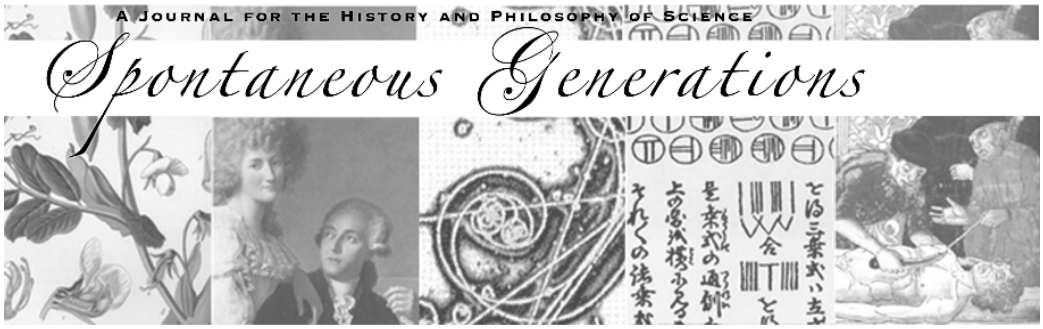

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Source: *Spontaneous Generations: A Journal for the History and Philosophy of Science*, Vol. 9, No. 1 (2018) 122-135.

Published by: The University of Toronto
DOI: *10.4245/sponge.v9i1.27046*

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Published online at *jps.library.utoronto.ca/index.php/SpontaneousGenerations*
ISSN 1913 0465

Peer-Reviewed

How Deployment Realism Withstands Doppelt's Criticisms*

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Currently one of the most plausible versions of scientific realism is "Deployment" (or "Partial", or "Conservative") Realism, based on various contributions in the recent literature (especially Kitcher 1993), and worked out as a unitary account in Psillos (1999). According to this version we can believe that theories are at least partly true (because that is the best explanation for their predictive success—especially *novel predictions*), and discarded theories which had novel predictive success had nonetheless some true parts: those necessary to derive their novel predictions. In fact, it has been argued that the partial truth of theories (including the discarded ones) is the only *non-miraculous* explanation of their success.[1]

According to Doppelt (2005, 2007) this account cannot withstand the antirealist objections based on the "pessimistic meta-induction" and Laudan's historical counterexamples. Moreover, it is incomplete, as it purports to explain the predictive success of theories, but overlooks the necessity of also explaining their explanatory success. Accordingly, he proposes a new version of realism, presented as the best explanation for both predictive and explanatory success, and committed only to the truth of the best current theories, not that of the discarded ones (Doppelt 2007, 2011, 2013, 2014).

I have argued elsewhere (Alai 2016a) that Doppelt's "Best Theory Realism" is not really a viable option, for it can explain neither the success of past theories nor their failures; moreover, it is rather implausible, and actually the easiest prey of the pessimistic meta-induction argument. Here instead I argue for the following claims: (a) Doppelt has not shown that Deployment Realism as it stands cannot solve the problems raised by the

* Received June 16, 2016. Accepted September 22, 2016.

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[1] See Putnam (1975a, 73). In Alai (2014c) I argue that there cannot be an equally viable antirealist explanation.

history of science; (b) explaining explanatory success does not add much to the explanation of novel predictive success; (c) Doppelt is right that truth is not a sufficient *explanans*, but for different reasons than he thinks, and this does not refute Deployment Realism, but helps to detail it better. In a more explicit formulation, the realist Inference to the Best Explanation (IBE) concludes not only that theories are true, but also that the scientific method and scientists are reliable, nature is orderly and simple, and background theories are approximately true.

