An Economist Scorned

THE VICES OF ECONOMISTS—
THE VIRTUES OF THE BOURGEOISIE
Deirdre N. McCloskey
Amsterdam University Press, 1996,
135 pgs.

Let me set readers' minds at ease. As most people will have heard, our distinguished author has recently found the gender in which he was born overly confining. Donald McCloskey, a noted economic historian, is now Deirdre McCloskey, and he often calls himself "Aunt Deirdre."

I fear that some readers will expect me to ridicule Deirdre's transformation. Of course I shall not do so: such unkind cuts are beneath me. Let us pass by this subject in silence: in particular, the temptation to remark on the photograph of the authoress that adorns the dust jacket must be resisted at all costs.

The Vices of Economists, unlike Deirdre herself, should be taken seriously. Though it contains a few useful insights, it needs to be handled with extreme caution. McCloskey sets forward three fallacies that, in her view, have ensnared many contemporary economists. Her choice of targets will delight some Austrians, who will rush to embrace this book, if not Deirdre herself. They should not do so; the positions she supports are far removed from genuine Austrian economics, and she reasons badly.

Austrians have been somewhat reserved in their enthusiasm for econometrics; and when McCloskey identifies what she deems a fundamental error in much of modern statistical economics, they will look on with interest.

According to our author, Lawrence Klein, a pioneer in the use of statistics in economics, made a "tragic mistake" that has misled generations of his successors (p. 28). Klein confused statistical significance with scientific significance. Establishing statistical relationships is important, but to do so does not tell us whether a particular effect matters. "What Klein and everyone else in modern science are looking for is a mechanical, uncontroversial way of deciding whether some effect is large or small. No human judgments, please: we're scientists" (p. 30).

No such mechanical procedure is to be had; once we have finished our regression analysis, we must decide—using criteria of human importance—how significant are our results. In particular, what exercises McCloskey is the danger of dismissing the statistically insignificant. Suppose that a particular study fails to demonstrate that seat belts save lives: the difference in survival rates between those who strap themselves in and those who fail to do so does not attain statistical significance.

Need this lack of statistical proof

matter?, our author asks. No, she answers: even if the number of lives lost through failure to buckle up does not reach some magic number, human lives have been lost. Are not these lives humanly significant, whatever Klein and his acolytes say about statistics?

McCloskey cannot be faulted in her contention that statistical significance and human or scientific significance are different concepts, *tout court*. But I cannot applaud her for uncovering a major fallacy. Quite the contrary, Deirdre seems to me to be forcing an open door.

Who is it who is supposed to have identified the two types of significance? McCloskey does not tell us. The cases she mentions do not support any such claim of confusion. She cites several instances in which economists state their failure to arrive at statistically significant results; but she never shows that these writers dismiss the hypothesis they have tested as scientifically unimportant.

The point, I should have thought, is quite otherwise. A statistically insignificant result does not exclude the possibility that a hypothesis is humanly important. But we have not succeeded in verifying the hypothesis by the test we have undertaken. That is all that our alleged statistical miscreants are saying, no more and no less.

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Consider, for example, her discussion of the master himself, Lawrence Klein. McCloskey quotes the following from Klein's first scientific paper: "The role of Y in the regression is not statistically significant.... This low value of the ratio means that we *cannot* reject the hypothesis that the true value of the regression coefficient is zero" (p. 31, citing Klein). How does this show that Klein confused the two sorts of significance? All that the passage says is that his results have failed conclusively to show a statistically significant relation.

Deirdre might appear to Austrians a useful ally on another issue, yet once more her aid cannot be accepted. Austrians extend their criticism of mathematics in economics far beyond econometrics. According to Mises and Rothbard, the key method in economic theory is deductive and verbal: mathematical models, they claim, do not adequately represent human action. Should we then welcome McCloskey's spirited condemnation of "blackboard economics"?

I do not think so. McCloskey's real target is not just mathematics, but all deductive reasoning. Lest I be accused of caricature, let us listen directly to Deirdre herself. Speaking of her spoof, "The A-Prime, C-Prime Theorem," she says: "All that this rigmarole means is the common-sense point that if you are free to choose your assumptions you can deduce seriously any conclusions you want. Actually it says a little more. It says that in some ways of judging how far an assumption is from another, you can *slightly* change the assumptions and get any conclusion you want" (p. 84).

But why object to this? Is not part of what McCloskey says perfectly true? Indeed it is: the conclusion "Some women economists were once men" follows from the premise "Some women economists were once men." The difficulty lies not with the trivial point that given a choice of assumptions, you can deduce what you want.

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Rather, the problem arises from what McCloskey does with the triviality. She in effect says: "You can deduce anything you want if you can pick your premises. Therefore, deductive theory by itself is useless. Without detailed historical investigations [naturally of just the sort in which McCloskey has specialized], economics will get nowhere."

