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Scientific realism as the most reasonable choice?*

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Abstract
Scientific realism, roughly, is the view that successful scientific theories are (at least partially or approximately) true. Is this the most reasonable stance to assume towards science? The no-miracle argument says it is: the stunning empirical success of our scientific theories is in need of an explanation, and (partial or approximate) truth seems to be the best explanation that we have at hand. The aim of this paper is to briefly reconstruct the trajectory of the success–to–truth inference, to critically analyze it in its latest formulation, and to sketch a possible way to go in order to make it a safer inference.

1. The «ultimate argument» for scientific realism

September 14, 2015. It is early in the morning, in Louisiana, when gravitational waves are measured for the first time by the twin laser detectors, one situated in Livingstone and the other in Hanford, USA. Einstein was right - so we hear most scientists enthusiastically announce. There are, indeed, ripples in space-time; what we have observed through those laser detectors actually is the *detectable effect* of the merging of two black holes in the distant universe. The pattern of reasoning behind these claims is one philosophers of science are familiar with: the stunning empirical success of our scientific theories is judged to be evidence enough to hold these theories true – at least in part, or to some degree. Empirical success justifies a realist

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attitude towards science, i.e., given their empirical success, it is reasonable to believe that our theories have a certain amount of true content – which goes crucially *beyond* the merely observational level. But how do we exactly get to scientific realism, i.e., how do we get to this allegedly justified belief in the truth-content of our scientific theories, from the *factum* of their empirical success? What is the way to go, is it a safe way and can we make it safer than it is?

A brief terminological clarification, before attempting to answer these questions.

"Scientific realism" does not refer here to any specific position in philosophy of science; it has to be rather conceived as an umbrella-term covering different kinds of positions. Considering what remains invariant among most positions being covered by the term, we might say that scientific realism has to do with a certain truth-commitment. Truth, from a realist point of view, might be seen either as the aim or science – i.e., as something we will reach someday towards the end of the scientific enquiry – or as something that we already achieved, at least partially, by means of our (past and actual) scientific theories. The kind of truth that matters to scientific realists is *theoretical*, and not merely observational truth. Generally, it holds that a subject *S* believes a theory *T* (to be true) when *S* believes that

(i) the (*theoretical* and observational) terms occurring in *T* successfully refer to existing objects;
(ii) *T* assigns to the objects in question the *right set* of properties and relations.

I.e.: to believe a theory to be true means not just believing that what the theory *says there is* actually inhabits our world; it means also to believe that the theory describes the existing objects correctly. Because, note, a theory will not be true unless its existence claims are true; but it could actually be the case that the theory is false, although successfully referential: imagine we take a true theory, we maintain its existence claims and we negate its theoretical ones. The theory thus obtained will refer to existing objects successfully, but everything it will say about them will be false (and it will very likely perform poorly in the prediction of observable phenomena)[1].

Now to the question we started with: what is the way leading us to truth from empirical success? Putnam, in his classic statement of what is known in the literature as the «no-miracle» or «ultimate argument»[2] for scientific realism (henceforth: NMA), claimed there to be an explanatory link bounding together truth and empirical success:

> That terms in mature scientific theories typically refer [...], that the theories accepted in a mature science are typically approximately true, that the same terms can refer to the same thing even when they occur in different theories – these statements are viewed not as necessary truths but as part of the *only* scientific explanation of the success of science [...][3].

In Putnam's formulation, scientific realism is to be favored and it is *compelling* in light of the fact that it provides the *only* explanation for the success of science. If we give up truth and truth-belief, so Putnam, we leave the empirical success of science unexplained – and this is unacceptable from any rational point of view. Putnam's argument was easily objected and confuted appealing to historical counterexamples (i.e., examples of theories of the past that, despite their unquestionable empirical success, turned out to be non-referring and to be associated with the wrong metaphysical picture)[4]. A theory that fails in referring to existing objects, so Laudan, not only cannot be true *simpliciter*, but it cannot even be true in an approximate sense.

