ISSA Proceedings 2014 – On The Persuasive Power Of The Best Explanation Argument

Abstract: Scientific realists claim that scientific realism must be accepted because it is the best explanation of the success of science. But arguments to the best explanation are objectionable. We explore the possibility that the greater or lesser resistance to those inferences depends on differences about the persuasion criteria that correspond to each context: participants of philosophical discussions usually apply stricter criteria than the ones considered to be persuasive in other kinds of argumentation.

Keywords: argument to the best explanantion, non-miracle argument, scientific realism.

1. *Introduction*

This paper focuses on the inference to the best explanation (IBE) as a kind of argumentation in philosophy of science. Several scientific realists argue that scientific realism is the best explanation for the success of science. But serious objections have been raised against IBE. Given the controversy generated by the IBE argument, this paper explores the possibility of the fact that the degree of resistance to accepting the inference to the best explanation depends on differences which are related to the persuasion criteria that corresponds to each context. We distinguish four different contexts in which IBE is used:

a) the common sense knowledge context;
b) the scientific research context;
c) the philosophy of science context: when talking about scientific theories some philosophers contend that the truth of a theory and the existence of the unobservable entities it posits are the best explanation of its success;
d) the philosophy of science context again, but in a higher level: when some philosophers argue that scientific realism is true because it explains the success of science better than the antirealist claims.

According to our hypothesis, participants of philosophical discussions often apply criteria that are stricter than the ones considered to be persuasive in other kinds

of argumentation, but many realists seem not to be aware of that. As they do not make any distinction amongst different contexts, they carry on IBE from every day belief formation to higher levels of philosophy.

We will start with a presentation of the no miracle argument (NMA) as an emblematic instance of IBE and we will try to show how realists use IBE simultaneously at different levels of argumentation. We will examine various formalizations of both NMA and IBE and we will compare the strength of IBE in different contexts. As a result, we hope to show that, contrarily to what realists believe, IBE is not a powerful tool for supporting their doctrine.

2. *The canonical formulation of the non miracle argument*

The so-called no miracle argument (NMA) is one of the most widespread beliefs amongst scientific realists. It can be synthesized, broadly speaking, in the idea that the explanatory and predictive success of our best scientific theories implies that they are true or approximately true because, if they weren't, their numerous successes would be a coincidence so surprising as miracles are. Putnam says it with the following words:

And the typical realist argument against idealism is that it makes the success of science a miracle. And the modern positivist has to leave it without explanation (the realist charges) that 'electron calculi' and 'space-time calculi' and 'DNA calculi' correctly predict observable phenomena if, in reality, there are no electrons, no curved space-time, and no DNA molecules. If there are such things, then a natural explanation of the success of these theories is that they are *partially true accounts* of how they behave […] But if these objects don't really exist at all, then it is a *miracle* that a theory which speaks of gravitational action at a distance successfully predicts phenomena; it is a *miracle* that a theory which speaks of curved space-time successfully predicts phenomena; and the fact that the laws of the former theory are derivable 'in the limit' fron1 the laws of the latter theory has no methodological significance (Putnam, 1978, pp.18-19).

Probably, many people would be persuaded by this argument because it has certain similarities with inferences that we make in everyday life. Some authors would try to justify this reasoning proclaiming that it is a special kind of inference, which Peirce called abduction or retroduction and after Harman is often identified under the name of inference to the best explanation.

3. *The IBE in everyday knowledge and scientific knowledge*

The incorporation of IBE in the second half of the twentieth century as an important concept for understanding the process of knowledge is mainly due to Hanson. He represented IBE as follows (Hanson, 1958, p. 86)

[1] The surprising fact C is observed
[2] But if H were true, C would be a matter of course
[3] Hence, there are reasons to suspect that H is true

As we have said, this type of inference is often used in common-sense knowledge. Van Fraassen, proposed an example that illustrates this: if you hear little noises that come from the interior of the wood walls, if in addition you see that a piece of cheese that had been left the night before on the table has disappeared and there are mouse droppings on the floor, you will accept the hypothesis that there is a mouse in the house. It is not fully clear whether van Fraassen would be willing to admit that in situations like these it is fair to say that the hypothesis is accepted as true (or probably true) or only that it is accepted as empirically adequate (Psillos, 1999, pp. 211ff); but in any case he rejects that a reasoning of this kind is valid in the scientific context. Recall that for van Fraassen the goal of science is not to find true theories but empirically adequate theories, i.e. find theories whose observational predictions are effectively met. According to van Fraassen, then, a scientist would not be entitled to believe that the predictive success of his theory implies that it is true and that the entities postulated by it, for example, the atoms, do exist.

