Macroeconomic Dynamics, **11**, 2007, 249–271. Printed in the United States of America.
DOI: 10.1017.S1365100506050395

MD INTERVIEW

AN INTERVIEW WITH COSTAS AZARIADIS

Interviewed by Steven N. Durlauf
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July 2002

Costas Azariadis was born in Athens, Greece, in 1943. He attended the National Technical University in Athens and received a degree in chemical engineering in 1969. He then attended Carnegie Mellon University, receiving an MBA in 1971 and his Ph.D. in 1975. His doctoral dissertation was advised by Edward Prescott and Robert Lucas. Azariadis's first academic appointment was as an assistant professor at Brown University between 1973 and 1977, after which he moved to the University of Pennsylvania. In 1992, he moved to UCLA. In addition, Azariadis spent two semesters at Hebrew University and Princeton and has held briefer visiting positions all over the world.

By any measure, Azariadis is one of the world's leading economists. His research has profoundly influenced the way economists think about labor markets, business cycles, and growth and development. No quick summary can ever capture the formal elegance or depth of his work.

Azariadis's first major set of contributions revolve around the origination and development of implicit contract theory. This research was of enormous importance in the revolution in macroeconomics that is associated with the onset of rational expectations. From the perspective of traditional Keynesian models, the rigidity of wages represented perhaps the best empirical support for the nonmarket clearing price assumptions that underlie the classical theory, at least in its textbook aggregate supply/aggregate demand instantiation. Azariadis's demonstration that wage rigidities may represent a mechanism by which firms insure workers against risk, so that wages do more than simply determine relative scarcities, showed that wage rigidity was not necessarily evidence in favor of the Keynesian perspective. It also led to the development of a broader literature on understanding how insurance

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 1365-1005/07 $12.00

FIGURE 1. Undergraduate work group with supervising professor, Athens 1968.

markets may be nearly complete even if formal markets for various risks do not exist. In turn, Azariadis demonstrated that the uncertainty in an economy may be structured so that wages cannot always perform a risk insurance role without affecting labor allocations, thus providing a coherent microeconomic explanation of unemployment, in this sense rescuing the Keynesian perspective.

A second fundamental area of Azariadis's research revolves around the study of sunspot equilibria. Azariadis's 1981 *Journal of Economic Theory* paper represents a pathbreaking contribution in the sunspot literature, showing how sunspots can arrive in a well-specified economic environment without reliance on special functional forms. This model has become a workhorse for much of the subsequent sunspot literature. The implications of sunspots for aggregate fluctuations are further developed in Azariadis's work with Roger Guesnerie. This work continues to have profound implications for how one conceptualizes economic fluctuations by demonstrating that animal spirits can indeed play a fundamental role in aggregate fluctuations. One of the continuing challenges in empirical macroeconomics is the full integration of this body of ideas into empirical models.

Since the middle 1980's Azariadis's research has turned from business cycles to long-run considerations. With Allan Drazen, Azariadis developed one of the pioneering models of new growth theory, one in which threshold nonconvexities in the aggregate production function can produce multiple steady states. Thus, poverty traps are readily interpretable as low-output steady states. This model continues to be one of the workhorses of growth research. An appealing feature of the Azariadis–Drazen model is that it provides a generalization of the Solow–Cass–Koopmans neoclassical growth model. Near each steady state, economies that obey the Azariadis–Drazen model behave "as if" they were characterized by a standard neoclassical production function. This correspondence to neoclassical models allows ready tests of the Azariadis–Drazen model versus a neoclassical alternative in that the neoclassical model is a special case in which all countries obey the same linear growth process. Empirical work has suggested that the general Azariadis–Drazen model in fact has additional explanatory power beyond the neoclassical special case. Subsequent work by Azariadis on growth and development has focused on the role of financial market imperfections in producing persistent poverty. This work represents one of the best articulated descriptions of the microeconomic foundations of poverty traps.

Azariadis's interest in financial market imperfections, in turn, has fed back to business cycle interests. In work with Bruce Smith, Azariadis has developed a business cycle theory in which endogenous changes in market imperfections influence aggregate outcomes by producing regime switches of the type empirically documented by James Hamilton and others. This continuing concern with mapping dynamic economic theory to data is reflected in some of Azariadis's most recent work, with James Bullard and Lee Ohanian, which explains aggregate output dynamics via neoclassical growth models in which aggregate consumption and savings are affected by the distribution of wealth across cohorts.

As the interview will indicate, one theme that links Azariadis's different research programs is a general interest in multiple equilibria. This relates to another persistent theme, the effort to understand empirical observations that appear difficult to reconcile with Arrow–Debreu type formulations of our economic environment that rely on convexities and market completeness. From the perspective of either theme, Azariadis's work has addressed some of the hardest and most fundamental questions in economic theory with remarkable success.

In addition to his own research achievements, Azariadis is well known for his contributions as a coauthor, colleague, advisor, and mentor. He has produced a remarkable number of successful students, many of whom started their careers as coauthors with him. What is less measurable but nevertheless important is the support that Azariadis has given younger scholars, especially those who have embarked on research programs that fail to fit prevailing fashions.

The interview was conducted in the Alburquerque airport after a Santa Fe Institute conference on poverty traps, a conference for which Azariadis was an inspiration.

Durlauf: Costas, first of all, thank you for agreeing to the interview; it is very much an honor for *Macroeconomic Dynamics* to have you appear in its interview series. I thought we could start off with some personal intellectual history. So, unsurprisingly, my first question is what first led you into economics?

