

MD INTERVIEW

OUTSIDE THE MAINSTREAM: AN INTERVIEW WITH AXEL LEIJONHUFVUD

*Interviewed by Brian Snowdon
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Axel Leijonhufvud made an enormous impact on macroeconomics in the late 1960s with the publication of his book *On Keynesian Economics and the Economics of Keynes: A Study of Monetary Economics* (1968). In this famous book, Leijonhufvud argued that the standard neoclassical synthesis (Hicks–Hansen IS–LM) interpretation of the General Theory totally misunderstood and misinterpreted Keynes. However, during the 1970's, interest in Keynes and Keynesian models waned as new classical equilibrium models became all the rage. Nevertheless, Leijonhufvud, from a position outside the mainstream, continued his research into problems of unemployment, business cycles, and inflation—issues that from his perspective are problems of coordination failure in complex dynamical systems. Axel Leijonhufvud is currently Professor Emeritus at the University of California, Los Angeles, and, since 1995, Professor of Monetary Economics at the University of Trento, Italy. In this interview the author discusses with Leijonhufvud a wide range of issues relating to his own work as well as his views on the development of macroeconomics after Keynes.

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Axel Stig Bengt Leijonhufvud was born in 1933 in the small town of Hässleholm in southern Sweden. After graduating from the University of Lund in 1960 he did his graduate work at the University of Pittsburgh and Northwestern University. After a year as a Brookings Fellow, he joined UCLA in 1964 where he remained until his retirement in 1994. In 1991, he founded the Center for Computable Economics at UCLA and remained its Director until 1997. Since 1995 he has been Professor of Monetary Economics and a Programme Director at the

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FIGURE 1. Axel Leijonhufvud, circa mid-1980's.

Computable and Experimental Economics Laboratory (CEEL), at the University of Trento.

I first met Axel by chance in January 2002 while attending the AEA Conference in Atlanta. He was sitting at the adjacent table at breakfast and we started talking

about macroeconomics. I asked if he would be willing to be interviewed with the aim of producing an article and he agreed.¹ After corresponding by e-mail, the interview was organized to take place on May 17 at the University of Trento, where Axel spends about 6 months every year. This was my good fortune since Trento is a beautiful city located in the mountains of northern Italy. I received a warm welcome from members of the Economics Department and from Axel, who proved to be an excellent and exceptionally entertaining host.

The publication in 1936 of John Maynard Keynes's *The General Theory of Employment Interest and Money* was a landmark in the history of macroeconomics. Indeed the *General Theory* gave birth to what we now call macroeconomics since what existed before 1936 consisted of an "intellectual witches brew: many ingredients, some of them exotic, many insights, but also a great deal of confusion" [Blanchard (2000)]. In the mid to late 1960's, Axel Leijonhufvud, along with Robert Clower, provided influential and provocative interpretations of Keynes's *General Theory*. Leijonhufvud's dissertation thesis, *On Keynesian Economics and the Economics of Keynes*, became an instantaneous success when published in 1968 and also the subject of intense debate and controversy, given its novel analysis of Keynes's most influential contribution.

After the initial enthusiasm and wide interest that Leijonhufvud's interpretation of Keynes aroused, during the 1970's, the younger generation of economists were soon caught up in the excitement created by the "rational expectations" revolution inspired by Robert Lucas. Interest in Keynes, and Keynesian economics began to wane.² By his own admission, Leijonhufvud (1993) "drifted out of the professional mainstream from the mid-1970's onward, as intertemporal optimisation became all the rage." As Leijonhufvud (1998) recalls, "macroeconomics seemed to have taken a turn very similar to the movies: more and more simple-minded plots but ever more mind-boggling special effects. One would like to look forward to a macroeconomics whose plots will give more insight into the human condition."

While the younger generation of new classical economists were everywhere pronouncing the end of the Keynesian era and embracing rational expectations and equilibrium theories of the business cycle, Leijonhufvud continued to argue that Keynesian economics has a future. Leijonhufvud (1992) suggests two main reasons for such optimism. First, the coordination problem is too important an issue to be kept indefinitely off economist's research agenda. "Will the market system 'automatically' coordinate economic activities? Always? Never? Sometimes very well, but sometimes pretty badly? If the latter, under what conditions, and with what institutional structures, will it do well or do badly?" Leijonhufvud regards these questions to be *the* central ones in macroeconomics. Second, Leijonhufvud believes that sooner or later economists must open up their theoretical structures to allow results from other behavioral sciences to be utilized in economic analysis. When that happens, "the 'unbounded rationality' postulate will have to go."³ From Leijonhufvud's (1993) perspective, it would seem that economists need to rediscover the "wild side" of macroeconomic behavior in order to begin the construction of "a not too rational macroeconomics."

In the late 1960's, Leijonhufvud's research focused on aggregate instability and unemployment. Although during the 1970's, Leijonhufvud's concern shifted to inflation, and later to problems relating to the transition economies, *the* major unifying theme throughout his work is a desire to identify the limits of an economy's capacity to coordinate economic activities. The "old" Keynesian model was relatively pessimistic, and the new classical model relatively optimistic, on the ability of a capitalist market system to smoothly coordinate economic activities. In contrast to these positions, Leijonhufvud offers the "corridor hypothesis" whereby an economy's homeostatic mechanisms work well within certain limits ("the corridor") but become progressively weaker outside those limits, hence his lifelong fascination with extremes of economic instability when an economy's capacity for self-correction may become severely impaired. Finally, and in contrast to the optimizing-equilibrium framework, Leijonhufvud emphasizes the important role that institutions play in governing market processes, thereby influencing aggregate economic activity.

In the interview that follows, I discuss with Professor Leijonhufvud a variety of issues relating to his work. The questions and answers are arranged within the following broad topic areas:

- background information;
- *On Keynesian Economics and the Economics of Keynes*;
- corridor hypothesis;
- costs of inflation;
- monetary regimes;
- economic history, growth, and transition;
- reflections on twentieth century macroeconomics.

The interview lasted about two and a half hours and was conducted in Axel's office at the Department of Economics (I had not shown any of the questions to Axel beforehand). A draft edited transcript was sent to Axel who made a few minor amendments, additions, and corrections.

1. BACKGROUND INFORMATION

Snowdon: How did you first become interested in economics?

Leijonhufvud: The one subject that had interested me throughout school during my teens was history. Although the history we studied was mainly military, diplomatic, and political history, I became quite conscious that the economic dimension was missing. That is where the interest began. I also had this desire to see the world and consequently wanted an occupation that would give me the opportunity to travel. So I started University at Lund working toward a political science degree and, like lots of other Swedes of my generation, I thought that I might go and work for the United Nations or the OECD, or some other international organization.

Snowdon: What attracted you to moving to the United States to continue your studies?

Leijonhufvud: I put in an application to the American-Scandinavian Foundation to study for a year in the United States and they chose to send me to the University of Pittsburgh. It turned out to be a wonderful year for me because I met a number of Americans who were personally very generous to me. In particular, James Witte was an influential teacher and just sitting in his seminar and talking with him got me interested, for the first time, in economic theory. He was a great, great fan of Franco Modigliani and, consequently, I became one too. I read all of Franco's famous papers and for the first time caught a glimpse of what economics could be. I was tremendously impressed by Modigliani's work.

