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Scientific Realism and the Stratagema de Divide et Impera

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ABSTRACT

In response to historical challenges, advocates of a sophisticated variant of scientific realism emphasize that theoretical systems can be divided into numerous constituents. Setting aside any epistemic commitment to the systems themselves, they maintain that we can justifiably believe those specific constituents that are deployed in key successful predictions. Stathis Psillos articulates an explicit criterion for discerning exactly which theoretical constituents qualify. I critique Psillos's criterion in detail. I then test the more general deployment realist intuition against a set of well-known historical cases, whose significance has, I contend, been overlooked. I conclude that this sophisticated form of realism remains threatened by the historical argument that prompted it.

1 A criterion for scientific realism

Scientific realists offer a hypothesis about scientific theories, which, they propose, should itself be treated as a scientific theory. In its basic formulation, the hypothesis is that our successful scientific theories are (approximately) true. In this hypothesis (approximate) truth is being attributed not only to a theory's claims about observables, but also, and quite crucially for the scientific realist, to the theory's claims about unobservables. Readily granting that we cannot conclusively establish the hypothesis, the realist claims that we are at least justified in believing it, given what is commonly called 'the no-miracles argument': It would be a miracle were our successful theories not (at least, approximately) true; the (approximate) truth of T provides the only (or at least the best) explanation for T's success. However, testing the realist hypothesis against the historical record, it looks as though numerous theories have been successful but cannot, as wholes, be approximately true (Laudan,

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[1981]). The basic realist hypothesis appears significantly falsified. And, unable to appeal to approximate truth to explain these successes, the realist appears to be faced with a list of inexplicable successes—or, taking her argument literally, 'miracles'. In defense, realists have modified their hypothesis. Here, I focus on the recent defense against this historical argument that appears to be most strongly favored by scientific realists.

The realist notes that theoretical systems can be divided into numerous constituents. She also notes that, while certain past successful systems, taken as wholes, may fail to be approximately true (by present lights), certain postulates contained within them have nonetheless been retained. Accordingly, the realist revises her hypothesis to focus on particular theoretical constituents. Of course, to be a realist about any actual constituents, one must specify particular *conditions* for identifying those constituents that are deserving of belief. Philip Kitcher ([1993]) and Stathis Psillos ([1999]) contend that we can justifiably believe those, and only those, constituents that are deserving of credit for the significant successes of the theory. Kitcher says, we must 'distinguish between those parts of theory that are genuinely used in the success and those that are idle wheels' (p. 143, footnote 22). Proposing that we 'really focus on the specific successes of certain theories' (p. 109), Psillos wants to identify those constituents that are 'responsible' (p. 108) for a given successful prediction, 'those which "really fuel the derivation"' (p. 110). He characterizes this step as the '*divide et impera* move' (p. 108). 'Realists', he contends, 'need care only about those constituents which contribute to successes and which can, therefore, be used to account for successes' (p. 110). The no-miracles argument then takes the following form: a theory's success would be miraculous if those constituents that had a genuine bearing on its successful predictions were not (at least approximately) true. That given, says the realist, we are justified in believing the hypothesis that those constituents are (approximately) true. And if, say, the proposition 'the ether exists' (Kitcher [1993]) was not '*deployed* in'—that is, if it were not among the constituents responsible for—successful predictions, it is not deserving of credit for those predictions. And despite its falsity, it would not stand as a counterexample (or an apparent 'miracle'). The historical argument against realism is thought to be deflected. Taking a cue from Kitcher, we can call this sophisticated position *deployment realism*.

Other philosophers who advocate this position are Jarrett Leplin ([1997]), Ilkka Niiniluoto ([1999]), and Howard Sankey ([2001]). However, no one of these philosophers has articulated and attempted to apply the deployment realist's intuition to the extent that Psillos has ([1999]). For this reason it is crucial to address his articulation (references to his [1999]). Psillos proposes that our criterion for deployment realism be the 'essentiality' of a constituent in bringing about a given successful prediction. 'Theoretical constituents which make essential contributions to successes are those that have an indispensable role in their generation' (p.110). He asks, 'When does a theoretical constituent H indispensably contribute to the generation of, say, a successful prediction?' (p. 110). He answers:

> Suppose that H together with another set of hypotheses H′ (and some auxiliaries A) entail a prediction P. H indispensably contributes to the generation of P if H′ and A alone cannot yield P and no other available hypothesis H* which is consistent with H′ and A can replace H without loss in the relevant derivation of P. (p. 110)

Anticipating the objection that we can always replace a given hypothesis, Psillos elaborates his criterion further:

> Clearly there are senses in which all theoretical assertions are eliminable, if, for instance, we take the Craig-transform of a theory, or if we 'cook up' a hypothesis H* by writing P into it. But if we impose some natural epistemic constraints on the potential replacement—if, for instance, we require that the replacement be independently motivated, non ad hoc, potentially explanatory, etc.—then it is not certain at all that a suitable replacement theory can always be found. (p. 110)

We can schematize Psillos's criterion in the following way. For H to be essential

1. It must be the case that H + H′ + A leads to P.
2. It must *not* be the case that H′ + A, alone, leads to P.
3. It must *not* be the case that any alternative, H*, is available

   a that is consistent with H′ + A and
   b that when conjoined to H′ + A leads to P and
   c that is non-*ad hoc* (which for Psillos means, among other things, that it does not use the data predicted by P (p. 106)), that is potentially explanatory, etc.

I will now address the adequacy of this criterion and contend that it is not suited to its task(s). While the deployment realist's fundamental insight will survive my critique of Psillos's specific criterion, that insight will be the focus of Sections 3–5.

2 Assessing the criterion

To assess the adequacy of Psillos's criterion, we must first unambiguously identify its purpose, goal, or motivation. It is natural to think that Psillos seeks to determine which constituents are 'essential', which have 'an indispensable role' in the 'generation' of a prediction (p. 110). However, this only pushes the question back. If we take 'H is essential/indispensable' as these terms are commonly understood—i.e. to mean that, without the specific hypothesis H, P cannot be derived—then *no* constituent can qualify. For it is well known and easy to show (as Psillos grants in the above quotation) that there will always be other hypotheses, albeit some that we find very unappealing, from which any given prediction can be derived. (We can create innumerable variants of the original hypothesis/theory, modify a competing hypothesis/theory to accommodate the prediction, etc.) That given, Psillos's criterion is not properly seen as an attempt to determine the *essentiality* of a specific hypothesis where 'essentiality' is taken to mean that, without the specific hypothesis, the relevant prediction cannot be derived. And we must avoid the strong temptation to suppose we are discussing this common understanding of essentiality/indispensability.[1] We recognize, instead, that Psillos's criterion provides a novel *definition* of 'essentiality'. Acknowledging that any number of conditions could be invoked to do this, we are pushed to uncover Psillos's motivation for those specific conditions he includes in his definition/criterion of 'essentiality'.

