Journal of Economic Methodology 3:1, 1–38 1996

Two portraits of economics

Uskali Mäki

Abstract This is an assessment of two recent philosophical accounts of the nature of economics, those given in Alexander Rosenberg's *Economics – Mathematical Politics or the Science of Diminishing Returns?* (1992) and in Daniel Hausman's The Inexact and Separate Science of Economics (1992). The focus is on how they portray the predictive capabilities of economics and the links between economic theory and empirical evidence. Some major suggestions of the two books are found wanting in interesting ways. Examples are Rosenberg's explanation of the predictive weakness of economics in terms of its folk psychological roots and his depiction of economics as a branch of political philosophy and applied mathematics; and Hausman's claim that the 'economists' deductive method' is appropriate while 'economics as a separate science' is not.

Keywords: economics, prediction, folk psychology, deductive method, separateness, dogmatism

1 INTRODUCTION

The nature of economics as an academic discipline remains a puzzling issue that invites accounts from practising economists themselves and from a variety of other people, including philosophers of science. The answers to the question of the character of economics are varied, and so are the approaches to answering it. And not only are the answers hotly contested, but so are the approaches, and increasingly so. We not only have rival portraits of an object, we also have rival techniques of painting. It is instructive to scrutinize and compare two recently completed attempts; both painters are philosophers, but they use somewhat different techniques and end up with different pictures.

In the early stages of the current wave in economic methodology, both Alexander Rosenberg and Daniel Hausman published a book on the subject. Rosenberg authored *Microeconomic Laws* (1976). Hausman's study was entitled *Capital, Profits, and Prices. An Essay in the Philosophy of Economics* (1981). Neither book received much attention, even though, relatively speaking, they were of excellent quality. Quite simply, there was hardly a literate audience for them. The times have since then changed. The most recent books by these two authors seem to be widely read and discussed. Even though *Economics – Mathematical Politics or the Science of Diminishing*

 ISSN 1350–178X

Returns? (1992) by Rosenberg and *The Inexact and Separate Science of Economics* (1992a) by Hausman are specialized enough not to reach just any accidental reader, there is an interested and sufficiently large audience around these days to make the two books major attractions in the field.

As the titles of the two books suggest, they offer us visions of the discipline that are both broad and detailed enough to deserve to be called portraits of economics. In his 1976 book, Rosenberg scrutinized some aspects of microeconomics. In his 1981 book, Hausman analysed a few theories of capital and profits. These endeavours had a limited focus, which may have helped in meeting standards of high quality. In contrast, both of the 1992 books claim to provide accounts of *economics*; they pursue portraits of the whole discipline by focusing on what the authors take to be the core parts of it. Portraits can be assessed in many ways, such as whether they are true to their objects, whether they emphasize the right features so as to create a balanced picture, for given purposes, of the essence of the object; and whether they manage to urge the object to engage itself in critical self-reflection.

Two characteristics of my portrayal of these two portraits have to be mentioned at the outset. First, my discussion neglects large parts of the rich materials included in the two books, which I believe can be done without distorting their main thrust. My focus is on some of those parts which appear to be both central to and shared by the two portraits. The major theme to be discussed has to do with the predictive capabilities of economics – estimated to be weak by both Rosenberg and Hausman – and the allegedly weak links between economic theory and empirical evidence. Second, my discussion is purposefully and shamelessly critical. While I am the first to celebrate the insightfulness of both accounts, my reading leads me to point out a number of problems in them; some of these problems are relatively harmless, while some others seem to be serious. While I grant that the number of problems listed may be unnecessarily large (yet it could be made much larger!), I wanted to keep it that way in order to provide an inventory of some of the questions that are left open by the two books. Indeed, I think both books are valuable precisely because they raise important issues and provide an opportunity for criticism, and criticism is what I believe the field needs to make progress; by implication, I take the painting of portraits (of scientific disciplines!) to be a potentially progressive endeavour.

The criticisms are based on a somewhat detailed reading (yet, I wish I could be still more detailed, but the confines of an essay like this do not allow it; details will have to be added on other occasions).[1] This means that, as a present consumer of the two portraits, I now choose to prefer 'realistic' pictures painted with carefully designed arguments to impressionistic and sensation-seeking sketches (this implies nothing about my preferences about portraits at other times or on other subjects). I presume this preference to be shared by the two painters. When two analytically trained philosophers start painting, one expects to have the details in a relatively good shape. On the other hand, portrait painting is an ambitious and risky business; failures do not come as a surprise.

I will first very briefly outline the core components of the main arguments of the two books. Then I will assess some of their elements, both separately and comparatively. I will also make occasional references to Hausman's collection of essays (1992b) which, in addition to covering the same ground as the monograph, also complements it usefully, yet does not help remedy its difficulties. It would have been nice if Rosenberg's relevant essays had been similarly available in one volume. This is notable, since Rosenberg may have decided to be content with rather thin and vulnerable formulations of his arguments in his book because he thinks that they are given more elaborate formulations in his other publications. If this were so, my choice of focus would add to the force of the criticisms against his intentions: I am in the business of assessing the portraits as they were painted in the two 1992 books.

2 THE TWO ARGUMENTS

Intermingled with a large number of themes, there appear to be a primary or dominant set of issues shared and addressed by Hausman and Rosenberg. Neither of them depicts the development of economics as a splendid success story. What they share is a concern about the predictive failures of economics and the apparent immunity to empirical evidence of economic theories. These two interrelated features will also constitute the primary focus of the present essay.

While there seems to be a shared set of issues at such a general level, the two authors differ in their responses, in how to account for economics' predictive failure and immunity to evidence. A significant difference, to be illuminated in the following sections, seems to be that while Rosenberg suggests to trace the problems back to the core ideas of individual choice theory, Hausman chooses to invoke the larger boundary conditions not covered by fundamental theory. This already suggests that Rosenberg's account is the more radical of the two.

If we look at the two projects more closely, a further more specific difference emerges regarding the primary issues they address. Rosenberg's foremost concern is the question of whether economics is a science, and, if it is not, what it is. Hausman's focus is on the question of whether economists are justified in sticking to a set of fundamental propositions about agents and the economy or whether in doing so economists exhibit unjustified dogmatism. These themes are interrelated, thus it is not surprising that the authors have something to contribute to one another's projects.

Rosenberg appears more radical also in relation to these more specific issues, arguing that economics – as we know it – is not and cannot be an empirical science at all, while Hausman describes economics as a science, albeit an inexact one. Hausman believes economics employs the appropriate scientific method, but is unfortunate with having to deal with a subject matter whose complexities do not allow for easy successes: 'scientific methods have not worked very well for economists and . . . they are unlikely to work well' (1992b: 99–100).

Against the background of a shared pessimism about economics, the orientations of the two books appear quite different. Rosenberg appears as an uncompromising critic of economics, while Hausman appears to make an attempt to articulate a balanced critical defence of economics. A more accurate statement would be that Rosenberg sees no way of defending and improving economics as an empirical science, suggesting that it be treated as being something else, while Hausman proposes a set of prescriptions that he thinks would make economics a better science.[2] In positioning myself in relation to these suggestions, I am finding myself criticizing both Rosenberg's critique of economics and Hausman's defence of it. This leaves me neutral with regard to economics, but not about the two portraits.

Rosenberg's message is dramatic: economics is not, and, given its current conceptual commitments, cannot be, a science. The core structure of the overall argument of the book seems to be something of the following sort:

[R1] Science is characterized by predictive progress.
[R2] Economics fails to meet criterion [R1].
[R3] The reason for [R2] is the dependence of economics on folk psychology.
[R4] Not being an empirical science, economics is a form of mathematical politics.

As is to be expected, each of these elements can be questioned. Instead of arguing directly against [R1]–[R4], we mostly approach them by asking what grounds Rosenberg himself provides in their support. With little effort, this approach reveals major deficiencies in the argument: none of the elements [R1]–[R4] appears to be appropriately supported. What is worse, the conceptual constituents of the argument remain unclear and imprecise to the extent that in order to critically assess the argument itself one first has to do some conceptual groundwork – and perhaps some conceptual guesswork.

Hausman's argument has many threads to it, but the following seem to be the core ideas:

[H1] Economics is an inexact science.
[H2] Economics employs a deductive method of theory appraisal.
[H3] The deductive method is the appropriate method in inexact sciences, but it does not serve economics perfectly due to the uncontrollable complexity of its subject matter.
[H4] Economics is a separate science.

[H5] The commitment to economics as a separate science has led economists to unjustified dogmatism.

These ideas are not free from problems either. Again, there are some aspects which are unclear in Hausman's formulations, thus certain refinements are needed before attempts to justify the basic claims can be considered. Fortunately, these refinements seem to be easier to come by than in Rosenberg's case – even though a major problem appears to remain, undermining the book's core claim about the separateness of economics.

I have formulated the two arguments using the somewhat different vocabularies of the two authors. As our commentary proceeds, the comparability of the arguments begins to unfold.

3 WHAT IS SCIENCE?

Rosenberg holds a principle that seems to give a necessary condition for something to constitute a science:

[R1] Science is characterized by predictive progress.

Rosenberg is not entirely unambiguous about the exact import of this principle. Here is the most precise specification we can find: he says that '[f]or the purposes of this book' he adopts the stipulation that 'a scientific discipline should be expected to show a long-term pattern of improvements in the proportion of correct predictions and their precision' (p. 18).[3] One problem, here and elsewhere, is that the very concept of prediction remains undefined in detailed terms. Later on I will point out some consequences of this omission. The second problem is intrinsic to Rosenberg's set of two criteria: it is not clear how to disentangle 'the two predictive criteria of proportion of hits to misses and precision' (p. 18). This is because a 'hit' or 'correct prediction' has to be defined in terms of precision. Thirdly, there is a problem about what Rosenberg's set of two criteria omits: the reader may find it somewhat surprising that considerations of *scope* are not incorporated into Rosenberg's criteria of predictive progress. Usually, some idea of an expanding scope is taken as one indication of predictive progress. It is notable that this criterion might throw economics in a much more positive light than Rosenberg's twin criteria. Finally, Rosenberg occasionally forgets that he has posited predictive *progress* rather than predictive *success* as the criterion of scientificity. In some important cases, these two criteria do not provide identical implications. Sometimes, he goes even further and tends to obscure matters by equating the weaker feature of *being supported by empirical evidence* with the stronger features of being predictively successful and progressive; that this should be treated as a separate attribute is suggested by passages like, 'not just successful testing but also predictive success' (p. 18).

Yet, let us suppose the following is what Rosenberg means, using his own words:

[R1*] Science is characterized by a long-term pattern of improvements in the proportion of correct predictions and their precision.

[R1*], as well as any other view of how science is to be defined, is challenged by many, which is an indication of major uncertainty on this question. Therefore, one would expect Rosenberg to provide good grounds for accepting [R1*]. But his grounds do not seem to be very strong. As an argument for [R1*], he repudiates the alternative view that 'understanding' is the goal and criterion of social science by claiming that 'without the empirical evidence, the internal feeling of understanding is no mark of knowledge at all' (p. 19), but this leaves many questions open. It is not clear that all relevant notions of understanding are dependent on 'internal feelings' of the kind Rosenberg seems to have in mind; it is not obvious that the evidentiality of empirical evidence is completely free from 'internal feelings' of some other kind; and it is not obvious that predictive success or progress exhaust all there is to successful appeal to 'empirical evidence'.

