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A Revision of the Neoclassical Economics Methodology
appraising Hausman's Mill-Twist,
Robbins' Gist and Popper Whist

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Abstract

The practice of neoclassical economics is characterised as an 'axiomatic positivism', which is far removed from the official (Popper-Lakatos) methodology of neoclassicism. Hausman (1992) attempts to provide a full revision of that official methodology, for which he takes recourse to the methodological work of J.S. Mill. Hausman's methodology is problematical because of: (1) an inadequate distinction between a normative and a descriptive methodology; (2) an insufficient consideration of the empirical stages of theory appraisal; (3) a misleading account of the tendential character of economic generalizations, as revealed by his treatment of them as ceteris paribus formulations. Further, an arbitrary part of the theory assessment in Hausman's approach seems to run in praxeological terms, apparently divorced from the methodological appraisal.
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Introduction

The core of neoclassical economics is equilibrium theory. Its empirical content is weak. It is either unfalsifiable or, if it is falsifiable, it is faced with apparent empirical disconfirmations (cf. e.g. Blaug 1992, Hausman 1992 and Rosenberg 1992). Nevertheless in the domain of economic policy, politicians and other citizens often seem to rely on advice provided by neoclassical economists. Why can they be confident in this domain? Perhaps because these economists seem to be good mathematicians?

The current methodological situation in which neoclassical economics finds itself appears to be complicated. Those that seem at least mildly sympathetic to the neoclassical tradition/program have split into two camps. The first is a camp which defends and advocates a methodology in line with (some variety of) Popper's falsificationism. This may be called the official strategy — which is in line with the professed methodology of neoclassicals, not with their practice. The leading advocate is Blaug (1980, 1992). The second camp is as least as old as the first: its unifying characteristic is that it takes the neoclassical practice as its starting point which is then developed one way or another into the norm for that practice.

In this paper I look at the methodological credentials of neoclassical economics from the perspective of the second camp. In section 1, I briefly present the three main twentieth-century episodes of their methodology of neoclassical economics (Robbins, Friedman, McCloskey). Against this background I discuss (in section 2) the recent account of neoclassical economics practice in Daniel Hausman’s 1992 book. Hausman attempts to provide a full revision of the methodology of economics. To do this, he starts with J.S. Mill’s approach to methodology and develops it into one suited to his purposes of describing the neoclassical economics practice. Starting from this same approach but developing it somewhat differently, in section 3 I consider how it might be possible to bridge the gap between the two methodological camps referred to.

1. An early proponent of this line in economics is Klant (1972, see also 1984 and 1994). Although I have worked with him I never found out if he, like Blaug, was in fact mildly sympathetic to the neoclassical research program. I bet he was not.

2. It is not the case that these two camps can be distinguished merely by normative versus descriptive methodology. This issue does play a role. However it is surely not the case that either of the camps considers one of the poles (normative, descriptive) unimportant. Nevertheless I should add that Blaug in the preface to his 1992 book further underlines the normative pole of the methodology he advocates.

3. Of course I am not a theory-free observer. In the perspective of this paper I feel I should try to make explicit my own position vis-à-vis the philosophy of social science and economics in particular. That position is, I am afraid, not a one-liner. (1) I am moderately sympathetic to Popper (even if I feel his advocates often insufficiently account for the specificity of the social sciences); falsifiability of theories is important. (2) I am critical towards any form of positivism in a broad meaning of the term (and in this respect I include Popper) to the extent that it takes the revealed appearances of the status quo for granted and as permanent fixtures (as if they were natural) — in (continued...
1. The methodological practice and appraisal of neoclassical economics

1.1 Axiomatic positivism

Mainstream economists in the nineteenth century through to the 30s of this century have invariably leaned in their **professed** methods towards an **empiricist positivism**. With the gradual mathematisation of economics beginning in the 1930s (the Marginalists in the 1870s had already provided a basis), the positivism took a Euclidian turn. From that time onwards the axiomatic mathematics has been the prototype for the practice of neoclassical economics — so much so that we may usefully speak of an 'axiomatic positivism'. Neoclassical economics takes the form of a deductive nomological system of axioms (here assumptions function as axioms) and rules of deduction (those of mathematics and formal logic). The content derives, via definitions, from the prevailing representations of a number of prevailing economic institutions. Both of these (form and content) apply not only to the theoretical economics, but also (with some adaptation) to the applied economics. Within this approach there is hardly any room for the development of concepts. This may explain...

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3. (...continued)

3. (continued) I sympathise with the Hegel-Marx-Horkheimer view of social science. (It is obviously not an easy thing to combine 1 and 2.) (3) I do not trust scientists who are not seeking truth — even if they, and I, are confident that there is no ultimate criterion for truth; scientists are condemned to provide criteria for theory appraisal in the face of the search for truth; if they cannot they should take on another job. (4) It is inherent to science to be critical and to weigh vested interests and to unmask prejudices. (3 and 4 are Enlightenment ideas — some call them 'modern'; 4 may and should scrutinise any positive statements of 3.) (5) In the face of 1-4 science proceeds by way of conceptual development (surveying the implications of some set of axioms (definitions) is not development; it is merely a — perhaps temporarily useful — analysis of concepts).

4. 'Positivism' is taken in a broad sense of somehow describing, explaining, etc. the outward appearances of the institutions of the status quo. It is definitely not restricted to logical-positivism. Whereas the professed method was an empiricist positivism the practice was mostly a rationalistic positivism. At the end of the century the Historical School in economics reacted against this practice of the Classicals (a line of reaction taken on somewhat later by the early American Institutional school against the then prevailing Marginalism/early Neoclassicism — cf. Morgan 1993).

5. Cf. Morgan 1990: 2-3. The reference here to the axiomatic mathematics of Euclides is drawn from Dow (1985: ch. 2). In the 1930s Tinbergen provided an important impetus to this Euclidian turn — see Boumans (1992) on the work of Tinbergen in this period. All this alludes to the neoclassical mainstream in economics. Resistance came from the Institutionalists, the Austrian School as well as from dialectically oriented Marxists. Keynes and Hutchison were also early critics, though from different perspectives.

6. There is room, however, for the exploration of the consequences of axioms. In an interesting and thought-provoking chapter on 'Models and theories in economics' Hausman (1992: ch. 5) uses in this context the term 'conceptual exploration'. I do not think we disagree here. Economist's models in his view are definitions and the exploration is about their consequences. This is what one might call 'analysis'. 'Conceptual development' in the sense in which I use it does not start from definitions. Conceptual development breaks away from that which was fixed (cf. Reuten & Williams 1989: 14-21).
why within the neoclassical approach theoretical innovation has been so poor. 7

The practice of neoclassical economic research has been developed largely in isolation from what happened on the methodological scene in other sciences or in the philosophy of science. At the same time one has paid lip service to the philosophy of positivism. The development of practice in isolation from general methodology and philosophy need not be an insurmountable problem. However, given this situation it is a problem that neoclassical economists themselves have neglected to develop a methodological foundation for what they are actually doing. 8 One price for this is that today neoclassical economics has completely lost sight of what progress within this tradition (program) might mean. The only one clear criterion of assessment that seems to be applied today is mathematical rigor — so has neoclassical economics become a branch of mathematics (cf. Rosenberg 1992)? 9

