OTHER THINGS EQUAL

Cassandra's Open Letter to Her Economist Colleagues

Deirdre N. McCloskey
University of Iowa
and
Erasmus University of Rotterdam

Cassandra, you know, was the most beautiful of the daughters of Priam, king of Troy. The god Apollo fell for her, and made her a prophetess. In exchange he wanted sexual favors, which she refused to give (we needed laws about sexual harassment in 1250 BC as much as we do now). So he cursed her, in a most peculiar way. He had already given her the gift of prophecy, to know the future of the interest rate (say) or of the S&P 500, or to know what was going to happen if the Trojans brought that big wooden horse into the city. The curse? That although she would continue to be correct in her prophesies no one would believe her.

Cassandra [to Trojan guys proposing to bring the horse in]: You dopes! The horse is filled with Greek Soldiers. If you bring it in Troy is lost!

Trojan Guy: Uh, yeah. I see what you mean, Cassie. Good Point. Enemy soldiers. Inside. Our city lost. Thanks very much for your prophecy. Really. Great contribution. [Turing away.] Well, come on, guys, let's bring this sucker in!

I have to admit, guys, that I feel like Cassandra. I tell you plainly that existence theorems and statistical significance are useless for a quantitative science like economics. I give you reasoning and evidence that none of you can answer, even the best of you – I mean Nobel laureates; I mean Ken Arrow, Bob Solow, Gerard Debreu, Doug North; name your laureate. When you try to answer my (blindingly obvious and therefore rigorously true) points you just make fools of yourselves, showing that you haven't thought through the place of existence theorems or of statistical significance in a real science. I invite you to look at a copy of the *Physical Review* (version C, say, the one about Nuclear Physics, the issue of November 1990, just as an example) and confirm that its 2,263 pages contain not a single use of existence theorems or of statistical significance, though every page has mind-stunningly difficult mathematics, and data or simulations measuring actually or hypothetically the oomph of the effect the

Other Things Equal, a column by Deirdre McCloskey, appears regularly in this *Journal*.

Deirdre McCloskey: Economics Department, S336 PBAB, University of Iowa, Iowa City, IA 52242-1000 . E-mail: deirdre-mccloskey@uiowa.edu

math has isolated. I explain patiently to you that real sciences like physics care only about How Big (actually or hypothetically), not at all about "existence" whether in the mathematical or the (allegedly) statistical sense. I ask you to consider any other science you care to – geology, say, or evolutionary biology or social history – and notice that all of them spend all their effort on questions of How Big, which never can be settled by an existence theorem or by an unadorned test of statistical significance.

I take Gerard Debreu's feeble defense of existence theorems to pieces at length [1994, esp. Chapters 10 and 11]; I debate publicly with Ken Arrow and he loses [1998a]; I utterly demolish significance-without-loss-functions, citing all the best authorities in theoretical statistics [1985; 1998b, (1985)]; I put the two points together in a semi-popular form that anyone can understand [1995a, 1995b, 1997]. Steve Ziliak and I show you empirically that you in fact use statistical significance, for no scientific gain, in most of your empirical papers [1996]. I guess it's obvious that you use existence theorems in most of your theoretical papers for no scientific gain, although I note a promising trend in the papers during the 1990s: having proven existence, the papers often then show that the effect has oomph, matters quantitatively, by simulation. But I've gotten merely strange looks when I suggest to the theorist that she dispense with the useless, Math-Department existence theorems and allocate her time to actual scientific work on the simulatable results.

I get plainer and plainer about these two points, and say them to you every chance I get, because after all if I'm right then most of what passes for science in economics is simply a waste of time – which is an urgently important fact about our science, if true. "Guys," I say, "You bring either of those two horses into the city of economics and the city will be ruined. Leave them alone. Get back to using mathematics and statistics for real measurements."

