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MD Interview

AN INTERVIEW WITH ROBERT E. LUCAS, JR.

Interviewed by Bennett T. McCallum

Summer 1998

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. His work provided the primary stimulus for a drastic overhaul and revitalization of that broad area, an overhaul that featured the ascendance of rational expectations, the emergence of a coherent equilibrium theory of cyclical fluctuations, and specification of the analytical ingredients necessary for the use of econometric models in policy design. These are the accomplishments for which he was awarded the 1995 Nobel Prize in Economic Sciences. In addition, he has made outstanding contributions on other topics—enough, arguably, for another prize. Among these are seminal writings on asset pricing, economic growth and development, exchange-rate determination, optimal fiscal and inflation policy, and tools for the analysis of dynamic recursive models.

Clearly, Bob Lucas is very much a University of Chicago product; he studied there both as an undergraduate and as a Ph.D. student and has been on the faculty since 1975. Also, he has served as chairman of the Chicago Department of Economics and two terms as an editor of the *Journal of Political Economy*. Nevertheless, I and several colleagues at Carnegie Mellon like to point out that Bob was a professor here in the Graduate School of Industrial Administration from 1963 until 1974, during which time he conducted and published the central portions of the work for which he was awarded the Nobel Prize. Consequently, I could not resist asking Bob a few questions about his GSIA years in the interview.

Many researchers in the economics profession have been impressed and inspired by Lucas's technical skills, but the clarity and elegance of his writing style also deserve mention, plus his choice of research topics. The latter is reflective of Bob's utter seriousness of purpose. Each of his projects attacks a problem that is simultaneously of genuine theoretical interest and also of considerable importance from the perspective of economic policy. There is nothing frivolous about Lucas's research, as he had occasion to remind me during our interview.

As is well known to those who have been around him, Bob Lucas is a person who never uses three words when one will suffice—but that one will usually

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Figure 1. Robert E. Lucas Jr.

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be carefully chosen. This characteristic shows up in the interview below. As a departure from standard MD Interview practice, and with the Editor's permission, this interview was conducted at a distance—i.e., via mail and e-mail. It yielded a smaller number of pages than have previous interviews, but I think that readers will find them stimulating. The process of obtaining them was somewhat challenging but highly informative and thoroughly enjoyable for me.

Keywords: Rational Expectations, Business Cycle, Policy Analysis, Economic Growth and Development, Asset Pricing, Optimal Taxation

McCallum: Let me begin by asking how and when you got interested in economics, both generally and as the subject for a career.

Lucas: When I was 7 or 8, my father asked me if I had noticed how many different milk trucks stopped at our block: Darigold delivered to some houses, Carnation to others, and so on. We counted to 5 or 6. He asked me if I thought there were any differences in the milk provided by these dairies. I thought not. He then told me that under socialism only one truck will deliver to all the houses on each block, and the time and gasoline wasted in duplicating routes will be used for something else.

I doubt very much that this was my first discussion of economics, but it is the earliest I can remember. My parents had come of age politically in the 1930s, and the virtues of free markets were not right at the front of their thinking, or mine. We took it for granted that an economic system should be intelligently managed, and we debated every day over the details of how this could and should be done.

As an undergraduate at Chicago in the 1950s, I got the idea that an intellectual career was a possibility, and knew that was what I wanted for myself. In college, these interests and prejudices led me to history. Early in graduate school, I shifted to economics.

McCallum: And how did you happen to go to Chicago as an undergraduate?

Lucas: My alternative was to stay at home and attend the University of Washington in Seattle. Chicago gave me a full-tuition scholarship, which was the ticket I needed to move out on my own. This was something I needed to do.

McCallum: Then as a graduate student in history? Can you tell us a bit about your reasons for shifting to economics?

Lucas: I drifted into economics from economic history, with no idea of what economics is or what economists do. This was just luck, but I soon discovered the essential role that mathematical reasoning played in economics, and it didn't take me long to see that this way of thinking about human behavior was congenial to me.

McCallum: How did you develop your outstanding mathematical tools?

Lucas: It is easy to forget how little math one needed to know to be at the technical end of economics, back in the early 1960s. I had had calculus and differential equations as an undergraduate, before I got into history. Samuelson's *Foundations* taught me (and the rest of my cohort) how people were using math in economics.

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Figure 2. Louis Chan, Robert Lucas, and Chi-Wa Yuen at Victoria Peak in Hong Kong.

In my summers as a graduate student, I took a linear algebra course and a rigorous calculus course. I also took the mathematical statistics sequence from Chicago's statistics department. With this background, I have kept learning on my own, and much of the math I use now I picked up since leaving graduate school.

McCallum: While you were a Ph.D. student at Chicago, which faculty members had major influences on your intellectual development? Describe these a bit, please.