Neoclassical economists hardly ever provide a methodological account of their work. In general they consider their science as an empirical one. The methodology referred to is mostly that of Popper's 'critical rationalism' and falsificationism. However, methodologists of economics agree that this reference does not fit the practice: it is hardly ever the case that falsifiable theories are submitted to empirical test. 10 Nevertheless in some fields there is an empirical practice which amounts to the empirical application of models and the statistical estimation (not the empirical testing) of relations. At best these are vaguely derived from the theory. Such estimation takes the form of a selective

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7. Although Rosenberg (1992) may perhaps not agree with the importance of conceptual development in its explanation, he would agree with the poor performance of neoclassical economics. 'Rational expectations theory, the most fashionable response to the alleged failure of Keynesianism, returns to be [neo]classical economic theories which Keynesianism claimed to have superseded.... [It is] nothing more or less than a return to the status quo ante to the [neo]classical theory of Walras, Marshall and the early Hicks, which Keynesianism had preempted. This cycle brings economic theory right back to where it was before 1937 and might well undermine confidence that economics is an empirical science. (...) ... economics has not moved away from the theoretical strategies that have characterized it at least since 1874, in spite of their practical inapplicability to crucial matters like the business cycle, economic development, or stagflation,' (Rosenberg 1992: 229-30)

8. Hausman's book (1992) should be of great help to start filling this gap (even if he may not consider himself a practicing neoclassical economist).

9. For a perceptive evaluation of current neoclassical economics as an applied mathematics, see Rosenberg 1992, ch. 8. See also Blaug (1992: xxii), who uses the term 'social mathematics' as a name for the 'empty formalism that has come increasingly to characterize the whole of modern economics'.

gathering of correlations. This gathering, however, usually has hardly any consequences for the theory development.

At the same time, there is scarcely any check or control over the way in which empirical-statistical research progresses. Neoclassical economists are hardly ever engaged in discussing the empirical findings of others, notwithstanding some feedback pertaining to estimation results within a field of specialisation. However, even within such a field the estimations are usually considered to be incomparable because they rely on very different model specifications. It is even commonplace for one and the same researcher to publish the results of one model specification in journal A and that of another in journal B. (Of course this reveals the lack of reliability the researchers themselves attach to their models — and thus how far they are removed from any representation of truth-likeness in their research.) Between different fields of specialization there is virtually no feedback, let alone a check on one another’s progress. It is never the case that a neoclassical economist is perturbed by the fact that when in a model within field A, for example, investment is dependent on quite a different set of variables than the very same investment within field B.

Thus in fact the link between theory and the empirical in neoclassical economics is unclear. Or, more to the point, usually theoretical research is not submitted to empirical test, and usually empirical research (the gathering of correlations) is not underpinned theoretically. Neoclassical economists do acknowledge this. The problem is that they do not draw any constructive consequences from it. It is rather the case that a sheltering of their core theory from confrontations with the empirical has been defended repeatedly. In this respect Hausman’s 1992 book on economic methodology (section 2 below) is part of a twentieth-century tradition. To illustrate this I will first discuss in passing the three main episodes of this tradition.

1.2 The sheltering of theory by Robbins, Friedman and McCloskey

The locus classicus of this sheltering of theory from empirical fact is to be found in Lionel Robbins’ An Essay on the Nature & Significance of Economic Science (1932). Over sixty years after its appearance it is still worth consulting it in order to understand neoclassical economics. The following text is crucial:

"The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience relating to the way in which the scarcity of goods which is the subject-matter of our science actually shows itself in the world of reality. The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in

11. An important exception is the correlation between the change in unemployment and the change in the wage rate as found by Phillips (1958). Even the status of this relation remains a subject of heated debate today. (Cf. de Marchi.)
fact do so. The main postulate of the theory of production is that there are [sic] more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. These are not postulates the existence of which admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious.' (Robbins 1932: 78-79 — italics added)

Even if Robbins would not want to admit dispute, in fact some of these 'chief' as well as lesser postulates have been put to empirical test, both in the field of the theory of the firm and in the field of consumer theory (see Hausman 1992: sections 9.4, 12.4 and ch. 13).

The early falsifications gave rise to Milton Friedman's instrumentalist reaction (1953). Friedman argues that the postulates do not need to be 'realistic' (pace Robbins) as long as they are fruitful in providing predictions. Although Friedman's instrumentalism did not remain uncriticised amongst economists, it did provide neoclassical economists with an argument to keep on exploring models on the basis of falsified postulates.

The Friedmanian view on assumptions has had a general impact on the neoclassical economics profession — even on those who would not want to call themselves instrumentalist. It has strongly contributed to the neoclassical economics style of axiomatic positivism referred to in section 1.2.

The puzzling thing about the instrumentalist position in neoclassical economics is not the instrumentalism in itself. Realists, dialecticians and critical rationalists may of course have severe objections to it from their perspective. It is rather the instrumentalist’s own perspective in the face of the neoclassical economics practice that is puzzling. For one does not need to be much of an insider to know that: 1) the larger part of the neoclassical economic publications is not empirical; 2) of those that are, few are predictive; 3) predictions of a more than generic character are mostly wrong. In fact so-called naive models (predictions on the basis of trends) do no worse than theoretically informed...
ones. If the theory is not predictively fruitful, even after it has been gestated for a considerable period of time, then the neoclassical economics theoretical practice becomes difficult to understand judged from an instrumentalist point of view.

In 1983 one of the leading neoclassical economics journals, the *Journal of Economic Literature*, devoted a considerable amount of space (37 pages) to an article opening with:

'Economists do not follow the laws of enquiry their methodologies lay down. A good thing, too.'

The first sentence contains no news. The second does. Its author, Donald McCloskey, does not think much of prediction in economics:

'The common claim that prediction is the defining feature of a real science, and that economics possesses the feature, is ... open to doubt.' (McCloskey 1983: 489)

For support he cites a distinguished economist, Ronald Coase, who a year earlier had written:

'economists, or at any rate enough of them, do not wait to discover whether a theory's predictions are accurate before making up their minds' (cited by McCloskey p. 489-90).

Toss out the Friedmanian predictive criterion?

The flesh of McCloskey's article is an apt description of the (neoclassical) economics practice — which he calls the workaday rhetoric — as it diverges from what he calls the modernist official methodology of economics. The latter is a blend of positivism and a Popperian falsificationism as canonized especially in the work of Blaug (1980). McCloskey's is not merely a plea for devoting more attention to empirical methodological studies (i.e. studying the actual practice). No, he pleads for making the actual practice the norm: the economists’ 'persuasive reasoning'. 'The invitation to rhetoric brings into the open the arguing that economists do anyway ...' (p. 509).

16. On prediction, see also Rosenberg’s account (1992), ch. 3 'Is economic theory predictively successful?'.

17. He leaves it somewhat implicit what he exactly means by rhetoric. One definition of it is 'the art of probing what men believe they ought to believe, rather than proving what is true according to abstract methods', and another: 'careful weighing of more-or-less good reasons to arrive at more-or-less probable or plausible conclusions' (p. 482, as borrowed from Wayne Booth). A third is 'Rhetoric is exploring thought by conversation.' (p. 482). In the end he actually works with the notion of 'persuasion' (see below).


