# Inside the Economist's Mind: The History of Modern Economic Thought, as Explained by Those Who Produced It

**William A. Barnett and Paul A. Samuelson (eds.)**

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# *Coeditor's Foreword:*

## *Reflections on How Biographies of Individual Scholars Can Relate to a Science's Biography*

**by Paul A. Samuelson, Coeditor**

This book adds up to more than the sum of its parts. When W. Somerset Maugham opined that "to know one country you must know two countries," he was saying in a different way that 1+1 can exceed 2. Adam Smith and Allyn Young categorized this as "increasing returns to scale."

When a discipline---economics, chemistry, or acupuncture---is in a dynamic stage of rapid growth, its up-front cyclists care little whether it was Newton or Leibniz who "invented" the calculus. The economics profession is in such a dynamic stage of rapid growth, as made clear by the interviews in this book. The book permits us to step back and view the whole of the field in a revealing context that otherwise is easily missed in the narrow focus of individual expert researchers. Twenty-first century's go-getters in economics go whole hours ignoring what more John Bates Clark did for marginal productivity theorizing, than Johann Ludwig von Thünen had not already done.

This helps explain the historical fact that the role in the graduate curriculum once played by "History of Economic Thought" has eroded down to a narrow cadre of learned experts. An unearned snobbery ensues, as is well illustrated by Bernard Shaw's canard: "Those who can, do. Those who can't, teach." Good history of science deserves a non-zero weight in the university curriculum. The dynamic growth in individual subfields of the economics profession needs to be supplemented by overviews of the whole, not just as the sum of its normally separated parts. This book provides such a view of the whole of the modern field of economics and the connection of that whole with the life experiences of famous economists whose work was seminal to the field.

Returning to the theme of how multiplicity of cases can be fruitful, let's test an alleged dictum of Socrates: "The unexamined life is not worth living." When I once read an excellent book about the principal philosophers, all the usual suspects were there: Spinoza, Kant, Hegel, Wittgenstein, Russell, .... My inductive finding was that Socrates had it completely wrong. An unhappier gaggle of misfits could hardly be imagined. Suicides abounded, melancholies persisted, celibacies and divorces competed for frequencies. A vulgar explanation would nominate as a common cause that the study of philosophy destroys the joy of life. Perhaps a better explanation would be that becoming an orphan early, or being born dyslexic, etc., predisposes one to choose philosophy over being a cheerful bartender. Acquiring an objective and insightful overview of the whole in any area of understanding is important, but less easily and enjoyably acquired than the skills of a bartender.

I return to economics and to economists and to the question of why the profession's directions have evolved in the manners evident from this book. A major conservative economist once explained that a source of his antipathy to government traced back to the defeat of his southern ancestors by a larger north economy. Here is a similar factoid. Joan Robinson once wrote that her opposition to having the UK enter the European Market was due to the fact that she "had more friends in [Nehru's] India than on the continent." Yes, it is a banality that personal piffle can affect ideology. But can we take autobiographical judgments as most accurate judgments? The Robinson I knew could well have thought back in the 1960s that her kind of post-Fabian socialism would flourish better in India than on the continent. And, alas, she may have been right in so thinking.

Published scientific research, by its very nature, is designed not to identify any personal motives of the authors. In understanding what is in this revealing book, need we be concerned with the personal motives for the directions taken by these eminent economists? If so, is this interviews-format the best way to gain insight into those motives?

I conclude with an unworthy hypothesis regarding past and present directions of economic research. Sherlock Holmes said, "*Cherchez la femme*." When asked why he robbed banks, Willie Sutton replied, "That's where the money is." We economists do primarily work for our peers' esteem, which figures in our own self-esteem. When post-depression Roosevelt's New Deal provided exciting job opportunities, first the junior academic faculties moved leftward. To get back ahead of their followers, subsequently the senior academic faculties shoved ahead of them. As post-Reagan, post-Thatcher electorate turned rightward, follow the money pointed, alas, in only one direction. So to speak, we eat our own cooking.

We economists love to quote Keynes's final lines in his 1936 *General Theory* --- for the reason that they cater so well to our vanity and self-importance. But to admit the truth, madmen in authority can *self-generate* their own frenzies without needing help from either defunct or *avant garde* economists. What establishment economists brew up is as often what the Prince and the Public are already wanting to imbibe. We guys don't stay in the best club by proffering the views of some past *academic* crank or academic sage.

Indeed, this book adds up to more than the sum of its parts. It provides a rare overview of the economics profession in a manner that reveals the relevancy of the personal motives and experiences of some of its leading modern contributors.

# *Coeditor's Preface:*

## *An Overview of the Objectives and Contents of the Volume*

**By William A. Barnett**[1]

Editor of the Cambridge University Press journal, *Macroeconomic Dynamics*,
Editor of the Elsevier monograph series, *International Symposia in Economic Theory and Econometrics*,
and Coeditor of this book

This collection of interviews contains unique insights into the thinking of some of the world's most important economists, whose work contributed to the evolution of modern economic thought. What makes this collection so unusual is the source of these interviews. They first were published in a highly regarded, peer-reviewed, Cambridge University Press journal, ***Macroeconomic Dynamics***, of which I am Editor. Publication in scientific peer-reviewed journals normally is subject to refereeing, which constrains authors to publish only what is deemed to be acceptable to the referees, associate editors, and editors of those journals. These constraints do not permit casual, free-wheeling discussion of the sort more commonly found in the popular press. But it is publication in those professional journals that is most highly regarded by scientists, since only publication in those journals has the stamp of approval of the profession, as being consistent with the rigorous standards of science. Hence it is through publication in such journals that scientists speak to each other in a manner that commands the respect of their peers.

To the layman, it may seem odd that even the world's most famous Nobel Prize winners are not permitted to speak to their profession within scientific journals in a manner that is free from the constraints of peer review. With recognition of this communication problem, I instituted an interview series within the journal, ***Macroeconomic Dynamics***.[2] That journal never publishes more than one interview in any issue, since the journal is otherwise a rigorously refereed scientific journal. But it has been made clear to the journal's publisher, Cambridge University Press, that an interview is entirely a quotation and cannot be touched by referees, associate editors, copy editors, the publisher, or me. It is a matter of freedom of speech and freedom of the press that quotations cannot be altered.