Snowdon: What attracted you to move to Northwestern University?

Leijonhufvud: It all started when I went for a personal interview at MIT when I was trying to get into Graduate School. On my visit to MIT, I was walking down the corridor when I saw Franco Modigliani's name on a door. On an impulse, I knocked and went in and talked to him. We had a long conversation, at the end of which he told me that he had just accepted a job at Northwestern. He suggested that, if I applied to Northwestern, he would call Robert Strotz and advise him to give me a fellowship. So that's how I ended up at Northwestern.

Snowdon: Were there any other economists whose work and ideas influenced the direction that you moved in economics?

Leijonhufvud: As a graduate student I read widely. Herbert Simon at Carnegie-Mellon University was *the* icon at Pitt's Administrative Science Center at that time and so I obviously read his work. Also influential at Northwestern was Meyer Burstein, whose writings were even more eccentric than he was personally. He was very bright, with a flexible and playful mind that made him great to talk theory with. He possessed a genuine curiosity about the world and this appealed to me. But Modigliani was the great discovery for me at that age. His papers that I read then made up a body of analysis that was undoubtedly his best work.⁴ I was fortunate to receive much of it directly from him as a teacher at Northwestern. I remember that a lot of the other students had great difficulty with Modigliani's teaching style, which was not very systematic, not linear, textbook exposition. But I liked it immensely—it was his many digressions that taught me how he thought.

Snowdon: Your published work in the 1960's is very much linked to that of Bob Clower's reappraisal of Keynes. Was it at Northwestern that you met Bob Clower?

Leijonhufvud: I only got to meet him just before I was about to leave Northwestern when he had just returned from abroad and we talked for an hour or so. He did become one of my thesis supervisors and I went on and finished my PhD at Northwestern.

2. ON KEYNESIAN ECONOMICS AND THE ECONOMICS OF KEYNES

Snowdon: In the period 1967–1969, you burst upon the world of economics with what amounted to a broadside of influential publications. Those publications, particularly your book, gave you instant international recognition. How exactly did you become interested in the work of Keynes leading to your book *On Keynesian*



FIGURE 2. Axel giving a lecture to the Central Committee of the Communist Party in Kazakhstan in early 1991. In the chair is President Nazerbayev.

Economics and the Economics of Keynes, which was an immediate and enormous success?

Leijonhufvud: I first read Keynes's *General Theory* when I was still in Sweden but found it difficult and was not captivated by it. I must have re-read the *General*

Theory after hearing Modigliani's lectures, although he of course taught his own version of Keynes. But the way that I came to write my 1968 book was via a different route. Initially that work had nothing to do with Keynes. When I left Northwestern in 1963, at the end of two years, I had already written my thesis proposal. At that time I was interested in the very same question that is still alive today, that is: Why was the Great Depression so very different from ordinary recessions? Existing theory was not very convincing in explaining the difference. So, I invented—or so I thought—the debt-deflation hypothesis. After Northwestern, I got a fellowship to go to the Brookings Institution for a year (1963–1964) and began to talk about my debt-deflation idea with various people, including, for example, James Tobin. But nobody told me that the debt-deflation idea had been already been done by Irving Fisher because that part of his work had been totally forgotten. Remember Fisher ruined his reputation during the Great Depression by having previously claimed that the stock market was on a permanently high plateau. His book and paper on debt-deflation (and the paper is much better than the book) were his own *ex post* rationalization of why he had been wrong.⁵ So I followed the debt-deflation track for the better part of a year looking for flow-of-funds data that I needed (but that did not then exist).

Then, one day I was talking with David Meiselman about my thesis, and he said that he seemed to recall that Irving Fisher had already written something similar to my idea. He suggested that I check the early years of *Econometrica* to find Fisher's paper, and sure enough, there it was! But that turned out not to be a catastrophe for me because by then my question had changed from accounting for the historical uniqueness of the Great Depression to the question of why couldn't debt-deflation happen in any of the macro models that I had been taught or had read about? It could not happen because they all worked with consolidated balance sheets. In the Modigliani—Miller framework, for example, debts and claims just washed out. So, this got me thinking about information problems. Why does this kind of aggregation not hold true? So, there is a lot in my 1968 book on aggregation which readers, I suppose, often fail to see the purpose of. But it originates from this question of why don't debts and claims just wash out? In the process of all this, the original dissertation idea that I had started with fell away completely and I began to write papers on the different kinds of information problems that were hidden in macro models. By 1966, I had been at UCLA for two years and had accumulated lots of unfinished manuscripts relating to Keynes which formed the basis of my thesis. But I never thought about it as a thesis about what Keynes really meant. It was more a response to the state of macroeconomic theory as it was in the 1960's than a discussion of the theoretical debates that followed the publication of Keynes's *General Theory*. Dissatisfaction with the state of macroeconomic debate in the 1960's was also what lay behind Bob Clower's 1965 paper on the "Keynesian Counter-revolution."

Snowdon: So what is *On Keynesian Economics and the Economics of Keynes* all about?

Leijonhufvud: The book is essentially about the kind of information questions that do not occur in neoclassical Walrasian general equilibrium models. The issues

I was dealing with had to do with how information and communication flow in the system so as to enable a coordinated solution to be achieved. This is an issue that I keep coming back to in my work. In particular, I was interested in finding some answers to the question “When and why does the capitalist market system sometimes fail?” This involved putting two sacred cows on a collision course because the combination of microeconomics and macroeconomics that was taught in the 1960’s was totally incoherent. I felt passionately about it at the time. I also thought it was scandalous, and still do, that people perpetuate the falsehood that Keynes’s *General Theory* was all about rigid wages.

Snowdon: But surely the idea of rigid wages being the essence of Keynes is very much associated with Modigliani and his famous 1944 *Econometrica* paper? In response to a question about this paper that was put to Modigliani in October 1997, he replied “I feel I was absolutely right in saying that the essence of Keynesian economics is wage rigidity.”⁶ You have already said how much you admire Modigliani’s contribution to economics, but you clearly disagree with him on this important point.

Leijonhufvud: Yes, and this was a great embarrassment to me because I have so much respect and affection for Franco Modigliani (I felt the same about John Hicks whom I met later). My conclusion was indeed that Modigliani’s 1944 paper was fundamentally wrong. Every generation of young researchers is supposed to challenge the older generation, but I felt very badly about it. My book is not quite honest about this because it avoids challenging Modigliani directly.

Snowdon: Did you ever discuss this problem with Modigliani after your book was published?

