from W.N. Parker, ed.
Economic History and the Modern Economist.
Oxford and NY: Blackwell, 1986. The piece
is a comment on papers by Solow, Arrow,
Temin, and David presented at the December,
1984 meetings of the American Economic
Association, held in Dallas, Texas.

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Economics as an Historical Science

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Like an Iowan relative to a Californian and a New Englander, an economic historian is a convex combination of two coasts. One may note, however, that convex combinations are often superior to their end points. Still the papers by Temin and David are, I think, *exemplary* in both senses of that word, exhibiting, as they do, the economist's grasp of theory and the historian's grasp of fact. Here my critique of them will rest. We economic historians have an agreement like the one among the other hybrid economists, the mathematical theorists, discouraging critical comment about colleagues. It yields good results in power and salaries. I would not wish to be the first in the amiable history of historical economics to violate the agreement.

I have no such agreement of mutual nonaggression with the theorists. Unluckily, however, I endorse most of the answers that Kenneth Arrow gave in his paper and all that Robert Solow gave in his. I am driven to attacking the question they were asked.

This hardly seems fair, since it was not Arrow and Solow but the organizers who asked the question. What, asked the elders of the American Economic Association, is the proper relationship between economics and economic history? Or, to put it another way, is economic history necessary for an economist? Our panel of theorists answered the question correctly. Yes, said Arrow, economic history supplies data for the theories of economists, and puts the theories through toughening

exercises. Yes, said Solow, a properly modest economic theory and a properly ambitious economic history could, with mutual advantage, exchange the equilibrium conditions of the one for the side conditions of the other. Arrow and Solow differ in mood. Arrow is optimistic about the present course of economic science, Solow is not. Yet both emphasize the gains from trade between economics and economic history.

In other words, Arrow and Solow accept the implicit premise that the two fields differ. That is the mistake that makes the question wrong. I say they do not differ. Since economics and economic history have the same tastes and technology and endowments they have no basis for trade. Economically speaking they are the same country.

Historical economists for their part sometimes mistake the sameness, arguing that economic history is a proper subset of economics. *Au contraire*, as they say in France. The point is that economics, in view of what it is rather than what it claims to be, is a proper subset of history.

By this I mean that economists are trying to do the same thing as historians, namely, to tell plausible stories about the past. The alternative view, which Arrow believes, is that economists are social physicists, looking for a unified field theory of society. Most economists cling to a quaint positivism supporting this notion, supposing that social physicists should predict and social engineers control. Economists are to test the theory at Iowa by its observable predictions, like the big boys at the Fermi Lab; then they are to use the theory at Harvard to design policy bombs, like the big boys at Livermore; then they are to drop them in Washington, like the big boys at Los Alamos. By emulating them the economists believe they will share in the peculiar prestige of the big (though young) physicists.

The notion is that there is a one-to-one correspondence between big physics and big economics. The econometric tests of cross-equation restrictions (*any* cross-equation restrictions, as Solow might say: don't worry, you'll find some) are

supposed to correspond to crucial experiments in physics. The axiomatization of general equilibrium theory is supposed to correspond to physical theory. Since the 1940s, from Samuelson to Sargent, economists have been telling one another repeatedly that economics corresponds to some piece of physics, pure or applied, to thermodynamics or to electrical engineering. Not knowing much about how research actually proceeds in physics or engineering the audience has believed the tale. Or at any rate they have believed it enough to teach it to their students and to get themselves into the National Academy of Sciences. But they have not believed it enough to actually do it. Economists are not hunters of laws; they are hunters of stories.

The case is easiest to make with applied economics, nine-tenths of the intellectual world of economists. Like economic historians studying the history of AT&T's divestiture or of QWERTY's persistence, economists studying the activities of the CAB in 1984 try to tell a story with a beginning, a middle, and (happily in this case) an end. They succeed or fail by narrative standards. They want to connect one event to another. For all their talk of hypothesis testing they are not actually testing, say, some theory of regulation (no wonder: the 'theory' amounts to saying that people usually do what is advisable).

Telling stories is how we make sense of what has happened. Stories tell: 'Where does all this stuff come from?' Once upon a time there was a big bang . . . 'How did we get so rich?' There were once some tinkerers in Britain . . . 'How did the French Revolution spread to Europe?' There was once in Corsica a son of Carlo and Letizia Bonaparte . . . 'What is our life?' There was once in Bethlehem . . .[1] The attempt by Carl Hempel and others in the 1940s to force the storytelling into a positivist model has failed (even Hempel knew that historians write stories, not laws). It works no better in the branch of economic history known as applied economics.

Applied economics commonly tells its stories these days with statistics and mathematics rather than mere words. But the

figures of speech it uses are beside the point. Simulation of the American economy in recession or of the Midwest in the railroad age are no less stories than *Through the Looking Glass*, with which indeed they share other features:

> 'I can't believe *that*!' said Alice.
> 'Can't you?' the Queen said in a pitying tone . . . 'I daresay you haven't had much practice . . . When I was your age, I always did it for half-an-hour a day. Why, sometimes I've believed as many as six impossible things before breakfast!'

The belief comes from mathematical storytelling. The rhetoric of statistics misleads the econometrician into thinking that by running a hyperplane through his beliefs about the statistics he is subjecting his beliefs to 'test.' But he is not testing them – as he can understand by recognizing how insignificant are his tests of significance – but expressing them, telling them, fitting them to the crude facts, in a word, simulating them. Simulation, the engineer's word for the telling of hypothetical stories disciplined by fact, is the economist's main figure of speech.

