

This interview, conducted by Professor Simon Bowmaker of the Stern School of New York University, will appear as a chapter in his volume *The Art and Practice of Economics Research: Lessons from Leading Minds* (Northampton MA: Edward Elgar, 2012). That volume is under copyright, and it may not be reproduced except by permission.

E. Roy Weintraub (Duke University)

E. Roy Weintraub was born in 1943, in Brooklyn NY, and graduated with an AB in mathematics from Swarthmore College in 1964, before obtaining an MS and a PhD in applied mathematics from the University of Pennsylvania in 1968 and 1969 respectively. Professor Weintraub taught economics at Rutgers College, The State University of New Jersey between 1968 and 1970, and then moved to Duke University, where he has remained ever since, currently serving as Professor of Economics.

Professor Weintraub's research interests are life writing and the history of economics, historiography of economics, and the history of the mathematization of economics. His most-cited articles include, "‘Uncertainty’ and the Keynesian Revolution," *History of Political Economy* (1975), 'The Microfoundations of Macroeconomics: A Critical Survey,' *Journal of Economic Literature* (1977), 'On the Existence of a Competitive Equilibrium: 1930-1954,' *Journal of Economic Literature* (1983), 'Axiomatisch Mißverständnis,' *Economic Journal* (1998), and 'Methodology Doesn't Matter, but the History of Economic Thought Might,' *Scandinavian Journal of Economics* (1989). His books include, *General Equilibrium Theory*, Macmillan (1974), *Conflict and Cooperation in Economics* (Macmillan, 1975), *Microfoundations: The Compatibility of Microeconomics and Macroeconomics* (Cambridge University Press, 1979), *Mathematics for Economists: An Integrated Approach* (Cambridge University Press, 1982), *General Equilibrium Analysis: Studies in Appraisal* (Cambridge University Press, 1985), *Stabilizing Dynamics: Constructing Economic Knowledge* (Cambridge University Press, 1991), *Towards a History of Game Theory* (Duke University Press, 1992), *The Future of the History of Economics* (Duke University Press, 2002), *How Mathematics Became a*

Mathematical Science (Duke University Press, 2002), and *Economists' Lives: Biography and Autobiography in the History of Economics* (Duke University Press, 2007), co-edited with Evelyn L. Forget.

Professor Weintraub's academic awards include the Best Monograph Prize from the European Society for the History of Economic Thought and the Joseph J. Spengler Prize for the Best Book in the History of Economics from the History of Economics Society, which were both received in 2005. He is a former President of the History of Economics Society, and currently he is Associate Editor of the *History of Political Economy* and the *Economics Bulletin*. In 2011 he was named a Distinguished Fellow of the History of Economics Society.

I interviewed Roy Weintraub in his office at the Department of Economics at Duke University. It was mid-afternoon of January 14, 2011.

BACKGROUND INFORMATION

You are trained as a mathematician. How did you end up as an economics professor?

The short story is that when I was a second-year graduate student in mathematics, a new PhD program was created that allowed you to write a dissertation in an applied area. At the time, I was struggling to complete my preliminary examinations, and it was becoming clear that I wasn't going to turn into the kind of mathematician who would have a major career. And so I started talking to people around the university who were affiliated with the program. This led to a call from Larry Klein in Economics. He had heard from my father, who taught economics, that I was on the loose, and he asked me to talk to him, because he had some problems that he thought I might be interested in working on. Klein was a family friend, but of course he was also an economist of great stature, and so I decided to write my dissertation in the area of mathematical economics under his co-supervision.

As a student, which professors were most inspirational or influential?

I didn't have any professors who were inspirational or influential. I grew up around them, and so my relationship with them was different from the norm. Being a professor was simply not a big deal. In fact, I used to joke that I was seven years old before I realized that not everybody carried the first honorific of "doctor."

Was an academic career therefore the default profession to enter into?

Sure. When I was in high school, I was even typecast as a professor of geology for the play *Our Town* by Thornton Wilder (*laughs*).