In the paragraphs prior to and following his criterion, Psillos tells us which constituents are deserving of credit: those that 'contribute' to a given successful prediction, 'those which "really fuel the derivation"' (p. 110). He tells us he wants to discern 'how' certain successes were 'brought about' (p. 109). This suggests that his criterion is *intended* to be a criterion for attributing credit (by way of which we have a criterion for what we can justifiably believe). If so, however, Psillos's criterion unacceptably overshoots its goal. Owing to condition 3, which pertains solely to the possibility of a competing/replacement hypothesis, his criterion could exclude any number of constituents that have genuinely *contributed* to a given derivation, and which, according to him, are thus deserving of credit. Whether or not H can be replaced by another available hypothesis, H*, whether H* is consistent with $H' + A$, whether H* is non-*ad hoc*, etc.—these issues have no bearing whatsoever on whether H itself *contributed* to, was deployed in, the derivation of a given prediction. They are irrelevant to the question of how H connects to the prediction, thus to the question of 'how' a given successful prediction was in fact 'brought about' (p. 109). If our goal is, as Psillos's words very strongly imply, to give credit

1 The temptation is so strong that Psillos himself appears to succumb to it, equivocating on 'essentiality' across different chapters. In his case study on ether theories he is primarily concerned to identify which theoretical posits were and were not *retained* in later theories. That certain constituents were abandoned is evidence that they lacked essentiality, in its common understanding, i.e. 'without the specific hypothesis H, P cannot be derived'. But even those that have been retained will fail to be essential in this sense. Nor does a lack of retention do anything to demonstrate a failure to fit (any part of) his criterion. This is another issue altogether, and, to the detriment of his argument, one he does not address—aside from providing evidence for the sociological thesis that some scientists expressed an epistemically non-committal attitude toward ether theories. (Psillos's selectivity with respect to such evidence is the focus of Stanford's [2003] critique.)

where credit is due, condition 3 is superfluous, unmotivated, and therefore inappropriate. His criterion as it stands is thus unacceptable as a criterion for credit attribution. That given, Psillos *cannot* be understood as claiming that we can and should believe those constituents, and only those constituents, that are deserving of credit. Significantly, then, by introducing his criterion, he has discarded the central idea of deployment realism—introduced by Kitcher and seemingly advocated by Psillos himself in the quotations above.

Recognizing this, while nonetheless seeking a charitable alternative interpretation of the purpose of Psillos's rigid criterion, we might take that criterion to be put forward to eliminate the possibility that H is only 'accidentally' deployed in its particular theoretical context, i.e. put forward to prevent commitment to an H that could be 'easily' replaced *while retaining its context*.[2] However, I contend that Psillos's criterion is irrelevant to such an end. The word 'available' in condition 3 presumably means 'available at a specified time, t (e.g. the time the prediction was made)'. One problem, of course, is that we may be incapable of showing that no scientists formulated or considered any qualifying competitors at t. Even setting that aside, it is clear and significant that the accidental/non-accidental status of H is not determined historically—that is, by the interests and endeavors of scientists at t. In fact, wishing to deny the availability of an 'easy' replacement, we cannot legitimately cite the fact that scientists at t did not come up with an H*. For, most notably, the situation at issue is the *success* of constituents rather than their failure. In the context of that success, scientists are not (and, many would contend, should not be) generating *any* individual competitors for the many successful individual H's at play, especially those that meet conditions 1 and 2. (And it is hardly the case that the scientific community tends to fail when dedicated to replacing specific constituents.) Being without an H* at t can itself be deemed little more than an historical accident; therefore, in itself, it can do nothing to indicate that H is non-accidental within its theoretical context.[3] Psillos's criterion has no relevance to such a goal.[4]

Psillos is *ultimately* asserting, in the face of the historical argument, that we can *justifiably believe* constituents to be (approximately) true if and only if they meet his rigid criterion (in respect to significant successes). And, given

[2] I am indebted to Richard Whal for suggesting this interpretation of the purpose of Psillos's criterion.

[3] Even taking 'available' to be temporally unrestricted—requiring that no H* *will ever* be available—the criterion must still be applied at a particular time t (see the next paragraph). And, given the points noted here, we would lack at t genuine evidence that our H meets this requirement. Hence, imposing a temporally unrestricted notion of 'available' onto the criterion would be (relevant but) futile toward the goal of informing us that H is non-accidental.

[4] One might desire to evade a timeless/non-contextual understanding of '*accidental*' by equating 'non-accidental' with the temporally restricted version of the criterion. However, this would render vacuous the claim that the purpose of the criterion is to eliminate accidental posits; the question of the motivation/purpose for the criterion would remain unanswered.

the historical argument, one is well advised to increase the rigidity of one's criterion for belief (which becomes in effect a criterion for scientific realism). Condition 3 achieves this. However, it is imperative that such a criterion be genuinely applicable, i.e. that it can be successfully implemented to pick out specific theoretical constituents. This is so for at least two reasons. First, and of course, in order to be realists about any constituents at all, we must at least be able to explicitly identify those constituents. Second, because the criterion is put forward as part of a scientific hypothesis that is supposed to answer the historical argument, that hypothesis must be testable. Shortly after presenting his criterion, Psillos offers a number of examples intending to show that deployment realism survives where basic realism fails. However, at no point does he show, or even attempt to show, that those retained constituents—in regard to which he recommends a realist attitude—meet his criterion. (Nor does he explicitly show that those toward which he espouses a non-realist attitude fail to meet his criterion.) In fact, after introducing his very specific set of conditions, he never mentions that set of conditions again.[5] One reason may be that his (very elaborate) criterion is simply inapplicable: it cannot be utilized to pick out any specific theoretical constituents. Condition 3 and its subconditions are excessively vague. As I have hinted at above, reference to the availability of competitors could pertain to any number of situations: 'when the theory is put forward', 'when the prediction is derived', 'when the prediction is confirmed', 'at some point in the future', and so on. Since the realist must appeal to a theory's success at a given time, a condition regarding competitors requires a specification of when those competitors can or cannot be available.[6] Nor could we apply his criterion without a clear specification of what 'potentially explanatory' and, quite notably, 'etc.' actually mean in 3c. We need to know the extent to which (or even whether) each of the elements of H' and A also need to be 'essential' by this definition and whether, given 3a, the replacement theory needs to be consistent with those elements of H' and A, which, on one hand, are 'essential' for other predictions but, which, on the other hand, are 'idle wheels' for the prediction of concern. We need to know whether, or to what degree, H^* can result in the loss of other confirmed predictions. As it stands, Psillos's criterion leaves us guessing on (at least) these points. Consequently, his criterion cannot be utilized to pick out any specific constituents. And testing his hypothesis—that constituents 'essential' for particular successes are retained—against history would be pointless, if not impossible. Most

[5] Unable to demonstrate these claims (and those in footnote 1) here—by, say, quoting the relevant chapters in full—I can only encourage the reader to test my claims against Psillos's text.

[6] And were he to provide the needed specification, he would be further pressed to epistemically motivate his choice over the alternative options just noted.

importantly, being unable to apply his criterion, we could not be realists about any given theoretical constituent. Finally, since there will always be some competitor, were we to employ 3 in our criterion without subconditions, no hypothesis would qualify for realism. We must discard 3 altogether in order to avoid eliminating the possibility of realism. As we have seen above, Psillos's criterion as presented is not acceptable as a criterion for credit or for eliminating 'accidentally' deployed constituents. We now see that it is also unacceptable as a criterion for belief.[7] Perhaps, it succeeds only in immunizing the realist hypothesis from refutation. To redirect Psillos's own quotation of Joseph Black: '*A nice adaptation of conditions will make almost any hypothesis agree with the phenomena.* This will please the imagination, but does not advance our knowledge' (quoted, p. 118). These words seem particularly relevant to condition 3.

