

CRITERIA FOR ATTRIBUTING PREDICTIVE RESPONSIBILITY IN THE SCIENTIFIC REALISM DEBATE: DEPLOYMENT, ESSENTIALITY, BELIEF, RETENTION . . .

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Abstract: The most promising contemporary form of epistemic scientific realism is based on the following intuition: Belief should be directed, not toward theories as wholes, but toward particular theoretical constituents that are responsible for, or deployed in, key successes. While the debate on deployment realism is quite fresh, a significant degree of confusion has already entered into it. Here I identify five criteria that have sidetracked that debate. Setting these distractions aside, I endeavor to redirect the attention of both realists and non-realists to the fundamental intuition above. In more detail: I show that Stathis Psillos (1999) has offered an explicit criterion for picking out particular constituents, which, contrary to Kyle Stanford's (2006a) criticisms, neither assumes the truth of theories nor requires hindsight. I contend, however, that, in Psillos's case studies, Psillos has not successfully applied his explicit criterion. After clarifying the various alternative criteria at work (in those case studies and in a second line of criticism offered by Stanford), I argue that, irrespective of Stanford's criticisms, the explicit criterion Psillos does offer is not an acceptable one. Nonetheless, the deployment realist's fundamental intuition withstands all of these challenges. In closing, I point in a direction toward which I've elsewhere focused, suggesting that, despite the legitimacy and applicability of the deployment realist's intuition, the historical threat that prompted it remains.

Keywords: constituents deployed in key successful predictions; criterion of essentiality; empirical testability; scientific realism; historical argument.

Deployment Realism

Larry Laudan (1981) has provided a well-known list of past successful theories that cannot be approximately true. In response, contemporary scientific realists have modified the hypothesis they claim we can justifiably believe. One of the most recent and promising tactics realists have employed involves changing their hypothesis to focus on, not theories as wholes, but a subclass of theoretical constituents contained within those theories. The following is the refined hypothesis these realists claim we can justifiably believe: those constituents that are "deployed" in (Kitcher) or "responsible for" or "essential to" (Psillos) key successful predictions are approximately true. The justification for this hypothesis is an overarching inference to the best or only explanation known as the no-miracles argument: it would be a miracle were our theories to achieve the success they have achieved were those constituents deployed in those successes not at least approximately true. Taking a cue from Kitcher, I call this sophisticated position *deployment realism*. Jarrett Leplin (1997), Ilkka Niiniluoto (1999), and Howard Sankey (2001) number among the advocates of this general sophistication on scientific realism. Yet it is Stathis Psillos who has offered the most careful articulation of deployment realism and attempted to apply it to the most significant extent (references to his (1999)). According to

Psillos, to defend realism against the historical argument the realist must take the following steps, which Psillos himself endeavors to do:

Step I: “identify the theoretical constituents of past genuine successful theories that made *essential* contributions to their successes; and” (*ibid.*, 110).

Step II: “show that these constituents, far from being characteristically false, have been *retained* in subsequent theories of the same domain” (*ibid.*, 111).

Of course this approach requires an answer to the question of what it means to be “essential.”

In a recent paper (2003) and book (2006a), Kyle Stanford challenges deployment realism. Stanford claims that, in the end, for the realist, which “parts” of a theory “were responsible for its success” must be determined “by appeal to our own *present* theoretical beliefs about the world.” (Stanford 2006a, 166). He contends that Kitcher and Psillos “do not (and perhaps cannot) provide” “criteria that *could have been* and *can now be applied*” “prospectively” (*ibid.*, 168). In short, according to Stanford, deployment realism

- offers criteria that can only be applied by assuming that our present theories are true
- fails to offer prospectively applicable criteria, and
- may be unable to provide prospectively applicable criteria.

In what follows, I will endeavor to show that, although the debate on deployment realism is quite fresh, a significant degree of confusion has already entered into it. As will become clear, both Stanford and Psillos have, unfortunately, been diverted from the deployment realist’s fundamental intuition. Additionally, I will show that, contrary to Stanford’s claim, Psillos has offered a criterion (or set of criteria) for picking out particular constituents that neither assumes the truth of theories nor requires hindsight. And against Psillos, I will contend that Psillos has not successfully applied the criterion he explicitly offers and seeks to apply. I will then argue that, Stanford’s concerns aside, the explicit criterion Psillos offers is not an acceptable one. Nonetheless, I do think the deployment realist’s *fundamental intuition* is on track: the deployment realist is wise to focus on particular constituents (rather than theories as wholes) that are responsible for key successes and, in doing so, is ultimately offering a well-motivated and applicable proposal. My primary goal in this paper, then, is to identify and clear away a number of distractions in the debate and to encourage both realists and non-realists to redirect their attention to that fundamental intuition. In closing, I will point in a direction toward which I’ve elsewhere focused, suggesting that, although the fundamental intuition is applicable and wholly legitimate, even when it is properly applied, the historical threat remains.

Textual Testability and Psillos’s Explicit Criterion

Allow me to preface this section and the next with a few methodological points regarding what we might call, textual debates, those pertaining ultimately to what has and has not been offered within a text, or set of texts. It may be that most of us interested in the scientific realism debate find ourselves reluctant to engage with one another in such debates. However, the realism debate is an increasingly complicated one, involving a wide-range of elements in the history and philosophy of science; and as the debate has progressed, or at least, transformed over the past few decades, such discussions have become necessary for bringing clarity to the increasingly many issues that have arisen. Furthermore, I think it is apparent that, in want of advancing our understanding of the issues in this debate, the competing parties genuinely share that quest for clarity. And the present paper, though no doubt critical, endeavors to forge constructive progress toward that end. Moreover, while other dimensions of the realism debate may offer results that (at least) appear more “conclusive,” I suggest that textual debates of this kind are particularly

capable of meeting a key virtue desired in the broader realism debate. The virtue I contend they afford is that of empirical testability—a virtue that is rightly emphasized in the realism debate since, ultimately, the latter is a debate over competing empirical hypotheses about science.