Realists have proposed various strategies of refinement of Putnam's argument. One strategy works on the notion of truth, trying to make it appropriately weaker. A parallel strategy works on the notion of empirical success, making it more demanding.

Take the case of an empirically successful theory T_1 that has been falsified and substituted by a better one, T_2. The fact that T_1 is false i.e. untenable when considered as a whole and applied to the whole of reality does not rule out the possibility that (i) T_1 still remains a reliable instrument to pre-

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[1] Musgrave (2006-2007). No scientific realist would claim that we have reason to believe our successful theories to be true *simpliciter*; theories, scientific realists argue, need to be conceived as at least approximately, or partially true.
[2] Van Fraassen (1980, 39).
[3] Putnam (1975, 73)
[4] Laudan (1981, 33).

dict observable phenomena when a certain restricted context of application is concerned and that (ii) we may explain its *restricted* instrumental reliability appealing to the fact that T_1 is, at least, *partially* true. We may explicate the notion of partial truth here involved, as Musgrave, e.g., does, as truth of parts. Theories are made up of a number of discrete elements, at the end of the day; so the idea of partial truth is that a theory which is false considered as a whole might either contain some true elements, or might have some true consequences we can derive from some of its false assumptions (true consequences, we assume, that belong to the intended content of the theory). This, roughly, is the way scientific realists have answered to arguments á-la-Laudan: the theories occurring in Laudan's famous list have been falsified as wholes, they turned out to be untenable when applied to the whole of reality. However, they keep on showing a certain amount of partial empirical success (by which we mean empirical success relative to a specific context of application). And as we would explain *complete* success of theories appealing to truth, we can plausibly explain *partial* success of theories appealing to *partial* truth. And note that, in such a view, it is always truth (not approximate truth but the exact truth of parts), which provides the explanation we need. The realist idea, hence, is *not* that we need to explain the success of a theory appealing to the fact that the theory is true, considered as a whole and applied to the whole of reality. The idea is rather that in the case of empirical success, there needs to be at least *some* true content (beside the merely observational level) that is actually responsible for this success.

The parallel strategy consists in trying to narrow down the set of past theories whose empirical success needs to be explained appealing to partial truth, i.e., to exact truth of parts. In order to count as empirically successful, this is the general idea, it is not enough for a theory to be able to predict known phenomena. It is also necessary that the theory in question generates *novel* predictions – in the sense that the theory either predicts phenomena that are unknown by the time they are predicted (*temporal novelty*) or that the theory predicts phenomena that were not used or taken into consideration when it was formulated (*use novelty*). Novel prediction capacity, not prediction capacity generally speaking, is what must be explained appealing to the exact truth of parts. And as no theory occurring in Laudan's list seems to have entailed novel predictions, his argument is not proving anything at all against scientific realism.

Granted these parallel strategies, here is, roughly, how a refined version of the ultimate argument looks like:

P_1 There are theories showing novel predictive success.
P_2 Approximate truth, or the exact truth of parts, is the best explanation we have for novel predictive success.
P_3 It is reasonable for us to believe the best explanation to be true.
C It is reasonable for us to believe our scientific theories showing novel predictive success to be at least approximately, or partially true.

More precisely, the argument in the current formulation has it that it is rational for us to hold true those parts of a theory that turned out to be essentially responsible for a theory's novel predictive success.

Whether this is a good inference or not depends upon many factors – among them, upon the availability of a criterion to individuate the responsible parts in question and upon the capability to show that no novel prediction, properly conceived, can be generated by an utterly false claim. Another aspect in need of consideration, however, concerns how well truth, so weakened, explains empirical success, so restricted. In what follows, I will try to point out which standards we are apt to apply in judging explanations generally and I will then turn to the question whether the truth–hypothesis, with all the qualifications and restrictions of the case, satisfies these standards or not. The result will be that it does only partially so.