Lucas: The biggest influence by far, on me and all my classmates, was Milton Friedman. His two graduate price theory courses were fabulously exciting and valuable: a life-changing experience. But I was a very receptive graduate student and learned a lot from many other people. Al Harberger was doing quantitative general equilibrium modeling then, in a way that still looks quite modern. Martin Bailey, Carl Christ, and Harry Johnson were our other macroeconomics teachers. Gregg Lewis went through his book on unions in an advanced seminar that I learned a lot from.

Among the younger faculty, Zvi Griliches taught econometrics, and encouraged technical types like me. Dale Jorgenson, a visitor in 1962–63, was inspiring to me. Don Bear taught a terrific course in mathematics for economists.

McCallum: Somehow I had the impression that Uzawa influenced you in some way. Is that just completely wrong?

Lucas: Uzawa joined the Chicago faculty the year after I left, so he was not one of my teachers. But I did attend two summer conferences on dynamic theory that Uzawa and David Cass organized, one at Chicago and another at Yale. These involved me in intense interactions with the best young theorists in economics. I

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liked the idealism and seriousness of the tone Uzawa and Cass set. I was flattered to be included, learned a lot, and gained a lot of confidence.

McCallum: Which workshops did you attend regularly?

Lucas: There were many fewer workshops then than we have now. Everything in econometrics and mathematical theory went on in the Econometrics Workshop. Zvi and Lester Telser ran it, and Merton Miller and Dan Orr from the business school were regulars. I was too. Al Harberger ran the Public Finance Workshop, which all the students working with him (as I was) attended. Gregg Lewis invited me to give a paper at the Labor Workshop, but I was not a regular there.

McCallum: So you did not attend the Money and Banking Workshop?

Lucas: Attendance in workshops then was by invitation, and I was never asked to attend the Money and Banking Workshop. But there was no reason why I should have been. Money and Banking was not one of my prelim fields (those were Econometrics and Public Finance) and I did not work with Friedman.

McCallum: I believe that you became an assistant professor at Carnegie Mellon—then Carnegie Tech—about 1963. Is that approximately correct?

Lucas: Yes. I came to the Graduate School of Industrial Administration—GSIA—in September 1963. Tren Dolbear, Mel Hinich, Mort Kamien, Lester Lave, and Tim McGuire came at the same time. I think we were the first cohort hired by Dick Cyert, then a new dean.

McCallum: How did you get started with rational expectations analysis? Did John Muth have much direct influence on your thinking?

Lucas: Before I left Chicago, Zvi Griliches told me to pay attention to Jack Muth, that he was someone I could learn a lot from. That turned out to be good advice! I learned a lot from Jack, but it was a few years before I appreciated the force of the idea of rational expectations. This happened when I was working on "Investment Under Uncertainty" with Ed Prescott.

McCallum: Do you have any thoughts about the intellectual processes that led Muth to his rational expectations hypothesis?

Lucas: The opening paragraphs of his "Rational Expectations and the Theory of Price Movements" are very informative and interesting. One can see the extent to which Muth was influenced by and was reacting to Herbert Simon's work on behavioral economics, and how this led him to such a radically nonbehavioral hypothesis as rational expectations. (I once tried to discuss this with Herb, thinking of it as an instance of the enormous, productive influence he had on all of us, but he took offense at the suggestion.)

Jack was the junior author in the Holt, Modigliani, Muth, and Simon monograph *Planning Production, Inventories, and Workforce*. This was a normative study—operations research—that dealt with the way managers should make decisions in light of their expectations of future variables, sales, for example. I'm sure it was this work that led Muth to think about expectations at a deeper level than just coming up with regression equations that fit data.

The power of thinking of allocative problems normatively, even when one's aim is explaining behavior and not improving it, was one of the main lessons I learned

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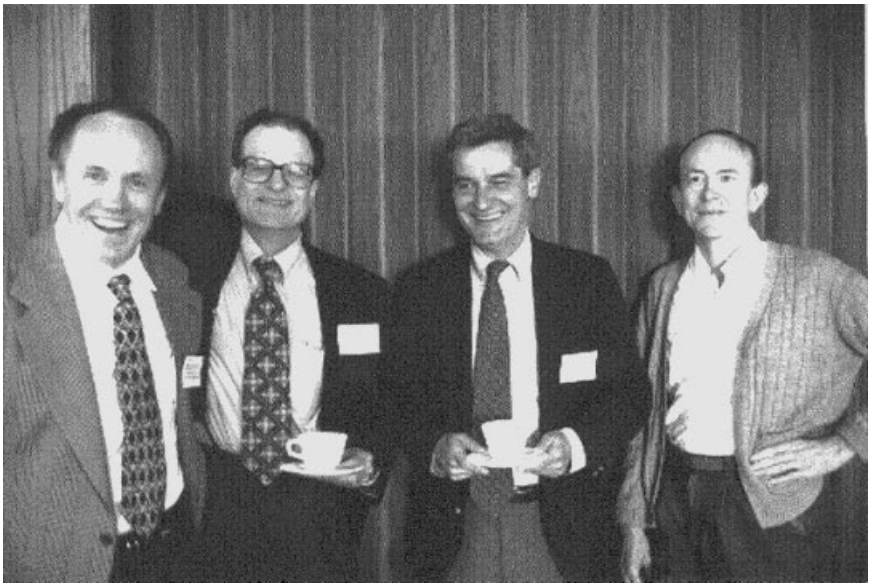


