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# Economic History and Economics

By ROBERT M. SOLOW\*

I have in the back of my mind a picture of the sort of discipline economics ought to be—or at least the sort of discipline I wish it were. If economics were practiced in that way there would be nothing problematical about its reciprocal relationship with economic history. It would be pretty clear what it is that economic theory offers to economic history and what economic history offers to economic theory. I will try to describe what I mean below.

For better or worse, however, economics has gone down a different path, not the one I have in mind. One consequence, not the most important one, but the one that matters for this discussion, is that economic theory learns nothing from economic history, and economic history is as much corrupted as enriched by economic theory. I will come to that, too, later on.

You will notice that I am using strong language. I am prepared to admit right away that I may be dead wrong in my judgements. But there is no point in pussyfooting. Bluntness may lead to an interesting discussion. After all, no one would remember the old German Historical School if it were not for the famous *Methodenstreit*. Actually, no one remembers them anyway. (There must be a lesson in that.)

To get right down to it, I suspect that the attempt to construct economics as an axiomatically based hard science is doomed to fail. There are many partially overlapping reasons for believing this; but since that is not the topic under discussion today, I do not have to lay them out in an orderly way. I hope the following hodgepodge will convey what I mean.

A modern economy is a very complicated system. Since we cannot conduct controlled

experiments on its smaller parts, or even observe them in isolation, the classical hard-science devices for discriminating between competing hypotheses are closed to us. The main alternative device is the statistical analysis of historical time-series. But then another difficulty arises. The competing hypotheses are themselves complex and subtle. We know before we start that all of them, or at least many of them, are capable of fitting the data in a gross sort of way. Then, in order to make more refined distinctions, we need *long* time-series observed under *stationary* conditions.

Unfortunately, however, economics is a social science. It is subject to Damon Runyon's Law that nothing between human beings is more than three to one. To express the point more formally, much of what we observe cannot be treated as the realization of a stationary stochastic process without straining credulity. Moreover, all narrowly economic activity is embedded in a web of social institutions, customs, beliefs, and attitudes. Concrete outcomes are indubitably affected by these background factors, some of which change slowly and gradually, others erratically. As soon as time-series get long enough to offer hope of discriminating among complex hypotheses, the likelihood that they remain stationary dwindles away, and the noise level gets correspondingly high. Under these circumstances, a little cleverness and persistence can get you almost any result you want. I think that is why so few econometricians have ever been forced by the facts to abandon a firmly held belief. Indeed, some of Fortune's favorites have been known to write scores of empirical articles without once feeling obliged to report a result that contradicts their prior prejudices.

If I am anywhere near right about this, the interests of scientific economics would be better served by a more modest approach. There is enough for us to do without pretending to a degree of completeness and

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precision which we cannot deliver. To my way of thinking, the true functions of analytical economics are best described informally: to organize our necessarily incomplete perceptions about the economy, to see connections that the untutored eye would miss, to tell plausible—sometimes even convincing—causal stories with the help of a few central principles, and to make rough quantitative judgments about the consequences of economic policy and other exogenous events. In this scheme of things, the end product of economic analysis is likely to be a collection of models contingent on society's circumstances—on the historical context, you might say—and not a single monolithic model for all seasons.

I hope no one here will think that this low-key view of the nature of analytical economics is a license for loose thinking. Logical rigor is just as important in this scheme of things as it is in the more self-consciously scientific one. The same goes for econometric depth and sophistication, maybe even more so. I mentioned "rough" quantitative judgment a moment ago, but that was only to suggest that the best attainable, in macroeconomics anyway, is not likely to be precise, if we are honest with ourselves and others. It would be a useful principle that economists should actually believe the empirical assertions they make. That would require more discipline than most of us now exhibit, when many empirical papers seem more like virtuoso finger exercises than anything else. The case I am trying to make concerns the scope and ambitions of economic model building, not the intellectual and technical standards of model building.

I claimed earlier that the natural relation between economics and economic history would be clear and straightforward if only economics were practiced in the fashion I have just sketched. Now I had better say what I meant. If economists set themselves the task of modeling particular contingent social circumstances, with some sensitivity to context, it seems to me that they would provide exactly the interpretive help an economic historian needs. That kind of model is directly applicable in organizing a historical narrative, the more so to the extent that the

economist is conscious of the fact that different social contexts may call for different background assumptions and therefore for different models.

The other direction of influence, what economic history offers to that kind of economic theory, is more interesting. If the proper choice of a model depends on the institutional context—and it should—then economic history performs the nice function of widening the range of observation available to the theorist. Economic theory can only gain from being taught something about the range of possibilities in human societies. Few things should be more interesting to a civilized economic theorist than the opportunity to observe the interplay between social institutions and economic behavior over time and place.

I am going to illustrate by referring to the work of W. H. B. Court, not merely because his book *The Rise of the Midland Industries* was on A. P. Usher's reading list when I took his course in the late 1940's. I choose Court for no better reason than that I happened to run across an obituary article about him in the *Proceedings* of the British Academy for 1982. (Since Court died in 1971, Fate did seem to be playing a hand.)

Here, for instance, is an excerpt from Court's volume on *Coal* in the U.K.'s official history of World War II.

Observers who found the conduct of the mineworkers puzzling assumed that, in the normal way, a man who finds himself faced with the possibilities of higher earnings will be prepared to put out extra effort to obtain them. An assumption about the conduct of an individual is as a rule, however, also an assumption of some sort about the society in which he lives and of which he is a member. The individual's demand for income, his views upon the getting and spending of money, are usually formed by the part of society which he is most in touch with. For most men the social code, whatever it may be in their time and place, is something which they accept as given and take over with little demur or questioning. Before one can assume that a demand for additional income existed

on the coalfields and could easily translate itself into extra work, one has to ask whether the mining community had those standards or those habits. If it did not, and if it was unable to develop them in a short time, then even a rapid rise of wage rates might bring about no appreciable change in the working habits of the industry.