From the startup of the journal, interviewers and interviewees have been informed that they can say whatever they want in these interviews, despite the fact that publication is within an otherwise peer-reviewed scientific journal. As a result, the leaders of the

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[1] I wish to thank Bill Cooper, at the University of Texas at Austin, from whom I first got the idea for this book.

[2] Interviews of statisticians can be found in the journal, ***Statistical Science***, and interviews of econometricians can be found in the journal, ***Econometric Theory***. But those interviews tend to focus on the more technical objectives of those two journals, rather than on the general evolution of economic thought.

field can openly reveal any matters that they may wish to share with the profession, whether personal, religious, or political.  Personal attacks; claims of unfairness or prejudice, of religious persecution, or of political oppression; and unvarnished strong statements about politicians, administrators, and public policy, while normally excluded from professional journals, are not excluded from these interviews.  Participants in an interview are free to put such matters “on the record.”  The nature of the fireworks contained in some of these interviews cannot be found in other professional economics journals.  Nothing is removed from those interviews by the journal’s editorial board or by Cambridge University Press, although in one interview, Cambridge University Press did replace an Anglo-Saxon expletive with the abbreviation “f---”.

The participants in these interviews include eight Nobel Laureates --- Wassily Leontief, Robert Lucas, Franco Modigliani, Robert Solow, Milton Friedman, Paul Samuelson, Robert Aumann, and James Tobin; two central bank governors --- Paul Volcker (former Chairman of the Federal Reserve Board) and Stanley Fischer (Governor of the Bank of Israel); and a Chairman of the Council of Economic Advisors --- Martin Feldstein.  Robert Aumann won his Nobel Prize as this book was in preparation.  Some of the other participants in these interviews are high on most economists’ lists for possible future Nobel Prizes in Economics.  Despite the fame of the interviewers and interviewees, you will not find comparably candid insights into their lives and views anywhere else but in this book or in the original interviews in ***Macroeconomic Dynamics***.

The following equally important interviews, which have appeared in ***Macroeconomic Dynamics***, are planned to be included in the anticipated volume 2 of this book, along with other important interviews that now are in process.  Each of the two books will be balanced in content to be comparably as informative and to reflect a broad spectrum of views of many of the world’s most influential economists.

Allan Meltzer interviewed by Bennett McCallum
(*Macroeconomic Dynamics*,  vol 2, no 2, 1998)
Elhanan Helpman interviewed by Daniel Trefler
(*Macroeconomic Dynamics*,  vol 3, no 4, 1999)
William Brock interviewed by Michael Woodford
(*Macroeconomic Dynamics*,  vol 4, no 1, 2000)
Karl Shell interviewed by Steven Spear and Randall Wright
(*Macroeconomic Dynamics*,  vol 5, no 5, 2001)
Axel Leijonhufvud interviewed by Brian Snowdon
(*Macroeconomic Dynamics*,  vol 8, no 1, 2004)
Anna Schwartz interviewed by Edward Nelson
(*Macroeconomic Dynamics*,  vol 8, no 3, 2004)
Guillermo Calvo interviewed by Enrique Mendoza
(*Macroeconomic Dynamics*,  vol 9, no 1, 2005)
Assar Lindbeck interviewed by Thorvaldur Gylfason
(*Macroeconomic Dynamics*,  vol 10, no 1, 2006).

In keeping with the high standards of the profession, we invited an introduction by one of the world's leading authorities on the history of economic thought, E. Roy Weintraub. Weintraub's Introduction follows this abstract. In addition, Paul Samuelson, who is a coeditor of this book, contributed the book's thought-provoking Foreword, which precedes this Preface.[3] To emphasize the colorful nature of much that appears in these interviews and the unusual insights available herein, a few of the more striking statements are briefly quoted below. These quotations are taken out of context and are no substitute for the full interviews, but are an indication of the unusual nature of this collection of important and fascinating interviews. The cover design of this book is a painting produced by a famous Swedish economist and artist, Assar Lindbeck, who generously permitted us to reproduce his painting, "***Overlappende generationer***," for this book's cover.

All of the interviews published in this book are reprinted in their entirety from the ***Macroeconomic Dynamics*** originals, although some of the photographs have been removed. The following are a sample of some of the quotations and observations that can be found in this book.

**1. *Wassily Leontief interviewed by Duncan Foley***: Wassily Leontief, best known as the originator of the fundamental planning tool, input-output analysis, won the Nobel Prize in Economics in 1973, while a professor at Harvard University. He was born in the Soviet Union. The following quotations are indicative of the insights about his life and views that can be found in his interview.

*"Marx was not a very good mathematician. He was always mixed up in math, and the labor theory of value didn't make much sense." "I left the Soviet Union in 1925. I got in trouble with the government, actually." "Richard Goodwin was my student....He couldn't get tenure. And this was the reason why he went to England...I think possible, it was politics. He was on the left."*

Regarding his views about the distant future, Leontief explains: *"I think problems of income distribution will increase in importance. As I mentioned before, labor will be not so important, and the problem will be just to manage the system. People will get their income allocated through social security---already now we get it through social security, and we try to invent pretexts to provide social security for people. Here, I think, the role of the government will be incredibly important, and those economists who try to minimize the role of the government, I fear, show a superficial understanding of how the*

[3] In a letter to me, Paul Samuelson wrote that, "I never mind it when my prose targets the most erudite of those who read it. Robert Browning said, 'Ah, but a man's reach should exceed his grasp, or what's a heaven for?'" In that context, Paul explained that his Foreword, "on purpose ... did not include the exact famous final words in *The General Theory*." Nevertheless, for the benefit of those who do not meet Paul's high standards of erudition, I here provide Keynes (1936, pp. 383-384) statement, to which Paul alludes in his Foreword: "Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back. I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas ... Sooner of later, it is ideas, not vested interests, which are dangerous for good or evil."

*economic system works. My feeling is, if we abolished the government now, already there would be complete chaos...it would be horrible."*

Wassily Leontief died in 1999, a year after the publication of his interview in ***Macroeconomic Dynamics***.

