

On the Best Strategy for doing Philosophy of Economics *

Hausman's book offers a very new approach to writing in the philosophy of economics. It attempts to break new ground in two directions, in philosophical analysis and in economic theory. This is a rather ambitious approach and at the same time a very risky one. Failure in either direction will jeopardize both objectives.

As many philosophers of science have undoubtedly discovered already, an analysis which would satisfy philosophers is too often also one which would not impress the scientists in question. This is not to mention the additional obstacle presented by disagreements amongst the philosophers which may mean that even some of them will not be satisfied. If one's objective is to impress philosophers then failure to satisfy the scientists may not be a serious drawback. I say "may not be" because there is one case of unimpressed scientists which should be of concern to the philosopher – namely, the case where the scientists are dissatisfied because the philosopher has not accurately represented the scientists' ideas or theories. Most of the recent literature about the philosophy of physics or of chemistry probably would satisfy the average physicist or chemist. However, the recently growing literature on the philosophy of economics written by philosophers has not been sufficiently accurate to satisfy most economists – and for this reason most of this literature has not made any measurable impact on the economics profession.

On the question of accuracy, Hausman's book stands in contrast to recent philosophy of economics literature. Unfortunately, accuracy is not enough since an equally important problem facing anyone writing in the philosophy of economics is one that concerns the appropriate strategy for achieving one's objectives. If one wishes to convince economists that there are significant philosophical problems within economics that need to be faced or solved, then it is wise to centre the discussion on the mainstream of economics so that economists will be willing to consider those problems. If one wishes to convince philosophers that there are some interesting philosophical problems in economics then one should try to follow closely the "currents" of mainstream philosophy. In both cases the presumption is that the respective mainstreams are well defined. In the case of economics, the mainstream is the overwhelmingly dominant and easy to define "neoclassical theory". As for the philosophy of science, analytical philosophy still has a strong foothold although it is now quite standard to have it strongly coloured by various aspects of Popper's contributions.

In his preface Dr. Hausman says that he wishes to show "what contemporary philosophy of science can contribute to understanding and solving the puzzles" presented by the economists' theories of capital and interest. And, he also wishes to show "what understanding capital and interest theory can contribute to philosophy of science". So, from the very beginning Hausman has set out a task which could succeed only if he follows both mainstream courses. Unfortunately he does not. By focusing on capital and interest theory he has ventured away from the mainstream of neoclassical economics and thus will not likely succeed in convincing mainstream economists that there are philosophical tools that they may want to employ. His primary purpose, however, is to convince philosophers of science that they need to create new philosophical tools to investigate the philosophy of economics – and most importantly, that economics is worthy of philosophical analysis.

Since he seems to have fashioned his book in a manner that is appropriate only for an audience of philosophers of science interested in studying economics, we might be able to understand why he introduces capital and interest theory as a mere case study to demonstrate the virtues of his new tools. The question to be examined later in this review is whether one can accomplish much by demonstrating a new philosophical tool using a topic of little contemporary interest to the theorists in question. It will be argued that despite Hausman's generally competent representation of the state of capital and interest theory, not much is accomplished.

Hausman begins by presenting three different views of capital and interest: "Austrian theories", "general equilibrium theories", and "Sraffa's work". By examining these three views he wishes to convince us that economists "possess no good theory of capital or interest, or of their relations to equilibrium prices" (p191). Of course, the key element here is what he thinks constitutes a "good theory" – he explains this in Chapter 7.

By "good theory" Hausman does not mean "true theory" since he tells us that (in the case of the available theories of capital) we are "not in any position now to decide what is the truth" (p193). For him, then, the key question is how theories consisting of apparently false premises can be explanatory. To answer this

question he (I think quite correctly) avoids developing a model of "messy" explanation (e.g., involving stochastic decision theories). Instead, he endorses the "standard deductive-nomological model of nonstatistical explanatory arguments" (p203). In avoiding "messy" explanations he recognizes that one must come to grips with some means of assessing "simplifications", "idealizations", and "inexact laws". He proposes four specific criteria to determine when simplifications or idealizations are "justified": (1) confirmation condition (simplification must lead to confirmable consequences); (2) no-accident condition (every confirmation is non-accidental); (3) sensitivity condition (relaxing any simplification will lead to more accuracy); and (4) convergence condition (whenever a simplification is a better approximation, one's explanation is more accurate). Obviously, one cannot expect Hausman to justify these criteria since that might lead to circularities or an infinite regress. Nevertheless, he claims they "seem consistent with the judgements scientists make" (p205).