Azariadis: Steve, thanks for interviewing me. It's a pleasure to talk about things that interest me with someone I personally like. How I got into economics was a pretty random process. I don't think there was much planning or rationality in it. It just happened. I came to the United States in 1969 to do an MBA, having been an engineer in my previous life, with the notion that an MBA was a natural thing to do after a degree in engineering, if one wanted to be successful in industry. In my second year of the MBA program at Carnegie–Mellon (the degree was called then an M.S. in industrial administration) I took some electives in economics. These

FIGURE 2. Happy times at Carnegie Mellon, 1972.

electives brought me in touch with some interesting young economists. One of them was Bob Lucas, one thing led to another, and it's easy to understand how I got into macro issues after all that!

Durlauf: Who was your primary advisor?

Azariadis: Bob Lucas became my mentor around 1971. He was already a rising star in the profession. What attracted me most was the depth of his commitment to business cycle issues and to carefully micro-founded macroeconomics. Later I spent a lot of time with Ed Prescott and, in the end, I became the joint product of Lucas and Prescott. I worked closely with them, especially with Ed, for two years. At the time, both of them were in their early or middle 30's, and teaching very little. They were doing very intensive research and had lots of time available to talk about economics and academia.

The graduate program was small, with 8 faculty members, about 20 students, and a handful of courses outside the first-year core. The Carnegie Mellon economics community was tightly woven around the younger professors, who served as elder brothers to the rest of us. Grad students spent most of their time talking to each other about issues that interested them or the faculty. Some of these issues, like rational expectations, time consistency, and implicit contracts, later became very popular in the profession. There were lots of interesting things going on inside that little group.

Durlauf: Having examined your publications list, I take it that your dissertation focused on implicit contract theory?

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Azariadis: Yes. My dissertation was about implicit contract theory. It was inspired by some material in Jim Tobin's 1972 Presidential Address for the American Economic Association. In that address Tobin talked at some length about the nature of the labor market and how it differed from ordinary spot markets. Tobin believed the labor market did not react in the short run to the usual supply-and-demand shocks to current incomes, productivity and the like, but reflected instead some sort of implicit long-run "understanding" between workers and firms. After Lucas read this passage, he asked me during coffee break one afternoon to try to think about these "understandings" in a systematic way, preferably in the language of modern microeconomics. Were these responsible for peculiar labor market phenomena like layoffs and the reluctance of employers to reduce wages? That's exactly what I did, with some help from my advisors. And the end product was the theory of implicit contracts I developed in my dissertation.

Durlauf: I think that it is no exaggeration to say that implicit contract theory is one of the most successful examples of applying microeconomic reasoning to macroeconomic puzzles. When you first went on the job market, how did people react to the idea? I ask this particularly as my guess is that it was a controversial way to do macroeconomics, at least at the time.

Azariadis: Well, it was pretty unusual. At the time many economists talked about "the new macroeconomics," a buzzword describing micro-founded theories of the labor market like the search-theoretic approach of Stigler, Phelps, and Mortensen. The contractual approach was newer than search theory, and people knew very little about it. Today it spans economic theory, labor economics, finance, macroeconomics and industrial organization. Back then we knew nothing about key analytical concepts like subgame perfection and the revelation principle. We did not understand private information very well. And, to top it all off, the theory was being pushed by two very junior professors, Martin Baily and me. My own background was in engineering with little or no economics. Two years of economics was all I had done by the time my dissertation was almost finished. I was technically strong, but my economic intuition was underdeveloped. The upshot of it was that I did a poor job of selling my ideas in the junior hiring market. Fortunately, contract theory diffused quickly by word of mouth and soon attracted the interest of major departments, which put some of the early papers on their macroeconomics reading lists. In a few years, contract theory was being taught in almost every major graduate program, sometimes as an independent course, and more often as part of labor economics or macro.

Durlauf: Now, your first job was at Brown. Am I remembering correctly or misremembering?

Azariadis: You have it right. Brown offered me a job because the late Herschel Grossman, then a freshly promoted full professor, had a keen interest in the issue of layoffs versus wage cuts. He saw that implicit contracts had something to say about that and guessed that the two of us might make interesting colleagues.

Durlauf: Very good. And how long were you there before you went to Penn?

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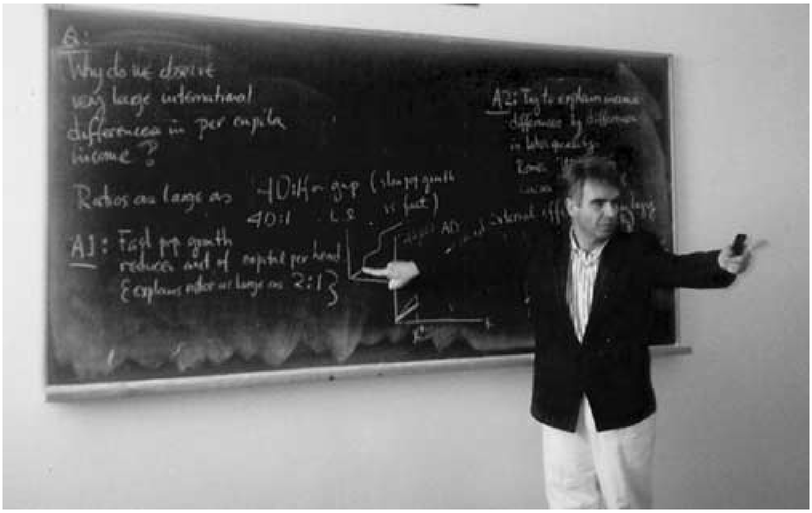

FIGURE 3. Teaching in Lisbon, 1989.

Azariadis: I was at Brown for four years, 1973–1977, before I went to Penn. The last semester of my term at Brown was spent in Jerusalem, visiting the Institute for Advanced Study.

Durlauf: How did the time at Brown affect your intellectual development?

