One version of the pessimistic meta-induction is self-undermining

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Introduction

Two versions of the pessimistic meta-induction (PMI) rely heavily on evidence from history of science. These versions of the PMI run as follows: there exists a number of examples of past scientific theories that are (ontologically, referentially, predictively) incompatible with our best-available scientific theories. Furthermore, these theories were mature and empirically successful.

Under the first (weaker) version of the PMI, maturity and empirical success of a scientific theory are therefore not indicative of a theory being (probably approximately) true. Therefore, the no-miracles argument (NMA) is refuted. Under the second (stronger) version, scientific realism PER SE is refuted by way of a REDUCTIO: we have good reason to believe that most currently accepted theories that are mature and successful will be replaced by future theories that are (ontologically, referentially, predictively) incompatible with present theories.

However, the disciplines of science and history of science have a long history of differing theories that were, for a while, mature and successful, but incompatible with presently-accepted theories. Specifically, these many incompatible theories are *whether certain past scientific theories were genuinely mature or predictively successful*.

This issue targets a key premise in both versions of the PMI: if there exists a long history of successful and mature theories that are incompatible with currently accepted theories, we either cannot infer maturity and success indicate (probable approximate) truth or we must believe current theories will be eventually replaced.

But if this reasoning holds, then we must make the same inferences about current theories presented by historians and scientists about whether past scientific theories were genuinely mature or successful. Therefore, the anti-realist that accepts this key premise of both the strong and weak versions of the PMI (that is, a past history of failure is not indicative of probable approximate truth in some domain) cannot reasonably accept that past scientific theories were once mature and successful.

This result is, however, manifestly *absurd*. Furthermore, the anti-realist *must* accept that past theories were genuinely mature and successful. Otherwise, they are left believing a conditional without affirming the antecedent. Thus there is an instability for the anti-realist: on the one hand, anti-realists that accept either version of the PMI must accept that some past scientific theories were genuinely mature and successful, but rely on reasoning for accepting that past scientific theories were genuinely mature and successful that is at odds with the PMI; on the other hand, the reasoning in both versions of the PMI exclude the possibility of accepting evidence that past scientific theories were once mature and successful.

In sum, if the anti-realist accepts that past scientific theories were mature and successful based on historical and scientific evidence, they must subsequently reject the soundness of both versions of the PMI. If the anti-realist accepts either version of the PMI, the anti-realist should not accept the evidence that past scientific theories were mature and successful. Thus both versions of the PMI are self-undermining.

1 The no-miracles argument

There are many different versions of *pessimistic meta-inductive arguments* (PMI). These arguments are often attributed to Larry Laudan (1981). Since Laudan, philosophers have developed two distinct *types* of PMI arguments. Some PMI arguments have stronger conclusions than others, targeting a number of key theses accepted by scientific realists; others are more limited, and target a specific argument for scientific realism, known as the *no-miracles argument* (NMA).

The NMA usually runs as something approximating the following:

(R1) Presently-accepted (mature) scientific theories are (predictively, experimentally, etc.) successful.

(R2) The best explanation of the success of (mature) scientific theories is that these scientific theories are (approximately) true.

(R3) Therefore, presently-accepted (mature) scientific theories are (probably approximately) true if they are (predictively, experimentally, etc.) successful.

According to the NMA, the (probable approximate) truth of scientific theories is the best explanation for (predictive, experimental) success, or as Putnam put it, this explanation is the only available explanation 'that does not make the success of science a miracle' (Putnam 1975, p. 73).

In what follows, I primarily address a strictly weaker version of the PMI that targets the NMA, although the stronger version relies on a stronger version of the core premise: many (perhaps a majority of) past scientific theories were mature and successful. If the weaker version falls, the stronger version falls with it.

2 The pessimistic meta-induction

This weaker version of the PMI takes the form of a following *reductio*. It is partly based on the formulation popularised by (Lewis 2001, p. 373), (Psillos 1996) and (Saatsi 2005).

(P1) (Mature) scientific theories are (predictively) successful.

(P2) If there is a long history of (mature) scientific theories that are each successful but are (ontologically, referentially, predictively) incompatible with present (mature) scientific theories, then scientists cannot reasonably infer (mature) scientific theories are (probably approximately) true based on predictive success alone.

(P3) Many past (mature) scientific theories were for a time successful and are (ontologically, referentially, predictively) incompatible with present (mature) scientific theories.

