

---

John Stuart Mill's Philosophy of Economics

Author(s): Daniel M. Hausman

Source: *Philosophy of Science*, Sep., 1981, Vol. 48, No. 3 (Sep., 1981), pp. 363-385

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

Stable URL: <https://www.jstor.org/stable/186985>

---

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <https://about.jstor.org/terms>



The University of Chicago Press and Philosophy of Science Association are collaborating with JSTOR to digitize, preserve and extend access to *Philosophy of Science*

JSTOR

# JOHN STUART MILL'S PHILOSOPHY OF ECONOMICS\*

DANIEL M. HAUSMAN†

*Department of Philosophy  
University of Maryland*

John Stuart Mill regards economics as an inexact and separate science which employs a deductive method. This paper analyzes and restates Mill's views and considers whether they help one to understand philosophical peculiarities of contemporary microeconomic theory. The author concludes that it is philosophically enlightening to interpret microeconomics as an inexact and separate science, but that Mill's notion of a deductive method has only a little to contribute.

John Stuart Mill's reflections on the nature of economic theory and on the manner in which it is to be justified have not received the attention they deserve. Although Mill's views are problematic, they have much to contribute to current thinking about the methodology of economics. Mill offers philosophical interpretations of the nature and justification of economic theory in three main places: in Book VI of *A System of Logic*, in his earlier essay, "On the Definition of Political Economy and the Method of Investigation Proper to It", and, less explicitly, in scattered passages of his *Principles of Political Economy* and of his other essays on economics. The relationship of the actual economic theorizing Mill did in the *Principles* to his philosophical construal of the nature of economic theory is complicated and will not be discussed in this essay (See Keynes 1890, pp. 19–20). Mill's views on the nature of economics differ somewhat among and within these sources. These differences will be discussed briefly where relevant. My goal is not, however, to give a detailed reading of the texts. I hope rather to provide an interesting and accurate philosophical reconstruction of Mill's remarks—to translate and interpret his views that they may be of use in current discussions.

Mill's view, in a nutshell, is that economics is an inexact and separate

\*Received November 1979; Revised November 1980.

†I have benefited greatly from discussing the issues considered in this paper with Margaret Atherton, Lindley Darden, Philip Ehrlich, Isaac Levi, Sidney Morgenbesser, Robert Schwartz and Frederick Suppe. Conrad Johnson, Catherine Kautsky, Alexander Rosenberg, Dudley Shapere, Allen Stairs, and Paul Thagard read earlier drafts and offered helpful advice and criticism. Anonymous reviewers for this journal offered some useful criticism. The support of the National Science Foundation (Grant #SES 8007385) is gratefully acknowledged.

*Philosophy of Science*, 48 (1981) pp. 363–385  
Copyright © 1981 by the Philosophy of Science Association.

science that employs a deductive method. Spelling out what this claim means brings one a long way in understanding the peculiarities of current microeconomics and equilibrium theories. In the claim are three different assertions—that economics is an inexact science, that it is a separate science, and that it employs a deductive method. In the following three sections I analyze these claims. In section 4 I suggest that these claims are largely correct and insightful as interpretations of current equilibrium theories and that they have much to contribute to current debates about the justification of economic theory.

**1. Economics as an Inexact Science.** After discussing difficulties concerning social science to which free will supposedly gives rise, Mill begins the argument of Book VI of his *Logic* by distinguishing between exact and inexact sciences. In an inexact science

the only laws as yet accurately ascertained are those of the causes which affect the phenomenon in all cases, and in considerable degree; while others which affect it in some cases only, or, if in all, only in a slight degree, have not been sufficiently ascertained and studied to enable us to lay down their laws, still less to deduce the completed law of the phenomenon, by compounding the effects of the greater with those of the minor causes. (6.3.1)<sup>1</sup>

The example Mill gives is the science of tides. Scientists know the laws of the greater causes, the gravitational pull of the sun and the moon, but are ignorant of the laws of the minor causes like the configuration of the shore and ocean bottom. One might suggest that there are no exact sciences,<sup>2</sup> although in some cases—for some purposes—the inexactness of a science may be negligible. Mill, however, believes that astronomy is an exact science,

because its phenomena have been brought under laws comprehending the whole of the causes by which the phenomena are influenced, whether in a great or only a trifling degree, whether in all or only in some cases, and assigning to each of those causes the share of the effect which really belongs to it. (6.3.1)

The model Mill has in mind when he speaks in the first quotation above of “compounding the effects” of causes is the vector addition of forces in mechanics. Mill draws an analogy between motives and forces and

<sup>1</sup>References in the form “(6.3.1)” will be to Mill’s *Logic*. Read “6.3.1” for example, “Book VI, Chapter III, Section 1.”

<sup>2</sup>As Allen Stairs pointed out to me, fundamental theories like Newton’s theory of motion or quantum mechanics raise different questions to which this paper may not supply any answers. Given Mill’s definition, I do not believe that there could be any exact sciences.

exploits it extensively. What are we to make of this notion of “compounding” the effects of causes (Cartwright 1980, pp. 78–80)? The answer, baldly stated (it will be discussed further in section 3), is that scientists so formulate laws that they are able to combine them in a theory from which precise consequences may be deduced.

Economics is not, however, an exact science. To provide a philosophical interpretation of economics, we need sensibly to construe the appealing idea that one may discover laws that state only how the “greater causes” operate. Possessing only such laws, scientists cannot infer invariably and precisely what actually occurs. There will be various “disturbing causes” (1836, pp. 330–32). The lawlike statements in an inexact science are thus themselves inexact or incomplete. Claims like “The preferences of consumers are transitive” are certainly (at best) inexact or incomplete. How are we to analyze them? How are we to make more precise the idea that scientists only know how some of the more important causes operate? How can we defend such knowledge claims?

When Mill talks about “an inexact science”, he is not concerned with every science from which only inaccurate or imprecise implications may be drawn. It may happen that knowledge of the relevant causes is complete, yet one remains unable to make accurate predictions or to explain in detail because of difficulties in specifying the initial conditions or because of limitations in human mathematical powers. Despite the fact that the science of tides may, in fact, be a better example of inexactness due to such difficulties in specifying initial conditions or calculating their effects, Mill is concerned with inexactness *within laws*. I shall only be discussing sciences which are inexact because their laws are in some way not fully adequate.

I can think of five ways of analyzing the lawlike statements of inexact sciences. Most of these have some support in Mill's text. They are (i) that the generalizations in inexact sciences are approximate; (ii) that the generalizations are probabilistic or statistical; (iii) that the generalizations are “rough”; (iv) that the generalizations make modal or counterfactual assertions; and (v) that the generalizations of inexact sciences are qualified with implicit *ceteris paribus* clauses. The last interpretation is, I shall argue, most faithful to Mill's pronouncements in *A System of Logic*. The fourth interpretation better represents his views in “On the Definition of Political Economy. . .”. Mill's later view of laws in inexact sciences, as implicitly qualified with *ceteris paribus* clauses, seems to me a promising interpretation of current economic theory.

