Philosophy Of Economics

Quo Vadis, General Equilibrium?

Predictive and Explanatory Power Rosenberg (1994) discusses the "cognitive status"—namely the aim and method—of economic theory.[1] Rosenberg defines economic theory as "a body of contingent laws about the causes of choice behaviour, and its aggregate consequences" (217).

Since the demise of falsifiability as a demarcation criterion between scientific and unscientific theories, Rosenberg notes, the status of economic theory cannot depend on its lack of falsification. No theory can be (strictly speaking) falsified. Yet,

> [this] will not explain the attachment of economists to the theory, because it is just not a very good explanatory theory of the causes and consequences of economic choice (ibid.).

Why is economics not explanatory? Because it has scarce predictive power[2]:

> Economic theory seems permanently stuck at the level of generic predictions – predictions that tell us that some change will happen some time and some place, without ever telling us when and where and how of a change will occur. Economics tells us that after a freeze a rise in orange-juice prices will occur, or that the demand for college education is not very elastic. But it never seems able to improve these predictions in the direction of exactly how much prices will rise or what the coefficient of inelasticity will be. [...] a theory that cannot predict cannot explain – or at least we have no way of telling whether its explanations are correct. (ibid.)

What could then be the non-explanatory rationale for the continuing commitment to neoclassical microeconomics? Rosenberg makes two proposals, namely that (1) economics is fundamentally normative—rather than positive, or descriptive—and that, if non-normative, (2) economics is just a branch of applied mathematics—and thus lacks empirical content. Either way, economics has no explanatory value.

Political philosophy Economics may be normative in various ways. The most obvious one is that it is "a body of prescriptions for how to be rational, and rationality is taken to be a normative concept" (218). A second, more interesting interpretation, according to Rosenberg, is that economics is normative because it is concerned with the justification of a particular view in contractarian political philosophy: a decentralized economy motivated by self-interest is desirable because it is conducive to the optimal satisfaction of the individuals' need for coordination, efficiency and equity. With the exception of public goods, a decentralized market scheme is better than central planning.

Since the propositions of normative economics are proven in the microeconomic framework of general equilibrium theory, general equilibrium theory becomes, and is justified as, "a species of formal political philosophy" (218), relevant to the choice *of* (optimal) constraints rather than to the (explanation of the) choice *within* constraints (221):

> [...] 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. It is of course an enquiry with many limitations. But at least now we can understand why economists continue to lavish attention on general equilibrium theory. It is not because they think it can be improved in the direction of a descriptively and predictively adequate explanation of economic activity, but because they believe it is already part of the best contractarian argument for the adoption of the market as a social institution. (220)

[1] The article summarizes arguments developed more at length in (Rosenberg, 1992).

[2] This view differs from Friedman's. Friedman seems to claim that high predictive power is sufficient for explanation. Rosenberg, instead, claims that predictive power is necessary for explanation.

Yet, if it is to inform *actual* choices, a theory of how one *ought to* behave should at least be descriptively adequate in the minimal sense that it predicts that one *can* so behave:

> If we ought to adopt institutions that approach those of the market that general equilibrium describes, then it must be the case that we can do so. But we have no assurance of this possibility unless economic theory has a certain amount of explanatory and predictive power. If the actions it counsels are beyond us it is irrelevant as moral philosophy. If rational choices are within our abilities, then the fact that we do not seem to engage in these activities fully enough to give the theory much empirical warrant must reduce its normative bearing as well. (222)

To the extent that one can*not* assume that agents are capable to "ruthlessly maximize their utilities everywhere and always", to "dissimulate when it is to their advantage", to "free-ride where they can", etc. (221), one should *not* recommend the institution that promotes optimality based on such assumptions.

Applied Mathematics For most economists, however, treating economics as just a kind of political philosophy is dissatisfying. Economics is supposed to have empirical content. But this view is problematic, for Rosenberg, because of the intentional foundations of economics.