19. The following provides some clue as to what he means by persuasion: 'One can tell whether it is persuasive only by thinking about it. ... There are some subjective, soft, vague propositions that are more persuasive than some objective, hard, precise propositions.' (p. 511). And: 'The economic fact he [the economist] has mostly from looking into himself and seeing it sitting there. ... The “scientific” character of the test is irrelevant. ... [Possible agreement on precise interpretation of (continued...)}
His arguments for this 'invitation' are the bones of his article — they take in a relatively small amount of the body, nevertheless this is what it is all about:

'To attempt to go beyond persuasive reasoning is to let epistemology limit reasonable persuasion.' (p. 512)

With his abhorrence of epistemology and methodology, McCloskey constructs a straw man. Wiener Kreis positivists and Popperians have never done anything else but try and persuade scientists of the usefulness of their normative methodology. Epistemology does not limit reasoning, rather it points at the limits of reason — as such it is an invitation to think.

Throughout the 1980s, McCloskey's ideas received quite some attention and applause not only from neoclassical economists — practice saved, as had Friedman thirty years earlier — but also from a number of methodologists of neoclassical economics. Why the latter? Between about 1950 and the mid 1970s it seemed that the neoclassical economists practice could be fitted into some rough version of verificationism/confirmationism (though not falsificationism). It seemed thus that the distance between methodology and practice was one that might be bridged. However, at the end of the 1970s, after the 'structural change' in the economies of the Northwestern world, even a rough form of verificationism/confirmationism was difficult to detect in the neoclassical economics practice, and this raised fears for a loss of contact between the two realms. McCloskey filled the gap, and this was facilitated owing to his jargon of 'modernism' which rang the bell of trendy 'postmodernism'.

More recently methodologists variously point at the dangers of an incestuous economics. Three assessments by Blaug (1992: xvii-xx), Hausman (1992: 263-269) and Rosenberg (1992: chs. 2 and p. 252) are indicative in this respect. Rosenberg considers McCloskey's position 'a dead-end' and the 'abolition of economics as an organized body of knowledge'; if the doctrine is right then 'there is no hope for improvement in economic knowledge — for in his view, at its best economics is already as good as we can expect it to get' (1992: 31). No one has ever denied, Rosenberg argues, the claim that economists 'attempt to persuade their readers'. 'What has been denied, since the time of Socrates, is that persuasion is ipso facto justification.' (p. 41-42)

19. (...continued)

"tests] is a poor argument for ignoring introspection if the introspection is persuasive ...' (p. 512).

Here we are back to Robbins as quoted above. And again: '... we have much information immediately at our disposal about our own behavior as economic molecules ... (p. 514).

20. McCloskey wants to get rid of method and methodology, 'The objections to modernist method so far, however, are lesser ones. The greater objection is simply that modernism is a method.' (p. 490). He seems to think that he can make do without method. This is the same kind of naiveté of believing that one can abstain from taking a position by not voting. But in fact McCloskey tries to persuade his readers to adopt a particular method: the one neoclassical economics actually uses.
'If economics is best viewed as more akin to a branch of mathematics [this is what Rosenberg forcefully argues — GR] ... the vacuum that economic theory leaves in the guidance of policy must be filled by something else, something that will provide improvable guidance to policy, both private and public. That there might be an alternative to economic theory that is better at policy guidance is an alternative the rhetorician declines to face.' (Rosenberg 1992: 252)

Hausman, at the time one of the two editors of the leading journal in the field, Economics and Philosophy, aligns himself with with Rosenberg:

'... if economics ever succeeded in repudiating all epistemology and answered only to its own standards, it would lose its influence on non-economists and its rational hold on economists themselves. If the standards of acceptance among economists had no connection to epistemologically significant goals, such as reliability or truth, then the fact that a particular conclusion was accepted by most economists would be of as little interest to policy-makers as is the fact that a particular conclusion is accepted by most astrologers.' (Hausman 1992: 269)

2. Hausman's methodological appraisal: 'not necessarily unjustifiably dogmatic'

Hausman, along with many other methodologists of economics, is well aware that a McCloskeyian giving up of epistemological and methodological foundations for economics amounts to giving up economics as a science. Given that (neoclassical) economists do not live up to the positivist cum Popper strand of methodology, one might be tempted to try to persuade economists to improve their lives — this is the position that has been forcefully adopted by methodologists such as Blaug (1980, 1992). For Rosenberg (1992), however, this is — given the make-up of neoclassical theory — a futile invitation or even an impossibility. Another strategy would be to tinker with the methodological rules. As long as the facts do not behave any better, however, it should be a more substantial tinkering than the Friedmanian. This is the project of Hausman's The Inexact and Separate Science of Economics (1992), which attempts to provide a full revision of the official methodology of economics.

Hausman’s book contains a perceptive description of the structure of the microeconomic core of neoclassical economics (Part I: chs. 1-7). It also contains a description of the problems of the mainstream methodology (roughly the Popper-Lakatos approach) and its application, much of which should not be too contentious (chs. 9-11). Hausman’s conclusions from this, however, are controversial. The constructive part of the book derives from his particular interpretation of J.S. Mill’s methodology of political economy (ch. 8), as well as his ‘modification’ of the Millian approach as applied to current neoclassical economics (ch. 12).
I think that much of the Millian approach that Hausman sets out in chapter 8 of his book is fruitful for the methodology of the social sciences, and I applaud Hausman's bringing this to the fore. Much of the methodological power of this approach, however, is lost by the 'ceteris paribus twist' that Hausman gives to Mill (discussed in section 3 below). I consider Hausman's approach weak when he applies 'Mill twisted' to current neoclassical economics. It seems to me that he has not really made up his mind as to whether his purpose is to describe the practice of neoclassical economics and set out a methodology that defends that practice, or to set out a sound 'demarcative' methodology that is applicable to that practice. I will turn first to Hausman's diagnosis and appraisal of neoclassical economics (this section 2).

2.1 A diagnosis of neoclassical economics

Hausman's diagnosis of the microeconomics of neoclassicism is given in chs. 9-11:

'... the striking methodological schizophrenia that is characteristic of contemporary economics, whereby methodological doctrine and practice regularly contradict one another.'

This diagnosis, of course, is not particularly remarkable. However, what is remarkable that he seems to have privileged knowledge as to where to look for the cure, for he continues:

'This schizophrenia is a symptom of the unsound philosophical premises underlying contemporary economic methodology, and it shows the importance of transcending the terms of the current debate.' (Hausman 1992: 152)

Might it perhaps be the case that the economic practice is unsound? Might it be the case that both methodology and practice are unsound? Let us dwell, for the time being, on Hausman's premisses — which are orthodox neoclassical microeconomics and the orthodox positivist-inspired methodological doctrine. In fact the main methodological doctrine Hausman refers to is Popper's falsificationism including the Lakatosian sophistications.

'If science consisted only of falsifiable but unfalsified claims, economic theory would be either an empirical failure or an unfalsifiable metaphysical theory that might be of use in the development of empirical theories.'

