

Arjun Jayadev and Josh Mason carried this interview with Axel Leijonhufvud out over two sessions in Pismo beach in fall and winter 2015. Some additions were made from email conversations. The video and audio to the transcripts are available on request from INET.

ON HIS YOUTH

I: Let us begin at the beginning. Tell us a little about what were your most formative experiences as a young person.

AL: I was a bit overage when I entered the University. In 1950, my parents – undoubtedly with considerable trepidation – allowed me to sign on to the school-ship, S/V Sunbeam, a 450-ton three-masted schooner operated by the Abraham Rydberg Foundation. The Sunbeam cruise went from Goeteborg to Las Palmas and from there to Recife in Brasil and then Barbados, Martinique and St Barthelemy in the Caribbean, Faial in the Azores – nine months in all. After Sunbeam I spent three months on a merchant ship (M/S Immen) around the North Sea but then sobered up a bit and returned to school to go through the Gymnasium.

We sailed from Sweden through the Bay of Biscayne to the Canary Islands and then over to Brazil and the *Azores* and back home. Been gone for nine months plus. I was 17. But then I did go back to school. Each summer I would actually quit school a couple of weeks ahead of ending and sign on to a ship and be gone all summer. The first summer I went through the Panama Canal up the West Coast here to –Vancouver, stopping at all the major ports and a lot of very, very small ports in Central America and back. And the next year I went down the West Coast of South America. But I didn't have 20/20 vision, and that meant that I couldn't be a merchant marine officer. I could be a merchant marine engineer. So my affection for very, very large diesel engines and living in very hot surroundings and breathing diesel infused air was limited. I did it for a purpose of seeing the world. Eventually I went back and finished this matriculation exam in Sweden .

In my day, all Swedes had to do a minimum of 9 months military service. If you had matriculated from the Gymnasium, another 6 months were required. I had naively believed that my merchant marine experience would automatically place me in the Navy but instead I was sent to a field artillery regiment. (It turned out that the Swedish Navy refused all merchant marine men because too many of them were disciplinary problems!). I actually had a good time in military service and eventually chose to go through officer school as well, adding another 6 months.

Towards the end of “Kadettskolan”, we were *asked* to go through a set of long IQ-type tests that some Army psychologists had prepared. The reward for volunteering was that you would get the psychologists’ advice on what careers you might choose. Tests were supposed to be too long for anyone to finish in the time allotted but I finished them. When I appeared before the board of psychologists, smartly dressed “spit and polished”

in my best leave uniform, I was told that I had “broken the tests” by scoring 100% on each of the tests. They had discussed what career advice to give – and the only thing they had come up with was that I might become a *poet!* I laughed.

So I had to work it out for myself.

I: And then how did you end up coming to the U.S. from Scandinavia?

AL: I began my university studies at Lund University in Sweden and was going for a “politie magister” (master’s) degree. At that time, I thought I wanted to go into the Swedish diplomatic service or else to work for some international organization (UN, IMF or World Bank). In the 1950s, quite a few Swedes were prominent in these organizations. (Dag Hammarskjold). The degree required some Economics and I became interested in the subject. Before that my first interest had been History.

My coming to the US was escapism again. I wanted to get out of there and so I applied to the Scandinavian American Foundation and got one of their fellowships that took you through the U.S. for nine or ten months, or something like that. I had gotten married at age 21 and by then we had a boy, a small boy. So came to U.S. in 1960, when I was 27 years old, and this foundation just chose where to put you. So I ended up at University of Pittsburgh. I had a couple of good economics teachers, particularly a man by the name of James G. Whittier. I liked him a lot. He, more than anybody else, changed my mind about what I wanted to do.

Midway through the year, the director of the Institute came to me and said, "Axel..we think you should go on for a PhD in the United States." And this took me aback because I said, "Well, I can't afford it." "No, no," he said. "You have to go. You can get a fellowship". "If you apply, I have some money we could send you to interview at the places where you apply and if they see you," they said, "you'll get a fellowship." This almost shocked me. Here I am actually in the Land of Opportunities. It was not part of my plans or part of my hopes or anything else. It just happened.

I. How did you end up at Northwestern?

AL: It's a long story. I was interviewed at Yale by Tobin and Willy Feldner at Brown, and by others at Princeton. Okay, so then I find that I'm having an interview at MIT at 5:00 on a Friday afternoon with the Chairman of the department. The only thing I remember of that interview was that he looked at my transcript and said, "I see you have actually no mathematics." I said, "Well, that's true." I'm taking a math for economists course. So he said, "You say your interests are in theory. Well, you can't do theory with MIT with your lack of mathematics background." But then he said, "If you chose some of the Institutional subjects like IO or economic development, we can give you a fellowship." So I guess I was on my high horse and said, "Well, the only reason I wanted to go on in it in economics rather than go home and enter this diplomatic service was that

I become interested in theoretical material." So that was the end of that.