Psillos suggests that if the reasons for van Fraassen to object the use of IBE in the scientific context are intended to prevent unwanted ontological commitments with new classes of entities because they allow inferring the existence of unobservable entities, then van Fraassen is wrong. Because the IBE is also used to infer the past existence of extinct species, i.e, a new kind of entities, from the discovery of fossils, and these animals, although unobserved by us, are not unobservable entities.

On our part, we believe that anyway there is a difference between the mouse and a possible extinct species. Although van Fraassen considers it appropriate not to draw any distinction between a theoretical vocabulary and an observational one, it seems undeniable that asserting the existence of an extinct species is very far from our everyday experience. There is a much more hypothetical and uncertain

character in the former assertion. In the event of having observed the behavior of mice, a prehistoric man surely would have reached the same conclusion as that a person of our day would, had he found the same indirect evidence of their presence. And, in fact, the finding of a fossil is a pretty different situation, to name one of the reasons because its identification as a fossil implies a controversial theoretical supposition. The case of the "Piltdown Man" is a good example.

Psillos extends the use of IBE from everyday life to scientific research very naturally. But at the scientific level, the postulation of theoretical entities, even though they might serve to explain and predict phenomena, has often been rejected. This was the case for atoms, which Mach never accepted. In addition, the entities of everyday life, such as the mouse that has eaten the cheese or the weasel that has eaten the hens during the night, belong to kinds of things that have remained unchanged for a long time, while theoretical entities have frequently resulted to not exist or their concepts have been modified so much that the realists have to make desperate efforts to sustain that the old theories were to some extent true and the entities they posited are eventually the same as those that are postulated today.

4. *The IBE in the philosophical argumentation*
Now let us consider the use of IBE at the philosophical level. According to what we have already seen, the example proposed by van Fraassen would have this form:

[1] The surprising fact C (the indications of the presence of a mouse) is observed
[2] But if H were true (if there is a mouse in the house), C would be a matter of course
[3] Hence, there are reasons to believe that there is a mouse in the house

But Putnam's argument about realism is considerably more complex. To begin with, in the text we quoted above there are overlapping arguments that operate at different levels of analysis. On the one hand, in a first meta-scientific level (MS 1), it is argued that the predictive success of scientific theories can be naturally explained if it is thought that theories explain properly how things really are in the portion of reality they deal with. On the other hand, Putnam climbs to an upper epistemological, a meta-meta-scientific level (MS 2) when he applies a similar form to argue not directly about scientific theories but about certain

epistemological conceptions, in this case, realism and antirrealism.

To facilitate the analysis, we will adopt a more precise formulation of the argument corresponding to MS 1. Magnus and Callender, for example, offered a schema that seems to pick up the core of Putnam´s argument (Magnus and Callender 2004: 320-338):

[1] The theory h is very likely to be successful
[2] If h were true, it would be very likely to be successful
[3] If h were false, it would not be likely to be successful
[4] Therefore, there is a high probability that h is true

At first sight, Magnus and Callender´s formulation of IBE differs from the one proposed by Hanson, because they do not make any explicit reference to the relationship between the explanatory and predictive power of a hypothesis and the likelihood of that being true. However, we can establish the connection because the success of a theory would be measured precisely according to its ability to explain and predict phenomena.