This is what we knew — Hausman, however, puts the problem in a particular perspective. He continues:

'Even though one might still find economics to be useful metaphysics, the costs of such an interpretation are considerable. For, in such a view, there are no empirical discriminations to be drawn between neoclassical economics and other approaches (unless, unlike neoclassical economics, some of the other approaches actually qualified as scientific) ...' (Hausman 1992: 189 — italics added)

21. In a comment on an earlier version of this paper (Reuten 1993) presented at the ASSA meetings in 1994, Hausman considered this text as an example 'of careless writing'. 'When I composed this most incriminating passage that Reuten quotes (from page 189) I was in fact borrowing from the draft of a letter I had written to Mark Blaug.'
And in a similar vein, somewhat further on:

'... once economists tie themselves to a philosophical system such as Popper's or Lakatos', they will be trapped with its unattractive consequences. A greater measure of philosophical agnosticism among economic methodologists would, I think, be sensible.' (Hausman 1992: 203 — italics added)

One might wonder about which 'costs of interpretation' and 'unattractive consequences' Hausman has in mind here. Anyway, his approach appears to be giving a rather strange twist to the issue. It does not argue for the establishment of some sensible criterion or other and the subsequent use of it to discriminate. No, he seems to want to discriminate 'neoclassical economics' from 'other approaches' in the first place.

Hausman simply seems to accept that economists hardly test their theories:

'It seems to me ... that economists are so little involved with testing because, first, many are involved with non-empirical conceptual work ... [1] Second, even those who are interested in empirical theory are also relatively uninvolved with testing ... because, given the subject matter they deal with, they do not know enough to formulate good tests or to interpret the results of tests.' (Hausman 1992: 190)

This looks like general failure of neoclassical economics as an empirical science rather than the failure of some Popperian 'methodological doctrine'. Is not such a statement the kiss of death for neoclassical economics as an empirical science? Hausman will no doubt have his reasons for putting these things rather mildly (or are they just understatement?). A less mild commentator (and economics Nobel prize laureate, by the way) comments on the economists in Hausman's second category:

'In no other field of empirical enquiry has so massive and sophisticated a statistical machine been used with such indifferent results. Nevertheless, theorists continue to turn out model after model and mathematical statisticians to devise complicated procedures one after another.' (Leontief, quoted by Rosenberg 1992: 64)

2.2 The underpinning: stage 1

After having rebutted prevailing methodologies (that is those reminiscent of logical positivism and Popper, as including — in Hausman's view — Friedman's) Hausman goes on to present, in chapter 12 of the book, his own methodological proposal. The basis for this (stage 1) is laid out in chapter 8, which is an interpretation of J.S. Mill's methodology: 'the deductive, or a priori method, as it may have been conceived by Mill to apply to economics' which consists of in outline four steps:

1. **Borrow** proven (ceteris paribus) laws concerning the operation of relevant causal factors.
2. **Deduce** from these laws and statements of initial conditions, simplifications, etc., predictions concerning relevant phenomena.

3. Test the predictions.

4. If the predictions are correct, then regard the whole amalgam as confirmed. If the predictions are not correct, then judge (a) whether there is any mistake in the deduction, (b) what sort of interferences occurred, (c) how central the borrowed laws are (how major the causal factors they identify are, and whether the set of borrowed laws should be expanded or contracted).

This method should be called the inexact method a priori, because the true deductive method relies only on facts and causes, not on simplifications, and one is supposed to include all the causes. The inexact deductive method, to which economics is condemned, cheats and omits significant causal factors. ... [This inexact method a priori also] differs from the hypothetico-deductive method. The differences are in step 1, where one begins with proven (but inexact) laws rather than mere hypotheses to be tested, and in step 4. Since the laws are already established, they are not open to question in the judgement step.' (Hausman 1992: 147-48)

Hausman is aware that his 'ceteris paribus' interpretation (step 1) may not be altogether faithful to Mill (p. 128), whence he adds here 'as it may have been conceived by Mill' (see further section 3 below).

His reasoning in chapter 12 is rather quirky. Let us look, he says, at the (neoclassical) economic practice.

'Since that practice appears largely to conform to the inexact method a priori ... its appraisal seems to turn on the appraisal of the inexact method a priori.' (p. 206)

In a footnote he adds:

'I have no systematic argument in support of this assertion [i.e. that the practice largely conforms to the method a priori] ... In a decade of presenting this view to economists, few have denied that economics appears to conform to Mill’s deductive method. [The latter is what he calls the method a priori.]'

Then after having listed objections to the method we hear:

'Although the methodological rules of the method a priori ... cannot be defended, I shall nevertheless defend the existing practices of theory assessment among economists.' (p. 206)

This is a bit dazzling. Neoclassical economists know that their practice does not conform to their official methodology (in the mode of Popper-Lakatos methodology). When asked they do not deny that, instead, their actual method appears to conform to 'the method a priori' (i.e. Hausman’s interpretation of Mill’s). However, this method cannot be defended. Nevertheless, the method adopted by these economists unknowingly (i.e. Hausman’s modification of his interpretation of Mill — and as against what they believe they are doing — or at least what they do not deny believing), can be defended (as Hausman is going to show us — see 'stage 2').
2.3 The underpinning: stage 2

On page 222 of chapter 12 Hausman suggests that with a further modification of steps 1 and 4 of the "inexact method a priori" we have a fair description of the neoclassical economics methodological practice which he calls 'the economists' deductive method' (so as to facilitate the comparison with the earlier interpretation of Mill, I put the modifications in small capitals and strike out the deleted phrases - the resulting text is Hausman's — cf. p. 222):

1. *Borrow proven (ceteris paribus) laws FORMULATE CREDIBLE (CETERIS PARIBUS) AND PRAGMATICALLY CONVENIENT GENERALIZATIONS concerning the operation of relevant causal factors.
2. *Deduce from these laws GENERALIZATIONS and statements of initial conditions, simplifications, etc., predictions concerning relevant phenomena.
4. *If the predictions are correct, then regard the whole amalgam as confirmed. If the predictions are not correct, then judge (a) whether there is any mistake in the deduction, (b) what sort of interference occurred, (c) how central the borrowed laws are (how major the causal factors they identify are), and whether the set of borrowed laws should be expanded or contracted COMPARE ALTERNATIVE ACCOUNTS OF THE FAILURE ON THE BASIS OF EXPLANATORY SUCCESS, EMPIRICAL PROGRESS, AND PRAGMATIC USEFULNESS.

With this modification Hausman is close to returning to Robbins' kind of approach rather than to J.S. Mill's — note that this is my interpretation and not Hausman's. Only, economists should take more seriously the message that (neoclassical) economics is the science of merely one aspect of social life. Although Marshall said this, and Robbins repeated it, neoclassical economists have almost always ignored it. In fact, they have sometimes taken the scarcity/choice aspect for the whole of the economic sphere — and even for the whole of society — and at other times, recently, just applied the scarcity/choice aspect to whatever sphere of life (the family, the polity). Hausman argues that apart from this ignorance, and the fact that many neoclassical economists go overboard in their commitment to equilibrium theory (p.245), neoclassical economics does fairly well.23

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23. In some notes (1994), which were the basis for a comment by D.M. Hausman on an earlier version of the current article, Hausman writes:
- 'There is little justification for the presumption that neoclassical economists must be doing their business correctly.'
- 'A good working assumption is that neoclassical economists are not stupid and that even when they make methodological mistakes, there are probably some good reasons for the mistakes.'
- 'In contrast to what Reuten's criticism suggests, I believe that there is little good reason to believe neoclassical economics, because the data against which economic theories are tested is so bad. There is some reason to believe neoclassical economics, for some of the basis "laws" are plausible, but there isn't much. In particular

(continued...)
Indeed if there is something wrong with economics then it is this commitment to equilibrium theory, not some sort of methodological misconduct — so argues Hausman. Equilibrium theory lies at the heart of neoclassical theoretical economics. 'Why believe equilibrium theory?' (this is the title of section 12.2 of the book):

'Economists do not regard the 'laws' of equilibrium theory as proven or obvious truths.[24] ... Why then do economists show such apparent confidence in these behavioral postulates? Why do they cling to them in the face of apparent disconfirmations? ... everyday experience and introspection are sufficient to establish that some of these laws, such as diminishing marginal rates of substitution and diminishing returns, are reasonable approximations. Without qualifications and a margin of error, they are false, but, with these, they seem true; and economists have good reason to be committed to them.