So I'm walking away from there in more or less deep thought, and I still remember there was a long corridor which met a corner and I just come up against the door. I at it and the card reads "Modigliani". Franco was just visiting there that year. So before thinking about it, before I had time to be intimidated, I knocked on the door and went in. Franco was packing his briefcase to go home. He opened the door and said, "Yes? What do you want and who are you?" Or something like that. So I told him what I was doing and that we had been reading all his papers of his and I said, "I'm sorry, but I saw your name on the door and I just knocked on an impulse." So he looked at me and said, "All right, sit down for a while and we talk." So we talked for a bit and then he said, "Tell you what I'll do. I'm moving to Northwestern and if you apply to Northwestern I will write to the chairman there and tell him to give you a scholarship."

So that's how I ended up at Northwestern.

I: Wow. Do you remember what the conversation was about? Obviously you made an impression on him.

AL: I didn't know much economics at the time. Well, you know I told him I had read all his papers of his. Maybe he checked the veracity of what I would recall. So I had applied to all of these other schools and not to Northwestern. And so I went back and I applied to Northwestern because of my interest in Modigliani and that's how I ended up there. As it turned out, Modigliani only stayed one year at Northwestern. So in that first year I took all these courses and then he left.

There was a department secretary that I always maintained chased him away. There was only one secretary to do typing and when Franco finished a paper he wanted it typed right away. Well, this lady, I used to remember her name but now it doesn't come to me, had her old favorites in the faculty. And he was just a new man there and a foreigner and whatnot and he had to wait in line. So he left.

So then how did you end up at UCLA?

Having finished my coursework at Northwestern, I got a one-year dissertation fellowship to Brookings. That winter I went to the AEA Meetings, mostly to get to hear some of the economists whose papers I had studied. My intention was to return to Sweden at the end of the academic year. Bob Clower, whom I hardly knew since he had been in Liberia during my two years of coursework, decided to "market" me and managed to get me a number of job-market interviews. The two best offers that came out of this were from Yale and UCLA. We (my first wife, Marta Ising, and I) had a brain-damaged 3-year old boy and it was clear that we could get better care for him in Los Angeles than in New Haven. To the surprise of some people I leaned towards UCLA in any case so the choice was clear-cut. I have never had any regrets about the choice I made. I was treated very well when I visited Yale but a friend at Brookings who had been an assistant prof there

warned me that one would meet James Tobin and other senior faculty members only on some occasion that sounded like the “annual review of the troops”. At UCLA, on the other hand, I got into interesting discussions with Armen Alchian and Jack Hirshleifer already at the time of the job interview in Los Angeles.

I: But you had trouble with your dissertation...

AL: That's true. I was working all the time. I worked long hours all the time and so I amassed manuscripts of this, that and the other thing but none of them was a finished paper that you could send to a journal. There were problems that bothered me, but they didn't lead to easy conclusion or anything like that. So I guess it was the end of my second year I was called down to the chairman and said, "Axel, everybody likes you here. But I have to tell you that the rules for the university is that you can't be an acting assistant professor for more than two years." And acting could only be removed by completing of dissertation. So I said okay. So the third year I was a lecturer, but I was mentally thinking that I was going to go home. But this whole business meant that I had to try something. I had heaps and heaps of manuscripts, none of which amounted to very much. I thought to myself that I'd have to go through all of these and see if I can stitch it together so I can go home with an American PhD. So I sat down, I remember, I read all this crap from beginning to end looking for a thread. And halfway through I realized there was a thread- *in the footnotes!* And the thread itself was a sizeable number of footnotes that says something to the effect “I was always taught that Keynes said this or that, but it's not true!” I realized that people had Keynes wrong. So the joke I used to tell was that I made the footnotes into text and the text into footnotes. That did sort of hang together. There had to be a little bit of glue here and there. But that became a doctoral dissertation in two volumes.

CAMBRIDGE, HICKS AND THE PATH NOT TAKEN

I: In the late 1960s, when your first book came out, it appears that there were a whole host of potential ways that the discipline could have gone forward. But only one of them survived: intertemporal optimization. In your great essay “The Uses of the Past,” you suggest always having a sense of the history of the discipline can help us find roads not taken. Can you tell us a little bit about why the kinds of approaches you suggested never took off as a research paradigm?

Well, if I think of the lines that I was working on--it couldn't be modeled by some simple modification of general equilibrium economics or something like that. There was no apparatus available to economists at that time that you could tinker with and go down the path that I was interested in. So it turns out that these shared model frameworks are important, sort of skeleton for a profession to build on. I wasn't supplying anyone on that and so the profession continues being very bound historically to a particular past and the link is general equilibrium or some other skeletal structure.