On the other hand, oddly, although Magnus and Callender´s schema aims to clarify the non-deductive form of IBE, it can easily be transformed into a deductive reasoning without adding any assumption. In fact, from the premises [1] and [3] of previous argument, that is,

[1] The theory h is very likely to be successful
[3] If h were false, it would not be likely to be successful

by *modus tollens* we can infer:

* Theory h is not false

And from there we can deduce the conclusion Magnus and Callender had reached:

** Theory h is very probably true

In this case, the crux of the matter is not in the kind of inference that leads to the conclusion but in the justification of the premises that associates the success of a hypothesis with a high probability of it being true. This situation was shown also by Musgrave, who feels that the classic formulations of IBE are deductively

fallacious. If you want to avoid the fallacy, he suggests, you should express the argument in this manner (Musgrave 1999: 285):

[1] If hypothesis H is the best explanation of the fact to be explained, then it is reasonable to accept H as true
[2] H is the best explanation of the evidence
[3] Therefore, it is reasonable to accept H as true

On the other hand, some authors -like Magnus and Callender- say that IBE is inductively fallacious. If all that is true, if IBE can't be justified either in an inductive or a deductive way, proponents of the idea that there is a relationship between the explanatory value of a hypothesis and its truth should think that the statement "If hypothesis H is the best explanation of the fact to be explained, then it is reasonable to accept H" is a reliable assertion on its own right, perhaps because it possesses a sort of intuitive evidence. In fact, it looks like some conclusions reached in everyday knowledge, as in the case of the mouse, rely on the implicit acceptance of that belief.

As we have shown, IBE is used at the meta-meta-scientific level (MS 2) to justify the scientific realism. Kukla expresses the argument as follows (Kukla: 12):

[1] The enterprise of science is (enormously) more successful than can be accounted for by chance
[2] The only (or best) explanation for this success is the truth (or approximate truth) of the scientific theories
[3] Therefore, we should be scientific realists

This argument may be reformulated so that the assumption that the virtue of being the best explanation involves the truth becomes explicit. This is achieved by adding the premise:

* If the scientific realism is the best explanation for the success of science, then realism is true.

The explication of this premise drives us back to considering the value of IBE in those different contexts in which it is used. Next, we will develop our conclusions in this regard.

5. *The scope of IBE*

According to what we have said so far, IBE is an instrument which has been used at least in the following contexts:

a) in the common-sense knowledge context;
b) in the scientific knowledge context, especially as a way of legitimating the belief in the existence of theoretical entities postulated by a specific scientific theory;
c) in the philosophy of science context, as a sort of generalization about scientific theories and the existence of theoretical entities ("successful scientific theories are approximatively true and theoretical entities postulated by them very probably exist");
d) in the philosophy of science context, but at a higher meta-philosophical order ("scientific realism is true because it is the best explanation for the success of the science").

We have already advanced that IBE probably has a different persuasive force depending on the context in which it is being used, that is, depending on the circumstances in which it is applied and the intended audience to which it is directed. In general, this remark seems to be true of any kind of inference, except perhaps those that are strictly deductive. It would seem that, for example, a simple enumerative induction would be more easily accepted in everyday life than in the context of scientific research, where it must comply with certain special conditions about the extension of the sample, its representativeness, etc. Inductivist philosophers often point out that the inductive inferences are used constantly in both common sense knowledge and factual sciences. But they have found it difficult to justify these inferences in the face of objections from Humean criticism so they had to elaborate more refined versions of the induction to reconstruct and validate their use when justifying scientific theories. From a philosophical point of view, Popper has not hesitated in sustaining that even if it were true that in everyday life and in the scientific research induction is continuously used, all who do so can be wrong. But even under the assumption that a persuasive defense of inductive inferences in factual sciences has actually been achieved, there is no room for them in, for example, formal sciences. The so called mathematical conjectures do not cease to be only conjectures no matter how many favorable cases they accumulate. Precisely, their interest lies in the fact that they seem to have no exceptions. But only a deductive demonstration could convert a mathematical conjecture in a theorem. Despite Mill´s attempt to

show that the laws of pure mathematics have arisen inductively and despite Quine's suggestion on the possibility that such principles are revisable in extreme cases, the prevalent conviction is that mathematical truths belong to the field of a priori knowledge. In the same vein, the fact that a kind of reasoning can be admitted in the context of common-sense knowledge or in factual sciences does not imply or makes it more likely that it is equally acceptable in the domain of philosophy.