Azariadis: It was a continuation, and perhaps the conclusion, of my short-lived doctoral studies at Carnegie Mellon. I spent most of my time with Herschel Grossman, who was looking for a substitute to Robert Barro. Robert, who had just moved from Brown to Chicago, had collaborated with Herschel on the macroeconomics of quantity rationing or "disequilibrium theory," as we then called that approach. Herschel abandoned disequilibrium and embraced implicit contracts. He wrote and published a couple of articles on contract theory, which kept us in close touch for the next few years. Young economists like George Akerlof, Stan Fischer, JoAnna Gray, and others soon joined the fray. I learned a lot about economics as I thought through the questions they asked me and each other.

Durlauf: So if I have the timing of your research trajectory correct, I believe the work on sunspots was the next major transition. Or do I have a gap in the history of Azariadis thought?

Azariadis: No, you have that right, too, although the Azariadis line of thought itself has many gaps (laughter)! I went from contracts into overlapping generations and multiple equilibria rather abruptly in the late 1970's even though I continued to work on contracts off and on until the middle eighties. In my last years at Brown I had began to look beyond the bilateral-exchange confines of contracts, for a more general methodology, a model that would serve as a common platform for lots of macroeconomics issues. It was clear to me, and many others, that the Keynes framework was too focused on the short run. Searching for an alternative vehicle

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in 1975–1976, I had come to the improbable conclusion that the most interesting macroeconomic model was overlapping generations or OLG. Neil Wallace had also reached the same conclusion about the same time.

The only problem was that few people, except Karl Shell and Dave Cass at Penn, cared much or knew much about OLG. When I suggested to Herschel Grossman that we had to bone up on that model if we wanted to do interesting work in macro, he asked how we should do that. To which I replied, only half in jest, that we had to move to Penn. And that is exactly what happened. I was offered a job at the Penn economics department in March 1977, moved to Philadelphia in August of the same year, and stayed there for the next 14 years.

The issue of multiple equilibria, which is a more serious term for "sunspots," came up first in May 1977 when I gave my first seminar at Penn. At the time I was working on the optimal degree of indexation for wage contracts in the general equilibrium model that Bob Lucas had described in his "Expectations and the Neutrality of Money." My paper was a Lucas island model with contracts in it. During my seminar Karl Shell asked "What is driving the business cycle in your model?" I gave the stock answer that cycles were due either to real shocks or else to nominal shocks that people confuse with real ones. Karl shook his head unsympathetically, and claimed that other things could also drive business cycles. He then asserted that business cycles may be driven by changes in beliefs. I found that a wild statement, as improbable as Keynes's fanciful stories about "animal spirits." I believed rational expectations had laid those stories to rest quite convincingly. At the time, Karl's comment didn't occupy much of my thinking.

Durlauf: What moved your thinking toward treating beliefs as a source of business cycle fluctuations? The idea that beliefs matter in this way presumably had been in the air for decades, given Keynes's animal spirits ideas, but you were able to substantiate this idea in a model, which I can say I teach to this day to students in Wisconsin. Can you talk a little bit about the progression of the research?

Azariadis: Yeah, that's the substantive aspect of the research at Penn in the late 1970's and early 1980's. There is also a somewhat less important social or personal dimension that has spawned several cute stories!

Durlauf: Well, I'm sure there's room in *Macroeconomic Dynamics* for both!

Azariadis: Yes. Each is part of the process of research. The substantive aspect is that you find sometimes, in real-world economic behavior, examples of collective overreaction, of very big or rapid changes in equilibrium outcomes that are not connected with big or rapid changes in fundamentals. The Great Depression is one potential example of overreaction to a reduction in the stock of deposits at provincial banks, and a corresponding shrinkage in money supply. This mechanism of falling money supply just before a decline in industrial production was a common feature of recessions before World War I; it is extensively documented by Milton Friedman and Anna Schwartz. Things seem to have gotten completely out of hand in the early 1930's when the destruction of deposits had a tsunami-like impact on production and employment.

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Everybody who was in graduate school, in my age group or earlier, was concerned about supposedly neutral monetary shocks with potentially disastrous consequences. There are also plenty of anecdotal examples of stock prices overreacting to mild changes in earnings, of prolonged recessions in developing economies after a mild devaluation, etc. Add to that the constant talk about various types of "bubbles" and you have a whole menu of small impulses with big responses in prices and output.

As you mentioned earlier, the notion that coordinated beliefs or animal spirits may be responsible for big economic movements, without some underlying switch in preferences or technology or policy that would justify such movements, goes way back to Keynes. So, yeah, it's a really old idea, but it was basically a cute story with no firm econometric evidence or solid theoretical grounding. The question we needed to answer at the time was whether animal spirits were consistent with rational expectations. Were "waves of optimism or pessimism" possible in an environment filled with rational decision-makers? Now, the way one generates new ideas is by questioning, refining, or rejecting old ones. Animal spirits in a rational expectations framework would seem to require multiple equilibrium paths in dynamic economies, whose laws of motion were thought to be uniquely defined saddle paths. I and many other people thought that rational expectations ruled out multiple equilibria; that was one reason that we regarded rational expectations as a vast improvement over adaptive ones.

But the issue of beliefs kept coming up in conversations between me and Karl Shell, mainly because he and I lived close to each other in West Philadelphia. We frequently walked home together from Penn, and used to debate about beliefs and other things. Karl had an early example of multiple equilibria in a 1977 French-language working paper. I found the idea interesting, but the example uncompelling. It was a static economy with flat indifference curves that became tangent to budget lines at many points. I told Karl his example was a fluke, something completely nongeneric. Rational expectations, I claimed, would surely rule out his "sunspots."

Durlauf: Which of course you later showed not to be the case!

