2011

The Best Explanation: A Defense Scientific Realism

Johnston T. Hill
University of Puget Sound, jhill@pugetsound.edu

The Best Explanation: A Defense of Scientific Realism

Johnston Hill
University of Puget Sound
September 2011

This paper offers a defense of scientific realism against one central anti-realist argument, the pessimistic meta-induction (PMI). More specifically, this paper initially considers and rejects an oft-stated version of the PMI, arguing that the historical sample size is insufficient to make any serious induction, optimistic or pessimistic, about the likelihood of current scientific theories being abandoned. After demonstrating the deficiency of the initially considered PMI, the paper takes into account a possible amendment to the PMI which could circumvent such sample-size worries, but then concludes that even this amended version of the PMI does not offer sufficient warrant for abandoning scientific realism. Before diving headlong into these arguments against the PMI, it will be helpful to review the general realist and antirealist positions that stake out the terms of the debate.

Anjan Chakravartty offers a general characterization of scientific realism when he states "Scientific realism is a positive epistemic attitude towards the content of our best theories and models, recommending belief in both observable and unobservable aspects of the world described by the sciences" (Chakravartty, 2011) (for the remainder of the paper I will refer to scientific realism and scientific anti-realism simply as realism and anti-realism. Any reference to other forms of realism or anti-realism will be explicitly noted). In connection, realism stakes predictable claims in historically fundamental philosophical debates. For instance, realists are overwhelming likely to reject idealism and embrace the existence of a mind-independent external world. But realism goes beyond a belief in the mundane ontic furniture of everyday life (e.g., trees, bees, bricks, cars), beyond a rejection of radical skepticism. It also endorses belief in mature, successful scientific theories whether they describe our mundane ontic furniture or whether they describe unobservables with which humanity has traditionally been unfamiliar

(unobservables being those entities that cannot be perceived under favorable circumstances by a person with normal functioning sense organs).

Though the realist position appears relatively straightforward, as it simply requires belief in the products of good science, there is significant room for variation among realists. Realists disagree about which theories are adequately mature and what constitutes a theory being approximately true. Additionally, the introduction of structural realism, the claim that only the underlying mathematical structure of mature theories should be regarded as real (in contrast to the unobservable entities), has further complicated the debate.[1]

More than the subject of debate in realist circles, anti-realists attempt to exploit the ambiguities of the realist position, particularly the notion of approximate truth, to strengthen their own claims (see Laudan, 1981). These conversations, while meaningful and interesting, require treatment of their own, and will not be the subject of this paper. In what follows one should assume I make reference to a sort of garden-variety realism in which both the mathematical structure and the entities described by our current best theories are approximately true representations of the world and that both the concept of approximate truth and maturity are sufficiently clear for the realist's purposes. Further, truth should be should be understood as a correspondence to fact, where true statements are those that correctly describe external reality, as opposed to a pragmatist or coherentist understandings of truth. With these qualifications in mind, we can consider the case for realism.

The most powerful and influential arguments for realism are motivated by the theory's explanatory power, with realists claiming that other theories do not adequately explain the success of science. One leading formulation of this appeal to explanatory power is the no miracles argument (NMA), made famous by Hilary Putnam. The NMA claims that scientific

[1] For an at-length discussion of structural realism see Worrall, 1989 and Ladyman, 1998.

realism “is the only philosophy that doesn't make the success of science a miracle” (Putnam, 1975: 73). By pointing to the success of our current theories realists argue that the truth of such theories is overwhelmingly likely. Such successes include the manipulation of the phenotypical properties of organisms, the engineering of atomic weapons, the development of effective medicine, and the ability to make highly accurate predictions about a wide variety of phenomena. If it were the not case that our scientific theories about these subjects were at least approximately true, their ability to make accurate predictions and facilitate continued successes would be a truly incredible coincidence, a coincidence on par or exceeding that of an individual consistently picking winning lottery numbers. Much of the motivation for realism comes from the intuition that the invocation of such a coincidence is insufficient to explain science’s success. The realist’s alternative is to assume that such a coincidence does not exist and that out current theories are mostly true.