Now we must ask ourselves: What relevance and validity the non-deductive inferences and in particular IBE could have in philosophical contexts? Philosophical discourse, even if we focus only in the philosophy of science, is so varied that trying to identify ways of justifying philosophical thesis seems more difficult than to agree about how to reconstruct the methodology of the factual sciences. But if we put aside the claim of naturalizing epistemology to the point where it would became simply one more of the empirical sciences, philosophy of science seems to depend essentially, although perhaps not exclusively, on a priori analysis. Notorious examples are the statements of Putnam about his well-known argument of the brains in a vat. After confessing that for several years he had many doubts about its validity, Putnam relates that his argument came to his mind while he was studying the Löwenheim-Skolem theorem and he saw a connection with some arguments developed by Wittgenstein. He points out that, even when some elements of his own argument have an empirical origin, it has a kinship with Kant's transcendental reflections because he developed it by thinking a priori about the conditions of the possibility of knowledge (Putnam 1981)

Another valuable testimony in favor of the independence of the philosophical inquiry with respect to the factual knowledge comes from Kuhn. Despite having been one of the creators of the so-called "historical philosophy of science", he confessed in his late works that his doctrine, also partially related to that of Kant, had been since its very beginning rooted in philosophical principles rather than in empirical data extracted from the history of science.

Philosophical discussions have a *sui generis* status. They cannot be empirically contrasted, as it is assumed that it can be done with common sense beliefs about the world or the hypothesis of factual sciences, neither can they be solved as problems of mathematics or pure logic. This does not mean, of course, that certain principles associated with inferences, such as the principle of induction or

the argument to the best explanation, become useless in philosophical discourse; but this shows that they deserve at least a special justification that so far seems to be out of our reach.

As an illustration of the difficulties involved in applying to philosophical theories concepts forged with the purpose of analyzing scientific theories, it should be noted, for example, that while the predictive success of a scientific theory could be a strong indication of its empirical adequacy, such an approach could not be extended to philosophical theories. It would be inapplicable because we can't even understand what empirical adequacy means for theories which, by its own origin, do not purport to describe the world in the same manner of the factual sciences. Similarly, while IBE presupposes a notion of explanation which, as in the Hempelian models, allows us to stablish the truth or the likelihood of the *explananda* as a consequence of the truth of the *explanans*, it is not clear in what sense a philosophical theory constitutes an "explanation" or a "best explanation". In addition, if we assume that the truth of a scientific theory means something like a correct description of both, the observable and non-observable characteristics of nature, it is not very clear in what sense we can say that a philosophical conception, as the scientific realism or its rivals, are "true" or "false".

6. *Conclusion*

In summary, logicians have identified the use of IBE in common-sense thought and scientific research and that discovery has inspired its explicit use in philosophy of science to underpin scientific realism. However, this maneuver, especially when IBE is expressed in the no miracle argument, far from overcoming the resistance of scientific antirealists, seems to offer evidence that the persuasive power of IBE becomes increasingly weak as we move further away from the domain of the beliefs of common sense.

References

Hanson, N.R. (1958). *Patterns of Discovery.* Cambridge: Cambridge University Press.

Kuhn, Th. (1992). *The Trouble with the History Philosophy of Science.* Rothschild Distinguished Lecture. Cambridge: Department of the History of Science, Harvard University.

Kukla, A. (1998). *Studies in Scientific Realism.* Oxford: Oxford University Press.

Magnus, P.D. y Callender, C. (2004). Realist ennui and the base rate fallacy.

Philosophy of Science, 71(3)
Musgrave, A. (1999*). Essays on Realism and Rationalism*. Amsterdam: Rodopi.
Psillos, S. (1999). *Scientific Realism: How Science Tracks Truth*. London: Routledge.
Putnam; H. (1978). *Meaning and the Moral Sciences*. London: Routledge and Kegan Paul.
Putnam, H. (1981). *Reason, Truth and History.* Cambridge: Cambridge University Press.
Van Fraassen, B.C. (1980). *The Scientific Image*. New York: Oxford UniversityPress.