(i) *Approximate Generalizations*. The first interpretation of inexactness as involving *approximation* is quite simple. Sometimes lawlike claims are made which are not true as stated, but which can be made true merely by specifying a margin of error in a certain domain. Kepler's

Laws are in this sense approximate. Within a certain percentage of the calculated angular velocities or periods of revolution, these laws appear to be *true*.<sup>3</sup> By “smearing” what the laws assert, we achieve literally true propositions.

Mill, however, never interprets the laws of inexact sciences as true within a margin of error. Moreover, in fact only a small part of the inexactness of economic generalizations is a matter of approximation. Suppose in a recession it maximizes profit for several companies each to lay off 1000 workers. If no company laid off fewer than 800 or more than 1200, then the evidence would confirm (within a margin of error of 20%) that firms attempt to maximize profits. Economic behavior is, however, more complicated. One can reduce the disconfirmations of economic generalizations by specifying a margin of error, but the bulk of the inexactness remains. Some firms feel responsible for their employees and might refuse to lay off any of them.

(ii) *Probabilistic or Statistical Generalizations*. Many economists have regarded the basic general statements of economics as probabilistic or statistical laws (see, for example, Hicks 1946, p.11). There is, however, little support in Mill’s writing for this construal and several considerations count against it. To regard economic laws (and indeed all empirical laws) as probabilistic, as Peter McClelland does (1975, ch. 1), is to confuse the results of testing with what laws assert. The fundamental general statements of economics do not appear to involve elements of chance or randomness. They are not stated statistically. They say nothing about the statistical distributions of properties. These generalizations merely appear to have counterexamples. To construe all generalizations that face counterexamples as probabilistic is merely to rechristen them. It may be that, unable to account for the inexactness of the basic laws of economics in any other way, one will eventually conclude that they are inadequately stated and understood statistical laws. Before coming to this negative conclusion one should, however, consider the other options.

(iii) *Rough Generalizations*. Mill suggests on several occasions that some of the laws of inexact sciences are rough generalizations, reasonably, but not perfectly reliable (Compare Rescher 1970, pp. 164–67). As I am using the term, a rough generalization is not a law, because it is not true. It is simply a generalization that faces some counterexamples, but not so many that one learns nothing from it. Mill believes that many

<sup>3</sup>There are also implicit *ceteris paribus* clauses involved. If a comet collided with Venus and changed its orbit, scientists would not regard Kepler’s Laws as falsified.

of the “*empirical laws*” of the social sciences are merely rough generalizations. Indeed, he suggests that this is all we can hope for:

All propositions which can be framed respecting the actions of human beings as ordinarily classified, or as classified according to any kind of outward indications, are merely approximate. We can only say, Most persons of a particular age, profession, country, or rank in society have such and such qualities; . . . (3.23.3)

Since rough generalizations are false, their explanatory power is dubious. Interpreting the laws of inexact sciences as merely for-the-most-part true leads one to question their adequacy. Mill recognizes this and should, I think, be interpreted as asserting *only* that the *empirical* laws of inexact sciences—those generalizations of inexact sciences which are in need of explanation—are rough generalizations. Mill believes, I think, that even in inexact sciences we can find causal laws, which are not thus “rough” (although informing us of “tendencies” only) to explain our (rough) empirical laws. Yet his claims are ambiguous:

This science of Ethology may be called the Exact Science of Human Nature; for its truths are not, like the empirical laws which depend on them, approximate generalizations but real laws. It is, however, (as in all cases of complex phenomena,) necessary to the exactness of the propositions that they should be hypothetical only, and affirm tendencies, not facts. (6.5.4)

This quotation apparently supports the view that in inexact sciences we have only rough generalizations. Yet Mill cannot believe that ethology (the science of formation of character) is an exact science. It is, as he concedes (6.5.6), hardly as yet a science at all. He would not maintain that social scientists know that laws of the minor as well as the greater causes and can compound them correctly. Nor can he mean that the truths of ethology can only be “real laws” when ethology becomes an exact science, since he goes on in the same passage to write:

It is a scientific proposition that bodily strength tends to make men courageous; not that it always makes them so: that an interest on one side of a question tends to bias the judgment; not that it invariably does so: that experience tends to give wisdom; not that such is always its effect. These propositions, being assertive only of tendencies, *are not the less universally true* because the tendencies may be frustrated. (6.5.4)[my emphasis]

What Mill means, I think, although he expresses himself badly, is that in inexact sciences one can come up with causal laws which “being assertive only of tendencies are not the less universally true”. Inexact sci-

ences need not consist of rough generalizations only. “Tendencies” are the genuine regularities which inexact laws express. The science of tides is an inexact science, although neither the law of gravitation nor the fundamental laws of tidology which are derived from it are rough. Mill writes:

. . . there is no reason that it [the science of human nature] should not be as much a science as Tidology is. . . .

. . . But in order to give a genuinely scientific character to the study, it is indispensable that these approximate generalisations, which in themselves would amount only to the lowest kind of empirical laws, should be connected deductively with the other laws of nature from which they result. . . . In other words, the science of Human Nature may be said to exist in proportion as the approximate truths which compose a practical knowledge of mankind can be exhibited as corollaries from the universal laws of human nature on which they rest, whereby the proper limits of those approximate truths would be shown, and we should be enabled to deduce others for any new state of circumstances, in anticipation of specific experience. (6.3.2)

Mill begins here writing just what I take him to mean. The “science of Human Nature” is at present inexact and is likely to remain inexact for a long while. It is nevertheless a science insofar as its rough empirical laws can be connected deductively to genuine universal laws of human nature. Yet near the end he suggests that connecting the rough empirical laws of human nature with the genuine underlying laws would enable scientists “to deduce others for any new state of circumstances, in anticipation of specific experience”. Notice, however, that if scientists can do this precisely and correctly, they have an exact, not an inexact science. Can Mill mean this? Surely one can show that certain rough generalizations are corollaries of “the universal laws of human nature” within an *inexact* science. Although much in Mill’s text suggests that he takes the laws of inexact science to be rough generalizations, I doubt that he regards *the fundamental explanatory laws* of such sciences as merely such rough generalizations. The possibility of developing inexact sciences deductively, which Mill stresses, demands that the fundamental laws be more than rough generalizations. Mill does not locate the inexactness of sciences in the roughness of their laws.