Because the empirical testability of textual discussions may not be obvious—given for instance a tendency to suppose that major differences in kind obtain between the sciences and the humanities—it is important to make clear my grounds for claiming that textual debates afford empirical testability. The first factor to notice is that a (proper) textual debate will pertain to a *restricted domain*, be it a series of papers, a specific set of chapters, etc. That given, there is at least one clear advantage that at least some theses about texts can offer over any number of scientific theories. Quite simply, the opportunity for observationally exhausting all parts of a text is clearly greater than any we might have for observationally exhausting, say, all particles in the universe. Irrespective of where this may take us in terms of fallibility, the point here is specifically about testability. Noting this, take “” to signify a restricted domain, ‘NX’ to signify non-existence, and ‘X’ to signify existence. Let *o* designate a specific textual occurrence (e.g. the spelling out of a criterion for essentiality, etc.). Employing these terms, we can specify here two of the kinds of restricted domain hypotheses with which we might find ourselves concerned, both of which are predictive:

- NX[~]-thesis: Observing restricted domain, *rd*, *o* will *not* be empirically located;
 - X[~]-thesis: Observing restricted domain, *rd*, *o* will be empirically located.
- We can accordingly specify two kinds of relevant empirical data reports:
- NX[~]-report: A report of an empirical search of restricted domain, *rd*, in an effort to observe *o*, that resulted in a failure to locate *o*.
 - X[~]-report: A report of an empirical search of restricted domain, *rd*, in an effort to observe *o*, that resulted in successfully locating *o*.

With two kinds of predictive hypotheses and two kinds of empirical data reports, we recognize significant potential for what we can appropriately deem testability. Specifically, an NX[~]-thesis is empirically testable in that it can be bestowed credit for passing-an-empirical-test by an NX[~]-report, and it can be empirically challenged by an X[~]-report. And an X[~]-thesis is likewise testable, in just the opposite way. It can be bestowed credit for passing-an-empirical-test by an X[~]-report, and can be empirically challenged by an NX[~]-report. Moreover, the NX[~]- and X[~]-reports themselves afford further empirical testing. (For instance, an NX[~]-report/X[~]-report can be taken as an empirically observed instance that permits the retention of an NX[~]-thesis/X[~]-thesis, the latter of which can again be tested.) Finally, although such testing need not be conclusive, it can be done objectively, or perhaps better, inter-subjectively. Of course, interpretations may well come into play; but these too can, in principle, be adjudicated by further empirical theses and argumentation connected to further empirical testing. While I leave open the question of how fallible/conclusive such textual debates may be, I hope to have made clear that they do afford an important degree of empirical testability. (To make clear the nature of the key textual theses I will be offering below, and (in the endeavor) to ensure that these theses meet the demand for testability, I will reference elements of what I’ve just outlined in what follows. In particular, while X[~]-theses will be evident by their supportive quotations, I will flag a series of NX[~]-theses in parentheses.) In fact, it is likely already clear that Stanford, in claiming that Psillos has neglected to provide a prospective and non-truth-assuming criterion for essentiality, is ultimately offering what we have here dubbed an NX[~]-thesis: a claim that observing *rd*, the restricted domain of Psillos’s chapters (5 and 6) on the historical argument, *o*, a criterion for essentiality that is prospective and does not presuppose the truth of contemporary theories, will *not* be empirically located. As noted, an NX[~]-thesis can be empirically challenged by an X[~]-report (which, as we’ve seen, affords further testing). My first thesis is against Stanford and in defense of Psillos, and it will fall into the latter category.

I agree with one component of the general sentiment expressed in Stanford's criticism: *if* Psillos had failed to provide a criterion for essentiality, Psillos's proposal for defending realism against the historical argument could not be undertaken, rendering that proposal empty. However, contrary to Stanford's claim above, Psillos does very explicitly provide a criterion of essentiality—one that Stanford does not address.¹ Psillos writes, "Theoretical constituents which make essential contributions to successes are those that have an indispensable role in their generation" (*ibid.*, 110). He asks, "When does a theoretical constituent H indispensably contribute to the generation of, say, a successful prediction?" (*ibid.*, 110). Before unfolding his answer, notice that the terms "essential" and "indispensable" (and "ineliminable") are quite naturally taken to mean that, without H, the relevant prediction cannot be derived. However, Psillos fully acknowledges that there will always be other hypotheses from which any given prediction can be derived. Noting this, we must remain mindful of two points. First, *no* H can qualify for this natural understanding of essentiality. Second, Psillos's criterion of essentiality/indispensability is not a *mere elucidation* of this kind of essentiality/indispensability.² Rather Psillos *defines* his term "essentiality" in the following very specific 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 P³
- 3) It must *not* be the case that any alternative, H^* , is available⁴
 - 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 (*ibid.*, 106)), that is potentially explanatory, etc.

Against Stanford's claims, I contend then, first, that Psillos cannot be faulted for failing to provide a criterion of essentiality. (In fact, and on the contrary, as I will make clear in Section IV, one problem is that Psillos provides *too many distinct* criteria.) Second, note that, since this criterion makes no reference to truth, it is not clear in any obvious way that applying this criterion to a constituent in a past or present theory requires that we presuppose that present day (or any other) theories are true. And, third, as long as we take "available" to be temporally restricted, e.g. pertaining to the time prediction P was made, then contemporary theories (or "prospective" assessments) pose no special problem. (Hence, I am offering an X'-report that directly challenges

¹ Stanford is not alone here: other recent responses to Psillos—for instance, Lyons (2002), Hutchison (2002), Chang (2003)—have altogether neglected, or at least set to the side, concern with Psillos's explicit collection of conditions for essentiality. As we'll see below, in a second line of criticism directed at Psillos's position, rather than addressing this criterion, Stanford addresses a distinct criterion (which is, to Stanford's credit, one that is *also* employed by Psillos).