Leijonhufvud: No. I was very reticent about contacting him. When he was awarded the Nobel Prize in 1985 I wrote him a warm congratulatory letter telling him how important his teaching had been to me and he wrote back a grateful note, saying how he did not realize how I had felt about it. Later, I hosted him when he came out to UCLA. When I teach macroeconomics in a historical fashion to students, I of course draw attention to Modigliani’s 1944 paper. But it is that whole postwar Keynesian literature and the way in which John Hicks’s IS-LM model was used that is to blame for the distorted view of Keynes that emerged during the neoclassical synthesis. If you want to see what a good Keynesian IS-LM model should look like, read the 1999 paper by Ingo Baren.

Snowdon: You are obviously very critical of the postwar IS-LM literature and have argued on numerous occasions that it is not a good vehicle for assessing Keynes’s contribution as well as highlighting some of its logical inconsistencies. And yet, it still forms the core of most undergraduate macroeconomic textbooks. Tobin once described the IS-LM model as “the tool of first resort. If you are faced with a problem of interpretation of the economy—policy or events—probably the first thing you can do is to try to see how to look at it in these (IS-LM) terms.”⁷ How do you account for the longevity of the IS-LM model?

Leijonhufvud: If certain institutional conditions—such as fiscal balance over the medium run and a correspondingly stable monetary regime—can be taken for granted, IS-LM will give you qualitatively the right answers to a set of important questions involving the system’s short-term responses to various shocks or policy measures. Then, IS-LM is a simple, handy way of thinking about—and, of course, teaching—how the economy responds. The trouble is, first, that the implicit institutional assumptions do not always hold and, second, even when they do, there are some types of disturbances for which routine use of it will not grind out the right answer. Learning IS-LM is easy. Learning when it is safe and not safe to use it requires a lot more sophistication.

Not all versions of IS-LM deserve the same degree of (qualified) respect. Once people had (wrongly) concluded that Keynesian economics was all about rigid money wages, all that was needed was an utterly primitive IS-LM “theory of nominal income” with virtually no price-theoretical content. Later on, this proved to be only too easy a target for the new classicals.

Snowdon: By distorting the interpretation of Keynes, did the neoclassical synthesis distort the subsequent path taken by macroeconomics?

Leijonhufvud: I stand by my position that the neoclassical synthesis is utterly incorrect in its interpretation of Keynes. The important error that was built into that interpretation has had far-reaching implications. To really understand any problem, you need to know from where a question originates. In a paper (unpublished) that I gave at the History of Economics Society Meetings at Harvard, many years ago entitled “The Uses of the Past,” I talk about the history of economics as a decision tree. When I teach students about the Keynes-and-the-classics controversy, I try to explain to them what the quarrel was about and what the choice was that the mainstream made. Errors made in that decision tree sometimes become apparent only a long time afterward. That’s exactly why those who are working at the frontier of the subject should know some history of economic thought. This is a different reason than just wanting to know the history of the subject for antiquarian interest. This view also suggests that economics itself exhibits very strong path dependence. So if you take the wrong path, the errors can be with you for a long time.

The neoclassical synthesis is a good example of this because the confusion caused by thinking that it was sticky nominal wages after all that lay at the heart of the unemployment problem led to an impoverishment of the models that economists were working with. From that point onward, people looked at the IS-LM framework as simply a theory of nominal income. All you need, to talk about short-run unemployment, is nominal income and the inherited sticky wage. After 1958 the Phillips curve was grafted on to complement the model because the original price-theoretic content had been dropped. But the theoretical and empirical foundations of the Phillips curve were weak from the start. Years later, Milton Friedman published his theory of nominal income and everybody said “Oh, that’s what we already teach!” Keynesians had set themselves up for Friedman’s attack.

By the time we got to about the fifth round of the Keynesian-Monetarist controversy, the Keynesian side had forgotten everything about the savings-investment

side of Keynes and the problem of intertemporal coordination. So, Friedman in that very influential 1970 *Journal of Political Economy* paper is saying two things. First, money should be neutral, so it does not make sense to think you can solve a social problem like unemployment by printing more money. The second point Friedman makes, however, is that flexible wages will suffice to guarantee that the system always converges on a particular rate of unemployment, namely, the natural rate. From a Keynesian or Keynes standpoint, this second argument is not acceptable; it is a mistake. What would a 1940's Keynesian have said about the idea of a natural rate of unemployment or output? Joan Robinson would have said "there you go again with the Treasury view." Even Erik Lundberg in Sweden would have said that flexibility of wages would carry you to the natural rate if, and only if, it so happens that savings equals investment at that rate of output. But if savings exceed investment at that rate of output, then labor market flexibility will not carry you to the natural rate. The doctrine of the natural rate of unemployment implicitly assumes that the economic system is always in intertemporal equilibrium. The trouble with the rigid wages interpretation of Keynesian economics is that it forgets about this intertemporal coordination problem. Certainly, during the Keynesian-Monetarist controversy, it was completely out of mind. Unfortunately, this elementary error has determined the path that macroeconomics has taken ever since. Milton Friedman won the Phillips curve controversy. Robert Lucas then took over and tried to tidy up Friedman's theory but argued that it is only unanticipated money that matters for nonneutrality. Then, everybody saw that money cannot be unanticipated because information on the money supply is easily available. So, in the 1980's, on the new classical side, we end up with nonmonetary real-business-cycle theory. But, the Great Depression remains a riddle, and especially so for the real-business-cycle story.

Snowdon: In your 1968 book, you look at Keynes from a Walrasian perspective but chose not to follow the fashion in the neo-Walrasian literature to use the language of mathematics. Your book contains only a couple of diagrams and no formal mathematical analysis. Was this a deliberate methodological decision on your part?

Leijonhufvud: There are two reasons for the style of that book. Number one, I had followed the linguistic line in school so that my math was relatively weak. But I had enough to understand perfectly well what I was being taught in Graduate School and was able to pass my examinations like everybody else. When I started with my debt-deflation idea, my original intention was to conduct that research within a Modigliani-Patinkin type of framework. So, I reasoned in terms of models, albeit primitive ones. Then I began to realize that it could not possibly work because there were fundamental problems that would not lend themselves to the kind of modeling that economists did at the time. Second, I thought differently about information problems than economists such as George Stigler who was awarded the Nobel (Memorial) Prize in 1982 for his contributions in that area. One of Stigler's witty remarks was that he had taught economists that information is a good just like potatoes. But that approach to information in economic theory has nothing to do with the way that information is transmitted in the process of moving



FIGURE 3. Axel at Kings College, Cambridge University, for the Keynes Centennial Conference, July 1983. Seated from left to right: Axel Leijonhufvud, Robert Solow, Paul Samuelson, and Luigi Pasinetti; standing center, David Worswick.

the system to an equilibrium. I was more concerned with conceptual issues that did not particularly lend themselves to modeling in a mathematical way and I did not have the mathematical equipment to move beyond what the better people were doing at that time.

Snowdon: One of the notable features of the postwar literature on Keynes was the variety of interpretations of the *General Theory* that gradually emerged. In Coddington's (1983) interpretation both you and Robert Clower are classified as "reconstituted reductionists." Do you attach any school of thought label to yourself? Do you see yourself as some kind of Leijonhufvudian Keynesian?