As a researcher, which colleagues have been most influential and inspirational?

The one colleague who's been enormously helpful to me over the years is Neil De Marchi. We're very good friends – I introduced him to his future wife - and he has the kind of broad intellectual interests and engagements that I resonate with. He's always been the best critic of my work, and the person I give my papers to first. As for inspirational, it has to be Barbara Herrnstein Smith, of Duke's Program in Literature, who models for me what intellectual honesty and integrity should mean.

GENERAL THOUGHTS ON RESEARCH

There is an increasing emphasis in many economics departments on applied research. Is this true at Duke?

Yes, over the last decade, we've moved very strongly into that area, and certainly with the encouragement of almost everybody in the department.

What would you say is the value of "pure" versus "applied" research in economics?

I don't know what that means anymore. There are many different categories of research in economics, and standards evolve over time. Someone from outside a particular research community doesn't really have a good idea of what constitutes good and bad work. And so you have to learn to trust, which is the basis of all scientific work.

There seems to be little dialog between theory and the history of ideas in economics, though perhaps that is starting to change a bit in light of the current economic crisis. Do you think that there is now the potential for an increase in such dialog?

The job of a historian of economics is to narrate the past, not to go back and find raisins in the 1870s that improve today's pudding. I don't think there's anything in the past that will make economics better. The only thing that will make economics better is by actually producing better economics, because "better" is a word that's only relevant within a particular research community. They're the ones who judge, award credit and make those kinds of decisions. Most historians of economics disagree, and believe it can be written in the service of improving the discipline. I think that's nuts in the same way that an historian of physics would never think of changing the nature of physics research. But those individuals think of the history of economics from the perspective of an economist, because many of them teach introductory economics courses and socialize with those from the mainstream of the profession; they don't fully engage with the historians' community, with the history of science community, and with the science studies community. In my view, they are quite amateurish historians.

How would you characterize your own research agenda and how has it changed over time?

I began as a mathematician teaching mathematics to economics students at Rutgers. Next I became a mathematical economist, and then evolved into a theorist who was interested primarily in general equilibrium theory. That led to an engagement in the early-80s with the philosophy of

science literature because I wanted to understand how one appraises that kind of scientific work. During this period, I realized that nobody knew very much about the history of specific work in modern economics. And so I started to develop a history of general equilibrium theory, and I quickly became fascinated with pursuing historical research. With the help of a year spent at the National Humanities Center, where I hung out with historians, philosophers, literary scholars and the like, I began to turn myself into a historian.

Do you think it is important to have broad research interests?

I'm reminded the sentence in Isaiah Berlin's essay, *The Hedgehog and the Fox*: "The fox knows many things, but the hedgehog knows one big thing." I would say you have to find a way to be authentic in terms of your personality structure, your socialization patterns, and the work that you do; otherwise, you're going to run into a lot of trouble.

In the end, do you think the profession has helped to bring out and shape your research for the best?

I don't have much contact with a "profession." I haven't much listened to others. I make enough money, I have fun teaching, I enjoy my colleagues, I seem to be productive, and I do work that others find engaging, which always surprises me.

IDEA GENERATION

Where do you get your research ideas?

From my past research. When I'm working on a project, it's always the case that questions remain unanswered, and I can return to them in the future. And when I'm completing a project, I will often have new ideas that will move my research in a different direction. I remember speaking to Joe Spengler at a Christmas party here many years ago. Joe was a former President of the AEA and, in my view, the creator of the fields of economic demography and the history of economics. He was about to leave the party early, and I asked him where he was going. He told me, "I have to deal with the page proofs for a book that I have written on Persian economic thought." I said, "Geez, how did you ever get interested in that?" "Well," he replied, "I was doing a book on *Indian* economic thought and one thing led to another..." And that's exactly how I work. For example, my research on the existence of competitive equilibrium led me to think about whether the work on the stability of general equilibrium was consistent and coherent. Using autobiographical accounts in some of my histories, I began thinking about the nature of autobiographical memory, and so on.