**2.** ***David Cass interviewed jointly by Steve Spear and Randall Wright***: David Cass has produced some of the deepest theoretical insights in the field of economics, including the discovery of "sunspot equilibria" in his joint research with Karl Shell. Cass, along with Hirofumi Uzawa and Karl Shell, has influenced economic dynamics in ways that have been pivotal in the history of economic thought. In keeping with that depth of intellect, this interview is uncompromising in its emphasis on technical advances in economics. Although his time on the faculty of Carnegie Mellon University overlapped with mine, as a graduate student there, one of my disappointments was that he did not stay. He moved to the University of Pennsylvania, for reasons made clear in this interview.

There is another more colorful side to Cass. That side is well known in the profession and clearly displayed in this interview by such statements as the following: *"We had to hire a new dean. At Carnegie, the faculty was very involved in this process....we settled on Arnie Weber...That turned out to be, from Carnegie's viewpoint and my own viewpoint, a disaster ... Arnie called me into his office for some reason, and I had an interview with him. He told me that I was a luxury good and that I didn't do business. I did theoretical economics and it wasn't something that business schools could really support, and he did it in a very obnoxious way that really pissed me off. And I said 'f--- you, Arnie.'... Yeah, I said 'f--- you.'"*

Cass said the following about Nobel Laureate Robert Lucas, who was on the faculty at Carnegie Mellon University at that time, *"Bob was in the Chicago tradition and was very concerned about empirical testing --- whatever the hell that means --- something that I have little sympathy for and very little interest in, to be perfectly honest."* Although himself a pioneer in real business cycle theory via the Cass-Koopmans model, Cass said, *"the thing about real business-cycle theory, I suppose, is that it is almost like a religion."*

**3.** ***Robert E. Lucas interviewed by Bennett T. McCallum***: Robert Lucas won the Nobel Prize in Economics in 1995, while a professor at the University of Chicago. In his introduction to this interview, Bennett T. McCallum wrote that *"Bob Lucas is widely regarded as the most influential economist of the past 25-30 years, at least among those working in macro and monetary economics."*

In this interview, you will learn how Lucas was motivated at age of 7 or 8 to be interested in economics by his father's stories about the economics of milk truck deliveries under socialism. About his later years as a graduate student at the University of Chicago, Lucas states that, *"The atmosphere at Chicago, when I was a student, was so hostile to any kind of planning that we were not taught to think: How* ***should*** *resources* *be allocated in this situation? How* ***should*** *people use the information available to them to form expectations? But these* ***should*** *always be an economist's first questions. My Dad was wrong to think that socialism would deliver milk efficiently, but he was right to think about how milk* ***should*** *be delivered."*

Among his other statements are, *"I am happy about the successes of general equilibrium theory in macro and sad about the de-emphasis on money that those successes have brought about."* Regarding the importance of technology shocks, he comments: *"If we are discussing the U. S. depression in the 1930s or the depression in Indonesia today or Mexico five years ago, I would say that technology shocks are a minor part of the picture. On the other hand, ... in postwar United States the relative importance ... is much larger."* In response to the question, *"is price stickiness an important economic phenomenon?"* Lucas replied: *"Yes. In practice it is much more painful to put a modern economy through a deflation than the monetary theory we have would lead us to expect."*

Lucas says the following about monetary policy, *"I am concerned about the kind of bad dynamics that Wicksell, and more recently Peter Howitt, worried about."* He further observes, *"My claim is not that monetary instability is incapable of causing great harm, but only that it has not done so over the past 50 years in the United States."* Lucas states the following about modern microeconomics, *"In the past 15 years, microeconomics has come to be synonymous with game theory in many places (not including Chicago!), and that is unfortunate."*

**4.** ***Janos Kornai interviewed by Olivier Blanchard***: To many in the economics profession, Janos Kornai is a true hero. While living in his home-country of Hungary under communism, he became famous among economists in the West, against the odds and at considerable danger to himself. As explained by Olivier Blanchard in his introduction to this interview, *"These difficulties have not prevented him from giving us the most informed and deepest critique of the socialist system to date."* At present, Kornai shares his time between Harvard University and Collegium Budapest.

Among the statements in this interview are the following: *"One of my closest friends was not only arrested, but tried and executed. Many of my best friends were arrested...I was attacked as a 'traitor' to socialism. I was fired." "I still admire Marx as an intellectual genius; he had many ideas which are still useful. He was, however, absolutely wrong on many fundamental issues." "Before 1963, I had been denied a passport. I had a standing invitation to the London School of Economics for years, for instance, and I couldn't go."*

Regarding his early book, ***Overcentralization***, and the events that led up to it, Kornai observes: *"My disappointment began in 1953 .... , when many facts that had previously been hidden, became known ... the horrible crimes the system had committed, the imprisonment, torture, and murder of innocent people, made my most sincere beliefs seem naive and shameful. Also, I began to recognize that the regime was economically dysfunctional and inefficient, created shortages, and suppressed initiative and* *spontaneity."* He continues, *"**Overcentralization** ... got worldwide attention because it was the first critical book written by a citizen living inside the Bloc."* He further observes that in the preface of the second edition of that book, he *"described the Kornai of 1954-1956 as a 'naive reformer.'"*

About his book, ***Anti-Equilibrium***, he stated: *"I feel slightly bitter about its getting hardly any attention. First, and nearly the last people who gave it any credit, were Arrow and Koopmans; then it somehow disappeared...it seems to me that asking relevant questions doesn't give you much reputation, at least not in our profession."*

With respect to his book, ***Economics of Shortage***, he states: *"The dysfunctional properties of socialism are systemic ... I was rather isolated from the rest of the so-called reformers who were working on small changes to the Communist system. In that sense, it's a revolutionary book...You have to change the system as a whole to get rid of the dysfunctional properties."*

Changing the subject to his book, ***The Socialist System***, he states that, "*The central idea of the book was to show that the classical Stalinist system, however repressive and brutal it was, was coherent, while the more relaxed, half-reformed Gorbachev-type of system was incoherent, and subject to erosion. I foresaw the erosion.*"

