

# OTHER THINGS EQUAL

## Samuelsonian Economics

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I just published a little pamphlet, in a new series from the University of Chicago Press, Prickly Paradigm Pamphlets, called *The Secret Sins of Economics*. The pamphlet is directed at people outside economics, such as anthropologists and English professors. As to its theme, if you've made a habit of reading my columns in the *Eastern* you get only one guess.

Yup: economics, especially mainstream American economics, for all its promise, is in *very* bad shape because it has fallen into a cargo-cult version of "science" in which *qualitative* theorem-making runs the "theory" and statistical significance *without a loss function* runs the "empirical work." Consequently, none of the high-prestige "work" in the journals is to be taken seriously. Most (say 95 percent) of its alleged "results" have to be done all over again, by economic scientists using—in preference to the mumbo-jumbo that has passed for scientific method among economists since 1947—real scientific methods (such as serious simulation disciplined by the world's facts; and functional-form math; and statistical significance, when relevant, *with* loss functions; and economic history; and inquiry into all the other human sciences we economists have been invited so long to ignore).

If on the other hand you have *not* made a habit of reading these columns . . . well. . . I have a suggestion for your ethical and scientific education. Lacking it, you will need a little explanation. I have given it in fuller form here and elsewhere (as in *The Secret Sins of Economics* and in *How To Be Human\* \*Though an Economist*) over and over and over again. Check it out at [www.uic.edu/~deirdre2](http://www.uic.edu/~deirdre2).

Here's the nub. A real science—or a real inquiry into *anything* about the actual world—should both think and watch, theorize and observe. That doesn't mean that pure thinking (philosophy or mathematics, for example) or pure watching (painting and narrative, for example) are to be disdained. It just means that a science—or any inquiry into the world, such as an inquiry into whether your lover will leave you if you forget his or her birthday again—does both. The inquiry into your lover's behavior will only be of value, of course, if it is quantitative: it's no use "proving" or "determining by testing at the 5 percent level" that the lover will be *somewhat* annoyed by the forgotten birthday, to some indeterminate degree; you need to know *how much*. "Oh, don't worry, dear: I know you love me" is very different, quantitatively, from "You jerk: that's the last straw." So: a real science (I am not distinguishing "science" from

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numerous other inquiries into the world, you see) is quantitative. All that's obvious, right?

To which the typical modern American economist will reply: "Great, Deirdre. I agree. It is obvious. *And that's exactly what we mainstream economists do!* We theorize and observe, and at a wonderfully high level of both. We do very sophisticated mathematical theorizing, such as in the Mas-Collel, Whinston, and Green textbook [1995], and then we test the theory in the world using very tricky econometrics, such as Jeffrey M. Wooldridge, *Econometric Analysis of Cross Section and Panel Data* [2002]. You can see the results in any journal of economics. Some of it is pure and applied theory, some theoretical and applied econometrics. Theorize and observe."

To which I say: Bosh. *She and her colleagues, when they are being most highbrow and Science-proud, don't really do either theorizing or observing.* Economics in its most prestigious and academically published versions engages in two activities, *qualitative theorems without entries for the world's data* and *statistical significance without loss functions*. These two look like theorizing and observing, and have the same tough math and tough statistics that actual theorizing and actual observing would have. *But neither of them is what it claims to be.* Qualitative theorems are not theorizing in a sense that would have to do with a double-virtued inquiry into the world. In the same sense, statistical significance without a loss function is not observing.

Paul Samuelson, in his modestly entitled Ph. D. dissertation, *The Foundations of Economic Analysis* (completed in 1941, published in 1947), told us we could get along in economics on *qualitative* theorems. This was good news, since economics since 1747 had been mainly engaging in such "work." The free trade theorem of Ricardo, for example, has no places for filling in actual numbers you could actually go and measure in the world (though of course the substantive argument *can* with a good deal of pushing and shoving be given a scientific and quantitative form, in simulations with real facts, for which see economic historians: I'm talking about Ricardo's actual book). Neither did Samuelson's factor-price equalization theorem give us a way of plugging in actual numbers. And have you ever asked how you might simulate for the world's work a Max U model with overlapping generations? So economists, such as Paul's brother-in-law Ken Arrow, happily went on doing—now in a "rigorous" mathematical version—the blackboard theorizing we economists had always loved.

So what?

This: *It ain't science.* It's just logic. It connects assumption A with conclusions C. My objection to spending half of the working hours and journal space on such exercises is not the same as being "against math" or "against theorizing." It is being against vapid existence theorems, whether mathematically or verbally expressed. *Existence is never a scientific issue, yet both of the Two Sins focus on the existence of an effect.* The trouble with qualitative theorems is that you can of course wander if you will through the hyperspace of assumptions and corresponding conclusions until the cows come home (as we do over and over again in every field of economics: witness trade theory since Hume; or the ebbs and flows of this or that assumption in macro; or Industrial Organization reduced to endless game-theoretic speculations). *Yet you haven't said anything about the world unless you can measure.* Make thus-and-such assumptions, A, about the following game-theoretic model and you can show that a

group of unsocialized individuals will form a civil society. Make another set of assumptions,  $A'$ , and they won't. And so on and so forth. Blah, blah, blah, blah, to no scientific end. (This particular branch of speculation has been growing since Thomas Hobbes in the late 17<sup>th</sup> century; it has not come to a single factually correct conclusion about the world, not yet.)