At what point does an idea become a project that you devote resources to?

I can't separate ideas and projects. I do it all simultaneously. Sometimes when a project is moving ahead, I'll realize that it is time to make commitments that force me to bring it to a conclusion, such as finding a book contract or a conference to give a paper, or inviting myself to Paris or London or Rome to give a seminar to my friends.

IDEA EXECUTION

What makes a good theoretical paper? Can you give an example?

One that I spent a lot of time trying to understand where it came from was (Kenneth) Arrow and (Gerard) Debreu's 1954 paper on competitive equilibrium.¹ What makes it a good paper? It depends on who is asking the question. The profession seemed to like it because it solved some problems that were longstanding and connected to important traditions. But it was also valued for particular kinds of techniques within a particular small community out of which it emerged, namely Cowles at Chicago during the 1950 to 1953 period. And so, as a historian, I think it nonsense to believe that you can provide an answer to your question that stands outside of time. The concept of a good paper is local and contingent. And it's the same for an empirical paper. The profession has questions that are open, and papers try to solve those problems. But are those problems the most important ones? How the hell would we know? We couldn't recognize that if it came up and struck us in the face. There is no view from outside the local and contingent circumstances of particular intellectual communities.

Is there a particular empirical paper that you like?

One that gave some drive to my own doctoral dissertation, at least in the mind of my advisor, was the Adelman-Adelman paper, which subjected the early Klein-Goldberger model to random shocks in order to see whether they could induce realistic business cycles in a macro model.² That was an interesting paper.

What advice would you give a young scholar on writing a paper relating to the history of economic thought?

¹ K.J. Arrow and G. Debreu (1954), 'Existence of an equilibrium for a competitive economy,' *Econometrica*, Vol. 22, No.3, pp. 265-290.

² Adelman, I. and F.L. Adelman (1959), 'The Dynamic Properties of the Klein-Goldberger Model,' *Econometrica*, Vol. 27, No. 4, pp. 596-625.

I don't use the phrase, "history of economic thought", because it makes it seem that those thoughts are "out there" and disconnected from people. But history isn't "out there"; people write history. I think a *history of economics* paper is useful and convincing if it constructs an interesting historical argument and employs real historical evidence; in other words, research has been undertaken. I find text-based, exegetical exercises very uninteresting; for example, when someone says, "Let's go back and see what Adam Smith really meant in Chapter 4 of the *Wealth of Nations*..." A history paper is narrative; you're telling stories about the past. And to make your reader emphasize with the past, you have to make the stories as compelling as possible.

Until about a decade or two ago, most historians of economics didn't do much historical research in the same sense that history departments require doctoral students to do. There were folks like Joe Dorfman and Bob Coats who of course did, but they were rather isolated. But at Duke, we now have the Economists' Papers Project archive, which is an immense collection of papers of distinguished economists. This includes professional correspondence, as well as the notes, drafts, and manuscripts of major works. For example, for a long time people have been writing about Paul Samuelson's *Principles* book, but it's been the same story over and over again, because the primary source is the text. But we now have access to the files and documents concerning the process by which Samuelson developed, wrote, and revised his book. When you use those archival materials, you get a much richer story; a sense that, "Yeah, that's what was *really* going on," which is every historian's ideal: you find new materials as a result of your research that allow you to illuminate and reinterpret a story that was pretty uninteresting or very straightforward and not as complex as human experience generally is.

When you hit a "brick wall" on a project, do you continue to work on the problem or do you take a break and work on something else?

I do both. Generally, though, I have several large projects going on at once, and so it takes a while to disengage emotionally and intellectually from a project. You are going to experience

losses in walking away, because streams of thought will no longer be as active on your behalf. But I find I do work in curious ways. My wife will often say, “What are you doing?” I’ll reply, “Oh...” and then she’ll say, “You’re working, aren’t you?” to which I’ll respond, “I guess I am.” And so I probably work 24 hours a day. Of course, I’m not writing when I’m in my sleep, but there are problems that I’m thinking about subconsciously.