Furthermore, each of the laws of equilibrium theory possesses pragmatic virtues ... Constant returns to scale figures in many economic theories for essentially mathematical reasons, and because it is hoped that its falsity does not do much harm.

Claims such as consumerism and profit maximization are not such good approximations to the truth ... [but there] is a great deal of truth to them, and their virtues in permitting determinate mathematical formulations are considerable. ... [The theory should be more accurate.] But the accuracy would be purchased at the costs of simplicity, and such complications could destroy the normative force equilibrium theory has ...

So one finds a combined empirica! and pragmatic basis for refusing to regard the basic propositions of equilibrium theory as disconfirmed.' (Hausman 1992: 209-210; the second italics are mine)

What exactly is the empirica! basis that Hausman alludes to here? (We had disconfirmations plus Robbinsian everyday experience and introspection.) It seems that the basis is merely pragmatic — as well as normative (see below).

23. (...continued)

(i) I do not think the neoclassical economics is well-confirmed, and I don't think I ever said anything to the contrary.

(ii) I do not think that much confidence in neoclassical economics is justified.

(iii) Although I didn't talk much about other theories in the book, I think that the same negative conclusions apply to the alternatives to neoclassical economics.'

I thought that in fairness to D.M. Hausman I should quote these lines. I leave it to the reader to decide if Hausman’s opinion here (1994) is different from Hausman (1992).

24. Throughout the book Hausman puts 'laws' in inverted commas. Of course most neoclassical economists would argue that their postulates are 'mere' assumptions. On page 139 Hausman rightfully argues that this is invalid: 'Theorists use basic economic 'laws' to try to explain economic phenomena. They cannot regard them as mere assumptions, but must take them as expressing some truth, however rough ... Otherwise their attempts to use them to explain economic phenomena would be incomprehensible. So at some point, with respect to some domains, economists must construe the assumptions of the basis equilibrium models as qualified lawlike assertions.'
So what do we have in the quotation above? The neoclassical microeconomic laws have been *disconfirmed*, or at least apparently. Economists do not regard them as *truths*. Some are blatantly wrong (it is hoped that this does not do much harm), some provide reasonable approximations (as such the latter seem true), some are not good approximations (nevertheless they contain a great deal of truth). The virtue of these laws is that they are simple and have normative force. 'So one finds a ... basis for refusing to regard ... equilibrium theory as *disconfirmed*.' (This 'so ... refusing' is what would distinguish his account from a falsificationist.)

Hausman's account is clearly a defence of the neoclassical economics practice (see also the quotations in the next two sub-sections). Nevertheless he seems to differentiate between a 'not unjustifiable dogmatism' (above) and another brand of dogmatism — it is however hard to see how we can discriminate between the two:

'Although not necessarily unjustifiably dogmatic, there is a serious risk that economists become so entranced by their models that they overlook anomalies and are unwilling to consider alternatives. ... In my view, the dogmatism of economists, such as it is, lies in an exaggerated commitment to equilibrium theory and to the theoretical strategy underlying it, not in a mistaken view of theory appraisal.' (Hausman 1992: 210-11)

This 'overlooking' and 'unwillingness' is not only a risk, but also what has been happening for a century now. Anomalies are overlooked and alternatives are not considered. How might this ever change if it were not for reasons of theory appraisal? If there is nothing wrong with theories being either empty or false, if anomalies have no apparent consequences for the theory, then we seem to be stuck. Obviously one should have a criterion for calling a stance dogmatic — if 'the economist's deductive methodology' is a sound and non-dogmatic one, then how could obeying it be dogmatic? Or is this methodology not so sound after all?

2.4 Neoclassical economics as normative theory

Hausman in his methodological account of neoclassical micro theory seems satisfied that neoclassicism indeed is normative. However, he tries to persuade his readers of the reasonableness of the neoclassical normative stance first via a category mistake, second by a queer view of explanations and third by a praxeological argument. (Note that I have no objection to a normative approach as long as it is (a) not mixed up with (ungrounded) positive claims and (b) not conceived of as a substitute for positive theory. This is of course key to all of the discussion in this paper: the positive theory is ill-founded or lacking altogether.)

Let us get the category mistake out of the way first. It is well known that neoclassical economics labels the individual that it puts on the stage as 'rational'. When this individual engages in economic behavior (as in consumer theory) we have the neoclassical
economics' rational economic man, i.e. the self-centered, egoistic individual. Neoclassical economics cannot claim the property rights of the term 'rational' for its description of economic behavior. Others may well of course claim the term rational for a different individual, e.g. a non-self-centered, altruistic and caring individual who is engaged in relations of production and consumption. But that is not the point. The point is that Hausman equates the 'rationality' of economic man with a notion of 'rationality' in some sense of truth or correctness of theories about actual individuals:

'The fact that utility theory is a theory of rationality seems to provide some grounds to believe that it is a correct theory of how people actually choose.' (Hausman 1992: 218)

This text is immediately followed by the following quote, which brings us to my second point, Hausman's unsound view of 'explanations':

'Furthermore the fact that utility theory is a theory of rationality may provide pragmatic reasons not to give it up and to accommodate anomalies via qualifications or interferences. First, ... Explanations in economics justify as well as explain, and they consequently depend on the same factors that economic agents focus on and find of interest. A quite different sort of explanation might be more successful empirically, but the costs of severing the links between economics and 'folk psychology' and the concern of economic agents are not trivial ...' (Hausman 1992: 218)

Explanations in economics justify as well as explain? What about, for example, a theory explaining exploitation, or a theory explaining unemployment or discrimination or pollution? Even if, generally speaking, neoclassical economics would see it this way (I doubt it) this would be a very idiosyncratic view of explanation, not shared by other traditions in economic theory, nor, I should think, by other social sciences or by the natural sciences — at least not nowadays.

25. Even if a formal separation is made between the rational individual of utility theory (which should be rather open-minded as to its objectives — leaving perhaps open the possibility of having emotions?), it is always the case that when this individual is the subject of neoclassical economic theory, then it is self-centered, greedy, etc., thence 'rational economic man'. For neoclassical theory then, altruism, etc. is either non-rational or non-economic. It is non-rational in case no formal separation is made between utility theory and economic applications of utility theory. It is non-economic for those who make this separation.

In the former case some neoclassical economists now protest. They hold that altruism and care may be put in one's utility function: if you enjoy being a caring individual then that is your preference, and thus you are neoclassical-rational. Of course at this point the neoclassical-rational economic man becomes empty (and Popper is right to lay his finger on empty statements). For then any individual is always a neoclassical-rational individual. Individuals do whatever they do.

