Laudan, Laudan, Donovan, Ch 1 of Scrutinizing Science

Stephen Perry

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Motivations for the Project: The State of H & P

In their book, *Scrutinizing Science* [Donovan et al., 1992], Arthur Donovan, Larry Laudan, and Rachel Laudan propose a new research program, where history is to be used as evidence to test various philosophical theses about science. The project, as introduced by them, arose out of an evaluation of the then-state of historical philosophy of science:

- The newer models of scientific change developed by the "historical school" (Kuhn, Feyerabend, Lakatos, Laudan, Campbell, Toulmin, Stegmueller, Cohen, Holton, Shapere) were developed in opposition to those of the positivists (Duhem, Carnap, Bridgman, Reichenbach, Popper, Hempel)
  - Historical school's claims to better models of scientific change drew from grounding in study of the history of science, yet such models have not been "extensively or systematically tested against the empirical record" [1] [Donovan et al., 1992, p. 5]
  - Historical school often uses history as illustration rather than evidence, and the cases most often used in 1960s and 70s formulations of models have come under closer scrutiny recently
  - Case studies "testing" philosophical models of science to date have been insufficient by the standard of "even the most tolerant view nof robust experimental or quasi-experimental design" [Donovan et al., 1992, p. 6]
  - Loss of momentum in historical school is due to "no serious attempt... to determine the extent to which relevant evidence supports" their models [Donovan et al., 1992, p. 6]
- Several models of scientific change developed by those in the "historical school" have passed into common usage
  - This is further important due to the social and political imoprtance of science and its image[2]
- Work in the humanities is, in general, individualistic, whereas what is needed at least for this problem is "large-scale, collaborative work" [Donovan et al., 1992, p. 8]

[1] Nickles interpets this as supposing that the historical school is "guilty of the double-standard fallacy" [Nickles, 1989, p. 665]

[2] Although they mention the social and political import of an image of science, they do not further discuss what exactly the role of science is in these domains and how this should/does affect philosophers' and historians' studies of science.

- We should strive for consensus in science studies[3] [Donovan et al., 1992, p. xv, xvi, 22]

[3] This seems to presuppose that we need some single, universal, monolithic account of scientific change. It is an open question whether scientific change might be a pluralistic affair.

Methodological Goals: Taking Up the H-D Model for Historical Evidence

Based on these presuppositions about the state of historical philosophy of science, Donovan, Laudan, and Laudan propose a methodology with the following guiding tenets:

- Particularism/locality/disunity/lack of demarcation may be true, but that can only be established "*after*, not before, a sustained effort to identify the rule and rhythm of scientific change"[4] [Donovan et al., 1992, p. 12]
- The case-study approach is preferable to experiment, surveys, or ethnomethodological studies because the first is "obviously" inapplicable for this problem, and the latter two would necessarily involve much more interpretive work given the "notorious unreliab[ility]" of potential respondents' memories[5] and the place and scale at which decisions constituting scientific change take place [Donovan et al., 1992, pp. 11-12]
- It is possible and necessary to distill from the various models of scientific change a list of isolated empirical claims/theses in a (more or less) neutral language so that such claims may be made comparable[6] [Donovan et al., 1992, p. 8]
- Once this list of theses is compiled, it is possible and necessary to consider subsets of them ion relation to a wide range of case studies in the history of science
- A strong analogy can be made with the testing of scientific hypotheses
  - A broadly Hypothetico-Deductive method is valid, given that such methods "have been more successful in science than inductive ones" on their reading of the history of science, and they "see no immediate prospect of inductive generalizations emerging from the historical scholarship of the last couple of decades" [Donovan et al., 1992, pp. 12-13][7]
  - Comparative testing of models of scientific change (given that "*all* theory appraisal is comparative") [Donovan et al., 1992, p. 8]

[4] The unity of science presupposition is especially pressing, given discussions last week surrounding Wray and Shech's criticism of PMI.

[5] Here we might find a place to push back; surely the "notorious unreliability" of contemporary scientists' memories and abilities to critically reflect are shared by previous scientists. Again, we have a question of the kind of historical evidence that should be used.

