OTHER THINGS EQUAL

How To Be Scientific in Economics

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I've been a fan of popularizations of science since I was a little boy. (I know that reads oddly, but there you are.) Merely the popularizations, not the actual, technical activities, at least if you use the crazy definition of "science" in English, which excludes economics and history and other sciences (all other languages, and English before 1850 or so, use the science-word to mean "systematic inquiry"). I was never any kind of physical or biological scientist. My compulsory college science course was "Rocks," a pre-tectonic geology which the conservative department at Harvard taught a year or two before American geologists could no longer deny that continents moved. When I was eight or nine I would read popular books on astronomy under the covers at night, Sir Arthur Eddington, that sort of thing, and at ten or twelve I subscribed to *Sky and Telescope*, the amateur astronomer's magazine. But I realized finally that astronomy was applied physics and that physics was applied math, and at age fourteen I "knew" I didn't like math, the way I "knew" I didn't like those oysters Rockefeller I'd never tasted. In high school and college I read popularizations of math itself, such as Bell's old (and historically unscientific) history of mathematics. After I actually learned some math in order to become an economist I went on reading in the history of mathematics, such as Kline's (less) old (and less historically unscientific) history, up through scientific histories of math, such as Mark Steiner's amazing work on Euler, *Mathematical Knowledge*. I haven't ever been able to read actual math books very seriously—I haven't even had a course in analysis, for Lord's sake—but I've studied their rhetoric and I've learned to appreciate pure math through popularizations, such as Courant and Robbins' exquisite *What is Mathematics?*, in the way one can appreciate art or music, and even sense the difference between Mozart and Stravinsky, without actually knowing how to do it. I especially favor life stories, such as G. H. Hardy's *A Mathematician's Apology* (1969), S. M. Ulam's *Adventures of a Mathematician* (1976); his younger brother Adam, who died a few months ago, was a close friend of my parents), Paul Halmos's *I Want to be a Mathematician: An Automathography* (1985), Constance Reid's *Courant in Gottingen and New York* (1976). You have to watch out in all these for an uncritically worshipful attitude towards pure math, in which num-

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ber theory turns out to be the purest of the pure. Imagine learning to appreciate economics by reading autobiographies of game theorists!

Notice, class, that I've made a distinction between science and math. Even in the books about math there is a recognition that math and science do not have the same ends in view. Constance Reid notes that

> Courant's primary concern was *existence*. The significance of this concern on the part of mathematicians is sometimes questioned by even quite sophisticated physicists. They are inclined to feel that if a mathematical equation represents a physical situation [the standard of "representation" being quantitative, calibrated simulation, oomph], which quite obviously exists, the equation must of necessity have a solution. (1976, 95.)

Well, fancy that: if a solar system or an economy seems to be pretty well following a rule of inverse square attraction or the picking up of profitable opportunities then what matters is *how big* an effect of Jupiter on Mars is, or *how big* an effect of taxation on investment is, not whether "a solution exists." Kline's book even argues that for the health of mathematics itself the mathematicians need to get back to subordination to physical (and I would say, social) problems.

The same point of course comes up in the *science* popularizations I like to consume. The great theoretical physicist Richard Feynman introduced a few simple theorems in matrix algebra into his first-year class at Cal Tech with considerable embarrassment: "What is mathematics doing in a physics lecture? . . . Mathematicians are mainly interested in how various mathematical facts are demonstrated . . .. They are not so interested in the result of what they prove" (1963, vol. I, 22-1). In the year of Our Lord 2000 Feynman's rhetorical question startles an economist. In the Math-Department lectures we call "theory" in economics nowadays it would be rather "What *besides* [Math Department, existence-theorem] mathematics should be in an economics lecture?" The other day here in Southern California I visited Mt. Palomar, where the 200-inch telescope resides, the heroic scientific project of my childhood, and bought a book about the instrument's building and use by Richard Preston. One of the Palomar observers said to Preston,

> The theoreticians"—he smiled—"the theoreticians are so clever. Once we have found something, they can find four ways to explain it. . . . In all these discussions. . . you find that you need hard numbers. How many quasars of this redshift? How many of that? [Preston, 1996, 27].

What I notice in the Scientists, as against the mathematicians, is an attitude towards How Big is Big which I don't see in most young economists these days. Young and old historians show it. Some older economists, yes; most young economists, no.