As another argument for [R1*], he says (first employing the weaker notion of predictive *success*) that 'the proximate goal of predictive success is the only one worth aiming for by a discipline that hopes to be relevant to the guidance of public and private policy' (p. 20). This is not convincing; it certainly is the case that, for example, many forms of understanding are 'relevant to the guidance of policy' (provided, of course, that 'guidance' be understood broadly enough and that the fact be ignored that many forms of interpretive understanding do indicate predictive success of some sort). It is even more questionable to make the stronger claim that 'long-term improvement in predictive success [i.e. predictive *progress*] is a necessary accomplishment of any discipline that claims to provide knowledge, and especially to provide guidance to policy' (p. 56). This does not appear convincing at all. Contrary to what Rosenberg claims, it is not difficult to conceive of a *predictively successful but non-progressive* discipline that would be useful for policy.[4]

Rosenberg's most important argument for [R1*] is based on a presumption about the majority opinion amongst the economics profession itself. He says that 'it seems evident that if forced to, most neoclassical economists would endorse' something like [R1] (p. 17) and that he has managed to identify 'an epistemology widely held among economists that I share' (p. 19). It is notable that no evidence is given to support this claim. There are several problems with this.[5]

To begin, Rosenberg's picture of the motives and commitments of economists may be too ahistorical. This is because major changes in the relevant aspects of the self-image of economics may have occurred. Such images are difficult to measure, and any suggested measurements are bound to be controversial; yet it has been argued that the image of economics as a predictive, policy-relevant science was more popular before about 1950 than after it. As Hutchison suggests, '[s]hifts in objectives and motives have been from one kind of mix, in which policy relevance or policy guidance has long and traditionally dominated, towards another kind of mix in which some kind of . . . academic criteria have come to occupy an important position, at any rate in some prominent and well-publicized academic circles' (Hutchison 1992: 30). These academic criteria, Hutchison claims, have to do more with formal elegance than predictive power. Yet, economists subscribing to such non-empirical criteria are no less convinced that they are engaged in scientific work. If nothing else, at least this claim suggests that Rosenberg's argument needs to be more informed by a historical perspective.

Secondly, Rosenberg ignores the fact that economics has long been divided into two major orientations in relation to this issue. There are those who regard economics as a *policy-oriented engineering science*, heavily dependent on predictive capabilities. This line is represented by people like Friedman and Klein, to mention two Nobel Prize winners. And there is another tradition of viewing economics as an *explanatory endeavour* that helps us resolve the paradox of social order by referring to the institutional structure of market society. Other Nobel Laureates like Hayek and Buchanan do not seem to subscribe to [R1*], yet find no compelling reason to deny the scientificity of economics – nor its policy-relevance!

The third problem is that the attribution of [R1*] to economists ignores the heterogeneity of types of activity even within the same 'school' or 'paradigm'. As an example, consider the simple distinction between *'theory'* and *'applied work'*. Even if it were the case that many applied econometricians subscribe to [R1*], it is not at all obvious that 'theorists' would typically endorse it. Many or even most of the 'theory' people might view as theoretical model-building directed towards 'providing insight into' or 'illuminating' a given type of empirical issue or the like. Such aims – even though not very clearly articulated – are not easily reducible to predictive progress even though they would seem to have an empirical character.

The fourth problem is that it is not evident that even all those economists who may endorse predictive aims would subscribe to [R1*]. Many of them may think highly of predictive *success*, but it is less obvious that all of them would aim at predictive *progress* of the sort envisaged by Rosenberg. Given that economic reality itself is in some serious sense structurally unstable, many economists may well be content with the aim of more or less constant predictive success and think of predictive progress in the sense of 'a long-term pattern of improvements in the proportion of correct predictions and their precision' as utopian. On the other hand, it also appears that some work in economics, especially in applied econometrics, is animated by an idea of predictive progress of some sort. But Rosenberg does not discuss this sort of research in his book; his focus is on basic abstract micro theory.

Finally, I cannot help pointing out two deviations from [R1*] that we can find in Rosenberg's book itself. In arguing against the progressiveness of the neo-Walrasian programme, he employs a criterion that deviates from [R1*]. In this context, we are told that it is not sufficient to expand the range of predictions thus potentially improving the proportion of hits to misses. It is now also required that the predictive improvements be due to 'significant changes in the theory itself', namely they have to be 'the result of adding new explanatory variables to the theory or of measuring its fundamental parameters more accurately' (p. 107). This additional idea that here appears just in passing, did not appear in the original articulation of the notion of predictive progress. The reader may taste a flavour of *ad hocness* here. On the other hand, this additional requirement is informative as it illuminates an aspect of Rosenberg's approach to diagnosing economics: his focus is on the core ideas of economic theory, to the exclusion of the functioning of boundary conditions. Therefore, this element should perhaps have been an integral part of his notion of science-as-predictive (and thus should have been incorporated into [R1*] as a further constraint).

The other, and more fundamental, tension is created by the fact that elsewhere in the book Rosenberg expresses skepticism about the sort of posit that [R1*] represents. He says that '[g]iven the unsettled character of the philosophy of science, it is hard to know exactly what a science is' (p. 228), and that 'the parties to this dispute do not share a common criterion of predictive power or empirical confirmation, because there is none' (p. 98). He also says that 'philosophers of science have given up trying to make sense of [principles of predictivist confirmation] in a way that will shed any light on the problem of demarcation' between science and non-science (p. 100). In the context of these statements (such as in arguing against Lakatosian economic methodology), Rosenberg endorses the skepticism, thus placing himself within the mainstream of the philosophy of science. In positing [R1*] this skepticism is absent, and Rosenberg appears to be swimming against the stream. Again, the reader may suspect that the deviations from [R1*] are *ad hoc*.

With all this said, it also has to be admitted that the idea – or more precisely: many ideas – of prediction plays a prominent role in what many economists do and what they say they do. Given the central role that the idea of predictive progress plays in Rosenberg's argument, we would need a more detailed – both conceptually and empirically – treatment of its different facets. The importance of this becomes even clearer as we move on to consider [R2]. Before doing this, let us see what Hausman has to say on the issue.

In his 'Introduction to the philosophy of science' appended to his monograph, Hausman is explicit about his view: 'the question of what distinguishes science from pseudo-science is not the right question to ask. For in addressing the differences between sciences and non-sciences as a single problem, one is forced to draw a single distinction where many distinctions should be drawn, and one is driven toward the view, which should be independently considered, that all sciences share the same methods of discovery and confirmation' (1992a: 316).

On the other hand, Hausman does employ the notion of science, categorizes economics as a science (just witness the title of his book), suggests that economists employ 'scientific methods', and supposedly does not think that just any intellectual activity can be appropriately characterized as science. There is a puzzle here. If one holds a concept of science and applies it to *X* but refuses to apply it to *Y*, does one not thereby imply some sort of demarcation criterion?

One way out of this dilemma would be to recognize that there are two different types of concepts of science, one normative, the other descriptive, and that the demarcation criteria are supposed to distinguish science from non-science in the normative rather than in the descriptive sense. One can then continue to use the term 'science' in the descriptive sense to denote, say, established academic disciplines, without implying a solution to the normative demarcation problem of how to tell intellectual endeavours that satisfy the norms of science from those that do not. Given this, one can practise normative judgement about the goodness of theories and explanations, irrespective of whether they are regarded as scientific or non-scientific in the normative sense. While this suggestion might help Hausman out of his problem, Rosenberg evidently holds a normative conception of science all down the line.

It has to be added that the solution that I offered to Hausman above may not be consistent throughout with what he actually says. It is one of his important claims that economics employs 'the scientific method' (1992b: 99–105) and that economists follow an appropriate view of theory appraisal (1992a: passim), thus presumably implying that economics satisfies at least some important normative criteria of scientificity. Hausman thus seems to exhibit some ambivalence on this issue.

4 IS ECONOMICS A SCIENCE?

Hausman's answer to this question is, 'Yes, economics is a science', but he does not think he needs to argue for it. Rosenberg's answer is 'No, it is not', and he tries to argue for it. Let us see if he succeeds to convince us.

[R2] Economics fails to meet criterion [R1].

This means that economics fails to be predictively progressive. Rosenberg claims – without documenting it – that since Adam Smith or Alfred Marshall (whether it is one or the other of these two points of comparison does not seem to make a difference to him) economics has not progressed from what he calls 'generic' predictions to what he calls 'specific' predictions. Let us reformulate:

[R2*] Economics fails to meet criterion [R1] in that it has failed to progress from generic to specific predictions.

Before examining the precise import of [R2*], it is worth pointing out that Rosenberg appears to hold a rather thin notion of progress. The sort of predictive progress that Rosenberg seems to have in mind is only one among many kinds of progress that sciences may exhibit. This is also witnessed by Hausman's short study of the development of theories of profit. Hausman concludes that 'there are at least seven regards in which there has been cumulative explanatory progress' in the attempts to explain the existence of profits: economists have recognized new regularities, they have found new facts and improved perspectives, errors have been corrected, the relevance of already known facts and generalizations have been recognized, there has been growth of systematization and conceptual articulation and clarification, and a 'deepening' or 'superseding' in the sense of 'qualifying old regularities and facts, recognizing their limitations, and in a sense incorporating them, with heavy revisions, into a more general account' (1992b: 196–8). Even if not everything that Hausman says on this case is uncontroversial, this is an effective reminder of the fact that there is more to progress in economics than narrowly conceived predictive progress. Unfortunately, we do not yet have a satisfactory conceptualization of the issue of whether and how economics has progressed in a richer sense.

Let us then take a closer look at Rosenberg's [R2*], claiming that economics has failed to progress from generic to specific predictions. On one obvious interpretation, a move from generic to specific can be understood as an improvement in predictive precision as required by [R1*] (even though there are problems with this interpretation as will be pointed out in a moment). Note, however, that I did not write 'Economics fails to meet criterion [R1*]'. This is because Rosenberg does not actually discuss the other element in [R1*], namely the 'proportion of hits to misses' when trying to persuade us that [R2] is true. This element turns out to be redundant for the argument. There is no smooth continuity between [R1*] and [R2*]. I wrote 'Economics fails to meet criterion [R1]', because [R1] can also be specified in terms of predictive precision only, excluding the idea of proportions.

