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Samuel Hollander

Sandra J. Peart
University of Richmond, speart@richmond.edu

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A REJOINDER TO ABRAHAM HIRSCH

BY

SAMUEL HOLLANDER AND SANDRA PEART

The dispute between Hollander and Peart, and Hirsch, turns on the nature and role of verification in Mill's perception of the appropriate method for Political Economy. Professor Hirsch maintains against us that, for Mill, the models constructed by political economists are insulated from verification. His case is based on two counterclaims. First, that when Mill writes of "verification" in Book III of the *Logic*, he has in mind a procedure differing from that appropriate for Political Economy, which allows only "indirect verification" (outlined in Book VI). Hirsch finds that Hollander and Peart confuse the two. Secondly, since the contexts of our case studies often relate to policy formulation, Hirsch finds our elucidations of an appeal to experience of a more basic order to be unconvincing.

Not surprisingly in the light of Mill's varied and active interests in policy formation, when Mill engages in specific "verification" exercises, he often does so in the context of policy applications. This does not imply, however, that verification *only* occurs in the context of policy analysis. Our purpose has been to demonstrate: (i) that verification plays a more significant role in Mill's methodological recommendations than has been allowed; and (ii) that he himself engaged in substantive and hitherto unrecognized *theory revision* as a result of such a process. We also remain convinced that Mill takes seriously the challenge of verifying what Professor Hirsch refers to as the more "basic science" and what we chose to call the axiomatic framework of the science.

Let us first establish the essential fact that *both* in Book III *and* in Book VI, Mill insists on the applicability of the deductive method to *all* scientific endeavors entailing multiple cause relationships. Chapter 11 of Book III, entitled "The Deductive Method," makes it clear that the method pertains to physical *and* social, inexact *and* exact, sciences:

> Thus, if the subject be social or historical phenomena, the premises of the Deductive Method must be the laws of the causes which determine that class of phenomena; and those causes are human actions, together with the general outward circumstances under the influence of which mankind are placed, and which constitute man's position on the earth. The Deductive Method, applied to social phenomena, must begin, therefore, by investigating, or must suppose to have been already investigated, the laws of human action, and those

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> properties of outward things by which the actions of human beings in society are determined (1843/1973–74, p. 454).

When Mill turns in Book VI to the Moral Sciences, he reiterates that the methods appropriate in these scientific endeavors had already been elucidated in Book III:

> In substance, whatever can be done in a work like this for the Logic of the Moral Sciences, has been or ought to have been accomplished in the five preceding Books; to which the present can be only a kind of supplement or appendix, since the methods of investigation applicable to moral and social science must have been already described, if I have succeeded in enumerating and characterizing those of science in general (p. 835).

Mill does indeed insist, as Professor Hirsch recognizes, that the process of "Verification" requires modification in Moral and Social Sciences. But what does this imply? Mill bases his case for a specialist science of Political Economy on the *pragmatic* grounds that economic transactions are, for the most part, motivated by the self-interest axiom (p. 901), which circumstance permits some reduction of the complexity of the causal framework. But confidence that the scientist may proceed as a "first approximation" on this basis does *not* constitute a claim that verification of economic theory is unnecessary or impossible. On the contrary, Mill remarks that "Verification by Specific Experience" is an "indispensable element in all deductive sciences" (p. 907). The difficulties of verification to which he now proceeds reflect the problem that rarely, if ever, do we observe repetitions of the exact set of causal relations in operation all at once, thus precluding the derivation of "empirical laws." But there does remain Indirect Verification of economic theory, represented by Mill as an *essential* ingredient of the method of Political Economy. It is wrong to suppose that he accords economic theory a heightened sense of certainty *or* allows only an insubstantial role (or none at all) for its verification. We shall return to this fundamental issue after considering some of the specifics of Hirsch's case. Professor Hirsch writes that he:

> Find[s] little evidence for the argument that "Mill allowed for the alteration of the basic model in consequence of the procedure of verification—testing against specific experience: its 'improvement,' 'correction,' 'completion,' 'extension' " (Hollander 1985, p. 120), giving the impression that Mill took this to be an everyday occurrence that goes on routinely and continuously over time. As I see it, what Mill says about testing hypotheses designed to deal with policy questions Hollander and Peart take to apply to the testing of the basic model of economic science (Hirsch 2000, pp. 355–56).

Now whether "verification" goes on "routinely and continuously over time" is not, of course, the fundamental issue. The fundamental issue is whether Mill allowed *at all* for model improvement resulting from a verification exercise, since it is the major premise of the secondary literature that he did not. By introducing the business of "everyday occurrence" Hirsch leaves himself an escape hatch—testing *might* conceivably generate model improvement but it would be highly unusual. Throughout his section 4—as in his 1992 article—we

encounter references to the difficulty, but not impossibility, of model improvement via verification:

> It is conceivable, as Mill suggests in the *Essay*, that the economist comes to the conclusion that the disturbing causes, particularly if they relate to the economic motive, are important enough to be incorporated into the general model of the science. But to do this intelligently the economist would have to wait for a great deal of additional evidence since unless this deviation is found in many other instances it would be inappropriate to make even a relatively small change (p. 355).
>
> Of course, it is conceivable that the conclusion that the basic model of economic science falls short could result from a verification process after a lot of tests of policy hypotheses had been performed over a long period of time and covering a very large number of different types of application, though the likelihood of this happening seems very small (p. 355).

Hirsch, after all, thus goes a long away in our direction on a matter of principle. Unfortunately, having made the concession he forgets about it entirely: "Mill may have moved more or less from the *a priori* in different places, but for economics he moved only to the extent of allowing for disturbing causes" (p. 358; also p. 357). Model improvement, for Mill, is not conceivable!

