Economists Talking with Economists: An Historian’s Perspective

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The ambitious and long-running project initiated by William Barnett, Editor of *Macroeconomic Dynamics*, has produced a number of conversations in which eminent economists are interviewed by other economists well informed about the interviewee’s work. What we have then is a collection of conversations about both economics and the economists’ lives and about, in a larger sense, how a community of modern social scientists conducts its business.

The conversations are unusual records. Though they provide the reader with a privileged seat at conversations with the eminent, and they enhance our understanding of those eminences, they are not themselves a history of economics, even as the conversationalists appear to be talking over their shoulders to “the historical record”. Yet there is a difference between what historians of economics consider to be historically useful and what their scientist-economist subjects find historically useful. The interviewees seek to construct a particular interested interpretation of the historical record, one in which they are featured\(^1\), and being interviewed by a former student or present colleague, senior or junior, accentuates this problem. I say “problem” because “scientists and historians tend to find different things interesting about the past, to want to use their history for different purposes, and to select their sources and write their accounts accordingly.” (Hughes 1997, 26) This point is well understood by historians of science, and to a lesser degree by scientists themselves. It is not so well understood by most economists.

“There are two principal issues of concern. First, there is the issue of contested interpretation and the difficulty of grounding historical analysis in the face of what might be a well-entrenched actors’ history (and, indeed, in the face of potentially litigious

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\(^1\) This issue is readily apparent in an earlier collection of interviews of macroeconomists, conducted by Arjo Klamer (1984) on the subject of what was called at the time the New Classical Economics, but which now is associated with Keynesian versus real business cycle approaches to macroeconomics.
actors). . . [Second] there are those scientists who wish to retain such control over their history that they will not tolerate anything that departs from the ‘official’ (heroic/celebratory/whiggish?) line.” (ibid., 27).

Both these issues surface in the conversations. As an example of the former, consider the interchange in the Milton Friedman interview about his work during World War II as a member of the Statistical Research Group. Friedman there presents a view of the economists’ ideas about optimization as having shaped the military’s understanding, whereas many historians who write about that period see the cause-effect nexus reversed. And as an amusing (at least to me) example of the second, I note the place in the Paul Samuelson interview where he wonders whether his own understanding of his writings on some biological topics might be re-interpreted by “future Philip Mirowskis and Roy Weintraubs.”

Non-interested conversations though may produce emotionally complex interview situations:

“For some scientists, moreover, history is so valuable a resource that to write history which doesn’t legitimate science in some way is actually seen as positively de-legitimating—in other words, as “undermining” science in some cases—which can generate a profound hostility toward professional historians of science and their writings. (ibid, 28)

We have some of these issues involved in the Robert Aumann interview, where it is noted that a lot of work in game theory was done as part of the cold war enlistment of mathematicians and economists in that war. The hypothesis of the politically disinterested scientist-economist is falsified by such work, and in Aumann’s case additionally by the connection of Israel’s defense-military needs and its large number of game theorists, but these are questions that cannot be raised (especially by Hart, Aumann’s former student) without its being said that such a line of questioning appears designed to “de-legitimize” some serious work in game theory.

As documents that form part of the historical record, the conversations collected in this volume share some features with more traditional oral history. But they do have their limitations:

“In the mere act of historian meeting scientist, and making the scientist aware that his or her opinions and recollections will be preserved and may be exploited by future historians, scientists may be prompted to adopt a public image, even a mask, if you will, that reflects what to they want to have remembered about themselves, their life and their accomplishments.” (DeVorkin 1990, 47)

Put another way, and with respect to the collection of conversations that follow, the fact that the materials were edited with the approval of (and in some cases rewritten by) the various
subjects suggests that the economists themselves were effectively in charge of the interviews, and no material that undermined their own understandings of their work would be developed in the conversation.

Even with that in mind,

“Underneath the intensions of the scientists, memory is faulty to start with, and imperfectly designed questions posed by historians stimulate improper responses, and therefore falsely distorted visions of history. In fact, there is good reason to suppose that the mere act of asking a question influences a reply. It is not unusual to find that an historian, already deep into his or her subject, may have a broader and quite different perspective on a scientist’s life and the scientist being interviewed, especially if that scientist did not work in isolation but within a larger structural or organization, as most do today” (ibid., 48).

