OTHER THINGS EQUAL

Quarreling with Ken

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I've praised Kenneth Arrow here from time to time. But during the January 1998 meetings of the American Economic Association in Chicago, at which Ken for theory and Ed Leamer for econometrics, and Barbara Bergmann in the chair, discussed my new book *The Vices of Economists; The Virtues of the Bourgeoisie* [University of Michigan and University of Amsterdam Presses, 1997], I did not.

Regular readers of this column will have heard the gist (which is a *poor* excuse for not buying the book, dears; I need the money and you need the amusement of seeing your colleagues go red when you use arguments out of it). It is: Economics is in serious trouble because of the three "vices" of statistical significance, blackboard economics, and social engineering. A bourgeois and anti-aristocratic virtue, the virtues of bench-and-field scientists like Rosalind Franklin [Sayre, 1975] and Edward Wilson [1996] or theorists with a serious interest in the world like Richard Feynman [1985], can bring economics back to its scientific senses.

At the Chicago meetings I focused on the first two vices—statistical significance and blackboard economics. Since Ed Leamer has been a leader against mindless use of statistical significance [1978; 1983], and Ken declared decades ago that its common use is indefensible [1959], and Barbara has long and consistently advocated simulation as the scientific alternative [1990], I did not get any disagreement on Point One: Statistical significance is bankrupt; all the "findings" of the Age of Statistical Significance are erroneous and need to be redone; economists have thrown away gigabites of scientific time better spent on finding out how big is big—observing and estimating and arguing rather than pseudo-testing by Student's-*t*. That's news: leaders of the economics profession think that the main empirical rhetoric in economics is nonsense.

Point Two, the futility of blackboard theory, evoked a lot more quarreling, predictably between Ken and me. Kenneth Arrow, with his sister's husband's brother Paul Samuelson, was the pioneer of blackboard economics, and by 1950 had set the standard for chalk talk in the field. I was an eager student in the 1960s of such mysteries, but eventually decided that they were not science. At Chicago in a big Hyatt ballroom filled with economists this January we quarreled.

"Quarreling" is not necessarily a bad thing. C. S. Lewis pointed out once that quarreling—as against mere angry abuse—"means trying to show that the other man [or woman, if you please] is in the wrong. *And there would be no sense in trying to do* *that unless you and he had some sort of agreement as to what Right and Wrong are"* [(1952) 1996, 18, italics supplied]. Ken and I agree on the definition of scientific right and wrong. We agree that economics is an empirical science, not mainly philosophy. We agree that people like Rosalind Franklin and Edward Wilson and Richard Feynman are in this empirical sense scientists. They want to know about the world. As smart and admirable as the other kind of people may be—people like David Hilbert or Bertrand Russell or T. S. Eliot or for that matter C. S. Lewis when talking about theology rather than medieval literature—they are not worldly scientists.

So here's my side of the quarrel, trying earnestly to show that the defenders of blackboard economics are in the wrong *by their own standards of what constitutes empirical science*:

We all agree that there are two things we need to do as scientists if we are going to find out the world. Thinking and watching. Theorizing and observing. Imagining and feeling. Speaking and listening. Models and history. Metaphors and stories. Projecting our ideas out onto the world and accepting with humility the facts in the world.

You can view these two if you want as yang and yin, the male and female principle. It is the case that men seem on average to be more comfortable with theoretical models, women with empirical observing. There's plenty of overlap, obviously, but an interestingly large difference in first moments. It's not necessarily a matter of comparative advantage. Contrary to the usual routine in (blackboard) economics, tastes after all do differ. The great biologist Barbara McClintock was no slouch at theoretical genetics such as it was in the 1940s, but wanted also to acquire by observation, as she put it, "a feeling for the organism" [quoted in Keller, 1985]. In his classic of self-serving autobiography, *The Double Helix*, James Watson claimed of "Rosie" Franklin that "model-building did not appeal to her" [quoted in Sayre, 1975, 133]. Well, yes and no. Franklin had done brilliant model-building work on the chemistry of coal before she studied DNA. But as Anne Sayre puts it in her reply to Watson, "Models are not built out of thin air. . . . If nothing is known about a substance, a model cannot be built at all" [*ibid.*, 134].

What is plain is that we need both, preferably in the same person, a sort of scientific androgyny. Economists are fond of defending the split of thinking and watching by appeal to specialization. Sure. I get it. I can even draw the diagram on the blackboard. But if you don't then *trade*, the economics is not being correctly appealed to, is it? Unless the thinking and the watching are brought together in a scientific argument, such as Wilson's sociobiology (to pick a controversial example) or Stephen Gould's and Richard Lewontin's punctuated equilibria or the Alvarez's meteor account of mass extinctions or Simon Kuznets' account of modern economic growth, nothing scientific happens. You get what professional historians sneer at as antiquarian writing, mere piling up of facts; or what professional physicists sneer at as math-mongering, mere piling up of proofs. This is not controversial. Two centuries ago Immanuel Kant said that facts without concepts are blind, and concepts without facts not much use, either.