The intelligibility and acceptability of [R2*] suffers from two kinds of omissions, one empirical, the other conceptual. Part of the empirical problem is that the evidence Rosenberg offers for [R2*] is almost non-existent and hardly at all documented. He relies on second-hand sources, most of all on Leontief's very general statements on the matter. The more interesting aspect of the empirical problem is that it is not a simple task to determine what should count as evidence and how it is supposed to bear upon [R2*]: predictive power or capacity is not easy to measure empirically. It is easier to measure predictive *achievements* than predictive *capacities*, since the latter do not necessarily manifest themselves straightforwardly in the former. For example, the appearance of constant or even diminished predictive success (as an achievement) may be accompanied by the actually improved predictive capabilities of economics. This is so for the simple reason that the world itself may have become more difficult to predict in that the larger initial conditions of the predictions are less stable than they used to be. As Hutchison puts it:

> Economic predictors, that is, may have had to walk faster up a more rapidly downward-moving escalator to stay at the same level. Factors making for increasing instability and unpredictability may be on the increase.[6] This would mean that actual improvements in the capacity of economists to predict (due, for example, to the compilation of many more reasonably prompt and accurate statistical series) do not superficially show through, as the improvements they actually are, compared with the predictive capacity and achievements of nineteenth-century economists.
>
> (Hutchison 1992: 83)

This possibility may have been difficult for Rosenberg to see. Because his approach to the alleged failures of economics is based on the attempt to trace these failures back to the core claims of fundamental choice theory, the framework of his argument does not make him sensitive to the possibility that part of the problem may be due to an increased instability of the larger boundary conditions.

A more fundamental, conceptual, flaw is that we do not really get to know what exactly [R2*] is supposed to mean. The problem is that Rosenberg fails to provide an analysis of relevant concepts of prediction and of the logic of prediction in economics.

The key concept of 'generic prediction' remains unclear. Let us first focus on the notion of 'generic' in 'generic prediction'. It seems to appear in two meanings at least in Rosenberg. On the one hand, it is equated with *qualitative* prediction, or prediction about the direction of change: 'The confirmed predictions about markets and industries of this theory have at best always been "generic" or, in Samuelson's term, "qualitative"' (p. 60). Qualitative is something that is opposed to quantitative. The import of [R2*] would then be that economics has failed to progress from qualitative to quantitative predictions. On the other hand, 'generic prediction' appears to mean something more general as witnessed by the following, which looks like a definition (we will take it as Rosenberg's definition of 'generic prediction'): 'By generic predictions I mean predictions of the existence of a phenomenon, process, or entity, as opposed to predictions about its detailed character' (p. 69). Here 'generic' is opposed to 'specific'.

At first sight this would seem to suggest that the generic/specific distinction is more general than the qualitative/quantitative distinction such that the latter is an instance of the former. However, this idea is difficult to sustain, given Rosenberg's definition of 'generic prediction' and his examples of what he calls (without defining the expression) 'generic theories'. First of all, his definition is not precise enough, since it lacks specifications of 'phenomenon', 'process', 'detailed character', etc. If one were to take a 3.5 per cent rise in the GNP of a given economy to count as a phenomenon, this would demolish the presumed link between 'generic' and 'qualitative'. Some specifications would be needed to avoid this, but they are difficult to come by, since Rosenberg's examples in this context (evolutionary theory and the second law of thermodynamics) talk about *specificity of process* – supposed to be lacking in exercises of a comparative statics kind employing 'generic theories' – rather than quantity. In general terms, he says that '[g]eneric prediction is characteristic of most theories that proceed by identifying an equilibrium position for the systems whose behavior they describe and then claim it moves toward or remains at this equilibrium value' without describing the course taken toward the equilibrium (p. 69). 'Generic' here appears in its second meaning; it appears close to being synonymous with *'comparative static'*.

It thus seems that 'generic' in Rosenberg's 'generic prediction' means two very different things: 'qualitative' and 'comparative static'.[7] The reader is not being informed about this; she has great difficulties with recognizing them correctly in different contexts of usage. This omission is far from harmless for the argument of the book. There are predictions that are specific in the second sense without being specific in the first sense; for example, those provided by, say, Austrian economic theory. And there are predictions that are specific in the first sense without being specific in the second sense, e.g., those found in textbook exercises in quantitative comparative statics.

Before moving on, it is worth pointing out that Rosenberg's account ignores an obvious type of predictive or explanatory progress that occurs occasionally in economics. This is *progress from no prediction to prediction of the existence of an institution*. An obvious example is progress that has been made by moving from no account to an account of the existence and boundaries of the business firm. It is also clear that 'predictions' of this qualitative sort are not useless for policy purposes.

In general, it would have helped Rosenberg to pay some attention to the distinctions that economists themselves make, for example, between conditional and unconditional predictions, between point predictions and interval predictions, and between pattern predictions and quantitative predictions. Such notions would have helped him in specifying the very idea of predictive progress and in assessing the predictive record of economics.

Another omission is a lack of a consistently held distinction between *prediction and confirmed prediction*. It is one thing to argue that it is not on the agenda of economists to provide, say, quantitative predictions; it is quite another to argue that the quantitative predictions they make are not confirmed. Rosenberg appears to make both claims, thus sliding into incoherence. For example, he claims that Keynesian macroeconomics only yields and intends to yield generic predictions; at the same time, he says that Keynesian theory is supposed to provide instruments for fine-tuning (pp. 72–5). But fine-tuning would be an unintelligible aim without the idea of quantitative predictions of some sort.

A related point needs to be mentioned. Rosenberg seems to be suggesting that policy relevance requires progress from qualitative (or generic) to confirmed quantitative (or specific) predictions. This implies that policy relevance is equated with relevance for something like fine tuning. But this is objectionable. It should be evident that kinds of predictions other than quantitative ones – such as qualitative predictions, pattern predictions, predictions of tendencies – may be relevant and useful for policy.[8]

An omission of fundamental importance is that Rosenberg is virtually silent about how economic theories are supposed to yield predictions. One would wish a book devoted to making radical claims about the predictive capabilities of a discipline to contain extensive analyses of the logic of prediction. In this context, the role and interrelations of various kinds of theoretical and empirical models should have been scrutinized. Without such analyses, the attribution of alleged predictive failure to the basic assumptions of economic theory remains ungrounded (more on this below). To make things worse, the term 'explanation' appears regularly instead of 'prediction'; yet, no comments are given about the intricate issue of the relationship between explanation and prediction.

In consequence, we cannot be quite sure about the import of [R2*]. Such a sweeping claim about the predictive capabilities of 'economic theory' would require a much more refined argument based on analyses of both 'theory' and 'prediction' and the connection between them. Too much is now left for intuition.

5 MODEL AND THEORY

Let us see if we can find some help from Hausman regarding the issues pointed out above. Indeed, it would seem that we can. Hausman makes suggestions concerning the structure of theory and the logic of theorizing in economics, themes that we found wanting in Rosenberg.

First of all, Hausman suggests that there is what he calls a 'fundamental theory' in neoclassical economics. He holds the interesting yet controversial view that the role of fundamental theory is not played by general equilibrium theory, but rather by what he calls both 'equilibrium theory' and 'the basic equilibrium model' (this was already suggested in his 1981 book). Equilibrium theory consists of consumer theory (which consists of utility theory, 'consumerism', and the 'law' of diminishing marginal rate of substitution), theory of the firm (consisting of profit maximization, diminishing returns, and constant returns to scale), and the 'law' that markets clear. Both general and partial equilibrium theories are then described as 'augmentations' of this fundamental equilibrium theory (pp. 53–5). Understandably, this last idea is controversial.[9]

Two chapters later, Hausman suggests a distinction between model and theory. In this context, the fundamental theory of economics is conceptualized as a model, hence 'the basic equilibrium model' becomes a more appropriate title for it. Hausman suggests that his conception of theorizing based on a distinction between model and theory 'helps one to understand how economists theorize' (p. 70). How does he characterize the distinction?

He says that models are 'definitions of predicates'. For example, the basic equilibrium model defines the predicate 'is an equilibrium system', that is, it informs us about what it means to say that something is an equilibrium system. Theories, on the other hand, are 'sets of interpreted lawlike assertions'. The important implication of this, he says, is that models should not be assessed in terms of truth and falsehood or predictive power, while theories are to be so assessed (pp. 74–7). It is theories that provide explanations and predictions, while models are not to be used for purposes for which they are not designed. A large proportion of work in economics is restricted to the refinement of models and is, by implication, devoid of any predictive entailments, either 'generic' or 'specific', to use Rosenberg's expressions. One only gets predictions by transforming models into theories.

There are ambiguities in how this idea is further specified. Hausman writes as follows: 'On this view of scientific theories, there is no point in asking whether the claims of a [model] are true or whether a [model] provides reliable predictions. Predicates cannot be true or false or provide any predictions. Definitions are trivially true' (pp. 74–5). Given that we have just learnt that models are *definitions of* predicates, the reader may wonder why, in this context, it is said that '*predicates* cannot be true or false', while models-*qua*-definitions are 'trivially true'. It appears as if Hausman is saying that models are trivially true, and that it makes no sense to ask whether they are true. When we read further, it turns out that the explanation for this unclarity may be found in the fact that Hausman is ultimately undecided about whether models are predicates or their definitions. He talks about a model being 'characterized as a predicate or as a definition of a predicate' (p. 75) and about models being 'trivially true or neither true nor false' (table on p. 77). From a substantial point of view, this may or may not be a harmless ambiguity; to find out, we should see this notion of a model put to work. Unfortunately, the notion is not employed to do any work in the analysis of economic models in the book.

A second set of ambiguities is involved in the characterizations of the third component that is supposed to play a role in transforming models into theories, namely 'theoretical hypotheses'. We may read that '[f]rom a theoretical hypothesis one "*recovers*" the assumptions of the model as assertions of the world' (p. 76, emphasis added) and that a model plus a theoretical hypothesis '*results in* a theory' (p. 77, emphasis added) and that '[w]hen one offers a general theoretical hypothesis asserting that something is the kind of system defined by a model, then one is *enunciating* a theory'

(p. 78, emphasis added). The italicized expressions are too vague to convey a precise idea of what theories are and how theoretical hypotheses are supposed to turn models into theories. A related problem is that it is not very clear how precisely considerations of truth are evoked by theoretical hypotheses. Hausman does not make clear the difference between theoretical hypotheses themselves being true or false assertions about models, on the one hand, and theoretical hypotheses rendering the assumptions of models true or false assertions, on the other. Do models – which as such are either trivially true or neither true nor false – become true or false by becoming objects of theoretical hypotheses? Or are theoretical hypotheses the true or false assertions of a theory, having the properties of models such as their applicability as their objects?

Despite these difficulties, the main message should be clear to the reader: 'Insofar as one is only working with a model, one can dismiss any questions about the realism of the assumptions one makes. But remember that the reason is that one is saying nothing about the world ... one's efforts are purely conceptual or mathematical. One is only developing a complicated concept or definition' (p. 79). This, together with denying models any predictive implications, would seem to be the only consequence stated explicitly. On the other hand, the way in which this conception of theory might help us understand the logic of explanation and prediction is not explored. It may therefore perhaps not come as a surprise that the distinction between theory and model does not play any major role in the later parts of the book. Unfortunately, this means that Hausman's suggestion that his distinction between model and theory 'helps one to understand how economists theorize' (p. 70) is not really substantiated in the book. The distinction remains an interesting idea which is unexploited in the main arguments of the book.