The curse? No one believes me. When I chose my new name a few years ago I guess it would have been prescient to choose Cassandra. "Yeah, I see what you mean. Statistical significance and existence theorems are useless for science. Uh-huh. Yup. Wow, you sure are a good writer! Iowa, eh? Hey, how about them Hawks! (Turns back to 'work.') OK, guys: bring in the two wooden horses!" Poor Cassandra. No one believes her. Well, not quite. I get letters of support from a few graduate students and professors, for which thanks. They sound furtive, like people in a police state whispering their politics. Arthur Goldberger has spoken out, devoting a page in his book on econometrics to McCloskey's Criticism [1991, 240-41]. Thank you, Arthur: I wish your econometrician colleagues would teach the point. (It would be bitchy of me to note that Arthur's page is one out of hundreds, the other hundreds being devoted to showing how to misuse statistical significance; so I won't compare Arthur's lucid act to including a long page on safe sex in the Kama Sutra.) I hear news of people saying, "Hmm. She's right." Thank you, thank you, on behalf of our amazingly misled science, which is not going to make progress in understanding the world until it gets over the strange conviction that blackboard existence theorems and tables of Student's-*t* can in themselves test our scientific hypotheses.

I have mixed feeling about the experience. Part of me says, "Grow up, Deirdre. Of course Normal Science resists listening to critics: what do you expect?" After all, Deirdre knows that science is a matter of rhetoric. Science is not a matter of just laying down your cards of reasoning and evidence and then winning the rubber, as people think who haven't followed science studies since Thomas Kuhn. Science is persuasion, a matter of woodcraft. The rhetorical theory of science says that every scientific advance, even in method, is a matter of persuasion. Science is rhetoric, persuasion, all the way down. It must be so if it is going to be a science in a free society. (Another, pure-prudence theory of science, popular among economists, would say that scientists are Max U-ers, and will therefore of course do any silly but profitable thing as long as journal editors let them. Don't I believe in economics? [Answer: no, not as an ethical guide.]) So if Larry Summers and James Buchanan and Franklin Fisher and Roger Backhouse and Ed Leamer and Vassily Leontief and Thomas Mayer and Mark Blaug and I can't persuade, well, that life, Toots.

And yet, even though I know that science is rhetorical and have been saying it even longer than I have been saying that existence theorems are pointless and that statistical significance without a loss function is silly, you can see I am indignant. Why? People get indignant when they feel some ethical norm is being violated. Persuasion in science is at bottom an ethical matter. One should be ethical in science, right? That doesn't mean, "Shut up and go along with the way we do things regardless of how indefensible they are," although I realize that my violation of this Shut-Up norm of normal science is what make people angry at me. It's why I've stopped attending most seminars in economics: I make people so angry by suggesting that we not use the nonsense procedures in modern economics and that we instead use the theory and econometrics for simulations of How Big that it's only polite to absent myself. I don't want to hurt my colleagues, whom I love, and it hurts them to hear my two criticisms, which they don't understand.

Ethics in science means – doesn't it? – something like the opposite of the Shut-Up norm, Max-U, do-anything-the editor-will-allow, go-along-to-get-along. It means looking seriously at serious criticism, and if you can't rebut them, accepting them and changing your practice to suite. In the end the reaction to the Lucas Critique, for example, was to agree with it and change practice. (But I have in mind the example of the Coase Critique, too, which at one level is the same as my existence-theorem criticism. The Coase Critique has not been understood by most economists [McCloskey 1998c]. So they haven't agreed with it and have not changed their practice of blackboard economics.)

I think the reaction of most (I mean 95 percent) economists to my two very simple points has been in this sense "unethical." It's been the Shut-up reaction. That means you, I'm afraid, dear. At any rate the odds are good, adopting a 5 percent level of significance.

Try to forget that I am a mere economic historian or a mathematical novice or a statistical idiot or a gender crosser or an Iowan or a Chicago-School economist or an irritating woman in seminars or whatever else about me you think disqualifies me for observing that the two emperors of modern economics, Existence and Significance, are naked. Try to act like an adult in science and respond to the actual case being made instead of seeking refuge in resentment about its tone or venue or whatever other piece of "mere" rhetoric you don't like.

I make a lot of jokes but actually I'm a pretty serious person, as you can see by having a look at my scientific work in economic history, or for that matter in economic rhetoric. So, a serious person makes two devastating, internal criticisms of the way modern economics has run itself since the War, yet no one listens. (The criticisms are "internal" because they are not criticisms of the sort, "Gosh, you economists depend on Max U when everyone knows that people have other motives" or "I wish you would pay more attention to class and gender" or "I hate math: why do you do so much of it?" Mine are criticism that bracket off the substantive criticisms, good or bad, that come from outside the mainstream of economics. My two criticisms by contrast come from within the very scientific program they criticize, namely the noble ambition of Samuelson, Arrow, Klein, Tinbergen, and others of the 1940s to make economics into a real science like physics or history. That's why the criticisms are "internal": Cassandra came from within Troy, and was making an argument that Trojans would have agreed with if not under Apollo's spell.)