Kornai comments on the current post-communist Eastern Europe: *"I think people belonging to the elite of the former socialist regime have, with few exceptions, totally forgotten the Communist Manifesto, but they have a network of friends from the old days. Right now these relations are extremely powerful in business, in politics, in cultural life. People who knew each other in the old system, know exactly who is a friend and who is an enemy."*

**5. Franco Modigliani interviewed by William Barnett and Robert Solow**: Franco Modigliani won the Nobel Prize in Economics in 1985, while a professor at MIT. This interview was conducted jointly by the 1987 MIT Nobel Laureate, Robert Solow, and me. Since my initial interest in economics was motivated by Modigliani's graduate course, which I took while an undergraduate student in engineering at MIT, I felt a particular responsibility to assure that Modigliani's remarkable life and contributions would be adequately covered in this interview. Many of the questions that I asked were based upon longstanding rumors heard by Modigliani's students. In this interview, you will learn the truth about those rumors.

Modigliani and his parents left Italy, while under Mussolini's fascist rule. As he explains in this interview, *"After the Ethiopian war and the fascist intervention in the Spanish Civil War, I began to develop a strong antifascist sentiment and the intent to leave Italy, but the final step was the close alliance of Mussolini with Hitler, which resulted in anti-Semitic laws, which made it impossible to live in Italy in a dignified way."* As explained in some detail in the interview, he and his family first moved to France and then to the U.S. He briefly returned from Paris to Rome, still under fascist rule, to defend his dissertation. As he explained, *"that operation was not without* *dangers, because by that time I could have been arrested. I had kept my contacts with antifascist groups in Paris, so there was the possibility of being harassed or being jailed."* He describes a code that he used with his father-in-law as a warning, while he was in Rome. It has been widely rumored, that Franco Modigliani was related to the famous painter and sculptor, Amedeo Modigliani. But that story seems not to have been true.

Modigliani's first position in the United States was at the New School University in New York City. He received an offer from Harvard, which he surprisingly turned down. His explanation is the following: *"Because the head of the department, Professor Burbank, whom I later found out had a reputation of being xenophobic and anti-Semitic, worked very hard and successfully to persuade me to turn down the offer."*

Having turned down the Harvard offer, Modigliani moved to the University of Illinois, where the salary was higher than at Harvard. Regarding his years at Illinois, he observes the following: *"The president of the university brought in a new wonderful dean, Howard H. Bowen. But the old and incompetent faculty could not stand the fact that Bowen brought in some first-rate people... The old faculty was able to force Bowen out, as part of the witch hunt that was going on under the leadership of the infamous Senator Joseph McCarthy. The leader of the McCarthyite wing of the elected trustees was the famous [football player] Red Grange. I then quit in disgust with a blast that in the local press is still remembered: 'There is finally peace in the College of Commerce, but it is the peace of death.' My departure was greeted with joy by the old staff, proportional to their incompetence. But 40 years later, the university saw fit to give me an honorary degree!"*

The interview was conducted shortly before the stock market bubble burst in 2000, and contained the following statement by Modigliani, *"I believe that indeed the stock market in the United States is in the grips of a serious bubble. I think the overvaluation of stocks is probably on the order of 25%....In my view, there will be a collapse, because if there is a marked overvaluation, as I hold, it cannot disappear slowly."* In this interview, he is on the record with that forecast, and indeed he was right. No wonder one of Modigliani's students, Robert Shiller, of *Irrational Exuberance* fame, has said of Modigliani, he is *"my hero."*

Modigliani says the following about Robert Barro, who also was in some of Modigliani's classes: *"In my view, Barro's theorem, despite its elegance, has no substance. I don't understand why so many seem to be persuaded by a proposition whose proof rests on the incredible assumption that everybody cares about his heirs as if they were himself."* Modigliani is referring to Barro's view on Ricardian-equivalence and its implication of the irrelevance of government debt financing.

About monetary policy and Friedman's rule, Modigliani says: *"in the battle between my recommendation to make use of discretion (or common sense) and Friedman's recommendation to renounce discretion in favor of blind rules ... , my* *prescription has won hands down. There is not a country in the world today that uses a mechanical rule."*

Franco Modigliani died in 2003.

**6. *Milton Friedman interviewed by John Taylor*:** Milton Friedman won the Nobel Prize in Economics in 1976, while a professor at the University of Chicago. As explained by John Taylor, in his introduction to the interview, *"His views have had as much, if not more, impact on the way we think about monetary policy and many other important economic issues as those of any person in the last half of the twentieth century."*

Regarding the "Great Inflation" in the 1970's, Friedman states: *"I believe that Arthur Burns deserves a lot of blame...From the moment Burns got into the Fed, I think politics played a great role in what happened. So far as Nixon was concerned, there is no doubt, as I know from personal experience. I had a session with Nixon sometime in 1970, I think it was 1970, might have been 1971, in which he wanted me to urge Arthur to increase the money supply more rapidly (laughter) and I said to the president, 'Do you really want to do that? The only effect of that will be to leave you with a larger inflation if you do get reelected.' And he said, 'Well, we'll worry about that after we get reelected.' Typical. So there's no doubt what Nixon's pleasure was."*

In this regard, I can mention that I was myself on the staff of the Federal Reserve Board from July 1973 to September 1990, which overlapped part of Burns' term as Chairman. I also met with him at the American Enterprise Institute, at his request, following the end of his term at the Board. He stated that he indeed did deserve a lot of the blame, but he denied that the reason was political pressure. He maintained that it was an honest mistake by him, based upon failure to recognize that the "natural rate" of unemployment had increased. He said that that failure resulted in a misguided attempt to lower unemployment to unsustainably low levels. But, of course, if political pressure from the White House really had played a role, it is unlikely that Burns would have admitted it to me.

