What's new about the New Induction?*

P.D. Magnus

Publshed in *Synthese*, 148(2): 295–301. January 2006.
This penultimate draft of the paper is
available on-line at `http://www.fecundity.com/job`

The problem of underdetermination is thought to hold important lessons for philosophy of science. Yet, as Kyle Stanford has recently argued, typical treatments of the offer only restatements of familiar philosophical problems. Following suggestions in Duhem and Sklar, Stanford calls for a *New Induction* from the history of science. It will provide proof, he thinks, of "the kind of underdetermination that the history of science reveals to be a distinctive and genuine threat to even our best scientific theories" [Sta01, p. S12]. This paper examines Stanford's New Induction and argues that it— like the other forms of underdetermination that he criticizes— merely recapitulates familiar philosophical conundra.

1 From Empirical Equivalence to the New Induction

The problem of underdetermination is often thought be the same as the problem of empirically equivalent theories. Witness, for instance, recent discussions by Laudan and Leplin [LL91], Hoefer and Rosenberg [HR94], and Kukla [Kuk93]. A typical expression of the problem goes something like this: A theory T is empirically equivalent to the theory that everything is *just as if* T were true when in fact T is false. So our belief in T is underdetermined.

We might easily have specified the rival to T as a worry about dreaming or demonic deception. In this way, there is nothing special about T being a *scientific* theory. The problem is no different than skeptical puzzles as they have been formulated at least since the time of Descartes. Thus, Stanford argues, the mere possibility of empirically equivalent rival theories is "no more than a salient presentation of the possibility of radical or Cartesian skepticism" [Sta01, p. S3]. In short, it is old news.

*I am indebted to Kyle Stanford for helpful comments on my earlier response to the New Induction [Mag03, pp. 151–5], to Craig Callender for discussions of the New Induction during a seminar in Fall 2002, and to a number of anonymous referees.

The problem of underdetermination *qua* problem of empirically equivalent theoires, Stanford thinks, is simply not very interesting. It says nothing particularly about science, and it reduces to a familiar philosophical puzzle. He goes on to pose a problem which he thinks would be a kind of underdetermination worth taking seriously. He calls his argument the New Induction.

2 Schema for the New Induction

The historical record, Stanford says, is full of episodes that fit this schema: At some past time, scientists accepted a theory T that was superior to all its *acknowledged* rivals. Subsequently, a rival U was developed that came to replace T. The evidence that favored T over its acknowledged rivals also favored U. The choice between T and U was underdetermined, even as scientists assented to T. T was underdetermined relative to a rival that scientists hadn't yet imagined.

A typical instantiation of the schema might be this: Classical mechanics enjoyed wide acceptance from the time of Newton through the cusp of the 20th century. In retrospect, however, we say that it was successful because it is a limiting case of relativistic mechanics. Classical mechanics T *was* superior to Aristotelean mechanics and other rival theories that scientists considered. It lost out not to one of those rivals, but to the then-unimagined relativistic mechanics U. (I'll return to this example below.)

For the New Induction, the rival theories T and U are not empirically equivalent; later scientists find evidentiary grounds to prefer U. To use the term Stanford takes from Sklar [Skl85b, p. 30], the underdetermination is *transient*. The underdetermination obtains only up to a specific historical development. It's just that one of the rivals was not available to scientists before this development. After it, standards favor one theory over the other.

3 Filling out the schema

Stanford admits that he is "unable to do more... than suggest... the verdict of the historical record" [Sta01, p. S11], but he nevertheless provides a flurry of examples. He mentions mechanics, chemistry, embryology, thermodynamics, electromagnetic theories, theories of disease, theories of light, theories of inheritance, theories of evolution, "and so on in a seemingly endless array of theories, the evidence for each of which ultimately turned out to support one or more unimagined competitors just as well." He concludes: "Thus, the history of scientific inquiry offers a straightforward inductive rationale for thinking that there typically are alternatives to our best theories *equally* well-confirmed by the evidence, even when we are unable to conceive of them at the time" [Sta01, p. S9, my emphasis].

One might worry about Stanford's insistence on *equal* confirmation of T and U; if degree of confirmation were a function from evidence and a theory to a real number, then we should expect that two different theories would *never* be equally well-confirmed. This objection, however, would be spurious. As Stanford notes, two theories may have different evidential successes and different anomalies. During periods where the old theory is weighed down by anomalies but the new theory is yet to score a panoply of successes, the choice between them is underdetermined. Such a period may be protracted, especially as conservative scientists work to extend the old theory and reformers refine the new one. Thus, it is implausible to insist that confirmation is on a knife edge.[1] Two theories may be remain (nearly enough) equally well-confirmed over a period of time even as evidence changes. In what follows, for reasons of readability, I write 'as well-confirmed' without adding 'effectively' or 'nearly enough'.