Another “path not taken” from 1960s-era macroeconomics is the Cambridge School of Joan Robinson, and the related work of people like John Hicks. Can you say something about your relationship to them?

I spent a term at Cambridge, and I met with Joan once a week, more or less regularly. We would have lunch at this faculty place on the river. When we started out, she obviously had a program of “setting me straight.” I still remember being at in this faculty club and I was hustling about getting this or that for her. And when I finally sat down opposite her, she fixed me with her very blue eyes and said, “Axel, I’m disappointed in you, and we have to get this straight.” She had in mind that she would browbeat me into being part of the Cambridge School. She felt that she needed somebody of a younger generation to keep her and Kahn’s version of Keynes alive. Once she gave up on me, I didn’t see her that often.

They must have had generations of students there, and surely they had people who were faithful enough, but they were not productive. It’s a bit sad that it didn’t have the potential to carry on.

I: And Hicks?

AL: I was a great admirer of John Hicks as a student. And when I was laboring over this endless dissertation of mine, there was a guy at Oxford University Press whom I was very friendly with. I’ve lost contact years ago; he’s probably not alive anymore. And he came to me and said that Armen Alchian had told him that I had a manuscript he should be interested in. He said, “Tell us who would you want to read your manuscript, and we will get him for you.” I sort of laughed and said, “Well John Hicks,” and he said, “That’s easy. He’s on our board of editors.”

So he read it, and I was waiting with bated breath on what he would have to say. He wrote a note of two little sentences which said that he had read it and of course they should publish it, and that was it.

I: So he didn’t give you any feedback, any advice?

AL: No. No. But Earlene and I later became very good friends with Hicks. And several times we went and stayed in their house. It was an ancient house. I forget how old it was; I remember how the oldest part was 1300 something. And there was one end of it they had for visitors. You were left alone during the day and it was explained they should not be disturbed. They both worked in their studies. And then the rule was that at 5:30 it was time to join them for drinks upstairs. Drinks meant gin and tonic, mostly gin – they both imbibed quite frequently. But the conversations were great.

John Hicks was a stammerer, and so when you had an ordinary conversation with him, every utterance started with uh, uh, uh, uh, uh, uh, uh, uh, uh and sometimes was interminable. And then the answer would sort of burst forth rapid fire and then he would

start all over again. But he was fond of poetry. When he was a student he had been the poetry editor for an Oxford student paper. He had memorized lots of poetry, including the whole fifth canto of the Divine Comedy in Italian. Now and then when you talked with him he would quote something, and that would flow perfect without stammering. Later on we were staying outside Sienna, and he would visit there regularly. At one point we were out with a bunch of students – it was the opportunity of their life to sit and listen to Hicks. I brought up the poetry, and he explained it this way: He had trouble falling asleep. So the way he would get to sleep was he would set his mind to rehearsing or remembering one stanza of poetry for each century, starting in four or 500 B.C. And he gave some examples and then he said, but from the second to the ninth century there wasn't much worth remembering.

ON HAYEK AND KEYNES

I: You are known as a Keynesian, but one of the interesting thing you did was interview Hayek. Nowadays, they are vulgarly considered archenemies. Can you say something about what you saw in both of them, that contributed to your own work?

AL: Well, if you look on that book on Keynes's, you find a quite number of footnotes on Hayek. The whole process got me to look at Keynesian Economics in a different way Keynesian and to say that the coordination problem in macro was a problem of information and how information was produced and disseminated in the system. I think I was right that this sort of implicit in Keynes, but this way of talking about it is pure Hayek. So I became very interested in Hayek and very influenced by that part of Hayek. I had very little interest in much of the other material. I was in a department which was almost an outcast because the senior people were known sort of market conservatives, free marketeers and I was the odd man out there. I got along famously with them but that was not the aspect of Hayek that interested me.

I: And later you met and interviewed Hayek, correct?

AL: My wife, Earlene Craver and I, both recorded interviews with Hayek.

Earlene was a historian who had specialized in Modern European History with a Master's in Economics. In talking with her I mentioned that I had read a lot of the literature on the intellectual migration of the twentieth century and had noted that economists had not been included in this literature. I showed her my name cards on the émigré economists who had made careers in the U.S. and said that I had never gotten around to doing anything with it. She suggested that we go ahead. I got some seed grant money and she later got other grants including a year-long grant from the National Endowment for the Humanities. I did a number of the interviews, including one with Hayek, and she did others. She did much of the work on the intellectual and cultural climate of that period, and, on the connections between these economists. (cf. E. Craver, "The emigration of the Austrian Economists," *History of Political Economy*, 1987). As a consequence we were

well-prepared for the video-filmed television interview of Hayek. I recall that Haberler, who had not met my wife, asked in correspondence if she were Viennese or Austrian. And, she replied, “No, but I am an historian”.