(iv) *Model or Counterfactual Laws*. Like many others (Schumpeter 1954, pp. 1049–50n; Gibbard & Varian 1978, esp. pp. 673f), Mill sometimes explains the inexactness of economic laws by arguing that these “laws” state how things *would be were* certain conditions met. They do

not describe actual regularities. When one attempts to use them to discover how things are, one is sometimes led astray. When the hypothetical conditions implicit in the laws are not met, things are not necessarily the way the laws say they would be. This counterfactual view of the inexact “laws” of economics is pronounced in Mill’s, “On the Definition of Political Economy. . . .” He writes there:

The conclusions of Political Economy, consequently, like those of geometry, are only true as the common phrase is, *in the abstract*; that is, they are only true under certain suppositions in which none but general causes—causes common to the *whole class* of cases under consideration—are taken into the account.

. . . They would be true without qualification, only in a case which is purely imaginary. In proportion as the actual facts recede from the hypothesis, he [the economist] must allow a corresponding deviation from the strict letter of his conclusion; otherwise it will be true only of things such as he has arbitrarily supposed, not of such things as really exist. That which is true in the abstract, is always true in the concrete with proper *allowances*. When a certain cause really exists, and if left to itself would infallibly produce a certain effect, that same effect, *modified* by all the other concurrent causes, will correctly correspond to the result really produced. (1836, pp. 326–7)

What are we to make of the claim (which Mill does not repeat in the *Logic*) that the conclusions of political economy are true “in the abstract”? What does he mean by claiming that the conclusions of the political economist will without qualification “be true only of things such as he has arbitrarily supposed, not of such things as really exist”? Mill can, I think, be read as suggesting that economic theories are “modal models”—that they are accounts of the relationships obtaining between certain possible entities in possible circumstances<sup>4</sup> (See Suppe 1974, 1976). They are thus not true, without allowances, of anything (real).

This position requires clarification. First, it should be carefully distinguished from the claim that economics contains *idealizations*. The proposition that  $L^*$  is ideal or contains idealizations is logically independent of the proposition that  $L^*$  is a modal or counterfactual claim. Roughly following Shapere (1969; see for example pp. 140–41), I regard an entity or property as ideal which scientists find useful to mention in stating or developing their theories, even though the existence of such entities or properties is conclusively ruled out by accepted knowledge. Ideal claims

<sup>4</sup>I am construing the modal interpretation realistically. This paper is written from a realist perspective. Translating my points into the language of a non-cognitive instrumentalist (see Morgenbesser 1969, p. 202) would change the discussion of the modal view considerably.



mention ideal entities or properties and are known to be false. Economics contains some ideal claims (*pace* Rosenberg 1976, p. 133). The assertion made in various general equilibrium theories that agents have perfect knowledge of the future is one example. This assertion may not conflict with any well-established scientific theory, but scientific theories are not, of course, the only source of knowledge. On the other hand, none of the purported *laws* of economics are ideal. Perhaps nobody's preferences are transitive, but that would be just chance. Given current knowledge, transitive preferences are perfectly possible.

Talk of idealizations thus does not contribute to understanding purported *laws* in economics. Indeed, even if some purported laws of economics were ideal, this fact would help little in understanding and rationalizing their inexactness. Whether a theory contains idealizations is a relatively superficial fact about it. When one finds that a theory contains idealizations, as many do, one has merely uncovered a problem requiring further analysis. Ideal statements are false. It is not clear how a theory which contains such false statements can still have explanatory power.

The modal interpretation of purported (inexact) laws is one attempt to explain how statements which are literally false (and in the case of idealizations necessarily so) can nevertheless be regarded as laws. In deriving the ideal gas law, physicists talk about volumeless point particles, which they know do not exist. This derivation thus contains an idealization. The interesting question is what to make of it. The modal view asserts that the derivation shows how a collection of such non-existent point particles would behave. According to the modal view, the antecedent of the ideal gas law is not satisfied by any real gas.

It is also crucial to distinguish the substantial thesis that economic theories make modal claims from the obvious fact that economists work with *models* (See Suppes 1957, p. 254). Economists often take a set of axioms as defining a kind of system and investigate the logical implications of those axioms without considering whether the axioms are true (Compare Giere 1979, ch. 5, esp. pp. 80–81). In his *Essays on Some Unsettled Questions of Political Economy* and again in the *Principles of Political Economy*, for example, Mill discusses the basic principles of international trade in the following way. First he employs a model of barter with no carrying costs between two nations (England and Germany) involving only two commodities (cloth and linen) (1844, pp. 232–61; 1848, pp. 595–617). These principles are then slightly modified by a consideration of some of the complexities initially assumed away. One can see what must be the case in the simple model and, from this, one can apparently understand what must, in essence, at least, be the case in messy actual economies. It might appear that economists are constantly developing modal models. But, in fact, there is no reason to infer from economist's

incessant use of models like Mill's that they regard the axioms of such models as true of some possible economy. Perhaps they do, but they need not. In such models economists investigate the logical consequences of certain axioms or assumptions. If any way can be found to regard the axioms as inexactly true in real circumstances, the models can then be used to make inferences about those circumstances. The mere use of models does not imply that one is making modal claims.

Much of Mill's discussion is compatible with attributing to him the view that the laws of inexact sciences are modal claims. Consider the passage quoted at the beginning of this subsection or the way Mill writes in 6.9.2. R. P. Anschutz believes that this modal interpretation of inexact laws is Mill's settled view (1953, pp. 85–96, 118, 167). Yet, since Mill strongly endorses a Humean view of causation, he would be unhappy with an interpretation committing him to some sort of modal connective. Furthermore, Mill implicitly repudiates a modal interpretation of economic laws in the revisions of his views on the philosophy of economics for the *Logic*. In "On the Definition of Political Economy. . ." Mill is, as we have seen, willing to speak of "truth" in the abstract. In the *Logic*, Mill demands empirical verification for inexact laws.

(v) *Implicitly Qualified Laws*. On the last interpretation, the "laws" of inexact sciences carry with them implicit *ceteris paribus* clauses. This interpretation seems to me most (but not perfectly) consistent with Mill's general philosophical position and with what he writes about inexact sciences (see Keynes 1890, pp. 217–21). It is also sensible and persuasive. Mill does not himself mention *ceteris paribus* clauses. The only sort of provisos or implicit qualifications he discusses are precisely specified exceptions or limits which, when added to approximate claims, enable one to treat them as exact (3.23.7). The range of different sorts of qualifications one might find appended to scientific claims makes the mere assertion that the laws of inexact sciences are implicitly qualified rather unhelpful. Much more needs to be said about these *ceteris paribus* qualifications.