3 A return to the crucial insight: responsibility

Deployment realism itself, however, need not be so immunized. Given the misleading ambiguity of 'essential' and the problems noted with condition 3 and its subconditions, I propose that we dispose of that term and those conditions altogether. Casting that baggage aside, let us isolate, and take seriously, the deployment realist's fundamental insight that credit should be attributed to those and only those constituents that were genuinely responsible for, that actually led scientists to, specific predictions. Doing so, we must resist imposing our ideals of the reasoning process. Credit will have to be attributed to all responsible constituents, including mere heuristics (such as mystical beliefs), weak analogies, mistaken calculations, logically invalid reasoning, etc.[8] Further, insofar as the no-miracles argument denies that accidental successes—those that are not validly derived from (approximately) true theories—occur, such contributors will stand as counterinstances to realism, no less than altogether false constituents that were employed in a relevant valid deduction.[9] Moreover, realism must avoid collapsing into its opposition. A non-realist may well hold that we need only attribute genuine credit to the empirical generalizations we obtain from multilevel theories. In contrast, a realism that takes this responsibility model seriously must attribute no less credit to those theoretical constituents that led scientists to the empirical generalizations (the latter of which may in turn have led to

[7] Another objection, which I leave to the side here, is that, without further justification, the subconditions in 3 are epistemically unmotivated.

[8] Awkward though it may appear, examples of the latter can be treated as 'constituents' and can be false (e.g. if a scientist asserting that 'A entails B' made an error in deriving B from A).

[9] We see here that, in this debate, to discount such counterinstances is to presuppose the no-miracles argument in the course of testing that argument and the realist hypothesis it is supposed to support.

successful predictions). In fact, when those generalizations provide us with abundantly more information than any specific predictions derived from them, the credit carried to the theoretical posits by those empirical generalizations should, it would seem, be far more robust than any credit the generalizations themselves receive from specific predictions. I suggest that any genuine attempt to attribute credit where it is due will have to embrace these facets of the responsibility model.

It is crucial to remain mindful of one further qualification that is often given a position of 'centrality' in the contemporary 'defence of realism' (Psillos [1999], p. 105): realists such as Psillos direct our attention to *novel* successes, specifically. There are generally thought to be two types of novelty. Temporal novelty denotes predictions that describe phenomena unknown to scientists at the time the prediction is derived. (Examples favored by realists would include special relativity's prediction of time dilation, taken to be confirmed by jets carrying atomic clocks, and general relativity's prediction that light bends around massive objects, taken to be confirmed by the Eddington expedition.) Use-novelty—a second, less demanding, and less rare type of novelty—denotes predictions that describe phenomena that may have been known but were not *used* in formulating the theory. By focusing on novel successes an element of wonder is thought to be ensured, giving the no-miracles argument its greatest degree of potency; and restricting *success* in this way is thought to further secure the elimination of potential counterinstances to realism.

With this sophisticated form of realism and the above clarifications in hand, I now turn to a few well-known historical cases, whose relevance to deployment realism, and scientific realism more generally, appear to be as yet overlooked. (Pre-reflectively, these cases may even be thought to accord quite well with the deployment realist hypothesis.) I will endeavor to isolate a set of constituents, show how these constituents were responsible for significant successes, make clear that these successes include novel successes, and indicate why, by present lights, the realist cannot take these deployed constituents to be even approximately true. Closing, I will seek to clarify the threat such counterinstances pose for deployment realism.[10]

[10] I suggest that, along with my above assessment of Psillos's explicit criterion, this set of goals distinguishes the present critique from two recent critiques of Psillos's constituent realism by Stanford ([2003]) and Chang ([2003]). The primary focus of Stanford's discussion is the ability to 'explain a wide range of phenomena' (p. 921) (despite some use of the phrase 'predictive success'); and Chang's stated concern is with 'explanatory successes' (p. 907) and the 'explanatory centrality' (p. 908) of posits. Whether or not these successes qualify as novel predictive successes is altogether unclear—a potentially serious problem given 'the centrality of novel predictions in [Psillos's] defence of realism' (Psillos [1999], p. 105). Further, these critiques contain little more than passing comments on just which and how particular constituents were deployed in specific successes and just why those constituents cannot qualify, by present lights,

4 A few case studies

Two constituents introduced in Kepler's early *Mysterium Cosmographicum* ([1596]; henceforth MC) that proved pivotal to his theorizing were

1. Planets move only when forced to move.[11]
2. The sun is unique and in fact supreme by its divinity (not its size); it is positioned at the center of world, etc.[12]

Toward the end of MC, Kepler sought to explain why (a) the planets are moved in paths around the sun and why (b) planets with a greater mean distance from the sun traverse their orbits at a slower pace than those closer to the sun. Crucial to his explanation for both was his general thesis of the *anima motrix* (moving spirit), toward which both 1 and 2 played a significant role:

3. The sun is that which pushes the planets in their orbits.
4. The sun emits rays that do the pushing (the *anima motrix*).[13]

While 1–4 explained (a), Kepler needed more to explain (b). He added 5, which rests on 1–4,

5. The sun's push on planets via the *anima motrix* decreases in proportion with the planets' mean distances from the sun.

He wrote, 'there is a single moving soul in the center of all the spheres, that is, in the Sun, and it impels each body more strongly in proportion to how near it is. In the more distant ones on account of their remoteness and the weakening of its power, it becomes faint, so to speak' ([1596], p. 199). Kepler assumed that 'motion is dispensed by the Sun in the same proportion as light' ([1596], p. 201). Holding, at this stage, that the intensity of light is

as approximately true. Hence, it remains unclear whether the examples discussed in the above texts pose a genuine threat to the sophisticated realism I am concerned to address here. Finally, both texts deem the historical argument a pessimistic *induction*, which, as I will suggest below, is an unnecessarily bold variant of the historical argument.

11 This is a special case of the more general thesis of forced motion. Kepler held that (magnetic influences aside) 'All matter tends to remain at rest where it is' ([1596], p. 171, author's note 5, Ch. 22, added in 2nd edition); there must be an efficient cause for an object's motion. Kepler concluded further that for planetary motion that cause is external.

12 The sun is that to which 'the life, the motion and the soul of the universe are assigned' and 'far excels all others in the beauty of his appearance and the effectiveness of his power, and the brilliance of his light. Consequently, the sun has a far better claim to such noble epithets as heart of the universe, king, emperor of the stars, visible God, and so on'. ([1596], p. 199–201). The sun's supremacy was apparently unrelated to its size, as for Kepler, 'bigness is of no special significance' (quoted in (Kozhamthadam [1993], p. 192), which contains a thorough discussion of Kepler's theory).