Of course McCloskey's conclusion does not follow. What happens if you start with realistic premises, known to be true? Then, anything you validly infer from them must be true as well. Our author has not at all shown that deduction, apart from the empirical work she favors, is useless. Though I shall not pursue the point here (for which readers owe me many thanks and subscription renewals) McCloskey's "metatheorem" is in part mere bluster. She gives us no reason to believe that you can prove, by small changes to your premises, the opposite of any conclusions you have deduced.

McCloskey has devoted much attention, and several books, to methodology; but one word never passes her lips—praxeology. Has Deirdre ever heard of Mises? One presumes so; but why does she never mention, much less analyze, Mises's deductive method?

In a review of an earlier book by McCloskey, *If You're So Smart*, I raised this point. McCloskey sent me a pleasant letter in response: indeed, he (as she then was) has been much more courteous to me than I deserve from the tone of my reviews. Though he took issue with several points in my review, he said not a word about praxeology. What is going on here?

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of reason extends even further. She thinks that economic theories need not be logically consistent: "Again, if you don't know much about science, you might think that, well, surely a scientific theory must be consistent, at least. No, it need not be. Consistency is one intellectual value among many" (p. 75).

In defense of her bizarre view, she quotes a passage from Wittgenstein. McCloskey is right that Wittgenstein had interesting but troubling views on consistency. Unfortunately, she does not tell us what these views are, nor does she attempt to analyze them. The mere quotation from Wittgenstein's debate with Turing is enough for her. That settles it. Out with consistency! McCloskey, after all this, avers: "I love economic theory" (p. 91). One cannot help recall the saying that we kill those we love.

Readers will have noted that my comments on McCloskey's book have so far accented its deficiencies. But I am no "nattering nabob of negativism." Matters improve somewhat in McCloskey's discussion of her third vice, the arrogance of social engineering. Here she forthrightly and eloquently condemns efforts by planners to control the market. Of course, Deirdre uses a bad argument to support her defense of freedom, but we cannot ask for too much. McCloskey's argument, in essence, is this: Planners cannot use an economic model that predicts outcomes better than the market. If they could, they would be rich. "But having such Faustian power is to be a god or, in human terms, to be rich beyond the wildest dreams of avarice" (p. 104).

McCloskey has here pushed a commonsense point further than it will go. Someone with a guaranteed system for beating the stock market is unlikely to disclose it to others; if such systems exist, they are not likely to be in the public domain. But this hardly suffices to show that no such systems exist, only that the systems are not public. The response to McCloskey's inquiry, "If you're so smart, why aren't you rich" is "Some people are very rich" (Readers should have a look at McCloskey's evasion of this objection on p. 109).

Further, it is not impossible that winning systems *are* revealed: perhaps their inventors think them very complicated and unlikely to be adopted by the public. I hasten to add that I know of no such system—and if I did, I wouldn't disclose it.

But suppose that McCloskey is right: No one can out-predict the market. Does this tell conclusively, as McCloskey imagines, against social engineering? I cannot see that it does. Remember, social engineers are proposing to interfere with the market, not to predict the results of its unimpeded operation. If one cannot beat the market, how does this show that one cannot successfully regulate it? Fortunately, excellent arguments against interventionism exist; but McCloskey's is not one of them.

Nevertheless, McCloskey deserves credit for her defense of freedom. "[I]f you try to achieve The Rational Society, the one that maximizes Utility, you can easily create nightmares of unfreedom. You can see it in modern architecture.... You can see it in the startling share of government in national income, over fifty percent against five or ten percent a century ago" (p. 117). Well said, Aunt Deirdre. ✦

All in the Family?

MARX, HAYEK, AND UTOPIA
Chris Matthew Sciabarra
SUNY Press, 1995. x + 178 pgs.

Within *Marx, Hayek, and Utopia* lies a very good book struggling to escape. Chris Sciabarra has asked a penetrating question and brought to light important material in his pursuit of an answer to it. Unfortunately, he is enamored of an odd philosophical doctrine that he cannot refrain from discussing. This skews, but does not ruin, his presentation.

As everyone knows, Friedrich Hayek criticized socialism to devastating effect. For Hayek, the assault on socialism extended beyond economics. As he saw matters, socialists were in the grips of "constructivist rationalism." They falsely thought that they could subject society to total control through planning. Their schemes ignored the fact that society is a "spontaneous order," a phrase that readers of Hayek cannot fail to recognize. Only the market can handle the complex details of social organization. It does so by coordinating the "tacit knowledge" of producers and consumers.

Sciabarra asks a fundamental question: is Marxism a type of constructivism that falls before Hayek's