One part of Hausman's suggestion, I admit, does have some intuitive appeal: a lot of research in economics appears to have the character of logical exploration. The criticisms submitted above imply that in order to assess the whole suggestion we need to see a more precise articulation of the distinction between theory and model, and we need to see the distinction in action, applied to concrete cases of theorizing. Another way of challenging Hausman's conception of theorizing would be to cast doubt on whether it is helpful in the way he suggests. The conception is based on a distinction between the conceptual (models) and the empirical (theories). This may be of some help in rationalizing the apparent immunity of equilibrium models with respect to empirical evidence, but this should not be taken to suggest that when working with models economists are indifferent with respect to what they believe happens in the world. One may argue that this would be implausible: even when working on 'models', economists are constrained by their factual beliefs. It may be that, in order to understand this aspect of economic theorizing, further conceptual resources are needed, such as a distinction (or a continuum) between abstract and concrete, or one between the possible and the actual.[10]

Despite its weaknesses, the brief chapter in Hausman's book on theories and models provides a hint about how to take a step forward in our extremely underdeveloped understanding of the structure of economic theorizing. And Hausman has other things to say on this theme as we shall see.

6 COULD ECONOMICS BE PROGRESSIVE?

Let us then return to Rosenberg's account. Predictive progress is Rosenberg's criterion of science. He claims that economics does not satisfy this criterion. But he does not stop here. What is interesting about his project is that he pursues a controversial explanation of economics' alleged predictive failure.

[R3] The reason for [R2*] is the dependence of economics on folk psychology.

This is the crux and most interesting part of Rosenberg's argument. He argues that economics 'proceeds by formalizing commonsense explanations of action into a theory of rational choice' (1992:118). Preferences and expectations as represented in economic theory are the equivalents of the folk psychological notions[11] of 'desire and belief' as he puts it. This folk psychological link, he argues, ultimately explains the lack of predictive progress in economics.[12]

Rosenberg's diagnosis is not the popular one of tracing back the predictive failures to the idealizations and simplifications of rational choice theory. It is more radical than that, it goes deeper: the blame is on the intentional nature of the fundamental vocabulary of choice theory, irrespective of how the mental, intentional states are represented. The problem is with intentionality, not with idealization.

Let us look at the argument more closely. Predictive progress, Rosenberg tells us, can be brought about in two ways only:

> either by more exact measurements of the initial or boundary conditions to which a discipline's theories are applied or by an increase in the precision of the theory's claims about the mechanisms that lead from initial conditions to consequences. In meteorology, presumably, the most important improvements have been made in the measurement, collection, and aggregation of data to provide the initial-condition inputs. . . . I argue that economic theory's character makes impossible both improvements in the specification of initial conditions and improvements in the generalizations of the theory itself
>
> (1992: 112)

This will certainly sound very odd to a practising economist: improvements in the measurement and collection of data on initial conditions have not only been non-existent, but are impossible! The macroeconomist, for example, will protest by reminding us that there have been significant improvements in national income statistics. Rosenberg must mean something peculiar for this to make any sense. Indeed, this turns out to be the case. The sweeping claim about the impossibility of improving upon 'initial conditions' is eventually watered down to a claim concerning preferences and expectations – something that would not occur to an economist as concerning the measurement of data on the initial conditions. Rosenberg explains that 'if we want to improve our predictions, we need to improve our measurements of the states of the agent to which we apply the theory in order to secure predictions about behaviour' (p. 124). The states he refers to are mental states of individual actors: 'the initial conditions to which we apply rational choice theory, that is the preferences and expectations of agents' (p. 126). As we shall see more closely, this curious focus is linked to the centrality of the mental as the explanans and of individual behaviour as the explanandum in Rosenberg's account of economics.

Rosenberg explains what he perceives as the fundamental difficulty in economic theory by invoking an analogy with the ideal gas law, $PV = rT$.

> To apply this law to predict the volume of a gas, one needs to measure the gas's temperature and its pressure, and of course one needs to measure the volume in order to assess the accuracy of the prediction.... But suppose the only way to measure the temperature of a gas is to measure its volume and pressure and then plug these values into $PV = rT$. That is, suppose that the only sorts of thermometers available depended for their accuracy on the truth of the ideal gas law. If this were the case, then the gas law would be vacuously true and useless in predicting the behaviour of gases. No set of observed values of pressure, temperature, and volume could disconfirm the law, because it is used to determine these values. That makes it vacuous .... Now, what we need to apply, test, and improve our theory of rational choice is the equivalent of a thermometer ... And this 'thermometer' – this means of telling what agents believe and what they want – must be independent of the hypothesis of rational choice: that is, the reliability of the instrument we use to measure strength of desire and degree of belief must not hinge on the truth of the theory of rational choice. However, this is just what we cannot get ... There is no way to tell what a person believes unless we already know what he wants and how he acts; no way to tell what a person wants unless we know what he believes and how he acts; no way to tell what a person will do unless we know what he wants and believes. The only way any two of these three factors can lead us to a prediction about the third is via the theory of rational choice.
>
> (pp. 124–6)

What this implies is that rational choice theory is *vacuously true*. Now this may make the reader somewhat puzzled, since, just a few pages earlier, she has been told that the theory is *false* in containing 'false assumptions' and that the 'theory of rational choice is inadequate, that is, makes false predictions, because people are irrational' (p. 116). The reader wonders whether Rosenberg is able to state these things without contradicting himself.

Rosenberg concludes: 'the upshot of the intentional character of the explanatory variables of economic theory is obvious. We cannot expect the theory's predictions and explanations of the choices of individuals to exceed the precision and accuracy of the commonsense explanations and predictions with which we have all been familiar since prehistory' (p. 129). Thus, predictive progress is not forthcoming. On the other hand, *a minimum degree of predictive success* about our daily lives has always characterized the commonsense accounts (viz. 'the precision and accuracy of the commonsense explanations and predictions with which we have all been familiar since prehistory'); otherwise their tenacity would be difficult to understand. But this leads to a puzzling observation about Rosenberg's discussion. Drawing on the analogy with the ideal gas law, he says that if a theory were vacuous, it 'would be predictively useless' (p. 125) and that 'it is a requirement for the predictive power of a theory that the instruments we employ to predictively apply it to initial conditions be independent of the theory to be applied.' (pp. 125–6). This implies that not only is folk psychology (and rational choice theory as its refinement) predictively non-progressive, it is also *predictively unsuccessful* to the extent of being 'predictively useless'. Quite obviously, this is both false – folk psychology is not predictively useless – and inconsistent with what Rosenberg himself implies about the weak predictive success that folk psychology has had 'since prehistory'. His discussion seems to be plagued by an unresolved ambivalence.[13]

A related curiosity is worth mentioning. For some reason Rosenberg employs the vocabulary of certitude when expressing his views about the predictive poverty of folk psychology. He says that 'any human behaviour can be the product of an indefinite number of combinations of expectations and preferences, so unless we can be certain what individuals want, we cannot be certain exactly [what they believe] and to be sure' (p. 144) and, in the same vein, 'there is no way to decide what exactly an individual really believes or desires' (p. 144); therefore, the attributions of 'beliefs and desires' to people are no 'more than just guesses' (p. 144). On the premise of this argument, one can easily agree: 'We cannot be certain exactly' what goes on in people's minds. Whether the conclusion – that our attributions of mental facts to people are no 'more than just guesses' – is agreeable depends on what else one says about this issue. In a sense, *all scientific hypotheses* are 'just guesses' – no absolute certainty is attainable. So there is nothing special about folk psychology and thereby economics in this respect. Yet, Rosenberg would seem to cite the ubiquitous phenomenon of the underdetermination of hypothesis by evidence – present in all disciplines – as somehow fatal to folk psychology and thereby economics specifically. This is to suggest that

Rosenberg appears to commit the fallacy of inferring from uncertainty to ignorance. No doubt more needs to be said to show that folk psychology and economics are in a radically unique situation with regard to the underdetermination issue.

One may also want to question Rosenberg's skepticism more directly. Towards the end of the long passage quoted above he says that 'there is no way to tell what a person believes unless we already know what he wants and how he acts; no way to tell what a person wants unless we know what he believes and how he acts'. One might want to argue that this is simply not true. One is able to tell (without being certain about it) what a person wants or believes by asking her or her mother, by past experience with her, by knowing about the relevant community beliefs and values and conventions, etc. There might be an interesting and important epistemic disanalogy between explanations employing the ideal gas law and those employing practical reasoning that Rosenberg chooses to ignore.

Let me then point out another debatable feature in Rosenberg's approach. He mostly talks about economics pursuing predictions of individual behaviour. In my opinion, this is a major problem of the whole book (and much of the earlier work by Rosenberg on the philosophy of economics, beginning with *Microeconomic Laws*). The bearing of our ability to predict individual behaviour upon our success in accounting for collective consequences is presented as an unargued claim. Here is an example: 'And if our predictions of the behaviour of individuals faced with individual choices are fated to be at best vague and imprecise, what can we expect when we aggregate individual behaviour? It is improbable that we can improve on the accuracy of claims about the aggregation of human choices without improvements in our accuracy about individual choice' (p. 129). The thought seems to be that the failures of economics are straightforward manifestations of its failure to predict why Ms Jones crossed the street to purchase a loaf of bread – presuming that the latter in fact is a failure, which would presuppose that such predictions are on the agenda of economics.

Rosenberg's argument hinges upon the presumption that economic theory is an attempt to predict individual behaviour, and that, if it fails in this attempt, this implies that it also fails in predicting social phenomena. But this implication is not well substantiated in Rosenberg's argument. In fact it is not substantiated at all. Here and there, he only adds incidental remarks about individual choices 'adding up' or being 'aggregated', but no analysis, not even a rudimentary one, is provided about the theoretical structures, often said to be constituted by 'composition principles', that account for the relationship between individual action and collective outcomes. Different theories involve different composition principles. It is these composition principles that have to be analysed in order to justify inferences from accounts of individual actions to accounts of collective outcomes.[14] One can also argue directly against the microfoundationist thesis and defend the autonomy of aggregate reasoning.[15]

This is related to the more general problem of the book, already alluded to above: the lack of an analysis of the logic of prediction. How precisely do economic theories yield predictions? No single answer would be possible, because there are different kinds of theories and different kinds of predictions. In any case, it is not enough to jump from controversial (and possibly incoherent) accounts of rational individual choice to radical conclusions about aggregate predictions without defending the microfoundationist programme and without scrutinizing the mediating composition principles.