² See Lyons 2006, 540. (Note that I will not be claiming, nor do I mean to imply that, *defining* essentiality is, in itself, illegitimate.)

³ In Psillos's words: "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" (*ibid.*, 110).

⁴ After presenting 1 and 2, 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" (*ibid.*, 110).

Stanford's NX~-thesis.) Nonetheless, in what follows I will raise a number of concerns with, first, Psillos's attempt to apply this criterion and, second, in section V, the criterion itself.

Psillos's Historical Applications

Having identified Psillos's criterion of essentiality, let us turn to his "historical applications" (Ch. 6)—two case studies meant to show that "both the foregoing requirements [Psillos's Step I and Step II, quoted above in Section I] can be met" regarding "several stages of the caloric theory of heat and the nineteenth-century optical ether theories" (1999, 111). Recognizing the need to avoid a purely retrospective assessment (*ibid.*, 112),⁵ Psillos claims that determining which constituents were and were not (or are and are not) essential is not so difficult. According to Psillos, "eminent scientists do the required identification all the time" [my italics] (*ibid.*, 112). In regard to Caloric theory, Psillos provides quotations to show that some scientists (e.g., Black) were not committed to the claim that heat is a material fluid. He contends that Laplace's derivation of the speed of sound in air does not rest on that supposition. He argues further that although Poisson appealed to caloric theory to derive the law of adiabatic change, the suppositions of caloric theory were not required to obtain this law. He then contends that Carnot's explanation of the Carnot cycle does not require the assumption (entailed in caloric theory) that heat is conserved. Psillos subsequently notes how Clausius showed that Carnot's law could be derived from an alternative theoretical framework, one that could replace the view from the caloric model that heat is conserved. Turning to ether theories, Psillos describes various models of the ether—the elastic model, the rotational ether, and the elastic jelly model—that were put forward to explain various characteristics of light. He emphasizes that these models did not demand the belief of scientists and that what was discarded in later theories were these models. By contrast, he notes, the Lagrangian dynamics of the carrier of light waves were retained.

Psillos is working through all of this with the explicit goal of providing historical support to the thesis that, if a constituent is essential, it will be retained in theory change. And, as we've seen above, he recognizes that the proper way to support that thesis would be to first identify all the essential constituents of a given theory (Step I) and then show that they were retained (Step II). In the last section, by flagging the explicit criterion that Psillos has put forward, I offered an X~report meant to refute Stanford's particular NX~-thesis that Psillos has offered no such criterion. I will now work my way toward clarifying a distinct kind of NX~-thesis, one that I contend *does* hold against Psillos, specifically in regard to these historical applications. In regard to the ether theory, Psillos does identify some constituents that were retained across theory change, Step II: "The most general theory—in terms of Lagrangian dynamics—which underwrote the research programmes associated with the dynamical behavior of light-waves was retained in the new framework of electromagnetism" (*ibid.*, 139). Yet to the significant detriment of his argument (and here I offer a preliminary NX~-thesis), he neglects Step I for these constituents—appearing to take retention (the concern of Step II) as directly implying essentiality (the concern of Step I). Further, we note that the empirical hypothesis in question,

⁵ Assuming present theories are in some sense more successful than their predecessors, the prime candidate constituents are those found in our contemporary theories. And to have a *justified* belief, we need to at least be able to specify what that belief is. We can't say we justifiably believe certain constituents of a contemporary theory without being able to identify just which contemporary constituents we believe. For these reasons I emphatically agree with Stanford, and in fact, Psillos himself, that our method for identifying essential (or deployed) constituents must be one that is not limited to a retrospective assessment.

elements that are essential by Psillos's criterion are retained, entails that those constituents that have been abandoned fail to meet Psillos's criterion. Yet, without showing that the ether models did not meet *his criterion* (a second preliminary NX[~]-thesis), Psillos writes that, "What became paradigmatically abandoned was a series of models which were used as heuristic devices for the possible constitution of the carrier of light-waves" (*ibid.*, 140) Just as he seems to be assuming that retention (II) implies essentiality (I) (as just noted), he appears to want us to take for granted that the lack of retention of a constituent (not-II) serves to demonstrate its non-essentiality (not-I)—"non-essentiality" in this context meaning that the constituent does not fit Psillos's *specific definition* of "essentiality." The non-retention of H might well be evidence of non-essentiality when essentiality means, "without H, P cannot be derived." But, as we've seen, even an H that has been retained will fail to be essential in this sense. In fact, as we've also seen, it is due to the latter point that Psillos replaces this natural understanding of essentiality with his own definition. And a lack of retention does nothing to indicate a failure to fit (any part of) Psillos's very specific criterion (i.e., his definition) of essentiality. Whether a constituent fits, that is a separate issue altogether, one Psillos has not addressed.

Psillos's discussion of caloric theory does fare better in this regard—since he directs some attention toward showing that rejected elements of that theory were not required in various reasoning processes.⁶ However, Psillos again neglects to show that any retained elements of caloric theory (the concern of Step II) meet his very explicit *criterion* for essentiality (the concern of Step I). (Here is my third preliminary NX[~]-thesis regarding Psillos's text.) The latter requires showing that the relevant cases are those in which H' and A would not be sufficient to render P (i.e., cases in which Psillos's condition 2 holds) and, most importantly, cases in which no alternative was available (i.e. cases in which his condition 3 holds).

Ultimately then, and unfortunately, Psillos does not show, or attempt to show, that *any* of those *retained* constituents—in regard to which he recommends a realist attitude—*meet* his criterion. (Here I offer a broad NX[~]-thesis.) Nor does he explicitly show that those constituents toward which he espouses a *non-realist* attitude *fail* to meet his criterion (a similarly broad NX[~]-thesis). In fact (and here is the primary NX[~]-thesis toward which I've been working), after introducing his very specific set of conditions, he does not make reference to that set of conditions again. Neglecting such crucial tasks in his own case studies, I'm afraid Psillos ends up disregarding his own requirements for "establishing" realism—or even licensing it, in the face of the historical argument. The problem is not, as Stanford would have it, that Psillos offers no criterion—nor that Psillos offers only a criterion that requires retrospective evaluation and/or requires the assumption that present (or past) theories are true. The problem is rather that Psillos does not apply the specific criterion of essentiality that he does offer. Neglecting that, he fails to achieve what he needs to achieve, Steps I and II, spelled out in Psillos's own words above (Section I).