All the real Scientists show it. In the *Feynman Lectures on Computation* [1996, delivered in the mid-1980s] the subject, Feynman says, is "not actually science. It does not study natural objects. [But] neither is it, as you might think, mathematics; although it does use mathematical reasoning pretty extensively. Rather, computer science is like engineering—it is all about getting something to do something" [*ibid.*, xiii]. This entails asking How Big is Big? In a lucid explanation of predictive coding, for example, Feynman shows how a string of good predictions (rational expectations?) can be compressed into a single, short binary number, and then he immediately launches into How-Big rhetoric: "That's some saving of transmission space! . . . Predictive coding enables us to compress messages to quite a remarkable degree." One can use a Huffman technique for "an even tighter code. . . . You can get pretty close to Shannon's limit" [*ibid.*, 129].

Now I ask you, what economic theorist would put her theories into such a quantitative rhetoric? Answer: a Scientific economic theorist. How many are there? Not many.

In the appendix to the same book one of its editors tells Feynman stories, such as the conversations at Cal Tech's cafeteria (known as The Greasy; the scene reminded me of lunches downstairs at the Quad Club at the University of Chicago in the 1970s, where we always talked economics, and only economics): "One frequent topic for discussion was Feynman's explanation of some new experimental results obtained at the Stanford Linear Accelerator Center on electron proton scattering" [*ibid.*, 285].

What economic theorist would spend lunch talking about how to explain the latest result from economic historians or economic experimentalists? Answer: a Scientific one. How many? Not many.

The distinction between scientific economics and the chess problems that pass for "work" among academic economists was brought home to me by a visit by my former colleague at Iowa, Chuck Whiteman, to Riverside (where I spent the spring writing a book and enjoying being an angelina). It's a trifle odd that I had to be visiting in Riverside to hear a paper by my colleague of nineteen years at Iowa, but I had gotten frustrated with the inability of some of my colleagues there to grasp, tolerate, or reply to my two criticisms of modern economics (criticism of the gross misuse of statistical significance and, our present subject, the gross misuse of existence theorems). Towards the end I didn't go often to seminars there. I mean, if people insist on giving seminars in arithmetic in which $2 + 2 = 3.14159$, and get very, very angry if you naïvely keep noting that after all it is actually 4, what's a girl to do? (It reminded me again of Chicago, and why in 1980 I resigned from the Department there: I was beginning to realize that the methodology of positive economics was a lot of hooey, and I couldn't shut up about it and just go along; it drove everyone nuts, so I left.)

Chuck gave a terrific paper co-authored with Christopher Otrok and another former Iowa colleague, Ravi Ravikumar, called "Habit Formation: A Resolution of the Equity Premium Puzzle?" What they did, with some very fancy applied math and some reasonable guesses about parameters, was to show that *in magnitude* if you assume that people value many-year patterns of return, not just annual means and variances, you get crazy sensitivities: the *magnitude* of the implied equity premium and riskless return bounce all over the place. So something's screwy, *quantitatively*, and we need to think and observe again. This is economic science, not economic math or economic philosophy. Why? Because it asks How Big, and sees a way to answering it.

Plain Science gets its results with a minimum of fuss. It uses math when it needs to, but anyway asks in every paragraph, How Big? By contrast, Fancy Science is still science, still interested in "natural [or social] objects," but is science with an (mathematician's) attitude. As I understand the situation from my diligent reading of physics for simpletons, plain science is Feynmanian, Fancy Science is Murray Gell-Mannian. Then there's a third thing, that math in the Math-Department sense, focused entirely on consistency and existence, entirely uninterested in magnitudes, lovely, wonderful, interesting. I mean, wouldn't you want to know a proof of the four-color proposition? Such mathematics (and most of the "best" economics) is not about natural or social objects. It's about a world of possibility, without magnitudes corresponding to our actual world. That map-makers have never come across actual countries in which four colors did not suffice for a map is utterly uninteresting to a mathematician. Yet it's all that interests a scientist. Whatever mathematics is (philosophy? poetry?), it ain't science. The Whiteman, *et al.* paper was a mixture of Plain and Fancy. But science.

I looked at the 11 full-length papers in the March, 2000 issue of the *American Economic Review* with these categories in mind. I didn't read them thoroughly and I'll bet I'm being unfair to a few; even on my superficial reading I could see that all of them exhibited great intelligence and creativity. I'm not just saying that to be nice. These are very "smart" guys. Which is *so* wonderful, guys (all the authors, btw, were guys; hmm. . . interesting). The question, however, is whether the smart guys in the *AER* were using their intelligence and creativity for *science*. Were they doing science, or a depraved species of pure math? Was it real war, or chess. . . or chess problems?