In fact even exact sciences might contain *ceteris paribus* clauses, were they fully eliminable in favor of precise qualification. The *ceteris paribus* clauses which render laws inexact are, however, imprecise and ineliminable. Is it sensible to regard statements so vaguely qualified as laws (see Hutchison 1938, pp. 40f)? It is certainly not the case that, *ceteris paribus*, we are all immortal or that ravens are pink. Not all appeals to *ceteris paribus* qualifications to explain away apparent disconfirmations are legitimate. One who regards the laws of inexact sciences as vaguely qualified claims must distinguish legitimate from illegitimate uses of ineliminable *ceteris paribus* clauses. When, if ever, can sentences with

ineliminable *ceteris paribus* clauses be true? When is one justified in regarding them as laws?

Mill has little to say about truth conditions for sentences containing ineliminable *ceteris paribus* clauses. Such sentences state what happens when certain not-fully specified conditions are satisfied. We can capture this aspect formally most simply by regarding the *ceteris paribus* clause as picking out different predicates in different contexts. When a sentence with the form, “*Ceteris paribus*,  $(x) (Fx \rightarrow Gx)$ ” is truly a law, it expresses an assertion with the form  $(x) (Fx \& Cx \rightarrow Gx)$ , where “*C*” is a vague predicate picked out by the *ceteris paribus* clause in the given context.<sup>5</sup> In sketching this view I am going beyond anything that Mill ever worked out, although the following comments seem to point in this direction:

They [laws affirming tendencies only] must not assert that something will always or certainly happen, but only that such and such will be the effect of a given cause, so far as it operates uncounteracted. (6.5.4)

Implicitly qualified laws assert what will happen when the predicate the *ceteris paribus* clause picks out is not unsatisfied and the law thus not counteracted.

Mill provides a more substantial discussion of the conditions qualified sentences should satisfy before one is justified in regarding them as laws. The question, “When is one justified in regarding qualified (or, alternatively, counterfactual) assertions as laws?” is for many economists and philosophers the central philosophical question concerning economics. It is around this question that methodological controversy concerning economics has turned. Mill suggests that a sentence (S) can justifiably be regarded as a law only when the following three conditions are satisfied:

- (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.
- (iii) Scientists have some knowledge of the interfering factors which violate the *ceteris paribus* condition in S.

The restriction of *ceteris paribus* clauses to lawlike statements is suggested in Mill’s concern with the operation of causes. Philosophers have found lawlikeness a difficult property to analyze (see for example Hempel and Oppenheim 1948, §6, Hempel 1965, pp. 338-47). I know of no good

<sup>5</sup>I have benefited a great deal in this discussion from Levi and Morgenbesser (1964) and from conversations with the authors. They do not fully agree with my conclusions.

solution to the well-known philosophical difficulties here, but I do not believe that those difficulties should lead philosophers and scientists to abandon the notion of lawlikeness. The fact remains that scientists can discriminate lawlike from accidental generalizations.

Condition (ii) restates Mill's view that in an exact science scientists only understand the operation of the "greater" causes. It demands that each generalization be *reliable*: deleting its *ceteris paribus* clause and possibly adding some specific qualifications, the generalization is, within a certain class of cases, usually confirmed and seldom disconfirmed. The class of cases considered must be selected in some "natural" or independent way. The generalization that all rabbits are white is highly reliable in the class of white rabbits and quite reliable in the class of pet rabbits, but it does not satisfy condition (ii). I doubt whether much more in general can be said about how such classes are to be specified. Testing for reliability depends heavily on substantive (scientific) knowledge. Mill seems only to consider the case when the generalization is, without *any* qualifications reliable—as, presumably, "greater" causes are. There is, however, no reason why scientists cannot seek or obtain knowledge of independently operating causes, even when these are not "greater".

Condition (iii) reformulates Mill's insistence that scientists have some knowledge of many causal factors operating, of which they do not yet know the laws, or to which they cannot assign any precise contribution to the net effect. This further knowledge would be empty if it were not possible to "refine" S and to "excuse" S. To refine S is to change or qualify S in such a way that it is reliable in a larger class of cases or more reliable in the same class. To excuse S is to explain away its apparent disconfirmations (Compare Rescher 1970, p. 172). Mill is quite specific:

But if our deductions have led to the conclusion that from a particular combination of causes a given effect would result, . . . and where the effect has not followed, we must be able to show (or at least to make a probable surmise) what frustrated it: if we cannot, the theory is imperfect, and not yet to be relied upon. (3.11.3)

These justification conditions are, I believe, plausible and sensible. They are a reasonable formulation of the implicit criteria by which scientists and laymen assess the legitimacy of invoking *ceteris paribus* clauses to explain away apparent disconfirmations. Unless a generalization meets these three conditions, one cannot reasonably regard it as a law.

Although Mill addresses the problem of justifying inexact laws most explicitly in the *Logic*, where he regards such laws as implicitly qualified, he is not unaware in his earlier essay, "On the Definition of Political

Economy. . .” that such problems exist. In the earlier essay verification is needed to establish the applicability of the scientific conclusions, rather than their truth; but it is equally necessary. Are the problems of justification appreciably different when one adopts, as Mill does in his earlier essay, a counterfactual interpretation of inexact laws? I think not. Unless a generalization satisfies conditions very like the ones sketched above, one will not, *pace* the doctrine of “On the Definition of Political Economy . . .”, be justified in regarding it as a counterfactual law. The grounds for accepting a counterfactual law must be factual. Unless such a purported law possesses some reliability in this, the real world, and scientists are able to account for its apparent failures in this world, scientists are not justified in accepting the purported law as a law.

Is there then any important difference between interpreting inexact laws as qualified claims and regarding them as counterfactual assertions? There remain two differences. First, to regard inexact laws as implicitly qualified with *ceteris paribus* clauses is more modest metaphysically. It carries no explicit commitment to talk of possible worlds. Notice that if one is willing to talk of possible worlds, the qualified view entails the counterfactual view. If a generalization is true, with qualifications, in this world, it is true in that possible world in which those qualifications are always met.

The difference between the two interpretations of inexact laws is not exclusively metaphysical. Whether one regards inexact laws as qualified or counterfactual claims affects how one does science. The modal view creates an almost inevitable temptation to take characteristics of models seriously, even when one has no grounds to believe that the axioms are true (with qualifications) of anything real. Mill’s discussion in “On the Definition of Political Economy . . .” is particularly instructive in this regard. He succumbs to this temptation, since he regards the theorems of economics as true “in the abstract” regardless of any observations of economic phenomena; yet he devotes a good part of “On the Definition . . .” to warning economists not to take such “abstract truth” (1836, p. 329) very seriously, unless it is verified! Consequently, the talk of “abstract truth” does not lead far. I am not denying that one can adopt a counterfactual interpretation of inexact laws, yet be entirely fastidious about the justification for such laws. Yet economists who interpret economic laws counterfactually too often console themselves unjustifiably in the face of unfavorable evidence with the conviction that they still possess the (counterfactual) truth. Confronted with apparent disconfirmations of their “laws”, economists often comment that these “laws” are only guides to which concepts are central or accounts of how things *would* work out given perfect competition. Such claims require justifi-

cation.<sup>6</sup> Insisting, as Mill does in the *Logic*,<sup>7</sup> that purported inexact laws are true only if they possess an unmysterious reliability is a good tonic for this carelessness.