I don’t know what “taking a break” means. Just as thoughts of my wife, my children, and my grandchildren are always with me, so are the elements of my research. You can’t just put them away. It would be like saying, “Today, I’m not going to think about my family while I work.”
(Laughs)

What would you say has been the biggest change, in the course of your career, in how your research fields conduct research?

I have seen the efforts that I, and a few others, have made to change the values of the history of economics community. Back in the early ‘80s, when I was beginning to think about the history of general equilibrium theory, Arjo Klamer was Neil De Marchi’s and my PhD student. He wanted to write about new classical economics. I said to him, “Arjo, the people who you are writing about, like Bob Lucas and Tom Sargent, are living. Why don’t you just go and talk with them?” Nobody had previously done such an oral history, and so we sent him off to conduct some interviews.³ What an opportunity! But my getting impatient with Arjo came about because, in gathering the evidence for my work on the history of general equilibrium theory, I had thought to write to many of the folks who were still alive, like Arrow, Debreu, McKenzie, Koopmans, Chipman, Georgescu-Roegen, Tintner, Menger, and Simon. They were all absolutely delighted to have questions asked about their past work. I developed fairly long correspondence files with them and used those self-reports as part of my paper that got into the

³ Klamer, Arjo (1984), *Conversations with Economists: New Classical Economists and Opponents Speak Out on the Current Controversy in Macroeconomics*, Totowa: Rowman and Littlefield.

JEL, which was different from any other paper that I had ever seen people write in history.⁴ And it got me thinking that maybe I and others could do this more systematically, which led me to persuade people at Duke to construct the Economists' Papers Project. So I've seen the history of economics move in this different direction; very different from the kind of history of economics that George Stigler thought was important. I'm very pleased that I've helped to create a major resource for historians of economics to do what I consider to be more interesting work; one that is closer to the history of contemporary science.

What is your view on the increased use of mathematics in economic theory papers today?

You could ask the same question in 1880, 1890, 1900, 1910...this has been going on for a long time.

When I spoke with Chuck Manski, he expressed concern that young theorists today feel the need to show off their math skills, whereas he values "simple insight"?

I heard exactly the same complaints on the part of faculty members at the University of Pennsylvania in Economics in the 1960s. They would say, "He came in and started proving theorems. What the hell did that have to do with economics?" As I say, this is a continuous complaint, and some of it is changing fashions, but also changing conversations within these particular communities. What the community takes as acceptable, useful, relevant and important changes over time. There isn't a correct way to write an economics paper, but Manski's comment suggests there is. There are folks who want to make that judgment a philosophical or even a moral issue, but my argument is that it is always a historical question. How did those tastes change? What was it in the service of? What were the people thinking? What were they

⁴ Weintraub, E.R. (1983), 'On the Existence of a Competitive Equilibrium: 1930-1954,' *Journal of Economic Literature*, Vol. 21, No. 1, pp. 1-39.

doing? How did it work? Those are the questions that interest me as a historian. Only an active scientist could ask your question; one who has no historical sensibility whatsoever.

THE WRITING PROCESS

Which aspect of the writing process do you find most difficult?

For me, the absolutely hardest part is the first draft. If you go back to the Paris Review interviews with famous writers, one of the major questions that was almost always asked was whether they edited during writing or after they had finished. Hemingway, for instance, couldn't write sentence number two unless he was absolutely satisfied with sentence number one. Others just let it rip and then threw away most of it. I'm in the latter category. But in the mid '80s, I figured out that since I talk much faster than I write, it would be best to dictate the material and send it to a typist. And so I generally dictate somewhere between five to ten pages at a shot, and there might be one useful page that makes it into the draft. The other material isn't wasted; it goes into an outtakes file, sits there, and I may come back to draw on it years later to work with it.