13 Kepler merged his Neoplatonic posit of solar divinity with his Christianity—the sun being analogous to God the Father (and the First Mover), the *anima motrix* being analogous to the Holy Spirit.

inversely related to distance, he accepted that the rate at which a planet moves along its orbit is inversely related to its mean distance from the sun.

Kepler held tight to constituents 1–5 throughout his career, and they are crucial to a set of his most significant temporally novel predictions made explicit in his 1605 work, *Astronomia Nova* ([1609]; henceforth AN): the sun spins;[14] it spins in the direction of planetary motion; it spins along the plane of the ecliptic; and it spins faster than any of the planets revolve around it ([1609], pp. 387–8). Notably, these are predictions for which Kepler receives little if any credit, perhaps because their success was altogether unrelated to any theory we now accept. 1–4 are crucial to the first three predictions, while 1–5 are crucial to the fourth. And he made these predictions well before the 'discovery' of the sun's spin by Fabricius and Galileo—who, in 1611–1612, drew that conclusion after observing sunspots and noticing that they moved. Looking back in his *Epitome*, Kepler even pointed to the temporally novel nature of his predictive success, 'I proved in my *Commentaries on Mars* [i.e. AN], Chapter 34, by reasons drawn from the very movement of the planets' and, it is clear, from the *anima motrix* and its assumptions, 'long before it was established by the sunspots, that this movement necessarily had to take place' ([1618–1621], p. 56).

We may well hear it said that Kepler's unprecedented rejection of uniform motion was determined by Brahe's meticulous data. It appears, however, that it was the *anima motrix* that drove him to reject uniform motion in MC—at least 3 years before he even met Brahe. For many years, Kepler remained (like Copernicus) committed to circular motion, and he allowed that the planets travel an eccentric path, with the sun removed from the center of their orbits. On this account, a planet's distance from the sun varies as it moves along its orbit. Conjoining this constituent to 1–5, an individual planet's speed will change as it traverses its eccentric course. As Kepler saw it, the *anima motrix* gave him 'the reason and the means' to 'defend' the 'irregularity in' the planetary paths: a 'planet will be slower' when 'further away from the Sun', where it is 'moved by a weaker power', and 'faster' when 'closer to the Sun', where it is 'subject to a stronger power' ([1596], p. 217). Not only did he predict that planetary motion is non-uniform, but also and more specifically that each individual planet will reach its highest speeds at its perihelion and its lowest at its aphelion. These predictions appear to be at least use-novel, if not temporally novel, for Mars, Jupiter, and Saturn. When writing MC, Kepler questioned whether they held for Mercury and Venus; yet, as he wrote 22 years later—by which time he had presumably acquired confidence from Brahe's data—his predictions are 'even more true of them' ([1596], author's note 6,

[14] Kepler wrote, 'since the species [or emanation] of the source, or the power moving the planets, rotates about the center of the world, I conclude with good reason [. . .] that that of which it is the species, the sun, also rotates' ([1609], p. 387).

Ch. 22).[15] Likewise, while he lacked evidence in MC to extend these speed predictions to the earth, as he notes in AN (p. 372), they hold for the earth as well. Kepler himself appropriately emphasizes that the credit goes to his *anima motrix* ([1609], p. 373). The predictions also hold for the then unknown planets Uranus, Neptune, and Pluto. Thus, we have successful temporally novel predictions regarding the apsidal speeds of these six planets—as well as numerous other bodies in the cosmos. Given the crucial role of 1–5, it looks as though those constituents must be credited with these successes as well.

A constituent that would later become pivotal for 'Kepler's laws' is his inverse-distance speed law, 6. On the grounds that 1–5 causally ensure a continual inverse relation between distance and speed, Kepler generalized from the apsidal distance–speed relations to the entire orbit:

6. The speed of a planet is inversely proportional to its distance from the sun.[16]

Notably, 1–6 carry over from his MC, so predate his access to Brahe's data. In a note added to his second edition of MC, 22 years later, he writes, 'You see, then, assiduous reader, that in this book there were scattered the seeds of each and every one of the things which since that time in this new and, to the masses, absurd astronomy I have established and demonstrated from the thoroughly exact observations of Brahe' (author's note 7, pp. 219–21, Ch. 22).

Turning to his 1605 theorizing in AN ([1609]), we see a more elaborate articulation of his *anima motrix*. Reaffirming 1 above, that the planets are 'inclined to rest' ([1609], p. 388), Kepler added that they resist the push of the sun. As the sun's push decreases with distance, the planet's resistance begins to take over.[17] And while he looked to Gilbert's theory of magnetism to supplement his theory, the rays that sweep the planets along their orbits are not, at this stage, attractive: he suggested that 'there is in the sun no force whatever attracting the planets, as there is in the magnet' ([1609], p. 390). There is 'only a directing force' ([1609], p. 390). The articulation of this picture precedes and culminates in Kepler's precise formulation of his area law, his 'second law' (Ch. 40), which preceded his 'first'. In working out the area law, Kepler employed 6, as well as 7—despite the fact that he had already seriously considered rejecting circular motion.

[15] In AN he wrote, 'Indeed, it was for this reason that I had come to Tycho, that I might use his observations to inquire further into my opinions expressed in that book [MC]' ([1609], p. 252).

[16] As with the apsidal speed postulates, Kepler did not apply 6 to the earth until AN.

[17] Planetary resistance is the 'disposition of the movable body itself to rest (others might say, 'weight', but I do not entirely approve of that, except, indeed, where the earth is concerned)' ([1609], p. 384).

7. Planetary orbits are circular.

Kepler also made a mistake in his reasoning. Seeking to determine the sum of the distances along the radii vectors, he opted to employ Archimedes' method of dividing a circle into triangles from the center—which Archimedes had invoked to determine the ratio between diameter and circumference ([1609], p. 418). Since Kepler's sun was at the center of the universe, he used the sun as his point of reference for describing planetary motion. However, because the sun was off-center from the assumed circular orbit, his triangles, in contrast with those of Archimedes, were not right angled. While Archimedes' method accurately measured each radius, Kepler's did not. In short, Kepler employed an error he recognized but accepted, expressed in 8.

8. The area can be equated to the sum of a planet's distances from the sun, the sum of the radial vectors.

Nonetheless, Kepler arrived at his area law:

9. Planets sweep out in equal areas over equal time.

The constituent 9 rests directly on 6–8. And given the cumulative force 1–5 had for 6, it looks as though 1–8 played a significant role in the development of, and are deserving of credit for, 9.

With 9 in hand, Kepler (re)turned to the task of determining the shape of the orbit of Mars. He rejected circular orbits, crucially and admirably after years of diligent effort to retain them. His best hypothesis was off by 8 minutes arc ([1609], Ch. 19), and it is often suggested that his rejection of circular orbits was based on data alone. However, it again appears ([1609], Ch. 57 and 58) that it was his fundamental commitment to forced motion and the need for a direct push from the sun—thus more generally 1–4—that drove him to disavow the long encouraged practice of (merely) saving the phenomena by positing epicycles, the centers of which are carried along the circular paths.[18] He opted instead to reject circular motion altogether. Considering then ovular motion, he found himself re-entertaining epicycles and wrestling to devise a causal account of epicyclic motion that would retain the sun's causal authority. Eventually, however, he found that, with 10, which became his 'first law', he could wholly eliminate epicycles and preserve the sun's direct push on the planets.