Other interesting statements in Friedman's interview include: *"Nixon had a higher IQ than Reagan, but he was far less principled; he was political to an extreme degree."* Friedman reveals the following about Burns as a Ph.D. student, *"Burns .... was living in Greenwich Village. He had long hair, long fingernails. You know, he was a different character than he was later on."* Friedman says of himself as an undergraduate, *"I probably would have described myself as a socialist, who knows."* In reply to a question about the use of mathematics in economics, Friedman states, *"I go back to what Alfred Marshall said about economics: Translate your results into English and then burn the mathematics."*

On the subject of the euro currency, Friedman says, *"I think it will be a miracle* --- *well, a miracle is a little strong. I think it's highly unlikely that it's going to be a great success."* In addition, at the time of the interview, Friedman said in this interview, *"The*

*Euro is undervalued; the U.S. dollar is overvalued...Relative to the dollar, the Euro will appreciate and the dollar will depreciate."* And indeed it has, in spades.

**7.** ***Paul A. Samuelson interviewed by William A. Barnett***: Paul Samuelson won the Nobel Prize in Economics in 1970, while a professor at MIT. I was an undergraduate engineering student at MIT from 1959-1963. To all students at MIT in all fields, there were two "gods" who loomed over the rest of the faculty: the great mathematician, Norbert Wiener, and the great economist, Paul Samuelson. At MIT, where all the tenured professors are world renowned research stars, to loom over the rest is possible only in the rarest of cases from any generation of scholars.

To this day I think that many economists feel intimidated by Samuelson's awesome intellect. In fact, I was surprised by the difficulty that I had in finding an economist who was willing to take on the job of serving as interviewer of Samuelson. I did finally find one (V. V. Chari at the University of Minnesota). But he brought the tapes of the interview back with him on an airplane, after running them through the X-ray luggage scanner at an airport. The tapes were destroyed by the scan. So in this one case, rather than trying to find another willing interviewer, I conducted the interview entirely myself. Indeed, it was an experience.

During his career, Paul Samuelson has averaged almost one technical paper per month. He once said, *"Let those who will --- write the nation's laws --- if I can write its textbooks."* It is widely reported that at the end of Samuelson's dissertation defense at Harvard, the great economist Joseph Schumpeter turned to the Nobel Laureate, Wassily Leontief, and asked, *"Well, Wassily, have we passed?"*

Regarding Leijonhufvud's interpretation of Keynes, Samuelson said in this interview that, *"I know him to have it wrong."* In this interview, you can find Samuelson's views on the *"rash Reagan fiscal deficit."* Changing the topic to his first economics teacher, Aaron Director, Samuelson says, *"He was the only man alive who could .... speak of 'my radical brother-in-law Milton Friedman'."* Discussing Samuelson's years as a student during the Depression, with Frank Knight as one of his professors, Samuelson says, *"the only present choice was between communism and fascism. And for himself, Knight would not choose the latter. Later, understandably, he recovered from that failure of nerve and reneged on his circulated text. Somewhere in my files will be found a copy of his doomsday text."*

With respect to his years as a student at the University of Chicago, he characterized that economics department as *"dogmatically conservative."* He then moved to Harvard University as a graduate student, and comments on those years as follows: *"Anti-Semitism was omni-present in pre-World War II academic life, here and abroad."* Samuelson comments on the faculty at Harvard as follows: *"Hitler (and Lenin) did much for American science. Leontief, Schumpeter, and Haberler brought Harvard to life after a lean period."* He continues that upon completion of his studies, *"When MIT made a good offer, we thought this could test whether there was great enthusiasm for my staying* *at Harvard. When Harvard's revealed preference consisted of no majority insistence that I stay, we moved three miles down the Charles River."*

Characterizing the nature of his influence on Washington during Camelot, Samuelson comments that, *"With great reluctance, I let Senator John F. Kennedy recruit me to his think tank...Only when they needed my extra heavy lifting from Cambridge did I weigh in."*

On the subject of globalization, Samuelson comments that, *"Trade is confirmed to be a substitute for massive immigration from poor to rich countries. U.S. labor has lost its old monopoly on American advanced know-how and capital ... Free trade need not help everybody everywhere ... Nowadays every short-term victory by a union only speeds up the day that its industry moves abroad ... A 'cowed' labor force runs scared under the newly evolved form of a ruthless corporate governance."*

As should be no surprise from these comments, Samuelson goes on to observe that, *"probably as a syndicated columnist, I have published at monthly intervals a couple of thousand different journalistic articles."*

**8. Paul Volcker interviewed by Perry Mehrling**: Paul Volcker was President of the Federal Reserve Bank of New York from 1975-1979 and Chairman of the Board of Governors of the Federal Reserve System from 1979-1987 under both Presidents Carter and Reagan.

With respect to Arthur Burns' views about suspending convertibility of the dollar into gold, Volcker said, *"Burns didn't want to do any of this. He was holding out to the end. I didn't think he had any realistic ideas as to how to reform the System, except he seemed to think we could negotiate a change in the price of gold without suspending convertibility."* About his own experience during that period of needed reform, he said, *"It's a sad story, engraved on my mind ... I was the American negotiator for reforming the system. I don't know how close we really came to an agreement. It was very difficult. But about the time when maybe an agreement was in sight, the oil price shock was used as an excuse to end the effort."*

I was myself on the staff of the Federal Reserve Board during much of the "monetarist experiment" years of 1979-1982, and it was very clear to me that Volcker was sincere in his wish to try a monetarist policy to tame the double digit inflation that existed in the late 1970s. But when that new policy produced a recession, it became fashionable among the monetarists of the time to say that the Board really was not following a monetarist policy, and was just using that claim as a cover-up for continuation of the old policy. I never agreed with that interpretation of what I saw, and indeed Volcker in this interview makes clear what really happened, in the following statement:

*"I used to rankle when some of the members of the Board who were all enthusiastic about this turn of policy would say, 'Isn't this just a kind of public relations ploy to avoid*

*being blamed for the rise in interest rates?' I never thought it was that, but a lot of people did think it was largely that. It was a very common thing to say that we just did it to obfuscate."* About the objective of controlling the money growth rate during that three year period, Volcker explained further, *"we had no other good benchmark for how much to raise interest rates in the midst of a volatile inflationary situation."*