You will find some references to Hayek in my 1968 book on Keynes and the Economics of Keynes. I read Hayek’s 1973 book on *Law, Legislation, and Liberty* and it was referenced in a paper entitled “The Costs and Consequences of Inflation” that I did for the International Economic Association conference held at S’Agaro, Spain in July 1975. (It is both in the conference volume and in a collection of my essays, *Information and Coordination* (1981).

I first met Hayek personally in Salzburg in 1975-1976. I met him several times after that at UCLA, Claremont, and Freiburg, and he was always gracious. He definitely was pleased by the revival of interest in his work but, I believe, did not really want slavish followers. In particular, I remember a pamphlet he had written for Institute of Economic Affairs in London on a currency reform proposal and I raised a theoretical objection to his proposal. I remember walking with him to the UCLA Faculty Club for lunch and explaining what I thought was wrong. He got quite worried and said he wanted to think more about it. I do not think he found a satisfactory answer and, as far as I can recall, he stopped promoting the proposal.

I: You wrote your first book about Keynes and you continued to write about Keynes for 50 years. In 2008 – 2009, you had four different articles about Keynes. Why have you been so drawn to reconsider Keynes so many times over the course of the years?

AL: For me, Keynes was an escape from the professional consensus. There might have been others. Hayek was another way of looking at the mainstream from outside and that attracted me as well. In both cases it was perspectives that were rather outside the mainstream at the time.

*I: In your book, *Keynesian Economics and the Economics of Keynes*, you already said that you did not think it as useful to go back to Keynes. You said “My own preference would be to go back to Hayek.” And you haven’t written very much specifically about Hayek. Why was Keynes was a more productive frame for you than Hayek?*

AL: With Keynes I felt that there was this misunderstanding, which had seeped through almost entire profession. It was important to try to straighten it out. Hayek was of course persona non grata for decades, but these late works of his that I admired and read closely, well, there was no misunderstanding them. They were very clear-the work spoke for itself. There was no need for me or anybody else to explain what Hayek was saying.

ON STABILITY AND INFLATION

I: Both Keynes and Hayek were perhaps also seen as great theorists of macroeconomic instability. The notion of stability is also a deep current in your work: macroeconomic stability, monetary stability, something that you can trace in your latest paper and your very earliest kind of work. Can you tell us about what motivates this concern with stability, social stability, macro stability and monetary stability?

AL: Well, social stability is obviously an issue. Economic stability, the reason why I'd been harping on it was that I felt it was completely almost neglected by my generation of economists. I don't remember any colleagues who told you it was a big problem. They did equilibrium models, they assumed that the system would... well actually I don't know what they assumed. They assumed that the world would be like the solution states of those models, perhaps. And how the solution state was arrived at did not seem to be a question. Maybe the fact that I came from Europe shows a messier history than the U.S. had something to do with. I remember being very impressed by my teacher in primary school. His name was Harry Johanson. And he told us how he had traveled in Europe – he had traveled in Germany during the great inflation after World War I. So perhaps some of this concern came out of the environment in a way that didn't do for one American students.

Earlene Craver: Tell the story of how Hayek came to study economics.

AL: Well, this is pretty relevant. It was Armistice at the end of World War I. And he had been fighting on the Italian front, and all a sudden the war was over and they started a retreat to Vienna. They marched back in good order with all the weapons, but they saw all around them on the roads units that are just falling apart -- people threw down their guns, said "I quit" and went home. And that made such an impression on him that he decided that he had to study economics – at that point it was still political economy -- to understand why this happened. It was to understand human behavior and societal behavior. And there was no mathematical equilibrium model of the defeat.

There has to be a problem, a question, out in the world that you have to struggle with. For another generation it was the depression.

I: Unlike many other Keynesians, though, you were particularly concerned with another aspect of instability - inflation. In your work on inflation you argued that the economics profession largely ignored its most important impact, which is the breakdown of contract enforcement and coordination through the price mechanism, as opposed to "shoe leather costs," the real money stock and distribution.

AL: Well, that paper was pretty bad –

I: I have to say I really liked it.

