Historical Inductions Meet the Material Theory

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Abstract: Historical inductions, viz., the pessimistic meta-induction and the problem of unconceived alternatives, are critically analyzed via John D. Norton's material theory of induction and subsequently rejected as non-cogent arguments. It is suggested that the material theory is amenable to a local version of the pessimistic meta-induction, e.g., in the context of some medical studies.

1. Introduction

My goal is to contribute to a growing literature that is critical of historical inductions such as the pessimistic (meta-)induction (PMI) argument (Poincaré 1952, 160; Putnam 1978, 25; Laudan 1981) and the problem of unconceived alternatives (Stanford 2001, 2006) against scientific realism, concentrating mostly on the former. The PMI can be construed in different ways (Mizrahi 2015, Wray 2015), viz., as a deductive *reductio ad absurdum* (e.g., Psillos 1996, 1999), a counterexample to the no miracles argument and inference to best explanation argument for scientific realism (e.g., Saatsi 2005, Laudan 1981), or, usually, as an inductive argument (e.g., Poincaré 1952, Putnam 1978, Laudan 1981, Rescher 1987). In the following I will argue against the inductive version of PMI—or any construal of the PMI that makes use of historical induction—using John D. Norton's material theory of induction (Norton 2003, Manuscript). The upshot is that one ought to be critical of historical inductions that seem to fit the general form or pattern of a good inductive argument, but may in fact lack inductive warrant and force. Various critiques have been put against the PMI (e.g., Lange 2002, Lewis 2001, Mizrahi 2013), along with some defenses (e.g., Saatsi 2005). In Section 2 I will present the PMI and briefly discuss some criticism in order to place my own analysis in broader context. Section 3 presents the material theory of induction and argues that it dissolves the PMI, while Section 4 extends such claims to the more recent problem of unconceived alternatives. In Section 5 I note that the material theory of induction does leave room for a local version of the PMI, which holds in some limited domain, such as in relation to certain medical studies (Ruhmkorff 2014). I end in Section 6 with a short conclusion.

2. The (Inductive) Pessimistic (Meta-)Induction

The modern formulation of the PMI is usually attributed to Laudan (1981) who argued that having genuinely referential theoretical and observational terms, or being approximately true, is neither necessary nor sufficient for a theory being explanatory and predictively successful. More generally, Anjan Chakravartty characterizes the argument as follows:

> [PMI can] be described as a two-step worry. First, there is an assertion to the effect that the history of science contains an impressive graveyard of theories that were previously believed [to be true], but subsequently judged to be false . . . Second, there is an induction on the basis of this assertion, whose conclusion is that current theories are likely future occupants of the same graveyard. (Chakravartty 2008, 152)[1]

The PMI then may take the following form:

[Inductive Generalization PMI]

P(i) Past theory 1 was successful but not genuinely referential or approximately true.
P(ii) Past theory 2 was successful but not genuinely referential or approximately true.
…
C) Therefore, current (and perhaps future) theories are successful but (by induction) probably not genuinely referential or approximately true.

Laudan (1981) suggests that the history of science contains a graveyard of theories that were previously believed to be approximately true and genuinely referential, but that subsequently were judged to be false and not to refer. Estimations of the number of such superseded theories have been debated (e.g., Lewis 2001, Wray 2013) and recently Mizrahi (2016) presents evidence that challenges the "history of science as a graveyard of theories" claim. Others voice concerns regarding the period of history of science used in order to extract historical evidence (e.g., Lange 2002, Fahrbach 2011) or the proper unit of analysis, i.e., theories vs. theoretical entity (e.g., Lange 2002, Magnus and Callender 2004). Similarly, Park (2011, 83) and Mizrahi (2013, 3220-3222) have argued that the PMI is fallacious due to cherry-picking data, biased statistics, and non-random sampling.

My own criticism of the inductive PMI comes from a different avenue. I will assume that the anti-realist does have randomly sampled historical evidence from the correct period of history and with the proper unit of analysis (whatever those

[1] cf. Wray (2015, 61).

may be) that is not biased or cherry-picked. Still, on the material theory of induction the PMI will not be a cogent argument. In other words, I aim to identify what I take to be a more fundamental (although not categorically different) problem with the PMI.