This appeal, an appeal to the seeming unlikelihood of current scientific theories being successful on the basis of a miracle, has at its roots abductive reasoning, or inference to the best explanation (IBE). A general description of IBE provided by Peter Lipton describes the process as “Beginning with the evidence available to us, we infer what would, if true, provide the best explanation of the evidence” (Lipton, 1991: 1). While it may be surprisingly difficult to give a principled or systematized account of IBE, in which it is clearly delineated what constitutes the basis of a “best explanation”, the practice is ubiquitous and unavoidable. This ubiquitous method of inference forms much of the basis of not only philosophy and the sciences, but everyday existence. For instance, if I venture outside during a Tacoma November with overcast skies and see that the pavement is wet, it is not reasonable to infer that the firemen down the street decided to go for a joy ride with their hoses on, soaking everything in sight. This inference is

unreasonable because it does not best explain the available evidence (if I had seen such a joy ride, the case would be different). The hypothesis that explains the evidence best is something to the effect of "it has rained recently", even though this hypothesis is not *guaranteed* to be true. Such inferences are fundamental to our decision-making and behavior, and forsaking such a powerful logical tool, even if only in a specific domain, requires powerful arguments. Of course, few anti-realists purport to make such a rejection. Part of the task of this paper is to demonstrate that, in the case of science, they do, and that such a rejection is unreasonable unless independent arguments can demonstrate the deficiency of abduction in the case of anti-realism. Until such arguments are made, it would seem the anti-realist ignores a valuable, even crucial, logical tool when making their claims.

Before describing anti-realism and its incarnations motivated by the PMI, it is important to note that realism does *not* entail the claim that our current scientific theories are absolutely true with no room for revision. As mentioned previously, most realists take a more qualified stance when defending the position. Science is argued to be approximately true, with room for development and reevaluation. For instance, science can certainly add knowledge within the existing framework, as with mapping of a new genome. Even fundamental theories and assumptions might be recontextualized in the light of new discoveries or more effective frameworks, as was arguably the case in the shift from Newtonian to Relativistic mechanics, in which Newtonian mechanics describes those objects in Relativistic mechanics which have "medium masses" and move at "medium" speeds.[2] However, the realist maintains it is unlikely that the fundamental positions in science will be abandoned wholesale or found to be totally

[2] Obviously, some objects that we might regard as very massive or very fast can still be accurately explained with Newtonian mechanics. Here we should take "medium" to mean those objects neither massive nor fast enough to be significantly effected by either general or special relativity (or small enough to be significantly effected by quantum mechanics).

misguided. Rather, they point in the right direction, even if there is ambiguity in the details or new ways of framing scientific questions.

Scientific anti-realism is the rejection of such a positive epistemic stance towards science and its theories. Broadly speaking, there are many motivations for anti-realism.. Of the assortment of anti-realist arguments, two of the most popular have been the constructive empiricism of Bas van Frassen (van Frassen, 1980) and worries about the underdetermination of theories, a concern historically associated with Pierre Duhem (Duhem, 1906) and Wilard van Orman Quine (Quine, 1953). While these anti-realist arguments may or may not be convincing (I, in fact, find them lacking), for the purposes of this paper they can largely be set to the side while attention is paid to a third anti-realist motivation, the pessimistic meta-induction. It is worth noting that while constructive empiricism, underdetermination, and the PMI can all be dealt with in separate conversations, there can also be substantial overlap, with each argument informing the others and each sharing similar motivation. This overlap is particularly evident in the work of van Frassen, who argues that observable evidence underdetermines the unobservable entities posited by science. It can also be seen in the work of Kyle Stanford (Stanford, 2006), who has recently worked to combine the historicist tack of the PMI with traditional underdetermination worries. Nevertheless, the boundaries of the PMI are sufficiently clear to allow for treatment on its own; what follows is the appraisal of that particular anti-realist strategy.

Proponents of the PMI argue that the empirical success of past scientific theories is in no meaningful way tied to their being true. In fact, most such theories contain theoretical elements currently regarded as patently false and posited entities regarded as non-existent. As such, we can perform an induction over the long history of science to conclude that our current theories

are likely to be abandoned and that their empirical success is not a barometer for truth-likelihood. Laudan provides a list of such empirically successful yet abandoned theories, reproduced below:

> - the crystalline spheres of ancient and medieval astronomy;
> - the humoral theory of medicine;
> - the effluvial theory of static electricity;
> - 'catastrophist' geology, with its commitment to a universal (Noachian) deluge;
> - the phlogiston theory of chemistry;
> - the caloric theory of heat;
> - the vibratory theory of heat;
> - the vital force theories of physiology;
> - the electromagnetic aether;
> - the optical aether;
> - the theory of circular inertia;
> - theories of spontaneous generation. (Laudan, 1981: 33)

Given such a lengthy array of abandoned theories, which Laudan claims he can augment *ad nauseum*, the intuitions behind the PMI are clear enough: it would require a sort of inductive misstep to assume current science had it right while others who had made similar claims were proven incorrect time and time again. Better to adopt the anti-realist stance and assume our current theories will be abandoned and are likely *not* even approximately true.