(P4) Scientists cannot reasonably infer (mature) scientific theories are (probably approximately) true based on predictive success alone.

One realist tactic has been to show how current historical and scientific evidence fails to support (P3): these rejected scientific theories were not mature or successful (or, perhaps, were not as mature or successful as presently-accepted scientific theories). The realist's response to the PMI then follows the usual path of determining whether these examples satisfy whatever conditions of maturity and success they adopt as licensing the inference from maturity and success to (probable approximate) truth. This debate has gone on for some decades.

This debate need not go on any longer in order to undermine versions of the PMI that rely heavily on history of science: the very existence of this longstanding dispute reveals that it is full of incompatible theories–be them historical and scientific–that cast doubt over whether many past scientific theory was genuinely mature or successful. Following the reasoning in (P2), this entails the anti-realist cannot accept (P3), and therefore the anti-realist, even if they should believe that more examples of past scientific theories were once mature and successful can be provided, cannot accept the PMI.

2.1 Motivations for accepting PMI

What motivates accepting the PMI? The anti-realist rightly focuses on the disconnect between (at least in retrospect) fleeting success and (probable approximate) truth. Employing this form of inference from success to probable approximate truth outside the most mundane cases of engagement in everyday circumstances is–at least to the anti-realist-*overly epistemically optimistic*.

This optimism lets in a large number of incompatible beliefs, for many false theories will appear (for a time) both mature and successful. This is not in dispute. Even with restrictions to mature scientific theories and to probable approximate truth, the realist must deal with this uncomfortable fact: 'at least some past theories that pass all realist tests of maturity and success are still considered false' (Psillos 1996, S307).

Implicit in the motivation to adopt the PMI is that the NMA does not or should not cover beliefs that extend beyond the empiricist's 'safe home' of *commonsensical beliefs*. Some commonsensical beliefs, like proverbs, folk physics and folk remedies, may be in error, or (ontologically, referentially, predictively) contradict our current folk theories. Furthermore, it is possible that perceptual and phenomenological beliefs may also err. However, on the whole, the empiricist's 'safe home', while fallible and subject to revision, does not exhibit (so the anti-realist claims) the same degree of constant ontological, structural and referential revision present in disciplines such as the natural sciences.

So long as there is a long history of incompatible mature and successful theories in some discipline and we attempt to infer from the present maturity and success of best-available theories to their probable approximate truth, we enter dangerous territory: we can no longer make the inference from the maturity and success of best-available theories to their (probable approximate) truth.

3 The pessimistic meta-induction is self-undermining

Most versions of the PMI are almost exclusively directed at the natural sciences, rather than other modes of inquiry outside the 'safe haven' of commonsensical beliefs. But other modes of inquiry suffer from a long history of theories on the scrapheap. The motivations for adopting the PMI transfer over to other disciplines of disrepute, such as the discipline of history. Furthermore, history of science relies *heavily* on scientific theories.

Does the discipline of history suffer from a long history of theories that were (for a time) accepted, mature and successful and are incompatible with our best historical theories? The answer is 'Yes'. Do these theories often rely on scientific theories? The answer is also 'Yes'. Thus the anti-realist cannot, by the PMI's lights, accept that past scientific theories were genuinely mature or successful. But what evidence or arguments would convince the anti-realist?

3.1 The PMI in historiography

An anti-realist may object that the discipline of history of science is dissimilar from the natural sciences: to many non-historians the reports of historians are taken as part of an immutable archive or repository of the past. Based on this archive, the historian conveys a narrative, a series of events, and reconstructs the social, economic, religious and material conditions that motivate the actions of historical figures and communities.

However, many historiographers conclude that historical scholarship is fundamentally untrustworthy (White 1973; Ankersmit 1994; LaCapra 1985; Kleinberg 2017): these historiographers argue historians fail to provide anything over and above a historical narrative reflecting the beliefs of the author, no different in standing than works of fiction. But why would historiographers make such a pessimistic conclusion?

The discipline of history may be understood as part of the empirical sciences–as a well-honed form of *Wissenschaft*–that tackles problems so complex they require inter-generational attempts at positing explanations, re-examination of the data, and re-examination of the standards accepted by the community.