Rosenberg says that '[a]s the history of science suggests . . . improvements in microfoundations are the best ways to improve the accuracy of macro-predictions' (pp. 129–30). This seems to leave room for other, even though presumably poorer, ways of improvement, but Rosenberg does not discuss them. The notion of 'improvements in microfoundations' may also be misleading. What Rosenberg must mean by this are improvements that go beyond the boundaries of the general folk psychological framework. But this raises questions. It is easy to think of improvements – improvements that actually have taken place or are taking place in economics – that do not satisfy these constraints. First, it is not inconceivable that improvements in 'micro-foundations' within the folk psychological framework will bring about predictive improvements; just think of incorporating asymmetric information or moral hazard into economic models. Second, the same can be said about the role of improvements in the composition principles in attempts to improve macropredictions. Third, recalling that Rosenberg tells us that predictive progress can be generated 'by an increase in the precision of the theory's claims about the mechanisms that lead from initial conditions to consequences' (p. 112), we may remind him that there are mechanisms other than the mental mechanisms denoted by rational choice theory that have both explanatory and predictive significance – the market mechanism, mechanisms of cooperation and negotiation, the money creation mechanism, the Keynesian multiplier mechanism, the money transmission mechanism, etc.[16]

In general, Rosenberg's account seems to be too much an abstract exercise in the philosophy of mind rather than a concrete analysis of actual economics. An important source of error seems to be his failure to make a distinction between what we might call the *folk psychological framework*, on the one hand, and the *folk psychological theory or model*, on the other. As a framework, folk psychology consists of the categories of intention, belief, and action, while different folk psychological theories and models are formulated in terms of specified versions of these abstract categories and their interrelations. Now Rosenberg is in effect saying that no predictive or explanatory progress can be attained by any possible move from one specific theory or model to another, as long as these models remain within the general folk psychological framework. In arguing that predictive improvements are impossible to come by due to the link with folk psychology, Rosenberg is in fact implying the incredible view that it makes no difference at all whether economists employ models with egoism or altruism, with maximization or satisficing, with certainty or uncertainty, with myopic or rational expectations, with symmetric or asymmetric information, with or without opportunism or moral hazard, with or without money illusion, etc. All models with conceptual ties to folk psychology are destined to possess a fixed low degree of predictive success if any. And what is fixed cannot be improved within the folk psychological framework. No economist, orthodox or heterodox, is going to buy these implications. Economists are actually debating about the relative (also predictive) merits of models with such rival elements. Rosenberg's account seems unable to make sense of these debates. His diagnosis suggests that these economists are mistaken and that such debates are futile. Therefore, the diagnosis must appear unconvincing to economists.

If one is willing to go along with Rosenberg in implying that the whole economics profession is under a collective illusion, there is another way of arguing that his diagnosis is questionable. This is, of course, to provide an alternative and more plausible account. For this, we turn to Hausman again.

7 THE INEXACTNESS OF ECONOMICS

In *The Inexact and Separate Science*, Hausman seems to identify economics as belonging to the large *genus* of science and as being distinguished from other scientific disciplines by the *differentia specifica* of being inexact and, in particular, of being separate. Economics is a science, but it is a different science. It is a science without extraordinary predictive accomplishments. How does Hausman account for this characteristic?

Let us start the discussion with considering the idea of inexactness in

[H1] Economics is an inexact science.

Inexactness is an idea Hausman borrows from J.S. Mill (this has been a theme in Hausman's work since his 1981 book). What is inexactness, and to what can it be attributed? Hausman says inexactness is a property of laws and theories – it is something that lies *within* theories – and he considers alternative ways of interpreting the idea (1992a: 128–32). He opts for the alternative according to which laws are inexact if they are implicitly qualified by vague *ceteris paribus* clauses: 'one should regard the "laws" of inexact sciences as carrying with them implicit *ceteris paribus* clauses' (p. 133). Let me raise some questions about the idea of inexactness as vague implicit qualification.

First of all, it is obvious that Hausman adopts a very broad reading of '*ceteris paribus*' – a reading that goes beyond its literal meaning as 'other things being equal'. Anything – not just violations of equality – that might interfere with what theory says happens in the world becomes covered by the clause. Thus, '[o]ne should regard economists as telling us how real agents behave in the absence of various complications' (p. 133). This, of course, plays an important role in the argument which is supposed to explain the predictive weakness of economics.

Secondly, I am not sure I see why the *ceteris paribus* qualification would need to be implicit; that this is Hausman's view would seem to be suggested, e.g., by the labelling of the conception adopted as 'the implicit qualification view' (p. 132). While it is clear that in many – actually in most – cases, the qualification is actually only implicit, it should make no difference at all regarding the inexactness of theory whether the qualification is implicit or explicit. The important feature is the vagueness of the qualification: the list of potential interferences is not – indeed, cannot – be completed.

Thirdly, the use of the term 'inexactness' in this context raises questions. On the interpretation of inexactness at hand, the term actually is understood to designate partiality or incompleteness: the premises of a theory cover only a small set of causes, while the rest are put aside by the *ceteris paribus* clause. This typically leads to inexact implications – including predictive failures – because the causes excluded from the theory make their impact on the actual outcome of economic processes, but this impact is not captured by the theory's implications. The idea of *inexact implications* would fit with this picture neatly. But Hausman talks about inexactness *within* laws and theories so as to make it unanalysable in terms of inexactness of implications. Inexactness in Hausman's sense would seem to be nothing but partiality or incompleteness understood in terms of the *ceteris paribus* clause.[17]

Be that as it may, here is a summary of Hausman's diagnosis of the predictive weakness of economics, to rival that of Rosenberg: 'In disciplines such as economics the correspondence between the data and the implications of theory is rough, and complete failures are frequent. Since economic phenomena are the effects of numerous causes, many of which the theory does not encompass, one can expect nothing better' (1992a: 148). This is, of course, an old story, but it still has a lot of plausibility. One wonders whether the point about the folk psychological link of some of the concepts of economic theories has much to add to this old wisdom.

Before moving on, I would like to point out an ontological tension in Hausman's treatment of inexact laws. On the one hand, he has the empiricist habit of calling laws 'regularities' and law statements 'generalizations'. On the other hand, he occasionally says that lawlike statements qualified by *ceteris paribus* clauses denote 'tendencies', thus apparently invoking far stronger, non-empiricist, ideas about the ontological underpinnings of economic theories. This might at first look like a minor terminological problem, but it can also be viewed as a major conceptual issue. The problem is that there is no easy way of getting from qualified general statements (concerned with what happens or would happen in the world) to tendencies (concerned with the causal powers that things in the world possess) without either watering down the whole idea of tendency within an empiricist ontology or else introducing a richer ontological artillery comprising concepts for causal powers, intrinsic natures, and all that. There seems to be no obvious way to settle this tension within Hausman's metaphysically thin framework.

8 ECONOMISTS' DEDUCTIVE METHOD OF THEORY APPRAISAL

Let us then briefly look at the methodological implications Hausman suggests to draw from his analysis of inexactness. The important conclusion he draws is that economists are justified in being reluctant to reject the basic tenets of equilibrium theory when faced with negative evidence. There is always the *ceteris paribus* clause to blame when a prediction fails. But this presupposes that the premises of a logical argument entailing a prediction are not epistemically equal; the basic propositions of equilibrium theory need to be more plausible than the *ceteris paribus* clause. This is, indeed, the view that Hausman holds. Considering claim [H2] makes this clear. It cites the deductive method of theory appraisal:

[H2] Economics employs the deductive method of theory appraisal.

But [H2] is not specific enough, since the deductive method appears in different variations, such as 'deductive method a priori' – the dogmatic version J.S. Mill is claimed to have held – and what Hausman calls 'economists' deductive method' of theory appraisal. It is the latter that Hausman focuses on:

[H2*] Economics employs the economists' deductive method of theory appraisal.

Hausman's formulation of 'the economists' deductive method' is in terms of the following four components (1992a: 222; 1992b; 67):

D1 Formulate credible (*ceteris paribus*) and pragmatically convenient generalizations concerning the operation of relevant causal factors.
D2 Deduce from these generalizations and statements of initial conditions, simplifications, etc., predictions concerning relevant phenomena.
D3 Test the predictions.
D4 If the predictions are correct, then regard the whole amalgam as confirmed. If the predictions are not correct, then compare alternative accounts of the failure on the basis of explanatory success, empirical progress, and pragmatic usefulness.

This is supposed to describe *economists'* deductive method because Hausman believes economists subscribe to it. Rules D1–D4 articulate what he calls the 'theory' or 'view' of theory appraisal that economists hold. Economists' deductive method is meant to leave less space for dogmatism than Mill's inexact method *a priori*. While the *a priori* method proscribes the refutation of the fundamental premises, here they are supposed to be open for refutation in principle: there is no methodological rule to prevent it. However, economists are inclined not to revise the fundamental propositions of equilibrium theory, and there are two reasons for this: first, these propositions have some independent 'credibility', or, as Hausman also puts it, there is 'a good deal of truth' to them; second, in the circumstances of uncontrollable complexity, tests are never decisive. In line with what he calls the 'weak-link principle' (p. 207), economists are inclined to blame the *ceteris paribus* clause and other non-fundamental simplifications for the predictive failures of their theories. The fundamental premises are supposed to be the relatively strong links that are more resistant to revision (p. 208). Hence:

[H3] The deductive method is the appropriate method in inexact sciences, but it does not serve economics perfectly due to the uncontrollable complexity of its subject matter.

Hausman says there is nothing wrong with this 'theory' or 'view' of theory appraisal, i.e., that put in terms of D1–D4, which he claims economists hold. Economists are just unlucky in having to deal with a recalcitrant subject matter which makes the task immensely difficult. Hausman says that 'the failure will lie in the difficulties of the task, not in any methodological mistake' (p. 207). Economists may appear dogmatic, but the 'apparent dogmatism may be just the result of the good fortune of beginning with a set of plausible generalizations coupled with the bad luck of being unable to perform good tests' (p. 211). Appearances notwithstanding, 'one should not leap to the conclusion that economists are following this rule' – viz. that 'one should never attribute apparent disconfirmations to shortcomings in one's laws' – 'It looks as if they are, but appearances may be misleading' (pp. 206, 207).

These suggestions give rise to several interesting questions. Consider first what we may call the *schein* argument, the argument that there is a difference between apparent behaviour and actual behaviour. Hausman is implying that economists behave as if they were following Mill's dogmatic method *a priori*. But, he suggests, this is just an appearance, a *schein*; because economists are actually following their own deductive method. This is an interesting contention. How could one substantiate it? Hausman gives support to it by devoting a chapter to a perceptive case study of economists' behaviour in the context of experimental work on the so-called preference reversal phenomenon. He claims that the reactions of economists to some of the experimental findings indicate that they are not following the dogmatic method *a priori*. This case study involves many interesting issues, but I am not going to address them here. I just want to mention a conceptual issue of some relevance. The reason for this is that there is an ambiguity in some of Hausman's typical phrases, such as economists holding a 'view of appraisal' or 'theory of confirmation', or economists following this or that method, and that these phrases play an important role in the formulation of his main theses.