Now, on that note, Psillos does often add that some scientists took certain retained constituents to be "well-supported." For instance, he asserts that "the parts of 'luminiferous ether' theories which were *taken by scientists* to be well-supported by the evidence and to contribute to well-founded explanations of the phenomena were retained in subsequent theories" (*ibid.*, 140) [my italics]. However, to invoke the expressed views of scientists as a means for determining when his criterion is met, Psillos must show something he does not, that scientists are in fact applying *his* criterion for essentiality: we have been given no reason to believe that the criterion for being "well supported" embraced by any one of these scientists even resembles

⁶ Hasok Chang challenges this aspect of Psillos's discussion, specifically (2003). Yet, like Stanford, Chang does not concern himself with the specifics of Psillos's essentiality criterion. (Here is an NX[~]-thesis pertaining to Chang's text.)

Psillos's very specific set of conditions.⁷ What has happened here, perhaps inadvertently, is that Psillos has changed the subject. (I will elaborate on this in the next section.)

In regard to ether models, Psillos does offer some textual evidence for a distinct thesis, the sociological thesis that some scientists were not epistemically committed to those models. Here Stanford offers part of a second line of criticism against Psillos—going beyond his first line of criticism that Psillos provides no criterion, etc. Stanford challenges the selectivity of Psillos's evidence regarding the judgments of these scientists, the focus of his critique in (2003, section 3 and 2006a, section 7.3), where he provides, for instance, textual evidence (X-reports) to show that some scientists were committed to posits, e.g. the *existence* of the ether, that have not been retained. I will claim in the next section that here, amid the complexities involved, the debate has been sidetracked from, not only Psillos's criterion of essentiality, but also from the question pertaining to the deployment realists fundamental intuition, i.e., the question of whether a given constituent was responsible for, deployed toward, success.

Before pursuing that point, however, allow me to bolster this second line of Stanford's criticism by *drawing on the fact that*, contrary to Stanford's first line of criticism, Psillos *has* offered a criterion of essentiality. That is, I suggest that, once we recognize that, contrary to Stanford, Psillos has offered the criterion he has offered above, we can offer a weighty addendum to Stanford's second critique. Specifically, we can note the following: were we to suppose that Psillos has accurately identified constituents that fit his explicit criterion of essentiality, some of Psillos's own citations provide explicit textual evidence that scientists *do not adhere to that particular criterion* in regard to their own commitments. For instance, Psillos grants that Laplace was "an advocate of the caloric theory" but says Laplace's derivation of the speed of sound "does not explicitly rest on any particular representation of heat" (Psillos 1999, 120). Psillos notes that "Carnot stated that the work produced in a steam engine was due to the *redistribution of caloric among the parts of the engine*" [italics in original] (*ibid.*, 122). And Carnot considered heat to be conserved, a thesis entailed by caloric theory (*ibid.*). Moreover, Carnot says that the conservation of heat was "*verified . . . in many cases by experiment with the calorimeter*" [my italics] (quoted in Psillos, 121). But according to Psillos, the conservation of heat (and more generally caloric theory) was not essentially employed in any specific predictions. Thus, not only has Psillos not connected his criterion to his examples (my primary NX-thesis), Psillos himself has given us evidence for the sociological thesis (counter to the one his argument requires) that scientists *do not* abide by Psillos's criterion of essentiality for their own commitments. Without showing that scientists actually employ his (or even, a) criterion for essentiality, Psillos's appeal to the expressed commitments of scientists is impotent. We can find quotations by many scientists in history who hold that certain claims are well supported that we now take to be false (as Stanford's X-reports detail), and we will likewise find scientists advocating an instrumentalist attitude for theories that have been retained.⁸ Even if a lack of epistemic commitment on the

⁷ The closest Psillos comes to connecting his own criterion to his case studies is in his discussion of Clausius's integration of Carnot's "essential" elements. But even here Psillos, at best, only makes it plausible that conditions 1, and, perhaps (with some stretch) 2, were met. However, he never shows us that those retained elements meet the very specific and elaborate conditions of his criterion specified in 3.

⁸ To note but one example: despite the *potential* essentiality of the early atomic hypothesis—toward, for instance, the law of multiple proportions—we find scientists espousing an instrumentalist attitude in regard to the atomic hypothesis as late as 1910. Alexander Smith wrote: "The atomic hypothesis provides a convenient form of speech, which successfully describes many of the facts in a metaphorical manner. But the handy way in which the atomic hypothesis lends itself to the representation of the characteristic features of a chemical change falls far short of constituting a proof that atoms have any real existence" (1910, 224).

part of those scientists could somehow be established with regard to the existence of the ether or any other posit (hence even if Stanford's critique of Psillos's sociological thesis could somehow be answered), by no means would it imply that a postulate failed to meet Psillos's explicit criterion. Given the severity of the problems raised in this section, I contend that such concern with the expressed opinions of scientists constitutes a serious distraction from the issue at hand: whether the fundamental intuition regarding responsibility and/or Psillos's explicit criterion of essentiality can stand as a legitimate and empirically successful answer to the historical argument against realism. Endeavoring, ultimately, to redirect our attention, I will now attempt to locate just where we have gone astray—and to make clear just how far we have wandered—from that core idea of deployment realism, with which our discussion began.