Leijonhufvud: In one sense the groupings have disappeared because not many economists are interested anymore. One of my weaknesses is that I am psychologically averse to running with some herd, or even breeding a herd of my own. Years ago, I used to have some students at UCLA who wanted to do "Leijonhufvudian economics" but I was always a bit suspicious of them. I always did better with more independently minded students. But I do remain very much influenced by this Keynesian business. If I was to describe my interests, to identify the questions that concern me, then almost everything that I have written has to do with the key question: "What are the limits to the self-organizing, self-coordinating capabilities of the market system?" And the reason for not being in any particular group has to do with the fact that the economics profession tends to split into groups that vary endogenously in size over time. We have people who will say that today's view is that the private sector works perfectly well except when the government messes things up. We used to have the opposing view, which argued that the private sector

is inherently defective and cannot coordinate activities except with the help of the visible hand of government. I think that both of those views are quite dangerous and in many ways not a little stupid as a description of the world we live in. So I am instinctively averse to both those views apart from having rational reasons to reject both positions.

Snowdon: Macroeconomics has undergone some dramatic changes during your career as an economist. Have you changed your mind on any issues relating to Keynes?

Leijonhufvud: I have changed my view on some things. I used to think that those economists who emphasized the hydraulic part of Keynes's analysis had bastardized the *General Theory* because, to me, they seemed to have missed the entire price-theoretical content. In retrospect, I think that I was a little bit too hard on hydraulic Keynesians in the sense that Keynes was trying to tackle a kind of problem that I don't think anyone today could deal with very cleanly. Keynes is not credited with having much of a micro theory, but if you read his *Treatise on Money*, you will see that he was looking for an explanation of why relative prices are going wrong in such a way that you get unemployment. That was missing in the hydraulic interpretation. But with cash-constrained agents, as Keynes had, it is also crucial to keep track of where the money flows in the system, that is to say, the hydraulics. At that time, in the 1930's, to theorize simultaneously about what goes wrong with relative prices (real interest rates, the demand price of capital goods versus the demand price of consumer goods) and keeping track of the hydraulics at the same time, was a very formidable task from an analytical standpoint. I am not sure that even someone as talented and innovative as Ed Prescott could do it now. Keynes was analyzing the state of the system at t to explain how it would transform itself into the state at $t + 1$. In his system, it was vital to trace the money flows because you needed to know who ended up with the money and who was income- or liquidity-constrained.

Snowdon: How would you now appraise Keynes's *General Theory*?

Leijonhufvud: In my 1968 book, I tried to explain Keynes's theory by relating it to what I have recently called the "Modern" tradition, or in more familiar language the neo-Walrasian tradition.⁸ In other words, Keynes was portrayed as a theorist struggling against the Walrasian branch of the classical tradition. The way I would talk about him today is to note that he was a price-theoretical Marshallian. He was brought up in the Marshallian branch of the classical tradition and can really be regarded as the last of the great classical theorists. Keynes was therefore trying to escape from the Marshallian classical tradition, and his *General Theory* is properly understood as a generalization of classical theory. Classical/neoclassical economics in those days was adaptive in that when you talked about the optimality conditions of an agent as an equilibrium, you meant that there was a *process* at work. The agents in Marshall's world learn as they go, they are adaptive and equilibria are the positions agents arrive at by trial and error. Marshall's "biological" approach viewed the economy as a highly complex system, where innumerable agents constantly adjust by trial and error. His agents obey simple feedback

rules and, consequently, do not optimize *ex ante* but follow what I like to call Marshall's *Laws of Motion*. If your demand price exceeds the market price, buy more . . . if the market price exceeds your marginal cost, produce more . . . , and so on. These are simple laws of motion that supposedly bring about short-run equilibrium. Marshall, Pigou, and others took it for granted that if you had a system where everybody obeyed the laws of motion, and were not prevented from doing so by some external force, then it had to be the case that the system would move to the full employment general equilibrium. Keynes found out that this is not necessarily so. If effective demand failures occur, the system will *not* go to full employment. And that's Keynes's real claim to his theory being "general" rather than a "special case" of classical theory. *It is the dynamics that are more general.*

Now that is a very important discovery because it tells you that there are limits to the self-regulating capabilities of market systems and that you have to be careful that you do not transcend those limits. If you do go outside those limits you have to figure out how to get back. Before Keynes, that was not properly understood. In Marshall's world, if the system does not return to equilibrium, then there is a presumption that somebody is not obeying the laws of motion.

Snowdon: What are the weaknesses of Marshall's argument?

Leijonhufvud: Keynes has some very strong statements in the papers he wrote between the *Treatise on Money* and the *General Theory* stressing the dangers of debt–deflation: If wages were indeed flexible, it would destroy the international financial structure. But this argument is hardly present in the *General Theory*. You need to be aware of these earlier statements to even see it in the *General Theory*. Hyman Minsky deserves a lot of credit for steadfastly insisting that this is an integral part of Keynes's thinking. But the *General Theory* by itself, I would argue, is generally wrong about what happened in the Great Depression. The reason for this lies in an analytical error, namely Keynes's liquidity preference theory of interest-rate determination. That theory says that the probability of the interest rate automatically coordinating saving and investment is zero. So, although the capitalist system has been a product of evolution, Keynes thought he had discovered this functional deficiency that is totally critical, namely that the interest rate cannot coordinate economic activities over time and therefore it has to be done by discretionary policy. I think that is simply untrue, it is an exaggeration. I think it is true that we can get into situations where the system can fail and where just waiting for it to recover spontaneously would be to court disaster. Keynes was right about those situations. But Joan Robinson and company stuck to the hard-line Keynesian position about the inherent failures of the capitalist system and if you get doctrinaire like that, and you are wrong, then sooner or later your position will be shattered and the table will be swept clean of your ideas.

Snowdon: If you had a time machine and could meet Keynes, what would you talk about?

Leijonhufvud: If I could talk to him when he was in his prime I would ask him about what he would do about the international monetary system today. I am sure his thinking would have moved far beyond Bretton Woods. I am sure he would

have been intensely interested in what has happened in East Asia, Japan, Russia, and Argentina. In his own day, he was very much concerned about the position of his own country in the international system. In 1919, Keynes recognized that a decent treatment of a defeated Germany after World War I was important to preserving a fragile social order in Europe. I am sure that if he was a modern-day observer of globalization, he would have things to say about how historically, in every other generation, a revolution of the financial system has carried us into new, uncharted territory where we had to learn anew how to stabilize the system. I think Keynes would have recognized the importance of figuring out an intelligent way to stabilize the international system. And he would remain concerned, perhaps fearful even, over the fragility of social order within and among countries—a perspective alien to the American temperament but more natural to Europeans. You cannot simply shrug your shoulders at problems such as those faced by Argentina today.

Snowdon: In your Keynes and the Classics (1969) lectures, given shortly before the award of the first Nobel Prize in economics, you posed the following question to the audience: “If John Maynard Keynes were alive today, whom would you nominate for the Prize?” Let me ask you the same question and, specifically, do you think he would have been awarded the first prize and, secondly, what would the award have been for?

