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OTHER THINGS EQUAL

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Why Don't Economists Believe Empirical Findings?

The Centre for Policy Research is a lively operation, founded in London in 1983 but involving economists from all over the world and especially Europe. It is a sort of European NBER, tending to Brookings (which distributes its papers in the USA), describing itself as "pluralist and non-partisan, bringing economic research to bear on the analysis of medium- and long-run policy questions." Among its many virtues is that it takes international comparisons seriously, takes economics history seriously, and takes the facts seriously. So the Centre does serious science.

The Centre sponsors a lot of conferences of economists trying to work things out. During mid-March to early August 1994 alone it sponsored or cosponsored twenty-one, six of them in that great centre for talk, Brussels, two in London, and the rest scattered from Cambridge, MA to Kiev. Out of the conferences come discussion papers and a fine *Bulletin* summarizing the conferences and the papers in English that any economist can grasp (write to the Centre at 25-28 Old Burlington Street, London W1X 1LB). So the Centre explains its scientific results clearly.

The *Bulletin* therefore is a good place to do a little experiment in how well economists persuade. I read the Winter 1993/94 issue from cover to cover, 44 double-spaced pages, asking myself how I as an economist reacted to the hundreds of arguments and findings reported there. I don't think my reactions are atypical. I've read a lot of applied economics in the past thirty years and read it no better or worse than the average economist. Being an economic historian, my interests in economics are perhaps broader than some. I was well trained in econometrics for someone of my generation, which means I'm an econometric idiot who doesn't know he is. My first job as an economist was as a transportation economist, which means I know that engineers know more than economists do. Admittedly, my economic priors are those of a Chicago School economist (subspecies *Coasiana fogelfania*). But everyone has priors, and in my experience it's wrong to believe that those of Chicago are especially tight. People say it, I know, but they're wrong about everyone except George Stigler, and are often speaking from a tighter dogmatism of their own. In short, as a sample of $N=1$, I'm not hopelessly biased.

The *Bulletin* reports on good stuff, all of it at least trying to be relevant to the actual economic world. I marked with a plus sign the findings that were on first blush scientifically believable and surprising, and put a zero beside those that were believable but unsurprising. In other words, when I felt myself learning something new about the world economy, or about the logic of economics, I marked it with a plus. In 44 tightly packed pages there were 32 pluses. When Angus Maddison said that the

high growth rates of the postwar period probably have a lot to do with the chaos before and during the War, it got a plus. When Robin Matthews said that measuring hours of work outside of manufacturing industry is hard, I learned something. When Richard Baldwin said that in the USA labor is mobile and in Europe capital is, I could see something I hadn't noticed before. When Gianni Toniolo and Konosuke Odaka said that beginning levels of economic growth were higher in Sweden and in Japan in the 18th century than has been realized, I sat up and listened. When Mathias Dewatripont said that a corporate culture was like infinitely-lived game players, I saw something new. The pluses were what is called in physical science, the "Aha!" effect. I could see my mind changing a little, learning more about the economic world. Mainly they come from what statisticians call "interocular trauma," a result that hits you between the eyes.

But what surprised me is how uncommon the experience was and how often I doubted what my fellow scientists were earnestly telling me. The result of reading 44 pages of hundreds of scientific results from the front line of applied economics was mainly that I believed surprisingly little of it. The minuses and zeroes outnumbered the pluses. Sometimes a minus seemed intellectually crude, ignorant about the history of the discipline, such as the assertion that "the Solow model" (of economic growth) was the same thing as "growth accounting," which it is not. Or: "the academic literatures on 'new growth' and economic geography have produced a variety of new theoretical and empirical insights" (on the contrary, they have reinvented the wheels of Smith and Marshall and the economic historians). Sometimes it contradicted itself, leading to indifference. Margaret Thatcher's reforms are said in successive sentences to have worked and to have not worked in raising British performance. Or on one page John Kendrick said that OECD countries converged after the War and on the next page Bart van Ark said they did not. Brief summaries, of course, can hardly do justice to a demonstration that the share of tradeables in Swedish national income has fallen, but on its face it's hard to believe, and I for one don't. No one really believes a scientific assertion in economics based on statistical significance, and the *Bulletin* contains quite a few (fewer than I had anticipated, though: I see that my prediction a long time ago that simulation would become the dominant argument style in economics is coming true). I therefore am not being weird when I disbelieve the statistically significant finding that knowledge services (e.g. computer consultations) don't affect manufacturing productivity. Nor does anyone in economics believe most of the arguments based on blackboard proofs, which even in such applied circles are embarrassingly common: such as Paul Krugman's blackboard proof that growth in places like Mexico City is a matter of complexity theory, nonlinear dynamics. Richard Baldwin and Jürgen von Hagen remarked on this one that "empirical...applications of complexity will pose major challenges," which puts it mildly.