I don't begin with an outline for the first draft, because if I did, I probably would never have found the issue interesting in the first place. I need to figure it out, which involves constructing the arguments. At the first draft stage, I have only partial, maybe mutually contradictory, arguments with weak evidence in support. At some point though that draft has enough structure that I can begin the most complex, most challenging, and most fun part of writing, cutting and editing the draft and shaping it. I can edit the draft, or a section of it, each day, put it away, and come back to it over and over to ask questions like: Why is that word there?, Does this really answer the rhetorical question asked in the previous sentence?, or 'How is this argument functioning? I'm trying to make the work speak in a convincing fashion. I love it.

What do you think of the standard of writing by economists?

Oh, economists can't write. Almost none of them have any ear for the English language. There's no sense of a rhythm in their writing, and no particular style that would identify them as the author compared to someone else. There are only a few folks whose writing approaches a level of literary competence that would lead you to read them even if you weren't an economist.

Such as whom?

I think (Deirdre) McCloskey writes well even though I'm not as intrigued by her arguments as I was in the past. Mary Morgan writes with a really authentic voice, and I have to attend very carefully to what Mary says, because she writes in a very understated fashion, but there's tremendous power in much of her work. (Philip) Mirowski writes in a very distinctive manner. I'm not always happy with what I'm reading, because it's too argumentative for me, but it's certainly well written, and there is no question of who the author is.

Who proofreads your writing?

I proofread it, and if De Marchi is around, I'll give it to him, but he's increasingly busy. When you write books, you must have a very good relationship with the general editor and then you need to be very accommodating and cooperative with the copy editor. I've been fortunate, because the folks who I've worked with at Cambridge University Press and Duke University Press have been great. Currently, I'm doing a piece that's going to appear in the Spring issue of

the *Journal of Economic Perspectives*.⁵ Their editorial work on papers is much better than any I've found in professional journals, because they have a different audience. The managing editor, Tim Taylor, is very, very good and helpful. Who else? I had one absolutely spectacular copy editor for a paper I did a while ago for the *South Atlantic Quarterly*.⁶ That's a literary journal, but it was just wonderful to have an experience of working with someone whose language sense was so good.

COLLABORATION

Most of your work is sole-authored, whereas much of the work conducted by the other interviewees in this book is collaborative. What are the pros and cons of your approach?

When I was Acting Dean of the Faculty here, I was problem solving all the time. I tend to have many ideas, and can produce ten different solutions to a problem very quickly. Eight of them are totally stupid, one of them is half-stupid, and the other one is worth thinking about. The function of my staff at the time was to critique my solutions. And I would say that in collaborative research, you see those two roles at work; one person produces different ideas, and the other sets out to test them. That's how my very early theoretical work with Daniel Graham was done; I would have ideas that he would attempt to prove and disprove, and vice versa. That was very useful. In fact, I'm sure that none of those technical papers could have been written individually by either of us.⁷

⁵ Weintraub, E.R. (2011), 'Lionel W. McKenzie and the Proof of the Existence of a Competitive Equilibrium,' *Journal of Economic Perspectives*, Vol. 25, No. 2 (Spring), pp. ??

⁶ Weintraub, E.R. (1995), 'Is "Is a Precursor of" a Transitive Relation?,' *South Atlantic Quarterly*, Vol. 95, No. 2, pp. 571-589.

⁷ Graham, D.H., Jacobson, E. and E.R. Weintraub (1972), 'Transaction Costs and the Convergence of a "Trade Out of Equilibrium" Adjustment Process,' *International Economic Review*, Vol. 13, No. 1, pp. 123-131.
Graham, D.H. and E.R. Weintraub (1975), 'On Convergence to Pareto Allocations,' *Review of Economic Studies*, Vol. 42, No. 3, pp. 469-472.

I haven't done collaborative work on my historical projects, but I'm about to co-author a book on the history of existence of general equilibrium with a young German researcher. He and I have been corresponding back and forth because he's unearthed, and has access to, the Debreu papers, which include lengthy interviews with members of the Debreu extended family. I'd been planning to make my McKenzie material into a book, because I have so much of it, and I started thinking, "God, I wish I had the Debreu papers..." I wrote to him asking whether he was the least bit interested in my McKenzie material. He jumped at it! And so we are going to write the book together. It should be interesting, because it's my first attempt at joint work on a historical project. As you get older, sometimes you want to try something different. Can we bring this off? I don't know. It may just crash and burn, but at the moment it seems like fun.