What I am suggesting of course is that these conversations are proto-oral histories for the very obvious reason that, with two exceptions, they were not conversations conducted by historians in a standard oral history format. A feature of a conversation in which an eminent economist is interviewed by another well-known economist who has a direct familiarity with a subject area of the interviewee’s work introduces various biases into the record. One difference for example between a historian interviewing a subject, and a colleague interviewing that same subject, is that the subject will likely assume that the historian does not have a detailed understanding of the particular ideas, topics, analyses that the subject believes are his or her own contribution. With a colleague, the interview subject is much more likely to move quickly over technical material, and is much less likely to attempt to justify, let alone explain, an interest in working with that material in the first place2. Thus in reading the conversations it will become more difficult for a non-specialist reader to understand the intricacies of what might appear to be a code-laden discussion between two colleagues than would be the case were that discussion conducted by a historian. Moreover the questions that the historian would wish to address are seldom similar to the questions about which an economist would seek illumination.

It is for this reason that the extensive record of the development of modern physics has been put together not by physicists but by the American Institute of Physics Center for the History of Physics in New York. This long-running program has its transcribed interviews on deposit at the Niels Bohr Library of the AIP in New York City. This project is conducted by professional historians, all of whom are specially trained as oral historians; and because of the cross connections of the interview subjects and the work they did, those historians are fully informed about the nature and scope of the interviewees’ work.

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2 I note that although both Perry Merhling and David Colander might be considered historians of economics, they each consider themselves to be primarily economists.
We have no such organization in economics\(^3\). The work of historians of economics is
carried out by “lone” individuals, and there is no funding source available to sponsor such a large
project. Instead, the historians who do conduct interviews prepare as best they can by studying
reports about what constitute good oral histories, and perhaps consulting one of several manuals
on how to conduct an oral history in the history of science (see for example (DeVorkin 1990)
and (Everett 1992)).

The conversations in this volume were not done in such a unified fashion: the editor did
not require the interviewers to attend “oral history school” nor did he require their accounts
to be homogenized in the same way that the accounts done by the AIP reflect a particular set of
questions that are asked of all subjects, albeit with flexibility to move off those topics as the
interview develops.

This tension between scientists as historians, and historians of science is nicely described
by Stephen Brush (1995) who points out that the conflicts range all the way from the belief
among some historians that scientists are incapable of historical writing because of the necessary
“presentism” and whiggishness, to the view of some scientists that only those who have
participated in the construction of science have the competence to evaluate that which is
important for the historical record. This position was starkly presented by Andre Weil (1978),
the distinguished mathematician, who argued in a plenary lecture at the World Congress of
Mathematics that “The craft of mathematical history can best be practiced by those of us who are
or have been active mathematicians or at least are in close contact with active mathematicians”
(440).

However the instincts and socialization of economists and historians of economics lead
them to ask different kinds of questions about the past. Most economists will see the
development of economics as a sequence of problems thrown up either by the world, called the
economy, or by the development of tools, techniques, and theorizations. That is, most
economists see economics as a problem-solving activity and the history of economics as a
sequence of problems posed, solved, re-described, and further re-posed and resolved. For them,
the economist is a figure who is trained and socialized to recognize these economic problems and
to operate in a world in which framing and solving such problems defines the profession of
economists. Certainly in the interviews that follow we hear the interviewer asking about the
origination of a particular problem, and the mindset and tools that were necessary to solve that
problem which represented the contribution of the interviewee. The interviewers and the
interviewees are in effect acting as economists, collaborating by stabilizing the community’s
understanding of the emergence of the problems, and the development of the tools and expertise
that were needed to solve them. Topics like the interviewees’ education, professional working

\(^3\) A partial exception involves the professional oral history interviews of economists who worked
for various US Presidential administrations. In this case, the historians at the National Archives
often interview or supervise the interviewing of economists and place the tapes and transcripts in
the appropriate Presidential Library. For instance, there is a set of interviews done in 1964 and
recorded by Joseph Pechman (from the Brookings Institution) with Walter Heller, Kermit
Gordon, James Tobin, Gardner Ackley, and Paul Samuelson for the Kennedy Library Oral
History Program (Barber 1975).
environment etc. are all associated with constructing the interviewee as well placed both intellectually, and emotionally, to answer the particular questions that the economy and the economic profession “put on the table”. This is fully consistent with a writing of the history of economics that historians have called OTSOG-ery, an acronym for “On the Shoulders of Giants,” reflecting the apocryphal statement by Isaac Newton that he could see farther, do better science, because he stood on the shoulders etc. This perspective is widely shared among scientists and is reflected in the process and result of the awarding of the Nobel Memorial Prize in economic science where the award citations speak of specific contributions. Thus it is the contributions that are the focus of the discussion and the contributors are in effect “channeling” the contribution to the larger economic community. It should be apparent however that the historian’s interest is different.