2. Good explanations, justified inferences

Suppose that we are confronted with *P* – where *P* is a phenomenon, fact or state of affairs that occurs and that we are in the position to observe. Imagine that *P* is puzzling to us: given our background knowledge, we cannot make clear sense of it. Given everything else that we know or that we hold to be true about the world, *P* was not to be expected. Hence, we find ourselves wondering: why *P*? How did *P* come about? How can I make sense of *P*? Or: why, *P*, rather than *P**? In other words: we want to understand *P*'s occurrence, and we are in need of an explanation.

After some reasoning or investigation, we realize that, if something else were the case, let us say *H*, *P* would probably have followed. *H* is our explanatory hypothesis: we realize that the probability of *H* being the case, given *P*, it's higher than the probability of *H*, given $\neg P$. Still, we are not yet justified in believing that *H* is, actually, the case: many factors might stand in the way. For example: *P* might raise the probability of *H*, but not enough in order to make our belief in *H* warranted or justified (disbelief or suspension of judgement about *H* might be the doxastic attitudes to be preferred – given, for example, other phenomena or considerations pulling towards $\neg H$). Or: *H* might turn out not to fit our background knowledge, i.e., to be puzzling itself, or to be even *more puzzling* than the phenomenon *P* we are trying to explain. If this were the case, by forming the beliefs that *H*, and that *H* brought about *P*, the coherence of our belief–system would turn out to be highly compromised[5]. If so, even if we were *very* confident that *H* occurred, we probably should suspend judgement on whether *H* – at least, until we are in the position to rearrange our belief–system appropriately, to the effect that we can make proper sense of *H*, and save consistency. Or: there might be different explanatory hypotheses *H** and *H*** that seem to explain *P better* than *H* does, given the evidence we have and the background knowledge we rely on. In this case, we should go for one of them and disregard *H*.

The idea is: we can and should believe that *H* is the case, in light of *P*, if *H* turns out the best explanation we have in the context in which *P* arises. This means, at least: *P* should raise the probability of *H* enough to make the belief in *H* reasonable; the acceptance of *H* should not make the coherence of our belief system sink significantly; and *H* must have proven to be better than other explanatory hypotheses, if there are some available[6]. Here is how the argument roughly goes:

P_1 *P* is the case.
P_2 *H* would best explain *P* in context *C*.
P_3 It is reasonable for us to believe the best explanation to be true.
C It is reasonable for us to believe that *H* is the case in context *C*.

Note what we are *not* doing here: we are inferring the *explanans* from the *explanandum*, but we are not deducing it. We are not deducing *H* in light of *P*, and in light of $H \rightarrow P$ (this would be a logical fallacy, a case of affirming the consequent). What we're doing is taking *P*, i.e., the necessary condition, as a sign, as a marker of *H,* the sufficient one. The presence of the necessary condition in a given context makes it *reasonable for us to believe* that the sufficient condition is actually instantiated. *P* (either alone, or together with other pieces or evidence) is evidence enough for *H*.

[5] See Schurz and Lambert (1994).
[6] See Lipton (1991 and 1993) and Douven (2011).

As an example, consider the following case.

P = high level of white blood cells in the blood of a patient;
H = pneumonia.

P_1 A patient shows a high level of white blood cells.
P_2 Pneumonia would best explain a high level of white blood cells in the present context.
P_3 It is reasonable for us to believe the best explanation to be true.
C It is reasonable for us to believe that the patient has pneumonia.

The crucial point, here, is this: what does it take it for pneumonia to be the best explanation for a high level of white blood cells in the context we have? I.e.: when is the belief that the patient has pneumonia a reasonable one, given that a high level of white blood cells is the evidence we can count on?

Suppose we know it for a fact that the probability of pneumonia, given a high level of white blood cells, is higher than the probability of pneumonia, given the fact that the white blood cells level is normal. Intuitively, although the point is settled, this is not reason enough in order to make a diagnosis of pneumonia. The interesting point, however, is why it is not.

