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Mill's Unrevised Philosophy of Economics: A Comment on Hausman

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Source: *Philosophy of Science*, Vol. 53, No. 1 (Mar., 1986), pp. 89-100

Published by: The University of Chicago Press on behalf of the Philosophy of Science Association

Stable URL: <http://www.jstor.org/stable/187922>

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**DISCUSSION:**  
**MILL'S UNREVISED PHILOSOPHY OF ECONOMICS:**  
**A COMMENT ON HAUSMAN\***

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Hausman has argued that Mill in the *Logic* demands verification of qualified, inexact statements if they are to be considered lawlike. This puts Mill in line with a reasonable interpretation of what modern microeconomists are about, but requires the additional hypothesis that Mill abandoned his earlier stress on modal truth in his 1836 essay on the method of economics. The paper maintains that neither textual nor contextual evidence supports this hypothesis. Moreover, it is superfluous if one attends carefully to how Mill conceived economic *science*, which occupied a peculiar, somewhat isolated place in his own views on the deductive method and on verification.

**I**

Daniel Hausman in his recent article in this journal on Mill's philosophy of economics has argued that whereas Mill in his early essay on method espouses modal truth (truth "in the abstract"), he "implicitly repudiates a modal interpretation . . . in the revisions of his views on the philosophy of economics for the *Logic*," where he "demands empirical verification for inexact laws" (Hausman 1981a, p. 371). Thus, in the *Logic*, Mill writes: "The ground of confidence in any concrete deductive science is not the *a priori* reasoning itself, but the accordance between its results and those of observation *a posteriori*" (Mill, 8: pp. 896–97).

Hausman's main purpose is not to provide textual exegesis but to shed light on the nature of statements in modern economics. He believes these are better understood in terms of inexact laws implicitly qualified by some *ceteris paribus* clause. He therefore finds Mill's discussion of inexact laws in the *Logic* more apt than his earlier discussion of economic laws as modal or counterfactual assertions. Then, too, modal analysis possibly suffers both a metaphysical limitation and a practical disadvantage. It seems to involve a commitment to talking of possible worlds; and it can

\*Received September 1984; revised December 1984.

†I have benefited greatly from correspondence with Dan Hausman. Uskali Maki, Margaret Schabas, Wade Hands, and Roy Weintraub have made useful comments and corrections for which I am grateful. I am indebted also to an anonymous referee for suggestions towards improving the presentation.

*Philosophy of Science*, 53 (1986) pp. 89–100.  
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issue in misplaced confidence in models, “even when one has no grounds to believe that the axioms are true (with qualifications) of anything real” (Hausman 1981a, p. 374). One can avoid these unwanted concomitants without introducing new philosophical problems simply by sticking to the inexact laws view. For, “if one is willing to talk of possible worlds, the qualified [inexact laws] view entails the counterfactual view” (p. 373). Thus, the inexact laws interpretation happily conforms to Hausman’s preferred criteria of being “an interesting and [an] accurate *philosophical* reconstruction of Mill’s remarks” (p. 373, emphasis added).

There are certain clear gains to be had from this approach. It forces one to consider Mill in a new light; in particular, it offers a way of reading his substantive contributions to economics in isolation from his more general philosophical views. Without denying these advantages, it seems appropriate to point out some of the costs of accepting Hausman’s reconstruction. His procedure exaggerates differences of emphasis between Mill’s early essay and the *Logic* into differences of methodological commitment. At the same time it underplays differences within the *Logic* between what Mill says of science in general and what he says about political economy (hereafter economics).

Accepting these distortions as an unavoidable cost has further implications: one is apt to miss asking certain interesting questions to do with the evolution of economics. Whether or not one agrees with Imre Lakatos’s suggestion that reconstructions be presented along with the “real” history, written in footnotes, a strong case exists for having both available. My purpose here is to complement Hausman’s essay by showing something of what may be lost if precedence is given to philosophical concerns over the historical record.

## II

To tackle the distortions first, the contextual evidence seems to point away from a switch in the way Mill viewed economic laws between “On the Definition of Political Economy . . .” (1836) and the *Logic* (1843). This evidence can be summarized quite briefly, although it is convenient to leave it until the end. A second point requires more elaborate development. Put bluntly, it is this. Hausman’s technique of philosophical reconstruction leads him to present a distorted version of Mill’s doctrine of verification as it applies to economics.