10. All planets move in perfect ellipses with the sun at one focus.

[18] Arthur Koestler ([1963]) emphasizes in this context the physical over the geometrical; my more specific point holds no less.

Not only did 1–4 have an immediate bearing on 10; given that 9 was crucial to obtaining that law,[19] and given the relationship between 1–8 and 9, we are led to attribute cumulative responsibility to 1–9 for 10.

Regardless of how, we unpack the steps to Kepler's first two laws, constituents 1–8 were no 'idle wheels' in respect to them. And the success of those laws was unprecedented, central as they were to the Rudolphine Tables. Not only did they lead to successful predictions (beyond those already noted) pertaining to the behavior of Mars and the Earth, the planets about which his initial theorizing pertained; his laws led to, and continue to lead to, innumerable successful predictions regarding Mercury, Venus, Saturn, and Jupiter, which are at least use-novel, if not temporally novel. In fact, Kepler did achieve further temporally novel successes early on, significantly predicting, not only two planetary transits, but also a separation between the two transits of less than a month. The Mercurial transit occurred on 7 November 1631. The prediction of the rare and particularly irregular Venusian transit was confirmed by Pierre Gassendi on 6 December 1631, a year after Kepler's death. Further his laws led to, and continue to lead to, a multitude of successful and temporally novel predictions regarding the then undiscovered planets, Uranus, Neptune, and Pluto, as well as legions of other bodies in the solar system and beyond.

There remain additional constituents, which, due to the ambiguity of their *cumulative* force, have not been numbered here. However, we have seen that they did play into Kepler's reasoning. For instance, in MC, Kepler drew on the following in formulating his speed law:

— The intensity of light varies inversely with distance.

And on the way toward developing his area law, Kepler supplemented his explanation of the speed law with

— The planet's inclination to be at rest, and to thereby resist the solar push, contributes to the planet's slowing speed when more distant from the sun.

Likewise, he enriched his articulation of the *anima motrix* by drawing on Gilbert's theory:

— The force pushing the planets is a 'directive' (and, at this stage, explicitly non-attractive) magnetic force.

[19] While Kepler often conflated 6 and 9, one of the two was required for 10. As Kuhn puts it: 'Unless the planetary orbits are assumed to be precisely reëntrant (as they were after Kepler's work but not before), a speed law is required to compute orbital shape from naked eye data. When analyzing Brahe's observations, Kepler made constant use of his earlier Neoplatonic guess [i.e. 6]' ([1957], p. 216).

While the impact of these latter constituents is not as clearly cumulative as that of the others above, each did play a role in his theorizing and is also deserving of credit by the responsibility model we are considering.

Notably, by the responsibility model, the predictions derived directly from Kepler's system do not exhaust the successes attributable to it. Kepler's laws were altogether fundamental to 11, as worked out by Newton in the pre-*Principia* text, *De motu corporum in gyrum* ([1684]):

11. Planets are subject to an instantaneous action-at-a-distance gravitational attraction to the sun that is in an inverse-square relation to their distance from the sun.

In the *De Motu*, Newton showed first that any body drawn by a centripetal force (not necessarily an inverse-square force) toward a fixed point will obey Kepler's area law ([1684], Theorem 1, p. 3). He hypothesized that there is an innate force in bodies which, when otherwise unimpeded, will cause the body to move in a straight line infinitely ([1684], Hypothesis 2, p. 1). He began working toward a procedure for measuring the external centripetal force that would displace a body from that rectilinear path—dictated by its innate force—so as to bring about uniform circular motion ([1684], Theorem 2, pp. 3–4); and he drew the conclusion that in uniform circular motion the period squared is proportional to the radius cubed (Kepler's third law in respect to circular trajectories) and that the centripetal force is reciprocally proportional to the radius squared ([1684], Corollary 5, p. 4, following Theorem 2). Newton then presupposed Kepler's area law, his own innate force hypothesis, and the existence of a single planet that moves in an ellipse. He demonstrated that the required force toward a focus of the ellipse would be inversely proportional to the distance squared ([1684], Problem 3, p. 5, following Theorem 3). Here, we have that fundamental instance from which Newton generalized to arrive at his law of universal gravitation. Assuming that an inverse-square centripetal force is acting on bodies orbiting in an ellipse, he then derived Kepler's third law[20] ([1684], Theorem 4, pp. 6–7). Finally, assuming the inverse-square law and a set of initial conditions, he went on to derive conical planetary paths, which would include, but would not be limited to elliptical orbits ([1684], Problem 4, p. 7, following Theorem 4). For Newton, it was a fundamental precondition that his law

[20] While I have not focused above on Kepler's third law, it was among the results of his continued quest to demonstrate the Pythagorean posit of the harmonious nature of the heavens. That law related the planetary orbits to one another by describing the fixed ratio between their periods and their mean distance from that which pushes them via the *anima motrix*, the sun: the square of the time is proportional to the cube of the mean radial vector (*Harmonices Mundi [1619]*, Book V, Ch. 3, Proposition 8).

describing the relation between distance and force share an intimate logical relationship with Kepler's laws.[21] And the generalization from 11—his inverse-square law of planetary attraction to the sun—was altogether crucial to his obtaining his law of universal gravitation.

12. The instantaneous action-at-a-distance gravitational force between two objects is proportional to the product of their masses and inversely proportional to the square of the distance between them.

Kepler's laws were likewise, then, crucial to the formulation of 12, which was, of course, no idle wheel in the success of Newtonian mechanics. Constituent 12 played a very significant role in a multitude of key use-novel successes regarding, for instance, the slowing of Saturn as it passed Jupiter, the behavior of the tides, the behavior of stellar objects, and the precession of the equinoxes. But it was also employed in successful temporally novel predictions pertaining to, for instance, the oblate shape of the earth, the returns of Halley's comet, many non-Keplerian perturbations,[22] as well as any number of recent predictions, such as the hurling of Apollo 13 around the moon, other gravity assists such as those achieved in respect to Voyagers 1 and 2, etc. However, those victories often claimed to be the greatest of Newtonian theory are the temporally novel predictions of not only the existence of a trans-Uranian planet but also its location in the vast sky. Given that Kepler's laws were crucial to 11 and ultimately 12, and given the central role of 12 in Newtonian successes, if we are genuinely seeking to give credit where it is due, I contend that, by cumulative force, the entire collection of Keplerian constituents above, 1–10, was significant toward, responsible for, and must be credited with, these novel Newtonian successes. I strongly suspect that any criterion by which we can deny credit to Kepler's theoretical posits, for even these Newtonian successes, will be arbitrary and contrived.