Figure 3. Ed Prescott, Tom Sargent, Bob Lucas, and Buz Brock at a conference.

at Carnegie, from Muth and perhaps even more from Dave Cass. 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.

McCallum: Please describe other aspects of the intellectual atmosphere at GSIA that were important to your professional development.

Lucas: I guess I have already referred to the influences of Herb Simon, Dave Cass, and Ed Prescott in answering your question about Muth's influence. In general, GSIA offerred me a nice mix of people whose point of view on economics was pretty close to mine, like Leonard Rapping and Allan Meltzer, and others like Simon, Muth, Cass, and Prescott, to name just a few, who could come at problems from angles I never would have hit on my own.

McCallum: Please describe aspects of the atmosphere at Chicago, after your return in 1974–75, that were important to your continued professional development.

Lucas: At Chicago, I began teaching graduate macroeconomics regularly for the first time in my career. (Allan Meltzer had done this at Carnegie.) This was a stimulus for me. My papers "Understanding Business Cycles" and "Problems and Methods in Business Cycle Theory" came out of the experience of organizing my

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thoughts on the entire field, the way teaching a graduate course in a top department forces one to do.

McCallum: Your Nobel Prize was awarded for work in reconstructing the fields of macro and monetary analysis so as to incorporate the hypothesis of rational expectations. Before we go on to other interests of yours, are there points regarding this topic that you would like to make? Has the macro profession evolved in a manner that you are pleased with?

Lucas: Like most scientists, I imagine, I tend to be pleased with developments that confirm my prejudices and make my conjectures look good. So 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. Pleasure aside, though, I feel I have learned a huge amount from research in real business cycle theory. I think about the relation of theory to data and about the sources of fluctuations now at an entirely different level from the way I thought 15 years ago.

McCallum: How important quantitatively are technology shocks, in your opinion, in generating business cycles?

Lucas: The answer must depend on which cycles we are talking about. 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, if we are talking about fluctuations in the postwar United States the relative importance of technology and other real shocks is *much* larger, something like 80% of the story.

McCallum: But "technology and other real shocks" would include shocks to preferences, government spending, terms of trade, and possibly other things. How about pure technology shocks—shocks to production functions—in the postwar U.S. context?

Lucas: I don't know how my 80% guess would break down among these and other real shocks. I'm not even sure there is such a thing as a "pure technology shock." I guess for me the central distinction is between shocks that competitive markets can deal with efficiently, without any intervention (all of those on your list, and more) and shocks that need to be offset by a monetary response.

McCallum: In your opinion, is price stickiness an important economic phenomenon?

Lucas: 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. I take this to be what we mean by "price stickiness."

McCallum: There has been some disagreement among monetary economists concerning the most appropriate target variable for the European Central Bank, with inflation and money growth targets being the leading contenders. What are your views on that issue?

Lucas: That's a classic question for any central bank. I like the policy you've studied of formulating a target for the path of nominal output and then using a slowly reacting feedback rule for the monetary base to keep the system moving toward that target. If you want to replace "nominal output" with "inflation rate,"

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this policy still has a lot of appeal, though less. If you want to replace "monetary base" with "M1," it has even more appeal, to me.

If you replace "monetary base" with "short-term interest rate," you get a version that everyone seems to like nowadays, and I'm willing to get on board myself for pretty much anything that keeps the focus on price stability. But I don't understand how this particular feedback system works, and I am concerned about the kind of bad dynamics that Wicksell, and more recently Peter Howitt, worried about.

McCallum: Do you actually believe that the welfare costs of cyclical fluctuations are as small as indicated in your Jahnsson Lectures, or were these numbers presented mainly as a challenge to the profession to explain?

Lucas: I don't write things I don't believe in just to be provocative! Those estimates may be too small, but if so, it is an honest mistake. The estimates I reported there are the welfare cost of postwar U.S. consumption fluctuations, under the assumption that idiosyncratic risk is perfectly pooled. As I explained in the lectures, the costs of 1930s-level crashes were vastly higher, and were aggravated by the absence of unemployment insurance and other features of a modern welfare system.

