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Why Does Laudan's Confutation of Convergent Realism Fail?

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SUMMARY: *In his paper "A Confutation of Convergent Realism", Larry Laudan offered one of the most powerful criticisms of scientific realism. I defend here that although Laudan's criticism is right, this does not refute the realist position. The thesis that Laudan confutes is a much stronger thesis than that which the realist needs to maintain. As I will exemplify with Salmon's statistical-relevance model, less strict notion of explanation would allow us to claim that (approximate) truth is the best explanation for such success, even if it is accepted that there can be cases of unsuccessful (approximately) true theories and cases of successful false theories.*

Key words: convergent realism, Larry Laudan, scientific success, scientific realism, pessimistic meta-induction

1. INTRODUCTION

Science is a very successful activity. This success has many faces, but could be summarised by two noteworthy achievements: a great predictive ability, which is especially evident in the prediction of novel phenomena, and a great ability to transform the world by means of technological instruments. Moreover, these abilities have improved throughout the history. New scientific theories have deployed, sometimes long after their acceptance, greater predictive and practical abilities than previous ones. Quantum theory, for example, has surpassed in this respect all preceding theories.

The predictive and instrumental success of science is extremely important from an epistemological and practical point of view, and it demands an explanation. First, given the physical and biological limits of human beings, it is surprising indeed that we have such a powerful tool to modify reality. Moreover, other forms of knowledge have

not achieved comparable results and therefore the reasons as to why this is the case invite scrutiny.

Scientific realism accounts for the predictive and instrumental success of science by means of the (approximate) truth or truthlikeness of scientific theories. In an informal but adequate way, truthlikeness can be defined as the conjunction of approximate truth and a high informative content (cf. Niiniluoto 1999). For an outstanding realist such as Richard Boyd, the approximate truth of scientific theories explains the instrumental reliability of scientific methods, which are theory-dependent. This reliability of methods explains in turn, in a dialectic way, the approximate truth of new theories (cf. Boyd 1996). Many other scientific realists have supported the idea that if the theoretical entities postulated by scientific theories did not exist at all, and if these theories were not approximately true, then the success of science would be a miracle (Smart 1963, and Putnam 1975 and 1978 are classical references).

One of the most important criticisms of this realist thesis, and, in case of being right, one of the most harmful, is displayed by Laudan in his 1981 article "A Confutation of Convergent Realism" (which I will quote from the 1996 reprint). Sometimes it is named as 'the pessimistic induction', or 'the pessimistic meta-induction'.

The aim of this paper is to evaluate Laudan's argument against realism. I will defend that Laudan is right in his criticism of the thesis that approximate truth implies predictive and instrumental success, but this criticism does not destroy the realist position. The thesis that Laudan confutes is a much stronger thesis than realist needs to maintain. As I will exemplify with Salmon's statistical-relevance model, a less strict notion of explanation would allow for the claim that (approximate) truth is the best explanation for such success, even if it is accepted that there can be cases of unsuccessful (approximately) true theories and cases of successful false theories.

2. HOW TO AGREE WITH LAUDAN AND REMAIN ANCHORED IN REALISM

According to Laudan, the realist is committed in his argument to the following two theses:

(T_1) If a theory is approximately true, then it will be explanatorily successful.

(T_2) If a theory is explanatorily successful, then it is probably approximately true.

Although only the explanatory success is mentioned here, Laudan includes the predictive success as well in his characterisation of success. The first thesis is called 'the downward path' and the second 'the upward path'. In his article, Laudan attempts to show that both theses are false.

Laudan's argument to refuse T_1 is short and simple. Whereas it is self-evident that a true theory will be a successful theory, since the conclusions derived from it must

be true, the logic of approximate truth does not allow for the same argument about an approximately true theory. The consequences inferred from an approximately true theory do not have to be approximately true. A theory can be approximately true, and yet all of its tested consequences could be false (cf. Laudan 1996, 119). So the approximate truth does not ensure the predictive success. Furthermore, we lack an adequate criterion for the ascription of approximate truth to a theory.