In his own methodological writing, Court made the point explicitly that men "living as they do in different societies...make their decisions according to different schemes of values and according to the habits and structures of the society they find themselves living in." Therefore an economic historian should be an "observer and re-creator of the codes, loyalties and organizations which men create and which are just as real to them as physical conditions." Add to that a command over two-stage least squares and you have the kind of economic historian from whom theorists have most to learn, if only they are willing to try. I have naturally lit on this passage about the labor market because that is the branch of theory I happen to be engaged in right now, but no doubt the thought would apply equally well to consumer spending or rivalry among firms. I must promise myself, before I lecture again on wage bargaining, to ask my students to read the chapters on "The Wage Bargain" and "The Concept of the Minimum" in Court's *British Economic History, 1870–1914: Commentary and Documents*. I wonder what they will make of it.

So much for the normative. If you read the same journals I do, you may have noticed that modern economics has an ambition and style rather different from those I have been advocating. My impression is that the best and brightest in the profession proceed as if economics is the physics of society. There is a single universally valid model of the world. It only needs to be applied. You could drop a modern economist from a time machine—a helicopter, maybe, like the one that drops the money—at any time, in any place, along with his or her personal computer; he or she could set up in business without even bothering to ask what time and which place. In a little while, the up-to-date economist

will have maximized a familiar-looking present-value integral, made a few familiar log-linear approximations, and run the obligatory familiar regression. The familiar coefficients will be poorly determined, but about one-twentieth of them will be significant at the 5 percent level, and the other nineteen do not have to be published. With a little judicious selection here and there, it will turn out that the data are just barely consistent with your thesis adviser's hypothesis that money is neutral (or nonneutral, take your choice) everywhere and always, modulo an information asymmetry, any old information asymmetry, don't worry, you'll think of one.

All right, so I exaggerate. You will recognize the kernel of truth. We are socialized to the belief that there is one true model and that it can be discovered or imposed if only you will make the proper assumptions and impute validity to econometric results that are transparently lacking in power.

Of course there are holdouts against this routine, bless their hearts.

As I inspect current work in economic history, I have the sinking feeling that a lot of it looks exactly like the kind of economic analysis I have just finished caricaturing: the same integrals, the same regressions, the same substitution of *t*-ratios for thought. Apart from anything else, it is no fun reading the stuff any more. Far from offering the economic theorist a widened range of perceptions, this sort of economic history gives back to the theorist the same routine gruel that the economic theorist gives to the historian. Why should I believe, when it is applied to thin eighteenth-century data, something that carries no conviction when it is done with more ample twentieth-century data?

The situation reminds me of a story I once heard told by an anthropologist who had spent some months recording the myths and legends of a group of Apache in New Mexico. One night, just before she was scheduled to end her field work and depart, the Indians said to her: We have been telling you our legends all these months—why don't you tell us one of yours? The anthropologist thought fast and then responded brilliantly by telling the Indians a version of the story of Beowulf.

Years later she picked up a copy of an anthropological journal and found in the table of contents an article entitled "On the Occurrence of a Beowulf-like legend among the such-and-such Apache." If economic history turns into something that could be described as "The Occurrence of an Overlapping-Generations-like Legend among the Seventeenth-Century Neapolitans," then we are at the point where economics has nothing to learn from economic history but the bad habits it has taught to economic history.

Let me recapitulate. If the project of turning economics into a hard science could succeed, it would surely be worth doing. No doubt some of us should keep trying. If it did succeed, then there would be no difference between economics and economic history other than the source of data, no more than there is a difference between the study of astronomical events taking place now and those that took place in the Middle Ages. In this dispensation an economic historian is merely an economist with a high tolerance for dust or—what is rarer these days—a working knowledge of a foreign language.

There are, however, some reasons for pessimism about the project. Hard sciences dealing with complex systems—but possibly less complex than the U.S. economy—like the hydrogen atom or the optic nerve seem to succeed because they can isolate, they can experiment, and they can make repeated observations under controlled conditions. Other sciences, like astronomy, succeed because they can make long series of observations under natural but essentially stationary conditions, and because the forces being studied are not swamped by noise. Neither of these roads to success is open to economists.

In that case, we need a different approach. The function of the economist in this approach is still to make models and test them as best one can, but the models are more likely to be partial in scope and limited in applicability. "Testing" will have to be less mechanical and more opportunistic, encompassing a broader collection of techniques. One will have to recognize that the validity of an economic model may depend on the social context. What is here today may be gone tomorrow, or, if not tomorrow, then in ten or twenty years' time. In this dispensation there is a clear and productive division of labor between the economist and the economic historian. The economist is concerned with making and testing models of the economic world as it now is, or as we think it is. The economic historian can ask whether this or that story rings true when applied in earlier times or other places, and, if not, why not. So the economic historian can use the tools provided by the economist but will need, in addition, the ability to imagine how things might have been before they became as they now are. These are the sensitivities Court spoke of in the passage quoted above. I take it, naively perhaps, that they represent the comparative advantage of the historian.

In return, economic history can offer the economist a sense of the variety and flexibility of social arrangements and thus, in particular, a shot at understanding a little better the interaction of economic behavior and other social institutions. That strikes me as a meaningful division of labor. It was once suggested—by my kind of economist—that the division of labor is limited by the extent of the market. Perhaps what I have just been doing can be thought of as marketing.