Princeton University Press, 2003. There are, of course, many ways the editors could have cut the cake. Their focus on entrepreneurs and economic growth, social capital and its unintended consequences, and micro-macro links in doing social science research are all familiar themes. A more precise focus might have better integrated the varied perspectives and told a more coherent story about what economic sociology is all about.

That said, in bringing such varied perspectives together in one volume—with their built-in tensions, range of theoretical underpinnings and differences in interpretation of similar concepts—the editors show a great deal of courage and tolerance.

EVA MEYERSSON MILGROM
Stanford Graduate School of Business

B Methodology and History of Economic Thought

How Economics Became a Mathematical Science. By E. Roy Weintraub. Durham and London: Duke University Press, 2002. Pp. xiii, 313. $18.95. ISBN 0-8223-2871-2.

JEL 2002-1276.

The title evokes a more ambitious book than the one Roy Weintraub has produced. There is little attempt here (apart from a few speculations) to explicate the forces that made economics an increasingly mathematical subject over the first two-thirds of the twentieth century and all but drove literary (i.e., nonmathematical) economics out of the profession. Nor does Weintraub pretend to do so. Rather, he argues merely that the trajectories of theoretical economics and mathematics in that historical period closely intertwine.

There is a sense in which the two subjects' developments are obviously connected: techniques such as game theory and mathematical programming were not well developed by mathematicians before the 1940s and so, of course, were not available to economists before then. But Weintraub attempts to show something deeper and more interesting, viz., that there was a change in mathematicians' very conception of their subject in the first half of the twentieth century, and that this transformation was mirrored in economics. Although there is an element of truth to this claim, I find it quite misleading and explain why below.

The book's organization is rather unusual, at least for a history of economic thought (perhaps books on the natural sciences in the Science Studies tradition—by which Weintraub acknowledges being influenced—also follow unorthodox formats). Instead of a sustained, cohesive argument, Weintraub first offers biographical sketches of a number of twentieth-century economists and mathematicians—some famous, some obscure. He then provides a transcript of a lengthy interview he conducted with Gerard Debreu and tells the fascinating story of how Debreu's and Kenneth Arrow's 1954 paper on general equilibrium wended its way through the editorial process at *Econometrica*.

Finally, he turns to his own family. He recounts the lives of his mathematician uncle Harold and economist father Sidney in considerable detail, as well as his own intellectual development. He closes with some personal reflections on the different ways one might write histories of science.

On the whole, the book makes for engaging reading. In particular, the family portraits are quite poignant; with Sidney struggling to teach himself mathematics as a serviceman in World War II and Hal racing to leave his mark before Hodgkin's disease overtakes him at the age of thirty-one.

However, I find the theorizing less compelling. Weintraub suggests that in the first half of the twentieth century mathematics to a large extent lost its grounding in physical reality and, especially through the work of the Bourbaki school, became the practice of formulating axioms and deriving theorems from these. (Weintraub maintains that the very meaning of the word "rigorous" changed from "based on empirical reality" to "following from airtight logical reasoning.") His central thesis is that the same thing happened to economics. Whereas Alfred Marshall famously wrote that mathematics is only a "shorthand" in economics—not an "engine of inquiry"—and that it runs the danger of obscuring the way that the economy actually works, Debreu, according to Weintraub, attempted to free economics from any dependence on the real world. Following Bourbaki, the Debreuvian philosophy supposedly holds that the role of the economic theorist is to deduce interesting propositions from mathematical models, once those models are in place.

I think that this reading of Debreu misses his true significance and that of his masterpiece, the

Arrow-Debreu-McKenzie general equilibrium model (ADM, for short). Although that model has indeed given rise to a somewhat self-involved literature devoted to its mathematical properties, this has not been its major contribution. To the contrary, its success in approximating the operation of real markets in a broad set of circumstances has been the key to its durability. The prime virtue of ADM is its generality, i.e., the fact that its assumptions ("axioms"), as strong as they are, are considerably weaker than those of other competitive models. This generality has proved extremely useful to researchers in diverse applied fields from macroeconomics to international trade. The models in those fields are typically special cases of the ADM framework, allowing one to transfer all that is known about ADM to the application at hand. But this transferability would be worthless if ADM had no empirical validity.

Of course, because of imperfections of competition, information, or missing markets, ADM fails to accord well with reality in many cases. But even then, it usually provides the benchmark to which one compares the models designed to deal with these imperfections.

One of Sidney Weintraub's great professional disappointments was apparently that his theory of income distribution was not taken as seriously as he thought it deserved to be. He believed it was overlooked because he could not "mathematize" it properly. Perhaps. But other theories with only rudimentary mathematics have made great splashes even in the era of Debreu; witness George Akerlof's "Market for Lemons" or Michael Spence's "Market Signaling." Economics may have become a mathematical science, but the best predictor of a theory's success remains how much light it throws on the economic world.

ERIC MASKIN
Institute for Advanced Study,
Princeton, New Jersey

C Mathematical and Quantitative Methods

When I received this book, my wife said it would be a great supplement for her high school AP statistics class. After reading the book, I agree.

The purpose of this volume is to expose students to statistical techniques that could be used to address questions in the social sciences. The breadth of subjects, from predicting elections, evaluating marathon performances as runners age, investing in wines, determining characteristics of those prone to extramarital affairs, as well as some macroeconomic issues such as interest rate determination and predicting inflation, is impressive for the size of this thin book. Furthermore, Ray Fair provides a set of "lessons," beginning with theory and including "pitfalls," that will be especially helpful to students.

However, some processes are used, such as tobit, that are discussed too succinctly to be helpful to anyone not aware of the process. An appendix that described why this technique was used for marathon running times would have extended the curiosity of those who might be as excited as the author clearly was about his use of this methodology. But no appendices are provided.

While some of the examples are interesting, such as the grade reduction caused by missing class in one college, and all examples are well presented, little new ground is plowed in the applications presented. Probably the biggest surprise for those who remember the "political business cycle" is the change made by Fair in the presidential prediction model.

Originally, Fair's work indicated that elections depended more upon economic performance in the election year than upon most other variables, such as campaign money used, intensity of the campaign, impact of foreign policy or economic conditions earlier in the administration, especially when incumbency is involved. If the electorate truly responded as predicted in those works, then an administration wanting to win a second term would insure that all the painful economic medicine be administered early in the term. Then, even above-optimum stimulus would be provided the year before the election to get those favorable quarters that insured re-election. The implications for policy induced business cycles were clear.

Now, Fair has included the number of above normal quarters of economic performance throughout the term to improve the accuracy of his model. While the political business cycle does not disappear, it clearly is blunted by this earlier need to provide some good economic returns.