Let us try to clarify this ambiguity by looking at the notion of methodological rule. Consider Hausman's usage of 'rule' in the context of the statement that 'one should not leap to the conclusion that economists are following this rule' to the effect that 'one should never attribute apparent disconfirmations to shortcomings in one's laws'. To begin, it is obvious that economists will not state such a rule as an explicit canon to be followed. But this would not be required for the rule being actually followed. Most rules are implicit social conventions or customs, which are not formulated and are hard to formulate as explicit canons of conduct. Consider Hausman's statement, 'refutation is largely proscribed, albeit by the circumstances, not by methodological rule' (1992a: 222). This implies the obvious truth that there is no explicitly formulated rule-*qua*-canon in place, proscribing refutations of fundamental statements. On the other hand, if the circumstances are such that they tend to force economists regularly to behave as if they were following such an explicit rule, we may well be justified in saying that a social convention or custom characteristic of the economics profession tends to be established and that individual economists are following such an implicit rule-*qua*-custom. Even in the absence of a rule-*qua*-canon, there may be a rule-*qua*-custom, existing and constraining as an implicit social convention according to which refutations of fundamental statements should be avoided. The existence of such rules is consistent with some members of the community breaking them occasionally. The important point is that in following a rule-*qua*-custom economists behave as if they were following the corresponding rule-*qua*-canon. There is an appearance, a *schein*, here, but there is also a rule actually followed.[18]

Consider next Hausman's suggestion that the tenets of equilibrium theory – diminishing returns, maximization, etc. – are 'credible' or that there is 'a good deal of truth' to them, and that they are 'pragmatically convenient' and that therefore they are able to serve as the 'strong links' in a test situation (e.g., 1992a: 210).[19] Now this is precisely where many readers are about to sense a conservative and defensive bent in Hausman's argument (e.g. Reuten 1996; Backhouse 1995). Are there grounds for this suspicion? Let us consider the claim about 'credibility' and 'a good deal of truth' first. How does one assess such a claim? There is a section in Hausman's book, entitled 'The justification of inexact laws', that would appear to provide tools for such an assessment (1992a: 139–42; the ideas are already there in the 1981 book). He suggests a formulation of Mill's criteria for justifying inexact law statements, namely the criteria of lawlikeness, reliability, refinability, and excusability (see also 1992b: 42). I find the suggestions of this section extremely insightful and important, but unfortunately they are not employed by Hausman to support the claim of 'credibility'. The section seems to be hanging in the air, and so seems the claim. Of course, it may well be that the fundamental statements of equilibrium theory are found 'credible' by many economists. It may even be that, as a matter of fact, there is 'a great deal of truth' to them. The problem, as I see it, is that it is not clear what grounds we have for making such claims. Some idea of such grounds should be part of any complete theory of theory appraisal, but it is missing in the present formulation of D1–D4. This is one obvious question on which progress should be pursued in future research.

Consider then the idea of the tenets of equilibrium theory possessing 'pragmatic virtues', by which Hausman means that they 'play an important role in making the theory mathematically tractable, consistent, and determinate' (1992a: 210). This will be found even more questionable by those economists who do not share the methodological values of standard neoclassical economics. Different schools of thought and research orientations underwrite different methodological values; to incorporate 'neoclassical values' into a formulation of a sound 'view of theory appraisal' appears to non-neoclassical economists nothing but defensive of neoclassical theory. By definition, neoclassical theory possesses the 'pragmatic virtues' of neoclassical theory. Hausman appears to have created a conservative circle in his argument.

Let us finally consider the normativity of Hausman's claim that there is nothing wrong with economists' view of theory appraisal, that there is no 'methodological mistake' involved in economists' practices. What are his grounds for this judgement? I am not sure I am reading him right on this matter, but it seems to me that the only justification given for his defence of 'the existing practices of theory assessment among economists' (p. 206) is that they conform to some well-established philosophical theories of confirmation. He says that 'they are consistent with the recommendations of standard methods of theory appraisal in the special circumstances with which economists have to cope. Although apparent Millians in practice, economists can be good Bayesians or hypothetico-deductivists in principle' (p. 206). If this implies that Bayesian and hypothetico-deductive methodologies provide the standards against which the soundness of appraisal practice is to be judged, one then naturally asks for the grounds for choosing these standards. It looks as if it is here that Hausman implicitly commits himself to a normative conception of science and that the basic norms are adopted with perhaps no other justification than the authority of standard philosophy of science. As we shall see soon, this type of normative inference seems to create an interesting dilemma in Hausman's account of economics.

9 THE SEPARATENESS OF ECONOMICS

Hausman provides a perceptive articulation of another old idea about the nature of economics. In contrast to 'the view of theory assessment' characterized by D1–D4, he talks about what he calls 'the structure and strategy of theorizing' when discussing the separateness of economics. The economists' 'view of theory assessment' is depicted as both conventional and rational, whereas the fault is on 'the structure and strategy of theorizing': he finds 'the vision of economics as a separate science as the key to its methodological peculiarities' (1992a: 245) and views some of these peculiarities as questionable. Here is the key:

[H4] Economics is a separate science.

He characterizes the idea in terms of four claims: Economics is defined in terms of a limited set of causal factors; those causal factors predominate in the distinctive domain of economics; the laws of those factors are reasonably well-known; thus, economics provides a unified and complete account of its domain (1992a: 90–3). In more substantive terms, the domain of economics is defined as follows: 'Economic phenomena are the consequences of rational choices that are governed predominantly by some variant of consumerism and profit maximization. In other words, economics studies the consequences of rational greed' (p. 95).

Given this characterization of separateness two questions arise. First, why is the above set of characteristics designated by the term 'separate'? Elsewhere, Hausman also talks about the familiar phenomenon of economics being separate from other disciplines such as psychology, but this seems to be a derivative feature relative to the above more fundamental characteristics. These more fundamental characteristics constitute economics as a science which subscribes, not to separateness directly, but to the ideal of theoretical and explanatory unification, the pursuit of maximal scope employing a parsimonious set of fundamental claims. Hausman is not explicit about this, but the idea is quite clearly present in a number of central passages in the book. He says variously that economists 'reject other approaches because they cannot be integrated into a unified theory of an economic realm' (p. 247); that '[n]o changes in fundamental theory itself are permissible unless they preserve its universal scope' (p. 274); that 'any good theory must, like the accepted theory, have both comprehensive scope and a compact or parsimonious theoretical core' (p. 235); that 'any alternative to accepted theory must preserve a peculiarly 'economic' realm to be spanned by a single unified theory' (p. 236); and that 'one finds a constraint in operation here against considering a narrow-scope hypothesis, regardless of its empirical vindication' (p. 236).

The primary commitment, therefore, seems to be to economics as a theoretically unified and explanatorily unifying science. Yet, let us retain the idea of separateness in the following modification of [H4]:

[H4*] Economics is a separate science committed to the ideal of theoretical unification.

Another question that arises is one concerning the relationship between inexactness and separateness. Hausman is nowhere very clear about this, which is unfortunate, because the impression the reader easily gets is that inexactness and separateness are quite separate notions. However, closer scrutiny leads to the suggestion that separateness is a special case of inexactness (or partiality as I might want to call it), namely separateness is inexactness (or partiality) plus the conviction that the partial theory has managed to capture the major causes that serve to unify the phenomena, and one is therefore justified in excluding other causes from the core theory.

We are now equipped to consider the last major element in Hausman's portrait of economics:

[H5] The commitment to economics as a separate science has led economists to unjustified dogmatism.

The key notion in Hausman's diagnosis of the dogmatism of economists is that of separateness: 'If economists are sometimes unreasonably dogmatic, it is because of this last commitment [to economics as a separate science], not because of their views of theory appraisal' (1992a: 275). This contention occurs over and over again in the book. It is no doubt one of Hausman's main theses. I find it highly problematic. This is because I do not see how one can separate 'the structure and strategy' of economics involving economists' 'commitment to economics as a separate science' from their 'views of theory appraisal'. Let me show why this is so.

Hausman suggests that D1–D4 depict the rules that constitute the method of theory appraisal that economists actually follow. He says that this amounts to a sound theory of theory appraisal. On the other hand, as implied by [H4*], he thinks that when committing themselves to economics as a 'separate science' economists hold a rule not included in set D1–D4 that might read as follows:

[U] An acceptable theory should satisfy the ideal of theoretical unification.

Now it should be obvious that rule [U] *is a rule of theory appraisal.* If [U] is what economists subscribe to, as Hausman believes they do, then [U] *should be regarded as an important part of the method of theory appraisal in economics!* Just like the requirements of credibility and pragmatic convenience, [U] gives a tight constraint on the kinds of fundamental propositions that may be considered for acceptance in economics. Therefore, [U] has to be incorporated into rule D1, possibly along the following lines:

D1* Formulate credible (*ceteris paribus*), pragmatically convenient, and *powerfully unifying* generalizations concerning the operation of relevant causal factors.

It now turns out that the economists' deductive method is D1*–D4 rather than D1–D4.[20] This implies the surprising conclusion that *it is not open to Hausman to separate the appropriate deductive method followed in the inexact science of economics, on the one hand, from the inappropriate* *dogmatism of economics as a separate science, on the other*. He cannot coherently say both that 'economists employ an uncontroversial method of theory assessment' (1922a: 253) and that economists should not subscribe to [H4*] and [U]. He cannot coherently 'defend the existing practices of theory assessment among economists' (1992a: 206) and claim that the 'commitment to the structure and strategy of a separate economic science . . . is unjustifiably dogmatic' (1992a: 274). This conclusion would appear to be quite dramatic, given the centrality of Hausman's distinction for the message of the book.

What Hausman might be able to say coherently is a variation of a combination of claims [H3] and [H5]:

[H] The method of theory appraisal followed by economists is sound except for one major component in it, namely [U].

The final question that suggests itself then is this: is there a platform in Hausman's framework from which he can deliver such a normative judgement about [U]? Does his framework include the necessary resources needed for blaming rule [U]? Given the normative weakness of the framework, there is reason to doubt it. If such a doubt is correct, then Hausman cannot justifiably assert [H]. On the other hand, recall that I suggested that his normative grounds in support of 'economists' deductive method' seem to consist of the consistency of D1–D4 with established philosophical theories of confirmation. One now wonders whether the same kind of inference would speak against [U] and thus provide support to [H]. However, this option does not seem to be open for Hausman. This is because [U] is one of those principles that are also often cited as an element in many well-established philosophical theories of theory assessment: unifying power is typically cited as one of the virtues that good theories are expected to possess. Employing Hausman's normative inference to the justifiability of D1–D4 would therefore provide normative support also to the acceptance rather than rejection of [U]. If this reasoning is sound, Hausman is facing a major dilemma. Either he has to revise his strategy of normatively grounding D1–D4 or he has to provide an extra premise that would justify a differential treatment of D1–D4, on the one hand, and [U], on the other.

10 ECONOMICS, MATHEMATICS, AND NORMATIVE CONCERNS

Let us then return to Rosenberg's argument. As we have seen, he holds the view that economics, as we know it, cannot be an empirical science due to the conceptual link to folk psychology. We can now turn to his positive thesis about economics, his thesis about what economics is.

[R4] Not being an empirical science, economics is a form of mathematical politics.

The first thing to note is that the thesis about economics being 'mathematical politics' is actually divided into the two theses of economics being a form of political philosophy – more precisely, 'contractarian political philosophy' – and economics being a form of 'applied mathematics'. Let us begin with the first idea. Before formulating it, it is necessary to note that instead of talking about 'economics' Rosenberg here talks mostly about 'general equilibrium theory'; the latter is the term that he typically uses in this context – unlike in other parts of his argument. Thus:

[R4*] General equilibrium theory is a form of contractarian political philosophy.

An interesting feature of [R4*] is that it is offered as an explanation of why economists stick to general equilibrium theory. They stick to it, Rosenberg suggests, because 'it is already part of the best contractarian argument for the adoption of the market as a social institution' (1992: 220). He lists a number of elements in a standard perception of the market economy – coordination with some efficiency features rather than chaos – and concludes that '[t]hese are the kinds of considerations that seduce intelligent young minds from socialism to capitalism. Because they work, because they actually move people, we should accord them considerable respect in any account of why general equilibrium should have any claims on our attention' (1992: 219). This is because 'general equilibrium theory is the formalized approach to the systematic study of this claim about how the unintended consequences of uncoordinated selfishness result in the most efficient exploitation of scarce resources in the satisfaction of wants' (ibid.); even more, it gives 'the firmest theoretical foundations' for opting for the institutions of the market economy (1992: 221). The idea seems to be that the appeal of general equilibrium theory is explained by the appeal of a political philosophy, because 'general equilibrium theory is a species of formal political philosophy' (1992: 217). Rosenberg is here invoking the principle of inference to the best explanation: if [R4*] gives the best explanation for economists sticking to general equilibrium theory, then we have good reason to believe [R4*] is true.