The issues here can best be presented by reconstructing Hausman’s own reconstruction of Mill’s ideas. Hausman believes that the propositions used in modern microeconomics comprise general empirical postulates that are not quite true (hence are inexact) and assertions cast as conditional predictions that are also inexact because they involve some more or less vague *ceteris paribus* clause. This description of what one finds

in modern economics matches what Hausman regards as “plausible and sensible” conditions which would justify one in calling any sentence *S* lawlike (p. 373). These conditions are:

- (i) *S* is lawlike.
- (ii) When one removes the vague qualifications in *S*, *S* is in some “natural” class of cases often confirmed and seldom disconfirmed [i.e., naked, *S* is true in important or frequently occurring cases].
- (iii) Scientists have some knowledge of the interfering factors which violate the *ceteris paribus* condition in *S* (p. 372).

According to this view one does not get far by dealing in terms of a theory true on certain supposed conditions. One needs to try out the theory, uncovering what is its natural domain and identifying why things have gone wrong in cases where it does not yield good predictions. In a real sense, one fills in the *ceteris paribus* condition in the process of using the theory.

The important step is to link Mill with this account. Hausman suggests that his conditions (i) through (iii) are merely a reformulation of Mill's views on justifying lawlikeness (pp. 372–73). In addition, he underlines Mill's insistence on verification, which Mill called the “indispensable” third stage of the deductive method (Mill, 8: 3.11.3). Verification, as hinted at above, seems to be a natural, even a necessary complement to qualified, inexact propositions. Hence, Hausman is inclined to see the relevant portions of Mill's *Logic* (notably 3.11.3 and 6.9.1) pointing to verification as essential to the acceptance of a proposition as lawlike.

At this point it is desirable to interpolate a short reminder about Mill's views on method in “On the Definition. . . .” Mill, as is well known, makes a sharp distinction between the science of economics and the application of the science (4: p. 312). It needs to be stressed that Mill's science was of very limited scope and potential. It dealt with one particular class of facts, the production and uses of wealth in so far as these are affected by the (psychological) drive to pursue material advantage. Also, it considered usually just one (composite) cause, the desire to maximize wealth with the least effort and subject to a certain degree of myopia. Occasionally it would incorporate in its laws the operation of “environmental” principles, namely, the urge to procreate and the limited powers of the soil (4: p. 323); but mostly the wealth motive was its sole concern. This, of course, meant that very little in the way of specific predictions or theorems could be expected. Such detailed work belongs to the application of the science, where individual cases can be examined and the broad tendencies supported by the science filled out so as to be made more precise.

It is not correct to regard the tendencies of Mill's economic *science* as

inexact laws. Rather, they are encompassing and wholly accurate as far as they go, but their domain is artificial and limited (4: p. 329). The applications, on the other hand, are always couched in the form of conditional predictions and it is there that the language of qualified, inexact laws applies.

Mill draws the line between Science and Art in such a way that Art or application contains the bulk of what we might be inclined to call science: virtually all specific theorems and predictions belong to Art. Of what use, then, is the science? Such a question reflects our frustration at what looks like a rather silly and unhelpful division. Mill, however, was doing battle against “practical men,” who would dispense with general maxims because specific experience was always special, with Euclid’s *Elements* because no human-made lines, angles, etc., exactly match the postulated constructions of the abstract geometry (4: p. 327). Against such claims it was important for Mill to establish a province for general truths in economics. There was no question whether the abstract truths he included in the science were relevant; for the wealth motive was very real, as everyone could affirm by looking inside themselves or at those around them (4: p. 329).

Notice one other element in Mill’s view. He speaks of verification in the essay as indispensable, just as he did subsequently in the *Logic*; but it is in the sense of “an indispensable supplement to” the method of abstract truth (4: p. 327). Verification, that is to say, belongs to the application not to the science of economics.