On the subject of credit controls, Volcker commented: *"There was a law that had been passed in the early 1970's to embarrass President Nixon, authorizing the president to call for credit controls. It was a two-stage thing. He could call for controls, but the Federal Reserve would have to implement them. So Carter took the view that he wanted credit controls. I didn't like the idea... But President Carter wanted to do something ... I said to the Board, 'Let us do as little as we possibly can, consistent with the request or demand that we have some credit controls ... I shouldn't have done anything, logically ...Consumption just collapsed ... We took the controls off as soon as we could."*

On the controversies regarding floating exchange rates versus a possible future single international currency, Volcker comments that, *"for many countries, particularly small and open countries, a floating currency is more trouble than the independent monetary policy is worth ... We will need to think in terms of some truly international standard, the role that gold used to play."* Volcker says the following about deregulation, *"I think that financial deregulation has been another big strand of what I've been concerned about ...When I was in the Treasury in the sixties, Wright Patman, an extreme populist from Texas and chairman of the House Banking Committee, made a speech complaining that we had too few bank failures and too little risk taking. Well, we have fixed that problem!"*

Volcker reveals his views on modern risk management in the statement, *"The whole concept rests on the idea of normal distribution curves, but there ain't no normal distribution when it comes to financial crises."* On the subject of the Russian central bank, Volcker says it *"is pretty well destroyed by accusations, rightly or wrongly, that they are corrupt in the most egregious sense."*

It is perhaps interesting to observe that this interview was acquired following a somewhat unusual exchange. I wrote to Paul Volcker on August 10, 1999, inviting him to be interviewed for publication in ***Macroeconomic Dynamics***. He replied in a letter on January 5, 2000 agreeing, but with the following qualification: *"I apologize for a long delayed response. Perhaps it was my allergy to 'Divisia monetary aggregates' that accounts for the lapse."*[4]

The reason for his hesitancy is not difficult to understand. I originated the Divisia monetary aggregates at the end of the 1970s, while on the staff of the Federal Reserve Board (in the Special Studies Section). During the "monetarist experiment" of 1979 - 2002, my aggregates were growing at half the rate of the official simple sum monetary

[4] An interesting contrast is Lucas's (2000, p. 279) more recent statement, *"I share the widely held opinion that M1 is too narrow an aggregate for this period [the 1990s], and I think that the Divisia approach offers much the best prospects for resolving this difficulty."*

aggregates. I advised repeatedly that the official aggregates were not accurately reflecting the restrictiveness of policy, and that policy would result in a recession. Perhaps the recession that followed, as I had warned, is the source of Volcker's "allergy." I subsequently published that data and documentation in a paper in the American Statistical Association's journal, the ***American Statistician***. When I submitted the paper to that journal, its editor, Gary G. Koch, had the paper refereed by an astonishing six referees. In addition on the telephone, he informed me he was worried that publishing my results would cause his journal to be overwhelmed by angry letters to the editor. I assured him that the kinds of people who would send such letters do not likely read his journal. The article appeared in Barnett (1984).

But there is more. After the economy had recovered from the recession (and I had left the Board for a professorship at the University of Texas), there was a huge spike in the simple sum monetary aggregates, but no spike in my Divisia monetary aggregates. On September 26, 1983, the world's leading "monetarist," Milton Friedman, in a full page article in *Newsweek* magazine (p. 84), wrote, *"The monetary explosion from July 1982 to July 1983 leaves no satisfactory way out ... The result is bound to be renewed stagflation --- recession accompanied by rising inflation and high interest rates ... The only real uncertainty is when the recession will begin."* But **on the exact same day**, September 26, 1983, I said in a full page article in *Forbes* magazine (p. 196), *"people have been panicking unnecessarily about money supply growth this year ... The Divisia aggregates are rising at a rate not much different from last year's ... the 'apparent explosion' can be viewed as a statistical blip."*

The stagflation never developed, as anticipated by my published analysis. To this day the monetarists have not recovered from the two successive public embarrassments. An overview of these events and the evidence can be found in Barnett (1997).[5]

**9.** ***Martin Feldstein interviewed by James Poterba***: Martin Feldstein spent two years as the Chairman of the Council of Economic Advisers during Ronald Reagan's administration, while on leave from his professorship at Harvard University. As explained by James Poterba in his introduction to this interview, *"he warned frequently of the long-term economic costs of large budget deficits, even though this was a very unpopular view on political grounds."* Poterba continues that, *"His 1995 Ely Lecture to the American Economic Association was a clarion call drawing economic researchers to the analysis of Social Security reform proposals, and it anticipated the very active policy debate of the last half decade."* Feldstein has had more than 60 Ph.D. students at Harvard, and has been President of the National Bureau of Economic Research, since 1977. In 1992, he was elected President of the American Economic Association.

Regarding his research on American health care, he observes that *"there was a dynamic in which the higher the price, the more insurance you wanted, and the more insurance you had, the higher the equilibrium market price...my estimates implied that the existing system was on an explosive path in which some exogenous force would be*

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[5] There also is an online press report from the Royal Economic Society's ***Economic Journal*** at http://www.res.org.uk/society/mediabriefings/pdfs/1997/July/barnett.asp.

*needed to stop the rise in the relative cost of hospital care...more co-payment and deductibles would make the health care market work better."*

When asked about his time as Chairman of the Council of Economic Advisors in 1982-1984 under the Reagan administration, Feldstein comments that, *"it soon became clear that the budget deficit was going to be an enormous problem."* In this interview, you will also learn of Feldstein's weekly breakfasts with Paul Volcker.

**10.** ***Christopher Sims interviewed by Lars Peter Hansen***: Chris Sims has been President of the Econometric Society and is a member of the National Academy of Sciences. His role in the development of multivariate time series methodology is fundamental to modern econometrics.