Such stuff is not theory in the sense used by, say, physics (or astronomy or geology: the issue is not experimental vs. observational sciences). Pick up a copy of the *Physical Review* (it comes in four versions; pick any). Open it at random. You will find mind-breakingly difficult math, and physics that no one except a specialist in the particular tiny field can follow. But always, in virtually every paragraph, you will find repeated, persistent attempts to *answer the question How Much*. Go ahead: actually do it. Economists would stop saying that the current “theory” and “observation” used in their field was a social physics if they actually bothered to look in on what physicists do. Don't worry; it doesn't matter that you can't understand the physics. You will see that the physicists always use a rhetoric of How Much—not the question we ask mathematically after Samuelson (and asked verbally before), Whether. *The physicists never prove theorems of the qualitative, existence-theorem sort beloved of mathematicians (and post-Samuelson economists)*. All the hard math in physics is for *derivations*, getting from one functional form to some another form easier to check quantitatively. No physicist asks Whether an equilibrium “exists” or is “stable,” the questions Samuelson and Arrow taught us to ask. The physicists *calculate*. Even the theorists as against the experimenters in physics spend their days trying to figure out ways of calculating *magnitudes* (consult Richard Feynman's elementary lectures to the Cal Tech undergrads for repeated showings that Math-Department intellectual values are of no interest to physicists). The giveaway that something other than science is going on in “theoretical” economics (and in math itself and in philosophy and, alas, in political science and sociology, those econowannabe fields) is that such an article contains not, from beginning to end, a single attempt at a magnitude.

“No worries,” says the Mainstream Economist, “We do econometric testing for fit when we need magnitudes.”

Give me a break. No one, including you, disagrees that not every statistically significant coefficient (or a high fit by any measure: insert here the latest vanity-named test in time series econometrics) is scientifically significant. Further, no one disagrees that some scientifically significant coefficients are not statistically significant.

All right. You like logic? Apply it. You've just admitted that statistical significance without a loss function (I didn't say “statistics” *tout court*) is neither necessary nor sufficient for scientific significance. Case closed.

You want observation? Go back to that copy of *The Physical Review*. I'll give you a nice, shiny euro if you can find a single instance in which statistical significance without a loss function is used in physics. Or if you wish, examine, as my students at UIC did last year, a big sample of articles in the magazine *Science* and discover for yourself that statistical significance is used in the idiotic way it is used in economics only in some few fields—meteorology, sometimes, and in parts of biology, population biology for example (the population biologists are also in love with qualitative theo-

rems, by the way: look no further for an explanation of the current hot romance between economics and evolutionary theory). Or on the economic side look into the *AER* in the 1980s, as Steve Ziliak and I did in a *JEL* article in 1996, and note that 70 percent of the empirical papers use *only* statistical significance to judge whether a coefficient is large or small (Steve has started our second paper, for the 1990s, and tells me that it's gotten . . . worse).

I am *so* tired of making these points. I am sure you are tired of me making them. But are you a serious scientist? OK, then: do you have a reply to either of them? If you think you do, publish it and send me a copy. In twenty years of making the two points with gradually increasing clarity I've not encountered a reasonable, or even logically coherent, reply to either. I have discussed them with some of the best economists in the world: no reply. If *you* don't have a reply, why do you go on committing what you admit are sins against real science?

What's going on? Maybe this: most scientists, like most people, don't want to change their minds, ever, about anything. They think it is undignified or something. I've never understood this, why people want to act like what Harry Truman defined as an Expert: "An Expert is someone who doesn't want to learn anything new, because then he wouldn't be an expert." It's true of all fields, of physics, of history, of geology, of literary criticism, of every science. We all, we mainstream economists, were trained in the two techniques, which came out in the 1940s. By now everyone in mainstream economics uses them. We must be right.

I've got a rhetorical proposal for breaking out of the mindset. It is to start calling the two techniques something other than "mainstream" economics. We need a name for the cargo cult in which so many of us were brought up. The cult can then be examined side by side, fairly, with other approaches to economics, such as Austrian economics or Marxist economics or institutional economics or the one I prefer, Scientific Economics Actually Researching the Causes of Happenings (SEARCH). As long as we call what fills up the *AER* and *JPE*, not to mention *JET* and *Econometrica*, just "the mainstream" the natural human tendency to float down the lazy river/ In the noonday sun will be irresistible. (Incidentally, European economics departments are becoming increasingly Americanized, leaping into the lazy, lazy river/ You can loaf along on the amazing argument that the leap will "raise scientific standards"; good Lord).