AL: What's wrong with it is the underlying revulsion in it. I was convinced that the inflation stuff at the time was not only pure nonsense, but it was also dangerous. It didn't occur to me like that, but the conviction grew very rapidly. I came across some stuff at the time on inflation, all of which had this theme that the only cost of inflation was shoe leather cost. So I wrote this paper. It's a mishmash paper, but in order to announce my conviction that the profession was just out to lunch on social costs of inflation and I remember being at one of the cocktail hours or whatever at that conference, hearing behind me how a group of guys from Cambridge thought that the criticism of inflation was really ridiculous. But that it made sense that I came from UCLA because everybody there was a dyed in the wool conservative. In any case it was not a good paper but just the fact of running into this sort of opposition and being convinced that basically the profession had gotten this all wrong. This made me go back to the subject over and over again.

I: Today there seems to be a revisiting of the question of how concerned we should be about inflation, after a consensus of 20 years that low inflation is the overarching goal of the policy. For a long time price stability was considered almost a sufficient condition of good macro policy, but nowadays there's an idea that higher inflation might be desirable to reduce debt or because it creates more space for monetary policy before you reach the zero lower bound. In your view, are people still kind not grappling with the real costs inflation, or conversely, perhaps, have people overlearned the lesson of the importance of avoiding inflation?

AL: I don't know whether the people who are opposed to inflationary policies today, know why they are. I do think it's a convention today but many people who hold that convention couldn't give you a very clear answer of why they hold it.

I: Inflation obviously has distributional effects. Could say something about the sort of norms of justice or fairness that you think should apply in this area? Obviously the nominal terms of the contract are being fulfilled even with inflation. So how do we evaluate the kind of morality or justice of inflation in terms of its redistributive effects?

AL: We don't. Do you see it being debated? I think maybe one should debate this in terms of should the government run an inflationary policy to reduce the real value of debt or at least long term debt or just repudiate. And obviously these have different distributional impacts. But given that the distributional effects are different, you could ask the groups that are hurt the most in this case and in that case, which group can afford it best or something like that. But for the United States it's almost unthinkable to openly repudiate a government debt as it would have been for the British Empire to do so 150 years ago. But the inflation is largely that of course to have a significant impact on the debt burden by inflating, it's very important to look at what the maturity structure of the debt really is. Because all the short terms are going over at higher interest rates and it's mostly the long-term debt that you're expropriating.

I: A few years ago you talked about a crisis as a situation where some contracts are not going to be fulfilled. So in a crisis is you're sort of inevitably faced with a choice of which commitments do we wish to uphold and which ones are we going to have to repudiate. It seems like again you need to have some notion of justice or fairness or some other criteria to decide that.

AL: Of course this is the fundamental difficulty. There is a structure of law, which is supposed to embody the concepts of fairness that this society adheres to. And now you're debating which way you are going to violate that and what is fair or unfair about that. So there aren't going to be very clear answers.

I: When you wrote that piece on inflation in the 1970s, it was very clear where you saw the greater danger of unjust outcomes. It was on the inflation side.

AL: I think for people of your generation, it is difficult to recapture the thinking of the time. People were very careless in how they thought about inflation at the time. That first paper I wrote on inflation was marred by ill temper -- you shouldn't try to write analytical papers out of indignation. Today, obviously we're at a difficult point. But is it one which could be resolved by inflation? I don't know.

One argument would be that allowing money contracts to be undermined or made meaningless by high inflation destroys the effectiveness of the price system as a coordination device. On the other hand, an excessively rigid instance on honoring all terms of money contracts can also undermine the social compact.

AL: Yes. I think can let them all a system or outstanding contracts, which puts us in this kind of bind. We have no mechanism institutional or devices that prevents this from happening. We just have contracts that are being concluded every day and just the stock grows and obeys no particular underlying principles that would prevent that. Now if it was possible to say let's have 20 percent inflation and then quit, something like that, if there was a way to engineer inflations in a predictable way and say by and large – I like this outcome a bit better than explicit repudiation or something like that. But you start inflation on policies, and we're not talking about a few percentage points a month. We're talking about something that would have a significant impact of the outstanding volume of debt. And the truth is then you can't put on the brakes very quickly. You tend to lose control of the situation. The high inflations are situations where the government does not control the situation.

I: Looking back the great inflation of the late '60s through the early '80s, it seems like a sort of distinct bounded historical episode. So are there any sort of lessons you might take from the sort of trajectory of that process in terms of sort of how this unfolded and what brought it to the end and what people might have learned from the whole thing?

AL: Well, it appears that strict anti-inflationary policies eventually became acceptable. More than acceptable; demanded almost. That was more a product of experience of the period than anything else. But memories like that fade and now we are 35 years later or

something and most of the current population don't have those memories. I suppose it's the function of the economics profession, the knowledge carriers.

I: You say it's almost like just a taboo without a clear set of meanings behind it.

AL: Yes.

ON WICKSELL

One of your contributions to macroeconomics is reviving interest in Wicksell. You have a copy here of Woodford's *Money and Prices*, with an inscription from him thanking you for getting him to take Wicksell seriously. How did you come to see Wicksell as central to the history of economics and to macroeconomic policy?

