approximations of ellipses. But if one denies this, we believe it would be plausible to claim that there is at least approximate structural preservation here.

- We arrive to the final step, from Ptolemy to Kepler, empirically equivalents under approximation. Is there theoretical retention in the relevant aspects? The answer depends on whether that kind of change in a law may count as theoretical retention. We do not know what to say at the metaphysical level, for it depends on the metaphysics of laws in which we cannot enter here, but at least it looks like a structural preservation since the old law approximates the structural mathematical relation of the former.

Summarizing this sequence of theoretical changes: combining all the steps, there clearly is an overall approximative empirical retention, but without enough metaphysical retention. As for structural retention, the variety of notions of “structure” already in play do not guarantee a preservation of structure in at least one crucial step. This means that this case constitutes a *prima facie* anomaly for retentive, selective realists, in both the metaphysical and the structural versions. Perhaps the structural realist may coin a new notion of “structure” that fixes this anomaly, but meanwhile the burden of the proof is on her side.

To conclude giving to the realist the best chances, let us see whether Vickers weak retentivism gives better prospects to the realist in this case. Vickers (2017) accepts that realists must be committed to some kind of retention, and that the part to be retained must be identified prospectively, not a posteriori, but he also claims that the propositions claimed to be approximately true and thus predicted to be retained, need not be exactly the ones actually used by the scientist in deriving the relevant successful prediction. The idea is that a scientist may actually use a proposition Q to derive the prediction P, but the derivation takes place not *in virtue of* Q but *in virtue of* a different, weaker proposition W implied by Q (and not implying Q). He mentions as an example Bohr’s prediction of the frequencies of the spectral lines of ionised helium. Although he used (together with other propositions) the proposition QB “The electron orbits the nucleus at specific, quantised energies, corresponding to only certain ‘allowed’ orbital trajectories”, the prediction obtained is not in virtue of QB but in virtue of the weaker WB “The electron can only occupy certain, specific, quantised energy states within the atom”, and it is the approximate truth of this latter proposition, and not of the former, to which the realist should be committed, and to its approximate retention thereof. According to Vickers, this happens some/many times in science, which allows the realist to overcome many/all? historical PMI-counterexamples to selective realism. May this strategy help the realist in our case study? We do not think so. In the two crucial steps, from Ptolemy and Ptolemy and from Ptolemy to Ptolemy, the only propositions, weaker than the actually used, still theoretical and still fueling the prediction, that we can figure out are propositions about the relative real positions and velocities. Though actually retained, as we argued above they nevertheless can hardly serve the realist for her retentive claim, for these preserved relative positions are (given a common background topology and optics) implied by the observations, thus the retention being a priori/infallible. Maybe there are other propositions we were not smart enough to identify that are weaker than the ones actually used and stronger than the ones about relative positions and velocities, and that were actually retained in this theoretical transit, but until the realist tells us which they are, the burden of the proof lies on her side.

4 Conclusions

Selective, retentive scientific realism is the most plausible realist position in the market. According to it, a theory's empirical success is explained by its relevant theoretical parts, i.e. the ones that fuel the successful predictions, being approximately true. Thus, past theories' successful predictions also made by posterior theories, are explained by the approximate truth of past theories' relevant theoretical parts, which thereby must be approximately preserved by the posterior theories. Past empirical success can then be explained *only* by the parts of past theories responsible of the successful predictions being theoretical approximations of the parts of posterior theories responsible of the same successful predictions. In our case study, we have presented an explanation of Ptolemy's successful prediction of Mars' longitudes from Kepler's perspective, and scrutinize whether all the theoretical elements responsible for Ptolemy's empirical success are approximately retained in Kepler. In order to give to the realist the best chances, we have tried different strategies, all failing. The final diagnosis is that: (a) there clearly is not enough theoretical preservation at the metaphysical level; (b) current notions of "structure" fail to show that there is enough structural preservation. The structural realist may claim that there is a different notion of structure of application here that provides the relevant sufficient retention. We have seen that the option of real relative positions and velocities (retained, but implied by the prediction) does not work for realist goals, and until she specifies a new, plausible and non ad-hoc notion of structure that works for this case, it is our claim that the historical episode discussed here poses a *prima facie* strong anomaly for selective retentive realism, which we take to be the best version of scientific realism at hand. If our historical analysis is correct, it is possible to understand, from the perspective of the posterior theory, the past theory's empirical success without the posterior theory approximately retaining the theoretical parts of the previous theory that fuel the novel successful predictions.

Funding Open Access funding provided thanks to the CRUE-CSIC agreement with Springer Nature. This study was funded by the Agencia Nacional de Promoción de Científica y Tecnológica of Argentina (research project PICT-2016-4487) and the Ministerio de Ciencia e Innovación of Spain (research projects FFI2016-76799-P, PID2020-115114 GB-I00).

Declarations

Conflict of interest The authors declare that they do not have any conflict of interest.

Ethical approval This article does not contain any studies with human participants or animals performed by any of the authors.