As to whether or not Mill envisaged the testing process as an "everyday occurrence," we find it convenient to let him speak—very briefly—for himself regarding the heavy and continuous responsibility of the "speculative thinker" in this regard:

> His knowledge must at least enable him to explain and account for what *is*, or he is an insufficient judge of what ought to be. If a political economist, for instance, finds himself puzzled by any recent or present commercial phenomena; if there is any mystery to him in the late or present state of the productive industry of the country, which his knowledge of principle does not enable him to unriddle; he may be sure that something is wanting to render his system of opinions a safe guide in existing circumstances. Either some of the facts which influence the situation of the country and the course of events are not known to him; or, knowing them, he knows not what ought to be their effects. In the latter case his system is imperfect even as an abstract system; it does not enable him to trace correctly all the consequences even of assumed premises (1836/1967, p. 335).

The task of seeking to account for "residuums" is a permanent obligation to be undertaken "upon every new combination of facts as it arises," and one that might "furnish the occasion for a consequent enlargement or correction of his general views" (pp. 335–36).[1]

The closing sentence of the above indented extract makes it crystal clear that Mill is discussing efforts to "perfect" an "abstract system." And it is the theorist's "general views" that would—as a result of a successful quest to explain a "residuum"—be "enlarged" or "corrected." As Mill puts it in the full extract, the process should not be undertaken "with the desire of finding his

[1] It should be unnecessary to repeat our position that the verification exercise itself might in practice be undertaken by someone other than the theorist, or by the theorist in another capacity.

[abstract] system complete, but of making it so." These are several of the terms—"enlarged," "corrected," "completed"—Hirsch prefers to pass over (above 351). It is wholly unconvincing to claim, as Hirsch claims, that the context applies *only* to "the testing of hypotheses designed to deal with policy questions"; it is certainly *not* "evident that Mill is … talking about the application even where [he] talks about 'theory' and 'abstract systems' " (p. 359, also 353–54, albeit that the object of model improvement is largely—though, we shall see, not entirely—to permit sensible policy applications.

The remaining terms on our list—"improvement" and "extension"—are found in the *System of Logic* in that section on "The Verification of the Social Science" alluded to above, with illustrations drawn specifically from *economics*: the effect … of corn laws, or of a prohibitive commercial system generally" (1843/1973–74, p. 908). The message conveyed is precisely that of the *Essay*, namely the need for ongoing verification, in economics, to assure a sufficiently perfect theory to enable us to assign their consequences even where "the facts which ought to be taken into account" are "completely known to us"—"a sufficiently perfect theory to enable us to assign their consequences" (p. 910). And the moral regarding the treatment of unexplained residuals is also precisely as in the *Essay*:

> To prove (in short) that *our science*, and our knowledge of the particular case, render us competent to predict the future, we must show that they would have enabled us to predict the present and the past. If there be anything which we could not have predicted, this constitutes a residual phenomenon, requiring further study for the purpose of explanation; and we must either search among the circumstances of the particular case until we find one which, on the principles of our existing theory, accounts for the unexplained phenomenon, *or we must turn back, and seek the explanation by an extension and improvement of the theory itself* (emphasis added).

Where in all this is there appeal to "policy?" Mill even affirmed that where theory falls short, we are not "in the present state of our knowledge, fully competent to draw conclusions, *speculative* or practical" (emphasis added). There may be no immediate policy implications at stake at all.

Considering Mill's position regarding theory improvement, his statement with respect to economics that "verification is no part of the business of science at all, but of the application of science,"[2] can only be read in our fashion, namely that verification plays on theory *indirectly* by revealing to the theorist the need to engage in model improvement (1999, p. 381). There is nothing particularly "sophisticated" about this, nor do we "explain [the statement] away" as Hirsch charges (p. 350). It is, we have seen, Mill who uses the term "indirect verification" in the *System of Logic*—and with reference to economics—to describe the process to which we have referred:

> Although, however, direct verification is impossible, there is an indirect

[2] And similarly the statement that "the method *a posteriori*, or that of specific experience," is of "great value in the moral sciences … not as a means of discovering truth but of verifying it …" (1836/1967, p. 331).

> verification, which is scarcely of less value, and which is always practicable. The conclusion drawn as to the individual case, can only be directly verified in that case; but it is verified indirectly, by the verification of other conclusions, drawn in other individual cases from the same laws. The experience which comes too late to verify the particular proposition to which it refers, is not too late to help towards verifying the general sufficiency of the theory. The test of the degree in which the science affords safe ground for predicting (and consequently for practically dealing with) what has not yet happened, is the degree in which it would have enabled us to predict what has actually occurred. Before our theory of the influence of a particular cause, in a given state of circumstances, can be entirely trusted, we must be able to explain and account for the existing state of all that portion of the social phenomena which that cause has a tendency to influence (1843/1973–74, pp. 909–10).

When Hirsch (p. 352 also p. 358) to reinforce his case, cites Mill (p. 908) to the effect that "in those more special inquiries which form the subject of the separate branches of the social science, [the] two-fold logical process and reciprocal verification is not possible," he misses the point. For, as already pointed out, Mill is here ruling out only that sort of verification which turns on the existence of "empirical laws"; but where (as the citation continues) "specific experience affords nothing amounting to empirical laws"—as in economics (*vide* the illustration regarding Corn Laws and Prohibitive Restrictions)—recourse to "indirect verification" is called for. Much of the problem with Hirsch's section 6 entails the apparent identification of Mill's discussion of "reciprocal verification" with that of "indirect verification" applicable in economics. And it is perhaps this conflation of the two categories of verification that leads him to exaggerate the differences between us (see in particular, his closing section).