My argument against the PMI is straightforward. It is simply that the historical record, by itself, does not offer the sufficiently large or unambiguous sample required for making serious inductions about the future of sciences' current best theories. Initially, this may appear to be a bold claim. It is easy to imagine objections pointing out that the history of science is hundreds or thousands of years old, as long or longer than the historical record that economists, sociologists, and historians use to level many of their claims. Given that the methodology and explanatory power of these disciplines is widely trusted, we should trust a historicist analysis of science as well. Laudan, after all, provides a substantial list with which to perform an induction. It might even be argued that the length of sciences' historical record is a strong virtue for the PMI, that few other samples map change over such an extended length of time, and that consequently we should be *more* confident in the PMI than most other theories.

These objections are misguided for a number of reasons. First, the quality of a sample for making predictions is not determined by a quantitative measure of the time over which the sample was collected. Other criteria, including the amount of possible confounding variables, the amount of data collected over that sample, the complexity of the examined phenomenon, and the number of successful tests, play essential roles in determining the predictive power we should come to expect from a theory. This can be illustrated through a number of examples, some hypothetical, others factual.

We can imagine circumstances which make it apparent that the amount of time a theory has enjoyed empirical success is less important than other features supporting a theory. For instance, consider a hypothetical theory A, which has enjoyed the success of making correct predictions over 500 years. However, theory A might only have been genuinely tested three times. That the theory has only had three opportunities for confirmation or falsification should weigh on us more than the fact that those opportunities occurred over a 500 year period. If a falsifying case did come along, say on the fourth test, the length of time the theory enjoyed predictive success, by itself, should not play a significant role in deciding whether to keep or abandoning the theory. This is not to say that the amount of time that a theory enjoys success cannot influence the other factors that play a significant role in theory choice. More time yields more opportunities for tests, for instance. But when individuals speak of the predictive success of a theory over many years, it is more an implicit appeal to the amount of successful tests of the theory, tests occurring frequently over a long period of time, than to the length of time supporting a theory taken in isolation. So, those who regard theory A as a unfailing guide to what to expect in the future might be likened to those who would make serious future predictions

about the flip of a coin after three flips on tails, citing as evidence in their favor that those flips occurred over hundreds of years.[3]

The PMI should be imagined as analogous to theory A. The sample size of abandoned theories, when one takes a critical view towards the anti-realist's list, does not, by itself, provide a sufficient sample of past abandoned theories to warrant the claim that we should expect current science to be regarded as distant from the truth. For example, we might question the true scientific credentials of the astronomical crystalline spheres theory, the humoral theory of medicine, effluvial theory of electricity, 'catastrophist' theories of geology, the vital force theories of physiology, the theory of circular inertia, and theories of spontaneous generation. As it is far beyond the scope of this paper to deliberate about each of these theories' scientific shortcomings individually, a brief group consideration will have to suffice. In short, each of these theories, while containing some sort of explanatory power, were unsuccessful by any reasonable standard in that they failed to make predictions beyond the patently obvious. For example, it would be trivial for me to count the prediction that "a thrown object will move in the direction it is thrown" in favor of the theory of circular inertia as such a prediction is also entailed solely by common sense. This is not the stuff which allows for the engineering of airplanes, the isolation of gases, the production of atomic weapons or the prediction of novel events. As such, we should refrain from calling them scientific theories, which we should demand make novel, precise predictions and do not merely have readily apparent observational phenomena built into them.

Keeping in mind that that the length of time over which a theory has enjoyed success should not, by itself, be a significant factor in determining the value of said theory, how might

[3] The flip of a coin might be a misleading analogy, as it carries along with it the implicit assumption that the probability of each flip is 50%. A better analog might be some sort of number generator, random or not, and regarding the first three of its outputs as indicative of what to expect in the future.

we evaluate the PMI? The inductive base provided by Laudan has been trimmed significantly, though some successful, yet abandoned, theories remain. However, even if we suppose that such theories were not approximately true despite their success, we can question the soundness of the PMI. Especially with the trimmed sample trimmed to truly scientific theories, the PMI begins to resemble theory A, a theory with success over hundreds of years, but only a very small sample of tests over that period. We might then infer that we do not have a sample sizable enough to induce that current scientific theories will be abandoned. Rather, such a limited sample recommends agnosticism about the issue; it is simply unclear, given only the historical record, whether our current theories will be largely abandoned and considered false or retained indefinitely and considered approximately true. We might also ask at what point the pessimistic induction becomes unreasonable: after theories have enjoyed well-tested success for a 100 years? 500? Or will the existence of such counter-examples always be sufficient to leverage the argument, even if science were to continue indefinitely without any major theory changes?