In my opinion, one of the most brilliant publications in the fields of econometrics and statistics is Sims (1971). About that paper, Sims states the following: *"Since the work on infinite dimensional spaces was technically beyond what was appearing in economics journals, I sent Sims (1971) to the **Annals of Mathematical Statistics** ... The editor wrote, 'Sorry it's taken so long. I had a hard time finding any referees. Here's a referee report.' The referee report said, 'I really don't understand what this paper is about, but I've checked some of the theorems and they seem to be correct, so I guess we should publish it.'"*

Among his other comments on applied econometrics, are the following: *"Specifications in which responses to what are purported to be monetary policy shocks are clearly ridiculous, tend not to be reported. This informal aspect has bothered some people."* He also says, *"I argue ... that econometricians have failed to confront the problems of inference that are central to macroeconomic policy modeling."* Revealing his views on macroeconometric policy models, Sims states, *"The models are now in a sorry state."*

**11.** ***Robert Shiller interviewed by John Campbell***: Robert Shiller is known to almost everyone, because of his famous popular book, ***Irrational Exuberance***, which was astonishingly prescient about the stock market bubble that burst shortly after the appearance of the book. That book came out in March 2000, at the top of the market. As he explains in his interview, he *"wrote that book at breakneck speed."* In his interview, Shiller comments that the view that *"every movement in the stock market must have a rational foundation ... is 'one of the greatest errors in the history of economic thought.'"* He further comments on *"the expected present-value model for aggregate stock prices, that is egregiously wrong."*

In his interview, you will learn about the influence on his thinking of his psychologist wife, Ginny, and her associates in psychology. On the general subject of the economics profession, Shiller comments that, *"Economists themselves are herd-like in their research directions, and so there is a lot to be gained by staying away from these common topics."*

**12.** ***Stanley Fischer interviewed by Olivier Blanchard***: Stanley Fischer has been Governor of the Bank of Israel since May 2005. He was interviewed by Olivier Blanchard, Professor of Economics at MIT. The interview was completed while the two of them were running together in Central Park, NY. Stanley Fischer was previously Chief Economist at the World Bank, First Deputy Managing Director of the International Monetary Fund, President of Citigroup International, and Professor of Economics at MIT. According to Olivier Blanchard, Stanley Fischer while a professor at MIT had *"acquired near-guru status,"* and now has become *"a Master of the Universe, and world VIP."* In his interview, you will learn about Stanley Fischer's youth in Southern Rhodesia, now called Zimbabwe.

Included among the statements in his interview, is the following: *"When I was in high school, Dag Hammarskjold was this great man. Then he was killed in the then-Belgian Congo, right next door. I knew he had done good in the world and my parents had brought me up to believe I should do good in the world. I realized that economics would help you do good ... That factor was probably there and moved me over the course of time."*

**13. Jacques Drèze interviewed by Pierre Dehez and Omar Licandro**: Jacque Drèze is one of Europe's most famous and deeply-respected economists. Having received his Ph.D. from Columbia University in 1958 and being a founder of Belgium's eminent economics research center, the Centre for Operations Research and Econometrics, his insights into the evolution of economic thought, and of his own contributions therein, span both sides of the Atlantic. He has received 15 honorary doctorates from universities on both sides of the Atlantic. From this interview, you can learn about the Louvain Bayesian School, the Belgian-French research on general equilibrium under price rigidities and quantity rationing, and other areas of economic research and policy less well known in the U. S. than in Europe.

Of particular interest is his commentary on the difference in policy influence of economists in the U.S. versus those in Europe. On that subject, he observes: *"It is indeed the standard view that economists are less influential in Europe than in the United States. Two comments on that issue. First, in Europe there is no economic authority comparable to the U. S. government. Why? Because Europe is a Union, a confederation of states, so the prerogatives at the level of the Union are limited; the decision process at that level is complicated and carries limitations. Economic advisers to the Commission are remote from the decision making body, namely the Council of Ministers. In contrast, in the United States, the chief economic adviser attends the meetings of the cabinet where the decisions are made. So, there is no chain of communications; the economic adviser is right there. In addition, the cabinet in the United States has much more direct authority than the Council of Ministers in Europe. In that sense, there is much less influence of economic advisers on policy decisions in Europe than in the United States."*

**14.** ***Tom Sargent interviewed by George Evans and Seppo Honkapohja***: As Samuelson's book, ***Foundations of Economic Analysis***, mathematized neoclassical microeconomics and educated a generation of economists in rigorous microeconomic analysis, Sargent's books mathematized modern macroeconomics and educated a generation of economists in rigorous macroeconomic analysis. In keeping with the deep insights evident in all of his published research, his interview is penetrating.

For example, on the evolution of calibration methodology in empirical economics and its relationship with formal statistical theory, Sargent observed the following: *"Calibration is less optimistic about what your theory can accomplish, because you'd only use it, if you didn't fully trust your entire model, meaning that you think your model is partly misspecified or incompletely specified, or if you trusted someone else's model and data set more than your own. My recollection is that Bob Lucas and Ed Prescott were initially very enthusiastic about rational expectations econometrics. After all, it simply involved imposing on ourselves the same high standards we had criticized the Keynesians for failing to live up to. But after about five years of doing likelihood ratio tests on rational expectations models, I recall Bob Lucas and Ed Prescott both telling me that those tests were rejecting too many good models. The idea of calibration is to ignore some of the probabilistic implications of your model, but to retain others. Somehow, calibration was intended as a balanced response to professing that your model, though not correct, is still worthy as a vehicle for quantitative policy analysis."*

He continues that, *"In the 1980s, there were occasions when it made sense to say, 'it is too difficult to maximize the likelihood function, and besides if we do, it will blow our model out of the water. In the 2000s, there are fewer occasions when you can get by saying this."*

Regarding Neil Wallace, Sargent observes: *"Neil thinks that cash-in-advance models are useless and gets ill every time he sees a cash-in-advance constraint. For Neil, what could be worse than a model with a cash-in-advance constraint? A model with two cash-in-advance constraints."* Sargent further observes, *"Except for our paper on commodity money, not our best in my opinion, Neil asked me to remove his name from every paper that he and I wrote together."* Of course Neil's name was not removed from all those papers; Sargent said the following about the introduction to one of the papers that they did coauthor: *"After he read the introduction to one of our **JPE** papers, Bob Lucas told me that no referee could possibly say anything more derogatory about our paper than what we had written about it ourselves. Neil wrote those critical words."*