At least the following three scenarios (or a combination of these) might occur. First: the pneumonia–hypothesis might not fit our background knowledge. We might know it for a fact, e.g., that the patient is vaccinated against the most common pneumonia–types; or we might be extremely confident that pneumonia has been completely eradicated (because, for example, the WHO made an official statement in this sense yesterday, and we read about it). Second: the symptom might raise the probability of pneumonia, but without being *conclusive*, or even without being *good evidence* for the disease. It's true that most patients affected by pneumonia show a high level of white blood cells in her blood; the symptom, however, is associated with many different cases of infection. These different infections, hence, must be considered as alternative explanatory hypothesis before being dismissed. Third: pneumonia might turn out to be a *bad* explanation also in the complete *absence* of any alternative explanatory hypotheses. For example: it might explain too little (e.g.: it may leave facts about the patient's condition unexplained), or be too demanding (e.g.: the patient might not show other typical pneumonia-signs). Pneumonia, in order to be reasonably inferable, must:

(i) Be coherent with our *background knowledge*;
(ii) *fit* the evidence;
(iii) fit the evidence *better* than other explanatory hypotheses do.

The term "fitting" occurring in condition (ii) and (iii) needs some brief further explication. An explanatory hypothesis fits the evidence not just when it's compatible, or *coherent* with the phenomena or data at hand (i.e.: not just when, given the explanatory hypothesis, the phenomena in question were *ceteris paribus* to be expected). An explanatory hypothesis H fits the evidence E_1, E_2, E_3, when it's *reasonable* to hold H, in light of E_1, E_2, E_3 (the evidence we have, i.e., is evidence *enough* to justify our belief in H).

All these conditions, it seems, must be satisfied in order to make a diagnosis of pneumonia reasonable. Still: if one of them is not, or even if none of them is, we are not in the position to conclude, i.e., to know it for a fact that pneumonia is absent. The inconsistency with our background knowledge might arise because of false beliefs we are holding about the domain in question (maybe we read fake news about the WHO statement). Something might have gone wrong in our evaluation of the evidence. There might be some evidence we have not yet considered and that we should have had. There may be some evidence that we do not yet know about, and that *defeats* the evidence allegedly supporting the alternative explanatory hypotheses. Still, if one condition is not fulfilled, the reasonable thing to do will be suspending judgement on whether the patient has pneumonia or not, and go on inquiring until we know better.

This very simple example, it seems to me, makes us appreciate something relevant. Abductive inference is usually not about jumping from a necessary to a sufficient condition. It is, rather, about carefully evaluating the evidence we have; and if the evidence we have is not enough, about gathering all the evidence we can in order to settle the question we are dealing with. Does the patient have pneumonia or not? This, it seems, is not a question we can settle considering just the level of white blood cells in her blood. The fact that the level of white blood cells in her blood is high may be *a reason* to think that the patient has pneumonia, but it is not a conclusive one, and probably it is not even good. In order to make a diagnosis of pneumonia reasonable, we need further reasons, and we need good ones.

This is why, in the usual situation, we are apt to look at the condition of the patient as a whole. We know more or less what pneumonia is. We know it to be always associated with certain signs and symptoms *besides* a high level of white blood cells – fever, cough and fatigue, to mention a few. To settle the issue, therefore, we check whether the patient shows the symptoms in question or not. If she shows fever, chest pain and fatigue, we give support to our pneumonia–hypothesis; if she just shows fever, but neither chest pain nor fatigue, we take it as a sign that, probably, we need to revise it.

3. Inferring partial truth

In the case of NMA, we have the novel predictive success of science as the fact in need to be explained, on the one side, and we have partial truth (i.e., the truth of the theory's parts responsible for its novel predictive success) as tentative explanatory hypothesis, on the other. The question I will be dealing with in this section is whether truth, so conceived, is a good explanatory hypothesis for novel predictive success – in light of the standards we are apt to apply in judging explanations generally.

