

## Economics - Mathematical Politics or Science of Diminishing Returns?

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Many economic methodologists like to dabble in epistemological and philosophical questions. The allure of these questions is similar to the allure of math to economic theorists; it lets one use impressive language that non-specialists can't understand, so that even if one doesn't have a whole lot to say one can still come across as a "deep thinker." This dabbling works fine until a specialist enters the fray and ups the ante, increasing the specialized language of the discussion, and changing the nature of the questions being posed. After that happens, one must either become a philosopher, mathematician, or a passive observer in the debate. Alexander Rosenberg's book, *Economics--Mathematical Politics or Science of Diminishing Returns?*, is a book designed to make passive observers of many economic methodologists. In it Rosenberg, a philosopher, considers the same themes he did in his earlier book, *Microeconomic Laws: A Philosophical Approach*, and covers much the same ground. His main focus is on cognitive status (I don't know what that is, but it must be important, since a philosopher is talking about it) and theory assessment--making judgments about whether theories are good or bad. In the book he considers the work of a number of economic methodologists, usually coming to the conclusion that while they have a piece of the truth, they have it confused or twisted, though sometimes he argues that they have it downright wrong. I'm not the one to judge whether he, or they, are right. His general theme is that general equilibrium theory (which he assumes constitutes the essence of neoclassical economics) fits into epistemology as a subsection of social contract theory, or alternatively as a branch of applied mathematics. Whichever of the two it fits into, it is not an empirical science. For much of what currently goes under the name of economics, I concur with his general thesis, perhaps because it nicely fits in (I think) with the arguments I've been making in my dabbling in methodology. His twist on this theme is that economists should not be ashamed of being a part of applied mathematics and a subsection of the social contract branch of philosophy. There are some deep thinkers in these fields and some practical use may some day come of their work. But he argues that if that's what economists are, they should be judged by their standards--and those standards are much higher than the standards currently used in much of the economics profession. I'm in agreement. My disagreement with Rosenberg concerns what the economics is. He, like most writers in economic methodology, sees positive economics--the development of abstract theories--as the core of economics. I don't. Most economists don't develop high theories, and many don't even try to do so. Instead, they teach economics and they apply economic reasoning to public policy debates. That, to me, is the core of economics and that work falls under what Keynes (pere) called the art of economics--the application of economic reasoning to real-world issues and the teaching of that application. In my view, positive economics--the development of abstract theories--is peripheral to the core of the economics discipline. The core of economics is much more akin to engineering than it is to pure theoretical science, and the methodology relevant to it is likewise a much more rough and ready methodology than the methodology discussed by Rosenberg and most economic methodologists. In the art of economics theories are not being tested to see if they are true; they are being applied to see if they work. It helps if the theories are true, but it's more important that they work.

In the art of economics theory is used only as an engine of analysis. Judgment and past history--what policies have worked in the past, and why--play as important a role in the analysis as does theory. Subtle, esoteric distinctions that much of Rosenberg's core focuses on may be fine for theorists, but when it

comes to applying the lessons of theory, the engineering fudge factor--figure out the minimum stress requirements for what you're building, then quadruple them--eliminates their importance for the art of economics. Over time, gradually this overbuilding is reduced until a prototype fails, but the limits of the analysis are not defined by the fine lines of theory, they are defined by real-world experience.

In practicing the art of economics, the grist of most discussions of economic methodology--the development of high theory--or what might be called the generalization process--is, for the most part, irrelevant; in practicing the art, one accepts the lessons drawn from the past and attempts to apply those general lessons to the particular problems society currently faces.

I raise this disagreement because it directly relates to Rosenberg's conclusion. Applying theory generally is not applied mathematics nor philosophy; it is engineering. For engineering, it does not matter whether the methodology of developing high theory is applied mathematics, a subdivision of philosophical social contract theory, or an empirical science. Thus, while I find myself in substantial agreement with Rosenberg, I see his work as tangential to my concerns about the relevance of the economics field.

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