Regarding the purported laws of economics as legitimately qualified in the above sense captures economists' conceptions of their own work. To assert that people's preferences are transitive or that the marginal utility of commodities is a diminishing function of the quantity possessed is to make a qualified claim. A change in tastes, for example, falsifies neither, since changes in tastes are ruled out by our implicit *ceteris paribus* clauses. Mill speaks of the "psychological law" "that a greater gain is preferred to a smaller". This "law" is relevant when the determining cause of action is the "desire of wealth". Mill is not claiming that people always prefer greater gains, but that this is one motivational "force" which often predominates in relevant circumstances. We should regard economists as telling us how real agents behave in the absence of various complications. The elaborate models which economists construct are intended to analyze the predominant factors that operate (although modified and sometimes counteracted by various complications) in real economic behavior.

Apparent failures of legitimately qualified lawlike claims are not falsifications, since one has the qualification to invoke as an excuse (see Rosenberg 1976, pp. 137–8). Yet this excuse must be deserved. In certain situations it must be invoked rarely, if at all. In others the excuse must be made specific. We can narrow the class of cases in which such an excuse is needed and make our excuse more specific by adding further predicates to the antecedents of our lawlike claims. This is one way in which we may "compound causes".<sup>8</sup>

The *ceteris paribus* clauses in our lawlike claims in economics remain ineliminable; not all of the operative causal factors are included. Economics is inexact. If one managed to include all the lesser causes of economic phenomena, economics would merge with the other social sciences.

<sup>6</sup>Weber's notion of an "ideal type" leads, in my opinion, to a similar carelessness. See Weber 1904, esp. p. 90.

<sup>7</sup>I have not yet shown very adequately that Mill *does* insist in the *Logic* that inexact laws be verified before one can regard them as true. See the discussion of the deductive method in Section 3 below. In (6.9.1) Mill writes explicitly, "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*."

<sup>8</sup>I am interpreting Mill's talk of "compounding causes" in two ways. One way that causes are compounded is by modifying a particular causal law, especially by adding to its antecedent. Another way is through deducing new causal relations from more than one law. The two interpretations of "compounding" are compatible with one another.

**2. Economics as a Separate Science.** According to Mill, economics is not only distinct from the other social sciences, but it is a “separate science”. Mill writes:

Notwithstanding the universal *consensus* of the social phenomena, whereby nothing which takes place in any part of the operations of society is without its share of influence on every other part; . . . it is not the less true that different species of social facts are in the main dependent, immediately and in the first resort, on different kinds of causes; and therefore not only may with advantage, but must, be studied apart: . . . (6.9.3)

Mill is not asserting, trivially, that some social phenomena depend principally on a limited number of causal factors. Rather he is suggesting that a few causal factors are sufficient to account for at least the major features of a whole range of social phenomena. A full statement of Mill’s view is the following:

There is, for example, one large class of social phenomena in which the immediately determining causes are principally those which act through the desire of wealth, and in which the psychological law mainly concerned is the familiar one that a greater gain is preferred to a smaller. I mean, of course, that portion of the phenomena of society which emanates from the industrial or productive operations of mankind, . . . . By reasoning from that one law of human nature, and from the principal outward circumstances (whether universal or confined to particular states of society) which operate upon the human mind through that law, we may be enabled to explain and predict this portion of the phenomena of society, so far as they depend on that class of circumstances only, overlooking the influence of any other of the circumstances of society, . . .

It makes entire abstraction of every other human passion or motive, except those which may be regarded as perpetually antagonising principles to the desire of wealth, namely, aversion to labor, and desire of the present enjoyment of costly indulgences. (6.9.3)

Mill’s vision of economics as a separate science consists of at least two assertions. First, since he believes that a single set of causal factors are “immediately determining” for “one large class of social phenomena”, he believes that economics is a unified science. It would be extravagant to assert that a single theory serves *all* the explanatory and predictive purposes that economists have. Rather Mill means that a single theory

accounts for all the major<sup>9</sup> economic phenomena and that much of economic theorizing consists in adding further auxiliary hypotheses to that theory or making minor emendations in it, in order to account for more phenomena.

The second component of Mill's belief that economics is a separate science, is the conviction that economics is, within its own domain, complete. No explanatory or predictive purpose of economists would be served by fusing economics with any other science. Consider the following definition of economics, which is due to Mill's friend and disciple with respect to methodology, J. E. Cairnes. It suggests clearly the sense in which Mill believed economics to be complete. Political economy is defined by Cairnes "As the science which traces the phenomena of the production and distribution of wealth up to their causes, in the principles of human nature and the laws and events—physical, political, and social—of the external world" (1888, p. 71). According to this view, the explanatory task of economics is tracing economic phenomena to non-economic causal factors. What Mill and Cairnes have in mind, I think, is analogous to Mill's psychologism (see section 3 and note 12 below). The fundamental laws in economics cannot be explained within economics. They derive from psychological or natural scientific laws and specifications of economic circumstances. Unified and complete, economics is thus a separate science. The task of its practitioners is to apply the basic laws to particular problems.

The general idea that Mill is espousing is both appealing and relatively clear. If one can isolate the principal causal factors upon which production, distribution, and exchange depend, one can develop economics as an inexact science. One will then be able inexactly to explain and predict the principal economic phenomena. The results will not be exact, since Mill denies "that any political economist was ever so absurd as to suppose that mankind are really thus constituted" that they are only influenced by the desire for material gain (6.9.3). As a separate science, economics is necessarily inexact.