Since he employs the deductive-nomological model he has to continually be concerned with "laws" or "lawlike" assumptions. For example, what if the "apparently false premise" in question happens to be one of the "laws" needed in the model of explanation? To deal with this problem he introduces four more criteria to justify when an apparently false generalization can be used as a "law": (5) lawlikeness; (6) reliability (disconfirmations are rare); (7) refinability (capability to produce more confirmations and less disconfirmations); and (8) excusability (disconfirmations can be explained away). Even equipped with all of these criteria one still needs to face the fact that the deductive-nomological model cannot deal with the alleged real intent of many economic model-builders – namely, to identify the "causes" of particular economic events. So equipped he concludes that "economists lack rational principles upon which to make [causal] judgments" (p207).

Although Hausman claims to be putting the worth of these philosophical criteria to test by examining various limited theories of capital and interest, he does draw some conclusions about economics in general. The general conclusions are based on his posited axiomatic analysis of what he thinks is the core of neo-classical economics – on what he calls "equilibrium theory".

Before I turn to an evaluation of Hausman's representation of standard economics, let me state the fundamentals of neoclassical economics. The research program of neoclassical economics is fairly easy to specify. The task for any theorist is to construct an acceptable explanation of all non-natural and non-psychological phenomena as the consequences of (universal) maximization. The obvious things to be explained are the level of supply of any good and the level of its price. Some economists even try to explain such diverse events as marriage, charity or capital punishment – that is, institutional rules and arrangements of all types. To be acceptable every explanation must include one particular behavioural assumption – namely, the assumption that all decisions are made in order to maximize according to a given objective function such as a profit or a utility function. There are, however, some hidden clauses here. Only individuals can make decisions or choices. If a price is to be explained as the consequence of clearing a market – that is, as an equilibrium price – then the market process must also be explained as the consequence of individual decision-making. Any market is in equilibrium only if all participants are maximizing their individual objective functions. In other words, the state of equilibrium is an epiphenomenon since it is an "unintended consequence" of the self-interested acts of individual decision-makers. If any individual is not maximizing his or her objective function then the market cannot be in a state of equilibrium since either demand exceeds supply or the reverse. Whenever the demand does not equal supply at least one individual is either not buying or not selling the quantity that he or she should have been in order to be maximizing his or her objective function. This is simply because the phenomena of demand and supply like all other social phenomena are explained as the consequences of maximization.

Economists call the things they wish to explain "endogenous variables" and distinguish them from the natural or psychological givens (or constraints) which they call "exogenous variables". One might (erroneously) say that economists claim that the states of the endogenous variables are "caused" by the state of the exogenous givens. This would be misleading. Actually, whenever the givens change, the states of the endogenous variables change as logical consequences of the revised maximization decision due to the revised logic of the entire situation facing each individual. In economics, the common idea of a causal explanation is purely a thought experiment in which it is presumed that the state of only one exogenous variable changes while all others are artificially held fixed (economists call this "comparative static analysis"). But in reality the effect of changing one exogenous variable cannot be completely attributed to that one variable since the nature of the effect itself depends on the state of all other exogenous variables.

Had the other variables been fixed in a different state then it is possible that the effect of the given change in the specific exogenous variable might have been quite different. What this means is that in the typical neoclassical explanation there usually is not a single "cause". Instead, we would have to say that all exogenous variables "influence" the effect in question. In economics then, the philosophical idea of a single "cause" is usually out of place.¹

What makes an economics explanation interesting is that economic theorists would have us believe that while generally observable phenomena such as prices are not caused by any particular individual, all individuals influence the level of all prices. Also, since every individual depends on some given prices (to calculate the benefits or costs of various possible actions), the specific decisions of every individual are indirectly influenced by the decisions of all other individuals. Such indirect influence is due entirely to the impersonal markets in which all individuals are thought to participate. In this neoclassical conception of the world, general equilibrium (i.e., a situation where all markets are simultaneously in a state of equilibrium) is possible only if all individuals are actually maximizing their respective objective functions. This means that for neoclassical economists the two observable variables on which Hausman focuses his attention – capital and its price (the interest rate) – must be explained as the consequences of maximizing choices or decisions.

For the most part Hausman's presentations of the above fundamentals and the various versions of capital theory are quite competent, indeed. Unfortunately, unless the reader is already quite familiar with the deeper theoretical problems involved it is unlikely that the reader will appreciate the high quality of his survey of capital and interest theories. This is not to say that his presentations are perfect because in many cases they are not as he misses the point of some of the more important models in the literature (such as the excellent paper by Oscar Lange). He claims that Lange, like all equilibrium theorists, does not succeed in explaining interest but this claim is based on a presumption that Lange was trying to explain the interest rate as only a phenomenon of a hypothetical state of equilibrium. Actually, Lange explains the level of the interest rate as being a measure of the extent of a given disequilibrium and he also explains why there is a disequilibrium. Similarly, Hausman claims that Piero Sraffa's explanation of prices (that they are logical consequences of an exogenously given income distribution) is somehow "too limited" on the grounds that the basis of the income distribution is treated as "exogenous". This is considerably misleading since, as Hausman seems to appreciate, all economics models must have at least one exogenous variable. What Hausman fails to explain is why Sraffa's exogenous variables are any less legitimate than those presupposed by neoclassical economists.