I started my study at the University of Lund That's where Wicksell had his professorship. And the professor I had there did something to keep Wicksell alive. And the Swedish tradition was still there although it was running out because there were no new great names. But it was still venerated [*laughs*], and it was a matter of course that you would learn about it. But I think I got early on more interested in Wicksell than most of the students for reasons of my own.

As for Woodford, I remember that we were both at a conference in Taiwan. We knew each other already. For a while I think he had some interest in my stuff and vice versa, but I was out of sympathy with the direction which his work developed, including this book that he wrote.

I: Can you say more about that?

AL: I didn't believe in intertemporal equilibrium models, even if stochastic. That became sort of a conceptual obstacle between us. But I was impressed with him. I thought he was super bright, which can sometimes be a handicap.

ON THE AUCTIONEER

I: Another contribution of yours is the idea of the auctioneer, which is so commonplace today it's hard to even imagine that somebody was the first one to come up with it. But that someone was you! Was there any specific thinking there besides just the idea of this abstract process more concrete?

AL: Well there were some sort of mechanical theory of market price adjustment, but those tended to assume and formalize that the price of a good in a particular way. A certain way of formulating mathematically had taken hold, and people were not thinking of it in behavioral terms. And so I'd argue in class that this would be true only in an auction market where the market where somebody announced the price and got feedback

on the quantities. I had been to a market in Sydney, Australia. It was an auction market for wool or something like that. And it was almost an auctioneer market. And I said there are only a few markets in the world like that.

I: And then this metaphor somehow made it into general use?

AL: There are hardly any markets in the world that work like that. But that notion of a market has been important in economics.

I: So you introduced to this to sort of make the point that this is not a realistic model or this mathematical formula doesn't correspond to actual market processes.

AL: Yes. And then it became a gadget. I used it only critically, but then it took on a life of its own.

ON CORRIDOR STABILITY

I: Another one of your major contributions is the idea of the corridor stability. When you introduced that idea, were you thinking specifically of the depression, or were you thinking of this as a more general concept?

AL: I was thinking both of depression and high inflation at the time. And it originated as a joke I used to use in class. You know that to discipline models you must suppose they have floors and ceilings or one or the other. And I used to make fun of these models in class, saying that it was like a drunken sailor stumbling down a corridor in a swaying ship, from that wall to that wall. It was part of my experience.

I: The corridor of stability seems like a powerful way of thinking about instability in general, where the price mechanism operates effectively within certain bounds, but outside of those bounds is the expectations adjusted in the liquidity constraints fined seems like a very simple and powerful way of thinking about stability. Why do you think this framework has not been more broadly accepted?

AL: I've had young economists in particular, after a conference come after me saying they're very interested in it, and ask what they can do with it. And I had no good answer for them. I do think this is inherently more difficult path to take and more difficult to formalize. I doubt that the concept is widely known today. At the same time, I believe that the extraordinary monetary policy of the last couple of years reveals a genuine apprehension that the economy may have come quite close to an unstable downward spiral.

I: There are people who seem to still continue along in some way. For example, the work of Richard Koo. Can you think of other work that continued in your framework?

AL: I got along very well with Koo and we met a few times. I have a high regard for

Richard Koo and his thinking on this subject. To my mind, his stuff is rather closely related to my “out of the corridor” idea. There may be others, but at this point I'm out of date. So for the last decade or a little bit more I'm unfamiliar with the literature.

I have a dear friend and former student, Daniel Heymann. He's done a lot of work on high inflation, and he's very much attuned with that. I think he's used a corridor as a term more than anything, and there may be others. I had a whole bunch of Argentinian students at one time, always looking at the question of high inflation

ON THE BREAKDOWN OF KEYNESIAN ECONOMICS

I: Let's talk a little more about the sort of intellectual situation in the 1960s and 1970s. As you point out, even then there was a clear disconnect between the sort of Keynesian macroanalysis used for policy questions and the sort of price theory that was taught in graduate school. And yet people seemed to accept that discrepancy.

I used to say when I taught one thing on Monday, Wednesday and Friday and the other stuff on Tuesday and Thursdays or something, and having a day in between was enough! Basically, I think most people were sort of satisfied with macroeconomics was general equilibrium with one price that wouldn't adjust. And if that didn't always fit, there could just be other rigidities or something like that. The story I just gave you about the days of the week was one of my little rhetorical devices to try to say “People that this is really a bit crazy.”

I: Keynes described the problem he was facing in a similar way – the determination of the overall price level seemed to occupy a different conceptual universe from the discussion of relative prices, and it was his job to somehow unify them.