Psillos's Various Criteria and Implications for Stanford's Critique

Let us then take stock and attempt to identify just what has happened here. Deployment realism begins with, or has at its core, a fundamental intuition: as Psillos himself puts it, the constituents deserving of credit are those that "contribute" to a given successful prediction, those "which really fuel the derivation" (Psillos 199, 110). We ask "how" certain successes were "brought about" (*ibid.*, 109). The constituents responsible for particular successes are those that must be attributed credit. This appeal to the responsible constituents we can dub criterion "(A)." Now the notions of being "essential to the prediction," "indispensable," and "ineliminable" are often used in the deployment realist debate; and, in casual moments, these notions are treated as *interchangeable*, if not synonymous, with "contributing to," or "being responsible for," a prediction, i.e. criterion (A). However, as we noted in Section II, the terms "essential," "indispensable," and "ineliminable" are quite naturally taken to mean that, without H, the relevant prediction cannot be derived. Let's dub this natural understanding of "being essential to P" criterion "(A[^])." It should be clear that (A[^]) is distinct from (A): H may well be involved in the reasoning that leads to P, but (as Psillos acknowledges in the course of motivating his explicit criterion) for any such H there will always be other hypotheses from which P can be derived. That given, not only can H meet (A) without meeting (A[^]), since *no* H can qualify for (A[^]), no H that meets criterion (A) will meet (A[^]). (A) and (A[^]) are distinct criteria. We also saw that, *because* Psillos recognizes that there will always be hypotheses other than H from which P can be derived, he *defines* "essentiality" in a very specific way (conditions 1-3). We can dub Psillos's explicit criterion, (A[∅]). Again (as we just saw for A), since *no* H can qualify for (A[^]), an H that meets criterion (A[∅]) will not meet criterion (A[^]). (A[∅]) is not a *mere elucidation* of the natural understanding of essentiality, (A[^]) (as we noted in Section II); it is rather a wholly distinct criterion. Hence, neither (A) nor (A[∅]) can be equated with (A[^]).

What about (A) and (A[∅])? One may get the impression that Psillos sees (A[∅]), his criterion for "essentiality" (110), as an elaboration of the core component of deployment realism, (A), the property of a constituent's being responsible for, thereby deserving credit for, key predictions. However, it should be clear that, as with (A[^]), (A[∅]) is not a mere elucidation of that general deployment realist's intuition, (A). Simply put, a constituent that is *used to arrive at* P need not meet the very strict conditions in (A[∅]). Put another way, (A[∅]) overshoots (A), including as it does, conditions that have no bearing on whether a given constituent itself *contributed* to, was deployed in, the derivation of a given prediction: e.g., 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.⁹ We have then, at this stage, three distinct criteria—the

⁹ Expressed yet one more way, Psillos's essentiality criterion, (A[∅]), as presented, is not acceptable as a criterion for giving credit where credit is due (Lyons 2006, 540-1).

first two of which Psillos invokes, (A) and (A \diamond), the third of which he acknowledges he must avoid, (A \wedge).

(A) H is responsible for P

(A \diamond) H meets the explicit conditions, 1-3 (above).

(A \wedge) H is “essential/indispensable” in the sense that, without H, P cannot be derived.

These criteria are introduced (or, as is the case with A \wedge , implicitly acknowledged) before Psillos begins his case studies (Ch. 6), our concern in Section III. What we saw in Section III is that, in working through those case studies, Psillos changes the subject from (A \diamond) (with no further mention of its explicit demands) and often even from (A). For instance, we saw that, while Psillos earlier concedes to the unacceptability of (A \wedge), later, in his case studies on optics, he concerns himself only with the non-retention of the ether models. That is, rather than showing that ether models fail to meet (A) or (A \diamond), he appears to take their non-retention as revealing their non-essentiality; yet non-retention is evidence only for non-essentiality in the sense that P can be derived without H (a situation that obtains for any H). Failing to qualify for (A \wedge) does nothing to indicate a failure to meet either of the other two criteria. Going beyond that brief appeal to criterion (A \wedge) in place of (A) and (A \diamond), what we also saw in Section III is that Psillos, unfortunately, resorts to a variety of yet other (a second set of) criteria, attempting, as he does, to identify which constituents do and do not meet the following:

B) H was *asserted* (and hence apparently judged) *by some scientists* to be deserving of credit (or to be “well supported” or “confirmed”);

C) H was *asserted* (and hence apparently judged) *by some scientists* to be true;

D) H was retained in later theories.

Although meeting criterion (D) does not imply meeting (A \wedge), the two are related in that failing to meet (D) constitutes a failure to meet (A \wedge). However, the fact that a constituent qualifies or does not qualify for either (B), (C), or (D) does nothing, in itself, to imply that it qualifies or does not qualify for (A) or (A \diamond). And most notably, qualifying even for the *full set* (B)-(D) does nothing, in itself, to indicate that a constituent meets Psillos’s own criterion of essentiality (A \diamond) or condition (A). In short, at those frequent points where Psillos’s case studies rest on the ancillary issues captured in (B) and (C), Psillos has, at least inadvertently, distracted us from the real issue. (And, with regard to (A) and (A \diamond), D is only relevant *after* we have discerned which constituents meet (A \diamond) (or (A)), i.e. in Step II, the last stage of empirically testing deployment realism.) Making this switch, Psillos fails to achieve what he needs to achieve, spelled out in his own words above, Steps I and II.