Choosing from among the key Newtonian successes, let us briefly attend to Neptune's discovery. In addition to Newton's laws, one constituent that both Adams and Leverrier employed toward their descriptions of this planet was a law asserted by Titius ([1766]) and Bode ([1778]). It states that adding 4 to each member of the series, '0, 3, 6, 12 . . .' and (to bring it in to accord with today's astronomical units) dividing the result for each by 10 gives us the semi-major

[21] In fact, Newton employed Kepler's laws rather than 'the empirical data'. At the time, Kepler's laws were apparently neither taken to be empirically established nor even generally employed by astronomers—despite the significant success of the Rudolphine Tables. Perhaps for simplification, 6 and 7 were more commonly taken to express the data than were 9 and 10 (see Hoskin [1999], p. 130–31; also De Gant [1995], p. 84). In depth articulations of the reasoning involved and the relations between the *De motu* and the first edition of the *Principia* can be found in De Gant ([1995]) and Brackenridge ([1995]).

[22] Counterintuitive though it may first appear, Kepler's laws were significantly employed toward the Newtonian system, and that system renders those very laws false.

axes of the planets in astronomical units (AU): '0.4, 0.7, 1.0, 1.6 . . .' Before its use toward predicting Neptune's location, the Titius-Bode law predicted that a trans-Saturnian planet would reside at 19.6 AU. Uranus, discovered by William Herschel ([1781]), was found to have a semi-major axis of 19.2.[23] This discovery was taken by many to stand as a significant success for the law, and it excited the search for a planet between Mars and Jupiter. There astronomers, also following the Titius-Bode law, found Ceres, termed an 'asteroid' by Herschel, and, later, Pallas, another asteroid. It is worth noting that the locations of Uranus, Ceres, and Pallas were roughly derivable from the Titius-Bode law and that these discoveries may arguably stand as successes for that law.

The observed behavior of Uranus, however, was found to be out of accord with predictions. In John Herschel's words, 'the existing tables could no longer be received as representing, with any tolerable precision, the true laws of [Uranus's] motion' ([1849], p. 661). Both Adams in England and Leverrier in France worked painstakingly to determine the orbit and mass of a planet that would cause the discrepancy. Not only did they successfully predict the existence of a trans-Uranian planet, never yet observed as such, they made exceptionally accurate specifications of its position in the vast sky in the Autumn of 1846. As Herschel reported Galle's observation, 'The geocentric longitude determined by Dr Galle from his observation was 325° 53′, which, converted into heliocentric, gives 326° 52′ arc minutes, differing 0° 52′ from M. Leverrier's place, 2° 27′ from that of Mr Adams, and only 47′ from a mean of the two calculations' ([1849], p. 668). However, numerous constituents from which Adams and Leverrier had derived their specific predictions were altogether false. While both used the Titius-Bode law only as an initial guide, the constituents they finally employed regarding the planet's semi-major axis nonetheless greatly exceeded today's value (30.047 AU): Leverrier exceeded today's value by six times the earth's distance from the sun (36.1539 AU); Adams by seven times (37.2474 AU).[24] Relatedly, that crucial constituent describing the distance between the semi-major axes of the planet and of Uranus asserted far too high a value (the present value being 10.85 AU): for Leverrier's it was nearly 17 times the earth's distance to the sun, for Adams's more than 18 times. Moreover, the constituents describing the shape of the orbit asserted it to be far too eccentric: the eccentricity

23 I am indebted to John Christie, Maureen Christie, and Alan Musgrave who, recognizing my concern with the general issue discussed here, encouraged me to look into this law and the discovery of Uranus.

24 Sears Cooke Walker charted observations of Neptune made by Joseph Lelande (who was unaware that he was charting a planet) to get a better account of its properties. John Herschel ([1849], p. 671) charted that data beside Leverrier's and Adams's (October) theoretical properties ([1849], p. 670). My values for the latter come from Herschel's columns. However, I am comparing them, not to Walker's data, but to updated values from NASA (Williams [2004]).

posited by Leverrier being 12.5 times (0.10761), and Adams 14 times (0.120615), the present measure of Neptune's eccentricity (0.00859). Leverrier's constituent regarding the mass of Neptune was more than double today's value, off by over 18 times the mass of the earth; while Adams's was nearly triple today's value, overshooting by nearly 33 times the mass of the earth. Related to each of these dramatically false constituents were numerous others.[25] For instance, the constituent regarding what the longitude of the perihelion would be was dramatically false—284°, 45′ for Leverrier; and 299°, 11′ for Adams, compared with a present value of 44°, 58′. And for both systems, the orbital periods overshot the present value attributed to Neptune (163.7) by more than half an earth century. Any number of additional false constituents would have been involved as well—such as those pertaining to the then in question mass, orbital period, eccentricity of Uranus, etc. As Herschel put it, 'the received elements [of Uranus] [...] must *certainly* be erroneous, the places from which they were obtained being affected by at least some portions of the very perturbations in question' ([1849], p. 669, emphasis in original). Nevertheless, the particular predictions of the existence of a trans-Uranian planet and that planet's location in 1846 as observed by Galle were temporally novel successes—two generally seen to number among the greatest scientific victories in history.

5 Assessing deployment realism

I suggest that this admittedly brief account of the above successes points to a serious problem for the deployment realist. If we are to genuinely give credit where credit is due—i.e. to those posits that played a significant role in leading scientists to their predictions—it looks as though we must, on pain of inconsistency, attribute credit to each link in the variety of constituent chains discerned above.

Notably, the deployment realist appears to offer a way skirt the notoriously problematic notion of approximate truth: she can attribute truth *simpliciter* to the deployed statements in a system, while allowing that others are false. A significant point to be drawn here, however, is that a *partial* (rather than approximate) truth variant of deployment realism fares very poorly against history. No constituent isolated in the above survey is true by present lights. Each is, strictly speaking, false. Granted, when discussing the historical argument, the hypothesis realists claim we can be justified in believing tends to be expressed in terms of approximate truth. Little

[25] In fact, Benjamin Peirce (C.S. Peirce's father) argued that the planet Adams and Leverrier had posited was not the one discovered ([1847]). While Peirce's point accords with the thesis that false constituents played into the predictions, it does nothing to negate the success of those particular predictions.

consolation comes to the realist, however, if we concede that, by present lights, some of the empirical claims—e.g. Kepler's speed law, the Titius-Bode law, etc.—while strictly false, nonetheless give approximate empirical results. The specification that a theory achieves this is perfectly non-realist. Nor crucially would the realist want to commit herself to the view that, say, the Titius-Bode *law* approximates a *law of nature*—when contemporary science takes its success to be little more than a partial coincidence. Not only does that law predict a planet between Mars and Jupiter,[26] it predicts the 8th planet at 38.8 AU (Neptune coming in at 30), and a 9th planet at 72.2 (Pluto coming in at 39.3). Such empirical 'approximations' aside, it is significant that many of the constituents (of both empirical and theoretical character) we have considered are patently false. By present lights, it is not even approximately true that

— the sun is a divine being and/or the center of the universe (Kepler);
— *the natural state of the planets is rest;
— there is a non-attractive emanation coming from the sun that pushes the planets forward in their paths;
— the planets have an inclination to be at rest, and to thereby resist the solar push, and this contributes to their slowing speed when more distant from the sun;
— the force that pushes the planets is a 'directive' magnetic force;
— there exists only a single planet and a sun in the universe (Newton);
— *each body possesses an innate force, which, without impediment, propels it in a straight line infinitely;
— *between any two bodies there exists an instantaneous action-at-a-distance attractive force;
— the planet just beyond Uranus has a mass of 35.7 earth masses (Leverrier)/50 earth masses (Adams);
— that planet has an eccentricity 0.10761 (Leverrier)/0.120615 (Adams);
— the longitude of that planet's perihelion is 284°, 45′ (Leverrier)/299°, 11′ (Adams), etc.