SEMINAR PARTICIPATION AND NETWORKING

What are the benefits to attending a seminar that is closely related to your work versus one that is not closely related?

There are different ways of taking in information. When I was a student, I couldn't both attend to what the professor was saying and take notes. It was one or the other, and I decided to screw it; I was more interested in getting a sense of what was going on and gave up on notes. My older son, who like me has some ADHD issues and specific learning disabilities, takes in information by ear; not by sight, not by reading, but if he hears it, he knows it, and he has figured out strategies over his life to get that kind of information to do it. It's just breathtaking to watch. And so, for him, going to a seminar would be great. For me, it is almost worthless. I can watch the performance and get a sense of the theatre, but I need to read the paper several times, otherwise I don't understand it.

How important is professional networking to success in research?

There's a sense in which it's so important that it's almost not worth talking about, because professional success *is* success in a network. That's how science is done; you have to be in networks, you have to convince people, and you have to learn from others. The community is where the science is embedded; it's not in a paper somewhere. Who's your audience? Who thinks what work in this area may or may not be worthwhile? That's what I think of as a network.

I've been on many promotion and tenure committees, and ultimately the one question you're trying to ask is: Is this person a player? And the answer is entirely network-based. You can't be a hermit and be very interesting or successful at the same time. Wittgenstein may have been anti-social, but he was at the center of a large number of other philosophers and scholars at King's College, Cambridge for a long period of time.

COMMUNICATION OF RESEARCH

How do you find the right balance between communicating your research at an early stage versus the close-to-finished stage?

I've never much cared about holding back work. We've got lots of informal ways of having internal conversations here, like lunch seminars, and I no longer feel embarrassed about going on the road with work that's not completely finished. I'm interested in talking to people and having them think with me. If I get comments such as, "That doesn't make any sense at all", I will deal

with it, because it doesn't harm me to be criticized. I don't go to places where I know I'm going to have a hostile audience, but there aren't many such places. I'm very easy to like (*Laughs*).

What are the unique challenges to giving a seminar and how do you overcome them?

What I generally do is talk about the context of the paper, which means historicizing it in terms of my own interests in projects, outline the major issues and questions that it opens up, and then turn the seminar into a conversation with as much of the audience as is interested. I'm fairly gregarious, and I don't regard public speaking as a chore; I find it interesting.

PUBLICATION

How do you decide upon the appropriate journal to send your research to? Related, whom do you view as the readership of your research?

I don't write articles with an idea for a particular journal. The project gets going, and if it's good enough, it'll find one. My paper on McKenzie started getting bigger and bigger, becoming more interesting, and opening up a number of different issues, so I decided to send a short more general interest piece of it off to the *Journal of Economic Perspectives*. There was a detective story involved in the research, because I uncovered a big-time surprise, namely that the referee for the McKenzie general equilibrium paper turned out to be Gerard Debreu. That came as a shock to a lot of people. The *JEP* hasn't published much in the history of economics that has been interesting historically, but I got an incredibly positive response from the editors – they said they couldn't put it down.

For about five years, I've been working on a manuscript called, "Charismatic Scholars, Intellectual Communities, and the Allegation of Anti-Semitism," which features case studies of Jung, De Man, Marx, Keynes, and Hayek. I have had some very positive responses from readers, but some of the different presses thought it was way too interdisciplinary. A reader who was a sociologist thought there wasn't enough sociology. The literary studies reader basically said, "Who the hell is this economist dragging his feet all over Paul De Man? What does he know?" Well, I know a lot more than the guy thought. Anyway at present I've ended up extracting the material on Keynes and it's been accepted at a history of economics journal.⁸ But I'll be back to the large project again, sometime.

How would you best describe your approach to dealing with a "revise and resubmit" request from a journal? How about an outright rejection?