Economists assume that economic laws are derivable from the assumption that the ultimate causes of economic behaviour are preferences, namely a kind of *desires*, and expectations, namely a kind of beliefs. In turn, 'belief' and 'desire' are categories of folk psychology. Unlike 'gene', 'acid' or 'electric charge', which are natural kinds, and thus respectable scientific kinds, 'belief' and 'desire' are intentional kinds. Hence, belief-and-desire generalizations do not carve nature at its joints, "in so far as its joints are revealed in already successful theories like those of neuroscience" (224). As a consequence,

> [beliefs and desires] cannot be brought together in causal generalizations that improve on our ordinary level of prediction and control of human actions, let alone attain the sort of continuing improvement characteristic of science. (ibid.)

What about those successful predictions of economics, which do rely on the categories of belief and desire? What explains the successful application of, say, the laws of supply and demand, which are a consequence of individual choices, preferences and beliefs? There is no mystery here, for Rosenberg. Economics may be successful even if false or vacuous, in the same way Euclidean geometry is.

There are no Euclidean natural kinds, so the theory of general relativity says. Still, the theorem of Pythagoras applies with reasonable approximation. Analogously, there are no "rational" agents (in the economists' sense). Still, the laws of supply and demand may apply well enough. The difference in applicability is only in degree. However, there is also a qualitative difference between Euclidean and economic kinds. There is no way to improve on the precision of economic predictions in the same way one would improve on geometrical predictions, because there is no associated (true) theory of economic behaviour that specifies the divergence between false and true predictions and the kind of corrections to be made.

> [Euclidean geometry and economic theory] differ in applicability only by degree, the predicate of neither pick out natural kinds; but they differ in kind because for Euclidean geometry there is a theory, physics, that enables us to correct and improve the applicability of its implications. There is no such theory that enables us to improve on the applicability of economic theory. (227)

Perhaps other sciences could provide such a theory in the future. But embracing the theory would involve surrendering the folk-psychology categories of belief and desire. Economists seem so attached to them that they rather ignore the apparent disconfirmations of the theory. For this reason, economic theory is best regarded as just a branch of applied mathematics,

> [...] one devoted to examining the formal properties of a set of assumptions about the transitivity of abstract relations: axioms that implicitly define a technical notion of 'rationality', just as geometry examines the abstract properties of points and lines. (230)

This notion of rationality may have interpretations beyond the domain of economics, for instance in ecology and sociobiology. However, applications to such domains, typically proofs of existence of equilibria and their properties, do not constitute actual explanations, only just-so stories, "the demonstration of an abstract possibility, a suggestive piece of applied mathematics" (231).

Criticisms Rosenberg's view is very controversial. To give an illustration of the sort of objections it has received, I will refer to (Mäki, 1996). To begin with, Mäki argues that predictive success is too restrictive a criterion of demarcation for what counts as science (§3). Other considerations contribute to progress beyond prediction, such as increasing scope or explanatory power. But even assuming the demarcation criterion based on prediction, it is debatable whether economics is not, in fact, predictively progressive (§4). Mäki cites the example of the development of theories of profit, which resulted, among other things, in an increase in predictive power (cf. Hausman, 1992, 196-98). Also, Mäki notices, Rosenberg's notion of "prediction" is too vague. Economists distinguish between many kinds of predictions, "between conditional and unconditional predictions, between point predictions and interval predictions, and between pattern predictions and quantitative predictions" (12). Of these, some are clearly more successful than others.

A second problem with Rosenberg's argument, fo Mäki, is that it is not clear why the use of the intentional categories of belief and desire prevents predictive progress beyond the success of folk psychology (§6). Without further qualification, the argument cannot make sense of the obvious differences in predictive power between the many theories that are conceptually linked to folk psychology:

> 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. (20-21)

As regards the claim that economics is formal political philosophy, Mäki objects that general equilibrium theory is neither necessary nor sufficient to account for the phenomena that would vindicate the economists' attachment to the theory (§10). Also, general equilibrium theory is not as central to economic practice as Rosenberg claims. So, the argument cannot serve to justify *the* essence of economic theory.

Finally, the claim that economics is a branch of applied mathematics concerned with the technical notion of rationality is debatable, too. Rosenberg's argument applies at best to a portion of economic theory, namely microeconomics, but not to others, such as macroeconomics or econometrics. And even in the microeconomic domain, it applies to the microeconomic explanation of *individual* behaviour but leaves out the study of *aggregate* market phenomena (cf. Wade Hands, 2001, 340).

References

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