I. Can Deployment Realism resist the pessimistic induction and meta-modus tollens?

The "no miracle" argument, according to which the (approximate) truth of theories is the only plausible explanation of their empirical success (and in particular of their novel predictions), was criticized by Laudan (1981). More recently Lyons (2002) has raised against it what he calls the "pessimistic meta-*modus tollens*": some theories now known to be radically false made successful predictions, even novel ones; therefore the predictive success of current theories is no reason to believe they are true. To this Doppelt adds a particularized version of the "pessimistic meta-induction": "Furthermore, if past *successful* theories are false, it is likely that current *successful* theories are false, indeed, wholly false ... because in all probability the entities to which they refer do not exist, just as in the past cases" (2005, 1077).[2] To these criticisms Psillos has three replies: (1) the true predictions of some discarded theories were not *novel*, so they could be explained by *a posteriori* accommodation, without appeal to truth: realism is committed only to the truth of theories with novel predictions. (2) Some discarded theories had novel success, but we can still recognize them as partially true: the assumptions that were essential in deriving their novel predictions are still part of our theories. (3) A suitable causal theory of reference shows that some successful but discarded theories actually referred to the same entities as currently held theories. Doppelt sees all of these three replies as unsuccessful.

Novelty

Against Psillos's claim (1) that only novel predictions warrant belief in the truth of theories Doppelt has three objections. The first is that the confirmation of a theory by data cannot depend on the contingent fact of when those data were discovered, or whether the theory was advanced before

[2] My emphases: the reference to *successful* theories distinguishes this version of the pessimistic induction from the more general one. An example of the original formulation of the pessimistic meta-induction is Putnam (1978, 25).

or after their discovery. It is more important that there is "a wide range of different kinds of evidence," or that the theory has "a broader explanatory scope" (2005, 1079-1080).

Now, this is a well-known objection used by *deductivists* to deny the advantage of novelty against *predictivists*. The latter however have convincingly replied that what is relevant to confirmation is not *temporal* novelty, but *use* novelty: i.e., what matters is not that the data predicted by theory T were unknown before it, but that they were not used (or more precisely not used *essentially*) in building T (Worrall 1985, 319; 2005, 819; Leplin 1997, chs. 2, 3). Of course temporal novelty implies essential-use novelty (for if a datum was not known before T, it obviously was not used in building T); but only essential-use novelty is required to bar all possible explanations except truth, so only essential-use novelty warrants belief in the truth of theories.[3] Therefore the only criterion of truth for realists should be essential-use novelty, and Doppelt's criticism of historical novelty doesn't apply to this criterion.

Actually, it can be shown that the requirement of essential-use novelty is equivalent to the consilience of disparate bodies of evidence, i.e., to broad explanatory scope, which Doppelt rightly takes as strong confirmation of theories. In fact, saying that datum D (even if used) was inessential in the construction of theory T is saying that T was already plausible independently of D, i.e., that T was already the best possible explanation of a sufficiently large body of evidence quite different from D; and this, in turn, is saying that T is the best possible explanation of a wide range of different phenomena (Alai 2013).

So, it is quite safe, for Psillos, to use novelty (understood as essential-use novelty) as a criterion for deciding which parts of theories realists should consider to be true.[4]

Doppelt's second objection to the novelty criterion is that if a scientific realist is a naturalist, as are many supporters of the "no miracle" argument, she should treat scientific realism itself as she treats scientific theories. But scientific realism does not make novel predictions. So, to be consistent, realists should not require novel predictions from theories (2005, 1080).

However, first of all, this argument applies only to naturalists, and one can be a scientific realist, even a supporter of the "no miracle" argument, without being a naturalist. Secondly, it might be pointed out that scientific realism actually does make at least one prediction: that science will go on

[3] See Alai(2013). For an overview of the literature on novelty see Gardner (1982, 1), Maher (1988, 273), Barnes (2008, 1-26), etc.