Historiographers, examining how differing historians at different times have come to differing and mutually incompatible theories answering questions about *Who, what, where, in what order, and how* certain events occurred, make the same inference the anti-realist does: there is a long history of apparently successful and mature theories in the discipline of history that were eventually superseded by incompatible theories. By the same reasoning set out in (P2), the historiographer concludes either we lack any grounds to believe we know what happened in the past or that our current best theories about what happened in the past will be superseded.

For one such example, we can turn to a simple question about the extent of economic growth in England during the industrial revolution in the later eighteenth and early nineteenth centuries. This question should be, presumably, easily answerable.

In the 1950s and 1960s, Phelps-Brown and Hopkins collected data examining the wages of craftsmen during the industrial revolution and collected it in a wage index. However, historians disagreed over interpretations of the data (Hudson 2014, p. 30), which data should be analysed (ibid., p. 31), what could be inferred from the data (ibid., p. 1) and which theories best explained the data (ibid., p. 137).

Based on the data available in the wage index, historians inferred a period of dramatic economic growth (ibid., p. 37). However, Deane and Cole's work, based on alternate data, lead Hoffman to develop his own index and revise the estimate of economic growth downwards, concluding 'that more rapid growth began as far back as the 1740s and that change was a gradual and cumulative process' (ibid., 38, cf. 25). Harley pushed for an even more radical lowering of the estimate of economic growth, leading to calls to revise the very idea of an 'industrial revolution' (ibid., p. 38). Later historians like Berg and Hudson did not merely question the data collected by Harley but contested the interpretation of the data that looked at national aggregate statistics, rather than regional development (Hudson 2014, p. 123).

Within a few short decades, the consensus amongst historians fluctuated between several different interpretations of the historical data, which of the indices should be adopted, and numerous incompatible historical theories. While the underlying ontology and referential structure remained relatively fixed in comparison to theories in the natural sciences, the apparent success of each mature theory was not indicative of (probable approximate) truth of any one theory.

Historians of science are (by supposition) no different than other historians in their methods and available evidence, and history of science has (again, by supposition) a similar history of revision and replacement of whether particular past scientific theories were mature and successful. If so, the anti-realist must reject the evidence supporting (P3).

3.2 One potential objection: comparing irrelevant sets

Something may not set right with you upon hearing this argument. You may not be interested in history of trade, geology, the dating of ancient bones or texts, history of war, and so on. *These* historical disciplines may have (by supposition) a long history of incompatible theories; *you* are interested in *history of science*, and I have yet to show you that there exist a long history of mature, successful and incompatible theories about whether past scientific theories were genuinely mature and successful.

There are two problems with this response. First, this restriction to *history of science* provides grounds to severely weaken the PMI. Second, there remains a long history of incompatible theories over whether past scientific theories were genuinely mature or successful.

3.2.1 A fork

One objection to this line of reasoning may be that appealing to disciplines other than history of science is the lack of specificity in what is meant by 'history': It is an accident of academic administration that the label 'history' is applied to encompass a number of unrelated subjects addressed by 'historians': historians working on translating ancient Sumerian cuneiform tablets often have a different history of success or failure, access to historical data, methodology and standards when compared to historians working on history of early twentieth century theoretical physics in the British and Austro-Hungarian empires. It would be improper to make the inference from a history of failures in one part of a discipline to another, unrelated part.

Two analogies serve to support the reasoning behind this response: it would be unreasonable to blame the son for the sins of the father; I have misattributed blame if I shame my neighbour for my failure to turn off my lights when I leave for work.

Similarly, a long history of many differing and incompatible translations of core parts of Babylonian texts, each supported by the best available empirical evidence at the time, and each reliant on specific scientific dating procedures and other scientific tools, is *one* thing; whether the 1919 Eddington experiment corroborated Einstein's theory of gravity and refuted Newton's is another. Just as we restrict the relevant class to mature scientific theories that were successful, we must similarly restrict the relevant class to mature theories in *history of science*.

This objection is on to something important; however, here is the rub: we should not conclude, for example, that (by supposition) because there existed numerous (mature) scientific theories in biology that were predictively successful and were (ontologically, structurally, predictively) incompatible with one another, that we therefore lack the grounds to infer that present (mature and successful) scientific theories in other, separate disciplines are (probably approximately) true.

The biological sciences may simply have (by supposition) a long history of incompatible mature and successful theories; other disciplines in the natural sciences may not. Perhaps, in some disciplines, once theories were genuinely mature and successful *enough*, there has almost always been (more or less) consensus within one scientific discipline.

