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Reply to Peter Mueser

Donald N. McCloskey

Peter Mueser agrees with much of the argument but has doubts. Recent books have provided in some detail what he and I both want, "an investigation of the arguments justifying the dominant methods and approaches," although a lot more remains to be done (see Reference list). But Mueser reckons that McCloskey Goes Too Far. I reckon not.

On statistical significance our differences are small; one might say insignificant. I agree that the importance of the "Iowa effect" depends on whether we can do something about it and what our theoretical context might be. That is what I said. The further notion that an economic theory "applies" to the murder rate if the death penalty shows up in even a small way seems to beg the question of what constitutes "applicability." The main point, after all, is that applicability is decided in the conversations of scientists not in a table of Students-*t*. For "samples commonly used in empirical work" *in economics* an insignificantly small effect by some (still unarticulated) standard might sometimes turn out to be statistically insignificant also. But samples in sociology, for example, or educational psychology are often enormously larger than the quarterly national income figures since 1946 beloved of economists. With large samples the silliness of statistical significance cannot be evaded.

Anyway, the case Mueser makes is not general. Some people think that statistical significance is a good "first hurdle." I would say here that they are wrong. That using statistical significance sometimes works out all right is not a good enough argument to justify the present practice, universal in economics and embarrassingly common in other social sciences, of taking statistical significance as the only meaning of significance. That the drunk's keys sometimes *are* near the lamppost does not justify his plan of always confining a search to the neighborhood with the best light. That a mainly irrelevant procedure generated painlessly by our canned computer programs sometimes produces the correct decision, by accident, is not much of the argument.

I stick therefore to my remarks about statistical significance. Statistical significance is useless, virtually, and the social science done with its help over the past thirty years needs to be done over again. We in economics have been barking up the wrong tree. In fact I'll go further. Name the economic finding since World War II that has come out of statistical significance. Permanent income: maybe. But it did not depend on statistical significance. Anything else?

Mueser and I differ more sharply about existence theorems. Let it be understood: I have no more patience than he does with the fatuous arguments that are often made against mathematical modeling. Mathematical modeling is necessary for economics (though not sufficient). I am certainly not against exploring mathematical models, simple or complex, though I record my economic judgment that approximately 4.6692016 times more resources have been devoted to doing so than would be intellectually optimal. Since I am an economic historian it is not surprising that unlike Mueser I view mathematical theories about perfect and imperfect competition as merely inspiriting fairy tales, a place where science starts, not ends.

The point on which we really disagree is more narrow. Mueser asserts something no one except a mathematician or an economist (that is to say, not a physicist or a sociologist) would believe, that "knowledge of existence . . . is a critically important element of such understanding." My reply is: no, it is not, as one can see by looking at the practice of other disciplines that use mathematical modeling, such as physics or engineering or sociology. Only in the math department and in some parts of economics does anyone care if such-and-such an equation known to be usefully descriptive of a physical or a social or for that matter a literary phenomenon has a solution. If it doesn't have a solution a trivial perturbation of the assumptions will yield a version that does. (I state that as a meta-theorem). A physicist does not care if the higher-order terms of a series diverge as long as the first two or three terms yield useful description. In the Department of Physics no one except the physical equivalent of mathematical economists (a small group) cares about proofs. Ask them. Look at the Feynman Lectures on Physics at Cal Tech. The physicists, and even the physical theorists, find proofs unspeakably boring, because irrelevant to the physical uses of the math. Mueser is therefore quite wrong in saying that a model without solutions is useless for understanding the real world. The Schödinger equation on which much physics has rested since the 1920s is not known to have solutions in general.

I know that my argument sounds strange and heretical to someone brought up in the conversation of mathematical economics. But that is merely because the mathematical economists have limited their conversation to themselves and their professors of differential equations. The mathematical economists have been taking their intellectual marching orders from the math department; many of them are in fact converted mathematicians, which is no sin unless it brings irrelevant intellectual values into the economics department. What I'm arguing is that existence theorems, and proofs in general (I do not mean the three-line justifications of this or that formalization, which of course any mathematical field will need; I mean the full-blown proof, which almost always has to do with existence rather than magnitude or approximation or the mathematical idea) are as useless in economics as they are in physics.

Let me put the point sharply again. Name the economic finding since the War that has come out of existence theorems. No fair claiming that existence theorems *are* economic findings: I'm asking for economic science, not for additional reaffirmations of math department values. Computable general equilibrium models? Maybe, although existence proofs are not necessary for their use, and the use is questionable anyway. Proofs in game theory? The existence theorems have mainly shown, as in the Folk Theorem, that the ambition of doing social science at the blackboard does not work. Anything else?

For the rest I put my faith in the magazine *Science*. In the fall of 1989 it contained an article about the new Sante Fe Institute, formed to bring the blessings of physics to the benighted field of economics. (Sociologists take note: intellectual hierarchy is worth millions of dollars.) The physicists involved were astounded by the mathematical musculature of the economists, and not in a flattering way. They thought the economists' obsession with proof to be strange at best. *Science* a couple of months later contained an article asking whether prediction had in fact played much of a role in the testing of physical theories. Unsurprisingly, actually, it had not.

If economics bases itself on a philosopher's idea of what goes on in physics or, worse, a mathematician's idea of what goes on in science the field will get nowhere. My evidence for this is how far economics has gotten since the War in the parts of it that depend on these so-called tools. If sociologists and political scientists adopt the mistaken use of statistical significance and existence theorems they see so gloriously displayed in economics, they will get nowhere, too. It is time to leave the lamppost and go out into the dark where the serious science is done.

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