However historians would treat the conversations as partial source material of some limited use in constructing a serious history. For historians, context is everything. The historical narrative is not a succession of this, then that, then that, then that. Rather, it is an interweaving of many stories in a tapestry involving the local, and contingent, in a contextualization of all the this-s and that-s. The historian is interested in a larger story, a more multi-layered story than “I came, I saw the problem needed to be solved, I figured out the way to do it.”

Let me now look more directly at the conversations to suggest how the particularities of these individuals and their experiences connect to some larger narratives that historians of economics have been developing over the past couple of decades.

First, it should be recognized that Samuelson, Friedman, Leonteif, and Modigliani are of a different generation from most of the other interviewees. These individuals came of age intellectually from the late 1930s through the 1940s. That period saw the two most important contingencies for the development of economics in the 20th century, the Great Depression and World War II. (James Tobin, just a few years younger, likewise might be associated with this group.)

Historians now are coming to understand that the story of the development of neoclassical economics as a progressive march from the marginalist revolution of the late nineteenth century, to today, is a fiction. It is especially a fiction with respect to economics in the United States. A number of recent studies have demonstrated quite convincingly to historians that what emerged as neoclassical (mainstream) economics in the post-war period was but one of a number of different approaches to doing economics (see Morgan and Rutherford 1998; Weintraub 2002; Mirowski 2002; Yonay 1998). It was not simply that institutionalism, an American kind of economics, was gradually pushed out by neoclassical economics, but rather there were a number of variants of neoclassical economics all competing for economists’

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4 Although I will not develop the point here, I must note that the interviews generally restrict the development of the subject’s autobiographical material to the circumstances of the economist’s contributions qua economist. We thus do not find the usual recollection “bump” for memories of the early adult years (Weintraub 2005).

5 For a fuller discussion of the alternative ways historians of economics might construct such histories, see (Weintraub 1999) and Weintraub 2002, 256-272.)
attention as late as the late 1930s. Moreover, the theoretical contributions of Keynes in his 1936 book were playing out side by side with a more general understanding that the policy recommendations that flowed from Keynes’ general theory had been part of public policy discussions much earlier (Hutchison 1968; Davis 1971; Howson and Winch 1977).

In the conversations presented in this volume one finds less of an emphasis on particular technical details, technical innovations and analysis, and a bit more of a sense of the “rootedness” of the contributions in larger problems. Indeed, in the Leontief interview we find even a series of complaints about the increasingly technical nature of economic theory. For it was over the course of the 20th century that economics became a scientific discipline in a very particular sense. The characteristic that most people think of when they associate economics with science involves the organized presentation of the core of the discipline, generally in a mathematical form. That is, individuals associate a science with various theories and laws that can be expressed mathematically, and that are derived from, or that confront, data that is separately generated although conceptually linked with the theories. Of course much of economics does have this kind of resemblance to work done in other scientific disciplines. But the characteristics of a science, at least a developed science, go far beyond the way its “texts” appear. These days, one doesn’t do an experiment in particle physics in one’s basement lab. One doesn’t attempt controlled fusion experiments out in the garage. Science is characterized by an enormity of scale, of funding, and of human numbers. It’s a long way from a time when one could walk around a 1930s university campus and find the Chemistry Department sharing space with both the Economics Department and the French Department. If one looks around at a modern university, especially one engaged in biological science work perhaps connected to an academic medical center, one sees how the scale has changed. We think of the Manhattan Project and understand the origins of “big science”, but it is not often appreciated just how the scale of “doing economics” has changed as well since World War II. These days when many graduate Ph.D. programs admit from one to two dozen or more students annually, it is hard to look back and see that Ph.D. study before the 1960s was a very unusual activity. There were simply not many graduate students. But in the post-Sputnik era with more students, and more mentors for those students, specialization and the division of labor produced research done by “the labor group” at university X or “the public economics group” at university Y. Ph.D. students are products of these groups much as Ph.D. students in the sciences come from Professor X’s lab or that of Professor Y. Generally gone are the days when an economics professor might supervise dissertations from many different areas over the course of a decade. That doesn’t happen anymore, just as a theoretical physicist these days does not supervise an experimental dissertation.