In sum, if domain-carving into 'history' is an administrative concern, so is sorting different disciplines together under the heading of 'science'; we should be interested in whether there exists a history of repeated failures of mature and successful theories within a *particular research programme*, not the entirety of what is lumped together as 'science' or 'history'.

More generally, ascriptions of blame to past failures in one research programme do not license ascriptions of blame to other research programmes. Consequently, the realist is permitted to believe that whatever research programmes that lack a history of incompatible mature and successful theories are probably approximately true based on a NMA.

3.3 A further reply: shared methods

You may object: the PMI targets inferring from maturity and success to (probable approximate) truth if there is a history of disrepute *using particular methods*. Since there is (by supposition) a shared methodology in the natural sciences, failures in one domain in the 'sciences' undermine inferring from success in all domains in the 'sciences'.

If so, if any saliently similar methods are shared between historians and scientists, or if historians are reliant on the findings from scientists, or if all historians share saliently similar methods, this indicates that mature and successful theories in the discipline of history of science are not (probably approximately) true, even if we feel confident that the content of the two subjects differ considerably.

In summation, we face a fork: either the PMI is far weaker than it appears to be, and furthermore, if historians of science rely on the findings of the natural sciences or share saliently similar methods to other historians, then the PMI is self-undermining; or the anti-realist concludes the PMI entails undermining reasons to accept that past scientific theories were mature and successful, and the PMI is self-undermining.

3.4 Why think past scientific theories were mature or successful?

Consider the question of whether a number of theories were *genuinely* mature and successful. Many different theories were proposed in history of science. Some of these theories were well-entrenched within the scientific community; others were not. Some were empirically and predictively successful; others were not.

It is illustrative to examine Laudan's theories that he considered 'both successful and (so far as we can judge) non-referential with respect to many of their central explanatory concepts' (Laudan 1981, p. 33):

- 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 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.

> This list, which could be extended *ad nauseam*, involves in every case a theory which was once successful and well confirmed.

In each of these cases, while the theories may have (more or less) fit the available empirical evidence for some time, we must ask whether these theories were *genuinely* mature and successful. However, historians, philosophers and scientists object to including a number of theories on Laudan's list.

For example, the theory of crystalline spheres–that planets were embedded in impenetrable, three-dimensional objects–may never have been *genuinely* successful, even according to the standards accepted at the time. One obvious example was the inability of the theory of crystalline spheres to adhere to any of the available astronomical data, thus failing to have been a successful scientific theory:

> … the maximum distance of a lower planet was assumed to be equal to the minimum distance of the next highest planet. Unacceptable

> gaps occurred, however … [by] measuring solar and lunar parallax, … the space so obtained failed to correspond exactly to the spheres nestled therein.' (Westman 2011, 58, cf. 291).

Historians, scientists and philosophers have presented a number of compelling arguments for why the other theories on Laudan's list should be now considered immature or unsuccessful. This is the usual realist 'historical gambit': nowadays, the humoral theory of medicine and effluvial theory of static electricity does not appear to have ever been mature (Psillos 1999, p. 108); more recent historical scholarship now says the 'catastrophist' geology under 'a universal (Noachian) deluge' was generated 'retroductively' to fit the available evidence and was never successful by the time it was abandoned (Baker 1998); some philosophers argue the theory of circular inertia may never have been predictively unsuccessful (Worrall 1994, p. 335); and so on. A literature review on the realist's 'historical gambit' can be extended *ad nauseam* (Park 2011). The only undisputed remaining theories on Laudan's list are phlogiston, caloric and optical aether, although even then there remains debate over caloric theory (cf. (Chang 2003; Psillos 1994)).

You may object to the realist's 'historical gambit' as follows: *other* theories may supplant the ones on Laudan's list, like Rankine's vortex theory (Hutchison 2002), Kirchoff's theory of diffraction (Saatsi and Vickers 2011), Kekulé's theory of the Benzene molecule (Lyons 2002), Dirac's relativistic wave equation (ibid.), pre-inflationary big bang theory (ibid.) and Bohr's and Sommerfeld's models of the atom (Vickers 2012). All are plausible candidate theories.

The 'historical gambit' has not succeeded in undermining either version of the PMI; however, the 'historical gambit' now serves a different role: there exist a history of competing, incompatible and mature theories over whether a particular scientific theory was genuinely mature and successful. This introduces a PMI about whether we have grounds to accept current theories about whether past scientific theories were genuinely mature or successful.

