# THE PLACE OF ECONOMIC HISTORY IN THE DISCIPLINE OF ECONOMICS

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*Despite the dominance of the field of economic history by trained economists who use the tools of economics, the output and services of economic historians are declining in demand. The explanation advanced in this address is that economic historians have not proved their value to economics. To play an essential role in economics, we must broaden the questions we ask and concommittently devise a set of theoretical tools to provide meaningful and testable theories about the structure and performance of economies over time. This address suggests the direction that such theory should take in order to make the field an indispensable part of the discipline of economics.*

Economic history is in a paradoxical dilemma today. On the one hand, research in the field is flourishing. The new economic history has replaced the old. Economic historians are predominantly economists rather than historians and systematically use the tools of economics. We have even managed to have one of our representatives appear as the star on the Today Show and be portrayed in *Time* magazine. Yet fame (if not fortune) has gone hand in hand with a declining demand for our output. A recent essay by Don McCloskey in the *Journal of Economic Literature*[1] tells the story. The major economics journals today publish far fewer essays in economic history than they did 50 years ago. The job market is an even more convincing demonstration. Economic historians were in widespread demand 5-10 years ago. Today we have an unemployment rate which probably exceeds the national average and certainly exceeds the scholarly average as compared to other branches of economics. By the market test we are a failure.

The importance of economic history to the discipline of economics has long been recognized. A distinguished list of our forebearers, from Smith and Marx to Marshall to Shumpeter and Viner, saw that economic history could provide a dimension of time to the static world of the economist; put together the pieces of an economic system rather than look at isolated bits of the jigsaw puzzle; incorporate an awareness of the interdependence of economic organization with other aspects of society; analyse the parameters that the economic theorist takes as given.

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1. "Does the Past Have Useful Economics," June 1976.

If economic history is a failure, the discipline of economics will be the poorer for it.

One can advance two alternative hypotheses to account for our plight. One hypothesis is that we have very little of interest to say to economists today and therefore they are correct in ignoring us. The other hypothesis suggests that market failure characterizes the discipline of economics. McCloskey opts for the latter argument — a peculiar position for an economist from the University of Chicago to take. McCloskey does make a case. There have certainly been important contributions made by economic historians in the past 25 years that have been slighted by their economics colleagues. Yet a powerful case also can be made for the former argument. Take, for example, the study of economic growth. What have economic historians contributed towards enlightenment in a field that is central to economic history. I must confess very little. Both standard neo-classical growth models and the more recent and fashionable evolutionary growth models are predicated upon an incentive structure which is taken as a given in the models. They have been employed by economic historians to analyze economic growth in the 19th and 20th centuries. These have by and large become exercises in applied economic theory in which making varying assumptions about the production functions involved, we have ground out relatively trivial results. Yet an unambiguous fact about the historical past is that incentives to engage in productive activity — in production, exchange and innovation — have varied enormously, and frequently if not typically have either been absent or severely circumscribed. This story that economic historians should be portraying is nowhere systematically developed.[2] Yet that is in fact what the serious study of economic growth is all about. Economic growth or the lack of it is obviously a consequence of the incentive structure which influences choices with respect to children, savings, and productive activity. But our study of economic growth has been an exercise in applied arithmetic or simply employed bad economics, and has remained a dull and sterile field.

It seems to me that we have been ignored by the economists for good reasons. We have simply been taking their traditional tools and applying these mechanically to the past — and the recent past at that, since implicitly most of the field appears to agree with Walt Rostow that the world of the pre-Industrial Revolution is quite irrelevant to the study of economic growth. If that is all we can do, then we are truly expendable and Economics Departments are quite right in relegating us to a marginal position in their staffing requirements.

While the new economic historians were capturing the field of economic history, they left the really interesting questions to the historians, the sociologists and the anthropologists. It is scholars in these fields who have been concerned with the structure of societies, with non-

2. For an examination of the issues, see Douglass North, "Economic Growth: What Have We Learned from the Past," supplement to *Journal of Monetary Economics* (forthcoming).

market forms of economic organization and distribution and with economic growth and decline. We left them with the interesting questions but without the tools to be able to produce interesting theory. The result is that the followers of Moses Finley, Karl Polanyi and most recently Immanuel Wallerstein point to the irrelevance of economic theory in analyzing the past. Karl Polanyi in particular was influential in reinforcing the view of anthropologists, sociologists, and historians that economic theory is simply irrelevant to the distant past, that it rather is applicable only to the world of the 19th century. Yet the failure here is not that of economics. The fault lies with economic historians who have not been willing to extend economic theory to make it useful in dealing with the range of problems that would enable us to analyze the past. What distinguishes the social sciences from the physical sciences is that the former is concerned with human choice. Human activity is now and always has been characterized by decision making on the part of participants and the job of the economic historians is and always has been to develop a framework of the choice alternatives that people faced over time in order to make sense out of the past. It is only with such a set of tools that we can develop consistent and refutable theory.