Azariadis: To my great and everlasting surprise! Proceeding from the standard rational-expectations intuition, I told Karl in 1979 that I was going to write down a robust example of an economy with indifference curves that were not flat. That, I hoped, would persuade him that "sunspots" could not exist. I had not counted on large income effects, which permit many equilibria in static economies, and many laws of motion in dynamic ones. When I finished my example, it turned out that I had made Karl's case better than he had done. He was right, and I was wrong, for a large family of OLG environments. I thought that example was pretty interesting, so I dug deeper and deeper into it over the next few years.

Durlauf: Some economists have argued that the sunspots literature has not been that successful in terms of empirical work. And the way I would interpret this view is that there has been relatively little work that's substantiated sunspot ideas into a structural estimation framework. Do you think this is a fair criticism?

What are your reactions to comments like that? And, obviously, Roger Farmer is an exception because he's worked on trying to do precisely what I have asserted has not been done.

Azariadis: I think it's a fair criticism—the empirical work has surely lagged way behind the theory. The theorists, too, deserve some blame for continuing to investigate oversimplified or exotic environments. We relied for too long on enormous income effects, large global increasing returns to scale, and similar assumptions that our applied colleagues could not relate to. The upshot was that we all got roughed up by the profession for being too good at generating lots of rational expectations equilibria, and too weak at documenting them empirically.

In particular, we were unable to connect sunspot ideas with everyday events in financial markets, like excessive volatility in exchange rates and asset prices. We also failed to connect the multiple equilibrium literature to closely related econometric work by Hamilton and others on Markov switching processes, or to exploit nonlinear econometric techniques to test our ideas. Last, and not least, many of the principal contributors to the multiple equilibrium literature chose to spend much of their time staking paternity claims over existing results instead of looking for new ones!

Durlauf: How would you evaluate the state of the sunspot literature?

Azariadis: Initially we had a period of high excitement; it seemed that animal spirits were indeed consistent with rational expectations. After that there were some important advances in the direction of learning rational expectations by Guesnerie, Evans, and Bullard and in the direction of calibrating multiple equilibrium models to postwar business cycles by Benhabib and Farmer, and some related progress in the theory of incomplete markets by Cass, Mas-Colell, Geanakoplos, and Polemarchakis. Despite these advances, the field stalled as researchers busied themselves crossing t's and dotting i's, rather than adding raw ideas that would help us understand financial markets, big recessions, or even growth. It is not enough just to write down multiple equilibrium models. We need models that explain our environment better than conventional dynamic equilibrium models, or else I don't think the profession will see much use in our work.

Durlauf: Econometricians have not helped in the sense that I can't think of any case where a macroeconometrician has formally asked questions about the identification of sunspot models, issues of observational equivalence, and the like.

Azariadis: Yeah. These are all hard issues and econometrics hasn't helped, in the sense that a lot of the early sunspot models describe environments that linear econometrics is not particularly suited for. Nonlinear econometrics had not developed back then. As you know from your own work, Steve, we need nonlinear methods to test for convergence in economic development; the same holds for multiple equilibria. One good example of applied research that could benefit from nonlinear econometrics is the work of Roger Farmer on indeterminate business cycles. Indeterminacy in his class of linear models requires implausibly large and globally increasing returns to scale. In a nonlinear environment, it would be enough to have locally increasing returns like setup costs.

Durlauf: Yes.

Azariadis: It's very hard to find evidence for the assumption that increasing returns to scale are everywhere 1.3 or more, which is what you need to have for multiple equilibrium to occur in a representative-agent economy. It might be easier to find support for these returns locally in neighborhoods of the input space rather than everywhere.

Durlauf: I agree.

Azariadis: And so the lack of nonlinear econometrics hasn't helped much. The funny part is an item we discussed earlier: the multiple equilibrium and Markov switching literatures have failed to connect. Theorists are excoriated for empirical irrelevance at the same time, and often by the same persons who voice doubts over the connection of Markovian models with economic theory!

Durlauf: I think that is a good point because it interacts partially with data limitations. One can think about two ways to identify nonlinearities. One is parametric, in which case the literature tends to have functional forms that aren't particularly appealing with respect to the theory. And the other is to take a semiparametric or nonparametric approach, but that requires very long data series to provide any precision to estimates. So that's a tough nut to crack, as they say.

Azariadis: It may be a tough nut to crack, if you're concerned about business cycles, but maybe progress can be made for financial phenomena. We do have lots of data observations and very long time series regarding asset markets.

Durlauf: That's a very fair argument.

Azariadis: In any event, I for one see a very close connection between multiple equilibrium models and Markov switching processes. The very idea of multiple equilibria is that current economic outcomes depend on a certain array of fundamental historical factors, current random shocks, and a process of selecting equilibria. The term "sunspots" is shorthand for Markov switching variables, that is, for an environment with many regimes or laws of motion. The key idea in both literatures is that, in a dynamic economy, the present and the future are not connected just by the history, or the current values, of economic fundamentals but also by the uncertainty over *which* law of motion will prevail. That depends on a particular selection mechanism that could be estimated from time series data.

Durlauf: To link up with the earlier part of our talk, did Herbert Simon have any influence on you intellectually? The reason I ask is that issues of belief formation, etc., can be associated with some of his thinking.

Azariadis: No. Even though I actually took one course with Herb Simon, and his ideas were in the air, they weren't connected with the kind of economics I ended up doing. And that includes some work that Simon had done in the 1940's on the employment relationship. The similarities between that work and my own on contracts are more of a coincidence of terminology and less of a deeper connection. Herb mentored many people at Carnegie; unfortunately, I was not in that group.