Open Access This article is licensed under a Creative Commons Attribution 4.0 International License, which permits use, sharing, adaptation, distribution and reproduction in any medium or format, as long as you give appropriate credit to the original author(s) and the source, provide a link to the Creative Commons licence, and indicate if changes were made. The images or other third party material in this article are included in the article's Creative Commons licence, unless indicated otherwise in a credit line to the material. If material is not included in the article's Creative Commons licence and your intended use is not permitted by statutory regulation or exceeds the permitted use, you will need to obtain permission directly from the copyright holder. To view a copy of this licence, visit <http://creativecommons.org/licenses/by/4.0/>.

References

- Alai, M. 2016. Resisting the Historical Objections to Realism: Is Doppelt's a Viable Solution? *Synthese* 124: 1–24.
- Carman, C. 2015. The Planetary Increase of Brightness During Retrograde Motion: An Explanandum Constructed ad Explanantem. *Studies in History and Philosophy of Science Part A* 54: 90–101.
- Carman, C., and J. Díez. 2015. Did Ptolemy make novel predictions? *Launching Geocentrism to the Scientific Realism Debate, Studies in History and Philosophy of Science Part A* 52: 20–34.
- Carman, C.C., and G.L. Recio. 2019. Ptolemaic planetary models and Kepler's laws. *Archive for History of Exact Sciences* 73: 39–124.
- Díez, J. 2018. A (Fatal) Trilemma for Best Theory Realism. *European Journal for Philosophy of Science* 8 (2): 271–291.
- Doppelt, G. 2011. From Standard Scientific Realism and Structural Realism to Best Current Theory Realism. *Journal for General Philosophy of Science* 42: 295–316.
- Doppelt, G. 2014. Best theory scientific realism. *European Journal for Philosophy of Science* 4: 271–291.
- French, S. 2006. Structure as a Weapon of the Realist. *Proceedings of the Aristotelian Society* 106 (1): 170–187. <https://doi.org/10.1111/j.1467-9264.2006.00143.x>.
- French, S. 2020. Resistance is futile! *Metascience* 29: 5–10.
- Frigg, R., and I. Votsis. 2011. Everything you always wanted to know about structural realism but were afraid to ask. *European Journal for Philosophy of Science* 1: 227–276.
- Goldstein, B. 1967. The Arabic Version of Ptolemy's Planetary Hypothesis. *Transactions of the American Philosophical Society*. <https://doi.org/10.2307/1006040>.
- Kepler, J. (2015). *Astronomia Nova* (W. H. Donahue, Trans.) Sante Fe: Green Lion Press.
- Ladyman, J. 1998. What is Structural Realism. *Studies in History and Philosophy of Science* 29: 409–424.
- Laudan, L. 1981. A Confutation of Convergent Realism. *Philosophy of Science* 48: 19–48.
- Lloyd, G.E.R. 1978. Saving the Appearances. *The Classical Quarterly, New Series* 28 (1): 202–222.
- Lyons, T. 2006. Scientific Realism and the Stratagema de Divide et Impera. *The British Journal for the Philosophy of Science* 57 (3): 537–560.
- Mizrahi, M. 2013. The argument from underconsideration and relative realism. *International Studies in the Philosophy of Science* 27: 393–407.
- Musgrave, A. 1991. "The Myth of Astronomical Instrumentalism". In *Beyond Reason: Essays on the Philosophy of P. Feyerabend*, ed. Munevar, G., 243–280. Dordrecht: Kluwer Academia Publishers.
- Niiniluoto, I. 1999. *Critical Scientific Realism*. London: Oxford University Press.
- Peters, D. 2014. What elements of successful scientific theories are the correct targets for "selective" scientific realism? *Philosophy of Science* 81 (3): 377–397.
- Psillos, S. 1999. *Scientific Realism: How Science Tracks Truth*. London: Routledge.
- Psillos, S. 2020. Resisting scientific anti-realism. *Metascience* 29: 17–24.
- Ptolemy, C. (1984). *Almagest*. In G. Toomer, *Ptolemy's Almagest* (G. Toomer, Trans., 27–659). Princeton: Princeton University Press.
- Rowbottom, D. 2019. Scientific realism: What it is, the contemporary debate, new directions. *Synthese* 196: 451–484.
- Saatsi, J. 2015. Historical inductions, old and new. *Synthese* 196: 3979–3993.
- Stanford, P.K. 2006. *Exceeding our grasp*. Oxford: Oxford University Press.
- Van Fraassen, B. 1980. *The Scientific Image*. Oxford: Clarendon.
- Vickers, P. 2017. Understanding the Selective Realist Defence Against the PMI. *Synthese* 194: 3221–3232.
- Vickers, P. 2019. Towards a realistic success-to-truth inference for scientific realism. *Synthese* 196: 571–585.
- Vickers, P. 2020. Resisting scientific anti-realism. *Metascience* 29: 11–16.
- Vickers, P. 2013. A Confrontation of Convergent Realism. *Philosophy of Science* 80 (2): 189–211.
- Votsis, I. 2011. The Prospective Stance in Realism. *Philosophy of Science* 78 (5): 1223–1234.
- Worrall, J. 1989. Structural Realism: The Best of Both Worlds? *Dialectica* 43: 99–124.
- Wray, K.B. 2018. *Resisting Scientific Realism*. London: Cambridge University Press.