The reason these costs came out so small is that they are proportional to the variance of consumption, which is very small in the postwar period in the United States. How can one get large costs from so little variability? No one else has, either, except by assuming enormous risk aversion. Of course, this reduced variability is due at least in part to the sensible monetary policy pursued over these years. 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.

McCallum: Could you make a few comments on your views regarding microeconomics over the past, say, 25 years?

Lucas: In the past 15 years, microeconomics has come to be synonymous with game theory in many places (not including Chicago!), and that is unfortunate. About 99% of all successful applied economics is still based on the idea of a competitive equilibrium. But game theory *has* given us a language for talking about resource allocation with private information and about issues of reputation that represents a huge advance over anything that you and I learned in graduate school.

McCallum: Some other major contributions of yours have concerned asset pricing theory, economic growth and development, and the role of economic theory in econometrics and policy analysis. Could you please tell us how you were led into each of them?

Lucas: The origin of my asset pricing paper makes the best story. I was interviewing Pentti Kouri, then a job-seeking new MIT Ph.D., in my office in Chicago. Kouri didn't want to waste our half hour talking about Chicago winters, so he asked me: "How would you price assets in the following economy?" and then went on to describe the model that is treated in my paper. I went to the blackboard and began writing Bellman equations and clearing markets, and the fact that you didn't need to know the value function to get a very tractable functional equation for prices

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fell right out in a few minutes. Kouri was not interested in collaborating, so I wrote up these results and others myself.

McCallum: What about your increased emphasis on growth and development? Did that stem partly from the Jahnsson Lecture numbers or had you been interested in this area all along?

Lucas: I taught an undergraduate elective in economic development at Carnegie Mellon, and have been interested in this area as long as I can remember. But my research is guided more by my hunches as to where I might be able to make some progress than anything else. I found myself slipping into the same old ruts in thinking about business cycles, and thought it would be good to think about something else.

McCallum: Your writing is regarded by many in the profession as quite elegant. Do you work hard at your writing?

Lucas: Thank you for the compliment. I revise a lot, though I think of that more as an effort to get the logic straight than as an attempt at style. I also read a lot of people who are *really* good writers, and I'm sure something rubs off.

McCallum: How did you manage to give up smoking?

Lucas: Well, I started smoking when I was 13 and quit when I was 56, so I'm not ready to set up as an advisor on this problem. I quit cold turkey, with the help of nicotine patches. Fear, nagging, and social stigma were all contributing factors.

McCallum: You and Paul Romer both made outstanding contributions to growth theory during the 1980s. Were you Paul's dissertation supervisor? Could you tell us a bit about your interactions on this topic?

Lucas: In teaching macroeconomics, I have been treating a many-country version of Solow's model as a (tentative) model of development for many years. Paul was certainly exposed to this set of problems in my class. But the increasing returns-externalities model that Paul developed in his thesis was entirely his, and new to me. Sherwin Rosen and Ted Schultz told Paul about Allyn Young's work, but I had never heard of that, either.

The model in Romer's thesis raises novel technical problems, since it does not converge to any steady state or balanced path. Jose Scheinkman helped him on this, and I believe chaired his thesis committee as well.

McCallum: Do you have any interest in working for a few years in an economic policy making position? Do you think that one or two years in such a position tends to improve or worsen an economist's subsequent academic work?

Lucas: Back in the late 60s, when George Schultz was Nixon's Secretary of Labor, Schultz asked me to work as an advisor to him. The job was then held by my friend Jack Gould, and it was an interesting position because Nixon was looking to Schultz for help on a much wider range of economic questions than just labor issues. Later Schultz moved to a more central job at OMB, and if I had taken the job I would have moved with him. Schultz called me in person, impressing my secretary at GSIA enormously, and for that matter (why be blase?) impressing me too! But flattered or not, I was excited about my research at that time and didn't want to interrupt my work with a stay in Washington. I declined.

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Do I regret this decision? When I turned the job down, Arthur Laffer accepted it. You never know about such things, but my guess would be that I, Art, and the U.S. economy were all better off as a result, and I can take some pleasure in my role in helping to locate a Pareto-dominant decision.

McCallum: How about writing a regular column on economics for a newspaper or popular magazine? Would you have any interest in such an undertaking?

Lucas: Maybe someday, but not now. I like the sense of discovery and intellectual progress that I can get from doing technical economics. In order to get this sense, one needs to spend a lot of time facing problems one doesn't understand and will probably never understand. This is hard to do, and as you get older and more famous you get more interesting and pleasant excuses to avoid doing it. The last thing I need is more such excuses.

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