One of the difficulties with most philosophers of economics is that they tend to dwell on historical relics rather than on active concepts in contemporary economics. The study of the history of economic thought (unfortunately) is no longer an essential part of any economics curriculum. For this reason it is difficult to understand how the ideas of such writers as John Stuart Mill could be seen to have direct relevance. Nevertheless, Hausman is fond of quoting Mill concerning questions of scientific methodology. Unfortunately he fails to stress the one lasting legacy that can be directly attributed to Mill. Namely, the view that all successful explanations in social science must be reducible to psychological laws. In effect, Mill's methodological prescription that says that the only acceptable exogenous variables are the psychological states of the individual decision-makers. I think this prescription which is unfortunately still taken for granted by itself accounts for virtually all of the significant philosophical problems in economics.

Another difficulty with many philosophers of economics is that as critics of mainstream economics they often do not recognize that economists have many more clever ways to accomplish their research program than they are given credit for. Virtually all equilibrium models in neoclassical economics presume (as does Hausman) that the production process exhibits "constant returns to scale" (which only means that whenever all inputs increase by a fixed proportion, the output of that process increase by the same proportion). In all such models, as a matter of elementary mathematics, the equilibrium theorists are always right – capital can be treated as just another input whenever all other inputs are being used efficiently (i.e., profit or surplus is maximized with respect to each inputs). This is obviously a question that cannot be decided here. Nevertheless, it should at least be noted that many neoclassical economists will not be impressed with Hausman's critical conclusions about the prevalence of inherent problems with capital and interest theory – since mainstream economists generally do not see any obstacle to providing a long-run explanation of the supply of capital and its price (the interest rate).

If the philosophy of economics is in any way an attempt to explain why economic theorists do what they do, then Hausman's case study of capital and interest theory cannot possibly accomplish much. Economists today are not concerned about formal questions of "lawlikeness". The question of whether or not the assumption of universal maximization is formally "lawlike" is of no significance to understanding the concerns of economic theorists.² Today economic theorists are more concerned with constructing an individualist explanation of the persistence of "disequilibrium" without in principle denying the truth of the presumption of universal maximization. This requires some very clever moves to avoid self-contradictory explanations. The usual way of reducing every explanation to exogenous psychological states is accomplished by seeing every decision to be one made from a perspective of a "long-run" equilibrium where everything but the psychological states are thought to be endogenous variables. Unfortunately, if one is concerned with "disequilibria" it is easy to see that such "long-run" explanations are irrelevant (since whenever so much time is allowed there is nothing exogenous to prevent the achievement of equilibrium).

The primary methodological question facing equilibrium theorists is: which variables can we accept as legitimate exogenous variables in any economics explanation? What bothers neoclassical economists about the work of Sraffa is that his exogenous variable (either the given income distribution or the wage-profit ratio) is not a psychological state of nature – it is a sociological event. What Austrian economics has put into question is the typical neoclassical presumption that the basis of the decision-maker's knowledge is psychologically exogenous. Austrian economists would have us be more concerned with substantive questions about what is assumed about the decision process of the individual decision-makers. To the extent that every decision made depends on the expectations of the decision-maker, economists today are far more concerned with whether or not the formation of one's expectations can be explained in the same way one's decisions are explained.

If mainstream economists do not see a problem with capital or interest then the questions of capital or interest cannot be central to economics. If so, why should philosophers of economics be worried about such questions? If one is going to explain the workings of mainstream economics then one must begin by examining the problems that concern mainstream economists today.

FOOTNOTES

* Review of Daniel M. Hausman [1981]: *Capital, Profits, and Prices: An Essay in the Philosophy of Economics*. New York: Columbia University Press. \$31.20 (U.S). Pp. xiv+253.

1. Unfortunately, Hausman's strategy of dealing with this problem by using terms such as "causal factor", "causal influence", and "causal condition" only creates another confusion by using the word "causal" to identify something which is supposed to be a "non-cause".

2. He does recognize that most economists do not talk in terms of "laws" or "lawlikeness" but he seems to think that the widespread use of "models" amounts to the same thing. If this is why he spends so much time discussing equilibrium models, then this is a mistake since most economists are using models in the engineering sense rather than the analytical philosophy sense.