[4] Lyons (2002) objected to Psillos that in a number of historical cases some false *claims* were essentially involved in successful novel predictions, so novel success is not evidence even for the truth of particular claims. I have replied to these objections in Alai (2014b).

being successful, nay even more successful, in the future. Of course, strictly speaking this is not a *novel* prediction, since although it predicts something new (future success) it is however of the same type as something (past success) already known and used in arriving at scientific realism itself.[5] Furthermore, a realist can be a naturalist even without holding that realism is a scientific theory, but rather simply by holding that realism can be supported by a typically scientific inferential pattern (the IBE), based on an empirical fact—the success of science (see Alai 2012). Even Boyd and Putnam claimed that realism is *science-like*, not that it is a scientific theory: "philosophy is itself *a sort of* empirical science" (Boyd 1984, 65; my italics) and "*in one way of conceiving it*, realism is an empirical theory" (Putnam 1978, 12; my italics).

Hence, the relation between scientific realism and a scientific theory can be seen more as an important analogy than as an identity; as such, it leaves room for both positive analogies (similarities) and negative analogies (differences). For instance, scientific theories describe facts about particular domains of the natural or social world; scientific realism, instead, hardly describes anything—or if it does, it describes an utterly general relation (truth, i.e., correspondence) obtaining between scientific theories and entities of *any* domain of natural or social reality. But it is easy to see that something really new can be discovered only about particular natural or social domains, not about most general facts such as those that are dealt with by scientific realism or philosophy in general. So, novel predictions need not be among the similarities between scientific realism and scientific theories. Therefore, *pace* Doppelt, from the fact that the former does not make novel predictions, it doesn't follow that the latter are not required to make them.

Doppelt's third and most important objection is that for Psillos (1999, 171) theories should not just predict in the sense of entailing the data, but *explain* them, in the sense of offering an account that *is simple, complete, intuitively plausible, consistent with background beliefs, embedding the entailed datum in a wide picture, etc.* (2005, 1080-1081, passim). One reason, Doppelt points out, is that this provides the only defence of realism against the empirical underdetermination argument: thousands of false theories may imply the same body of data, but (supposedly) just one can explain it (i.e., imply it in such a virtuous way) and so be a candidate for belief (2005, 1083). So, he argues, what realism should explain is not only predictive success (i.e., why the theory entails novel data), but also explanatory success (i.e., why it has the theoretical virtues of simplicity, completeness, consilience, broad scope, plausibility, consistency with background knowledge, etc.) (2005,

[5] In Alai (2013, § 3.3) I explained why data that are new but of the same type as those used to build the theory cannot count as novel to the ends of realist commitment.

1081). However, some of the successful-but-false theories cited by Laudan, and rejected by Psillos because they lacked novel predictions, no doubt possessed the mentioned theoretical virtues. (Doppelt mentions Le Sage and Hartley's contact-action gravitational ether theory, but the phlogiston and caloric theories and others might also be cited.) In this sense, they had *explanatory* success. Hence, since for realists truth is the best explanation of success, they should be committed to the truth of these theories (2005, 1081; 2007, 107). Therefore, since we know those successful theories were actually false, for Doppelt it follows that success is not evidence of truth.

Doppelt's argument is fallacious, however, for the kind of explanatory success that is explainable only by truth is not just any sum of novel predictions and theoretical virtues, such that in the limiting case it might consist of many theoretical virtues, but no novel predictions; rather, it is the conjunction of novel predictions with theoretical virtues, where the two conjuncts are both necessary. To see why this is so, suppose a theorist wants to explain a body of *known* data D: if she is ingenious and patient enough, by reasoning backwards from D she might well succeed in building a theory T that has explanatory success in Doppelt's sense, i.e., which entails all the data in a way that is *simple, elegant, complete, plausible*, etc. However, one cannot conclude from this success that T is true: in fact, this "success" (i.e., that T entails D and has those theoretical virtues) is simply due to the general principle that for any body of data there are numberless false theories entailing them (plus a true one), and to the fact that the theorist was ingenuous and patient enough to build not just any theory which entailed D, but one that entailed D in a theoretically virtuous way.