Regardless of the degree of agreement with the reasons adduced by Laudan,[1] the realist should admit –and this is my first claim– that truthlikeness is not a sufficient condition for predictive and instrumental success. In other words, he should admit that the downward path is not always clear. More things than approximate truth are needed in order to be a predictive and instrumentally successful theory. It is necessary to have a set, as complete as possible, of right auxiliary hypotheses and data about initial conditions. Without them, predictions fail or cannot be realised. It is also necessary to specify how the theory can be technologically applied. It is not always easy, immediate or feasible to obtain technological norms from a given theory, but without them the theory will not be applicable in practice, however truthlike it may be. It is, then, possible that approximately true theories, or even highly truthlike or true theories, lack predictive and instrumental success. The realist could and should accept this conclusion.

Nevertheless, he could retain the weaker thesis that a high truthlikeness usually leads to predictive and practical success when other appropriate circumstances are given. After all, Laudan himself does not deny the possibility of some connection between success and approximate truth; he just claims to argue for it in an independent way (cf. Laudan 1996, 118-9).

In order to show that the upward path is not clear either, Laudan assumes that "a realist would never want to say that a theory is approximately true if its central terms failed to refer" (Laudan 1996, 121). Once he establishes this principle, Laudan then mentions several examples of successful past theories that nowadays we consider as non-referential with respect to their central terms. The crystalline spheres of ancient astronomy, the phlogiston theory, the caloric theory, the electromagnetic ether, the optical ether and the theories of spontaneous generation are among them.

From the publication of Laudan's article, the realist strategies to respond to his criticism have proliferated (cf. Niiniluoto 1999, 190-192). The following are the most interesting:

(I) To deny the relevance of mentioned examples since most of them are not taken from *mature* sciences. These failures in reference are not to be expected in many of the current sciences, where the methodological controls have increased significantly (cf. Hardin and Rosenberg 1982, Devitt 1984, 146, Boyd 1996, and Worrall 1996).

(II) To deny that they really are successful theories, especially in relation to their capacity to make novel predictions (cf. Musgrave 1988, McAllister 1993, and Leplin 1997, ch. 6).

(III) To use a less strict concept of reference, so that mentioned theories would not fail to refer and they could be, then, considered as approximately true theories. Partial reference, approximate reference, heterogeneous reference potential, principle of charity, etc. are some proposals (cf. Hardin and Rosenberg 1982, Devitt 1984, 147-149, Cummiskey 1992, Psillos 1994, Kitcher 1993, 141-149, and Niiniluoto 1999, 129-132).

(IV) To maintain that theories whose central terms fail to refer may be, in spite of that, approximately true theories (cf. Hardin and Rosenberg 1982, Niiniluoto 1984, 182-183, and 1999, 190-192, and Psillos 1994).

(V) To argue that the theoretical constituents that included non-referential or manifestly wrong terms did not play any indispensable role in the success of the mentioned theories (cf. McMullin 1984, Kitcher 1993, Psillos 1994 and 1996 a, and Leplin 1997, chap. 6).

However, each of these strategies has weak points. The first one only allows us to reduce the list of counter-examples. Furthermore, it does not explain the success of the discarded "immature" theories, nor does it explain why we should expect non-referring theories not to be successful in mature sciences. If there can be success without reference in immature sciences, why not in mature sciences? Finally, it introduces an element of arbitrariness when it comes to deciding what a mature science is and what it is not (cf. Leplin 1997, 141). The second strategy is much better, but the concept of novel prediction on which it is based is somewhat controversial (cf. Brush 1994). On the other hand, it is not clear that non-referring theories have made no novel predictions at all. Martin Carrier mentions two examples: phlogiston theory predicted the reductive properties of hydrogen, and caloric theory predicted the equality of thermal expansion of all gases (cf. Carrier 1991). Using strategies III or IV, the realist could reply that these theories were approximately true. However, both strategies are also problematic. The third strategy could become too generous in its conception of reference, for how far has the concept to be expanded in order to permit that the crystalline spheres, the epicycles, the vital force, or the four humours can be referential concepts? Something similar can be said about the fourth strategy. As André Kukla explains, the danger here is that "the more liberal we make our construal of approximate truth, the more likely it is to succumb to the charge that the approximate truth of our theories doesn't license our taking a realist attitude toward them." (Kukla 1998, 15).