Durlauf: Of course, you were in graduate school at the time when the rational expectations idea began to permeate economics. Given Simon's views, were

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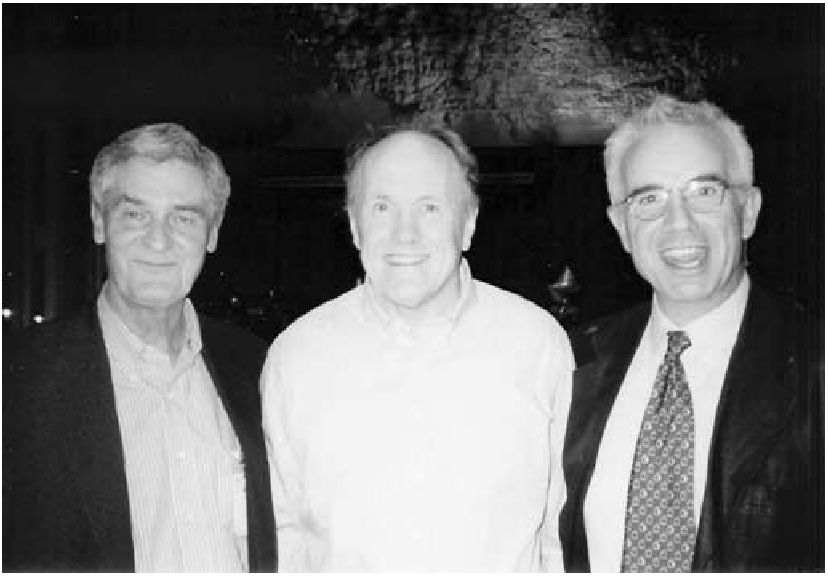

FIGURE 4. With Bob Lucas at Ed Prescott's Nobel Prize Party, Minneapolis 2004.

there palpable conflicts between the old master and the young rebels, Lucas and Prescott?

Azariadis: Behavioral economics had been popular at Carnegie in the 1950's and early 1960's because of older figures like James March, Richard Cyert, and Herb Simon himself. There was some friction between them and the younger set of economists trained in the neoclassical approach, but nothing more than the ordinary clash of generations. Students like me were on the periphery of the disagreements; we didn't know enough about people and ideas.

The more important disagreement took place outside Carnegie. It was between the neoclassical group of Lucas, Prescott, and Sargent on the one hand and the Keynesian establishment on the other. When I went to Penn in 1977, some of my older colleagues viewed me as an enemy agent. One of them said to me pretty emphatically "Lucas is wrong!" just a few days after I had arrived. Later, as I met some of the protagonists on the Keynesian side of the macroeconomics debate and observed the debate unfold, it was clear that Bob Lucas and Ed Prescott were indeed the rebels. They were challenging the old Keynesian verities, taking some flak and plenty of unjustified hostility as they confronted the other side. The new ideas, however, were powerful and compelling. The rebels prevailed, and that became a lesson for young macroeconomists.

Durlauf: Looking at the current state of macroeconomics, you were very much present at the creation, working with Lucas and Prescott at such an early stage of their careers. Are there some recollections you would like to share? Did they realize they were producing an intellectual revolution?

Azariadis: Bob and Ed should be answering this question instead of me. All I can give you is the eyewitness testimony of an untrained graduate student with a 35-year memory lag! Those were at once painful and heady days, with the Vietnam War slowly winding down and the economy stagflating. The Keynesian paradigm was in a state of jeopardy, perhaps even of collapse, along with some other things that people took for granted. I, for one, was not aware of a revolution in economic methodology, just of young researchers like Phelps, Lucas, Sargent, Prescott, and some others digging for an alternative to the IS/LM framework of Keynes and Hicks. Search theory was a hot new idea then, and so was the concept of rational expectations, even though that was a direct extension of the perfect foresight concept, which had found wide use in the neoclassical growth theory of the 1960's. The real revolution was very hard to predict then, at least for me. Who could have guessed in 1970 that the Keynesian framework would be displaced by growth-theoretic models, even in the analysis of short-run issues like business cycles or asset price fluctuations?

One story from that period captures how suspicious the profession was of these newfangled ideas. Bob Lucas had submitted a draft of his "Expectations and the Neutrality of Money," which quickly became the signature paper of the RE revolution, for publication in a major professional journal. The paper was turned down, and eventually published in *JET* because, among other things, the editor of the more established journal could not fathom why Bob had to use functional equations in macroeconomics. When I met that editor two years later, I advised him (on the basis of one semester's service as an academic economist!) that he had made a pretty big mistake in rejecting my advisor's article. Thirty-two years later he sheepishly admitted to me that he had been mistaken. The occasion was an AEA reception honoring Bob Lucas's Nobel Prize.

Durlauf: Your longstanding interest in multiple equilibria has continued, albeit in the very different context of inequality and growth. How did this evolution in your work come about?

Azariadis: That actually relates in a backhanded way to the work I had done earlier on implicit contracts. Sometime in the middle 1980's I started thinking about dynamic contracts. I wanted to understand how workers accumulate human capital in environments where they cannot borrow or buy income insurance. Would these market imperfections condemn them to permanent poverty?

I published a paper on dynamic contracts in the late 1980's but I was not happy with my progress. I started discussing things with Allan Drazen, who was visiting Penn at the time; we also debated the old Rosenstein–Rodan issue of industrialization takeoffs and what it takes to get a poor economy going. Did that have anything to do with achieving "critical mass" in the amount of human capital? The data seemed to suggest that human capital was crucial in achieving economic miracles. More broadly, were economic progress and economic development linear processes? Or were they processes that first build up some steam and then charge ahead? Allan and I then looked at a bunch of extremely poor African countries and saw the problems they had achieving a minimum level of skills in human capital.

We thought of human skills as the "steam" of a development engine that cannot get going until steam pressure reaches a critical value or "threshold." That was the idea of locally increasing returns to human capital. In effect, we were doing a very special sort of endogenous growth theory.

Durlauf: This may be an odd question. Do you think the fact that you grew up in another country and then came to the United State had any bearing on the way you think about these issues?