One might suspect that finding such a theory is easier said than done.[6] Granted, it is certainly not easy, but it is possible, and the history of science is full of such theories that were nonetheless radically false. The Ptolemaic system was extremely ingenious; it explained everything there was to explain, and was so plausible that it was accepted for some 1400 years. Perhaps it was not exactly simple and elegant by our contemporary standards, but these are relative notions, and at least it was simple and elegant enough to be considered explanatorily successful for all those centuries. The Tychonic system, the caloric and phlogiston theories, the vortex theory of heat, etc., thanks to the intelligence and hard work of their authors, were also so explanatorily successful that they were believed to be true, and only with great difficulty were they eventually superseded by their true competitors. So, the "explanatory success" of T in Doppelt's sense is by itself no evidence for its truth. After all, good historical novels entail a number of true claims about the historical period at hand, and do so in a simple, complete, consilient, and

[6] I owe this suggestion to this paper's Reviewer B.

plausible way—but they are false!

In a word, explanatory "successes" of this kind *can* be accounted for without assuming the truth of T. The keys to this explanation are ingenuity, patience, and most importantly the possibility of working abductively from a body of previously known data, like in solving a puzzle. But if there is at least one phenomenon that T not only explains, but anticipates (i.e., that was not used in building T), then the "ingenuity and patience" explanation is no longer possible; in this case the only possible explanation, which therefore becomes overwhelmingly probable, is that T has captured some structures of reality that are responsible for those novel effects, i.e., that it is (at least partly) true. So, the realist need not be committed to the truth of any theory or theoretical component, epistemically virtuous as it may be, unless it also yields some novel predictions. Therefore Psillos has no need to consider successful in the required sense those past theories that explained virtuously but yielded no novel predictions, and those theories are no counterexample to the claim that novel predictive success is evidence for truth.

Summing up, Doppelt's criticisms of Psillos's appeal to the novelty of predictions as a criterion of commitment is off target.

Partial truth

The second defensive move of Deployment Realism is the *divide et impera* strategy: when a discarded theory made genuine novel predictions, one may assume that it had not only false components, but also some true ones: those responsible for such predictions. The false assumptions, instead, will be found to play no role in those discoveries. For instance, the existence of caloric and its properties were not necessary to the novel predictions made by the caloric theory, which can be credited instead to different and *true* assumptions of the theory (Psillos 1999, 108-118). But Doppelt objects that such false components, idle with respect to predictive success, were necessary to the theory's explanatory success: i.e., they were an essential part of the simple, consilient, plausible, etc. account given by the theory. So, realists should be committed to their truth, or give up the idea that theoretical assumptions essential to success must be true (2005, 1084-1085; 2007, 108).

But as just noted, theoretical virtues by themselves are not evidence for truth; they become a significant success only in connection with novel predictions. So, even if a false component were essential to make a theory virtuous, realists wouldn't need to accept it as true. Moreover, it is simply not the case that false components of the caloric theory, or the phlogiston theory, etc., were essential to their explanatory success. In fact, take any of these false theories T_1, which made novel predictions NP and had true components TC directly responsible for NP; a false component FC (e.g., the existence of caloric, phlogiston, etc.) would be essential to T_1's success

only if it were a necessary component of any other theory T_2 predicting *NP* and at least equally general, complete, simple, plausible etc. However, as a matter of fact, this is simply not the case: each of those discarded theories was rejected precisely when scientists realized that there was an alternative theory T_2 that (a) dropped *FC*, but (b) was at least as simple, plausible, etc., and (c) made all the predictions *NP* and more. So, why shouldn't we accept *TC*, which is responsible for *NP* and preserved in T_2, as true, while rejecting *FC*, which has been discarded by T_2, as false? Thus, the "divide and conquer" strategy works quite well even with respect to the more robust notion of explanatory success.