### 3. The Deductive Method.

When an effect depends on a concurrence of causes, these causes must be studied one at a time, and their laws separately investigated, if we wish, through the causes, to obtain the power of either pre-

<sup>9</sup>There is some circularity here, since the "major" economic phenomena are largely (but contra Stegmüller 1976, pp. 93, 176–77, *not* entirely) those which are central to the given theory. It is, of course, not obvious what counts as an "economic phenomenon". For more on these matters see Hausman 1981b, ch. 9, §2 and ch. 10, §2.



dicting or controlling the effect; since the law of the effect is compounded of the laws of all the causes which determine it. (6.9.3)

This talk of “compounding of the laws of all the causes” is crucial both to Mill’s methodological views and to his actual economic theorizing. This compounding is, for Mill, largely a matter of deducing; and he argues that economics does and must follow a deductive method. Knowing that individuals seek wealth and that they tend to have many children, economists investigate deductively what follows from these tendencies in various situations given other plausible assumptions and simplifications. To some extent the deductive method is needed for all inexact sciences, since inexactness reveals a complexity of causal factors, which renders Mill’s methods of induction inapplicable.<sup>10</sup> In inexact sciences the implications of theory will only agree with the results of experiment approximately for the most part. Inductive investigation will not be decisive. One will not have the sort of definite *proof*, which Mill believes that valid induction provides (3.2.4, 3.3.3, 3.9.6). Yet differences in degree here are significant. Unless we demand of induction the proof that Mill does, difficulties in theory construction and justification will only be acute in disciplines like economics where the correspondence between the data and the implications of theory is rough and complete failures are not infrequent. Since economic phenomena are the effects of numerous causes, many of which the theory does not deal with, we can expect nothing better. Yet, with only this sort of evidence, how could economists rationally come to construct their theories? What good reason do they have to accept them? Mill believes that we cannot answer these questions if we only consider how well the claims of economic theory are confirmed by observations of economic phenomena. Only the deductive method renders the construction of an economic theory reasonable and confidence in that theory justified.

By a deductive method Mill does not mean what others have meant by talking of a ‘hypothetico-deductive method’. Mill calls the latter method the ‘hypothetical method’ and is critical of it when it fails to prove its conclusions inductively (3.14.4–5). In insisting on the need for a deductive method, Mill is also not primarily concerned with how laws and theories are discovered. In discussing Whewell’s views, Mill makes clear that his methods of induction, although they may serve the purposes of discovery, are most important for the justification of scientific claims

<sup>10</sup>I doubt that Mill believes that inexact sciences demand that one employ the deductive method. His philosophical position implies this conclusion, since inexactness prevents inductive *proof*. Yet, if inexactness demands the deductive method, it follows that experimental sciences are all exact—which is surely an unacceptable implication. In the discussion below I shall challenge the claim that inexact sciences must employ a deductive method.

(3.9.6). In discussing the need for a deductive method, Mill does sometimes sound as if he is criticizing induction as a method of discovery. He argues that theorists will lack solid inductive proof for economic generalizations and insists that the results of deductions be verified by empirical tests. Yet Mill is not maintaining that scientists create hypotheses rather than derive them from evidence. Quite the contrary—the deductive method is in part an account of how one can *derive* economic laws from evidence of a different kind.<sup>11</sup>

Mill's deductive method consists of three stages (3.11). In the first, one establishes certain laws by *induction*. Whether induction functions here as a method of discovery does not matter. First, for example, scientists induce the laws of mechanics and of gravitation. Second, they deduce the laws of tides from these fundamental laws and specifications of the relevant circumstances (which themselves may or may not be lawlike). Third, scientists must verify the deductive results. But notice that they are not testing the basic laws, just the validity of their inexact lawlike consequences concerning the tides. Since many causal factors are left out, one does not know without testing how accurate or reliable the theory of tides is. Mill concedes that the testing of inexact laws also to some extent tests the inductively established laws upon which they are based, but believes that the weight of such testing is generally slight (3.11.3). The more inexact the science, the less one can test its fundamental laws and the more one needs to develop it deductively on the basis of independently established laws borrowed from other disciplines. It is important to stress that for Mill induction and deduction are *not* contraries. What is opposed to deduction is experimentation (2.4.5). Deductive grounds

<sup>11</sup>In one of the more confusing chapters in the *Logic* (6.8), Mill discusses what he calls the "geometric method" and contrasts it to the deductive, or "concrete" deductive method. Those who follow the geometric method do not allow for the "case of conflicting forces" (6.8.1). They suppose that each social phenomenon "results always from only one force, one single property of human nature" (6.8.1). Yet Mill concedes that the "Bentham School", against which the criticism is directed, applied their principles "with innumerable allowances. But it is not allowances that are wanted. There is little chance of making due amends in the superstructure of a theory for the want of sufficient breadth in its foundations" (6.8.3). Mill is trying to incorporate what he regards as the valid points in Macaulay's attack on James Mill's *Essay on Government* (see Mill 1873, pp. 121–22). Yet the philosophical point remains obscure to me. Mill is dissatisfied with the particular principles of the "Bentham School". I do not, however, see the methodological moral. R. P. Anschutz (1953, pp. 87–8), Alan Ryan (1974, pp. 89–91) and James Bonar (1893, pp. 243–44) assert that Mill holds that political economy employs the *geometrical* method. According to Ryan, Mill diagnoses the error his father and other Benthamites commit as applying the geometrical method to government, which, unlike political economy, cannot be studied separately. Ryan's interpretation makes Mill's critique of the geometrical method sensible, but I cannot agree that Mill believes that political economy employs a geometrical method. Mill discusses the method of political economy in (6.9), entitled "Of the Physical, or Concrete Deductive Method" and shows, step by step, how the method of political economy matches the deductive method as outlined in (3.11).

for belief are all ultimately inductive. That evidence which supports (inductively) the premises of a deductive argument is the (inductive) basis for one's belief in the argument's conclusions (2.3.3).

In the case of economics, theorists first borrow basic lawlike assertions from the natural sciences or psychology (which Mill regards as an introspective experimental science). Then theorists develop economics deductively. Verification is important and unavoidable, but not in order to test the lawlike statements; borrowed from other sciences they are already established. In fact as I mentioned earlier, in his earlier essay, "On the Definition . . .", Mill regarded resultant theorems concerning political economy also as true (in the abstract) regardless of what observation and experiment report (1836, pp. 325–26). The laws may turn out to be of no importance, but testing will never show that they are not laws, except by bringing to theorist's attention some mistake made either in the specific premises concerning the prevailing "outward" circumstances or in the deductions themselves. By the time he wrote the *Logic*, on the other hand, Mill is emphatic in insisting that deductively derived purported laws be verified in order to be counted as laws at all (3.9.3, 6.9.1).

Mill believes that the deductive method can be employed in economics because social phenomena are governed by "mechanical" laws. Mill distinguishes mechanical from chemical laws as follows:

In the one [the case of mechanical laws], we can compute the effects of combinations of causes, whether real or hypothetical, from the laws which we know to govern those causes when acting separately, because they continue to observe the same laws when in combination which they observed when separate: whatever would have happened in consequence of each cause taken by itself, happens when they are together, and we have only to cast up the results. Not so in the phenomena which are the peculiar subject of the science of chemistry. (3.6.1)

The deductive method is applicable in economics, because theorists can "cast up the results" of the various causes when acting separately. Mill never justifies his claim that the phenomena of society are mechanical. In his view mechanical phenomena are the general rule in most domains (3.6.2). One can see how his own economic investigations would have made him confident that social phenomena are mechanical. In Chapter III of Book VI of the *Principles of Political Economy*, for one example ("Influence of the Progress of Industry and Population on Rents, Profits, and Wages"), he examines separately the effect on the development of an economy of an increase of capital (population and technology held constant), of an increase of population (capital and technology held constant) and a change in technology (capital and population held constant).