In the latter case (i.e. separation) the theory becomes equally empirically empty, because here a refutation of 'rational neoclassical-economic man' will be said to belong to a non-economic domain by definition. (Note that in this variant of the theory the 'neoclassical-economic study' of non-economic domains, such as in public choice theory or the economic theory of the household, will be rather murky in this respect.)

26. Hausman describes a variant of neoclassical economics (chs. 1-2 of his book) which strictly separates utility theory from its economic application (see the previous note). Notice however that in this quote he indeed links utility and rationality to economics and economic agents.
Third, the praxeological argument:

'But there is an even more striking pragmatic argument in favor of preserving a theory of rationality as one's basic theory of actual choices. For one might reasonably hold that, when people behave irrationally, the theorist's response should not be to revise utility theory, but to encourage agents to change their behavior. ... This educative function of expected utility theory provides a good pragmatic reason for accepting it ... I am not proposing that theorists pretend that people behave according to expected utility theory even when they do not do so. But the educative function of a theory of choice gives one reason to describe the divergences as lapses or interferences and to retain [it] ... rather than opting for a non-normative alternative.' (Hausman 1992: 218-19)

The argument is indeed striking. We might cast it in Hausman's frame for 'the economists's deductive method':

— Individuals are neoclassical-rational subjects (= step 1*: obeying the law, the generalization, the postulate);
— or, individuals are rational neoclassical-economic subjects (presumably a simplification = step 2*);
— when people behave in fact irrationally (test = step 3*),
— then we may try to change their behavior so that they fit neoclassical rationality (= step 4*? try to change the 'data'?).

And this function of the theory provides 'reason for accepting it'.

2.5 'Little can be learned'

It is really a closed shop when Hausman asserts later on:

'The alternative [to neoclassical equilibrium theory] might be better confirmed — in better accordance with the facts — so economists might be driven from their allegiance to equilibrium theory. But, given the difficulties involved in testing and confirmation in economics, this is an unlikely prospect.' (Hausman 1992: 248)

And on the same page: 'most economists neither seek alternative theories nor believe that they can be found'!

Finally, alternative theories will always be swept away because:

'Unfortunately, owing to poor data ... little can be learned about which theories are better confirmed. Given [...] the initial credibility of the basic behavioral postulates of economics, it is rational to remain committed to them in the face of apparent disconfirmations.' (Hausman 1992: 253)

Nevertheless Hausman seems aware that this situation is rather dubious. He pleads for better data gathering as well as for extending the field of economic theorizing.

27. Analogously, because Freudian psychoanalysis claims that it can heal people by moulding them into its model, is this a reason for accepting it?
'One must be prepared to consider alternative kinds of theorizing, or else there is little point to such data gathering.' (p. 253)

This, however, sounds rather hollow and unconvincing in the face of his account of neoclassical economics practice in general, which goes as follows: it is 'rational' to stick to the neoclassical economics equilibrium theory because that is 'rational'.

2.6 A descriptive cum normative methodology

The mainstream Popper-Lakatos methodology is of course not without problems. Hausman is quite right to address the issue. It makes sense to search back in history, as he does, to find inspiration so as to develop alternatives which are suitable for the social sciences and, in particular, economics. Mill then is an important source of inspiration. The problem for Hausman's project, it seems to me, is that he has tried to do two things at the same time — descriptive and normative methodology — without realizing that that in itself requires a particular (meta-)methodology. Hausman starts from some norm (Mill) and then gradually adapts that norm until it more or less fits the description he is aiming at. In fact this way he winds up doing descriptive methodology — though of course not theoretically uninformed. We are theorizing all the time, and Hausman tells us the story of his theory-fact vindication — viewed this way I was in this section perhaps too harsh in my judgement of his effort. However, this insight does not alter the fact that what Hausman does is in fact descriptive. Moreover, he has an opinion on the behavior of the investigated subject — on which he is explicit. As a philosopher of science he approves of the methodical behavior of his subject (neoclassical economics). But that by itself (Hausman's approval) need not be convincing. I happen to have another opinion — so we would need persuasive arguments to sort out whose position is most convincing. In fact Hausman provides only negative arguments — those concerning the Popper-Lakatos standard approach. Hausman (1992) then in fact sanctions a body of theory that shelters itself from empirical test. From this perspective he is not far removed from Robbins, Friedman and even McCloskey — Hausman just lacks the arguments for a position to the contrary.

3. Mill's methodology on a different road

Nevertheless, Hausman might have had the beginnings of a demarcative methodology in his hands had he refrained from giving a 'twist' to Mill's methodology. In this section I provide some reasons why putting Mill, normatively, on a different road might be interesting. This is different from Hausman's normative cum descriptive intention.28

28. The latter is not an impossible project and the Hegel-Marx method of immanent critique (rather than external criticism) offers sources of inspiration (for the most part neglected by methodologists).

(continued...)
My main disagreements with Hausman's *Mill-twist* (cf. section 2.2 above) concern: (1) the notion of 'inexactness' or the notion of a 'tendency' (p. 128); (2) the representation of 'step 1' — in particular the fact that inductive and other evidential resources are missing. I also have disagreements with the 're-twist' or *modification* to what he calls 'the economists's deductive method' (section 2.3 above), something which I would prefer to call 'the axiomatic method'. These disagreements concern: (3) insufficient specification of the deductions of 'step 2*'; (4) the adaptation in 'step 4*' — resulting in lack of feedback in case of disconfirmations.

3.1 The notion of a tendency: 'inexactness' and 'ceteris paribus'? 

I will start by reformulating in my own words what I take to be the gist of Mill's view on tendencies (and their formulation in laws) in his 1836 essay on method (reprinted in his 1844 collection of essays). In the essay, Mill seems to make no distinction in ontological status between the natural and the social to the extent that in both domains certain 'powers' or 'forces' are operative that produce 'results' (phenomenal). Some results are more complicated than others in that several different powers rather than one or a few different powers are operative so as to produce a result. If we had a full picture of the world we would have for the sum of all powers (*P*) and results (*R*):

\[ P(1, ...n) \rightarrow R(1, ...n) \]

For each result taken in isolation (e.g. the result numbered R(146)) we would know its cause or causes, for example, \[ P(2,7,286) \rightarrow R(146). \]

Now step aside from this hypothetical case of full knowledge. We hope to reach such an (ideal) full picture via an 'upwards' process of induction and a 'downwards' process of deduction (Mill 1836: 324-25). Now suppose we already have grounds to know that \( P(i) \) is an operative causal power (borrowed from other sciences or ascertained via induction from within political economy), though we do not know (all of) 'its' results. We even do not know if there exists a result produced by just this one cause. Then let us take \( P(i) \) in isolation (because we do not have a full picture and we wish to study causes one at a time — 1836: 322). Now suppose we have information about the working of \( P(i) \). In this case we may have grounds to argue that \( P(i) \), in isolation, *tends* to produce or *tendentially* produces an Effect. (j). 29

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28. (...)continued
Here the starting point, however, is the descriptive rather than the normative. Thus Hegel and Marx start at the other end *vis à vis* Hausman (of course not in theoretical void) and then end up with norms. The point of application for such an immanent critique would be the contradictions in — in this case — the neoclassical economics practice and *their* ideals. In fact Hausman provides plenty of the bare material for this approach — sometimes cast in such terms as schizophrenia and contradiction. (For a general account of this method see Benhabib 1986: chs. 1 and 4.1.)