Historians do it, too. The historian of medieval English law wishes to tell a story that by 1300 someone recently dispossessed of his property (a victim of 'novel disseisin') could take it back only with the help of the king and not by vigilante justice. He imagined how Bracton could have come to his four-day rule of ejectment or how purchasers of land would have needed protection against the death of their seller.[2] Like an engineer or applied economist, he practices the trick of simulating the important possibilities, disciplined by expert knowledge of the social structure. The economic historian of medieval open fields imagines how scattered plots would affect risk and simulates the result mathematically. Both historians simulate in aid of a story. So does the economist trying to fit his equation for the demand for money: they all want to tell stories, of how the king won the rule of law, how communal agriculture rose and decayed, how the Federal Reserve has done its job since the War.

If applied economists were law-hunting instead of storytelling they would predict. As the Queen said, 'It's a poor sort of memory that only works backwards.' The pressures to try to work the memory forward are immense. The businesspeople, the bureaucrats, the journalists, his mother – all want the economist to tell what the future will bring. The governing metaphor is weather forecasting.

However, it is becoming pretty clear that economists are poor predictors. On practical and, what is more depressing, theoretical grounds the economists cannot forecast well. The cold fronts are listening, and the forecasters are themselves part of the weather.

If economists go on indulging the misapprehensions of their customers, issuing predictions about next month's exchange rate or next spring's interest rate, the loss of reputation when the customers catch on will be large, and richly deserved. It would be better to declare a victory and go home. The failure to forecast *is* a victory for the science. Precisely because economic science is such a fine way of telling stories about the past there are no unexploited opportunities lying about to be seized by professors in battered tweed jackets. A leading principle of economics, after all, is the American question: If you're so smart, why aren't you rich?

Applied economics, then, is the economic history of the recent past. When done well it has the air of good history written by someone who has taken Differential Equations 152. But the hard case is supposed to be economic theory itself, and its handmaiden, econometrics. Surely these are 'nomothetic': 'law giving.' Surely this is the physics, as Arrow claims, the laws for all time (or at least until the next regime, or until once again the stationarity breaks down). In such an analogy, as Arrow says, economic history would be the geology to the physics of economic theory, applying the laws developed in the lab to tell a story in the field.

Solow argues that this characterization of economic theory is unpersuasive, and I agree. To his weighty case I can only add a

further question. What *are* these laws of the social physics? When economists name something a Law, in the style of Boyle's Law or Ohm's Law, there is commonly a playful irony involved, a sense that after all it is a poor thing, a mere fact, such as Denison's Law or Okun's Law (whose recent histories have not been happy).

The permanent laws that economists believe are bold enough. The Law of Demand or the Law of Profitable Entry, for instance, irritate anthropologists and have other features commending them for use. But the scientific ceremonies of testing have little to to with their persuasiveness. How many economists, for instance, have been persuaded of the Law of Demand by the ceremonies involving complete systems of demand equations popular amongst the Netherlanders? Really and truly? Or again, are economists really going to succeed in 'testing' the law of rational expectations? And if they do, will the test be worth anything beyond the telling of a good story about 1933 or 1984? What *are* those 'results' so long promised by econometrics?

Understand, I (like Solow) am no machine breaker. Theory is thinking about economic behavior, and econometrics is thinking about economic statistics. We're all in favor of thinking. The gains in penetration of understanding forced on economists by 200 years of economic thinking, and in breadth of understanding forced by 100 years of economic history, have been immense. But the gains in lucidity forced on economists by the 40 years of mathematization of economic theory and statistics have been large, too. It is the false analogy with physics to which Solow and I are objecting, not to the use of the calculus of variations when the dog is, after all, pursuing his master. It is not the formal techniques of physics themselves that are the problem but the metaphysical incantations that come along with them.

To put it another way, if economists need a big brother to admire, it should probably not be the physicist. Economists are more like geologists or paleontologists, telling stories of the

Pacific plate or the panda's thumb. There is no prediction, no experiment. There is just mucking about in libraries and computing centers, thinking the stories through and checking to see if they square with historical facts laid up in archives. If economists need a big brother, he could come from these historical sciences, or from history itself. A field that took the English legal historian Frederick Maitland or the French rural historian Marc Bloch for its heroes would not do badly.

Except, of course, in pay. A corrupting feature of the myth of social physics is its claim to undergird an insightful, profitable, purchasable social engineering. But of course if economics were to give up the nomothetic myth it would have to give up, too, the big con of big science, namely, that the study of the age of the universe or the character of quarks, like the seventeenth model of a possible world of international finance, is more *useful* than the study of Indo–European vowels or the structure of Latin poetry. This, however, is another matter, this relationship between an economics that saw itself plain and the other half of our civilization.

Anyhow, the cat is out of the bag, the shoe is on the other foot, the emperor is without his clothes. Economics is neither social physics nor social engineering: it is more like a peculiar variant of social history. Economics does not merely have a lot to learn from history: history is what it is.

Notes

1 Wayne C. Booth, *Modern Dogma and the Rhetoric of Assent*, Chicago University of Chicago Press, 1974, p. 186.

2 Donald W. Sutherland, *The Assize of Novel Disseisin*, Oxford: Oxford University Press, 1973, pp. 103, 113.