As an illustration of the difficulties faced by historians of science, there have been differing theories between historians and scientists over, for example, related astronomical theories about the solar system were primarily adopted for theological, astronomical, astrological, instrumentally pragmatic, or personal reasons (Westman 2011); if the experiments and observations were accurate (Graney and Grayson 2011); if the available forms of technology were reliable or unreliable (and to what extent) (van Helden 1985); what these astronomical theories entailed (Westman 2011); if a 'Copernican revolution' occurred (Kuhn 1957; Grant 1984); and if there were any clear crucial experiments in astronomy until long into the nineteenth century (Gingerich 2011, p. 136).

One example puts into stark relief the difficulties in determining whether a historical claim is (probably approximately) true. Consider the eminently plausible theory:

> (Crucial Test): By 1911, Einstein's theory of gravity was relatively mature. It then survived a surprising crucial test in the early part of the twentieth century, and was therefore experimentally or predictively successful. However, Newton's theory, while no doubt mature, failed to survive a crucial test, and was therefore rejected as experimentally or predictively unsuccessful.

At the time of the publication of Sir Arthur Eddington's photographic plates of a solar eclipse, the popular picture is that astronomers and physicists almost universally accepted the results. In fact, '[n]umerous reanalyses between 1923 and 1956 of the plates used by Eddington yielded the same results within ten percent' (Will 2015, p. 5).

However, by the early 1970s, a number of physicists have re-evaluated the available data and found it was not a crucial test of either theory. The number of confounding variables involving atmospheric effects, differences in temperature, movement of the telescopes and potential minute variations in the lenses all lead to a fairly definitive conclusion: this was not a crucial experiment (Kennefick 2011).

A later 1979 reanalysis of the plates using more modern methods (Harvey 1979, p. 195) vindicated the theory: it was a crucial experiment after all.

And yet, in 1980, philosophers John Earman and Clark Glymour charged Eddington and members of his team with throwing out data that appeared to support Newton's theory (Earman and Glymour 1980). In short, 'The British results, taken at face value, were conflicting, and could be held to confirm Einstein's theory only if many of the measurements were ignored' (Earman and Glymour 1980, pp. 50-51).

Furthermore, Earman and Glymour also point to a number of unpublished contemporaneous attempts at conducting crucial experiments. Campbell and Herber Curtis analysed plates from a 1990 eclipse and 1918 eclipse and found the results vindicated Newton's theory, not Einstein's; after a 1922 eclipse in Australia, Campbell and Robert Trumpler reported measurements that conflicted with Eddington's (Bertotti, Brill, and Krotkov 1962).

Further experiments were conducted in 1947 and 1952, although accuracy remained low and produced several different measurements 'giving values anywhere between three-quarters and one and one-third times the general relativistic prediction' (Will 2015, p. 6).

In the mid to late 2010s, more attempts at reanalysis of the data indicates that Eddington's experiment was not a crucial experiment due to a separate issue–comparing the plates exposed during the solar eclipse with the control plates (Schaefer 2017). In fact, although at least six (apparently) successful experiments using more advanced technology appear to be in line with Eddington's findings, systematic issues with developing photographic plates remained up until the later part of the twentieth century (Will 2015, p. 8) and early part of the twenty-first century, in which new technology permitted running the first modified version of the experiment using modern-day digital cameras (Schaefer and Hynes 2018).

Is Crucial Test (probably approximately) true? If we were to be writing in the 1920s, perhaps the answer would be 'Yes', but the answer could very well have been 'No' had we been informed about the results of experiments that were not publicised at the time; if we were writing in the 1970s or 1980s, the answer may have been 'Yes' if we took (Harvey 1979) at face value, but 'No' in light of (Earman and Glymour 1980) some years later. Or perhaps, if we found issue with the photographic plates of all experiments up until the development of digital plates, as (Schaefer 2017) argued decades later, we never would have thought these results could be accepted.

This historical case-study of a purported crucial experiment is ripe for a PMI: any current attempt at determining whether Crucial Test was (probably approximately) true is faced with a history of mutually incompatible historical and scientific theories over whether or not Crucial Test was (probably approximately) true. Following the reasoning of (P2), we must conclude we lack the requisite grounds to infer whether Crucial Test is (probably approximately) true. But if so, then the PMI entails the anti-realist cannot accept any current theories by historians and scientists over whether the Eddington experiment confirmed Einstein's theory and refuted Newton's.