Referential continuity

Psillos's third defensive move against Laudan's attack (and more generally against the pessimistic meta-induction) exploits the causal account of reference, in the footsteps of Putnam (1975b): when a term (e.g. "luminiferous ether") is involved in existential claims that are essential to novel predictions, it can be understood as actually referring to a real entity (e.g., the electromagnetic field) that shares some crucial properties with the entity postulated by the discarded theory, and plays the same causal role. Thus, for instance, ether theorists actually referred to the electromagnetic field, even if attributing to it partly wrong properties; so, their existential claims on "ether" were true, after all.

Doppelt's objection to this move is very similar to the previous one: the wrong properties that the ether theorists attributed to what they called "ether" were probably essential to the explanatory success of their theory, since they were required to make their theory general, complete, simple, plausible, etc. Hence, how could we explain its success, while regarding the attribution of those properties as false? (2005, 1086; 2007, 109).

This objection fails for a reason similar to the earlier one: on the one hand, the fact that the ether theory, including those wrong attributions, was coherent, simple, plausible, etc., does not require an explanation in terms of truth, but merely in terms of the ingenuity of its authors. On the other hand, that such a theory predicted (hence, thanks to its virtues, explained) some novel phenomena calls for an explanation in terms of truth, but those wrong properties play no role in that explanation: in fact, even if those predictions were originally drawn from false claims, which attributed wrong properties, the "divide and conquer" strategy shows that they could also be derived by weaker and true claims, which were entailed by the theory but did not involve any wrong properties (see Alai 2013, §7, Alai 2016a §3). Moreover, those weaker true claims are precisely those made by the equally or more virtuous theory (ours) which has dropped those wrong attributions. The success of the idea of luminiferous ether, in other words, is explainable because, and to

the extent that, it resembles the electromagnetic field.

It has been objected that understanding "ether" as referring to the electromagnetic field may be overstretching (Worrall 1995). In fact, I don't think the causal theory of reference as such plays a crucial role in resisting Laudan's objection and the pessimistic meta-induction: if the ancient ether theorists could come back to learn today's physics, would they claim that (a) in their time they were actually talking about the electromagnetic field, or would they rather grant that (b) they believed in a non-existent entity? In these terms this is merely a psychological question of little interest to us. But if we ask whether they *should* hold (a) or (b), there is simply no fact of the matter about this question, because there is no fact of the matter about just how much descriptive difference is compatible with referential continuity.

Nor would this matter much to Deployment Realists anyhow: what matters for them is whether (and how many, and which) true components are found in theories. But just as claims about real entities can be false, so claims about inexistent entities can be partially true. For instance, there is no truth in the claim that "ether has weight, " whether by "ether" we mean ether or the electromagnetic field. On the other hand, if "ether" refers to the electromagnetic field, the claim that "ether oscillates" is true, while the claim that "ether is an elastic fluid" is false. If instead "ether" just means ether, and so fails to refer to anything, the claim that "ether oscillates" is false or void of truth-value, yet it entails the *true* claim that the transparent, weightless, etc., agent responsible for the transmission of light oscillates.

Summing up, none of Doppelt's criticisms refute Psillos's defence of Deployment Realism against historical objections.

II. Is predictive success a sufficient *explanandum* and is truth a sufficient *explanans*?

Doppelt also criticizes Deployment Realism as such: as seen above, he holds that (1) realists should explain not only predictive success, but also "explanatory" success; it follows that (2) the (partial) truth of theories is not enough to explain *all* there is to explain about their success. While I reject the former claim, I substantially agree with the latter, although for reasons partly different from his. What this claim reveals, however, is not a mistake, but something missing, or perhaps just left implicit in Psillos's version of Deployment Realism. However this gap can be easily filled: in fact, in his proposed reconstruction of realism Doppelt himself supplies part of what is missing, and I shall add what is needed to provide a complete account.

Doppelt understands explanatory success as the capacity to offer a theoretically virtuous account of data: therefore, "what realism must explain is why a theory succeeds in producing a simple, unifying, consilient, intuitively plausible, and empirical adequate explanation of phenomena"

(2005, 1082; 2007, 102).