Taking at face value Mill’s distinction, then, the counterfactual method has its own province and rationale.<sup>1</sup> Verification belongs to applied economics. Hausman is correct in characterizing much of modern economics as concerned with inexact laws, and is right to emphasize the crucial role of verification in fleshing out vague *ceteris paribus* clauses. Moreover, the inexact-laws approach does seem to capture the main thrust of Mill’s *Logic*, both in respect of the conditions justifying lawlikeness and in the important role Mill assigns verification in his discussion of the deductive method in general. Hausman has to square his reconstruction with Mill’s essay, however, so he suggests that Mill’s early stress on modal truth was subsequently abandoned. I suggest that this interpretation is textually one-sided and historically false, the misformed product of Hausman’s

<sup>1</sup>I have not discussed the rationale, but it is familiar, having to do with the vanity of hoping “that truth [general maxims] can be arrived at, either in Political Economy or in any other department of the social science, while we look at facts in the concrete, clothed in all the complexity with which nature has surrounded them, and endeavour to elicit a general law by a process of induction from a comparison of details” (Mill 1963, 4: p. 329).

method of reading backwards to Mill from (a certain understanding of) modern economics.

### III

In what sense is it fair to say that Hausman's interpretation is textually one-sided? I confine myself here very largely to the *Logic*. Hausman appeals primarily to Book 3, chapter 11 of the *Logic*, in which the three stages of the general deductive method, including verification, are outlined and given equal weight. Book 6, chapter 9, section 1 is also brought in, since there Mill stresses the complementarity of the a priori and the a posteriori for grounding one's confidence "in any concrete deductive science." Economics being classed as a concrete deductive science, this would seem to be conclusive evidence for verification and against the suppositional approach as valid in its own right.

The problem of textual interpretation, however, is more complicated than that. Consider the following:

(1) The very chapter preceding the exposition of the stages of the deductive method (3.10.6 through 8) is a tirade against "attempts . . . to prove economic doctrines by . . . a recital of instances" (Mill, 7: p. 453, note 3.10.8). So Mill's stress on verification can hardly be read as somehow in conflict with his general and longstanding antagonism to specific experience.

(2) The sections of chapter 9 in Book 6 following after the first section—the one cited by Hausman—stress the view that "all the general propositions of the deductive science, are therefore, in the strictest sense of the word, hypothetical" (7: p. 900, 6.9.2; original draft wording). Not only that, but verification's role is restricted quite severely. (a) Where empirical laws can be found—not the case in economics—still "the experimental process is not here to be regarded as a distinct road to the truth . . ." (Mill 8: p. 908, 6.11.5). This is language reminiscent of the essay on method, where Mill says that "the method *a posteriori*, or that of specific experience, is altogether inefficacious . . . as a means of arriving at any considerable body of valuable truth" (4: p. 327). (b) In cases where we cannot derive empirical laws because social experimentation or unlimited observation is not open to us—the case of economics—"the only instance really fitted to verify the predictions of theory is the very individual instance for which the predictions were made; and the verification comes too late to be of any avail for practical guidance" (8: p. 909, 6.9.6; original draft wording). Notice that Mill makes an apparent allowance. If the case turns out to match the prediction it does indirectly verify "the general sufficiency of the theory" (p. 909). But this "general sufficiency" relates not to whether the statements of the theory qualify as lawlike, but only to whether it "affords safe ground for predicting (and

consequently for practically dealing with) what has not yet happened” (pp. 909–10).

(3) This bears on a third element. The ground of confidence in theory to which Mill here and elsewhere refers obviously has to do with whether general tendencies can safely be *applied*. Hausman, recall, wants to use it as support for the notion that verification is essential if propositions are to be considered lawlike. This use is legitimate if Mill is talking about using verification to flesh out theory by enlarging our understanding of the vague *ceteris paribus* clause attached to inexact laws. Whatever Mill’s intention in Book 3, by the time he comes to discuss economics and “political ethology,” in Book 6, chapter 9, most decidedly he is not talking about verification to that particular end. He is talking about checking predictions in order to gauge how far they are off; that is, to identify how close truth in the abstract is to truth in the concrete. The check may show that theory needs to be extended or improved (Mill 8: p. 910, 6.9.6); but there is no question of its being less lawlike for having been shown to be incomplete in ways or in a degree that we were not fully aware of at the start. As Mill says of the neglect of “ethological” elements in separate social sciences such as economics: “The omission is no defect in them as abstract or hypothetical sciences, but it vitiates them in their practical application . . .” (8: pp. 905–6, 6.9.4).