3.5 Another reply to the argument: different degrees of maturity and success

One may retort that while it may be true that many historians, philosophers and scientists have had differing theories over whether past scientific theories were mature or successful, it does not lead to concluding that *all* current plausible cases of mature and successful past theories are in disrepute. We can correctly determine that at least *some* past scientific theories were mature and successful according to reasonable standards of maturity and success.

What would be some the plausible reasons provided by the anti-realist were they to defend this decision? Perhaps the anti-realist responds as follows:

(a) Modern-day historians and scientists are often operating with far more exacting *experimental standards* than previous historians and scientists (therefore escaping the PMI). If there is a current theory about whether a past scientific theory was successful and mature that is *itself* mature and successful, and based on a number of scientific theories that are mature and successful, it is (probably approximately) true.

(b) The theories from present historians and scientists are often usually *better* theories–more systematic, unifying, broad and deep in their explanatory content–than the theories of past historians and scientists (therefore escaping the PMI). We have reason to believe that if mature and successful theories entail that some past scientific theories were once mature and successful, these theories are (probably approximately) true.

(c) Modern-day historians and scientists have access to a substantially greater amount of *evidence* than past historians and scientists, and their theories incorporate more historical and scientific data than past theories; they are also *far* more mature and successful than past theories (escaping the PMI). We have reason to believe that if their best-available theories entail that particular past scientific theories were mature and successful, and these best-available theories are also mature and successful, then these theories are (probably approximately) true.

If the anti-realist were to deny (a)-(c) and accept the strong version of the PMI, they have good reason to reject (P3): this version of the PMI leads us to conclude that we lack the requisite grounds to infer that simply because a number of compelling theories were once accepted by historians of science that they will continue to be accepted by historians of science in the future.

If, however, the anti-realist were to adopt the weaker version of the PMI and admit merely that (a)-(c) are not good reasons for believing particular past scientific theories were mature and successful, then the anti-realist lacks any plausible reason to accept (P3).

However, any stated reason for accepting (a)-(c) can be provided in support of realist objections to the PMI.

Consider a variation of (c): history of science is a relatively new discipline, and historians, philosophers and scientists are bound to make mistakes. The standards for maturity and success will inevitably become stricter. Newer theories will incorporate more data, and unify disparate information. These forms of scholarship have simply reached this level of analysis that permits making correct determinations that some past scientific theories were genuinely mature and successful. In sum, our current theories are more mature and successful than past theories.

However, this response from the anti-realist destroys the grounds for accepting the conditional in (P2), for the anti-realist must face an optimistic meta-induction (OMI): 'our current best theories enjoy far higher degrees of success than any of the refuted theories in the past, which enjoyed only fairly low degrees of success' (Farbach 2011b, p. 1285).

As Farbach claims, we may assume the examples provided by Laudan may show a long history of incompatible scientific theories that were *modestly* successful, but our current theories have a very high degree of success (Farbach 2011a). What is good for the goose is good for the gander: by accepting this form of reasoning to salvage the grounds for accepting that past scientific theories were genuinely mature and successful, the intransigent anti-realist opens the door for the realist to accept a scientific OMI.

Version of the PMI that rely heavily on historical evidence has been superseded by the OMI, for the OMI undermines (P2), and on grounds that are eminently plausible to the intransigent anti-realist: there exists a salient difference between presently-accepted and past scientific theories, *viz.* their differing degrees of maturity and success. Thus the anti-realist must begrudgingly accept the OMI.

4 Conclusion

A summary: the discipline of history of science must be relied on by the anti-realist for the anti-realist to accept that past scientific theories were once genuinely mature and successful. However, the long history of differing theories about whether a theory was genuinely mature or successful entails the PMI, which undermines accepting (P3).

But that result is absurd: reliance on the theories from history of science and science are necessary to accept (P3). But this means accepting that the PMI does not apply to history of science, a discipline that, if not easily distinguishable from the natural sciences, is on equal or weaker ground than the natural sciences. Thus (P2) relies on a form of fallacious reasoning. Therefore the weaker version of the PMI is self-undermining. But if the weaker version should fall, the stronger version of the PMI falls with it. Thus both versions of the PMI are self-undermining.

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