By and large, referees are serious people who take themselves seriously, so I address carefully their comments. As you get older, you tend to insulate yourself against rejections by finding ways to anticipate that a particular journal won't be interested in a particular topic. And so I'm not going to send my work to the *AER*.

Do you think that the current structure of the publication process in economics facilitates or impedes scientific understanding and knowledge production?

Journals are increasingly irrelevant for the transmission of knowledge. Work is seen, thought about, and talked about, by the established communities long before it appears in a journal. Getting it published is only serving the function of validating credit in certain ways; senior people are not publishing in journals as much as they used to – they're submitting their work to collections or giving it as addresses – and this is effectively leaving the publications for junior faculty in order to make reputations and gain tenure. The AEA's creation of a bunch of new

⁸ Weintraub, E.R. (2012 forthcoming), 'Keynesian Historiography and the Anti-Semitism Question,' *History of Political Economy*.

journals is a reflection of the fact that there needed to be more avenues through which junior faculty members could be certified.

What has been your best and worst experience during the publication process?

Many years ago, I had one very bad experience with a very prestigious journal. There was simply no response whatsoever to the submission, even for a year any acknowledgement that it had been received. What I was able to put together from a couple of conversations some years later was that there was some embarrassment at the journal, and it was quite possibly associated with the paper having gone to an anti-Semitic referee in Germany.

I've had exceptional good fortune with several papers. Both of my papers in the *Journal of Economic Literature* were magnificent intellectual exchanges with the editor, Mark Perlman. I felt really, really aided and quite pleased to be part of such a co-operative process.

BOOK WRITING

You have written several books. Why do you write them?

Because I have to! (*Laughs*) Don't scholars write books? I thought they did! (*Laughs*) Sometimes the story is large and complex, and that's what a book is.

Do you enjoy writing books?

I love it. It's a very satisfying process because you can always be working on it. But it takes a long time to write a good book because it is very, very difficult to sustain an argument over several hundred pages. I've written three really good, "mature," single-authored books, but what I also am pleased about is that every book that I've done is, in my view, better than the one before it. That's why I'm apprehensive about doing the present one, because I thought my last book, *How Economics Became a Mathematical Science*, was terrific! I had to interweave many different stories so that they mutually supported one another. That was hard, and it took ten years to do, but it was very satisfying to bring off something as large and complex. Why do you think people write symphonies rather than works for solo piano? (*Laughs*)

Books have long been a staple of the history of economics literature. Yet, the book publishing industry seems to be in a state of flux, and the increasing prominence of "impact factors" and the like are pushing the profession increasingly toward journal articles as the means for professional advancement. What advice would you give to a young scholar in the field about book writing versus journal article writing?

For history of economics, I think you have to be doing both. In institutions and countries where there are comprehensive research assessment exercises, you have to write journal articles, and if you want to be taken seriously as a scholar of such-and-such, then you have to write a book on it. For example, probably the best scholars of Jevons are Harro Maas, Margaret Schabas, Sandra Peart, and Michael White. Michael has written many magnificent articles on Jevons, but he has not written his book on Jevons, and I hope he does it.

What are your thoughts on the future of book publishing in economics?

Books have been around for a very long time, and I don't think they're going to disappear. Some arguments are long and complicated; they can't be tweeted (*Laughs*).

REFEERING AND EDITING

Do you have any advice for a young scholar on being a referee?

No. It would only be the same kind of advice if I were asked a question like, "How can you become a better human being?"

My wife is a marital therapist, and she tells me that one way to improve communication in a couple is to ask one party, before responding to statements, what the other has said, to make sure there is a baseline of respect for the other. That is helpful to think about when you are refereeing. And so I've tried to keep it a practice of attempting to restate the major points and the argument of a paper in the first paragraph of my report in such a way that I can convince myself that the author would agree with me. The author is seeing that I do understand what he or she is saying, and I'm already forcing myself to be in a position of asking how this paper can be stronger, or what are its weaknesses that either can or can't be repaired. That strategy removes my persona to some degree from the refereeing.