He then, as it were, sums these three causal factors. The analogy to mechanics is palpable and persuasive. The conviction that social phenomena are mechanical is in essence what Karl Popper (1966, pp. 89–93) calls Mill's "psychologism".<sup>12</sup> Since laws governing social phenomena are mechanical, all fundamental laws in the social sciences must be psychological or, minimally, individualistic.

It should be stressed that the deductive development of economics (or of tidology) is not a matter of proving theorems with established laws as the only axioms. The premises of the deductions include as well a number of other stipulations or auxiliary hypotheses. Not only will these often be poorly established, but one will often know that some are great simplifications (and thus false). Furthermore, the existence of implicit *ceteris paribus* qualifications in the fundamental lawlike claims complicates matters. If one combines lawlike claims which are qualified with different *ceteris paribus* clauses, what sort of qualifications are the theorems supposed to carry? In the deductions, economists often simply ignore the implicit qualifications, which is tantamount to assuming that in the given case all things are in fact equal. But they seldom have much confidence in such an assumption.

The messiness of the "deduction" in the deductive method makes such a method of discovering and justifying scientific theories both more problematical and more interesting. One of the tasks of a logic of discovery (if there can be such a thing) is to lay bare the reasoning which makes plausible first attempts at scientific theories. The sort of wishy-washy deduction described above does make plausible what is deduced. If some economic claim can be shown to follow from more fundamental generalizations and auxiliary hypotheses which are reasonable approximations or simplifications, one has reason to take that claim seriously. Principles like Say's Law were embraced by economists on such grounds.

**4. Application: Justifying Current Economic Theory.** Much remains to be said about the adequacy of Mill's deductive method, but that discussion is best combined with an assessment of what Mill's views can contribute to current philosophical discussion concerning neoclassical economics. Mill's conception of economics as a separate science, although restrictive, is, I think, extremely helpful in interpreting the conceptual structure of the fundamental theory of current microeconomics and general equilibrium models and the strategy of neoclassical theoriz-

<sup>12</sup>Popper's critique of Mill's "psychologism" seems to me insensitive to Mill's thought and confused. Mill thought ethology (the science of the formation of character) to be the source of the fundamental causal laws of the social sciences. Popper's "situational logic" seems to me to treat social phenomena as, in Mill's sense, mechanical.

ing. This contention cannot, however, be well defended or even illustrated without extensive discussion of current economic theory, which I have provided elsewhere (Hausman 1981a) and which would be out of place here. Current equilibrium theory is intended to be both unified and complete in the senses discussed above in section 2. It is illuminating to regard it as a separate science (see Hausman 1981b, ch. 9, §2).

One need not, however, provide a detailed description of current economic theory in order to discuss whether it is helpful to regard economics, as Mill did, as an inexact science that employs (as it must) a deductive method. Philosophical discussion of economics has focused on one peculiarity of economic theory. The fundamental theory of microeconomics and of general equilibrium models seems simultaneously sophisticated, successful, bursting with explanatory power and full of false statements. These false statements are not only simplifications or auxiliary hypotheses which enable economists to apply the fundamental "laws", but seem to include the fundamental "laws" themselves. We know full well not only that commodities are not infinitely divisible (which is only intended as a simplification), but that businessmen do not always attempt to maximize their profits and that the preferences of consumers are not always transitive. "Businessmen maximize profits" and "A consumer's preferences are transitive" are fundamental economic "laws". How can economists rationally accept a theory which is so full of falsehoods?

This is not the occasion to discuss the many answers which economists and philosophers have given to this last question. What I want to consider here is whether Mill's conceptions of inexact sciences and of the deductive method provide any solution to this problem of justification. I shall argue that Mill's conception of the laws of inexact sciences as implicitly qualified is a significant contribution. Mill's discussion of the deductive method, on the other hand, contributes only a little to understanding whether one is justified in regarding microeconomic theory as a good and well-confirmed scientific theory.

Many apparent difficulties with the "laws" of economics can be met by arguing that they contain legitimate implicit *ceteris paribus* clauses. Once we recognize that inexact sciences are perfectly respectable and, indeed, in my view, the best one can hope for in economics, some measure of apparent disconfirmation is only to be expected. Hedged, as they implicitly are, with *ceteris paribus* clauses, one can regard economic generalizations as laws despite apparent disconfirmations. No economist regards the generalization that consumer's preferences are transitive as falsified by a change in tastes. An entrepreneur who suddenly decides to give away the business and become a nun does not falsify the purported law that entrepreneurs attempt to maximize their profits. In each of these apparent disconfirmations, the *ceteris paribus* condition is violated.

Of course, not all uses of *ceteris paribus* clauses are intellectually defensible. We must go on to ask whether the "laws" of economics satisfy the justification conditions specified above. This question seems to me a difficult and unresolved empirical question. Except in restricted applications, I am dubious. If these conditions are not met, Mill's discussion of the legitimacy of inexact sciences may not contribute to the *defense* of microeconomics, although it may help us to understand its inadequacies.

Mill's discussion of the deductive method is not nearly as helpful. The deductive method is supposed to resolve difficulties in establishing the claims of inexact sciences. Yet, if the generalizations of an inexact science satisfy the justification conditions discussed above, the deductive method seems largely unnecessary. If one can come up with lawlike statements that are nearly always true in a certain natural class of circumstances and if one has some idea how to start filling in the implicit *ceteris paribus* qualifications, what need does one have of a deductive method? One does not have the sort of definitive inductive *proof* which Mill believes the sciences can achieve, but, in my view, one never has inductive evidence for any lawlike statement which is better in *kind* than this sort of evidence. To say this is not to deny that there are extremely important differences in *degree*. Nor is it to deny that deducing a given generalization from other statements which one has reason to accept provides additional confirmation for that generalization.

One might indeed argue that the deductive method can only be necessary when it cannot be applied. Crucial to Mill's notion of the deductive method is verification of the derived generalizations. But, if one can carry out this verification, what warrants the asymmetry between the claims of psychology, which Mill believes can be inductively established, and those of economics, which require the application of the deductive method?