Leijonhufvud: In 1969, he would have been awarded the first prize for sure. The citation would have talked about how Keynes taught us how to manage the economy. But somewhere in the citation there would probably have been a little Swedish poison pill. Gunnar Myrdal would have been on the awarding committee and he would have repeated his famous line about that “charming British unnecessary originality.”

Snowdon: In your essay on rational expectations [Leijonhufvud (1983)], you note that “To the younger generation of economists, Keynesian economics, all of it, not just Keynes himself, belongs to the history of economic thought.” Robert Lucas would certainly not advise his students to read Keynes’s *General Theory*.⁹ Do you still recommend your students to read Keynes?

Leijonhufvud: At UCLA last Autumn (2001) I had a quite brilliant graduate student from Beijing. He had sailed through the first-year theory course with excellent grades and he had all the mathematical equipment to master the modern techniques used in economics. He found the dynamic stochastic general equilibrium models he was taught to be easy but lacking any economic content. I was teaching a course that I called “One Hundred Years of Macroeconomics.” So, after attending some lectures, he decided to read Keynes. As a result, he became inflamed with enthusiasm. I am not sure that he clearly saw Keynes’s weaknesses, analytical errors, and errors of emphasis, that contributed a lot to the undermining of Keynesianism that came later. But what this student could see was that Keynes was someone deeply concerned with the great issues of his time, and attempting to grapple with these problems honestly and urgently. This young man is so different from most of today’s graduate students and economists.

3. THE CORRIDOR HYPOTHESIS

Snowdon: In your “Effective Demand Failures” paper (1973), you introduced the idea that market economies operate reasonably well within certain limits that you refer to as “the corridor.” However, outside the corridor, equilibrating tendencies become weaker “as the system becomes increasingly subject to effective demand failures.” Recently, Paul Krugman (1999) has been reminding economists about the dangers of “Depression Economics” and the potential for a liquidity trap. Do you think that during the 1990’s Japan slipped outside the corridor?

Leijonhufvud: In my view, Japan is right on the line or maybe a little bit beyond it. But I would argue that the Japanese situation has not been entirely Keynesian and Paul Krugman is not entirely correct if he thinks that the Japanese situation is a liquidity trap in the 1950’s–1960’s Keynesian textbook sense. When I talked about situations where the system is not recovering rapidly by itself due to effective demand failures, there are basically two of them that we can find in the *General Theory*. First, a fresh act of saving is not an effective demand for future goods. Second, the wishes of the unemployed for consumer goods do not constitute an effective demand. But there is a third effective demand failure that can be very important. This is when the financial system is in a state where for most entrepreneurs it is not possible to exert an effective demand for today’s factors of production by offering future goods. That is, it is not possible to make a deal by saying: “I have this investment project that will pay off in the future and I want to trade that prospect for the factors of production today necessary to produce those future goods.” And that’s where we end up if the financial system is totally clogged with bad loans. That has been and still is the Japanese situation. If the problem was the conventional Keynesian one (of consumers being cash-constrained), then there is a rationale for public works. But that was never the Japanese problem. Their problem was that they did not move directly to clean up the banking system after the collapse of the real estate and stock market bubble. They did engage in conventional Keynesian policies but all that accomplished was to run up a large public debt which is now constraining their policy options.

Snowdon: What is it that draws you to this idea of the corridor?

Leijonhufvud: I have always had a fascination with extremes of monetary instability. I have spent years studying high inflations and find it extremely interesting, also from a purely theoretical point of view, to see how thoroughly coordination is disrupted. The majority of economists hold the view that the system works exceedingly well but I prefer to think of economies as complex dynamical systems. Complex in two senses. First, the system is open, has a large number of agents, a large number of distinctive activities, and the knowledge of actors is necessarily only local, never global. Second, there are nonlinear adjustment dynamics, which in normal, tranquil periods, are very nearly linear, but in extreme situations may go chaotic. All self-regulating systems we know of, whether natural (ecologies) or manmade (automatic pilots) have *bounded* homeostatic capabilities. Surely, that is

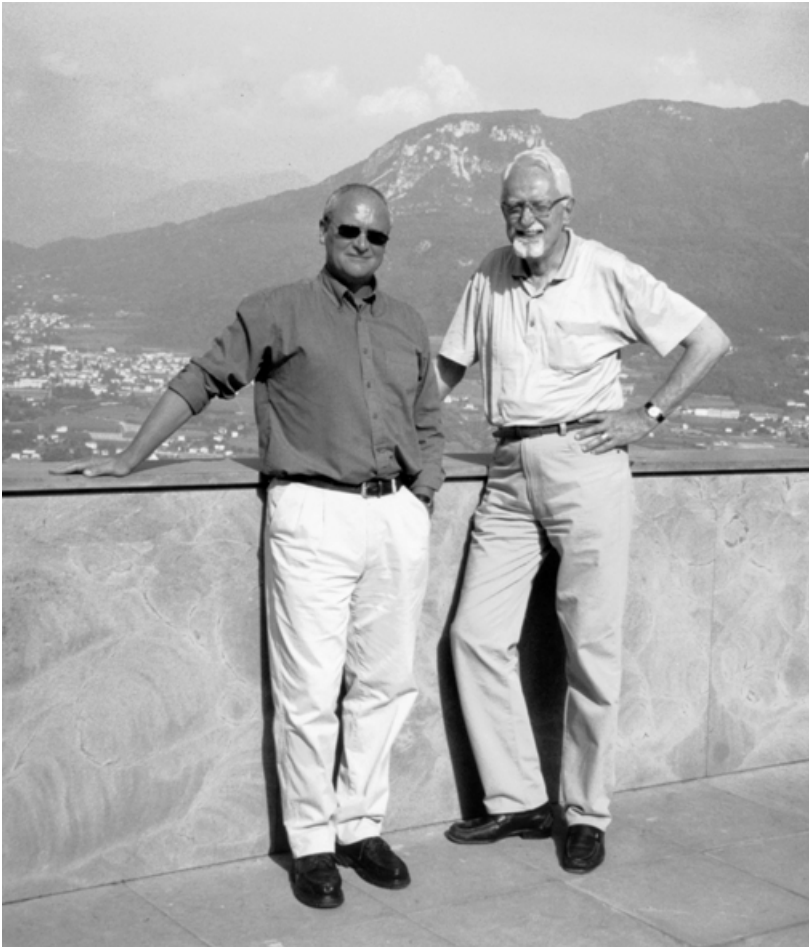


FIGURE 4. Axel and Brian Snowdon taken on May 16, 2002, at the University of Trento Conference Center, in the mountains above Trento.

also true of market economies. One studies situations of extreme instability to learn where the boundaries lie and how the system behaves when they are transgressed.

Snowdon: What about the concept of “involuntary unemployment.” Here is a central “Keynesian” idea that appears to have been extinguished by Lucas and others from the mainstream macroeconomics literature even though staunch Keynesians like Alan Blinder, Robert Solow, and James Tobin always defended the concept. Should we perhaps think of involuntary unemployment as unemployment that occurs outside the corridor?