Azariadis: In a very natural way. Greece had been ravaged in the 1940's by WWII and then civil war. Economic growth was palpable and rapid through the 1950's and 1960's. Standards of living improved visibly. Growing up in a small country also keeps you aware of what happens in other parts of the world. When I started visiting other countries in the course of my academic career, I found pockets of poverty that I never thought were possible. Once you see what happens in Africa, and even Latin America, it's hard to put the issue of persistent poverty out of your mind.

Durlauf: A paradox in the growth literature is that the interesting and, I think, most compelling new growth theories are supportive of nonconvexities, market imperfections, and the like, whereas most of the regression literature purports to support linear Solow-type models. From your perspective, what do you think is the most compelling evidence in favor of the divergence, nonconvexity, multiple-steady-state perspective?

Azariadis: The most important thing is to look at the broad facts. We know what facts we regard as important: those that attract the abiding interest of good growth theorists and visionary policymakers. And the most important fact for me is sub-Saharan Africa, the largest and most persistent pocket of abject poverty in the world. Actually, any sample of countries that's not dominated by OECD, East Asian, or Southeast Asian nations will show no evidence that the poorest nations are catching up with the rich ones. The data from 1960 to 2000 come down decisively against convergence; rich countries have been growing faster than the world average, if one ignores population weights. John Rawls would and so should we. If you take out of your sample a small group of populous countries, like Japan, Korea, China, India, Turkey, and a few others, the evidence for nonconvergence for Africa and Latin America in the last 40 years is overwhelming and heartbreaking. Any growth theory we teach our graduate students, and our undergraduates for that matter, must explain growth disasters as well as growth miracles. Persistent poverty is an important fact of everyday life for roughly one billion of our fellow humans. Growth theories that cannot come to grips with that fact are not worth teaching.

Durlauf: What thoughts do you have on the state of monetary economics? To be honest with you, I'm asking this partially because it strikes me as a puzzle that monetary economics is less prominent within macroeconomics now than it was, say, 40 years ago.

Azariadis: Monetary economics used to be much more active when people like Milton Friedman were heavily involved in macroeconomics and monetary policy. With 20/20 hindsight, we can find a few reasons that this receding interest in money came about. Friedman and other monetarists believed that money exerted powerful influence over both inflation and short-run economic activity at business cycle frequencies but affected only prices in the long run. In the short run, monetary policy would be non-neutral, working through credit markets on the volume of borrowing and lending. Monetarists had in mind an economy with serious credit market imperfections that monetary policy can mitigate or worsen. One example is borrowing constraints that tighten and long-term credit that's shut off at high rates of inflation. But they could not articulate their view of money and financial markets in the neoclassical language favored by the younger generation of macroeconomists.

Sidrauski, Brock, and the generation that started working on money in the late 1960's felt more comfortable analyzing simpler economies than the monetarists had contemplated. They assumed perfect foresight, perfect markets, and "helicopter drops" of money to cheering households. Money is completely powerless in these environments unless you use it to finance fiscal deficits. That may be a good reason for the waning of interest in pure monetary theory, as distinct from credit markets, and for deemphasizing monetary theories of the business cycle. Notice that central banks and central bankers are to this day firmly wedded to simple Keynesian or neo-Keynesian models; they have not embraced the neoclassical viewpoint.

Durlauf: Please continue.

Azariaidis: Still, I don't fully understand how this situation came to pass. In my mind there remain two very important monetary issues of concern to all macroeconomists. One is the deeper issue of circulating exchange media. What types of promises are generally acceptable in exchange, what kinds are not, and why? Whose IOU's are generally acceptable, and why? For example, American Express and other large international financial institutions issue IOU's that move around the world; IOU's by you and me are generally acceptable only to our family and friends. What makes American Express so different from you and me?

The other important issue is monetary policy. What are socially desirable operating rules for a central bank? This is a question that Milton Friedman asked in his own imperfectly articulated theoretical framework and came up with a k-% rule or, equivalently, a zero-inflation prescription. People who were gifted at manipulating neoclassical dynamic models found that a socially optimal monetary policy requires a zero nominal rate of interest and negative inflation. Neither rule comes close to what we observe in real-world economies with fiat money. The best central banks in our world seem to pursue inflation targets around 2% in rich countries, and close to 5% in developing ones. Friedman did not get realistic answers because his framework was too idealized and not fully articulated. Let's ask his question again in a fully articulated but less idealized framework. What would the answer be?

Durlauf: Work on monetary policy rules is certainly an argument against my assertion about monetary economics in general. At the same time, perhaps the point is that the richness of the microeconomic foundations of modern macroeconomics is higher in other subfields than in monetary economics in the sense that it has proven uniquely difficult to develop microfoundations for money that reflect the complexity of advanced economies.

Azariadis: We have troubles getting money to our models in ways that are empirically plausible and logically coherent at the same time. So if you're a central banker and you ask an academic economist a question about monetary policy, you're likely to get an answer that is either well micro-founded or policy relevant, but not both.

Durlauf: How do you view behavioral economics from the perspective of macroeconomics? Behavioral economics has of course become a very active area. Do you see it as useful for understanding aggregate phenomena?

Azariadis: Now, there are some phenomena, like bubbles and the mispricing of assets, which seem very difficult to explain without putting some restrictions on the ability of traders to arbitrage rate-of-return differences. We have two basic ways to limit arbitrage: restrictions on short sales and restrictions on information or knowledge. A crude way to put it is that some people know there are opportunities out there but they cannot take full advantage of them because they cannot borrow enough, they can't short their existing portfolio to take a long position in this new asset they want to buy. There are others who could borrow and go short, but they're not smart enough to know that the specific arbitrage opportunity is available.

Concepts like bounded rationality and behavioral finance are perhaps essential ingredients if we are to understand some anomalies in financial markets. Having said that, I would add that I personally prefer to explain as much as I can without bounded rationality, without behavioral finance, and to reserve these tools as a last resort when all else fails.