**15.** ***Robert Aumann interviewed by Sergiu Hart***: Robert Aumann won the Nobel Prize in Economics in 2005, while a professor at the Hebrew University of Jerusalem, one month before his interview appeared in ***Macroeconomic Dynamics***. Aumann is widely viewed as one of the world's most brilliant mathematicians, at the forefront of advances in economic game theory. Born in Germany and educated in America, he is an Israeli who is deeply orthodox in his Jewish religion. His doctorate is in algebraic topology from MIT, and his post-doc was at Princeton. In his interview, he explained that his *"interest in mathematics actually started in high school---the Rabbi Jacob Joseph Yeshiva (Hebrew Day School) on the lower east side of New York City ... I did a bit of soul-searchinig when finishing high school, on whether to become a Talmudic scholar, or study secular subjects at a university. For a while I did both. But after one semester it* *became too much for me and I made the hard decision to quit the yeshiva and study mathematics."*

About his study and research as a Ph.D. student at MIT, Aumann observed: *"Like number theory, knot theory was totally, totally useless. So, I was attracted to knots ... Fifty years later, the 'absolutely useless' --- the 'purest of the pure' --- is taught in the second year of medical school."*

He reveals the following about a conference in 1961: *"Kissinger spoke about game-theoretic thinking in Cold War diplomacy ... People were really afraid that the world was coming to an end."* Regarding the Cuban missile crisis, Aumann states, *"Kennedy was influenced by the game-theoretic school ... Kissinger and Herman Kahn were the main figures in that. Kennedy is now praised for his handling of that crises; indeed, the proof of the pudding is in the eating."*

On the subject of "rationality," Aumann comments, *"People often make the mistake of saying that war is irrational ... We take all the ills of the world and dismiss them by calling them irrational. They are not necessarily irrational. Though it hurts, they may be rational. Saying that war is irrational may be a big mistake ... If we simply dismiss it as irrational, we can't address the problem."*

In reply to a question about religion, Aumann states, *"Religion is very different from science. The main part of religion is not about the way that we model the real world ... Religion is an experience --- mainly an emotional and esthetic one ... When you play the piano, when you climb a mountain, does that contradict your scientific endeavors? ... It doesn't contradict; it is orthogonal ... in science we have certain ways of thinking about the world, and in religion we have different ways of thinking about the world. Those two things coexist side by side without conflict."*

In an interesting commentary on his move with his family out of Germany in the 1930s, Aumann explained: *"We got away in 1938. Actually we had planned to leave already when Hitler came to power in 1933, but for one reason or another we didn't. People convinced my parents that it wasn't so bad; it will be okay, this thing will blow over. The German people will not allow such a madman to take over, etc., etc. A well-known story. But it illustrates that when one is in the middle of things, it is very, very difficult to see the future. Things seem clear in hindsight, but in the middle of the crisis, they are very murky."*

By analogy, Aumann similarly commented on the Six-Day War in 1967: *"In hindsight it was 'clear' that Israel would come out on top of that conflict. But at the time ... it wasn't at all clear that Israel would survive ... Prime Minister Eshkol was very worried. He made a broadcast in which he stuttered and his concern was very evident, very real ... Herb Scarf was here during the crisis. When he left, about two weeks before the war, we said good-bye, and it was clear to both of us that we might never see each other again."*

On another subject, he states, *"I have serious doubts about behavioral economics, as it is practiced. Now, **true** behavioral economics does in fact exist; it is called empirical economics. This really is behavioral economics. In empirical economics, you go and see how people behave in real life."*

**16. *James Tobin and Robert Shiller interviewed by David Colande***r: James Tobin won the Nobel Prize in Economics in 1981, while a professor at Yale. This joint interview of James Tobin and Robert Shiller at Yale was different from the others published in ***Macroeconomic Dynamics*** and was characterized as a "dialogue" rather than an "interview" in the journal. The other interviews were of one person and focused exclusively on the work and life of that one economist. This interview was in the form of a dialogue among two persons and a moderator on a particular topic, "The Yale School of Economics." While Tobin is clearly central to this dialogue, it is interesting to contrast Shiller's part of this interview with the interview of Thomas Sargent. While the Shiller and Sargent interviews are in many ways very different, both provide deep, penetrating, and clearly contrasting insights into modern macroeconomics.

In this dialogue, there seems to be more sympathy for the Milton Friedman version of the conservative "Chicago School" than for the more recent real-business-cycle approach. In response to the following question from the moderator, *"How about the real-business-cycle theorists?"*, Tobin replied, *"Well, that's just the enemy ... That's what we've been fighting about all these years, and that's just a repetition of the conflict between Keynes himself and the economists he regarded as Classicals."* He continues, "*The New Classicals and the real-business-cycle believers are much more extreme than the people that Keynes was arguing with in his day, but it's the same argument over again. Actually Pigou was a much more reasonable, plausible economist than Lucas and some of the other New Classicals."*

In Shiller's part of this dialogue, he says, *"the Yale school must be thought of as politically much more liberal than the conservative Chicago School ... What image do we have of Tobin? To me, he comes through as a very moral person and who has genuine sympathy for others. That means he sees what other people are suffering and he wants to correct that. You get that sense more from him than from very many economists."*

James Tobin died in 2002.

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## *History of Thought Introduction:*
## *Economists Talking with Economists, An Historian's Perspective*

**By E. Roy Weintraub**

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[6], 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 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,

[6] 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.

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 do 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 place[7]. 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.

We have no such organization in economics[8]. The work of historians of economics is carried out by "lone" individuals, and there is no funding source available to sponsor

[7] I note that although both Perry Merhling and David Colander might be considered historians of economics, they each consider themselves to be primarily economists.
[8] 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).

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 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[9]. For historians, context is everything, so they would treat the conversations as partial source material of some limited use in constructing a serious history. 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[10] 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’ 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).

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[9] 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).

[10] For a fuller discussion of the alternative ways historians of economics might construct such histories, see (Weintraub 1999) and Weintraub 2002, 256-272).

But the development of economics is also a small part of a larger story, one in which over the course of the 20th century 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"[11] 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.

In the conversations presented in this volume one does not find much of an emphasis on particular technical details, technical innovations and analysis, as much as 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. 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 influenced 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

[11] I note, from the Samuelson interview, his particular connection to the Bush report.

see that the history of economics is not just the history told by the research scientists themselves, but it is 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 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.

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