Beyond this diagnosis of what was detailed in the last section, I am also suggesting, that, Psillos’s (inadvertent) shifts between these six criteria have led to a significant degree of confusion in the *debate* on deployment realism, particularly with regard to Stanford’s criticisms. First, as I noted in Section I, tracking Psillos’s historical applications, as he does, Stanford overlooks (A \diamond) and (A). He claims Psillos is “evading the need for any *explicit* criterion of selective confirmation [i.e., essentiality/credit] at all” (Stanford 2006a, 173). He says the realist “must offer criteria that *could have been . . . and can now be applied*” “prospectively” ([italics in original] *ibid.*, 168) (a requirement with which I agree), and he claims “that such criteria are just what” Kitcher and Psillos “do not (and perhaps cannot) provide” (*ibid.*, 169). Stanford claims that, in the end, for the realist, responsibility/credit must be determined “by appeal to our own present theoretical beliefs about the world” (p. 166). He says, Psillos “inherits” “the very problem Kitcher could not solve and Psillos sought to finesse: providing an explicit, historically reliable criterion . . .” (*ibid.*, 179). Perhaps a concern with *prospective* applicability stems from the past tense nature of (B), (C), (D), and a concern with the presumption of truth stems from the explicit reference in (C) to a judgment that H is true. In any case, as should now be clear given that Psillos does specify the criteria (A \diamond) and (A), Stanford’s claims here are ultimately unfair to the deployment realist.

Second, as I've noted at the end of the last section, after denying that Psillos has offered any criterion, Stanford's second line of criticism is directed at only those criteria employed in Psillos's attempted historical applications. That is, while Stanford endeavors to refute "existing attempts to provide" a criterion (2003, 913), his second line of criticism targets only the distractions, (B) and (C), i.e., criteria to which Psillos shifts only in changing the subject from (A \diamond). Specifically, Stanford notes that Psillos's "textual evidence" does not capture "Lavoisier's endorsement of" the caloric account (pertaining only to (C)) or Lavoisier's "judgment that it is well confirmed by the existing evidence" (pertaining only to (B)) (2006a, 178). On (C), Stanford questions Psillos's claim that, "scientists of this era were not committed to the truth of the hypothesis that the cause of heat was a material substance" (*ibid.*). Stanford says Psillos's point depends "on an extremely selective reading of the historical record" (*ibid.*)—and he challenges Psillos with "textual evidence" (*ibid.*)—regarding, not (A \diamond) or (A), but (B) and (C). He writes, "Psillos's case for the reliability (by current lights) of *scientists' own judgments of selective confirmation* must itself rely on an extremely selective reading of the historical record [textual evidence regarding (B) and (C)] that ignores or dismisses many if not most of those very *judgments*" ([my italics], *ibid.*, 179).

Now Stanford both recognizes and emphasizes that Psillos's appeal to scientists is a diversion. (For instance, he indicates frustration noting the "pointlessness of the detour through the historical record of (unreliable it turns out) judgments of selective confirmation by scientists themselves" (*ibid.*.) My point is not that Stanford's counter-case-study, in itself, is a problem. In fact, I think Stanford is providing important evidence that strikes at the common realist temptation to rely on the beliefs of scientists. However, focusing on only (B) and (C), Stanford is striking at only the weakest part of Psillos's defense of realism. Psillos's core offering in response to the historical argument is criterion (A), the deployment realist's intuition; and Psillos's most explicit articulation is (A \diamond), even if Psillos has failed to apply the latter. This oversight on Stanford's part is of considerable consequence, leaving Stanford's critique insufficient to challenge the core of Psillos's proposal. It is not, for instance, clear that any of the criteria (A \diamond)-(D) require hindsight in order to be applied to particular constituents; and, even if (B)-(D) do, (A \diamond) and (A) do not. Nor is it clear that (A \diamond) and (A) require that our past or present theories are true. The latter criteria—and variants of realism that employ them—survive Stanford's criticisms in tact. That given (provided our realist is willing to set aside the details of Psillos's case studies), our realist has, in Psillos's text, a potential solution to Stanford's criticism.

Finally, allow me to step briefly beyond the particular criticisms Stanford directs at Psillos's response to the historical argument, and to at least indicate that, and why, Stanford's oversight also has profound implications for Stanford's *primary* argument against realism in general, which makes up the core of his (2006a) and (2006b). Drawing on the insights of Pierre Duhem and Lawrence Sklar, Stanford dubs his general concern with realism, "the problem of unconceived alternatives." Stanford sees this argument as a whole as posing a "much more serious threat" than the "traditional arguments in support of underdetermination" and "the pessimistic induction (2006b, 122)." He writes,

If the historical evidence confirms that *past practitioners have indeed routinely failed to conceive of well-confirmed alternative hypotheses* of this sort that were sufficiently serious as to be actually accepted by later scientific communities, then *we have every reason to believe that there are similar alternatives to our own contemporary scientific theories* that remain presently unconceived, even if we cannot specify or describe them further (my emphasis) (2006b, 123).

And, insofar as there are unconceived alternatives, "we cannot responsibly infer that" our "best or only explanation . . . is even likely to represent the truth of the matter"; "we cannot justifiably regard the products of . . . eliminative or abductive inferences as even probably or

approximately true” (*ibid.*, 143). In a forthcoming paper I have criticized, in detail, this core argument of Stanford’s. Here I will appeal to and add to those concerns in the context of our present discussion. Put briefly, Stanford fails to take into account the restrictions imposed by a deployment realism based on (A \diamond) and (A): He does not provide empirical evidence that there were alternatives to (sufficiently many, if any of) *those particular constituents that were genuinely deployed* in (or essential by Psillos’s criterion for) any successful (*novel*) predictions. (Here I simply offer this as a testable NX’-thesis against Stanford.) Wrongly assuming there to be no such criterion, he neglects this utterly crucial empirical task demanded by his core argument. (And, of course, this is not to deny that there were such alternatives.) What this means is that, Stanford provides no evidence that “the problem of unconceived alternatives,” to which he dedicates his book, poses any problem at all for the most promising contemporary variant of realism. I submit that a deployment realism based on (A \diamond) and (A) survives—wholly untouched—against each of Stanford’s criticisms here considered.