TIME MANAGEMENT

How do you divide up your working day, both in terms of quantity and timing of different kinds of work?

I don't. When I'm walking on the indoor track, I'm thinking about work. I can't compartmentalize anything.

How do you balance multiple research projects?

It's just something you do.

Some of the interviewees prefer to complete one project at a time...

I can't do that. I read five or six books at a time. Look, here are some that I cleared off my desk for you today (*shows me several, weighty history of economics volumes*).

How about the balance between your research and non-research activities?

I really like teaching. I'm teaching small classes of juniors and seniors who are interested in the courses that I teach. I find the students utterly delightful and very smart, and I like engaging with them; it keeps me connected. If you have to convince people that what you do is interesting and enjoyable, you're also confirming that for yourself. And so if I can't do that to a class of juniors and seniors, I think I should retire, because I would no longer believe in what I'm doing.

As you get older, your energy level is lower for doing all kinds of other things, but I've done everything here at Duke. I was Director of Graduate Studies for a dozen years, Chair of the

department for four years, Chair of a second department at the same time for a year, twice Chair of the Academic Council, and Acting Dean of the Faculty of Arts and Sciences for two years. I've been very involved with the life of the university. These days, I've withdrawn from most of it, but I'll probably be pulled out of retirement to sit on an august committee to give advice to an important person (*Laughs*).

How about the balance between your professional and personal lives?

Part of my marriage contract with my second wife over twenty years ago was to take seriously "wither thou goest, I will go." And so we go to all conferences together. That's a balance.

I was very, very active physically - playing tournament tennis and squash - but that had its own toll. I now have a badly damaged shoulder, so I just can't do some sports anymore. But it's better than being dead (*Laughs*).

REFLECTIONS AND THE FUTURE OF ECONOMICS

What have been your most important findings and contributions in your research fields during the course of your career?

I've shown the history of economics community that the technical economics of the post World War II period can be historicized; one does not have to keep writing about Smith or Ricardo or Keynes or Hayek. More importantly, I've both shown and argued that such historicizing should be based on both archival material and oral histories. Almost nobody was doing this before me. And so I've helped to inaugurate a new era in the history of economics.

My other lasting contribution is the Economists' Papers Project. I hope I'm thought of like Bob Gallman who collected an enormous set of pre Civil War plantation records across the South to get a full and complete picture of southern slaveholding, and those records have been used by American economic historians ever after. Gallman is lionized. I was the initiator and internal force of the Duke archival facility, and I'm very proud of it.

What are the biggest challenges facing the history of economics?

Figuring out what it's supposed to be doing; whether it's economics or it isn't.

What are the strengths and weaknesses of your own research?

Something that others, particularly Mark Blaug and Philip Mirowski, have claimed as a weakness, but I see as a strength, is that I don't use the history to promote one or another view of how economics might be improved. It's a constant criticism that I'm too neutral; that I have no position on whether general equilibrium theory is a good or bad thing, for example. But I criticize Mirowski's work for being too engaged in writing history in the service of particular kinds of intellectual positions.

On the other hand, I've always been pleased that people comment on how well written and how engaging my narratives are, and that the range of my scholarship is a model for how one can approach historical writing in economics.

Do you have any professional regrets?

I don't much like counterfactual history. I'm sorry though that I'm not going to live for another hundred years; it's so much fun to be doing what I'm doing. That said, God knows what shape my body would be in by that time! (*Laughs*)

Do you have any professional ambitions?

To keep working on the kinds of projects that I've developed, showing that it can be done.

How would you describe the state of economics today? Are you optimistic about its future?

I don't really consider myself an economist in the sense of being a member of the mainstream economics community anymore. Unlike many who write in the history of economics, I like economists, I respect them, and I think by and large they're doing very interesting work. I'm enthralled by the work of my junior colleagues. I can't open issues of major journals these days without just being tremendously impressed at the topics that people are engaging and arguments that are being employed. It's wonderful.