Mill's deductive method thus has little to contribute to current philosophical discussions of the justification of microeconomic theory. Either the theory can meet the conditions for the legitimate use of *ceteris paribus* clauses, in which case Mill has provided a potent philosophical vehicle for defending current theory, or it cannot. The deductive method contributes little more. The inductively established "psychological laws" upon which theorists are supposed to base economic theory turn out to be unexciting platitudes about human beings (they do what they prefer) which, as is the common fate of platitudes, are not quite true anyway. At best, the deductive method points out that one can find further evidence for the claims of economics in everyday experience and casual introspection.

One should not, however, entirely discount the importance of Mill's deductive method in rationalizing the esteem in which current economic

theory is held. Even if current microeconomics can pass the tests for the legitimate use of *ceteris paribus* clauses, it cannot pass them in all domains in which it is applied with the wide margin by which well-established theories of physics pass. Apparent disconfirmations are frequent. Yet the explanatory power of the theory seems great. We could use further grounds for accepting such purported laws and further rationalizations for the confidence that we place in the explanations of the economist. Consider a generalization such as, "Other things being equal, when the price of a commodity *C* goes up, people will buy more of those commodities which are substitutes for *C*." My suggestion is that whether or not we believe that this generalization is a genuine law, we know from "psychology" (conceived introspectively as Mill conceived it) that there is a real causal connection between the increased price of *C* and the shift in consumption. Even if this connection cannot be captured (at least at present) in any unqualified universal generalization, we know that this is not merely an accidental correlation. Our ability to borrow this law from "psychology" or, more prosaically, to find support for it in our own experience, justifiably increases our confidence that this generalization is lawlike. The deductive method boosts in this way the explanatory power of inexact economic theory. Mill's deductive method thus has a modest contribution to make to current philosophical debate concerning microeconomic theory. It enables us to see how that theory can be explanatory, even if it only contains generalizations which, if true at all, require extensive qualifications.

Economists usually do not insist that their theories are as stated literally true. Rather economic theories are supposed to be close to the truth—perhaps as close as their simplicity allows. Mill's conception of inexact sciences permits us to give a coherent philosophical construal of such a view. His notion of the deductive method helps rationalize the confidence economists place in their theories despite their empirical difficulties.

## REFERENCES

- Anschutz, R. P. (1953), *The Philosophy of J. S. Mill*. Oxford: Clarendon Press.  
 Bonar, J. (1893), *Philosophy and Political Economy*. rpt. London: Allen & Unwin, 1967.  
 Cairnes, J. E. (1888), *The Character and Logical Method of Political Economy*. 2nd. ed. rpt. New York: A. M. Kelley, 1965.  
 Cartwright, N. (1980), "Do the Laws of Physics State the Facts?" *Pacific Philosophical Quarterly* 61: 75–84.  
 Gibbard, A. and Varian, H. (1978), "Economic Models", *Journal of Philosophy* 75: 664–77.  
 Giere, R. (1979), *Understanding Scientific Reasoning*. New York: Holt, Rinehart & Winston.  
 Hausman, D. M. (1981a), "Are General Equilibrium Theories Explanatory?" in J. Pitt, (ed.) *Philosophy in Economics*. Dordrecht: Reidel.  
 Hausman, D. M. (1981b), *Capital, Profits and Prices: An Essay in the Philosophy of Economics*. New York: Columbia University Press.

- Hempel, C. (1965), *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: Free Press.
- Hempel, C. and Oppenheim, P. (1948), "Studies in the Logic of Explanation", rpt. in Hempel (1965).
- Hicks, J. (1946), *Value and Capital* 2nd. ed. Oxford: Oxford University Press.
- Hutchison, T. W. (1938), *The Significance and Basic Postulates of Economics*. rpt. New York: A. M. Kelley, 1960.
- Keynes, John Neville (1890), *Scope and Method of Political Economy*. 4th ed. rpt. New York: Kelley & Millman, 1955.
- Levi, I. and Morgenbesser, S. (1964), "Beliefs and Dispositions", *American Philosophical Quarterly* 1:221-32.
- McClelland, P. (1975), *Causal Explanation and Model Building in History, Economics and the New Economic History*. Ithaca: Cornell University Press.
- Mill, J. S. (1836), "On the Definition of Political Economy and the Method of Investigation Proper to It", rpt. in Mill (1844).
- Mill, J. S. (1843), *A System of Logic*. rpt. London: Longmans, 1949.
- Mill, J. S. (1844), *Essays on Some Unsettled Questions of Political Economy*. rpt. in *Collected Works of John Stuart Mill*, vol. IV. Toronto: University of Toronto Press, 1967.
- Mill, J. S. (1848), *Principles of Political Economy*. 7th ed. rpt. in *Collected Works of John Stuart Mill*, vol. 2 and 3. Toronto: University of Toronto Press, 1965.
- Mill, J. S. (1873), *Autobiography*. rpt. New York: New American Library, 1964.
- Morgenbesser, S. (1969), "The Realist-Instrumentalist Controversy", in S. Morgenbesser, P. Suppes and M. White, (eds.) *Philosophy, Science and Method*. New York: St. Martin's, pp. 200-218.
- Popper, K. (1966), *The Open Society and its Enemies*, vol. II. Princeton: Princeton University Press.
- Rescher, N. (1970), *Scientific Explanation*. New York: Free Press.
- Rosenberg, A. (1976), *Microeconomic Laws: A Philosophical Analysis*. Pittsburgh: University of Pittsburgh Press.
- Ryan, A. (1974), *J. S. Mill*. London: Routledge.
- Schumpeter, J. (1954), *History of Economic Analysis*. E. Schumpeter (ed.) New York: Oxford University Press.
- Shapere, D. (1969), "Towards a Post-positivistic Interpretation of Science", in P. Achinstein and S. Barker, (eds.) *The Legacy of Logical Positivism*. Baltimore: Johns Hopkins University Press.
- Stegmueller, W. (1976), *The Structure and Dynamics of Theories*, tr. W. Wohlhueter. New York: Springer-Verlag.
- Suppe, F. (1974), "Theories and Phenomena", in W. Leinfellner and W. Kohler, (eds.) *Developments in the Methodology of Social Science*. Dordrecht: Reidel, pp. 45-92.
- Suppe, F. (1976), "Theoretical Laws", in M. Przełeczki, K. Szaniawski, and R. Wojcicki, (eds.) *Formal Methods in the Methodology of Empirical Science*. Wrocław: Ossolineum, pp. 247-67.
- Suppes, P. (1957), *Introduction to Logic*. New York: Van Nostrand-Reinhold.
- Weber, M. (1904), "Objectivity in Social Science and Social Policy", tr. E. Shils and H. Finch. rpt. in M. Weber, *The Methodology of the Social Sciences*. New York: Macmillan, 1949.