(4) Finally, if we ask why Hausman conflates verification which fleshes out inexact laws with checks which merely show the degree of incompleteness of truth in the abstract, a plausible suggestion is that he has simply been misled by his desire to interpret Mill’s discussion of economics as a discussion about inexact laws. This causes him to underplay, even though he is aware of, the fact that Mill’s conception of economics in the *Logic* is extremely narrow, just as it was in the earlier essay. Economics is not co-extensive with social science; and certainly it forms but a small part of the investigations that Mill says are characterized by the deductive method in general. It is one branch of the study of social phenomena and along with what he calls “political ethology,” or the laws of collective (national) character (8: pp. 904–5, 6.9.4), it is the only one to which the method of hypothetical truth applies. In both these branches we are concerned with just one class of social facts which, in turn, are dependent on distinct causes. Hence it is possible and convenient to carve out a preliminary science based on these causes alone (p. 905). It is this which gives to each its hypothetical character. History, sociology, psychology, politics—all are different in this respect from economics and ethology. On closer examination, it emerges that of these two it is only economics that is well developed. Of ethology Mill writes: “Of all the subordinate branches of the social science, this is the most completely in its infancy” (p. 905). Given the very special position occupied by eco-

nomics, it should not be difficult to accept that Mill's views on lawlikeness and on the role of verification in general might have to be understood as distinct from what he had to say about that one discipline. Hausman may be right in this interpretation of Mill's general views but he is wrong in assuming that these views apply equally to Mill's economics.

#### IV

This raises a problem that we shall notice briefly. Grant that Mill accorded economics a special and limited place even in the field of social inquiry. Grant, too, that Hausman is substantially correct in characterizing the propositions of modern microeconomics as qualified, inexact laws. Are we not forced to conclude that Mill's economics and modern economics are different entities? I accept that Mill may have viewed economics rather differently than modern economists. This is more a problem for Hausman than for me, since it is he who wants to forge a connection. For me the problem is to explain (a) what, then, happened to Mill's peculiar (modal) economics; and (b) how it is that Mill's general influence as a methodologist seems to have outlived his particular insistence on the modal interpretation of economic laws.

I offer here only some fairly conjectural remarks on these questions. In terms of substance, Mill's economics simply rapidly reached its natural limits; and that happened for the reason that you can't say much in detail if your explanations have to run in terms of the direct influence of the wealth motive.<sup>2</sup> More circumspectly put, one *can* supply detailed answers involving long chains of reasoning, provided mathematical techniques are used to set up elaborate models involving precisely specified initial conditions. These interpose between the remote cause and the particular results. Mill was in command of virtually no mathematical techniques, hence it is not surprising to find him suggesting that value theory, for example, had been fully explored by the mid-nineteenth century. Part of the expansion of economics as a discipline in the last half century has been made possible by mathematical models. The first hint of the power of mathematical tools came with the so-called Marginal Revolution. In the face of the differential calculus, Mill's economics yielded surprisingly quickly. A way in which economics *has* flourished while clinging to fairly

<sup>2</sup>Notice, for example, that Mill seems to regard the laws of distribution as so general that they are applicable in any specific circumstances—with proper institutional allowances (Mill 1963, 8: pp. 903–4, 6.9.3). Indeed, all that one can say on the basis of the wealth motive, he admits, is what *kind* and general *direction* of effects might be expected (8: pp. 908–9). This became known in the 1940s and 1950s among economists as qualitative analysis, and it turned out to be pretty empty except for very simple cases such as competitive and monopolistic (two-variable) market structures. See Archibald (1961, 1964) and Klant's criticism of Samuelson (Klant 1984, chap. 4, sec. 7, 9).



direct explanation in terms of the wealth motive has been by moving into other disciplines, exemplified in the economic analysis of politics, marriage, and the family. But this so-called economic imperialism has been strictly a lateral movement.