This is, roughly, what emerges from our pneumonia–example in the previous section. In order for *H* (partial truth, or the exact truth of parts) to be the best explanation for *P* (novel predictive success), at least the following conditions need to be fulfilled:

(i) *H* must be coherent with our background knowledge;
(ii) *H* must fit the evidence (i.e. the evidence we have must be good evidence, or evidence enough in order to justify our belief in *H*);
(iii) *H* must fit the evidence better than other explanatory hypotheses do.

Let us start by considering condition (iii). Realists were able to show that all antirealists' attempts to explain success (broadly conceived, or novel) fail, or has failed so far[7]. Consider, briefly, van Fraassen Darwinian explanation of the success of science. Why do mice escape from predators? This fact is not a miracle to a Darwinian mind: all mice that failed to do so have been eaten. Analogously for scientific theories: only the one that were empirically successful (or: that showed novel predictive success) survived, because we just abandoned the ones that were not. In the environment theories happen to live in, being successful is, and has always been, a feature

[7] See Alai (2014b).

promoting survival[8]. Realists have replied to this pointing out that van Fraassen misses the point of the realist why–question completely: he is explaining to us why we have the kind of theories we have, not why the theories we have possess the specific feature they do. These are two completely different questions – and in a Darwinian explanation the very *reason* for success is left, essentially, unexplained[9]. Leplin's "surrealism", or "surrogate realism" (that suggests we should commit ourselves to the belief that everything in the world behaves exactly *as if* our theories were true) is apparently no better – as Musgrave, among others, has shown: saying that everything looks as if our theories were true does not provide us with any explanation whatsoever for the fact that our theories are empirically successful; it is just a way of restating the very fact of their empirical success[10]. Stanford, instead, has suggested that we may explain the empirical success of a (false) theory T_1 appealing to its predictive similarity to another theory T_2, where T_2 is the true account of the domain of enquiry of T_1. The explanation Stanford suggests is meant to be anti-realistic, in the sense that it does not *directly* appeal to a semantic relation between the theory and the world in order to explain its empirical success; the success of a theory T_1 is not explained appealing to its *own* truth, but appealing to the truth of another theory T_2, which T_1 bears predictive similarity to[11]. Psillos has convincingly proved this account to be inadequate, reflecting on the formal features of predictive similarity and pointing to the counterintuitive scenarios Stanford's account leads us to[12]. As far as we know today, then, we can take condition (iii) to be satisfied.

Let us now turn to condition (i). Recall the situation we are in. We are in need of an explanation: our scientific theories show stunning predictive success (call this fact *P*), and we find ourselves wondering *how come*? How

[8] van Fraassen (1980, 40).
[9] Leplin (1997a), Ladyman&Ross (2007, 73-74).
[10] Musgrave (2006-2007).
[11] Stanford (2000, 272-277).
[12] Imagine we have three theories, T_1, T_2 and T. T_1 and T_2 are both false and empirical successful, while T is the true account for the domain of enquiry in question. In Stanford's account, we should explain the empirical success of T_1 and T_2 appealing to the fact that they are both predictively similar to T. Now, predictive similarity is symmetric, so we might say that as long as T_2 is predictively similar to T, also T must be predictively similar to T_2. Suppose we hold that (i) T_1 is predictively similar to T and that (ii) T is predictively similar to T_2. Now, as predictive similarity is *not* transitive, we get to the counterintuitive result that we cannot infer that T_1 is predictively similar to T_2 from the conjunction of (i) and (ii). See Psillos (2001, 353-354).