3. PMI Meets the Material Theory

3.1 The Material Theory of Induction in a Nutshell

Consider the following formally identical inductive inferences (Norton 2003, 649):

> P1) Some samples of the element bismuth melt at 271 degrees C.
> C1) Therefore, all samples of the element bismuth melt at 271 degrees C.
>
> P2) Some samples of wax melt at 91 degrees C.
> C2) Therefore, all samples of wax melt at 91 degrees C.

What makes the first argument an inductively strong and cogent argument while the second a weak and non-cogent inductive argument? Norton (2003, Manuscript) has argued that formal theories of induction, which provide universal schemas that are meant to identify the inductions that are licit and those that are not, stand against an insurmountable difficulty when facing such a question.[2] Instead, he offers a material account of induction:

> In a material theory, the admissibility of an induction is ultimately traced back to a matter of fact, not to a universal schema. We are licensed to infer from the melting point of some samples of an element to the melting point of all samples by a fact about elements: their samples are generally uniform in their physical properties. … *All inductions ultimately derive their licenses from facts pertinent to the matter of the induction*. (Norton 2003, 650; original emphasis)

Norton calls the local facts that power inductive inferences "material postulates." Material postulates themselves are supported by other instances of induction that are licensed by different material postulates.

3.2 Material Analysis of PMI

Many of the criticism of the inductive PMI discussed above amount to the claim that the universal schema used by the likes of Laudan (1981), namely, (P3) Some A's are B's, (C3) Therefore, all A's are B's, does not apply in the case of the PMI because various criteria needed to implement the scheme, e.g., random sampling, correct historical period, proper unit of analysis, have not been met. What I wish to do here

[2] I will not defend Norton's theory or claims here. He dedicates an entire book to the matter in Norton (Manuscript).

is conduct a material analysis of the PMI. Considering the above presentation of the PMI in its [Inductive Generalization PMI] form we may ask, what powers the inductive inference, i.e., what material postulate licenses the pessimistic conclusion?

In context of the two inductive arguments considered in Section 3.1, we note that there is no material postulate that licenses the inductive inference in the case of wax (P2 too C2) but there is one in the case of bismuth (P1 to C1): Generally, chemical elements are uniform in their physical properties. By analogy, the presumption of the meta-induction is that each historical case study looked at is an instance of the same thing, a discovery of induction in science. If we are to perform the meta-induction then there needs to be something in the background facts that unifies all such inductions, just like the fact chemical elements are generally uniform in their physical properties warrants the inductive inference regarding the melting point of bismuth. Let us consider several options.

First, perhaps the material fact is that most scientists use a common rule or method in constructing or discovering successful theories, something along the lines of Mill's methods of experimental inquiry in his *System of Logic* (1872, Book III, Ch. 7). If so, the properties of the rule would be used to authorize the induction. Is there such a rule, or perhaps, some common scientific method? A glance at the history of science suggests that this is unlikely. Newton's deduction from the phenomena, is very different from Darwin's inference to best explanation, which in turn differs radically from Einstein's thought experiments with lights beams, trains, and elevators.[3] More generally, there seems to be a consensus among historians and philosophers of science that something like "the scientific method" is really more of an umbrella term for very different methods used by scientists to construct and discover theories. After all, novel problems necessitate novels solutions, and the commonality that does arise in different cases, say, attempts to minimize error or to be objective, is not the kind of commonality that we seek in powering the PMI and drawing the pessimistic conclusion. For instance, in his book *Styles of Knowing: A New History of Science from Ancient Times to the Present*, Chungling Kwa (2011) argues that there is no single, fundamental method used in science: "there is not just one form of Western scientific rationality; there are at least six." The framework of six "styles of knowing," includes the deductive, the experimental, the hypothetical-analogical, the taxonomic, the statistical, and the evolutionary style, and is based on Alistair Crombie's (1994) three-volume work *Styles of Scientific Thinking*. Similar, Ian Hacking (also taking lead from Crombie's work) has argued that there are distinct "styles of reasoning" used in science, such as the postulational style, the style of experimental exploration, the style of hypothetical construction of models by analogy, the taxonomic style, the statistical style, the historical derivation of genetic development, and the laboratory style (Hacking 1992). This further

[3] In fact, see Norton (Manuscript, Ch. 8-9) who argues that even in historical cases where the *same* principle is applied by scientists, viz., inference to best explanation, "at best we can find loose similarities that the canonical examples of inference to best explanation share," so that no common rule of the kind needed to power the PMI can be found (Ch. 8, p. 1).

corroborates the idea that scientific methods used for theory construction and discovery, as well as for scientific explanation, are very diverse.