Durlauf: How do you view the new institutional economics and specifically the effort to introduce institutions in the growth context?

Azariadis: Steve, you know how strongly efficient governance, contract enforcement, and well-defined property rights correlate with high standards of living. It should not surprise you then that I am very much in favor of including some *tractable* institutional variables in the theory of economic growth. By "tractable" I mean something less ambitious and intellectually less satisfying than the institutional theories of Doug North or the political economy models of Acemoglu and Robinson. For example, it would be a relatively simple thing to incorporate some aspects of bankruptcy into our modeling of credit markets and then study how economic growth is influenced by the property rights of lenders, as measured by the size of the penalty for default or some similar parameter. The harder task is to generate from first principles a description of institutions and institutional change over time. How do we choose to organize our affairs as a society? Who votes and who does not? What are the boundaries between public and private property? How do we pay for our government? These are hard questions. That doesn't mean we should not ask them or that they are irrelevant for growth. It just means that answers will come slowly and in a very piecemeal way.

Durlauf: You said earlier that you moved to Penn in 1977 to learn about the overlapping generations model. You have since used that model in most of your papers and in your graduate text. Has the OLG model been as influential in the profession as you expected it to be in 1977? If not, do you have a sense why?

Azariadis: Ouch! This one hurts. Maybe I should have stayed at Brown (laughter). To be perfectly frank with you, Steve, the OLG model has not evolved as rapidly, or become as useful for applied work, as I expected back then. I am fully aware that many good economists have found OLG well suited for analyzing low-frequency phenomena in fertility and growth, and almost indispensable in discussing social security and other fiscal policy issues related to the life cycle. The model also has seen some use in asset pricing and international finance. The bottom line is that, despite its life-cycle realism, the OLG model does not come close to the neoclassical representative-agent model in popularity, and it may be less popular than the current crop of neo-Keynesian models that stress staggered prices and monopolistic competition. It is not easy to grasp how the profession ended up valuing so modestly a model with the potential of the OLG vehicle.

Durlauf: But you must have thought about this issue!

Azariadis: Quite a bit, as we often do when our forecasts go awry. One useful observation is that OLG never became a serious vehicle for business cycle research or asset pricing. It was scooped in the early and middle 1980's by the real business cycle, or representative-agent, model that soon became dominant in the medium-to-high-frequency issues that make up most of modern macroeconomics. As more and more people adopted the RBC model, less research effort was directed into refining the OLG model, into adapting it for the study of high-frequency issues. The model stalled about the same time as research on multiple equilibria did. Most people did not use OLG because they correctly expected that most other people would not. "Sunspots," however, do not satisfy me as the compete answer to the woes of OLG. There is some fundamental question to be asked, too: Why was the talented OLG crowd unable to move away from two-period lifecycles and make the model more attractive for applied work? By "attractiveness" I mean fully articulated descriptions of equilibria in environments like those simulated by Auerbach and Kotlikoff, with lifespans of 55 periods or more that correspond to annual frequencies or higher.

To be fair, research did move a bit in that direction. Balasko and Shell around 1980, and then in 1985 Kehoe and Levine, in much greater generality, studied existence and optimality of equilibria with arbitrary deterministic lifecycles. That was clearly not enough for applied work. Also in 1985, Olivier Blanchard wrote a great article on OLG with stochastic lifecycles; he laid down an eminently tractable model that the profession never picked up. After that, there was the usual wrangling about who had proved what when, but little real progress.

Right now, OLG is in a state of deep hibernation. Few people work on the theory, even though the number of users is large and growing. Young macroeconomists know very little beyond Diamond's two-period version. Few people are aware that when the lifecycle goes to three periods or more, wealth distribution among

https://doi.org/10.1017/S1365100507050390 Published online by Cambridge University Press

generations gets interesting, and dynamical equilibria become very different from the Diamond model. One day people may rediscover this model. In retrospect, it is possible that my 1977 guess about OLG was not deeply wrong. But my timing was surely way off!

Durlauf: Could you describe your philosophy of dealing with graduate students, how you train them and the like?

Azariadis: I try to follow with graduate students the habits I learned from my own teachers from the day I went to grade school. When I needed help in understanding issues and concepts, my teachers often gave me unlimited time access and a sympathetic ear. This was completely true of my advisors in graduate school who did spend gobs of time trying to get me going in my dissertation. It applies equally well to my grade school teachers who took pains to explain compounding, discounting, and the lessons to be learned from the Trojan War!

And that's the philosophy that guides me with my own students. My function is to lay before them some issues that I think both are interesting and also agree with their own tastes, and gently to guide them away from problems that are already solved, or are passing fashions, or just plain uninteresting. When the research gets going, and the ideas start gelling, my job is to act as a sounding board as often as they need me for as long as they need me.

Durlauf: Can you talk about the process by which you generate ideas for papers? There are two things that I wanted to ask you to address. One is the ability to identify the sort of big facts that are puzzles. Second, there's almost an aesthetic quality that you model extremely cleanly. I mean, I read one of your papers and think, this is the simplest structure needed to communicate an idea, as opposed to a Rube Goldberg behemoth!

Azariadis: Well, I don't know about style but it grows out of what one reads. Mine comes from reading the work of people like George Orwell and John Hicks who are parsimonious thinkers and parsimonious writers as well. That's real important. I don't think I can describe in an organized way how I picked the problems that I chose to work on. Maybe it was by osmosis. Research problems are a bit like investments. How do you pick an investment opportunity?

Durlauf: I wish I knew an algorithm!