29. I am making a strict distinction between a result and an effect for reasons that will become clear later on. Mill does not seem to make this strict distinction (cf. 1836: 337-78). Note that both results and effects are occurrences even if we may not be able to perceive the effect (thus there may not be an *immediate* empirical counterpart for an effect).
P(q) \rightarrow E(j)

[where \rightarrow stands for tendency]

Note that there is no principal difference between the latter case of isolation and the former full picture case. In the case of result R(146) we had three different causes P2, P7, P286 each producing a tendency towards some effect, perhaps counteracting each other, the end-effect of which is result R(146).

Now back to my disagreements with Hausman's interpretations of Mill. Let us get the (minor) issue of the 'inexactness' out of the way first (cf. the quotation from page 147-48 in section 2.2 above). I merely think that the term 'inexact' is confusing because it suggests a principal difference in this respect between the natural and the social sciences. There is none. And Hausman agrees when he writes 'there are no exact sciences, although in some cases and for some purposes the inexactness of a science might be negligible' (1992: 126).

The next issue is more serious. Hausman considers the interpretation of 'inexact laws' or tendencies as 'modal claims' faithful to Mill (cf. de Marchi 1986). I cannot, however, agree with his formulation here:

'Inexact laws make counterfactual assertions about how things would be in the absence of interferences.' (Hausman 1992: 128)

I think the reference to counterfactuals is besides the point and confuses the whole issue. If we have laid our hands on a true tendency or a true amalgam of tendencies, then an effect or a result, as defined above, are each occurrences. The one is not more or less factual than the other! Quite a different issue is one of if and how we can perceive effects (especially if we lack the possibility of isolating powers in an experimental situation). This indeed is the problem, that in economics as a social science we can most of the time perceive effects (if at all) only in results. But surely if we could isolate a power in an experimental situation — as one seems to be able to do in physics and chemistry — then that situation is not a counterfactual. In an experimental situation one indeed shows, and next asserts, how things in non-experimental situations 'would be in the absence of interferences'. Mill indeed affirms:

'That which is true in the abstract, is always true in the concrete with proper allowances.' (Mill 1836: 326; cited by Hausman 1992: 130 cf. 131)

30. In fact the full picture case should have been written as: P(1, ...n) \rightarrow R(1, ...n), (where \rightarrow stands for tendency).

31. This has also been argued in the same connection very powerfully by Bhaskar 1979.

32. In this connection I do not understand what Hausman means when he writes (p.128): 'Economics employs inexact laws and thus inexact theories. ... They are not literally true ...' I would think they are either true or false (even if we take this as provisional and if we had not yet made up our minds). The tendential character of a law does not make it more or less true.

33. The same goes for 'necessities' in dialectical logic. They cannot be perceived in isolation from their contingent make-up (cf. Reuten & Williams 1989: 30-31).
Hausman comments:

'Although Mill finds this modal view consistent with his empiricism ... I have my doubts, for the confirmation of claims about possible worlds is puzzling.'

Again, this possible world is not a counterfactual; it is not a possibility in the sense of an assumption by which anything is possible (as in many economic models). It is what, in dialectical jargon, one would call 'a determinate possibility'.

Hausman's Mill-twist is executed when he states (immediately following the quotation above):

'In any event, since the intuitions that support the modal view can for the most part be accommodated by the ceteris paribus qualification view, I shall focus my attention on it.' (Hausman 1992: 131)

This view reads:

'Inexact laws are qualified with vague ceteris paribus clauses.' (Hausman 1992: 128)

Although Hausman elaborates on ceteris paribus clauses (section 8.2) — i.e. the neoclassical flat way of theorizing — his view and discussion categorically do not fit what Mill understands by tendencies. I insist on this point, because I think the latter may prove fruitful for the development of an explanatory economics (though the former fits the current practice of neoclassical economics). Mill surely is not arguing that there are no other tendencies at work apart from the one(s) that an investigator is taking under consideration. So we have a set of tendencies acting. The question is to what extent they interact so as to produce a 'result'. However, what is the meaning of the expression 'bla bla (cet. par.)'? Surely not: 'This tendency produces x, and we assume other things being equal (i.e. cet. par.).' Because 'other things being equal' would mean that the other tendencies are indeed in operation. No. The cet. par. magic is constituted precisely by our supposing that in the hypothetical situation other (counter) tendencies are not operative. But this is not what Mill is aiming at! Surely he has a particular view as to which tendencies dominate in the economic domain (a view with which one can agree or disagree — this is besides the point), however, that is a factual statement. And the factual statement is in need of what he calls verification (see section 3.3). This has indeed, pace Hausman, much more empirical meaning than a cet. par. statement.

Finally, as a related issue, there is some risk that if neoclassically trained economists read the notion 'tendency' in connection with 'ceteris paribus' that — in an empiricistic vain — they will interpret a tendency merely as a trend.

36. For some reason the distinction has been retained mainly in marxian theorizing only (cf. Reuten 1991: 81 for a summary statement). I have no explanation for this. I wonder when and why it
3.2 Laws and inductions: proofs and evidence

Hausman's representation of Mill's method step 1 reads: 'Borrow proven (ceteris paribus) laws concerning the operation of relevant causal factors.' (Hausman p. 147; cf. section 2.2 above). Firstly, on the basis of the argument in section 3.1, I propose to strike out the ceteris paribus. Secondly, here to 'borrow' is half the issue. Mill indeed refers to laws adopted from other sciences. However, he also refers to inductive procedures from within political economy to arrive at these. Hausman is well aware of this:

'I have left out of the summary formulation the 'proving' of the laws concerning relevant causal factors, which Mill takes to be the first step of the deductive method, because I want to focus on the tasks of economists, who are concerned with applying psychological and technical laws, not with establishing them.' (Hausman 1992: 148)

I am perplexed by this statement. (a) The part omitted is crucial to Mill's method. (b) Are the laws that neoclassicals 'borrow' indeed proven? As for the 'psychological' laws applied in economics: have these indeed been proven in psychology? (cf. Hodgson 1988: 83-86, 106-111). (c) Why are particularly these 'the tasks of economists'? Why is the establishing of economic laws excluded? Is not what is wrong with economists that they do not establish laws, but dream them up instead? Indeed as we have seen the 'neoclassical modification' of Mill-twisted reads:

1. * Borrow proven (ceteris paribus) laws **FORMULATE CREDIBLE (CETERIS PARIBUS) AND PRAGMATICALLY CONVENIENT GENERALIZATIONS** concerning the operation of relevant causal factors.

Of course descriptively this is fine. But why should one have to believe in these generalizations? Again, Mill untwisted should be fruitful for economics.

3.3 Falsifiability

As we have seen, steps 2 and 3 in Hausman's summary formulation of Mill's method (slightly modified for neoclassical economics) read:

2. * Deduce from these laws **GENERALIZATIONS** and statements of initial conditions, simplifications, etc., predictions concerning relevant phenomena.