Big science emerged during World War II with the immense activity of building the atomic bomb, and the direct engagement of scientists in the war effort. Aircraft design and production, radar, sonar, guidance systems, computation systems, all emerged in that war time period through the collaboration of scientists, engineers, military planners and strategists, and social scientists, particularly economists. The kinds of tools and symmetries in analysis that Samuelson had explored in his pre-war doctoral dissertation were fully in play during the war as optimization analysis became central to the work of the research groups involving economists linked by the Applied Mathematics Panel to the RAD Lab at MIT, the Statistical Research Group at Columbia, and the soon to emerge RAND in Santa Monica. It is not just that economics
became more scientific through these interconnections, but rather that science became more like what we now think of as science. The public relations call to continue public support of science at such a high (wartime) level was made by Vannevar Bush (1945) in his “Science, the endless frontier” but of course economics was on that frontier. That economics eventually was to partake of the largesse of the National Science Foundation was one result, as was the support of economists through the Army, the Air Force and the Office of Naval Research.

Nevertheless, the technical details of economic analysis are not totally absent from the conversations.Listening in on the younger economists like Fischer and Cass and Lucas we hear scientific-technical conversation, in which matters at issue are problems, and problems are meant to be solved. To some degree of course this is a particularly American perspective. The career-problems faced by Jacques Drèze and Janos Kornai are systematically quite different from those faced by economists working in the United States. Nevertheless the perspective of this volume confirms that mainstream economics is pretty much an American invention, and has been sustained in its intellectual vigor by the American higher education system, specifically the rise of a large number of research universities in the post-war period. Though Volcker had long spells in government service, and in recent years Fischer has worked in the private sector, scientific economics is a university discipline, and is not simply something that, because of its public policy importance, is merely taught within universities. This of course reflects a change from earlier times. For what these conversations record are the careers of individuals who have made contributions to economic research and that research is the coin of the realm in particular academic communities. Teaching, mentoring graduate students, and developing new economic analyses for emerging economic problems are by and large activities that are carried out in universities, not in think tanks, and not in government agencies.

Yet another feature of these conversations that would interest historians is that while research in economics is carried on in universities, much of this research engages a larger public through the efforts of these very same researchers. It is as if the nuclear physicists took their concerns, at the same time they were scientifically active, to larger public discussions. Here particularly one needs to take note of the work done by Martin Feldstein at the National Bureau of Economic Research, and Paul Volcker in his many roles both in and outside of government. Kornai as well has important stories to tell about the connection between economics and politics, stories that are increasingly recognizable as it is understood by historians that the history of economics is not simply a recounting of how great ideas came to be understood and developed and promulgated, but how ideas moved across the boundaries of tightly organized professional communities into the larger community interested in economics. This is a story of the increasing importance of economists in public life, a process that was heavily influence by Roosevelt’s years and moved quickly in the 1940s with the creation of the Council of Economic Advisors following on the Employment Act of 1946. Historians have begun to see that the history of economics is not just the history told by the research scientists themselves, but it’s a history of the import and impact of ideas (see Bernstein 2001).

In this passage of ideas, what is termed the transmission of economic knowledge, it is not only government and the military who are the receivers. There are as well large numbers of

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6 I note, from the Samuelson interview, his particular connection to the Bush report.
foundations which have helped to support economics and economic research for particular purposes of their own, over a long period of time. The story of the Rockefeller Foundation’s support of business cycle research internationally in the inter-war period is well known, and of course much of the modern work on business cycles, and indeed econometric models, dates from those years. The Volker Fund (not associated with Paul Volcker) in the 1940s supported the reconstruction of the University of Chicago Economics Department and helped Capitalism and Freedom’s author publish that volume; moreover it provided the funding/impetus for Hayek’s position at Chicago. All of which is a way of noting that economists’s ideas ramify: as Keynes famously remarked, “indeed, the world is ruled by little else” (Keynes 1936, 383). And thus any enhanced understanding of the genesis of economists’s ideas, as may be gleaned from the set of interviews collected here, should serve to make our world more comprehensible.

Works Cited