[6] They do note that such a process will necessarily involve a distortion of the views, stripping them of their normative tone and ignoring the accompanying theories of meaning. They ignore at least the first consideration given that they are interested in "how science *in fact* functions." [Donovan et al., 1992, p. 9] We might wonder to what extent ignoring the aims and intended scope of the models has on their investigation. Such concerns were raised by Thomas Nickles [Nickles, 1986, pp. 254, 260] in response to the original *Synthese* article laying out this research program [Laudan et al., 1986].

[7] They additionally defend use of the H-D method in the 1992 introduction [Donovan et al., 1992, pp. xiv-xv], noting that:

1) It's the most convenient way to proceed due to public dimension,
2) It has been used by scientists for centuries,
3) Nuanced discussion was expected to result in modification/refinement rather than acceptance/rejection, and
4) (Against induction) "Theoretical novelty is much rarer than empirical novelty"

Evaluation of Actual Methods: Kinds of Historical Evidence and Their Interpretation

Even if we are convinced by the presuppositions of this program, convinced that we need some kind of program to empirically test various philosophical models of scientific change against the historical record, we may still have reservations about how such a research program should be designed:

- How we define scientific change and the "success" of a given theory or set of guiding assumptions; how, for example, could we (and should we?) separate out the contributions of empirical success, conceptual success, social success, theological success, etc... to the overall success of a theory in a given period of scientific change?[8]
- What kinds of historical evidence should be brought to bear on theses concerning scientific change? Do we restrict ourselves to the published work of historical scientists, or do we also look into various letters, diary entries, notes in the margins of their books, etc?
- To what extent does how we define a "scientific community" affect our ability to test the empirical claims against historical cases?[9]
- Does this research program, given the normative tone of many philosophical models of scientific change, commit the naturalistic fallacy? To what extent should we balance normative claims about science against the historical record of science?[10]
- What do we lose by going into historical cases already having specific theses in mind (even if our goal is merely to test them)?

[8] For example, we might consider Mike's example from last class of Richard Owen's idealist morphology.

[9] This worry is actually raised somewhat by Mauskopf in reference to language- or culture-specific scientific communities [Donovan et al., 1992, p. 28]

[10] Interestingly, the editors consider this criticism in the 1992 introduction, but quickly dispense with it, asserting that it is not actually a fallacy and referring to Larry Laudan's *Science and Values*.

Nickles raised several worries in his 1986 response to the original *Synthese* paper:

1) In using the H-D method, they are not paying sufficient attention to how historians actually use historical evidence [Nickles, 1986, pp. 253-4]
2) "history discloses that really useful (powerful) methodological strategies are nearly always context- and content-dependent" [Nickles, 1986, p. 257]
3) Nickles brings up the *Naturwissenschaften-Geisteswissenschaftem* controvery from the late-19th century, specifically as warning us that "historicism is not necessarily compatible with naturalism in the sense in which philosophers still maintain the latter"[11] [Nick-

[11] Nickles seems to be referring to the fact that Donovan et al seem to be "aiming to disclose the ahistorical principles underlying science"[Nickles, 1986, p. 258]

les, 1986, p. 257]

4) Perhaps most interestingly, Nickles proposes that scientists' own reconstructions/reconceptualizations of past events can be useful, notably playing a possible justificatory role later on and "crystallizing out the relevance of previous work to current problems and techniques" [Nickles, 1986, pp. 262-3]

NICKLES ELSEWHERE VOICES THE WORRY that, not only is seeking generalizations about science the wrong way to go, but that, as long as we continue assume commonality amongst all of the sciences, "historical criticism can actually push methodology to a *more* general, abstract level" [Nickles, 1989, p. 666]

FINALLY, WE MAY WORRY about the actual execution of the program as given in the book:

- It is a serious question whether the choice to focus on case studies exclusively from the physical sciences may have impacted the conclusions reached.[12]
- We might worry that there's only one author addressing each case study; perhaps, in the spirit of Longino's transformative criticism, it would be preferable to have diverse viewpoints on each historical episode [13]

[12] For example, they acknowledge that there was more confusion among studies of the biological sciences as to "units of change" [Donovan et al., 1992, p. 13].

[13] And here are my own gems/lumps of coal:

(gem) The clarification and modification of many of the theses in response to difficulties analyzing case studies in terms of them

(gem) interdisciplinary, collaborative approach to science studies

(coal) seemingly (based on the text) unreflective approach to integrating history and philosophy

References

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