36. (...continued)

disappeared from the neoclassical tradition. It did not happen, so it seems, with J.N. Keynes. With him we do find the joint notions of tendency and ceteris paribus:

'As a matter of fact, in the instances that actually occur of the operation of a given cause, counteracting causes sometimes will and sometimes will not be present; and, therefore, laws of causation are to be regarded as statements of tendencies only. (...) the pure theory assumes the operation of forces under artificially simplified conditions (...) laws are statements of tendencies only, and are therefore usually subject to the qualifying condition that other things are equal; and that, in the second place, many of its conclusions depend upon the realisation of certain positive conditions, which are not as a matter of fact always realised.' (Keynes, 1891: 86, 89-90)

Or did it? Is it perhaps the joining of the two notions that started us on this track?
Mill quite rightly recognizes that prediction is problematical in political economy (e.g. 1836: 323). Nevertheless he states:

'We cannot, therefore, too carefully endeavour to verify our theory, by comparing, in the particular cases to which we have access, the results which it would have led us to predict, with the most trustworthy accounts we can obtain of those which have been actually realized.' (Mill 1836: 332)

The spirit of this and similar formulations in his essay is indeed 'careful' verification. It is not ludicrous, I believe, to suppose that if Mill would have known the distinction, made by Popper a century later, between verifiability and falsifiability, that he would have urged for 'laws' (or 'generalizations' for that matter) to be stated in falsifiable form. That makes sense because it makes them 'non-empty'. This quest makes sense for a normative methodology to guide the economist's practice. Obeying it would make that practice more trustworthy. This is not a plea for falsificationism. No. All of Mill's insights indicate that rigid falsificationism is not fruitful. However, if we want to learn from comparison with the empirical, there will have to be confrontations and not just prepared confirmations. This takes us to the last step — and my final disagreement with the working out of Hausman's project.

3.4 Judge! — expand and/or contract laws: creative theorizing

Towards the end of his essay (p. 336), Mill writes:

'With all the precautions which have been indicated there will still be some danger of falling into partial views; but we shall at least have taken the best securities against it. All that we can do more, is to endeavour to be impartial critics of our own theories, and to free ourselves, as far as we are able, from that reluctance from which few inquirers are altogether exempt, to admit the reality or relevance of any facts which they have not previously either taken into, or left a place open for in, their systems.'

[and so on in the same vein on pages 336 and 337]

Whilst we can find the gist of this critical attitude in Hausman's step 4 summary of Mill, the neoclassical economics modification of step 4 reads quite differently:

4. * If the predictions are correct, then regard the whole amalgam as confirmed. If the predictions are not correct, then judge: (a) whether there is any mistake in the deduction, (b) what sort of interferences occurred, (c) how central the borrowed laws are (how major the causal factors they identify are, and whether the set of borrowed laws should be expanded or contracted COMPARE ALTERNATIVE ACCOUNTS OF THE FAILURE ON THE BASIS OF EXPLANATORY SUCCESS, EMPIRICAL PROGRESS, AND PRAGMATIC USEFULNESS.

What is missing here is the judgement and the feedback from step 4 to step 1. That is indeed crucial for Mill, and I would think, fruitful for the development of economic theory. Of course this is not an easy thing. It requires a scientific attitude. That is a precondition. This might subsequently mean being prepared to augment theories in the face
of disconfirmations (for which they would need to have been falsifiable in the first place). But that may not be enough. The quest, in case of repeated disconfirmations, is for creative theorizing. But that can hardly be called a methodological rule. No. Some scientists are born creative? Perhaps. However within a scientific climate creativity may come to flourish. (Think of the Colander & Klamer interviews of economics graduate students.) As this paper is already abound with citations, I may as well end with one, one somewhat 'modern':

'The growth of normal science, which is linked to the growth of Big Science is likely to prevent, or even destroy, the growth of knowledge, the growth of great science. (...) Superimposed upon this danger is another danger, created by Big Science: its urgent need for scientific technicians. More and more Ph.D. candidates receive a merely technical training, a training in certain techniques of measurement; they are not initiated into the scientific tradition, the critical tradition of questioning, of being tempted and guided by great and apparently insoluble riddles, rather than the solubility of little puzzles.' (K.R. Popper, Reason or Revolution?, quoted by Katouzian 1980: 131)

Summary and Conclusions

The neoclassical economists' practice of theory appraisal is far removed from their official methodology — which is (a Lakatosian variety of) Popper's falsificationism. The empirical underpinning of economic theory is a weak spot, and the method of neoclassical economics has increasingly developed from an 'empirical positivism' into an 'axiomatic positivism' (where 'positivism' is taken broadly to refer to the study of outward appearances of the institutions of the status quo, and 'axiomatic' to a procedure typically encapsulated in those economist's models) (section 1.1). Within the latter approach the theory is largely sheltered from empirical testing. Such a sheltering has been variously defended, and in section 1.2 I briefly reviewed Robbins, Friedman and McCloskey from this perspective.

D.M. Hausman, in a recent methodological study of neoclassical economics (1992), takes a stance far distant from the official Popper-Lakatos methodology and attempts to put the economics methodology on a new road altogether. In developing it, however, he comes close to the gist of Robbins' approach. Hausman provides an insufficient consideration of the empirical stages of theory appraisal (section 2.3, 2.5) and an arbitrary part of the theory assessment runs in praxeological term, apparently divorced from the methodological appraisal (section 2.4)

Hausman's book deserves careful study. Although in the course of my reading I discovered weaknesses, at the same time I became to increasingly appreciate Hausman's
refreshing and thought-provoking approach, it is refreshing that he has taken up economic methodology at a fruitful point in its history: Mill 1836. This is combined with an interesting and scholarly description of the current methodical practice of neoclassical economics.

These two, the methodical practice and Mill 1836, are the main building blocks of the book. I see problems in the way Hausman combines them. The main problem is that he wanders between a normative and a descriptive methodology, without explicitly making up his mind. He does however implicitly make up his mind, because in the end he chooses to take the descriptive side and to adapt a 'normative' (?) methodology to it (section 2.2, 2.3, 2.6). The way he does this is to, first, give a twist to Mill. I am not happy with this Mill-twist which loses much of Mill's perceptive insights on the methodology of social science and economics in particular. Hausman's ceteris paribus interpretation of Mill's notion of tendency misses, I think, the kernel of this notion — it also risks having economists interpret tendencies as trends (section 3.1). I am also concerned that Hausman, at least in his summary statements of Mill's methodology, neglects the stage of empirical underpinning of laws or generalizations (section 3.2). Second, Hausman modifies his Mill-twisted so as to fit it to the description of neoclassical practice. In it Mill, I think, has almost disappeared (sections 3.3 and 3.4).

Science, I believe, is inherently a critical project — in an Enlightenment sense. Philosophy of science and methodology are part of this general project, even if their subject of study is a particular part of society (i.e. science). In turning — in the end — to mere description I think Hausman has risked losing one of his tools. In fact he is confronted with himself in this respect. For some reason Hausman is critical towards neoclassical economists' 'exaggerated commitment to equilibrium theory' as well as to their taking insufficient cognizance of alternative approaches in economics. But why should neoclassicals listen to this? This criticism does not follow from any criterion of theory appraisal, at least not directly, because Hausman argues that the neoclassical way of theory appraisal is all right (p. 211 — cf. section 2.3 above). So what is the tool? What is the foundation of this criticism?

These issues require further discussion. In the meantime, I congratulate Hausman for opening the research agenda of the philosophy and methodology of economics.
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