None of this is directly relevant to the fate of Mill's economics as modal truth. That lived on for many years in the form of taxonomic analysis of possible cases, conditioned somewhat by a demand for realism.<sup>3</sup> However, it too had largely succumbed by the 1940s, swamped by demands for still greater relevance (Clapham's empty boxes; the need to explain the Great Depression), and the rising tide of operationalism and econometrics. It has survived into modern economics only in very attenuated form,<sup>4</sup> as the practice of dealing in assumptions that are plausible: close enough to the actual world being studied to guarantee the applicability of a model version of it. This, however, is part of the economics profession's general preference for dealing in *true* assumptions and *true* arguments and is really quite distinct from counterfactual (modal) analysis of the sort Mill pursued. Counterfactuals can be characterized as involving unalterably false antecedents, even if these are specified as Mill defined economic man, to be fairly close to reality.

As to the second question, I conjecture that Mill's influence as methodologist has been more persistent but, paradoxically, *not* because of his conviction that economics is a science of truth in the abstract. Rather, he has continued to influence economic methodologists and economists because he was an early user of what we now know as the deductive-nomological model of explanation,<sup>5</sup> replete with the requirements that the general ("covering") laws be testable and true (Mill, 8: pp. 671–76). (In Mill's case, the wealth motive was said to be open to direct inspection.) Despite the criticisms to which this model has been subjected by philosophers, economists have continued to accept it, at least in its Popperian dress (Blaug 1980, chap. 1). Whereas operationalism helped to kill the practice among economists of analyzing and classifying possible worlds, it gave a fillip to the deductive-nomological model of explanation. Economists produced explanatory hypotheses out of exact, deductive struc-

<sup>3</sup>An excellent example is Oscar Lange's 1944.

<sup>4</sup>This is not true in one very important respect. General-equilibrium analysis has something of the character of Mill's science of economics in the sense of being counterfactual analysis. Significantly, even Hausman agrees that general equilibrium does not fit his inexact-laws characterization (Hausman 1981b). But modern general-equilibrium theorists have not stressed as consistently as a Mill might the modal-truth interpretation of their own work. On the contrary, many simply emphasize the importance of general-equilibrium analysis in shaping correctly more concrete studies (see Hahn 1973; Weintraub 1984). This is a role of theory Mill stressed in the *Logic* more explicitly than in the *Essay* (8: pp. 899–900, 6.9.2).

<sup>5</sup>Popper (1976) draws attention to this (p. 117). He refers to Mill, 8: p. 464, 3.12.1.

tures. Many among the early econometricians tended to search for these "true" relationships in the data and were wont to explain regression residuals as being due to omitted variables.<sup>6</sup> This is exactly how an *applied* Millian economist would go about things. It is the analogue of Hausman's inexact-laws approach. Note, however, that it has little but the use of models in common with the modal-truth approach. As indirect evidence of a lack of interest in modal truth, it is striking that Mill's essay on method is reprinted infrequently in collections of classics of economic methodology whereas, say, extracts from J. N. Keynes or Lionel Robbins, both of whom stressed the a priori deductive, but not the strictly suppositional character of economics, are not uncommon.<sup>7</sup> Mill, it seems, has survived, but in ways that he himself might well find not wholly appropriate.

This digression illustrates the desirability of combining historical and philosophical inquiry. Philosophical reconstructions, of course, have their place; but it is well to guard against distortion by making explicit the implied historical interpretation behind the reconstruction.

## V

We have two bits of unfinished business, one of a textual and another of a contextual sort.

Whatever one's disposition towards the argument to this point, one substantial obstacle to accepting Hausman's reconstruction remains. Mill, in Book 6 of the *Logic*, restates his views on the nature of economics more or less *without change from the earlier essay*, even quoting extensively from his own text (8: pp. 901–3). He includes as well the earlier caveats about verifying theory.<sup>8</sup> Unless one wishes to maintain that the *Logic* is an inconsistent patchwork, the fact of this largely unrewritten restatement makes it unlikely that Mill's views elsewhere in the *Logic* represent a switch. Nor can Mill have meant to undermine his modal truth view by stressing the need to check theory, since in the *Logic* this forms part of one extended paragraph in which he outlines his position. It is simpler and more accurate to say that Mill's views on economic *science* remained unchanged but that he regarded economic science as a very special part of social inquiry, with features that also gave an almost unique twist to the way in which the deductive method was deployed within its special domain.