I propose the term "Samuelsonian." I was talking the other day in Rotterdam to Mark Blaug, that very learned and insightful historian of economic thought, and he agreed that Samuelson can fairly be blamed for the sin of qualitative theorems (Mark has a recent paper in which he details the Formalist Revolution of the 1950s). He was surprised to learn, however, that Samuelson, who has never done any empirical work, can *also* be blamed—at least as an initial and important cause—for the sin of statistical significance without loss functions. As I have explained (for example, in *The Vices of Economists, The Virtues of the Bourgeoisie* [1996]), Samuelson's first Ph.D. student, in the early 1940s, was Lawrence Klein, and the supervisor suggested to Klein that he do a Tinbergen-type study of the American economy, *using statistical significance as the criterion for the "importance" of, say, interest rates in determining investment expenditure.*

Two sins, one scientist. It's only fair to call the method of modern American economics "Samuelsonian." I mean, if he is going to get credit for this stuff he's also got to get the blame, too. (Modern economics could alternatively be called "Friedmanite," but Samuelson was advocating long before Friedman the same naïve logical positivism that economists associate with Friedman—incorrectly, in view of Friedman's rich and sophisticated empirical work such as *The Monetary History of the United States*).

Everyone agrees that Samuelson is a very nice guy, ethical, upright, warm. We have been very lucky in economics to have such a person as our Great MIT Leader. Believe me: linguistics has Noam Chomsky as its Great MIT Leader, and the results (the dogmatic refusal to admit pragmatic considerations into linguistics, for example; accompanied, I am told, by nasty personal behavior in aid of his ideas) has been bad for that science; linguistics programs are closing all over the United States. And you and I can agree as expert economists that Samuelson is a very great economic scientist, and that there was nothing at all unnatural about him being the third person to receive the Nobel (Tinbergen being the first).

So I'm not dissing Samuelson. But he would be the first to emphasize that we must consider not only honorable intentions but unintended consequences.

Let me give an example, in the career of Paul's friend and colleague and follower and defender, another very nice person and Great MIT Leader, Bob Solow. I am not dissing Solow, either. He too is a famously nice guy. He has been generous in his praise of my work, as of other work, such as feminist economics, with which he does not entirely agree. He's a prince. But he, too, would want to be judged by high standards as an economic scientist.

You may be surprised to learn that Solow's Ph. D. dissertation was empirical. One piece of work after it for which he is well known is his brilliant article in 1957 offering a way to measure "technological change." That was real science. It gave some math—"advanced" by the sorry mathematical standards of economics in 1957—that exhibited a functional form that allowed one to separate the contribution of investment from the contribution of technology to rises in output per person. In the article he went on to a simulation with real data and drew real historical conclusions, carefully examined (the theory took only a few paragraphs, and no proofs: just derivations in the spirit of physics). It was the framework for my own Ph.D. work a decade later, and for that of numerous other economic scientists interested in the actual world, such as Edward Denison, among the best applied economists of his generation, and Zvi Griliches, ditto, and Bob Gordon, ditto.

But then Solow went dramatically, Samuelsonianly wrong. He abandoned serious empirical inquiries into the world and started to do, among various other purely Samuelsonian projects, growth "theory," for which unhappily the Nobel committee cited him. (I'm not unhappy about him getting the prize: I'm saying that in a properly functioning science he would have gotten it for the technological change article.) Growth "theory" is not, the way it is usually practiced, like "theory" in physics. Like many economic ideas it *can* be brought to the actual world and confronted in actual functional forms: Bob Barro's text is a case in point (Barro was a Cal Tech undergrad, and didn't ever entirely lose the physicist's sense—maybe he learned it from Feynman—that Proof is trivial and that Simulation is not). But most growth "theory" is just

drivel, just logic, just one damned assumption after another with no attempt to answer the only scientific question, How Big.

Solow has gone on to more Samuelsonian economics. He fairly recently did a theory book with Frank Hahn that's got all the very great intelligence of these two men in it, but amounts to chess problems relative to any actual scientific use. What is worse, Solow's generation, and Solow himself as a great teacher and public figure in the field, educated and then hired people of my generation, and then of my students' generation, doing more and more refined Samuelsonian drivel in theory and more and more maniacal Samuelsonian misuses of statistical testing.

So: we need to start calling it Samuelsonian, not "neoclassical" or "mainstream" or any of the terms that give away the game before it's well started. The Samuelsonians are to be distinguished from the empirical if non-econometric Marshallians like Ronald Coase. Or with the empirical and econometric Economic Historians like Robert Fogel. Or with the empirical and mathematical Neo-Marxists like Sam Bowles (though they have sometimes fallen for Samuelsonianism). By contrast with these more scientific versions of economics, the Samuelsonian school of economics, in which I was trained, has left its scientific common sense behind—not because it overemphasizes Prudence as against other springs of action (it does) or imperfectly admires capitalism (it does). But because it indulges in the Two Secret Sins. We need to get beyond the Age of Samuelsonianism in economics, and get back to SEARCHing.

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