The Unacceptability of Psillos’s Explicit Criterion, (A \diamond)

We are prompted then to set aside concern with Psillos’s “historical applications,” and the distractions, (A \wedge), (B), (C), and (D), invoked therein—and, in contrast with Stanford, to deal directly with (A \diamond) (and (A)): We must ask how (A \diamond) (and/or (A)) will fare as an answer to the historical argument. I will now critically address criterion (A \diamond) itself. Notice that in order to qualify as a testable hypothesis, to answer the historical argument, and to license a realist attitude toward any constituents at all, a realist criterion must be applicable: it must be such as to be successfully implemented to pick out specific theoretical constituents. In my (2006a, Section II), I contend that Psillos’s explicit criterion, (A \diamond), is simply inapplicable: it cannot be utilized to pick out any specific theoretical constituents. This is not, as Stanford would have it, because it requires retrospective analysis, or because it requires the truth of past or present theories. These concerns do not hold against (A \diamond). The problem is rather that, given its subconditions, (A \diamond) is excessively *vague*. (To make clear how this point differs from Stanford’s first line of criticism, my claim is that (A \diamond) is inapplicable due to the intrinsic vagueness of its details, while Stanford, neglecting (A \diamond) altogether, addresses neither its details nor their intrinsic vagueness.) Among the conditions on which it leaves us guessing are

- just when the potential competitors in 3 can and cannot be available;
- what “potentially explanatory” (and “etc.”) actually mean in 3c;
- the extent to which (or even whether) each of the elements of H’ and A also need to be “essential” by this definition;
- whether, given 3a, the competitor or 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;
- whether, or to what degree, H* can result in the loss of other confirmed predictions, etc.

Given its inapplicability, (A \diamond) is neither testable nor, then, an appropriate answer to the historical argument; nor can it stand as even a criterion for belief: for even accepting it, we could not identify which constituents we are licensed to believe. Moreover, since there will always be a competitor, as Psillos grants, no hypothesis would qualify for realism were we to employ 3 in our criterion without sub-conditions. Condition 3 then, I’ve contended, must be altogether discarded (Lyons 2006).

Condition 3, however, faces even more problems than these. And I will direct a new line of criticism against it here. Note that, in want of saving realism from the historical threat (and setting aside the issue of applicability), one could, *in principle* invent any number of diversionary and immunizing standards for what qualifies as a competitor; doing so,

it may seem, one could thereby force the non-realist to show that any H the non-realist is inclined to offer as a counterinstance to realism did not have qualifying competitors. (That is, if H's competitors did not meet our realist's standards, our realist will deny H's status as a counterinstance to the realist hypothesis.) However, in the context of the realist (or any proximate) debate, at least one non-contentious empirical constraint can be invoked to block the use of invented diversionary and immunizing standards. In short, any standards one may wish to impose on, and so require of, competitors must be such that they license science's most exemplary theories as genuine competitors: if one imposes standards that are so restrictive that our best theories fail to qualify as competitors, those standards are simply unacceptable.¹⁰ That given, rather than concede to any arbitrary standards, we can test their legitimacy. Along these lines, I will now contend that condition 3 of (A $\hat{\Delta}$) is an altogether unacceptable condition imposed on the acceptability of competitors, and that, as such, realists cannot legitimately invoke it. As we saw in Section II, Psillos's condition 3 states the following: *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 (*ibid.*, 106)), that is potentially explanatory, etc.

My contention, in short, is that condition 3 would preclude as legitimate competitors theories that were chosen in some of the most important advances in the history of science. This is especially clear in 3a, which dictates that we reject a rival hypothesis H*, simply because it clashes with H' and A. To accept this rule is to attribute the auxiliaries that happen to be connected to a hypothesis a greater status than any rival hypothesis. However, given the history of science, and given that any rules proposed must accord with that history, this rule cannot be granted. Einstein's theory is inconsistent with a significant set of auxiliaries in Newtonian theory (e.g. that distance and mass are constant, that space is properly described by Euclidean geometry, that gravity is an instantaneous action-at-a-distance force). That given, 3a would exclude Einstein's theory as an adequate competitor. Because the heliocentric hypothesis clashes with the claims that particular epicycles and particular invisible celestial spheres exist, 3a would deny its legitimacy as a competitor to Ptolemaic theory. Constituents often arrive in sets; and accepted theories tend to have come with their own package of constituents. The realist simply cannot discard competitors merely because they are inconsistent with H' and A. (One suspects that Psillos's criterion is inspired by defenses of the rationality of theory choice. Nonetheless, running contrary to his high regard for scientific practice, it looks here as though his rules would render much of that practice irrational, rendering theories that were *accepted* by scientists unable to qualify *even as competitors* against the theories they *replaced*.)

Turning to 3c, Psillos also imposes the condition that any potential alternative must be "non-*ad hoc*." He characterizes one condition of ad hocness as follows: "A body of background knowledge B entails the existence of phenomena E. Information about E is used in the construction of a theory T, and T accommodates E" (*ibid.*, 106). Apparently, for Psillos, any theory that meets this condition is *ad hoc*. When Psillos mandates that H* be non-*ad hoc* in regard to P, he precludes the use of the data that matches P in the formulation of H*. However, it is quite plausible that most (if not all) well-respected theories are *ad hoc* in respect to many

¹⁰ Above I argued that the realist cannot appeal to statements regarding the individual beliefs of scientists; nonetheless, both realists and antirealists are attempting to provide a theory about science, and doing so, must offer hypotheses that accord with the data of scientific practice (irrespective of what scientists might write regarding what they do and do not believe).

specific important bits of data. (I address this common realist demand for non-adhocness in considerable detail and with another set of examples in my (2009).) Even at the level of the individual theorist, previously accepted data are crucial in determining whether or not to even consider a theory. Clausius, like Thomson, for instance, very explicitly strove to reconcile Carnot's work with Joule's. Clausius surely used "the fact that maximum work is produced in a Carnot-cycle" (Psillos 1999, 112) in the construction of his replacement for caloric theory. By some accounts even Einstein used Mercury as a "criterion of adequacy" for his general theory of relativity (Leplin 1997, 51). Moreover, there is always the possibility of data whose clash with the theory would prevent the theory from being considered, let alone promoted/published, by its creator. Theories are rejected or accepted depending on whether or not they can accommodate at least some data; and at least some data (or general hypotheses taken to express the data) are used in formulating theories. If it is the case that "Information about E is used in the construction of a theory T, and T accommodates E," renders T ad hoc in respect to E, then many if not all well respected past and present theories will be ad hoc in respect to many specific sets of data. Demanding non-ad hocness in respect to particular data is far too strict to be compatible with scientific practice. Again, because the specific conditions in 3 are too strict to permit even some of the most exemplary scientific theories (those our realists will favor) to qualify as competitors, they are not acceptably imposed as standards for competitors.