There are a number of problems with this suggestion. First of all, the presumed commitment of economists to general equilibrium theory is underdetermined by the above considerations about the beneficial features of the market economy. General equilibrium theory is in no way *necessary* for a plausible account of a decentralized market economy capable of accommodating these considerations. There are other theories that suggest themselves as more plausible and more capable of accommodating the standard perception of the market economy. Accounts given by Friedman and Coase as well as versions of Austrian economic theory and constitutional economics are obvious candidates. Secondly, general equilibrium theory is not *sufficient* for accommodating all the features of the market economy listed by Rosenberg; sensitivity to innovations (p. 219) is an obvious example. Again, there are other theories that suggest themselves as superior candidates with regard to such individual features. Versions of evolutionary economics come to mind. Third, and not surprisingly, a subscription to general equilibrium theory is neither necessary nor sufficient for a subscription to a contractarian argument for the capitalist market economy. Just think of such major general equilibrium theorists as Lange, Lerner, Arrow, and Hahn.

Thus, the link between the explanans and the explanandum in Rosenberg's proposed explanation does not look like a very tight one. There has to be something defective in the suggested explanation if it is the case that those economists who most strongly believe in the contractarian argument for the market economy are not advocates of general equilibrium theory, and if many advocates of general equilibrium theory are not enchanted by the relevant version of contractarian political philosophy.

A fourth problem is that I am not convinced that Rosenberg has his explanandum right. One aspect of this difficulty is that I am not sure I understand what the explanandum phenomenon is supposed to be. Rosenberg variously puts it as 'the centrality of general equilibrium theory to the enterprise of economics' (p. 221, p. 216); the fact that economists 'find it attractive in the first place' (p. 201); the fact that 'economists are not about to give up' the theory (p. 220); the fact that 'economists continue to lavish attention on general equilibrium theory' (pp. 219–20). None of this is sufficient to make it clear what the explanandum is. Moreover, because no evidence is given for the claim that the explanandum fact obtains, we cannot use this evidence for an inference to a hunch about the fact itself. One actual fact about current economics may be that a decreasing amount, and already a very small amount, of research is being done on general equilibrium theory in the narrow sense that Rosenberg seems to have in mind. It appears to be a more or less exhausted theme, and economists are now behaving like good intellectual entrepreneurs searching for other avenues to theoretical innovation. I suppose this is relevant for Rosenberg's claim, and I even suspect it casts some doubt on his thesis – whatever his thesis actually is.

Finally, it is interesting to observe that Rosenberg is offering what he says is his best explanation of why economists have certain beliefs and that this explanation is given in terms of other beliefs and preferences. He is proposing *a folk psychological explanation* for the popularity of – or some such fact about – general equilibrium theory. As we have seen, elsewhere in the book he argues that folk psychology is non-explanatory – or at any rate not progressively explanatory.[21] *Rosenberg's main argument implies that this argument cannot succeed* – or at any rate cannot be systematically better than any other rival argument. This creates an unresolved – and possibly unresolvable – tension of self-reference in Rosenberg's account.[22]

Consider then the second part of [R4]. In this context, Rosenberg mostly makes claims about 'economics' or 'economic theory' rather than 'general equilibrium theory'. Thus:

[R4**] Economic theory is a form of applied mathematics.

Again, there is no guarantee that the reader will understand the precise import of this claim. About the 'mathematics' part, Rosenberg says variously that 'economics is a branch of applied mathematics' and that it is 'a branch of abstract mathematics' (1992: 237) and that it is 'somewhere in the intersection between pure and applied axiomatic systems' (p. 247). The closest he comes to a precise idea is when he says that 'economic theory is a branch of applied mathematics, the study of mathematical properties of transitivity among elements in a set' (p. 220). No further definitions are given, so the reader has every right to feel somewhat insecure about what is precisely being suggested.

What is it that would make it justified to say that economics 'is mathematics' or that it 'is a branch of applied mathematics'? Just like other disciplines, economics applies mathematics, but this cannot be the justification. So it is not open for Rosenberg to join those who claim that economics 'applies too much mathematics' or is 'too mathematical'. No, the claim is stronger: economics belongs to the category of mathematics. Rosenberg is not clear about what this means and why this would be true. Now if economics belongs to the category of mathematics, this has to be because economics shares with mathematics some essential characteristics. I was able to find three candidates for such essential characteristics in the book. One is that 'like other applied mathematicians, economists are relatively unconcerned about the factual application of their most central theoretical accomplishment' (p. 220). The second is that the development of economics has been 'insulated from empirical influences' (p. 247). The third is the 'derivational, deductive structure' of economic theory (p. 237). Of course, none of this is sufficient for substantiating the idea that economics *is* mathematics. Irrespective of whether economics in fact has all these features, there is no conceptual obstacle to these features being possessed by intellectual endeavours outside mathematics. In other words, the three features are not sufficient for economics being a part of mathematics.

Indeed, it would appear that the status of the above three features is not one of *defining characteristics*. They are rather supposed to be *manifestations* of the fact that economics is mathematics. Just like in the case of [R4*], Rosenberg offers [R4**] as an explanation for alleged features of economics such as the three listed above. However, the problem still remains that no independent characterization is given for economics being a branch of mathematics. What is it that the above three features manifest? What is it that *makes* economics a branch of mathematics? Another problem, of course, is that there are contending explanations of such manifest features of economics, such as those referring to parts of economics as consisting of logical and conceptual research on models, and other parts as involving partial theories facing uncontrollable complexity and as subscribing to the principle of explanatory unification or the like – to cite a couple of Hausman's ideas. Once again, it seems evident that the alleged explananda of the argument where [R4**] functions as an explanans, are severely underdetermined by the presumed explanans.[23]

Yet another feature of Rosenberg's argumentation deserves to be mentioned. It must be found annoying by most economists to read Rosenberg's over-aggregated claims about economics. It has to be hard for them to swallow claims such as 'it is often claimed that economics is a body of prescriptions on how to be rational' (p. 216) or that in the 'discipline' of economics advancement has 'consisted in improvements of deductive rigor, economy, and elegance of expression, in better axiomatizations, and in the proofs of more and more general results, without much concern for the usefulness of these results' (p. 244), or that for 'most economists . . . the really important question . . . was whether Walras's theorem that a general market-clearing equilibrium exists, that it is stable and unique, follows from the axioms of microeconomic theory' (p. 245). Some of economics – and much of the most prestigious economics – can no doubt be characterized in these terms, but to suggest that this is so about all or most (or even one fourth) of it, will surely not be accepted by the economics profession, not by those working in academia in the 1990s nor by those working in business and government.

This is related to Rosenberg's overall argumentative strategy. He began with the presumption that [R1] (or [R1*]) is what economists subscribe to. He then claimed that economics is unable to live up to [R1], and then suggested that economics is better characterized by [R4*]. This reasoning gives rise to the following two doubts. On the one hand, there is a strong tradition in economics – from Smith through Hayek to Buchanan and others – which does not subscribe to [R1] but does subscribe to something like [R4*] (I am not saying these economists accept precisely [R4*], since many of them do not endorse general equilibrium theory; they are willing to attribute something like what is attributed by [R4*] to general equilibrium theory to their own favoured theories.) By forgetting about this tradition in his premises, Rosenberg is able to end up with a misleadingly surprising outcome about the nature of economics. Given that economists in this tradition do not typically strongly adhere to general equilibrium theory, Rosenberg is at least half wrong about these economists. On the other hand, those economists who do subscribe to something like [R1], are often also unenthusiastic about general equilibrium theory, such as monetarists like Friedman or Keynesians like Klein. Again, Rosenberg is half wrong about these economists.

Finally, it needs to be added that I do believe that links of economics with both mathematics and political philosophies will have to play important roles in a comprehensive account of some of the important features of economics. The above remarks suggest that to establish the – most likely somewhat complex – nature of these links, the arguments need to be far more refined than those offered by Rosenberg.

11 CONCLUSION

The criticisms I have submitted should be taken as an indication of how difficult it is to do philosophy of economics. One is required to have a deep knowledge of both economics and philosophy, and combine the two in a creative and rigorous fashion. This is almost too much to ask from one person. This fact makes me somewhat suspicious of the art of portrait painting when the subject is supposed to be the whole or a major part of the discipline of economics and when the painter is a single person. The project can be made more collective at least in two ways: engaging oneself in collaboration and intensifying the practice of criticism. I have tried to do my part by offering respectful criticisms on two ambitious – maybe overambitious – projects.

Of course, the two books under review are not literally about economics as a whole, their misleading titles notwithstanding; they are about parts of traditional neoclassical microeconomics.[24] Yet, they are about those parts of economics which the authors controversially believe – but do not systematically argue – to constitute the foundational core of economics. I feel that too much is being attempted, and that this may account for at least part of the shortcomings that I have tried to identify. It is all right if philosophers want to take the risk and try to provide grand assessments of entire scientific disciplines. However, given the current situation in the study of the nature of economics, I tend to think that the best favour that philosophers could offer to economists and economic methodologists is to clarify many of the most fundamental concepts that tend to be employed in these endeavours, such as explanation and prediction, theory and model, law and tendency, testing and confirmation, truth and relevance. Hausman goes some way to clarify some of these concepts.

One general impression about Rosenberg's book is that it has not been written and edited with sufficient care, it is rather a provocative yet interesting impressionistic sketch.[25] Perhaps my main discomfort with the book is due to a perception of a disproportionality between theses and arguments. Rosenberg's arguments are too weak to support his theses; in other words, his theses are far too strong in relation to the strength of his arguments. All of this on top of the problem that neither the theses nor the arguments are articulated in sufficiently precise terms. With such characteristics, the book is able to serve as an intriguing source of inspiration.

Hausman does well in employing a number of relevant concrete cases in economics and analysing them in some detail to illustrate and support his arguments – even though the connection does not always look perfectly straightforward. The book is extremely rich not only with these illustrations but also with critical and insightful discussions of a variety of methodological positions. The problem may be that the book is too rich. The framework and the detailed organization of the argument may have suffered from a congestion of materials. It is my opinion that *The Inexact and Separate*

Science of Economics is the best philosophical account of traditional microeconomics written thus far. The portrait it offers of economics is painted in relatively solid lines with some resemblance to the model – even though the left ear and the right eye are not quite in place, and not all organs are tightly connected to the main body of the subject.

Rosenberg portrays economics as unimprovable, while Hausman's portrait depicts it as improvable. If I were to use such a dichotomous pair of attributes about the two portraits themselves, I might, in my less self-controlled moments, conjecture that Hausman's portrait is improvable, while Rosenberg's is not – for reasons somewhat analogous to those that Rosenberg himself applies to economics.