did *P* come about? Why *P*, rather than *P**? How are we to make sense of *P*? Realists provides us with an explanatory framework where *P* can be embedded. They tell us, roughly, that where *P* was the case, there was some true content crucially *responsible* for *P* (e.g., in our starting example: the reason why scientist were able to detect gravitational waves is that Einstein was right about the merging of the black holes in the distance universe). Is this explanatory framework appropriate, given our background knowledge? One might initially be apt to suppose that a subject confronted with Laudan's famous list would find truth as explanatory hypothesis puzzling. If even a theory that is false (i.e. that fails in referring to existing objects) can show empirical success, if looking at the history of science what we see is a track record of false and empirically successful theories, truth cannot be the explanation we need. However:

(a) the explanatory framework provided by scientific realists is not particularly wide in scope. Given the set of all phenomena predicted by a theory, only a limited subset of these (i.e., the phenomena that were unknown by the time they were predicted or that were not used essentially or taken into consideration in formulating the theory) is in need to be explained.

(b) it is not truth *simpliciter*, but rather only the exact truth of parts, that is doing the explaining.

Once (a) and (b) are appreciated, the explanatory link regains *prima facie* plausibility. Condition (i), hence, seems to be satisfied, too.

Let us now finally turn to condition (ii). The question that remains to be settled is this: is novel predictive success *good* evidence, and is it *evidence enough* for the belief in the partial truth–hypothesis? Remember that, in order to make an explanatory hypothesis reasonably acceptable, it is usually not enough that the probability of the hypothesis, given a certain piece of evidence, is higher than the probability of the hypothesis, given that the piece of evidence in question is absent. A piece of evidence may make the probability of the hypothesis rise but suspension of judgement on whether the hypothesis is, actually, the case might still be the doxastic attitude to be preferred. Besides that, recall the lesson I claimed we had to learn from the pneumonia–case: abductive inference is usually *not* about jumping from a necessary to a sufficient condition; rather, it is a matter of carefully evaluating the evidence one has, and of gathering more evidence in case the evidence available is not evidence enough. Does our patient have pneumonia or not? Usually, we cannot settle this question by considering only the level of white blood cells in her blood. This might well be *a reason* to believe that the patient has the disease in question. But in order to make a diagnosis of pneumonia reasonable, we need further reasons. And as we know pneumonia to be associated with a bundle of signs and symptoms besides a high level of white blood cells, we can check the general condition of the patient in order to settle the issue. If a high level of white blood cells is everything we can count on, the most reasonable thing to do will be suspending judgement on whether the patient has pneumonia or not, until we know better. Suspending judgement on whether *H* (where *H* is an explanatory hypothesis for a certain phenomenon), however, does not mean giving up every effort in settling the issue of whether *H*. On the contrary: if we suspend judgement on whether *H*, we are apt to assume an inquiring attitude towards *H*. We are apt to weight *H* against ¬*H*, and to do the best we can in order to gather the evidence that might help us in settling the issue.

Now, can we do something like this in the case of NMA? Can we eventually gather pieces of evidence in order to reach the threshold that makes the belief in the truth-hypothesis reasonable? Because note, if we cannot, either condition (ii) is not satisfied, or novel predictive success must have a very special evidential status.

4. Novel predictive success as perfect pathognomonic symptom?

Not all diagnostic cases resemble the pneumonia-case depicted above. There are certain symptoms, in medicine, that have a very special evidential status. They are called *perfect pathognomonic symptoms.* The term "pathognomonic" comes from ancient Greek πάθος (disease) – γιγνώσκω (I know, I recognize). A perfect pathognomonic symptom is a symptom so specific that its presence *univocally* indicates the presence of a certain disease and its absence *univocally* indicates the absence of the same. Perfect pathognomonic symptoms differ from imperfect ones in the sense that in the case of an imperfect pathognomonic symptom the presence of the symptom univocally indicates the presence of the disease but the absence of the disease in question cannot be inferred from the absence of the symptom. An example of a perfect pathognomonic symptom is the *erythema chronicum migrans*, a cutaneous rash of regular circular shape, which is the only manifestation of Lyme disease that is sufficiently distinctive to allow clinical diagnosis in the absence of laboratory confirmation. Symptoms of this specific kind are not just evidence for the corresponding disease; they are *conclusive* evidence.