This formulation of the question is objectionable for two reasons: (1) taken literally, the question is trivial, and so is the answer—the theory succeeds in giving a simple, etc. explanation, because it *is* simple, etc. What should be asked is rather: Why has the theorist succeeded in producing a theory that is simple, etc. (and so constitutes a simple, etc. explanation)? It might now be pointed out that (2) the answer becomes easy, and does not involve truth or other realistic assumptions: the theorist produced a theory that was simple, etc., because she wanted to produce such a theory and was ingenious enough to succeed. The really hard question, the answer to which requires the assumption of truth, is how scientists succeed in finding theories that (beside being theoretically virtuous) are predictively successful.

Doppelt also provides what he considers an equivalent formulation of his question, although it is actually a different question: "Is it just a lucky accident that true theories turn out to be simple, consilient, unifying, and plausible, as well as empirically adequate?" (2005, 1082). That is, for the realists the predictively successful theories are true; moreover, they turn out to be simple, consilient, etc. Therefore, this conjunction of theoretical virtues and truth is another fact to be explained.

Doppelt's answer is that the true theory is also simple, consilient, etc., because "nature itself is simple, unified and symmetric, in the way implied by these theories" (2007, 102). In his reconstruction of realism, therefore, a new "metaphysical" (2005, 1082) assumption, the simplicity and order of nature, is added to the usual realist claim that theories are true.

But, upon reflection, this new metaphysical assumption actually adds nothing to what was already implicit in realism: since theories represent nature as simple and orderly, the claim that they are true already implies that nature is simple and orderly. Moreover, there is a problem with the argument leading to that conclusion: Doppelt asks why *true* theories turn out to be simple, consilient, etc., and empirically adequate. But who says that they are true? The realists do, on the basis of their explanatory argument! So, realists are not allowed to take the truth of theories as a datum to be explained: truth can only appear as the ultimate conclusion of the realist's argument, i.e., as the *explanans* of whatever other facts the realist is entitled to take for granted.

A question realists are entitled to ask is rather: Why are the theories *making true predictions* also simple, consilient, etc.? (In fact, that theories make true predictions and that they are theoretically virtuous are not assumptions, but easily observable facts.) It might be objected that this question needs no answer, for it is just a logical fact about the theory that it has the particular consequences it has, and it is as simple, etc. as it is (see White 2003). Nonetheless, there is a real problem here, which is brought up by

a more explicit formulation of the realist question: How do *scientists succeed in producing* theories that make true predictions *and* are simple, consilient, etc.? As I argued above, the explanation for the simplicity, consilience, etc. can well be the theorists' ingenuity and desire to have theories with such virtues, and the explanation for true but not novel predictions can be the theorists' ingenuity and desire to accommodate those known phenomena. Only true novel predictions cannot be explained in this way. Hence, they are the *only* basis of the realist explanatory argument: *pace* Doppelt, realists don't need to explain explanatory success, but only predictive success.

III. The realist explanation and what it requires beside truth

Although Doppelt's claim that truth is not a sufficient *explanans* is derived from the wrong thesis that predictive success is not a sufficient *explanandum*, it is still true, albeit for different reasons. We can appreciate this by spelling out the realist IBE in its questions and answers:

Question(A): Why and how do scientists succeed in producing theories that make true novel predictions?

Answer (A): Because they produce theories including theoretical assumptions close enough to the truth and deep and fruitful enough to predict further significant phenomena in addition to those used in their construction.

But: *Answer A* as it stands is quite unsatisfactory. Why not just answer that:

(A′) theorists are lucky enough to produce theories with unexpected true consequences?

(After all, there are much greater chances of finding *empirically adequate* theories than *true* theories.) So Answer (A) is implausible, unless we in turn explain it: in order to make it more plausible than answer (A′), we need to explain how finding (partially) true theories is possible. In *this* sense Doppelt is right that truth *per se* is not a sufficient *explanans*. So, in order to answer Question (A) we also need to answer the following:

Question (B): How do theorists succeed in producing theories that include theoretical assumptions close enough to the truth and deep and fruitful enough to predict further significant phenomena in addition to those used in their construction?