But you've got to do the real thing. You have to really be thinking— about the world. And you have to be really watching—the world. The trouble is that the two master techniques of modern economics, statistical significance and existence theorems have crowded out the real science.

The existence theorems which crowd the journals—and by this I mean *any "demonstration" of an effect that does not ask How Big*—are not real science. That is because the economists have adopted the intellectual values of the Math Department—not the values of the Departments of Physics or Electrical Engineering or Biochemistry they admire from afar. Gerard Debreu, in his presidential address to the American Economic Association, notes that the mathematical economist "belongs to the group of applied mathematicians, whose values he espouses" [1991, 4]; and he speaks of "the values imprinted on an economist by his study of mathematics" [*ibid.*, 5]. Debreu realizes that physicists do not share these values: unlike economics, "physics did not surrender to the embrace of mathematics and to its inherent compulsion toward mathematical rigor," but on the contrary occasionally was led "to violate knowingly the canons of mathematical deduction" [*ibid.*, 2]. You're not kidding. Physicists use with abandon self-contradictory mathematics when it works as simulation, the first few terms of divergent infinite series, for example. But economists, says Debreu, do not have enough experimental data, and therefore must rely on deductive methods. Considering, he claims, that economics is "denied a sufficiently secure experimental base"—"economic theory has had to adhere to the rules of logical discourse and must renounce the facility of internal inconsistency" [1991, 2]. That is, we have to stay on the blackboard, and be rigorous there by the standards of the Math department, because we poor economists have so little information about the world. Would that we were physicists and had all those data! But sadly it is not to be, and we are condemned to the blackboard.

It needs to be said how silly this argument is. (My remarks about Debreu did not please Ken in Chicago; he tried to claim that I was thereby opposing Theory, which makes it easy to deflect my case; but the case is not about the existence of Theory; the issue is quantitative, its amount.) For one thing economists are drenched in data, as hard as may be, and recently even experimental data. Unless astrophysics and geology are to be accounted non-sciences because they do not experiment much, observational data are data, too, what we mainly can hope to have in paleontology or history or economics. The word "data" anyway shows the real problem: it means in Latin "things given," which suits the scholasticism of blackboard theory but not modern science. The better, less mathematical, and more scientific word would be *capta,* "things seized" in long, cold nights at the telescope or long, dry days in the archive. The data are not "given" to physics: they are seized, with great difficulty. An astrophysicist studying neutron stars has thin and puzzling data, but she examines them closely, and lusts to have more. A theoretical economist, by contrast, fabricates some "stylized facts" out of his head and then devotes the rest of his career to axiom and proof.

And for another thing the claim that consistency-mongering can lever us up into a scientific world is obviously silly. Debreu has not thought much about *why* you would want to elevate consistency to the only intellectual value. He brings out the old chestnut that "a deductive structure that tolerates a contradiction does so under the penalty of being useless, since any statement can be derived flawlessly and immediately from that contradiction" [*ibid.*, 1991, 2]. But if this is true, if a deductive structure that tolerates a contradiction is useless, then calculus for the first two centuries of its existence was useless. And much of economics before G. Debreu and K. Arrow and their less gifted students came over from the Math department to slay Inconsistency was according to his criterion similarly useless.

The notion that "if not consistency, then chaos" is not admitted even by the best logicians. In the work of logicians such as Anderson and Belnap, reports James McCawley, "a contradiction causes only *some* hell to break loose" [1981, xi]. Consistency is not to be spurned, but it is not the master virtue, except in the Math Department.

So it just won't do, these qualitative defenses of endless A-prime, C-prime theorizing. But the crux of the matter between me and Ken is scientific, that is, quantitative. Even if you thought, as I do, that some economists should be assigned to exploring the non-quantitative connections between assumptions A and conclusions C, the percentage of academic economists who now spend their days on such stuff—in the "best" departments on the order of half of the person hours (the rest spent on statistical significance)—is bizarre. Ken tried to argue that mathematical economists are a beleaguered minority, which I hope is correct. Want a job? Flee formal theory. But the trouble is that meanwhile the A-primers have persuaded everyone to do their same job, spending all day on qualitative logic when what matters for science is quantitative logic.

That was our quarrel: the quantitative question of how much time economic scientists should spend on quantitative questions of How Much. Ken and I differ sharply on this. Ken wants more A-prime, C-prime theorizing in the absence of quantitative discipline (and forget about Student's-*t* as "discipline"), the usual exploration of assumptions, low brow or high brow, diagrams or fixed point theorems. I want less. How much less? About a tenth of what we have.

And here's the odd part: Ken agrees with my definition of science. As I said, there would be no sense in quarreling unless he and I had some sort of agreement as to what Right and Wrong are. Why then doesn't he agree with my quantitative judgment, that we're spending too little time on quantitative work by a factor of ten? You'll have to ask him, and the other economists who defend the unscientific status quo. He'll be polite and masterful as always. But as at Chicago, I think, he won't know.

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