Suppose, with that caveat, that we look to the history of science and find many cases that match the schema. We might conclude from this that our present theories are underdetermined against some as yet unimagined rivals. Of course, instances of this schema being *common* is insufficient to support an inductive generalization. One would further need to know something about how observed instances relate to the population of past and present theories; cf. [Lew01] and [MC]. Let's set this concern aside and consider what would be required for there to be even a single instance that fits the schema.

In a typical case, a new theory U is formulated during a period in which the prior theory T sags under the weight of anomalies. In the ensuing controversy, some scientists develop U while others try to accommodate the anomalies within T. During this period, the choice between T and U is genuinely underdetermined. As further evidence is collected, the underdetermination ends and scientists rightfully come to see U as superior to T. This story so far is compatible with what Philip Kitcher [Kit93, ch. 6] calls the *compromise model* of scientific change, a model which acknowledges that things might legitimately be up in the air during controversies but also that controversies might be closed by the weight of evidence and argument.

The New Induction requires more that this. It is not enough that the choice between T and U be underdetermined during legitimate controversy. If we are to conclude anything about our uncontested present theories, then the choice must also *have been* underdetermined before the controversy, before there was pressure for U to be formulated— that is, before T was in crisis. Are instances like this so easy to find?

4 The example of mechanics

Take the example of classical and relativistic mechanics. There was clearly a period in the late 19th century— with irregularities in the perihelion of Mercury, the absence of ether drift, and so on— when classical mechanics faced anomalies unlike ones it had faced previously. Surely, there is some time before the actual introduction of special relativity when it would have been a viable contender.

[1] If the worry was that two arbitrary real numbers will almost never be equal, then it may be avoided by representing degree of confirmation as a function from evidence and a theory to a real interval.

(This could only fail to be the case if special relativity was proposed at the first instant in history in which it would have been viable. Even if this was so, it would be far-fetched to imagine that such extreme timing characterized all or even most of Stanford's examples.) Yet to be an instance for the New Induction, special relativity must have been as well-confirmed as classical mechanics before the anomalies developed. Is it reasonable to think that special relativity was well-confirmed in the late 18th century, even as Kant penned a metaphysical foundation for classical mechanics?

Perhaps this equality is something Kant could have recognized— if only he were to have considered relativity. If it were, then there would be a clear reason to see this as an instance for the New Induction. That is: If expert, situated practitioners could have acknowledged the point, then we 21st-century armchair critics should acknowledge it, too. Yet there is no straight-forward way to determine what Kant would have thought if he had entertained relativity but considered only the evidence available at his time. Whether Kant would have given as much credence to Einstein as to Newton is not a question that historical evidence can decide directly. It would surely depend on differences of interpretation, and these might be just as contentious as the New Induction itself.

Suppose instead that we divorce the alleged equality between classical and relativistic mechanics from anything that Kant or other historical actors would have said about it. The equivalence, one might hope, can be shown as a theorem in confirmation theory. The proof might go something like this: There are only detectable differences between classical and relativistic mechanics in the perihelion of Mercury and other phenomena that had not been observed with sufficient precision in 1780. Before precise observations had been made, there was no ground for picking one theory over the other.

The stand-off between classical mechanics and relativity in 1780 now seems like the stand-off between 'Emeralds are green' and 'Emeralds are grue' in 1980 (where 'grue' means 'green before the year 2000 and blue afterwards.') Both rivalries exhibit a kind of transient underdetermination such that observations available at the time could not discriminate between the rival theories. The rivalry between 'Emeralds are green' and 'Emeralds are grue' is not an instance for the New Induction— to be sure— since 'Emeralds are grue' never gained wide acceptance. The point is merely that (1) the claim that classical mechanics and relativity were equally well confirmed in 1780 is a necessary condition for that rivalry to be an instance of the New Induction and that (2) if those two were equally well confirmed in 1780, then 'Emeralds are green' and 'Emeralds are grue' were equally well confirmed in 1980. If there is no way to decide against grue-like hypotheses, then scientific theory choice would be impossible— but that's old news. If there is some way to decide against grue-like hypotheses, then by a double modus tollens the original case is not an instance for the New Induction. The same may be said *mutatis mutandis* for other putative instances. Now it seems as if the New Induction either fails or trades on an old chestnut of confirmation theory. Empirical equivalence might reduce to Cartesian scepticism, but the New Induction seems to reduce to a problem of induction.