The answer (as suggested by Maher 1988 and White 2003) is roughly that theorists succeed in producing such theories because:

Answer (B): Theorists

(B.i) aim to produce true, deep, and fruitful theories, and

(B.ii) employ the scientific method, which is *reliable* and heuristically effective, i.e., truth-conducive and favouring novel discoveries.

However, if the reliability and heuristic effectiveness of the scientific

method are not to remain an empty *virtus dormitiva*, they should be explained and motivated in their turn. We should explain what makes the scientific method truth-conducive and makes it favour novel discoveries. So, in order to answer Question (B), and hence Question (A), we also need to answer the following:

Question (C): Why is the scientific method so reliable and heuristically effective?

The realist answer, now, is:

Answer (C): Because

(C.i) the scientific method endeavours to explain observed phenomena by theoretical hypotheses whose consequences go beyond those phenomena and tend to be very general;

(C.ii) it respects empirical constraints;

(C.iii) it is based on the assumption that nature is simple, symmetrical, consilient, etc., and thus it reconstructs the unobservable natural systems by analogy, abduction, and inductive extrapolation from observable ones;

moreover,

(C.iv) nature actually *is* simple, symmetrical, consilient, etc.

(C.v) any background theories employed by theorists or presupposed by their method (see Boyd 1981) are themselves true since they are derived by sound scientific method.[7]

So, Answer (C) explains Answer (B), which explains Answer (A), which in turn explains the only data to be explained, novel predictions. The full-blown realist argument is that Answer (A) is the best (nay, the only non-miraculous) explanation of predictive success, but only if coupled with Answer (B); and Answer (B) is untenable unless supported by Answer (C). Therefore the answers (A), (B) and (C) are all required to make up the only plausible explanation of predictive success. Hence, they all are part of the conclusion of an IBE from predictive success (the "no miracle" argument), and realists are committed to all of them.

It may be observed that (B.i), (C.i), (C.ii), and (C.iii) are rather uncontroversial, as they describe plainly observable facts. In contrast, (B.ii), (C.iv) and (C.v), as well as (A) itself, are rather bold claims, concerning epistemic and metaphysical facts which are not open to direct empirical ascertainment; therefore these claims can only be argued for abductively,

[7] The presupposition of earlier theories by the theorist or by her method does not launch an infinite regress, since the earliest, "take-off" theories were introduced without the theorist relying on previous theories or on theory-dependent methods (Barnes 2008, 146-155). This does not mean that we have no reason to assume that those earliest theories were (partly) true, because the claim (C.v) that background theories are true is required only when background theories are employed, which is obviously not the case for take-off theories.

and supporting them is a remarkable achievement of Deployment Realism.[8]

In particular, (C.iv) is nothing but Doppelt's metaphysical assumption of the simplicity, consilience, etc. of nature. But I am arguing that this is just *one* of the further explanatory factors required to explain and make plausible the assumption that theories are true, which in turn is the only plausible explanation of novel predictive success.

Claims (B.i), that theorists follow the scientific method, and (C.iii), that the scientific method presupposes the simplicity, consilience, etc. of nature, together explain what Doppelt calls the explanatory success of theories, i.e., why they are simple, consilient, etc. But, contrary to Doppelt's claim, these theoretical virtues do not figure in the realist IBE as the primary *explanandum*, like novel predictions do. Rather, together with the other necessary assumptions, these virtues work to explain novel predictions. In this sense, nothing really needs to be added to the argument from *predictive* success, once it is made fully explicit.

Summing up, Doppelt's criticisms don't refute Deployment Realism; at most they help to spell it out more completely.

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[8] For a more extensive discussion of this topic see Alai (2014a), §5, and Alai (2016b), §4.

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