What should one – I mean you, dear reader – do when hearing my criticisms? It seems to me that one should, ethically, either: Come up with counterarguments that are at least as serious in their practical and philosophical reasoning as the Cassandra Critiques. This requires understanding them. It won't do to "reply" by saying things like "But we must do theory" (of course we must, you silly man: but existence theorems are not theories in any science) or "You are against statistics" (that's ridiculous: I'm against misuses of statistics that every theoretical statistician since Neyman and Pearson has been against, too).

Or one should: Agree with Cassandra and stop offering existence theorems and statistical significance as "science."

What is not open to a serious scientists is to: Skulk around looking for opportunities to put arrows into Cassandra's back.

Unhappily, that third, unethical option what most of my colleagues, near and far, have done. They won't face my arguments; they won't listen; they don't understand. They are so angry.

My thesis supervisor, Alexander Gerschenkron, wrote a devastating review of a translation from Russian of a book in economics, attacking in detail the author's apparently feeble command of the Russian language. Later at a conference the translator had the temerity to approach Gerschenkron and say amiably, "I want you to know, Professor Gerschenkron, that I'm not angry about your review." Gerschenkron turned on him: "Angry? Angry? Why should you be angry? Ashamed, yes. Angry, no."

If you haven't got serious answers to my two internal criticisms of modern economics you shouldn't be angry. You should be ashamed. (The same holds for the external criticisms, by the way. If you haven't got an answer to the Marxist's criticism that class interest governs the economy or the Austrian's that entrepreneurial creativity does or the feminist economist's that gender does, you should be ashamed. What kind of scientist are you? These are serious, intelligent people: what's wrong with you that you haven't got the wits or will to reply? But you should be *really* ashamed if you can't even make your *own* program cohere.)

It's time to get serious about scientific rhetoric, about how economists ought to persuade. If you disagree with my two criticisms I think you have a responsibility as a serious scientist to answer them.

Let me restate them (my fingers are wearing out doing so over the years, but I've observed that people need them to be stated over and over and over again before their truth and importance sinks in; and there's no reason that everyone should be conversant with my works; so here's the short version):

1.) A theorem that asserts the existence of an effect is useless for science. What makes the mathematical assertion of the effect useful for science is accompanying reasoning (mathematical, for example; or observational or experimental) that can lead to a way of judging How Big the effect is.

For example, general equilibrium theory is in this sense useless. And (just in case you thought I would stop with an old-fashioned example that by now pretty much everyone in economics agrees is useless) so is most of game theory. An example of useful and mathematical theory is Solow's way of splitting increases in output per labor input into increases in capital per man and change in a neutral factor, A, called "technical change," stuck in front of the production function. Another is supply and demand curves, with functional forms and guesses as to the parameters. So for that matter are general equilibrium simulation models. All these ask How Big. Useful theory shows the way to answering How Big.

2.) No test of significance that does not examine the loss function is useful as science. What make tests of significance useful are situations in which the sampling error is the scientific issue (which it commonly is not, by the way) and in which the costs of accepting or rejecting the hypothesis are explicit and sensible.

Thus unit root tests that rely on statistical significance are not science. Neither are tests of the efficiency of financial markets that rely on statistical instead of financial significance. Though to a child they look like science, with all that really hard math, no science is being done in these and 96 percent of the best empirical economics (McCloskey and Ziliak 1996. The 96 percent is the figure for the 1980s in the *American Economic Review*; we are retesting on the 1990s). Scientifically meaningful statistical procedures are answers to the question How Big.

Anyone who has a serious answer to either of my points is invited to submit them to the *Eastern Economic Journal*. Or to any other journal. Make you scientific fortune. Ingratiate yourself with the powers that be. Get a job at Princeton. Show that the way economics is presently being done is scientifically defensible. If you can.

But please, no skulking. No cowardice. No Shut-Up ethics. No saying in seminars, "You can't raise that point because it's too fundamental." Send me a copy of your answer, and be ready for me to point out that you are still not bothering to look into how actual sciences operate. And if you're going to shoot arrows at me have the courage to shoot them with me facing you, my own little bow in hand.

Hmm. Diana, the huntress: that would have been a good name, too.

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