Saving the New Induction from such a fate would require showing that (2) is false, that there is some asymmetry between the two cases. One might suggest that the critical difference between relativity and a grue-like hypothesis is that the grue-like predicate has an explicit expiration date— the time when the transient underdetermination will end is built into the theory itself. This might be an important difference. So consider instead the rivalry in 1980 between 'All emeralds are green' and 'Emeralds are germat' (where 'germat' means 'green usually, but blue if within 3 meters of someone who knows a proof Fermat's last theorem.') The latter does not have an explicit expiration date. It's just that nobody had made observations in 1980 which could have decided directly between the two. One might object that including Fermat's last theorem is outré for some reason or other, but including the ratio of an object's velocity to the speed of light might have seemed similarly outré in 1780.

5 Scientific respectability

Imagine there is an ahistorical answer to whether a possibility is outré or scientifically respectable. If so, then the New Induction would not require showing that Kant or his contemporaries should have taken relativity seriously. It would be enough that physicists came to take it seriously— since they eventually took it seriously, it *had always been* respectable. Unfortunately, there does not seem to be any such ahistorical fact. Scientific respectability is contextual and historical. Physicists took relativity seriously because it was able to deal with specific anomalies. If the anomalies had not arisen, relativity would never have counted as a serious candidate. Similarly, one can imagine observations which would have made 'Emeralds are germat' a contender— imagine that chromatic aberations had become commonplace following Andrew Wiles' published proof. Such observations would have been rather shocking, and without them 'germat' is a philosopher's predicate rather than a scientist's. What is scientifically respectable depends in part on what observations we have made, on what the world is like, and on what we believe it to be like. (This need not entail any sort of anti-realism. Savvy realists such as Boyd [Boy82] already acknowledge this contextuality of scientific standards.)

What one needs if this is to count as an instance for the New Induction is a contextually-savvy argument for the conclusion that classical mechanics and relativity were equally well confirmed in 1780— an argument that does not also entail that 'Emeralds are green' and 'Emeralds are germat' were equally well confirmed in 1980. Regarding cases like this one, Stanford takes his cue from Sklar. Yet Sklar does not suggest, as Stanford does, that Einstein and Newton were ever on equal footing. Rather, he thinks that the history of science shows us how "the wealth of data previously taken to support Newtonian theory was, *when taken in conjunction with new data incompatible with the older theory*, equally supportive of novel theories incompatible with the Newtonian" [Skl85a, p. 149, my emphasis]. Sklar's point, then, is that present evidence may form *part* of the *body of evidence* that eventually favors a different theory than the one we favor now. This is true, but it does not follow that the present body of evidence favors the future theory.

6 Conclusion

Stanford makes a telling point against underdetermination as it is usually understood: It ought not count as a distinct philosophical problem if, when scrutinized, it just reduces to one or another philosophical commonplace. Yet Stanford's New Induction, like the arguments he criticizes, fails to pose a novel problem under the rubric of underdetermination.

References

[Boy82] Richard [N.] Boyd. Scientific realism and naturalistic epistemology. *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association (1980)*, 2:613–662, 1982.

[HR94] Carl Hoefer and Alexander Rosenberg. Empirical equivalence, underdetermination, and systems of the world. *Philosophy of Science*, 61:592–607, 1994.

[Kit93] Philip Kitcher. *The Advancement of Science*. Oxford University Press, 1993.

[Kuk93] André Kukla. Laudan, Leplin, empirical equivalence and underdetermination. *Analysis*, 53(1):1–7, January 1993.

[Lew01] Peter Lewis. Why the pessimistic induction is a fallacy. *Synthese*, 129:371–380, 2001.

[LL91] Larry Laudan and Jarrett Leplin. Empirical equivalence and underdetermination. *The Journal of Philosophy*, 88(9):449–72, 1991.

[Mag03] P.D. Magnus. *Underdetermination and the Claims of Science*. PhD thesis, University of California, San Diego, 2003.

[MC] P.D. Magnus and Craig Callender. Realist ennui and the base rate fallacy. Forthcoming in *Philosophy of Science*.

[Skl85a] Lawrence Sklar. Do unborn hypotheses have rights? In *Philosophy & Spacetime Physics* [Skl85c], pages 148–166.

[Skl85b] Lawrence Sklar. Methodological conservatism. In *Philosophy & Spacetime Physics* [Skl85c], pages 23–48.

[Skl85c] Lawrence Sklar. *Philosophy & Spacetime Physics*. University of California Press, Berkeley, 1985.

[Sta01] P. Kyle Stanford. Refusing the devil's bargain: What kind of underdetermination should we take seriously? *Philosophy of Science*, 68 (Proceedings):S1–S12, September 2001.