Leijonhufvud: That is roughly right. In Keynes’s sense, involuntary unemployment occurs when, in the presence of effective demand failures, Marshall’s “Laws

of Motion” carry the system into a short-run equilibrium where unemployment exceeds what is today called the “natural rate.” If involuntary unemployment of this sort occurs on a large scale, the system would be outside “the corridor” in the sense that endogenous recovery would tend to be very slow at best. To Lucas and other “Moderns,” involuntariness is utter nonsense in any properly formulated choice-theoretical context. A number of Keynesians or neo-Keynesians—add, for example, Frank Hahn and Joe Stiglitz to your list—protest, arguing that for some class or other of workers’ opportunity sets, the use of the term makes common sense. Neither side in this exchange is using “involuntary unemployment” in a sense that has the remotest connection to what Keynes was talking about. But scholarly standards being what they are in economics nowadays, neither do they give a damn.

4. THE COSTS OF INFLATION

Snowdon: Although we tend to associate Keynes with depressions and unemployment, he also worried about inflation. In his famous attack on the Versailles peace treaty, *The Economic Consequences of the Peace* (1919), he noted that Lenin was once said to have declared that the best way to destroy the capitalist system is to debauch the currency. What was the background that led you to write papers stressing the costs of inflation in the mid-1970’s?

Leijonhufvud: By the way, the Lenin quote is untraceable—nobody has been able to find it. In the early 1970’s one of my good friends, Ned Phelps (1972), wrote a book about inflation that I got upset about. I greatly admire his work but his analysis of the costs of inflation I regarded as methodologically unsound. And there were many others writing at that time who took a similar line to Phelps. I remember that in 1975 I had accepted an invitation to go to an International Economics Association Conference in S’Agaro, Spain. John Hicks was one of those organizing the program. He asked me to come and at that time, in the mid-1970’s, I thought that economists did not know what they were talking about when discussing the costs of inflation. There was a total lack of understanding on this issue. The general doctrine that the social costs of inflation come down to the shoe leather costs is ridiculous. If inflation is that trivial, then let it rip. . . . who cares? So this is an area where economic theory remains totally incoherent and I think this is an intellectual scandal. But the paper that I wrote for the S’Agaro conference is one of my worst papers because it was written in a bad temper. Later, I had this brilliant Argentinean student, Daniel Heymann, and eventually we decided to write something together about high inflation and how inflation impacts on growth and real variables. This is not something that can be treated by fatuous statements that money must be nonneutral during high inflations. You need to understand why the price mechanism, the market system, does not work in the same way during high inflations as in normal times. You cannot understand this problem by writing down a nonmonetary general equilibrium model, grafting money onto it, and then play around with different rates of inflation tax.

Snowdon: Do you share Hayek's view that inflation damages the efficient functioning of the price mechanism as the inflationary noise created by movement of the general price level drowns out the relative price signals thereby damaging economic efficiency?

Leijonhufvud: Yes, that is OK as far as it goes (not very far!). But Daniel Heymann and I (1995) have many more quite specific things to say. For example, we argue that many of the principal-agent problems in the economy become impossible of solution because nominal auditing and bookkeeping are the only methods invented for principals to hold agents to account in various situations. This is most obvious in the government sector itself because the national budget for a year becomes meaningless when money 12 months hence is of totally unknown purchasing power. In this situation, you cannot hold government departments responsible for not adhering to their budgets. You have lost all control. It is not just a case of the private sector not being able to predict what the monetary authorities are going to do, the monetary authorities themselves have no idea what the rate of money creation will be next month because of constantly shifting, intense political pressures.

The literature stresses the holding cost of money over the next month and the predictability of prices for maybe the next year. But it forgets totally about the maturity structure of the financial system. The financial system does not work at all the same under inflationary conditions as it does under price stability. In the U.S. during the 1970's, when the rate of inflation never exceeded 15%, the market for 30-year bonds disappeared. Higher rates of inflation will destroy markets for much shorter maturities. Daniel Heymann and I argue that high inflation destroys institutional arrangements and routines that under stable conditions enable agents to come close to optimizing.

5. MONETARY REGIMES

Snowdon: In recent years, many countries have adopted a monetary regime of inflation targeting. How do you view this development?

Leijonhufvud: At the S'Agaro conference I was regarded as some kind of Keynesian until I gave my paper on the costs of inflation. After giving that paper, I became known as an anti-inflation hawk when it was not at all fashionable to be one. I even overheard some younger people say "Oh, it makes sense, he comes from UCLA and they are worse than Chicago!" But more recently I have found myself at the opposite side of what seems now to be the consensus. At a conference in Frankfurt a couple of years ago, for example, several papers were given on inflation targeting. I pointed out that Japanese monetary policy during the 1980's looked like inflation targeting and yet they let this enormous disaster grow right under their noses. So, inflation targeting cannot be the end-all of monetary policy.

Snowdon: Do you think that monetary policy is best conducted by an independent central bank?

Leijonhufvud: It is inflation targeting—inflation as the *only* target—that makes true central bank independence feasible. Monetary policy can then be turned over to technocrats. Add more goals and any democracy must, in my view, retain political control over the central bank since the trade-offs among goals are certain to have distributive consequences. This issue has been much fudged in recent years. It is not just coincidence that central bank independence and inflation targeting have become simultaneous fads. Step back from inflation targeting—as I believe we sooner or later must—and the independence issue will have to be reevaluated.

Snowdon: I was interested to see that the very last sentence in your 1968 book reads: “The upshot of all this is that the monetary authority ought itself to take the Long View and not use its powers in efforts to counter every temporary change in ‘business conditions’ and employment.” Where do you now stand on the rules versus discretion debate on the conduct of stabilization policy?

Leijonhufvud: At one extreme the people who favor discretion ask: Why should we make rules today to constrain the behavior of central bankers in the future when those people in the future will know much better than we do what the situation is and what is the best thing to do? On the other hand, consider what the absence of rules would do in other contexts, such as sports, where we always rely on rules. So, I am in the middle on this issue. To discuss how restrictive the rules ought to be or how much latitude to leave to discretion, I always want to know the particulars and the particulars have to do with specific historical situations.

6. ECONOMIC HISTORY, GROWTH, AND TRANSITION

Snowdon: In your 1973 paper, “Life among the Econ,” you describe economists as a “quarrelsome race” with a “social structure” exhibiting two main dimensions, namely, caste and status. The Math-Econ represent the “priestly caste” and have the highest status in the profession. You also note that “Among the younger generations, it is now rare to find an individual with any conception of the history of the Econ.” But what about economic historians. What is their status within the Econ tribe?