However, there is still an a-historical warrant for endorsing realism, namely, the NMA. The realist need not make any appeals to the historical record of science to leverage their appeals to the explanatory power of realism (although such appeals are by no means incompatible with realism, and may even strengthen the case). All the realist must point to is *current* science. If such science successful, then the models used to produce such successes are presumably true descriptions of reality, barring a sort of miracle; no reference to the past need be made. This is in contrast to advocates of the PMI, who must appeal to the historical record to leverage their claims. As such, if the historical record is taken as insufficient to leverage claims regarding the likelihood that current empirically successful theories will be abandoned the proponents of the

PMI lose much of their ground, while realists may still use current science to make appeals to explanatory power of their view. This appraisal seems to favor the realist.

Nevertheless, as it stands the argument leaves substantial room for the anti-realist to maneuver. If it is admitted that there are examples of even a few non-approximately true but empirically successful theories, as the above argument allows, the anti-realist can make a convincing case against the linchpin of the realist position, the NMA. By pointing to such examples, the anti-realist can give counter-examples to the NMA, providing cases in which science was admittedly not approximately true, yet still successful. This is precisely the sort of miraculous occurrence the realist wishes to deny, and such counterexamples certainly strains the credibility of the realist who maintains such a possibility is hugely unlikely (Note, however, that the realist can maintain her position without any explicit contradiction. Just because an event is unlikely does not preclude it from happening, nor does it preclude it from happening very frequently over a period of time). To continue to assert that the NMA can function unscathed in light of such worries might reasonably be seen as begging the question against the anti-realist.

The realist has answers to such claims. This is to claim that genuinely successful abandoned theories were, in fact, approximately true and that the theoretical aspects of abandoned theories which were crucial for their success were carried over to future theories, while it was only theoretically inaccurate portions which were abandoned (see Psillos, 1999: 108-143 and Kitcher, 1993: 127-177). For instance, the shift from Newtonian mechanics to Relativist mechanics was not an abandonment of Newtonian mechanics *per se*, but rather the relocation of a largely correct theory into a broader framework, in which it is a limiting case. Similarly, in the case of the caloric theory of heat, Psillos claims "the cause of heat as a material fluid was not as central, unquestioned, and supported as, for instance, Laudan, has claimed" and

that "the empirical success of the caloric theory was not essentially dependent on claims concerning the existence of an imponderable fluid which caused the rise (or fall) of temperature by being absorbed (given away) by a body" (Psillos: 113, 115-130). Psillos makes a similar point about dynamical optical ether theories, demonstrating that the supposition of such an ether was not central to the theory's success and that the optical laws generated could function independently of such theoretical suppositions (Psillos: 114, 130-145). Given such theoretical carryover, it is reasonable to call such theories approximately true and to claim they describe portions of the world in meaningful ways, even if such descriptions were not perfectly accurate and later superceded by improved theories. With such arguments in hand, the realist might confidently assert that successful but abandoned theories were approximately true, and that proving otherwise becomes the burden of the anti-realist.

Perhaps these answers to the PMI are convincing, perhaps not. However, another answer to the PMI that is often overlooked, perhaps ironically, as it is firmly rooted in the central tenant of science discussed previously, is inference to the best explanation. According to this answer we might assume that our previous answer failed in some cases (or perhaps all cases), that there was *not* significant theoretical carry-over between truly successful, mature theories and that some (or all) past mature theories were not even approximately true even though they were empirically successful (again, to emphasize, this is not the view I think most prudent, but for the sake of the following answer it is an illustrative assumption). Even then, the realist might still leverage a convincing argument utilizing inference to the best information. The case goes as follows: even if these past theories were untrue, it does not follow that anyone was misguided in thinking them true or that we are wrong for believing in the truth of our current theories. Realism, after all, is rooted in an inference to the best explanation and any inference to the best explanation will not

be perfectly infallible; we can never conclusively *prove* such inferences true as we can with sentences in a deductive system. However, this does not discredit inference to the best explanation as a reliable tool for describing reality in the vast majority of circumstances. Abandoning it in response to a few failed attempts of its use would be an extreme overreaction. Rather, it should continue to be embraced, as it generally offers a mostly correct description of events.

