Book Review

Economics—Mathematical Politics or Science of Diminishing Returns? By ALEXANDER ROSENBERG.
Chicago: University of Chicago Press, 1992. Pp. 266 + xvii. $13.95.

Most economists would agree with Paul Samuelson's jesting remark that there is a "strong inverse relationship" between the "fruitfulness" of a discipline and its "propensity to engage in methodological discussion" ([1963] 1966, p. 1772). The belief, while undoubtedly sincere, is convenient since it excuses the economist from allocating any further thought or effort to the subject.

Economists are proud of their discipline's status as a "science," at least relative to the other so-called social sciences. On the basis of conversations with colleagues in other social "sciences," they find Thomas Kuhn's (1962) claim plausible that the prescientific stage of a discipline is marked by disagreements over definitions, assumptions, and method. Only when practitioners no longer have to waste their time on method can they make incremental progress and lay justified claim that they are "scientists." A corollary to this view is that any return by the economics discipline to a discussion of method would raise doubts about whether economics is indeed a science.

Economists may also doubt the value of methodology because they see it result in too little substantive progress. Here they agree with Joseph Schumpeter that when progress on the substantive issues grinds to a halt, economists talk methodology as a way of verbalizing "a sigh of despair" (1954, p. 539).

Against the common view, Rosenberg argues that accumulating evidence makes it increasingly hard for economists to avoid facing a fundamental methodological question: "Why is economic theory not very good at prediction, and why isn't it getting better?" (p. 55). Those who take the question seriously will find themselves in the midst of methodological issues.

When Rosenberg examines the stated methodology of most economists, he finds that almost all accept a basic common goal for the discipline: to explain human behavior. The most widely read, cited, and endorsed example is Milton Friedman's "The Methodology of Positive Economics" (1953). But the goal is also endorsed far beyond the Chicago School. Even those who support a mathematical research program usually defend the program on the grounds that the investment in technique will eventually result in greater explanatory power (see, e.g., Gerard Debreu's presidential address to the American Economic Association [1991]). Later, Rosenberg examines the mathematical research program in more detail. Here he observes that the research strategy is initially plausible but that the plausibility diminishes as more time passes without significant gains in explanatory power: at some point "the check is in the mail" will no longer be accepted in lieu of payment.

Rosenberg suggests that, whatever the internal goals of economists, much

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of the external demand for economics derives from the hope that policy will be better if it is based on economic knowledge. To be useful in advising policy, economics must be an empirically progressive discipline, which means that it must go beyond generic predictions to achieve an increasing number of specific predictions, and these predictions must successfully be made with more and more precision. Most of the tools for successful generic prediction were present with Adam Smith, and almost all were present at least from the introduction of the tools of marginal utility and general equilibrium analysis in the 1870s and 1880s. By the criterion of generic predictions, economics has been on a plateau of modest success for the past 100 years. By the more ambitious criteria of specific and precise predictions, economics has never achieved success. If, as Rosenberg argues, the current research program has been in place for over 100 years (and its most mathematical form for over 40 years) and there has not yet been a significant empirical payoff, a reasonable scholar might be justified in asking how long we shall have to wait.

Utility maximization theory is the source of the problem, according to Rosenberg: it is based on a theory of human behavior that, while consistent with common sense, is in principle incapable of being made more testable. Having identified the problem, he examines whether there are any possible reforms that might make the discipline empirically progressive. He suggests that neither of two avenues for rescuing economics can succeed: the pursuit of psychology to make the foundations more realistic and Gary Becker's (1976) flirtation with a sociobiological interpretation of the utility function.

Rosenberg first examines whether the foundations of utility theory might be made more realistic by using tools from psychology, even though he recognizes that few economists have any interest in pursuing this line. The main thrust of his argument, in the obscurest part of the book, is that there are philosophical reasons why it will always be impossible to get clearer on the foundations of utility theory. The argument is dense, including all that economists most dislike about philosophy: fine-grained distinctions between words such as "intention" and "intension" and a highly abstract, hypothetical argument about how we cannot ever know what anyone believes or values. Of course, such an argument, if it works, undermines more than economics: it undermines practical common sense. Rosenberg recognizes this but tries to finagle his way out of it with a little end-of-chapter hand waving.

The second potential solution that Rosenberg examines is Becker's household production function. Rosenberg seems to see more promise in this approach, praising Becker's extension of economics into new domains. He is most intrigued by Becker's occasional suggestion (e.g., p. 145) that the arguments of the household production function be defined in terms of the evolutionary requirements of survival. Rosenberg ultimately rejects this approach because he believes that it will be impossible to reliably induce agents to reveal their household production technologies.

In the next chapter, Rosenberg examines general equilibrium theory. He argues that the theory assumes a process of achieving equilibrium that is not observed in the real world, and, as a result, the theory has not increased the explanatory power of economics. But this will not be persuasive to a profession that is used to believing that the realism of assumptions does not matter. He might have been more persuasive if he had mentioned the empirical evidence indicating that of a randomly selected sample of 92 articles citing Debreu, only five were in any sense "empirical," and none of those five made significant use of Debreu's work in the empirical section of the article (Diamond 1988).

Given that general equilibrium theory has not increased the explanatory power of economics, Rosenberg asks why so many economists continue to invest so much time and effort elaborating it. He proposes two answers. One is that the theory provides a possible world that is useful to think about in discussions within the economics of the public choice school and within the contractarian political philosophy that studies issues of institutional design. The second reason economists pursue general equilibrium theory, and the one he finds most compelling, is that they enjoy solving the theory's mathematical puzzles.