Leijonhufvud: For 20 years, I taught a course in European Economic History, so clearly I find that economic history has very useful things to say for economists. So, leaving out the study of economic history in the education of economists may be even worse than leaving out the study of the history of economic doctrines, although a good theorist should know what was previously said and not just repeat the false myths about what our predecessors said. I am a consumer of some econometrics but I am deeply, deeply, distrustful of replacing history with time-series statistics or, for that matter, replacing the study of comparative systems with these awful cross-country regressions. I have written a couple of papers on economic history: One is called “Capitalism and the Factory System” (1986) and the others are basically spin-offs from that. These papers look at the question “How did the medieval economy transform itself into capitalism?” An important element is how more and more activities became the object of private property. Also, my visits to

Kazakhstan in 1991 started me thinking about the problems of transition. A real understanding of such issues is impossible without first having a firm foundation in the history of the economies that are undergoing these transitions.

Snowdon: Should modern growth theorists read more economic history?

Leijonhufvud: I share the view that economic history can contribute a great deal to our understanding of economic growth. It seems to me that once the Cobb-Douglas production function was invented, no *mainstream* economist has since studied production in the way that Alfred Marshall (1919) did in his *Industry and Trade*. Taking derivatives of production functions tells you nothing about production. So, if you are really serious about wanting to understand the modern world, and how we got to where we are, you should read people like Nathan Rosenberg (1986), Eric Jones (1988), and David Landes (1998). I do think that, to some extent, the revival of interest in growth theory will lead people back to economic history and hopefully many will get hooked.

Snowdon: In several papers, you have emphasized the importance of understanding the full implications of the “Smithian” division of labor and the phenomenon of increasing returns in production. This is something that both Keynes and most Keynesians have neglected.¹⁰ Why is this important for the macroeconomics agenda?

Leijonhufvud: I firmly believe that there are increasing returns everywhere arising from the entire network of cooperation in production and the division of labor. Unfortunately, mainstream macroeconomics insists on using a production theory that has the Ricardian farm as the representative unit of production. So, we are seen to always live in a world of constant returns to scale and diminishing returns to the variable factor. I prefer to think of production in terms of Smithian factories—price-setting firms that operate under conditions of increasing returns to scale. The productivity of labor increases with the division of labor—and the division of labor depends on the extent of the market. This has all sorts of interesting implications. Okun’s Law is one. Another example relates to real-business-cycle theory and the procyclical pattern of productivity that we observe in the Solow residual. I start from the notion that the division of labor gives us increasing returns and we therefore live in a system where it is impossible to scale back production and scale down inputs in the same way. So, the system is always inefficient at low levels of activity but very productive at high levels of activity. Therefore, any cyclical theory should imply procyclical productivity. But in this Smithian world, the line of causation is from increases in the level of aggregate activity to changes in productivity and not as the real-business-cycle theorists argue from productivity to aggregate activity. The fact that there is no pattern in real wages is consistent with this although it is a problem for standard theory. When people start from the Solow growth model they find that the residual is 60% of it. This very important fact ought to make us rethink the fundamentals of production theory rather than taking the “Truth” of the neoclassical production function as established so that what remains is to tinker with it to “save the phenomena,” for example, by modifying the measured labor input to take into account years of schooling and so on and so forth.

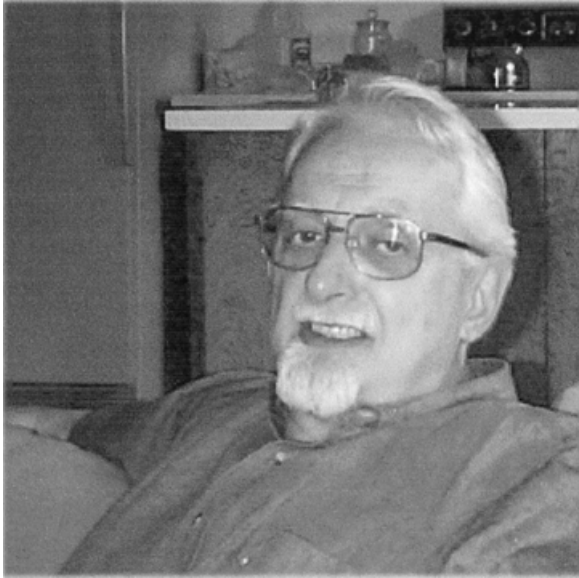


FIGURE 5. Axel from his Web page at Trento.

Snowdon: Are you in any way sympathetic to the view that business-cycle fluctuations, of the magnitude experienced in the past 50 years, do not impose significant costs on society and therefore it is more important for economists to worry about increasing their knowledge about the underlying causes of economic growth?

Leijonhufvud: People like Bob Lucas stress the importance of compound interest to show how growth is a more important issue for economists than business cycles. Keynes was well aware of the power of compound interest. In his famous essay, “The Economic Prospects of Our Grandchildren,” you will find that Keynes gives the same message entirely.¹¹ So, Lucas is not the first to say this. But unlike many modern macroeconomists, *Keynes had a definite sense of the fragility of the social order*. Keynes’s observation was that *if we manage to keep growth going, this is how our grandchildren will live, but there are all sorts of ways in which the system can be derailed, in which case the outcome will be very different*. A derailment of the system as a whole is not an issue that is in the mental setup of most of today’s macroeconomists. They have little interest in short-run issues. So, you can see I am a little alienated from the “Moderns” because my life-long interest has been in establishing under what conditions the system can do well and under what circumstances it does poorly.

Snowdon: You mentioned earlier that you are very interested in the economic problems facing transition economies. As an observer of the transition process, what is your overall assessment of the progress so far?

Leijonhufvud: There is obviously a yawning gap between the actual state of the transitions in, say, 2000 and the optimistic expectations widely held in 1990. In accounting for this gap, some people will point to the utter naiveté of those initial expectations while others will focus on the mistakes that were made. The point to make for economists, I believe, is that whichever position one takes on that, the implication is that we did not (and do not) understand the world we live in very well. So, we have much to learn but it does not seem to me that the profession as a whole is jumping at the opportunity.

One more point perhaps. Appraisals of transitions tend to run in terms of before-and-after comparisons of GDP and mortality statistics. Not enough attention has been given to the legitimacy of the distributions of income and wealth resulting from the privatizations. There is no coherent set of values that lends rhyme or reason to the distribution of wealth that the transition has wrought in Russia. This, I think, spells trouble down the road.

7. REFLECTIONS ON TWENTIETH CENTURY MACROECONOMICS

Snowdon: You have been a professional economist for some 40 years and have therefore witnessed the many profound changes that have taken place in macroeconomic analysis. Taking a broad view, do you see considerable progress and are you optimistic about the future of macroeconomics?

Leijonhufvud: I am Swedish, so I am never optimistic. Looking back I do believe that we know a lot more. I am not among those who complain about the increasing use of mathematics in economics because when you read the literature, from say the 1920's and 1930's, you can see economists making many errors that can be easily straightened out with a little bit of mathematics. We also have a lot more quantitative information compared to the past, such as the Summers and Heston data. However, I am really quite suspicious of the data itself, not just the uses to which it is put by people like Robert Barro and Xavier Sala-i-Martin.

