

# SIMULATING BARBARA

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*Deirdre McCloskey*

## ABSTRACT

Barbara Bergmann has a tough style of confrontation and a scientific style of asking How Big Is Big. Economics would be a lot better off if it dropped Mathematical "Proof" and Statistical "Significance" and started simulating Barbara.

## KEYWORDS

Bergmann, Barbara, simulation in economics

I first met Barbara Bergmann years ago, and we got along fine. This might seem odd, because our politics are not the same. I'm a feminist free marketeer, and in those days I was just a free marketeer, without the feminist part. Barbara is not a 100 percent "statist," as we label the enemy in libertarian circles, but she's nothing like as worried as I am that the next intervention by the Federal government will end up somewhere between the United States Postal Service and the Soviet gulag.

I think we have gotten along so well in our few encounters because we have the same intellectual style and purpose. This itself might seem odd, because our backgrounds are different. But Barbara's purpose, like mine, is to know How Big Is Big; and in finding it our style is a little confrontational.

"Confrontational" in a good sense. For example, unlike some people with a confrontational style, Barbara is consistent, and doesn't get sore when other people are confrontational back at her. She showed this in her calm reaction to Donald McCloskey's bizarrely large role in the Great Market Debate on Femecon a couple of years ago. When other people, especially other women, were becoming very angry at Donald's domination of the airwaves, Barbara defended him – not his views on the cuddly sweet nature of The Market, Lord knows, but his confrontational style. Like me, Barbara believes that confrontation gets us right to the point and keeps us there. Barbara said on Femecon that when she was a graduate student she was the most outspoken member of the class, challenging the professors, asking questions, getting to the point and staying there. At Harvard in the late 1950s such behavior was not, she noted, popular in a woman. A few years later when I was in the same classes she had taken it was acceptable, even encouraged, in a man.

When I saw Barbara in Mexico City in July of 1997 and we wandered through the streets together trying to find the hall for the IAFFE reception she immediately and with no preliminary social chit-chat started quizzing me on my views about some economic issue – I think it was the minimum wage, but confess that I was so overwhelmed by her eagerness to Get to Work that I can't remember. Her attitude was: Here I have a living, breathing member of the Chicago School. What a great opportunity to sharpen my own arguments! Let's see if she has anything to say on the issue. (I didn't, but that didn't stop Barbara.)

The idea is that through confrontation we improve each other's ideas. We Consistent Confrontationists don't get angry at the other person's Getting to Work, because we regard the friction of confrontation as a great favor that our opponent is doing for us. We don't get "offended," a favorite word of the nonconfrontationists. They are always getting offended and going off in a huff. Not Barbara. She stays and confronts and learns something. It's not a feminist style, in one version of that essentially contested concept, though it easily could become so. It's even a little macho, which I don't think troubles Barbara one bit. After all, she advocates that women should get into men's jobs just as fast as they can, to prove what Charlotte Whitton said somewhere: "Whatever women do they must do twice as well as men to be thought half as good. Luckily this is not difficult."

So too for academic confrontation. Luckily, with a feminist flavoring it is not difficult. "What is crucial," writes philosopher Amelie Oksenberg Rorty (1983: 562) "is our ability to engage in continuous conversation, testing one another, discovering our hidden presuppositions, changing our minds because we have listened to the voices of our fellows. Lunatics also change their minds, but their minds change with the tides of the moon and not because they have listened, really listened, to their friends' questions and objections."

The confrontation is good if it involves really listening. Barbara asks, "What is truth?" and stays for the answer. I had not read much of Barbara's work. We get along on reputations and rumors in academic life, especially in economics. (Historians are more insistent that if you are going to have an opinion about someone you ought to know what you are talking about, and do the reading.) I was satisfied with the amiable assumption that Barbara was a good economist. That is at least better than the default assumption most economists seem to favor, that anyone they haven't read (a sociologist, say, or a philosopher, or someone in another school of economics, or Barbara Bergmann) is probably an idiot.<sup>1</sup>

When I recently started reading Barbara's work, non-arrogantly, I was astonished at how similar our views are. Well, not about policy and politics. What we agree on is how to go about being an economist. That is, we agree on how to talk as an economic scientist, on the proper rhetoric of our field. In particular Barbara has been an early and insistent advocate of simulation,

a radical alternative to the way we do economics now. The way we do it now, and have since Barbara and I were graduate students, is mainly either to prove things on the blackboard or to measure the statistical significance of fitted hyperplanes. That is one reason we have gotten nowhere in economic science since 1955. You can prove anything if you are free to play with the assumptions, so proof (what philosophers call "validity") is no guide to social truth. On a blackboard you can prove that the North American Free Trade Agreement is, under some assumptions, wonderful or, under other assumptions, terrible. Take your pick. (People do.)

Moreover, the statistical significance of a finding is virtually irrelevant to its scientific significance. The two overlap more or less by accident. That a sample size is large enough to produce a "statistically significant" result at conventional levels is not the same thing as saying it is important. It just isn't. Unfortunately the economics profession since 1955 has adopted these two unscientific methods, proof and statistical significance, and has made them into the only two acceptable rhetorics, although neither can ever tell you how big an economic effect is. Proof is what they do in the math department; testing of statistical significance is what they do in the psychology and sociology departments. Neither is science. To prove that *C* follows from *A* does not prove anything in the world, unless you already know that *A* (convexity, say, or perfect competition, or equilibrium) holds in the world. Similarly, to show that the coefficient on *X* is statistically significant does not say anything about the world (Deirdre McCloskey 1997).<sup>2</sup>

By contrast, simulation is what physicists and engineers and evolutionary biologists and (in effect) historians do. These real scientists ask How Big. Barbara has advocated all through her professional career getting to the scientific point of How Big Is Big and staying there. It is not surprising that she is a great fan of simulation.