Is this what scientific realists have in mind, when they say that partial truth best explains novel predictive success? Is novel predictive success to be conceived as a kind of perfect pathognomonic symptom of partial truth? The realist strategy of refinement of the success–to–truth inference reconstructed in the first section suggests that this could be the case: every time we encounter novel predictive success, *there must be* a true content responsible for this success. It cannot be the case for a theory to show novel predictive success, and not to be at least partially true. The interpretation of novel predictive success as a perfect pathognomonic symptom of partial truth, however, it is a claim that needs an independent argument; besides that, it might turn out to be problematic.

Suppose we have a certain theory *T*. For a theory to show novel predictive success, it is enough that given the whole set *S* of phenomena that *T* was meant to predict, there is a non-empty subset *S** of *S*, the members of which were unknown by the time they were predicted and/or not taken into account in formulating *T*. Suppose now that, as long as *T* is concerned, set *S** has exactly one member, and *S* without *S** is empty. I.e.: *T* was able to make exactly one novel prediction, while everything else it observationally implied turned out to be false. If novel predictive success is really to be conceived as a perfect pathognomonic symptom of partial truth, we ought to believe that *T* is partially true. But, especially in the absence of a criterion to clearly identify the part(s) responsible for the novel predictive success in question, wouldn't it be more rational to weight the amount of success of *T* against the amount of failure, and to adjust our degree of belief in *T* (and/or in *T*'s parts) accordingly?

Besides that, there is a further worry. For explanations generally, we do have the rational expectation that an explanation is fruitful and epistemically valuable for us not just because it helps us in making sense of a single, isolated fact but also because it helps us in making sense of a *bundle* of interconnected facts altogether. Think of pneumonia: if it turns out to be the best explanation for a high level of white blood cells in the blood of a patient in a certain context *C*, then it will help us in making sense of the condition of the patient as a whole. The pneumonia-hypothesis will not just explain the fact that the level of white blood cells is high; it will also explain the fact that the patient in question, in context *C*, shows chest pain, and not, e.g., headache. The same applies to the case of Lyme disease, even if the diagnosis is immediate and unproblematic in the presence of a cutaneous rash of a certain shape.

Now, what might this bundle of facts be, for the case of partial truth? What might the truth–hypothesis be epistemically valuable for, besides as an explanation for novel predictive success? Note that if scientific realists were able to answer these questions, at least tentatively, they would have a way to give support to the truth–hypothesis *independently* from novel predictive success (and maybe independently from empirical success, broadly conceived). This would make their success–to–truth inference a safer and stronger inference, and they would have an alternative way to go in case, e.g., antirealists were able to convincingly show that a novel prediction can actually be derived from an utterly false claim[13]. Here are three tentative suggestions of what might count as a truth–indicator, or truth–symptom, alongside empirical success. An indication of the partial truth of a theory *T* might be:

(i) the fact that *T* played an essential role in the development and formulation of another theory, *T**, that turned out to show greater predictive and explanatory power than *T*;
(ii) the fact that *T* has heuristic value, in the sense that it generates new theoretical questions and opens new avenues of research;
(iii) the fact that the image of the world provided by *T* is internally consistent, and that it can be conjoined with the so–called *immanent image* without generating dramatic inconsistencies in our system of beliefs[14].

These are just schematic suggestions, in need to be spelled out in more detail and to be developed further. But if the way briefly sketched here is feasible, and if it does not turn out to be a dead end, even granted the antirealist claim that it is possible for an utterly false claim to bring about a novel prediction, and even granted that novel predictive success cannot be proved

[13] Lyons (2002).
[14] See Sellars (1963) and Allori (2013). This criterion could be made even stronger, eventually demanding that *T* provides us with an explanatory scheme where the assumptions belonging to the immanent image can be properly embedded.

to be a perfect pathognomonic symptom of partial truth, not everything would be lost for the success–to–truth inference.

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