I think you would have the same reaction. In fact I think you have it daily when you read the journals or listen to a colleague's talk or browse through the conference volumes produced by the Centre or the Bureau. Scientific results pour over you daily, but only a small percentage of them stick. Economists don't believe each other.

Now in a way this is not shocking. Most science has to be wrong or irrelevant, or else science would advance at lightening speed. It doesn't. The crystallographer and philosopher of science Michael Polanyi pointed out long ago that science supersedes itself and therefore most of what even the best scientists do will eventually be proven to be mistaken. As John Maddox, the editor of *Nature*, put it recently, "Journal editors, if they are honest with themselves, will acknowledge that much, perhaps most, of what they publish will turn out to be incorrect."

But I think it's worse in economics than in what we English speakers call "science." And I know it's worse than in historical science. Historians don't believe everything they read in the library. But they expect, rightly, to be able to rely on sheer factual assertions by their colleagues, and to have some confidence in their interpretations, if the signs of haste or of party passion are absent. (You can work out the signalling equilibrium here and make a pretty accurate prediction about what the profession of history is like.)

I would claim that in economics we have nothing like this degree of scientific agreement. To repeat, I don't believe it's merely a matter of my personal priors crashing up against the facts. You could put a minus sign in front of my priors and come to similar results. Most of the *Bulletin*, and most allegedly empirical research in economics, is unbelievable or uninteresting or both. It doesn't get down to the phenomena. It's satisfied to be publishable or clever. It's unbelievable unless you have to believe temporarily to get tenure.

The explanation is that economists when they write are tendentious. Good word, that: it means what we understand by "strong priors." Because they know always already (a useful phrase from German philosophy), they are not curious about the world. Contrast the books in history or biology or astronomy. The results come thick and fast and surprising. I read a book a while ago by E. C. Pielou called *After the Ice Age: The Return of Life to Glaciated North America* [Chicago University Press, 1992], and it was a page-turner, though it concerned three-spined sticklebacks and the East Coast refugia. Like economics, evolutionary biology and geology use stories constrained by simple principles of maximization. So why don't economists write books as gripping as Adrian Desmond's *The Hot-Blooded Dinosaurs* [Radius, 1990]? I have a British friend, Michael Summerfield, who is a geomorphologist (we hiked up a mountain in Italy together, and I tell you that an expert on landscape is the ideal guide for such an expedition). He wrote a textbook, *Global Geomorphology* [Harlow, 1991], 537 big pages in double columns filled with fact and argument about the landscape, without a dull paragraph. Why? Because it's all about the world, and it's all documented and believable. It's motivated by curiosity (which is why a lot of economic history or legal economics is better economic science than most economics).

Other fields have the same problem. I suspect that physics, for all its prestige, has something like the same oversupply of dull or clever unbelievabilities. Anthropologists, who on the whole fear and loath economics and therefore never bother to learn any, seem at first more curious. But read anthropology that takes the journalistic book by Karl Polanyi in 1944 (Karl was Michael's less smart brother) as the last

word on What Happened Under Capitalism and you'll find that they too are reading the world as they want it to read, tendentiously and therefore unbelievably.

Economists in other words are not curious enough to get their own data, talk to the economic actors, get right down into what is certainly the most interesting set of events in social life, the economy. They already know. How? This diagram on a blackboard. This clause in the platform of the Democratic Party. What economist do you know who has changed her mind? Bob Fogel has, four times. I have twice. Both are well above the mean and modal number of times, which is zero. But it's not a record of which either of us can be proud.

I think we should be worried that so much of economics is unbelievable tendentiousness. I think — don't you?— that economics ought to start getting serious about learning the economy. If even the Centre for Policy Studies, and the NBER, are going in circles, we've got a scientific problem.

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