Credit Where It's Due, Criterion (A)

Were we to eliminate Psillos's sub-conditions, however, condition 3 would amount to no more than the rule that there must be no alternative, H^* , available. Yet, as Psillos himself acknowledges in the course of motivating his explicit criterion, for any such H there will always be other hypotheses from which P can be derived. And, discarding his subconditions, we find ourselves with no hypothesis that can live up to condition 3. To avoid eliminating the possibility of realism, then, we are forced to discard 3 altogether. (For much more on the general tactic of employing "non-competitor conditions" in the realism debate, see my (2009).) However, just as Psillos's various appeals to (A[^]), (B), (C), (D) in his case studies divert us from his formal criterion, (A \diamond), that formal criterion (A \diamond) has diverted us from the deployment realist's fundamental insight, (A). The notion that a scientist *used* or *deployed* a constituent in the course of arguing toward a conclusion, namely a prediction, is clearly not to be equated with, and does not share the ambiguity, of the notion of "being essential/indispensable, etc." Casting aside the baggage of (A \diamond) and misleading terms such as "essential," "indispensable," and "ineliminable," we can isolate, and take seriously, the deployment realist's fundamental insight: *Credit should be attributed to those and only those constituents that were genuinely responsible for, deployed toward, specific significant predictions.* Embracing (A) as our criterion we will then seek to determine which constituents actually *led* scientists to specific successful predictions. To do this, we look to the texts in which scientists first arrive at those predictions, and we examine the premises and the reasoning they employ in doing so. We need not worry about such issues as who believed what to be true, or which competing hypotheses those scientists *might* have also been able to draw on to obtain the predictions they obtained. Nor need we worry about conclusions scientists might have drawn *from successful predictions to the truth of their theories.* None of this matters when we apply (A) to the history of science. We need only concern ourselves with how scientists *got to*, i.e., with the premises scientists *employed* in their arguments *toward*, a specific *subset* of their conclusions, namely, their empirical predictions that were (found to be) empirically successful. Although there may be instances where we have difficulty obtaining a solid grasp of the reasoning process, by appealing to their texts as our data, we can often make an objective case for the (testable) thesis that specific constituents were responsible

for, led scientists to, their specific successes. We need only identify the arguments that scientists offer toward specific conclusions. And identifying arguments employed toward specific conclusions—that is a particular task that’s been done for centuries . . . by philosophers, natural or otherwise. That is, concerning ourselves with the relevant data—in this context, *the arguments deployed by scientists*—we can generate NX[~]- and X[~]-theses which themselves afford further testability, as outlined in Section II, by way of the relevant and respective NX[~]- and X[~]-reports.

My aim in this paper has been to work through some of the considerable confusion that has led the deployment realism debate astray, to clarify that debate, and ultimately to redirect focus on criterion (A). Having sifted our way through discussions pertaining to at least five distinct alternative criteria, we find ourselves returning full circle, to the fundamental insight with which we began, that credit should be given to those constituents that are genuinely deployed in successful predictions. Since criterion (A) has survived each of the criticisms we’ve seen offered by Stanford,¹¹ as well as those criticisms I’ve directed at (A[∅]) (in the last section), we are called to test criterion (A) against the history of science. More specifically, in want of determining whether deployment realism in its strongest form will serve to answer the historical argument, we must test the hypothesis, “those constituents that were genuinely deployed toward, i.e., that led scientists to, key successful predictions are approximately true.” My hope here is to encourage the appropriate case studies. That said, I can at least *indicate* that and why, despite the virtues of this hypothesis and the criterion it contains, (A), I do not think it will suffice as a response to the historical argument. I’ve carefully detailed numerous counterinstances to this hypothesis in my (2006) (and have pointed to others in my (2002)). In particular, exploring the reasoning employed by Kepler, Newton, Leverrier and Adams, I have shown that the following constituents are among those that were a) genuinely deployed in b) novel successes, but which, by the lights of contemporary science, c) fail to qualify as even approximately true.

- 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 .10761 (Leverrier)/.120615 (Adams);
- the longitude of that planet's perihelion is 284°, 45′ (Leverrier)/299°, 11′ (Adams), etc.

The problem with criterion (A) is not that it is inapplicable or unacceptable; on the contrary, I think it is wholly applicable and very well-motivated: we should and can give credit where

¹¹ Put another way, denying that there is any way to tell whether constituents were employed in their successes, Stanford neglects to show that any false constituents were so employed. And, as noted at the end of Section IV, he provides no empirical evidence that there were alternatives to those particular constituents that were genuinely deployed in any successful *novel* predictions. So he leaves unaddressed the question of whether deployment realism will suffice as a response to, not only the (standard) historical argument, but also his own argument regarding unconceived alternatives.

credit is due. I suggest the problem is rather that, given the history of science, the more basic and applicable criterion (A) will not stand as an answer to the historical argument, so will not suffice as a criterion for epistemic realism (or, more specifically, as part of an empirical meta-hypothesis that we can justifiably believe). Nonetheless, despite its counterinstances, it is a criterion that is well-motivated and practicable or applicable. And, having now set aside five other criteria that have significantly diverted the course of the debate, it is a criterion that calls for further testing in the manner indicated in Section II, that of employing empirically informative data reports to adjudicate between the relevant NX^{\sim} - and X^{\sim} -theses.

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