More generally, scientific theories are not kind of things that portray the type of uniformity needed to license inductive inferences on Norton's material theory. Albeit in a different context, a similar point is nicely made by Mizrahi (2013, 3218):

> A uniform—as opposed to diverse—sample might be a sample of, say, copper rods. From a sample of just a few copper rods that are tested for electrical conductivity, it is reasonable to conclude that all copper rods conduct electricity because, if you have seen one or two copper rods, you have seen them all (given their uniform atomic structure). Scientific theories, however, are not as uniform as copper rods. The point, then, is that any sample of theories is not going to be uniform in a way that is required for a "seen one, seen them all" inductive generalization.

Similarly, and second, perhaps there are some facts about investigating scientist themselves, how they work, and/or the problems situations that they work in, which can unify the historical evidence in a way that provides us with the inductive warrant we seek. Maybe such facts will include something about the psychology of scientists: their fastidiousness and fear of error, their facility at jumping to conclusions, or perhaps their curiosity, logic, creativity, skepticism, etc. However, in a similar manner to the search for a common rule used in constructing successful theories, the history of science furnishes us with scientists that are heterogeneous enough in their psychological traits, and work in such varied contexts, so as not to provide us with any was to unify the various historical cases in a way pertinent to licensing the pessimistic inference of the PMI.

Third, perhaps we can circumvent looking to a common rule of constructing or discovering theories, or searching for common traits among scientists, by noting that the follow candidate material postulate would power the PMI:

> MP-PMI: Generally, successful theories are not genuinely referential and/or approximately true.

But how would we establish MP-PMI? One option is to appeal to the PMI itself, but this would either be circular or else push us to look for another material postulate. Another option is just to grant the MP-PMI as a reasonable assumption. Perhaps anti-realists or instrumentalists would think that this is a sensible starting point, but their target realist opponent would surely reject such an assumption as question begging. Last, perchance there is some fact about explanatory and/or predictively successful theories that renders them, generally, not genuinely referential and/or approximately true? Possibly part of the essence of successful theories is to misrepresent the world? To me this seems highly unlikely and at odds with any levelheaded intuition but, in any case, if we could argue that successful theories are essentially inaccurate then we would not need the PMI in the first place!

Fourth, we may want to construe the PMI in its inductive generalization form as a kind of abductive argument with the following type of material postulate:[4]

[Inductive Generalization PMI – Abductive version]

P(i): The success of past theory 1 (constructed using method m) is not best explained by its truth.
P(ii): The success of past theory 2 (constructed using method m) is not best explained by its truth.
…
MP: Scientific theories constructed using method m are generally uniform with respect to what best explains their predictive success.
C: The success of our best current (and perhaps futures) theories (constructed using method m) are not best explained by their truth.

Stating the PMI as above has the merit of directly engaging with the "no miracles argument" for scientific realism, namely:

> That terms in mature scientific theories typically refer [to things in the world] …, that theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories—these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate scientific description of science and its relations to its objects. (Putnam 1975, 73)

But worries abound. First, the realist may very well deny P(i), P(ii), etc., and argue that the success of past theories is best explained by their truth but that, as it turns out, either the best explanation did not hold in this case or else there is some sense in which past theories, insofar as they were successful, were approximately true or on the road to truth. Second, construing the argument as an abduction opens up a Pandora's box of problems associated with the notion of explanation: What is explanation? Are there accounts of explanation where success is best explained by truth and ones in which it isn't and, if so, which account of explanation is relevant in this context? And so on.

Third, the cogency of the argument depends on the idea that all theories appealed to were constructed with some method m, but we already judged that there is no one method that is relevant to constructing scientific theories. Perhaps phenomenological models are good candidates for the type of things that can provide empirical success but are not generally approximately true.[5] Thus, at best, the above argument can power a kind of local PMI: Successful theories constructed

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[4] Thanks to Tim Sundell for suggest this line of thought.
[5] Phenomenological models are, generally, not considered explanatory.

by method m are not approximately true. We'll consider one such case in more detail in Section 5.