<sup>6</sup>This is made very clear in Mary Morgan's 1984.

<sup>7</sup>Ironically, the exception to this is the recent collection edited by Hausman (Hausman 1984).

<sup>8</sup>Actually, he *omits* some of the stronger language in favor of verification in the essay. See Mill 1963, 4: pp. 331–23.

Turning, finally, to the contextual evidence which, as I have said, seems to point away from a switch of position, I offer the following considerations. Mill hoped that his essay on method would “become classical and of authority” (12: p. 211). It was written in 1830 or 1831, revised in 1833, and first published in 1836. It was republished, with no major changes, in 1844, in the collection *Essays on Some Unsettled Questions of Political Economy*. Book 3 of the *Logic* was in draft by mid-1838, and Book 6 by 1839 or 1840 (Mill, 7, textual introduction by John Robson, p. xcvi and passim). It was published in 1843. There was sufficient, though not much, time for a significant change of view to be incorporated in the *Logic*. But we lack a cause which might have provoked such a major change. Moreover, it is implausible that Mill would have allowed an essay to which he had devoted so much care and for which he entertained such high expectations to be republished just a year after one of its central emphases had been so seriously undermined—if that is what we are to believe—in the *Logic*.

To this may be added that there can be no doubt that Mill believed the underlying laws shaping economic investigations (the desire for wealth, the procreative urge, diminishing returns) were both known and true. That being so, there would have been little need felt on his part to retreat from hypothetical analysis into “an unmysterious reliability” (achieved by verifying inexact laws) as “a good tonic” for taking abstract truth too seriously.<sup>9</sup> Not only that but, as I have stressed, Mill’s *bête noire* in the essay on method was not the over-confident modal reasoner but his opposite, the “mere empiric,” or the man who would make generalizations on the basis of specific experience.<sup>10</sup>

Hausman’s purpose, recall, was to show how the inexact-laws interpretation can shed light on “current microeconomics and general equilibrium models and the strategy of neoclassical theorizing” (Hausman 1981a, pp. 381–82). It was not to provide a detailed reading of the texts nor, presumably, to stress the historical context of Mill’s writing. My dissenting remarks stem from a conviction that this is a dangerous separation of tasks, involving certain costs which it is well to identify.

Hausman reads Mill on economics in the light of a set of conditions justifying lawlikeness and an interpretation of modern economics that matches these conditions. This is an exercise in (largely) philosophical reconstruction. It is driven by the view that Mill believed in similar con-

<sup>9</sup>Hausman does not suggest openly that Mill’s seeing the need for such a tonic might help account for his supposed revision; but he stops only just short of saying that this would have been an appropriate adjustment on Mill’s part.

<sup>10</sup>This is plain from the essay on method itself but is also clear from the sequence of still earlier essays written in the preceding decade or so. See Mill 1963, 4: especially pp. 19, 78–79, 324–25.

ditions justifying lawlikeness, and the inference that he *must* therefore have held an interpretation of economics akin to that which Hausman believes is expressed in modern economics. This inference leads him to gloss over the sharp methodological distinction between economics and other parts of social science that Mill espouses in the *Logic*; and it induces him to suggest that Mill must have abandoned the views on economic science contained in his early essay, "On the Definition of Political Economy. . . ." My reading is that Hausman's inference is gratuitous and has unfortunate consequences. Glossing over the peculiarities ascribed to economics in the *Logic* requires a one-sided reading of the texts; while the associated suggestion that Mill switched views between 1836 and 1843 seems to be without historical warrant.

In the process of generating these distortions, certain intriguing historical issues are sloughed off. On my reading, Mill held and continued to hold a modal-truth view of economics, but it was gradually abandoned by the emerging economics profession as a whole. Why? Again, Mill seems to have retained a considerable reputation even among economic methodologists, but for reasons not linked to his espousal of modal truth. This process, too, is worth investigating. Hausman has done us a very useful service in restating Mill's position on verification and inexact laws. His contribution would have been that much richer, and considerably less misleading, had he been more sensitive to the historical implications of his reconstruction.

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