If this theory is to be interesting, it must focus on the structure of the economy and the relationship between structure and performance. The structure must specify the sources of decision making in the system and the parameters within which decisions are made. Then and only then are we in a position to see the nature of incentives to engage in productive activity and therefore to derive from these some analytical statements about the performance of economies over time.

Let me be explicit. There are four sources of decision making by any society. 1) Decisions made within households. 2) Decisions made by voluntary organizations. 3) Decisions made within markets. And 4) decisions made by governmental bodies. A useful economic history must explain the constraints governing decision making within units and must also explain why changes in the source of decision making between households, voluntary organizations, markets and governments have varied over time. The tools of the new economic historian have been focused only on explaining one of these and that is how markets work and there only within a framework of zero transactions costs.

It is asking too much of new economic historians that they should single-handedly develop theory to deal with this range of issues. Help is on the way from economists and the extension of economic theory to deal with these issues is the promise of the future and the hope of economic history. Let me briefly review some of these developments that hold promise for the future. First of all, the new economics of the household, building on earlier work by Leibenstein and Easterlin, promises to provide fundamental extensions of theory about household behavior and in particular about fertility behavior which should unravel a good deal about the structure of decision making within the household and how it varies over time.[3] Second of all, a theory about voluntary organizations must explain why such organizations exist, at what margins they buy from the market rather than internalize decisions within the organization, and what determines the size and shape of such voluntary organizations. Beginning with Coase's fundamental contribution to a theory of the firm many years ago[4] and extending onward to the most recent study by Alchian and Demsetz[5] which provide an extension and modification of Coase's model, we have at least a start towards explaining this fundamental aspect of economic organization and the nature of decision making within organizations. Third, a key to extending our analysis of micro-economic thoery with respect to markets is a systematic study of transactions costs. The study of the specification and enforcement of property rights not only is leading to an overhauling of micro economic theory but, as Coase pointed out, is a key to explaining the structure of voluntary organizations. For the economic historian transactions costs are a fundamental building block toward a useful body of theory. Here, too, important contributions are being made, most recently by my colleague, Steven Cheung.[6] And finally, concerning the most intractable and most important of all the sources of decision making: we must theorize about decision making within the political process since property rights are specified and enforced by the state. Here, too, economists today are engaged in promising research. Clearly, a theory of the state can only be developed when we have also developed a systematic theory about voluntary organizations. The two are inextricably intertwined. But recent research by James Buchanan, Gordon Tullock, Anthony Downs, Albert Breton, and most recently George Stigler, provides the promise of refutable theories about economic decision making within political organizations.[7]

The tenor of my remarks by now should be obvious. Economic historians must continue that fruitful interchange with economic theorists which made the new economic history 15-20 years ago an exciting and new field of research. By using, extending and modifying the tools being developed by theorists, we can usefully analyze the structure and performance of economies, not just in the recent past but in the distant past as well.

3. See for example, *Household Production and Consumption*, N. E. Turleckyj, ed., Vol. 40, Studies in Income and Wealth, National Bureau of Economic Research (New York: Columbia University Press, 1975).

4. "The Nature of the Firm," *Economica*, November 1937.

5. "Production, Information Costs, and Economic Organization," *American Economic Review*, December 1972.

6. "A Theory of Price Control," *Journal of Law and Economics*, April 1974.

7. A summary of the literature is provided in the bibliography to R. A. Posner, "Theories of Economic Regulation," *The Bell Journal*, Autumn 1974.

Our job is something more than simply testing models for economists by applying these theories to the past. Our job is to attempt to put together a larger explanation of the overall structure of economies in the past which can explain their performance and change.

I have said that the failure is not that of economics. That is not quite true. If there has been exciting research in economics, it has been by a comparative handful of the total discipline. The major drift of economics is accurately characterized by McCloskey. It has turned to more and more elegant mathematical models characterized by a wonderful disregard for the institutional constraints. At a time when the margins of economic decision making are being fundamentally altered, most economic research appears to be oblivious to the basic alteration of the parameters that is occurring. An analytical economic history that focuses on structural change is an essential part of a healthy discipline of economics.

The place to start is with a systematic explanation of changes in transactions costs in the past. We can then begin to understand the role played by voluntary organizations, markets, and government and to account for the change in the mix over time. Analysis of the basic institutional structure and the consequent performance of economies in the past makes our task quite distinct from that of the economist who tests specific hypotheses with the use of historical data. Certainly, the function of the economist and the economic historian overlap, but by providing an analytical view of the way the parts have been put together in past societies we can offer the economist a perspective which cannot help but improve his myopic vision. It seems to me that economic history will then fulfill its promise as an integral part of the discipline of economics.