The 11 papers on these definitions contained only two pieces of Plain Science, one from an oldster and one from a youngster: Gale Johnson's "Population, Food, and Knowledge" and Thomas Nechyba's "Mobility, Targeting, and Private School Vouchers." Two others were Fancy Science, Boyan Jovanovic and Dmitriy Stolyarov's "Optimal Adoption of Complementary Technologies" and Gary Bolton and Axel Ockenfels's "ERC: A Theory of Equity, Reciprocity, and Competition." They contained, alas, many useless pages of theorem-and-existence-proofs of a Math-Department character. But they both made serious attempts at last to bring the theory to the world (both, unhappily, used "significant correlation" in place of scientific importance, which makes some of their tests meaningless). So the score was: 2 + 2 = 4 pieces of real science out of 11.

One of the other 7 was a *t*-test hyperspace search. (Orley, *when* are you going to grasp this point and stop publishing this stuff?) You-all, gentle readers, know what I think of such searches. It would be as though the Palomar telescope had some flaw that made all the photographic plates wrong and irrelevant. The astronomers would spend many long, cold nights, and would think they were doing science. But they wouldn't be.

The 6 other papers of the 11 were entirely existence-theorem "theory," that is, statements that such-and-such might possibly happen *with no attempt to show that the happening was probable in the world as we know it and giving no guidance as to how one might find out*. (Alarmingly, most of the 6 blackboard exercises came to what they called "policy conclusions." Would you want to live in an economy in which uncalibrated exercises on blackboards were used to decide important things? I wouldn't.) As Kenneth Judd says in his remarkable book of scientific methods for our field, *Numerical Methods in Economics*, "Conventional theoretical analysis is very good at telling us what is possible and at exploring the qualitative nature of various economic phenomena. However, only after we add quantitative elements to a theory can we determine the actual importance of an analysis" [1999, 8]. Yup: it turns out that actual importance is the only thing that actual scientists care about. Isn't that *weird*?! So on the *AER* sample—you admit it's replicable, don't you, now?—over half of academic economics published in the best journals is not science. It's very hard stuff to do, the way some chess problems are hard to do, and the guys doing it have to be very, very smart, so on a labor theory of value or an IQ theory of value it's "work." Unhappily, on a scientific theory of scientific value it's useless. The work is diverting immensely smart guys from serious engagement with actual social objects.

How to be an economic scientist? Ask, How Big is Big? Simulate. Calculate. Measure. Observe. Think, too, of course: no one's against "theory" in the sense of finding four ways to explain something once we have found it. But find it in a usable functional form, and then calibrate on the basis of observation. That's the test of the theory in something other than a fool's version of positive economics (I've explained to you *many* times now why statistical "testing" in its usual form is not a test).

Feynman did physical science. Whiteman *et al.* do economic science. So should you.

Mostly, you don't. Please, *please*.

REFERENCES

Bell, E. T. *Men of Mathematics.* New York: Simon and Schuster, 1937.

Bolton, G., and Ockenfels, A. ERC: A Theory of Equity, Reciprocity, and Competition. *American Economic Review,* March 2000, 166-93.

Courant, R. and Robbins, H. *What is Mathematics? An Elementary Approach to Ideas and Methods.* London: Oxford University Press, 1969.

Feynman, R. P. *Feynman Lectures on Computation.* A. J. G. Hey and R. W. Allen, eds. Reading, Mass.: Perseus, 1996

Gould, S. J. *The Panda's Thumb: More Reflections in Natural History.* New York: Norton, 1980.

Halmos, P. *I Want to be a Mathematician: An Automathography.* New York: Springer-Verlag, 1985.

G. H. Hardy *A Mathematician's Apology.* London: Cambridge University Press. 1969.

Johnson, D. G. Population, Food, and Knowledge. *American Economic Review,* March 2000, 1-14.

Jovanovic, B., Stolyarov, D. Optimal Adoption of Complementary Technologies. *American Economic Review,* March 2000, 15-29.

Judd, K. L. *Numerical Methods in Economics.* Cambridge, MA: MIT Press, 1999.

Kline, M. *Mathematics and the Search for Knowledge.* New York: Oxford University Press, 1985.

Nechyba, T. Mobility, Targeting and Private School Vouchers. *American Economic Review,* March 2000, 130-46.

Preston, R. *First Light: The Search for the Edge of the Universe*. Revised ed. New York: Random House, 1996.

Reid, C. *Courant in Göttingen and New York. The Story of an Improbable Mathematician.* New York: Springer-Verlag, 1976.

Steiner, M. *Mathematical Knowledge*. Ithaca: Cornell University Press, 1975.

Ulam, S. M. *Adventures of a Mathematician*. New York: Scribner's, 1976.

Whiteman, C., Otrok, C., and Ravikumar, B. Habit Formation: A Resolution of the Equity Premium Puzzle? Discussion Paper: Department of Economics, University of Iowa, 2000.