I think the legacy of Ed Prescott's work will be in terms of the analytical machinery available to technically minded economists although those techniques are not always appropriate and you cannot always apply them. In particular, you cannot meaningfully run these dynamic stochastic general equilibrium models across the great catastrophes of history and hope for enlightenment.¹² To the extent that I understand these high-tech developments, I am impressed. These people are extending the tool box available to economists. However, there are important conceptual problems of the real world that are neglected. So, there is progress but there have also been cognitive losses of very considerable significance.

Snowdon: You have surveyed the postwar macroeconomic debates using your "Swedish Flag" taxonomy [Leijonhufvud (1992)]. This taxonomy classifies aggregate fluctuations in a 2×2 matrix distinguishing between impulse and propagation mechanisms that can be either real or nominal. Now we have nominal frictions being added to dynamic stochastic general equilibrium models and

Goodfriend and King (1997) talking about a “New Neoclassical Synthesis.” How do you view these developments?

Leijonhufvud: Bob Clower used to like quoting Dennis Robertson’s famous analogy of the hunted hare: “highbrow opinion is like a hunted hare; if you stand in the same place, or nearly the same place, it can be relied upon to come round to you in a circle.” I modified this image to one of a spiral staircase: If you stand still in the same place, you find that when highbrow opinion comes around to where you’re standing, it is all above your head! The theory is old and tired, but the technology of modeling is new and supposedly invigorating. Note that the new Keynesians basically started out in Friedman’s nominal shocks, nominal propagation quadrant of the Flag. Conceptually, the recent trend you mention, the so-called new neoclassical synthesis, reminds one of the discussions that took place in the 1920’s and early 1930’s. It’s *classical economics with frictions*. Again we have come full circle. But few economists these days want to think or talk about the corridor problem.

Snowdon: As you recognize in your 1983 *Journal of Economic Literature* review of *Studies in Business Cycle Theory*, during the 1970’s, Robert Lucas “defined the issues” and “reformed the methodology of contemporary macroeconomic research.” However, in several papers, you have been critical of the rational expectations hypothesis and the assumption of unbounded rationality that pervades modern mainstream macroeconomics. Axel Leijonhufvud’s macroeconomic world is one populated by agents who are “believably simple people” facing “incredible complex situations.” How is your involvement with the Computable and Experimental Economics Projects at UCLA and Trento linked to your rejection of the intertemporal optimization approach and your interest in exploring the properties of complex dynamic systems?

Leijonhufvud: Rational expectations are necessary in order to extend the optimization paradigm to intertemporal behavior. Intertemporal equilibrium becomes an inescapable consequence. For 30 years, I’ve been convinced that this is a conceptual cul-de-sac for macroeconomics. By now, we’ve explored it enough and learned, I suppose, pretty much all there is to learn in it. It is high time that we extricate ourselves from it and get on with the work that has to be done sooner or later if economics is to be a serious science.

I think we are seeing the beginning of this in the recent literature on *learning* in macroeconomics [I would single out Evans and Honkapohja (2001)] and especially learning in repeated games. Recent learning algorithms are, of course, a lot more sophisticated than the very simple (and occasionally stupid) adaptive expectations schemes that were ruled out of court in the early days of rational expectations—but learning *is* adaptation nonetheless. It may be “procedurally rational,” but it is not going to be “substantively rational” in the sense of Herb Simon. This stuff is encouraging from my point of view, although these developments have a long way to go before they get back to the issues raised by Keynes, namely, given the institutional structure of the macroeconomy, will it always produce the market signals that will guide all the error-learning adaptation in the direction of general equilibrium?

Snowdon: What would a future economics *not* built on optimization and equilibrium look like?

Leijonhufvud: A central component of it will have to be a behavioral economics that studies, in particular, how people cope in complex environments despite their cognitive limitations. Experimental economics is then bound to grow steadily more important to us. I also believe that institutional economics has to be approached from this cognitive perspective, that is to say, that economic institutions have to be seen as structured so as to simplify the decision problems of boundedly rational agents. Economists don't know much about how different kinds of markets actually work. The empirical study of market processes has to be given more importance. The modeling of such processes will be done by computer simulation. Similarly, agent-based computer modeling is the only feasible way to build macrostructures from experimental and behavioral microeconomics so that such complex dynamic systems can be investigated in a systematic fashion. Such a reorientation of economic theory is apt also to change the kind of mathematics that economists will rely on. Recursive functions, for example, will be used not just as a method of solving dynamic programming problems but to model adaptive behavior. The Intensive Graduate Courses ("Summer Schools") that we have run at the Computable and Experimental Economics Laboratory of Trento University have pursued these approaches.¹³

Snowdon: In his recent survey of the development of macroeconomics in the twentieth century, Olivier Blanchard (2000) suggests that "progress in macroeconomics may well be the success story of twentieth century economics." Furthermore, he argues that economists who present the history of this development as a series of "battles, revolutions and counter-revolutions" convey the wrong image. The right image, according to Blanchard is "of a steady accumulation of knowledge." As an economist who has used the word "revolutions" when discussing the history of macroeconomics [Leijonhufvud (1976)], how do you react to Blanchard's assessment?

Leijonhufvud: Blanchard may be thinking of the steady accumulation of analytical techniques perhaps. But as for our understanding of the world, economists at one time thought the economy stable as long as the government did not interfere; later, the common belief was that the private sector was unstable, but could be stabilized by a wise and benevolent government; later still, the consensus view has been that the private sector would take care of itself quite perfectly and that business fluctuations can only be understood as caused by the time-inconsistent blundering of government. And that particular pendulum of professional opinion is presumably poised to reverse course yet again. Or consider how Friedman's monetarist theory of the cycle won out over Keynes's real-cycle hypothesis, only to be undermined by Lucas's unanticipated money hypothesis and then replaced by Prescott's real-business-cycle theory, which came to dominate just in time to usher in a decade of spectacular financial crises. I see no monotonous approach to "Truth" in this story. And surely there *were* "battles" along the way.

NOTES

1. This is one in a series of interviews published over the past decade; see Snowdon et al. (1994), Snowdon and Vane (1999), and Snowdon (2002).
2. Backhouse (1995) provides an interesting discussion of the relative influence of the work of Leijonhufvud and John Muth for the period 1966–1990 using citation counts.
3. See Akerlof's (2002) Nobel Lecture on a similar theme.
4. See Modigliani (1980, 1989).
5. See Fisher (1932, 1933).
6. See Snowdon and Vane (1999, p. 244).
7. See Snowdon and Vane (1999, p. 95).
8. Leijonhufvud (1998) defines the "Modern" tradition as one "whose hallmarks are optimizing choice and equilibrium . . . Arrow, Debrue, and Lucas are obvious examples of Moderns in my sense."
9. See the interview with Robert E. Lucas, Jr., in Snowdon and Vane (1999, p. 148).
10. For an exception, see, for example, Thirlwall et al. (1983).
11. Keynes (1930/1972) noted that "The prevailing world depression . . . blind[s] us to what is going on under the surface . . . of the true interpretation of things . . . the power of compound interest over two hundred years is such as to stagger the imagination."
12. See, for example, the collection of papers edited by Kehoe and Prescott (2002).
13. For information, see <http://www.ceel.economia.unitn.it/>

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