In the end, Rosenberg concludes that economics is applied mathematics but is not an empirically progressive science. He hastens to add that he does not mean any criticism in this conclusion and reinforces his benign intentions by suggesting that economists should be allowed to continue to solve their puzzles. Here he "doth protest too much," since he clearly believes that while Rome burns, economists fiddle. He suggests that the rest of the community should look elsewhere for advice on the important policy issues of the day and that the void left by economics should be filled by developing some other discipline into an empirically progressive, and policy-relevant, science of human behavior.

Rosenberg's final conclusion that the future is hopeless for economics will obviously have little audience within the profession itself. But a growing number of economists will sympathize with his criticism that economics has not been predictively progressive. These would-be reformers of the discipline will find much in the book that is congenial.

Rosenberg cites Nobel prize–winning economists Herbert Simon and Wassily Leontief to document the profession's internal concerns about the slow rate of predictive progress. But they could be dismissed by many economists as too far outside the mainstream. More recently, there is evidence that some of Simon and Leontief's concerns are more pervasive. Klamer and Colander (1990), for example, found many economics graduate students at prestigious schools to be bitter at being trained in technical puzzle solving with little policy relevance. Some of these concerns were also voiced in a study under the auspices of the American Economic Association and chaired by Lee Hansen.[1] Even Debreu has warned that because of the increased mathematization of the profession, when one is choosing research questions, "the danger is ever present that the part of economics will become secondary, if not marginal, in that judgment" (1991, p. 5). Former mathematical economist Glenn Loury put the issue even more bluntly when he admitted that he was drawn to economic theorizing because "one could do math while maintaining the credible pretense of relevance by labeling the variables" (as quoted in Wessel [1987, p. 70]).

Among those who agree with Rosenberg that economics has not been empirically progressive, there is no consensus about whether reform is possible or, if so, which reform. Lawrence Klein (1981) compares economics with meteorology in that both study systems in which the high level of noise in the data limits accurate forecasting to the short term. In this view, although modest improvements are possible, the prospects for major reform are limited by the high costs of obtaining more accurate data. Edward Leamer (1983) suggests that the problem with economics is that we spend too little time in testing the robustness of our empirical conclusions. Oskar Morgenstern

[1] See Hansen (1990). A discussion follows (pp. 445–50) by Alan S. Blinder, Claudia Goldin, T. Paul Schultz, and Robert M. Solow.

(1963) (and an early incarnation of Donald McCloskey [1976]) argued that the profession underinvests in improving the quality and sources of data.

But calls for reform have been regularly occurring for at least 30 years without having any noticeable effect on the direction of the discipline. In the absence of a decline in the demand for economists, affecting salaries, mobility, and institutional support, it is not likely that most economists will listen seriously to those who call for fundamental reform of the discipline. Friedman has argued that those who would reform economic institutions in society are ignored until a crisis occurs, at which time the politicians will be groping for solutions. Similarly, in the absence of an external crisis, economists who have been well trained as mathematical puzzle solvers are unlikely to listen seriously to reformers of the discipline, especially reformers from the discipline of philosophy.

But an external crisis may in fact be brewing in the form of a decline in the demand for economists. In Washington, a proposal to abolish the Council of Economic Advisers has been seriously debated. Fewer students are taking economics courses and choosing economics as a major. The main accrediting association of business colleges (the American Assembly of Collegiate Schools of Business) has reduced the importance of economics and other formal disciplines in its curriculum guidelines. Firms are hiring fewer economists. Legislators and regents are demanding a closer relationship between theoretical research and applied payoff.

If these trends are indeed indicators of a decline in demand for economists, then Rosenberg's book may find a congenial audience, not only among the small number of philosophers of social science but also among economists interested in reversing the decline. Although Rosenberg is clearly more at home using the language and arguments of philosophy than those of economics, he has just as clearly made a significant effort to understand and take seriously the discipline that he is studying. But being a tourist among the "Econ," he rarely delves too deeply into concrete examples of what he is talking about (in the way, say, McCloskey frequently does). And, unfortunately, in one of the handful of equations included in the book, he gets the Slutsky equation wrong (p. 158)—the sort of error that will give mathematical economists easy grounds for dismissing the credibility of his critique of economics as applied mathematics. He then criticizes the Slutsky equation for imposing a restriction (that utility be held constant) that is impossible to implement. But as Friedman explained a long time ago (1962, pp. 52–53), Rosenberg's complaint applies to the Hicks version of the equation, not to the Slutsky version, which holds income (an observable quantity) constant.

Rosenberg writes clearly, although he is no match for McCloskey, the genre's master of scintillating, witty, and outrageous prose (see McCloskey 1985). And if you are not a philosopher or a Latin and French scholar, be sure to keep a dictionary ready to hand. Economists who disagree with Rosenberg's conclusions will find grounds for dismissal in his overattention to the finer distinctions of philosophy and in his underattention to the details of economists' theory and practice. Many others will accept Rosenberg's diagnosis that the patient is ill without going so far as to conclude that she is terminal.

Rosenberg suggests that citizens look elsewhere for policy advice, but he devotes only a couple of paragraphs at the end of the book to where they should look. And he finds no promising directions. He admits that economics is the most scientific of the social sciences. He also admits that using basic tools of analysis, such as supply and demand analysis, economics has had some success at making generic predictions. Perhaps economists should devote fewer resources to mathematics and more to philosophy, psychology, biology, or data collection. Perhaps other disciplinary approaches should be explored, and supported. But in the meantime, for practical policy purposes, you cannot beat a theory without a theory. Policy makers would be wise to continue to employ their economists.

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