In short, on the material theory of induction inductive arguments are powered by facts, by material postulates, but in the context of the PMI it seems unlikely that any such non-question begging postulates, which wouldn't render the PMI obsolete, can be found. This is so even if, say, the historical data was not cherry-picked, and the right unit of analysis and correct period of history were used. In other words, I'm equally skeptic of projects that attempt to block the pessimistic conclusion by, for example, taking a random sample of past scientific theories, e.g., Mizrahi (2016). In the following section I'll attempt to extend such claims to the problem of unconceived alternatives.

4. Extension to the Problem of Unconceived Alternatives

Recently, P. Kyle Stanford (2001, 2006) has developed what may be characterized as a new version of the PMI:

> … I propose the following New Induction over the History of Science: that we have, throughout the history of scientific inquiry and in virtually every field, repeatedly occupied an epistemic position in which we could conceive of only one or a few theories that were well-confirmed by the available evidence, while subsequent history of inquiry has routinely (if not invariably) revealed further, radically distinct alternatives as well-confirmed by the previously available evidence as those we were inclined to accept on the strength of that evidence. (Stanford 2001, S8-S9)

The problem of unconceinved alternatives as an argument against scientific realism has been criticized on various grounds (e.g., Chakravartty 2008, Devitt 2011, Mizrahi 2015), but my goal here is just to note that the discussion of Section 3 can be extended to this new version of the PMI, which can be construed as follows:

> P(i) In the past time of theory 1, theory 1 was successful but there were unconceived alternative theories that were as well supported by available evidence but with radically different ontology.
> P(ii) In the past time of theory 2, theory 2 was successful but there were unconceived alternative theories that were as well supported by available evidence but with radically different ontology.
> …
> C) Therefore, in present times, current theories are successful but (by induction) there probably are unconceived alternative theories that are as well supported by available evidence but with radically different ontology.

What we need for the material analysis is something like: Generally, successful theories are underdetermined by data due to possible unconceived alternative theories. In a similar fashion to the MP-PMI, we could look to some common rule used by scientists to conceive theories, or some common psychological traits among scientist, that may ground the idea that successful theories are such that empirically adequate unconceived alternatives always exists. But for the same reasons discussed above, it seems unlikely that any such common rule or traits will be found. That said, perhaps cognitive facts about human scientists might support the inductive inference to the conclusion that we always miss some alternative theories, which in turn are consistent with the available evidence. What is attractive about this line of thought is that it does seem plausible that due to our cognitive limitations there are always “unconceived alternatives.” However, mere cognitive limitations do not support the further conclusion that there are unconcieved alternative theories that are *consistent with available evidence*.

Alternatively, one may think that Stanford’s new induction circumvents the material objection: modal reflections alone convince us that there are always unconceived alternative theories that can explain and predict empirical phenomena just as well or better than conceived theories. But how can we come to such a conclusion based on modal reflections alone? Isn’t it conceivable if not possible that there would be a point in history with no unconceived alternatives and isn’t conceivable if not possible that we are at such point in time in history? Moreover, it is unclear what to make of theory-independent modal claims (unless one has logical modality in mind, which isn’t the case here). Certainly, we can talk about different physically possible worlds given a particular physical theory. For instance, various solutions to the Einstein field equations are taken to denote different possible universes according to relativity theory. But it isn’t clear what is meant by different possible or alternative conceivable *theories* given no meta-theory as a constraint, so to speak.[6] In any case, if we know that unconceived alternative theories always exist based on modal reflections alone, then the historical induction is doing no work for us at all.

5. Room for a local, material pessimistic induction?

Although the material analysis given here may prompt us to be skeptical of historical inductions (insofar as one is moved by the material theory of induction), it can help us understand why *local* pessimistic inductions may be tenable. Specifically, I want to look at a recent discussion by Rumkorf (2014) who contends that meta-analyses in medicine such as Ioannidis’ (2005a, 2005b), which show that a disconcertingly high percentage of prominent medical research findings are refuted by subsequent research, can be developed into a local pessimistic induction. Ioannidis (2005a, 2005b) is concerned with studies, denoted “M-studies,” that satisfy the following criteria: “being highly cited, using contemporary research and statistical methods, and being among the first studies to investigate a question at issue” (Rumkorf 2014, 420). Rumkorf’s (2014, 421) then uses the various conclusions of Ioannidis (2005a, 2005b) to generate a local PMI in the field of medicine (PMI-M):

[6] What would count as a (logically possible but physically) impossible theory in such a context?