To make the point clear, imagine circumstances in which you are playing baseball, only to turn around to briefly glimpse another baseball hurtling at your face before falling unconscious. Upon awakening, it is reasonable to infer that you were hit in the head with a ball, which subsequently knocked you out. In fact, this seems extraordinarily likely, given the information at your disposal. The inference that a ball hit your head is the inference to the best explanation. Nevertheless, it is impossible to guarantee with absolute certainty that this was the case. It is also possible that you were struck by lightening moments before the ball would have struck you, knocking you unconscious before the ball contacted with your head. Such circumstances are extremely unlikely, but metaphysically possible. And one would not be unreasonable in believing strongly that you were knocked unconscious by the ball, even though fate conspired against such circumstances. Additionally, one would *not* be reasonable if, faced with the identical or similar circumstances after the initial occurrence, they assumed they had again been struck by lightening. Rather, one should assume it was the ball, even if one had been incorrect in their reasonable judgment in the first occurrence. After all, the event of being struck by lightening in similar circumstances for a second time is still enormously unlikely and being foiled by exceptional circumstances when using a reliable method does not warrant abandonment of the reliable method.

Analogously, even if we concede that previous empirically successful theories were not approximately true, this does not give us license to assume that out current empirically successful theories are untrue as well. Rather, we should view those past theories as the aberration, as the errant lightening strike, while continuing infer to the best explanation with confidence. This argument might be regarded as particularly effective if the previous realist arguments were largely, yet not completely, successful in demonstrating that most abandoned theories were approximately true.[4] For the fewer incidents of genuinely non-truth-like yet empirically successful theories, the more plausible it becomes that such theories are errant lightening strikes rather than regular occurrence. And with fewer aberrations, the more convincing the abductive reasoning which claims we should expect few such anomalies.

To conclude, the realist has a variety of weapons against the PMI. Besides appeals to the explanatory power of realism, arguments that the historical sample size is insufficient to draw serious conclusions about the future success of science, or arguments that there is significant theoretical overlap between truly successful abandoned theories and current science, they might also argue that abductive reasoning gives good reason to embrace realism *even if* some past empirically successful theories were not approximately true. Given this, the anti-realist needs independent reasons to explain why abductive reasoning should not be trusted to when evaluating the truth-likelihood of current scientific theories. Until such a convincing argument is made, we should regard the PMI as an insufficient motivation for abandoning realism.

[4] It might also be argued that this argument becomes largely unnecessary if the previous realist arguments were completely effective. If such was the case, there would be little motivation for a realist to appeal to abductive reasoning to demonstrate the unlikeliness of problem cases, as there would be no problem cases to explain.

Bibliography

Chakravartty, Anjan, (2011)"Scientific Realism", *The Stanford Encyclopedia of Philosophy (Summer 2011 Edition)*, Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/sum2011/entries/scientific-realism/>.

Duhem, Pierre. (1906) *The Aim and Structure of Physical Theories*, trans P. Wiener, Princeton: Princeton University Press, 1954.

Kitcher, Philip. (1993) *The Advancement of Science,* Oxford: Oxford University Press.

Ladyman, James. (1998) "What is Structural Realism", *Studies in History and Philosophy of Science* 29: 409-424.

Laudan, Larry. (1981) "A Confutation of Emergent Realism." *Philosophy of Science* 48: 19-49.

Lipton, Peter. (1991) *Inference to the Best Explanation*. New York: Routledge Press.

Psillos, Stathis. (1999) *Scientific Realism: How Science Tracks Truth.* New York: Routledge Press.

Putnam, Hillary. (1975) *Mathematics, Matter and Method: Philosophical Papers, Volume 1*. London: Cambridge University Press.

Quine, Wilard Van Orman. (1953) "Two Dogmas of Empiricsim" in *From a Logical Point of View.* Cambridge: Harvard University Press.

Stanford, Kyle. (2006) *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives.* New York: Oxford University Press.

van Frassen, Bas. (1980) *The Scientific Image.* Oxford, Claredon Press.

Worrall, John. (1989) "Structural Realism: The Best of Both Worlds?" *Dialectica* 43: 99-124.