Consider the theoretical constituents marked by *, which I take to be those the realist is most tempted to call her own. Rising from the realist's repertoire of responses may be the cliché that one or more of these constituents 'is true in the limit', or 'the world is empirically just *as if* it were true', or some variant thereof. In light of contemporary science (e.g. general relativity), the former

[26] While Olbers had posited that the asteroids were once a planet, contemporary theory asserts that Jupiter's attractive strength has kept them separated. (See Hoskin [1999], p. 162).

property can mean little more than 'T is empirically successful in a limited range'[27] and is a perfectly non-realist claim despite its realist guise. The latter, 'as if' assertion, is by contrast, unabashedly non-realist. A similar point to that above is relevant: to reduce approximate truth to empirical success (or approximate empirical truth) is to discard scientific realism altogether, especially when approximate truth is supposed to explain empirical success. The non-realist would likely welcome aboard a 'realist' who appeals to these responses.[28]

In any case, it looks as though no one of these *-constituents can be said to meet even these non-realist descriptions. Granted, one might say that, from the context of contemporary science, within certain parameters, the world is approximately empirically 'as if' the Newtonian *system* (coupled with our background system) is true. However, the deployment realist insists that we evaluate each constituent as a candidate for approximate truth *in isolation from the whole theoretical system*. And in isolation, these constituents give us no successful empirical results: in order to obtain any genuine predictions or tangible consequences whatsoever, they must be connected to other constituents—which describe, for instance, the past locations of points of light in the heavens, properties of those points of light, the way the telescope works, etc. No one of these constituents will *in itself* give us a single empirical prediction that can be judged successful by looking up at the night sky or at an image on a screen. And since the successes of those constituents only come to them as part of a set of statements, no one of these constituents is in itself then 'true in the limit' about, or an empirical approximation of, anything.

Similarly, and notably, since the approximate truth status of the above constituents must be a property each possesses in isolation, the deployment realist cannot avail herself of any account of verisimilitude/truthlikeness that pertains to the proximity to which the set of consequences or a model of the system stands in respect to the world. And sacrificing the appeal to those system-attributed notions of approximate truth comes at a significant price. Taken in isolation the *-constituents do not approximate any 'truth' of contemporary science. Nor, to use Psillos's preferred phrasing regarding approximate truth ([1999], Ch. 11), is there any 'degree' to which, or (realist) 'respect' in which, planets *are at rest* or the posited forces *exist*. Perhaps integrating these constituents into some substantial system would provide a model or a possible world that can be said to approximate to some degree, or be similar

27 As John Worrall notes ([1989], p. 143, footnote 6), even the empirical claims of Newtonian mechanics are strictly false according to general relativity.

28 More generally, realists are barred from the depleted non-ontological/non-literal interpretations of the dynamical theories of Kepler and Newton that realists may have been pushed toward when introduced to the relations between these theories and general relativity; such interpretations are non-realistic, if not positivistic.

in some respects to, the world itself. But taken alone these constituents provide no such model and describe no such possible world. Each *-constituent is, in isolation, unequivocally false by present lights.

I strongly doubt that a notion of approximate truth could be forged that is able to accommodate these constituents while remaining intimately related to the realist notion of truth. Allow me to hint at a further threat, persisting even if that doubt can be answered. The realist employs approximate truth in the no-miracles argument, which is deemed an abduction (for instance, by Psillos [1999], p. 71.), a mode of argumentation articulated by C.S. Peirce. An abduction's vital explanatory premise states that the explanans would make the explanandum 'a matter of course' (Peirce [1905], p. 189), i.e. likely.[29] Laudan ([1981]) points out that realists have failed to show that an approximately true theory will be successful. Realists appear to have largely neglected this point. (For instance, Psillos's Chapter 11 of his ([1999]) dedicated to articulating the notion of truthlikeness contains no apparent solution to, nor even an acknowledgement of, this problem.) The problem becomes especially salient given the deployment of constituents so explicitly contradicted by the constituents of present science. Accommodating them would require an exceptionally non-restrictive notion of approximate truth, allowing an enormous class of statements to qualify as approximating any given true statement. Hence, given the mere stipulation that the constituents responsible for predictions are approximately true, empirical success may well be, not only questionable, but altogether unlikely—rendering the realist's explanatory premise false.[30]

Finally, it is hardly too bold to insist that, in order to *justifiably believe* that S is P, we must be able to articulate what it means to be P, in this case, approximately true. The attribution of that property is otherwise, it would seem, empty. And it remains implausible to claim justification for believing that those constituents responsible for key successes are approximately true until the realist explicitly formulates a theory of truth approximation that pertains to individual statements rather than systems, is intimately related to the realist's notion of truth, and possesses genuine explanatory force toward success.

[29] If Peirce's account is to be replaced or interpreted in another manner, it is most pressing that the realist explicates just how.

[30] For more on this, see my ([2002], [2003]). Related problems come with stretching a theory of reference to be so broad (and thin) that 'instantaneous action-at-a-distance force', and/or 'the *anima motrix*' (i.e. the emanation from the sun that, at a right angle, pushes the planets forward in their paths), etc., *really* refer to the spacetime curvature brought about by massive objects. So capacious a notion of reference threatens to welcome as referring far too many terms, in far too many contexts; scientific statements will be depleted of content; and, most significantly, the loss of force for making success likely, thus for explaining success, threatens to be drastic. These are hardly preferred realist results.

The realist ultimately seeks to provide a criterion for belief. On the responsibility model, the realist is asserting that those constituents that are responsible for significant successful predictions are deserving of belief. If it turns out, as is suggested here, that there are noteworthy occasions in which patently false posits—as well as invalid reasoning and the like—have played a significant role in leading scientists to successful predictions, then such a realism will not evade the historical argument.

It is crucial to emphasize that, contra the standard treatment of the historical argument, there is no dubious 'pessimistic meta-induction' being made here, no inference from past to present day constituents, no generalization to say that the constituents of contemporary science are false. It is rather an epistemic conclusion drawn via a *modus tollens* argument. The hypothesis that the deployment realist says we can justifiably believe states, 'those constituents that are genuinely employed in key successes are approximately true'. If that hypothesis is correct (A), then each constituent genuinely employed in a key success will be approximately true (B). However, we have (just seen) a set of constituents genuinely deployed in key successes that are not approximately true (Not-B). Therefore, the realist hypothesis is false (Not-A).[31] Each false constituent included among those responsible for a key successful prediction stands, each time it is included, as a falsifying instance of the deployment realist hypothesis. In fact, unless we already assume that hypothesis to hold, and hence beg the question in favor of realism, we can point to no instance where a theoretical constituent both leads to success and is, in fact, approximately true. The epistemic conclusion: having identified numerous falsifying instances and unable to identify, in a non-question begging manner, a single instance in which both correlates obtain, we can hardly be justified in believing the realist hypothesis. The hypothesis is not even an acceptable conjecture. Moreover, if we take seriously the no-miracles argument that is put forward to justify that hypothesis, we have witnessed numerous miracles here. And despite an advertised explanatory ability, deployment realism finds itself unable to explain even one success for which a patently false constituent is among those responsible. Meanwhile, such failure to explain is the very fault realism points to in its opposition. Finally, since no one in the debate will accept the positing of miracles to explain any such successes, there must be some alternative, non-deployment-realist explanation for each such success.[32] The deployment realist's ability to note that a few rejected posits (e.g. the posit that the luminiferous ether exists) were idle wheels provides little consolation (and, of course, in itself no positive argument) for realism.