> E1 41% of the associative or causal claims made by M-studies in the sample were inconsistent with the results of subsequent published studies either (1) because the later studies provided evidence against the existence of the association or effect; or (2) because the later studies provided evidence that the magnitude of the association or effect was significantly different.
>
> E2 Therefore, we can expect approximately 41% of the associative and causal claims made by M-studies to be inconsistent with subsequent published studies.

On Norton's theory we need to appeal to a material postulate to license the pessimistic inductive inference in the transitions from E1 to E2, but since we are now working in a limited domain without many heterogeneous examples as in the whole history of science, we may now find some significant commonality between the methods used in different M-studies that can act as licensing facts. What are the background facts that power the PMI-M? Here are some options extracted from Ioannidis's diagnosis of his meta-analysis and quoted in Ruhmkorf (2014, 219):

> Contributing factors include: bias in research (Ioannidis 2005b); non-randomized trials (Ioannidis 2005a); smaller rather than larger sample sizes in refuted studies (Ioannidis 2005a, 224); and publication and time-lag biases (whereby studies with highly significant and potentially aberrational positive results are overrepresented among published articles in major journals and are published more quickly than other articles) (Ioannidis 2005a, 224). Particularly intriguing is the idea that large-scale features of the structure of medical and biological inquiry contribute to the high contradiction rate. Having a number of distinct working groups looking at the same problem increases the chances that at least one of them will find something statistically significant, especially if they are looking at a wide array of possible relationships (Ioannidis 2005b, 697–698). The computational power and richness of data sets available to researchers increases the chance that some of them will be successful in achieving statistical significance, even when no real relationship exists (Ioannidis 2005b, 701).[7]

These various factors, insofar as they are common to most M-studies, are the type of background facts that warrant the pessimistic induction from a material point of view. One may worry of course that the pessimism associated with local PMI generalizes since, presumably, facts about biases and the like are facts about researchers in general, not just researchers in medical science in particular. But, although all scientific studies have to deal with challenges such bias, it may be the case that a particular local subfield, due to its specific nature and whatever social

[7] It should be noted that there are some problems with Ioannidis's (2005a, 2005b) methodology, as identified in Ruhmkorff (2014, 419-421), but they do not seem to be problematic enough to render the PMI-M not cogent.

norms are in place for collecting and disseminative evidence, is especially challenged in a way that can justify the pessimistic induction. The above suggests that this is indeed the case for M-studies.

To end, Ruhmkorff (2014) argues against global PMI on independent grounds (namely, he argues that the PMI commits a statistical error previously unmentioned in the literature and is self-undermining), and but he also argues for the plausibility of a local PMI, viz., M-PMI, and contends that there are clear advantages of PMI-M over PMI. What I wish to note here is that an additional advantage of PMI-M, or local pessimistic induction generally speaking, is that whereas global PMI dissolves upon a material analysis, a material account of PMI-M does seem viable.

6. Conclusion

I have argued that historical inductions such as the (global) PMI and the problem of unconceived alternatives dissolve if we work with the material theory of induction. The reason is that we lack the material postulates needed to license the pessimistic inference: the great heterogeneity of case studies from the history of science of conceiving, constructing, and discovering (explanatory and predictively successful) theories, along with abundant variety of context that scientists find themselves in and traits that they exhibit, make it unlikely that any commonality will be found strong enough to authorize the induction. One may of course object: so much worse for the material theory of induction! This is a fair point, but there is a more general moral to consider. In various situations one may be able to appeal to the notion of "induction" without much being at stake, but in the context of historical inductions like the PMI and problem of unconceived alternatives "induction" is doing a lot of (philosophically) heavy lifting and so the situation rightful calls for scrutiny. Such scrutiny has led to the various discussed criticism that are presented in the context of more traditional, non-material theories of induction. Accordingly, it seems appropriate to show that—even if we assume randomly sampled historical evidence from the correct period of history and with the proper unit of analysis that is not biased or cherry-picked, with no statistical error, etc.—historical inductions do not fare well on the material side of things. I leave objections to the effect that one ought to construe the PMI as a deductive argument, or through a different framework for induction, e.g., via hypothetical or probabilistic induction, for future work.

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