There is nothing mysterious about "simulation." It has been one strand in economic thinking since the English political arithmeticians of the seventeenth century. It is just sophisticated, theorizing, mathematized accounting.<sup>3</sup> Thomas Schelling is fond of pointing out that what economists really, truly know, and noneconomists do not, are largely matters of accounting. Learning to think like an economist, Schelling argues, consists in good part of learning to speak bits of accounting logic. Add whatever behavioral equations you think you have evidence for (and never mind statistical significance!) and you can find out, sometimes, How Big Is Big.

You can learn about How Big from observation and from simulation in light of the observations in an accounting framework. At the Santa Fe Institute, which brings economists and physicists together, an economist indignantly asked a physicist presenting a paper, "Where are your proofs?" – to which the physicist replied, "You can whip up theorems, but I leave that to the mathematicians" (Robert Pool 1989: 701). The economist was amazed;

it shows how little economists know about how a science like physics actually works.

As the mathematical economist William Brock (1987: 2) put it:

We remark, parenthetically, that when studying the natural science literature in this area it is important for the economics reader, especially the economic theorist brought up on the tradition of abstract general equilibrium theory, to realize that many natural scientists are not impressed by mathematical arguments showing that “anything can happen” in a system loosely disciplined by general axioms. Just showing existence of logical possibilities is not enough for such skeptics. The parameters of the system needed to get the erratic behavior must conform to parameter values established by empirical studies or the behavior must actually be documented in nature.

(Brock 1987 [1988]: p. 2 of typescript)

Well, fancy that: “documented in nature.”

A good example of asking important economic questions grounded in the real world is Barbara’s book *Saving Our Children from Poverty* (1996).<sup>4</sup> The book makes an impressive scientific argument that adopting a French-style payment for child care would save millions of American children from pathology-producing destitution. It relies on economic theory to note that ADC has a disincentive effect on job holding (for instance, p. 107), and that food stamps have a shadow price less than their face value (p. 99), and on statistical theory to make use of numbers gathered from sampling. But I can’t find any place where Barbara uses the bankrupt rhetorical forms “theory says that” or “the coefficient on  $X$  is statistically significant at the .05 level.” She does economics the way physicists do physics, not the way mathematicians do it. She asks How Big. And she is explicit about the loss function, as in “How much poverty among children are we willing to tolerate in order to discourage improvident child-bearing or to penalize impecunious single parents who avoid paid work?” (p. 138).

In light of all that, I go back to a theme in the Good Old Chicago School: If you want to help people of  $X$  description, you better just give the money to  $X$ . That is Barbara’s scientific conclusion, too. The French target the children, and resist the temptation to punish the adults with the same instrument used to help the children. Yes. If Milton Friedman and I thought American governments would not grossly misallocate most of such money, and end up giving much of it to middle-class holders of government jobs and middle-class taxpayers, we would sign on. So Milton and I don’t agree with Barbara’s conclusion. But we have no doubt that she is on the right track in the form of argument she uses.

Simulation is not usually explicitly defended, and certainly not with the fervor mixed with confusion that existence theorems and statistical

significance have been. An early defender was Guy Orcutt, and Barbara cites his work on what he called “microsimulation.” A decade or two before the fall of computation costs made it feasible, Orcutt (1961) was trying to model the whole economy as little computer people running around. It’s the way one models any difficult system, such as evolutionary processes or a building subject to wind and earthquakes, and it doesn’t require analytic results of demonstrably imperfect generality. As Barbara put it in 1990, “Highly mathematical accounts of the derivation of individuals’ behavior are not infrequently followed by vague verbal descriptions of what goes on when the individuals come together and interact” (p. 100). The “rigor” of the analytic results is of course phony.  $A \Rightarrow C$ . So?

The two characteristics I’ve claimed Barbara and I share, confrontationism and being interested in How Big Is Big, are not entirely separate. Someone who is confrontational and then listens, really listens, can be thought of as trying on the arguments for size. Testing means torturing the data to see how it works, but in actual economic practice these days it has been reduced to the phony and easy standards of absolute Proof and irrelevant Significance.

We can get economics back to scientific work. By simulating Barbara.

Deirdre McCloskey, University of Iowa, Department of Economics,  
PBAB, Iowa City, IA 52242, USA  
e-mail: deirdre-mccloskey@uiowa.edu

## NOTES

- <sup>1</sup> When Richard Palmer, a physicist, attended a conference some years ago involving both fields he reported his amazement at the default assumption of idiocy: “I used to think that physicists were the most arrogant people in the world. The economists were, if anything, more arrogant” (Robert Pool 1989: 700).
- <sup>2</sup> That statistical significance does not tell How Big, in the absence of an explicit loss function, was clear to the founders of modern statistics. See Deirdre McCloskey (1997: Ch. 2).
- <sup>3</sup> They are the few things an economist would be willing to stand before God and declare to be True: assets equal liabilities plus net worth;  $GNP = C + I + XM$ ;  $MV = PT$ . Adam Smith’s first and foundational sentence in *The Wealth of Nations* is that national income equals national product equals national expenditure: “The annual labour of every nation is the fund which originally supplies it with all the necessaries and conveniences of life which it annually consumes” (Smith 1776: 10).
- <sup>4</sup> Economists like Bergmann or Wassily Leontief or Irma Adelman or Claudia Goldin who favor simulation tend to write books, which the proofsellers and significance peddlers find too taxing to write or to read; when economists get back to books as their characteristic intellectual product you will know they have gotten back to serious scientific work, since serious simulation often requires a lot of room; though simulation is gradually becoming more common in the *American Economic Review* (Charles Ballard 1988; John Campbell 1993).

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