[31] For further explorations pertaining to this meta-*modus tollens* argument, see (Lyons [2002]).
[32] My own contender for an alternative explanation can be found in (Lyons [2001], [2003]).

Other arenas of science hold the potential to reveal additional counterinstances. In my ([2001]) and ([2002]), I have traced a further and rather substantial set of such counterinstances in domains ranging from chemistry to cosmology. Additional candidates are pointed to by Worrall ([1994]), Stanford ([2003]), and Chang ([2003]). In this paper, I have used particularly familiar examples to indicate how easily we can overlook the counterinstances to scientific realism. Yet those less known are no less relevant. W.J.M. Rankine, for instance, derived a number of novel predictions from the dramatically false constituents of his thermodynamic theory. In fact, Rankine himself attempted but failed to derive the same predictions without the theoretical constructs.[33] There may be any number of counterinstances hidden in history—hidden perhaps because they are not open to a presentist and pedagogically effective mischaracterization as being among the truth-accumulating steps toward present day theories. In any case, since each false *constituent* that is deployed in a key successful prediction constitutes a counterinstance, and since a particular false constituent stands as a counterinstance *each time* it is deployed in a successful prediction, deployment realism has the potential to fare far worse against the historical argument than the 'naive' holistic versions of realism over which it is thought to be an improvement.

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References

Brackenridge, J. B. [1995]: *The Key to Newton's Dynamics: The Kepler Problem and the Principia: Containing an English Translation of Sections 1, 2, and 3 of Book One*

[33] See (Lyons [2002]) and especially (Hutchison [2002]).

From the First (1687) Edition of Newton's Mathematical Principles of Natural Philosophy, Berkeley: University of California Press.

Chang, H. [2003]: 'Preservative Realism and its Discontents: Revisiting Caloric', *Philosophy of Science*, **70**, pp. 902–12.

De Gant, F. [1995]: *Force and Geometry in Newton's Principia*, translated by C. Wilson, Princeton: Princeton University Press.

Herschel, J. [1849]: *Outlines of Astronomy*, 1869, *tenth edition*, London: P. F. Collier and Son.

Hoskin, M. [1999]: 'Newton and Newtonianism', in M. Hoskin (*ed*), *The Cambridge Concise History of Astronomy*, Cambridge: Cambridge University Press.

Hutchison, K. [2002]: 'Miracle or Mystery? Hypotheses and Predictions in Rankine's Thermodynamics', in S. Clarke and T. D. Lyons (*eds*), *Recent Themes in the Philosophy of Science: Scientific Realism and Commonsense*, Dordrecht: Kluwer, pp. 91–120.

Kepler, J. [1596] *Mysterium Cosmographicum*, *The Secret of the Universe*, 1621 second edition, translated by A. M. Duncan, 1981, New York: Abaris Book.

Kepler, J. [1609]: *New Astronomy*, translated by W. H. Donahue, 1992, Cambridge: Cambridge University Press.

Kepler, J. [1618–1621]: 'Epitome of Copernican Astronomy', in *Epitome of Copernican Astronomy and Harmonies of the World*, translated by C. G. Wallis, 1995, Amherst: Prometheus.

Kepler, J. [1619]: 'Harmonies of the World', in *Epitome of Copernican Astronomy and Harmonies of the World*, translated by C. G. Wallis, 1995, Amherst: Prometheus.

Kitcher, P. [1993]: *The Advancement of Science*, Oxford: Oxford University Press.

Koestler, A. [1963]: *The Sleepwalkers*, New York: Universal Library.

Kozhamthadam, J. [1993]: *The Discovery of Kepler's Laws: The Interaction of Science, Philosophy, and Religion*, Notre Dame: University of Notre Dame Press.

Kuhn, T. S. [1957]: *Copernican Revolution: Planetary Astronomy in the Development of Western Thought*, Cambridge: Harvard University Press.

Laudan, L. [1981]: 'A Confutation of Convergent Realism', *Philosophy of Science*, **48**, pp. 19–49.

Leplin, J. [1997]: *A Novel Defense of Scientific Realism*, Oxford: Oxford University Press.

Lyons, T. D. [2001]: *The Epistemological and Axiological Tenets of Scientific Realism*, Ph.D. Dissertation, University of Melbourne, Australia.

Lyons, T. D. [2002]: 'Scientific Realism and the Pessimistic *Meta-Modus Tollens*', in S. Clarke and T. D. Lyons (*eds*), *Recent Themes in the Philosophy of Science: Scientific Realism and Commonsense*, Dordrecht: Kluwer, pp. 63–90.

Lyons, T. D. [2003]: 'Explaining the Success of a Scientific Theory', *Philosophy of Science*, **70**, pp. 891–901.

Newton, I. [1684]: 'De motu corporum in gyrum', in *The preliminary manuscripts for Isaac Newton's 1687 Principia, 1684–1686* (facsimiles), 1989, Cambridge: Cambridge University Press.

Niiniluoto, I. [1999]: *Critical Scientific Realism*, Oxford: Oxford University Press.

Peirce, B. [1847]: 'Investigation in the Action of Neptune to Uranus', *Proceedings of the American Academy of Arts and Sciences*, **1**, p. 65.

Peirce, C. S. [1905]: *Collected Papers*, Vol. V., 1958, Cambridge: Harvard University Press.

Psillos, S. [1999]: *Scientific Realism*: *How Science Tracks Truth*, London: Routledge.

Sankey, H. [2001]: 'Scientific Realism: An Elaboration and a Defense', *Theoria*, **98**, pp. 35–54.

Stanford, K. [2003]: 'No Refuge for Realism: Selective Confirmation and the History of Science', *Philosophy of Science*, **70**, pp. 913–25.

Williams, D. [2004]: 'Neptune Fact Sheet', *Planetary Sciences at the National Space Science Data Center*, N.A.S.A. Available at: <nssdc.gsfc.nasa.gov/planetary/factsheet/neptunefact.html>.

Worrall, J. [1989]: 'Structural Realism: The Best of Both Worlds?', in D. Papineau (*ed*), 1996, *Philosophy of Science*, Oxford: Oxford University Press, pp. 139–65.

Worrall, J. [1994]: 'How to Remain (Reasonably) Optimistic: Scientific Realism and the "Luminiferous Ether"', in M. Forbes and D. Hull (*eds*), *PSA 1994*, Vol. 1, East Lansing, MI: Philosophy of Science Association, pp. 334–44.