The fifth strategy demands rigorous historical analyses, which remain undeveloped (and this is its main weakness), but it seems to be the most hopeful. As Kitcher notes, it is not enough to indicate that geological theories prior to 1960's were successful but not approximately true, since they denied the lateral motion of continents. In order to take Laudan's point, it would be necessary to show that the denial of the lateral motion of continents actually plays some role in the success of these geological theories (cf. Kitcher 1993, 142).

In accordance with this idea, Niiniluoto (1999, 190) proposes to replace Laudan's thesis T_2 by another thesis T_2' less misleading and simple:

(T_2') If a theory is empirically successful, and its theoretical postulates are indispensable to the derivation of the empirical consequences, then the theory is probably approximately true (or probably truthlike).

Although these replies deserve careful attention, I think that the realist has to side with Laudan here too. Throughout history of science there have been theories which could not be labelled as approximately true in spite of the fact that they enjoyed relative success.

The astronomy of Ptolemy is the most outstanding example. Ptolemy's epicycles (*pace* Niiniluoto 1999, 192) cannot be considered as approximately true, because they are not even remotely connected with the actual mechanisms that cause planetary motions.[2] However, these epicycles played an indispensable role in fact in the success of Ptolemy's theory. They were the main tool used to reproduce the observed data. It is true that, as Niiniluoto points out, "they became necessary only after Ptolemy's false assumption that the earth is the immovable centre of the system." (*ibid.*). But, precisely for this reason, they were indispensable *for Ptolemy's theory*. They were not an irrelevant assumption for achieving success, as was the denial of lateral motion of continents in geological theories prior to 1960's. Without them, the theory would not have had the success it had. And it cannot be denied that Ptolemy's system was explicative and instrumentally successful, although it was unable to predict novel facts in a strict sense (that is, excluding eclipses). If all that is true, T_2' cannot always be right. And indeed Niiniluoto admits its fallibility and the possibility of counterexamples.

But if we agree with Laudan that approximate truth does not assure predictive and instrumental success, and that predictive and instrumental success does not always mean approximate truth, what remains of the realist thesis about truthlikeness as an explanation of scientific success?

I think that even in that case the realist could defend truthlikeness as the best explanation of scientific success. As a matter of fact, Laudan does not deny that successful theories may be approximately true. What he denies, as Psillos (1999, 102) remarks, is that "there is an *explanatory connection* between empirical success and truth-likeness" which should warrant such a claim. But it can be argued that Laudan does not find this explanatory connection mainly because his presuppositions are restrictive. Laudan's argument states that there cannot be such an explanatory connection, since there are cases of approximately true theories without success and cases of successful theories which are not approximately true. Laudan seems, then, to assume that the approximate truth could explain the success only if every approximately true theory is successful and if a successful theory is probably true. In other words, the approximate truth should imply success. That is at least what can be inferred from his characterisation of realism by means of theses T_1 and T_2. But a less strict criterion for explanation would allow for an explanatory connection between approximate truth and success even if we concede that the counterexamples mentioned by Laudan are right. I propose to take to this aim –only for the sake of argument, and without assuming its general validity– the statistical-relevance model of explanation (cf. Salmon *et al.* 1971).[3]

Let us recall that, according to this model (and in a simplified way), *C* is *positively relevant* to the occurrence of *B* and, thereafter, it can be an explanation of *B* if $p\,(B / A \bullet C) > p\,(B / A)$, that is, if the probability of *B* given *A* and *C* is greater than the probability of *B* given only *A*. Applying this to our issue, we could say that the approximate truth of a theory can explain its success if the probability of being successful (*B*), given the fact of being a theory (*A*) and being approximately true (*C*), is greater than the probability of being successful for a theory in general.

More specifically, according to the statistical-relevance model, the explanation would have this form:

Question to answer: Why is *x*, which is a scientific theory, successful?

Let us consider

A: the class of scientific theories,
B: the class of successful scientific theories,
C_1: the class of approximately true scientific theories,
C_2: the class of non-approximately true scientific theories.