Erasmus University Rotterdam

ACKNOWLEDGEMENTS

Earlier versions of this paper have been presented to audiences at the AEA meetings in San Francisco, January 1996, at the University of Amsterdam, December 1995, and at the Finnish Postgraduate Program in Economics workshop on economic methodology, June 1995. Special thanks for extensive and very helpful written comments go to Daniel Hausman, Roger Backhouse, Bruce Caldwell, John Dupré, Tom Mayer, Alan Nelson, Koen Frenken, Maarten Janssen, Jack Vromen, Wade Hands, and Kevin Hoover. Financial support by the Yrjö Jahnsson Foundation is gratefully acknowledged.

NOTES

1 Books can be read in many ways. My early encounters with these two books generated fascination and puzzlement that resonated with my intuitions in an unarticulated fashion. It was only upon later and more detailed readings that I began to understand why I was fascinated and puzzled – and to see the extent to which the initial reception was justified.

2 Part III of Hausman's book, 'Conclusion', is more critically prescriptive than the rest of the book. However, the critical proposals provided there do not seem to be entirely smoothly linked with the methodological framework outlined in the earlier parts of the book: hence the inescapable feeling of a gap that strikes the reader. As Backhouse says, 'his criticisms of economics are in a sense hanging in the air, for they do not follow from a methodological critique' (Backhouse 1995: 119).

3 On the same page, Rosenberg lists a third criterion along with the other two: 'the proportion of right predictions to wrong ones and the precision of the predictions [the theory] makes, along with *the amount of surprise generated by its predictions*' (emphasis added). He then goes on by saying that the first two 'are especially important for economics', thus implying that the prediction of 'novel facts' is not. Indeed, the element of 'surprise' or 'novelty' does not play a role in Rosenberg's argument about economics. As Rosenberg himself explicitly puts it, '[f]or the purposes of this book' he adopts a stipulation that lacks the element of 'novelty'. This seems to have escaped Rappaport's attention. He

(1995: 139–45, 146) seems to misrepresent Rosenberg's position by relying too much on this third element in his interpretation and critique. To avoid apparent failure, Rappaport should adjust his arguments to accommodate the explicit exclusion of novelty by Rosenberg.

4 Here I disagree both with Rosenberg and with Rappaport. Rappaport (1995: 145) agrees with Rosenberg by saying that it 'is true' that policy relevance presupposes predictive progress, while I think it is false to link the two in this way. Thomas Mayer (1995: 113) puts it with a smart epigram: 'In shifting the focus from the question whether economists can predict to the question whether their predictive capability is increasing, Rosenberg has confounded a stock and a flow.' There is a more general point to be made. On a number of polemical occasions, Rosenberg has relied heavily on the idea of economics as a policy-relevant discipline. What I have found wanting in these arguments is an analysis of how precisely economic theory is supposed to relate to economic policy. This is a very complicated issue and no simple answer should be taken for granted.

5 One problem worth noticing is that it is not clear what sort of 'forcing' would be needed to generate the presumed majority opinion. This is not irrelevant, since there is no doubt that economists can also be misled to express views that do not correspond to what they actually do in their research practice. This is witnessed by the history of a variety of metatheoretical ideas that economists have held, influenced as they have been by changing philosophical fads. It is their research practice itself and the metatheoretical commitments that closely accompany it, implicitly or explicitly, that should be carefully studied before any strong claims about economists' endorsements can be made.

6 Hutchison cites the following three factors as generating increased instability in the economy: '(a) more rapid rates of technological change; (b) greater world-wide political and economic interdependence, producing such phenomena as world wars, oil-shocks, and world-wide stock-market collapses; (c) higher levels of more haphazard, "luxury" spending, more unpredictable than subsistence expenditure, in the more advanced economies'.

7 These two do not seem to exhaust Rosenberg's usages. At least one more – a third – meaning of '*generic* prediction' appears in the book, this one concerned with possibility, namely 'derivations of whether conditions of a certain sort can or cannot obtain' (Rosenberg 1992: 226).

8 As Thomas Mayer (1995: 108) says, 'If weather forecasts were to tell us only that tomorrow will be hotter than today, without saying by how much, they would still provide useful information.'

9 For an example of somebody who is uncomfortable with it, see Rosenberg (1992: 210).

10 Note that Hausman explicitly rejects – for philosophical reasons – the employment of modal categories such as that of possibility (footnote to p. 73).

11 Rosenberg has the habit of formulating the folk psychological scheme of accounting for people's action – also called the 'practical syllogism' in the relevant literature – in terms of 'desire and belief'. The generalized usage of 'desire' is problematic here, given that action may well be animated by mental states such as a sense of duty or obligation that may be devoid of the element of desire. A more neutral notion would seem to be needed to designate this component of the account, such as 'intention' or 'goal'. Note also that 'preference' typically designates such a neutral concept, lacking the connotation of 'desire'. See Mäki (1991) for a discussion of the practical syllogism in the context of economics.

12 The idea of a conceptual continuity between economics and common sense may be generalized to cover other items than just the mental elements of folk psychology –

such as firms and households, exchange and price, money and inflation, interests and taxes, etc. Interesting philosophical peculiarities of economics seem to follow from this observation. See Mäki (1995); for a suggested modification regarding macroeconomic entities, see Hoover (1995a, b).

13 Allin Cottrell's (1995) strategy of criticizing Rosenberg's views includes identifying two claims, the 'vacuity thesis' and the 'non-improvability thesis', and arguing that both are false, basing the arguments on Dennett's notion of 'intentional systems theory'. I am inclined to agree with Cottrell on both counts: folk psychology (or 'intentional systems theory') both indicates predictive success and is improvable.

14 It can be argued that it is these different composition principles theorized in 'scientific economics' that often mark a major departure from the conceptual fundamentals of 'folk economics' (see Mäki 1995). For an argument for why the practical syllogism – the folk psychological intention/belief scheme of explaining individual action – is not sufficient for explaining social outcomes see Mäki (1991).

15 Hoover (1995a: 730) proposes a radical twist to Rosenberg's argument, suggesting that 'Rosenberg should have drawn a different conclusion: the intentional character of economic behaviour, because it limits the precision of prediction and explanation at the individual level, demonstrates [that] the microfoundations of macroeconomics . . . is impossible.'

16 It is mechanisms such as these which often escape the conceptual resources of 'folk economics' (Mäki 1995). See note 14 above.

17 This may violate some innocent and intelligible intuitions, such as the idea that partial descriptions may be exactly correct about the parts of the world that they are about (see Mäki 1994). On the other hand, if the premises of economic theories are construed as universally quantified conditional statements, they may be true only if qualified by the clause; otherwise they tend to be false.

18 In his comments on an earlier draft, Tom Mayer writes: 'The case you make may be stronger than the evidence of published papers indicates. A number of papers that violate the rule may never make it past the referees. Moreover, such a rule may be influential in the selection of research projects.'

19 Lacking a definition of 'credible', I am inclined to doubt that what we have here is yet another surrogate concept for something we have a difficulty with expressing clearly. Economists have the habit of using terms such as 'valid' and, increasingly, 'reasonable' in similar contexts without anything like a clearly definable meaning in mind.

20 It should be noted that [U] is not a *Fremdkörper*, an alien element, in a deductive method; on the contrary, [U] is a rule very much in line with a deductivist approach to theorizing.

21 There is furthermore a problem with the way Rosenberg phrases his core idea here. He says that 'general equilibrium theory is best viewed as one important component in the research program of contractarian political philosophy' (1992: 220). This means that he is suggesting that viewing an economic theory as a *component in a philosophy* explains why *economists* stick to this theory! This is somewhat implausible. Some other formulation would be needed to make it even minimally plausible.

22 Of course, Rosenberg may maintain that his folk-psychological account of the beliefs and preferences and behaviour of economists is not intended as a 'scientific' account, and that there are criteria – even though he does not tell us what they are – for assessing the relative merits of such 'non-scientific' accounts.

23 In a limited sense, Rosenberg and Hausman are close to one another on the question of the relationship of microeconomics to mathematics. Recall Hausman's distinction between models and theories and the idea that much of

research in economics is on models only and therefore has a conceptual or mathematical character, insulated from evidential input. This idea of work on models is close to Rosenberg's idea of economics as applied mathematics. The difference between the two is, of course, that Hausman thinks economics also has empirical theories based on but not reducible to the non-empirical models.

24 Hausman reminds the reader about this at the outset: 'I shall usually omit the adjective "neoclassical" and just speak of "economics" when I am discussing neoclassical economics' (1992a: p. 3). Yet, when drawing a conclusion on a later occasion, he seems to forget about this: 'Economics resembles individual theories such as Newtonian dynamics or Mendelian population genetics more closely than it resembles disciplines such as physics or biology. For an orthodox theorist, it is in effect a one-theory science' (Hausman 1992a: 95). If one defines neoclassical economics in terms of 'equilibrium theory' as Hausman does, then, by implication, it is a one-theory field like Newtonian dynamics or Mendelian population genetics.

25 This impression is further confirmed by the scandalous number of misspellings of various individuals' names, beginning with 'Alchain'.

REFERENCES

Backhouse, Roger (1995) 'An empirical philosophy of economic theory', *British Journal for the Philosophy of Science* 46: 111–121.

Cottrell, Allin (1995) 'Intentionality and economics', *Economics and Philosophy* 11: 159–76.

Hausman, Daniel M. (1981) *Capital, Profits, and Prices. An Essay in the Philosophy of Economics*, New York: Columbia University Press.

——— (1992a) *The Inexact and Separate Science of Economics*, Cambridge: Cambridge University Press.

——— (1992b) *Essays on Philosophy and Economic Methodology*, Cambridge: Cambridge University Press.

Hoover, Kevin (1995a) 'Why does methodology matter for economics?', *Economic Journal* 105: 715–34.

——— (1995b) 'Is macroeconomics for real?', *The Monist* 78: 235–47.

Hutchison, Terence (1992) *Changing Aims in Economics*, Oxford: Blackwell.

Mäki, Uskali (1991) 'Practical syllogism, entrepreneurship, and the invisible hand: A critique of the analytic hermeneutics of G.H. von Wright', in Don Lavoie (ed.) *Economics and Hermeneutics*, London: Routledge, pp. 149–76.

——— (1994) 'Isolation, idealization and truth in economics', *Idealization in Economics*, edited by Bert Hamminga and Neil de Marchi, special issue of *Poznan Studies in the Philosophy of the Sciences and the Humanities* 38: 147–68.

——— (1995) 'Scientific realism and some peculiarities of economics', *Boston Studies in the Philosophy of Science* 169: 425–45.

Mayer, Thomas (1995) *Doing Economic Research: Essays on the Applied Methodology of Economics*, Aldershot: Edward Elgar.

Rappaport, Steven (1995) 'Is economics empirical knowledge?', *Economics and Philosophy* 11: 137–58.

Reuten, Geert (1993) 'Not necessarily unjustifiably dogmatic', unpublished.

——— (1996) 'A revision of the neoclassical economics methodology: appraising Hausman's Mill-twist, Robbins-gist and Popper-whist', *Journal of Economic Methodology* 3: 39–67.

Rosenberg, Alexander (1976) *Microeconomic Laws*, Pittsburgh: University of Pittsburgh Press.

——— (1992) *Economics – Mathematical Politics or the Science of Diminishing Returns?*, Chicago: University of Chicago Press.