AL: Keynes did write models. He was literarily gifted, but it turned out that the way he wrote up his own understanding of the situation was subject to misinterpretation that could grasp and tie a profession and go on for decades. Literary devices are slippery things. The monetarists saw the same problem, I think. But they also felt that it was easily fixed. You just had to be more rigorous with using micro-theoretical tools.

I: Why do you think that approach won such a total victory? It's very striking, if you look, compare the debates of the '70s or the early '80s to what has really continued ever since then to be the absolute dominance of these inter-temporal optimization models. Do you have any idea of how that came about?

AL: It gave a lot of people something to do that they could do so armies of graduate students could, once they learned this, find ways to tweak this or that and carrying on. I think that's a big part of the answer. The other part of the answer is the weakness of the empirical methods. There's no regression to run that would tell you this is nonsense.

I: What are the costs of this?

This may not be the answer that you're looking for, but the first thing that occurs to me is it's made economics boring, so I'm uninterested in it! I suppose one of the dangers of being stuck in a mathematical framework is can make the whole profession hide bound so to speak because this mathematical structure is the framework within which you're supposed to play the game and get tenure and so on. You have to find something new, but it has to be within this framework. And these models, if you try to transport yourself out of the profession so to speak and look at it from a distance, there's something almost ridiculous of the grip that it has over us.

I: So a big part of the success of the current paradigm is, as you say, it gave graduate students something to do. Once you learn what kinds of tweaks are acceptable and you can tweak the standard model and produce papers. But one of the interesting things about the intertemporal optimization approach is that it removes the whole problem of coordination over time from the scope of economics.

AL: That problem is always solved within the framework. The pretense that we know the future probabilistically as a given set of probability distributions of every damn thing is I think a pretty dangerous delusion, but it's also a comforting one to some people.

I: Some people have really tried to emphasize this as sort of the aspect of Keynes that we should think about: the unknowability of the future, or fundamental uncertainty. Are you sympathetic to that?

AL: Yes and no. What's striking about Keynes is that he found a way of carrying on anyway so to speak, and to understand the world and do very, very useful things. And there's a way of teaching this sort of fundamental uncertainty I feel. Maybe I'm not right about that. That would make people throw up their hands and say, "well, what can I do?"

I: Your work, while analytical, takes a particular form: historically informed analysis. You are, for example very suspicious of cross-country regressions and the use of time series econometrics. This harkens back to old debates between Keynes and Tinbergen, but also echoes discussions of scientism. Is a 'science' of economics possible?

AL: I am not sure that the question is important. The point is not to fake it. "Don't shoot the piano player – he is doing the best he can!"

ON PRODUCTION

I: A lot of your work is about coordination problems in the sense about bringing coordination to kind of the center of the sort of the economic problem as opposed to efficient allocation and the intertemporal coordination, but you also have had an interest

in the question of the division of labor and why we have factories and such.

AL: Yes, it's a prior problem to efficient allocation, which is not determined by the price mechanism. I did write about it in "Capitalism and the Factory System" but I don't think it got built on by other people. For me it was definitely a piece of the puzzle so to speak. But I guess I talked about it much more in courses of economic history than I did in macro. I think those students that didn't take history with me probably didn't know that stuff, I think.

I: It seems like there's a suggestion there when we're thinking about the capitalist, we should think as much about the organizer and the production process, at the entrepreneur in that sense, as opposed to simply the steward of a particular set of long-lived assets.

AL: One reason for getting back to it over and over again in teaching now was a certain animus that I had against the neoclassical production function as a way of understanding how production is organized and so on. So I think I used to harp on that quite a bit. But in all these macroeconomic models that they read, there was the neoclassical production function. And I wanted them to understand that this was a piece of a swindle.

I: I think you called it a recipe for bouillabaisse?

AL: Yes. Production is just like a big soup pot and you throw in all the ingredients and you wait and there's soup, as opposed to something where we have to consider the manner in which things are combined. I think that maybe economists need to rethink the way they think about production theory. Right now I'm not thinking about this matter of neoclassical constant returns versus increasing returns. But just the way we think of production, especially assembly line physical production. I think production theory has sort of lost contact with the way the system is developing now, and that is a big problem for economic theory.

I: So what do you think we should do about this?

AL: I have no solution to it. I think economics was always poor in capturing the intellectual side of production of the whole process. So we came up with these gadgets that was a neoclassical production function. And we got a certain amount of mileage out of it but the distance between these gadgets and the reality is constantly increasing. I don't have an alternative gadget to put in its place. I mean I think it helps a bit to realize increasing returns to scale when you see it, and it would help to understand structures of industry and so on in a way that standard stuff does not. Increasing returns might be a disruptive idea in terms of current macro, too. Anything to stir macro up now would be good for you.