We assume that C_1 and C_2 make a homogeneous partition of *A* with respect to *B*, that is, $A \bullet C_1$ and $A \bullet C_2$ are mutually exclusive and exhaustive cells.

Let us also suppose that $p\,(B / A \bullet C_1) = 0.3$ and $p\,(B / A \bullet C_2) = 0.01$.

The explanation would be then as follows:

$$p\,(B / A \bullet C_1) = 0.3$$
$$p\,(B / A \bullet C_2) = 0.01$$
$$x \in C_1$$

This explanation states that *x* was successful because it belonged to the class C_1 of approximately true theories, which is a (positive) relevant factor to be successful.

As it can be noted, we have selected the data in order to disallow inferring from the fact that a theory is approximately true that is also successful, nor vice versa. But, in spite of this, the approximate truth would be a relevant factor to explain success. What is important here is that the probability of the first circumstance should be greater than the probability of the second one, but it is not necessary for it to be a high probability. We can see, then, why Laudan is too strict in his criticism of the realist's argument. In order to accept that the approximate truth can explain the success of a theory, he demands that $p\,(B / A \bullet C_1)$ be 1 and $p\,(B / A \bullet C_2)$ be 0.

Similarly, we can see that the pessimistic induction would be a compelling argument against realism only if a significant number of historical case studies reveals that $p\,(B / A \bullet C_2) \geq p\,(B / A \bullet C_1)$. But this is something that Laudan does not show. He only assures, after listing twelve examples of non-referring but successful theories, that the list "could be extended *ad nauseam*." (Laudan 1996, 122). No argument, however, supports this statement. Obviously, the realist has a very different view of the issue. For

the realist, if there is an inductive conclusion to be drawn from the history of science, it is an optimistic one: there seem to be more cases of genuine reference (and of approximate truth) joined with predictive and instrumental success than not. Regardless, this is a matter that only should be decided upon empirically and after numerous historical analyses. What I have tried to argue here is that some examples of non-referring but successful theories do not make a strong enough case.

I am not suggesting that the statistical-relevance model is indisputable. It is fairly easy to find examples in which statistical relevance does not seem to provide an explanation for a given fact. Salmon himself completed it with a causal mechanical model of explanation in which it is causal relevance and not statistical relevance that has genuine explanatory import (cf. Salmon 1984). I use the statistical-relevance model just to make clear that it is possible to accept with Laudan that the approximate truth does not imply success and to consider still that the approximate truth may explain the success of many scientific theories.

The realist's claim can be illustrated with a well-known example by van Fraassen. Every time that the cheese disappears in the house, it is not necessarily because of the presence of a mouse, and every time that there is a mouse in the house the cheese does not necessarily disappear. But if the cheese has gone missing on some occasion, the best explanation is that there is a mouse in the house. Any other explanation would require more unlikely hypotheses. The cheese might occasionally disappear because it was stolen by a neighbour or because it was devoured by ants, but its disappearance is usually caused by a mouse. Similarly, scientific success could be due to different causes (to chance, to the use of false but empirically adequate theories (as in Ptolemy's system)[4], to some pre-established harmony, to the divine providence, etc.); for the realist, however, it is sufficient that in a significant number of cases the cause be truthlikeness.

The realist thinks that, if some additional conditions are given, it is probable for truthlike theories to be predictive and instrumentally successful. These theories provide us with an approximately true knowledge about natural phenomena, which can be used in a reliable way to manipulate objects, to predict and control their behaviour, and to do things with them. Without it, prediction and control would be, if not impossible, very difficult to carry out. The antirealist is right when s/he adduces that from false premises true consequences also can be derived, but it cannot be expected that we frequently draw relevant consequences able to be used in practice from false theories.[5]

All this means that the realist's account should be applied to long periods in history of science, but not to every episode of success. The realist can admit, as noted earlier, that in occasional circumstances scientific success is due to a different cause, but a lengthy and reiterated success of a theory in very different contexts is for him a sign –of course fallible– that there is more than a simple empirical adequacy between the theory and the reality. Consider the following analogy. Army A has won the war against army B, and army A is more numerous, better trained, and better armed than army B. In those circumstances, the superiority of A in number of soldiers, training, and arms is the best explanation of its victory over B, because that is what can be expected if we have no other information. It does not imply, however, that army A won all

battles against army B, or that every victory of A over B has been due to these reasons. The morale of army B might have been on some occasion higher than the morale of A, so that B won a battle against A despite its inferiority. Likewise, A could sometimes defeat B not because of its superiority, but because of the bad weather or another accidental matter.[6]

