

Underdetermination or why &HPS makes a difference

It is a revealing experience to compare Quine's "Two dogmas of empiricism" with the relevant passages about underdetermination in Duhem's "The Aim and Structure of Physical Theory". While Duhem's book is filled with examples from the history of physics, in Quine's text references to the history of science are almost completely lacking. While Duhem, the acclaimed historian of physics, was practicing &HPS, Quine was not. My aim in this paper is to trace in their respective rendering of the underdetermination thesis the influence of their different stances toward the history of science and to establish that &HPS really makes a difference, in that Duhem's version of the thesis is more plausible as well as more useful for understanding the scientific enterprise.

Many authors have pointed out differences between Duhem's and Quine's underdetermination theses (Veullemine 1978, Ariew 1984, Quine 1986, Gillies 1993), without however linking these differences to Quine's and Duhem's attitudes toward the history of science. Both claim that evidence does not uniquely determine theory. But Duhem carefully imposes limitations on the scope of his thesis, while Quine refrains from doing so by stating claims that are all largely tenable only "in legalistic principle" (Quine 1986, 619). Arguably, the most important differences are (Gillies 1993): (i) Duhem accepts the existence of phenomenological sciences like physiology, that are not affected by the ambiguities of underdetermination. These trouble only theoretical statements in abstract sciences like physics. Quine denies any such distinction framing his discussion in an outright denial of the analytic / synthetic distinction *per se*. (ii) The scope of the thesis is specified differently by Duhem and Quine. Quine thought it to concern the whole of science, while Duhem restricted it to physics or even only certain groups of hypotheses within physics. (iii) Duhem makes reference to 'good sense' as an aid complementary to deductive logic which will (at least in most cases) tell the physicist which theory to choose in specific situations. Quine's account includes nothing of this sort.

It is quite straightforward why someone interested in an adequate historical reconstruction of the evolution of physics has to impose Duhem's careful and sensible limitations. Conversely, underdetermination was introduced by Duhem as a conceptual tool in order to account for historical episodes that could not easily be explained otherwise. I will elaborate on these issues in my talk.

The separation of the underdetermination thesis from its historical embedding has been detrimental in a number of ways: (iv) In a large bulk of the literature the focus has turned from discussing underdetermination in relation to the actual evidence at a specific time as in Duhem (1906) to the discussion of underdetermination in relation to all possible evidence as in Quine (1975), i.e. from observed events to observable events. (v) Much effort has been devoted to the issue of constructing *empirically identical* alternatives from existing theories, instead of searching the history of science for episodes when theories existed that were *equally strong at the time in terms of empirical adequacy*. (vi) Underdetermination is today mostly illustrated with examples drawn from current science instead of using much more convincing examples from the history of science. (The numbering is consecutive, because these issues also represent differences between a Quinean and a Duhemian rendering of the thesis.)

I will now elaborate, in which way these developments result from the neglect of historical perspective and why exactly I think these issues have been detrimental to the underdetermination thesis. Essentially they have opened up the underdetermination thesis to easy and ultimately fatal criticism as that in Norton (2008). (iv) The shift from actual to possible evidence has recently been criticized in an insightful book (and earlier articles) by Kyle Stanford (2006) who treats underdetermination in terms of recurrent transient underdetermination—in what is largely a Duhemian spirit. Obviously, someone interested in the historical reconstruction of science must consider actual instead of possible evidence.

(v) Directly dependent on this issue is the question if the theories that are compared must exactly agree on every possible observation, past and future. In most contemporary discussions this is assumed, just because underdetermination is mostly thought to be about possible evidence. However, if we relate underdetermination to actual evidence, the two theories may very well differ

with respect to events that have not been observed yet. Furthermore, they may even disagree in the reconstruction of events that have already been observed as long as the theories are on a par in terms of empirical adequacy and epistemic virtues.

Underdetermination is all about different theories, that are comparably strong concerning actual evidence, but that make different predictions with regards to some future experiments. Only in the aftermath of those experiments is it possible to adjust the protective belt of both theories in order to make both comply with the evidence. If underdetermination were really about possible evidence then both theories would always make the same predictions, which immediately opens the door for the fatal criticism that in all examples of underdetermination we are really talking merely about different formulations of the same theory.

Those who want to formulate underdetermination in terms of possible evidence would reply, that underdetermination is not about induction but that it is about different metaphysics accounting for the same observations. The point is that metaphysics and induction cannot and should not be separated. If metaphysics were really independent from the inductive business of science then why not choose an arbitrary one that works. Rather, the metaphysical basis of our theories is a necessary and indispensable guideline for the scientist how to behave in the world, which experiments to do, what theoretical problems to tackle next. Time permitting I will illustrate this by means of a largely neglected example of underdetermination in the history of science, that between electrodynamics as a field theory and in terms of action at a distance, of which Maxwell wrote in his *Treatise*: “[I]t is exceedingly important that two methods should be compared, both of which have succeeded in explaining the principal electromagnetic phenomena, and both of which have attempted to explain the propagation of light as an electromagnetic phenomenon, and have actually calculated its velocity, while at the same time the fundamental conceptions of what actually takes place, as well as most of the secondary conceptions of the quantities concerned, are radically different.” (1873, p. xii)

(vi) Notably, instead of this example always the much weaker case of the Wheeler-Feynman theory is cited as an example of underdetermination in electrodynamics. On the one side, it seems understandable to choose examples that are recent and concern current science, as those cited in Norton (2008 25-26). On the other side it is perplexing that Duhem did not care that most of his examples, e.g. that of corpuscular and wave optics, were already terribly outdated at the time he presented them. What is implicit in Duhem's choice of examples is the assumption, that our (or his) time is not much different with respect to evidence from previous historic situations, in particular that we are not and can never be in an essential way close to having all relevant evidence (cp. also Stanford 2006, Godfrey-Smith 2008). This point can be argued on the basis of Hume's problem. If we grant this point, then we are in fact permitted to go far back in history to choose relevant examples of underdetermination—even if much additional evidence has been accumulated since. Of course, now something beyond evidence must explain, why we are not faced with alternatives in crucial parts of today's science. Underdeterminists usually point to the social context as one piece of the puzzle: “If physicists had offered a price, they would have without doubt come up with a theory of optics based on that hypothesis [that light consists of particles], in accordance with Fourier's experiment.” (Duhem 1906, p. 307)

The questions concerning the influence of social context and the availability of all relevant evidence are at least in part historical. They require comparing our situation with situations in the past, when underdetermination actually existed, and thus they can only be answered in the context of &HPS. It stands as an &HPS-challenge to all opponents of the underdetermination thesis, that philosophers like Duhem and Kuhn, that were as informed in the history of physics as in the physics of their days, believed they could establish a historical case that the present situation with regard to evidence is essentially not different from that in the past.

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