I: In Capitalism and the Factory, you focus on the invisibility of the sort of central issues of real issues such as the question of ownership, of who is in control, which if you pick up the Wall Street Journal, this is what it's about. It's about changes in ownership, about the existence of businesses. And yet that's almost invisible in standard economics.

AL: The concentration of ownership I think is becoming a more serious issue all the time. I think like I lived in a more idyllic time. Only world wars and that sort of thing to disturb us! [laughs]

ON STYLE AND BEING IN OPPOSITION

I: You have a very distinctive style among economists. It's at these very memorable metaphors. Do you have people you admire as writers, who influenced your writing style?

AL: No. Today I'm not longer a great reader, but I used to read a lot. I never read as much as historians read. I didn't try to imitate anybody's writing style. The jokes are there to relieve my own boredom, or because they worked in the classroom.

I: This is linked to your sense of individuality. One of the consequences of being in many ways sui generis, there is no group of "Leijonhufvidians" if you will. Would you like to have had a school?

AL: No. Definitely not. I had some students that were congenially closer to me than others of course – Daniel Heymann is an obvious example. But he's his own person. I didn't like the idea of schools basically. And some of my opposition, psychologically, some of my oppositions are monetarism, say or the Keynesians that I was opposed to – probably had to do with not liking schools and that sort of attempt to create a homogenous group of people.

*I: It seems like given them your work in *Life among the Econ*, there is some tribalism within economics and in fact sometimes tribes went out with complete decimation of other tribes and so on. So is it a value to having a heterodoxy, which is self consciously different and talks within itself to keep these kinds of tensions, creative tensions alive?*

AL: Yes and no. The heterodox groups usually become sort of encapsulated. No one reads them and they read other stuff in order to dismiss it or something. When I was at UCLA, it was an out of the way place in a sense. People like Alchian and Hirshleifer, well, they had friends in Chicago but sometimes were called Chicago West, but that was a misnomer that they were really independent people. And I liked that.

I: But suppose you're unhappy with the direction of the profession and the sort of monoculture that exists in a lot of particular macroeconomics. Is it possible to challenge that effectively from the standpoint of just being an individual, or is it necessary at some level to have a counter movement?

AL: I'm not sure I know how one could derail the current train. Maybe you have to wait

for the next catastrophe or something for the real world to impinge on this kind of modeling. I don't think that you can write a paper that gets into the AER but upsets the apple cart.

I: That's certainly true. But how big a problem is it? Are there really questions that the current methodology does not allow us to see? Or is it a question of people not being creative enough with the methodology?

AL: Well, the insistence that things have to be said within a particular framework I think is misguided. I think schools have always had some mechanism of enforcement. It helps to make people concentrate and keep it in this and forge straight ahead and so on. But it tends to shut out original ideas and insights. So there's a trade off. You can't just depend upon kooks to renew the subject.

I: But how do we distinguish the sort of useful potentially productive heterodoxies from the kooks and the cranks and the crackpots?

AL: The answer to that occurs to me (it could be turned on me in a sense) that one way to distinguish is this thing is to look at the person's teaching. Does this man or woman's teaching lead anywhere? Do the students do useful work? Is it growing over time? That can be turned on me in a sense. But I still think that's one way of thinking about the problem.

ON TEACHING AND ADVICE FOR THE FUTURE

I: For those of us who teach economics, how much we have a duty to give the established view of the profession even where we think it's wrong or misleading, and how much we have a duty to try to give students our ideas of the most productive ways in thinking about economic problems even if they're sort of underdeveloped or idiosyncratic? How did you deal with that as a teacher?

AL: In undergraduate courses I was pretty cautious I think. Well eventually I couldn't stand the undergraduate macro textbook, so I developed a sort of class notes. But I still had to assign a textbook to the class, and I think I changed it almost every year, hoping to find something better.

You had to teach enough of the mainstream, and then if you knew the criticism you would add that. But I never felt I could start out from completely left or right field, as it were, and do the whole thing from scratch.

I taught a modicum of the textbook stuff and then I tried to teach my own. I probably made a bit of a muddle of teaching, but very good students liked me.

I: Do you have any advice to someone just starting off in economics?

AL: This is what is called Avuncular Advice [Laughs]

I: Would you say, "don't do it"?

AL: No, no, no. By all means, do it. Do I have a recipe? No. I don't have a recipe. But be inspired. You must stick with the questions that are interesting to you, which means, I think, that you don't write about things that you understand perfectly. You write on things where you're trying to straighten yourself out. Things that are not perfectly clear. And if you do that you're more likely I think to find readers than if you make variants of things that are very well established. And also, if that's the way you think about it you know you won't run out of material if it's things you don't understand perfectly well. There are always going to be plenty of them.