3. CONCLUSIONS

Laudan's criticism of the realist explanation of scientific success is only valid against a very strong form of realism. At best, Laudan has shown that approximate truth does not always come together with predictive and instrumental success: one can have approximate truth without success and success without approximate truth. But this is something that the realist is prepared to admit, and it does not confute the thesis that approximate truth is the best explanation for the success of science. Laudan assumes that approximate truth can explain the success of science only if approximate truth implies success and success is highly probable with approximate truth. However, this is a very strong demand for an explanation. A less strict notion of explanation would allow for the claim that (approximate) truth is the best explanation for such success, even if it is accepted that there can be cases of unsuccessful (approximately) true theories and cases of successful false theories.

Notes

1. Niiniluoto has replied to these objections. Firstly, his concept of expected degree of verisimilitude provides a fallible epistemic criterion for the ascription of approximate truth to a theory (cf. Niiniluto 1984: 179). Secondly, it follows from Niiniluoto's definition of truthlikeness that under some conditions if a theory is truthlike, the degree of approximate truth of its deductive consequences has to be relatively high (cf. Niiniluoto 1999: 194-196).

2. Despite the difficulty in making the idea more precise, I adopt here Leplin's words: "The difference between a judgement of partial truth and a judgement of outright falsity depends on whether or not the ways in which the theory departed from the truth, as currently reckoned, are important to our current interests. If believing the theory, rather than what we now believe, would not radically alter current directions of theoretical work, but only set that work back a ways –would not mislead us but only lead us less far– then the theory's falsity is less important to us than its truth, and it makes more sense to regard it as partially true than as simply false. Thus, we are not inclined to regard geocentric physics or phlogistic chemistry as partially true, but we are inclined so to regard Newtonian theory or special relativity. The advances

to which the latter theories contributed outweigh their mistakes, from our current perspective." (Leplin 1997, p. 134).

3. A similar suggestion is made by J. R. Brown, although he prefers to develop the idea that realism provides a narrative explanation for the success of science (cf. Brown 1994, 20-25). I think, however, that the use of Salmon's statistical-relevance model of explanation, as I try to show in this paper, gives a simpler and more convincing answer to Laudan's criticism.

4. The success of epicycles to fit the planets' apparent motions is less surprising when we consider that adequate combinations of epicycles and rotation speeds are able to generate an infinite variety of bilateral symmetry curves (ellipses, ovoids, etc.) and even rectilinear, triangular and square paths (cf. Hanson 1973: 2, I).

5. Even Laudan seems to admit that when he argues that genuinely referential theories need not be successful, "since such theories may be 'massively false'." (Laudan 1996: 113).

6. It is well known that the realist's difficulties do not come to an end with an answer to the pessimistic meta-induction. He must cope with further criticisms. One discussed at length is the anti-realist refusal of the legitimacy of the abductive inference on which realism is based. This inference starts from the thesis that realism has the better explanation of scientific success to conclude that realism is true. Van Fraassen calls it 'the *Ultimate Argument*". He thinks that the inference to the best explanation, as used by realist in this case, is circular, since it presupposes the truth of realism in order to prove the truth of realism. When the realist follows the abductive rule of inference, he assumes that we are always willing to believe that the theory that best explains the evidence is true. But this is precisely what the anti-realist denies. For the anti-realist the fact that a hypothesis is the best explanation of a phenomenon is not a guarantee of its truth (cf. van Fraassen 1980, 20; similar objections are raised by Fine 1986 and Laudan 1996). I have tried elsewhere to defend realism from this criticism (cf. Diéguez 1998, chap. 4). Very cogent replies to this and other of van Fraassen's objections can be found in Psillos 1996 b and 1999, and Okasha 2000 (and for a recent defence of van Fraassen's thesis see Ladyman, Douven, Horsten and van Fraassen 1997).

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