Azariadis: It's difficult to describe how you did it at the time. Perhaps it is an art. But it is a little bit easier to explain the process ex post. We know now that any good macroeconomic theory must be able to explain *simultaneously* a list of fundamental observations about economic growth, business cycles, and asset markets. That list includes, but is not limited to, growth miracles and growth disasters, the moments of detrended time series in rich and less developed lands, the impulse responses to important cyclical shocks, the risk-free rate, the equity premium, stock market volatility, the home bias in international asset portfolios, etc.

I do know that once we identify an interesting problem in macroeconomics, it often remains unanswered for a long time. We go through a process of trial and error, using an array of models that we progressively strip down, without getting to the point where the model cannot handle the question you're asking. Einstein once said that models should be made as simple as necessary, but no simpler.

Durlauf: Can you describe some of the people in your career that most helped you intellectually, in terms of both the generation of ideas, as well as, frankly, the opportunities that are necessary to get your work known? Of course, you've already talked about your advisors.

Azariadis: There are lots of them, teachers, mentors, interlocutors and collaborators, all of whom contributed to my thinking. I am not sure what I contributed to my thinking (laughter)!

Among the teachers I think I owe much to my paternal grandfather Gregory and to Mr. Spyropoulos, my Athens schoolteacher from second through fifth grade. They taught me basic math, language, and history and passed along some values that I still hold dear. Among mentors that I have not mentioned before, Bob Solow was probably the most important. He used to send me written comments about my research and books. His letters were encouraging, substantive, and humorous. He'd begin with a joke, end with another one, and in between he packed much sensible advice.

There was also a group of interlocutors, people I used to talk to fairly often in the late 1970's and early to middle 1980's. They taught me by osmosis. I learned about overlapping generations from Karl Shell and Dave Cass, about private information from Sandy Grossman, Joe Stiglitz, and Russ Cooper, about nonlinear dynamics from Pietro Reichlin, Jess Benhabib, and Roger Farmer.

After 1980, most of my work became collaborative. I worked with colleagues whose training was very different from mine, and whose outlook was also different. I learned much from them, especially from Roger Guesnerie about expectations, the late Bruce Smith about money and credit, Oded Galor about growth, Jim Bullard about lifecycle economies, Leo Kaas about debt constraints, and many other people.

Durlauf: Can you give me your thoughts on the social structure of the economics profession? I am thinking here of how journals function, research grants are allocated, etc.

Azariadis: New ideas in economics are a public good produced by a worldwide group of researchers like you and me, paid from a mixture of public and private funds. Some researchers are new in their field and their contributions are not easy to evaluate. Given the number of fashions and fads in economics, it is sometimes hard to make up one's mind even about mature researchers with well-defined research agendas. How can we tell if a currently popular approach will last or vanish?

Ideally we want a social structure that aligns as much as possible the private interests of the researchers with the public interest in scientific progress, and reveals as much information as possible about the quality of individual researchers and their agendas. We want to encourage true innovators with grants and publications in top journals, and discourage crackpots, but sometimes we cannot tell who is who. Crackpots often claim to be innovators. This is a standard adverse selection

https://doi.org/10.1017/S1365100507050390 Published online by Cambridge University Press

problem with an established solution: punish self-declared innovators to discourage the crackpots. True innovators, says this solution, will take the punishment, persist in their ideas, and will eventually become accepted. That's the theory of it.

In practice our profession is organized in "clubs," overlapping informal associations defined by your narrow field of interest, methodological bent, doctoral institution, etc. Time-series econometricians, MIT Ph.D.'s, and freshwater macroeconomists are examples of such clubs. Clubs collect information about their members'qualifications and dispense rewards (publications, research grants, conference invitations) according to competence, seniority, and loyalty. My experience is that club loyalty pays too well; too many goodies are directed toward intellectual followers. As a society of scholars, we don't want that. On the other hand, it is human nature to be selfish. Researchers are not social planners, just human beings with kids to raise, tax bills and mortgages to pay. And so, in the course of our everyday struggle to make ends meet and to advance our careers, some of the larger social values survive and some fall away. The end result is that we do not support intellectual innovation as much as we ideally should. We guard too stubbornly against nonstandard ideas, and reward handsomely the *i*-crossers and the *t*-dotters. That's unfortunate, but it's human nature. We've got to live with it.

Durlauf: That's right. We do believe in . . . *homo economicus* . . . to some extent!

Azariadis: Indeed we do.

Durlauf: Do you have any regrets in your career intellectually, in terms of directions not taken?

Azariadis: No. I don't have regrets about directions. I think I was lucky in my choice of subjects to work on. I was able to recognize a good idea when it entered my radar screen. I was equally lucky in the choice of collaborators and interlocutors. But if I had to do things all over again, I would not emulate the Lone Ranger as much as I have. I'd be more careful to build up a social network of like-minded people, a "club" if you like, before I engaged in serious research projects, and certainly before I tried to sell new ideas to the profession.

Durlauf: So if I ask you to speculate a bit on what you think are particularly promising new research areas in macro, are there some that come to mind? It's somewhat of an unfair question, of course, since almost by definition, if you have them, that's what you're working on.

Azariadis: No, it's a fair issue to raise, and a valid one, too. I am not sure my answer will be up to your question. Part of it goes back to your earlier query about "big facts" as scientific puzzles; I would add some big policy issues to that list. Whatever those key facts and issues are, they deserve a piece of our attention, perhaps the bulk of it. After all, our job as theorists is to serve applied economics, to help our colleagues understand a little more (just a little, mind you!) about the world we live in.

Another job we have is to keep the tools of the trade in good working order and to invent new ones when we need to. By "tools" I mean a logically coherent

FIGURE 5. At home in Amphissa, Greece, June 2005. Photographed by Bryan Ellickson.

and self-contained mathematical model or class of models, with a minimum of institutional assumptions. The only set of tools I see on the horizon that comes close to filling that bill is the theory of dynamic general equilibrium (DGE) as described in the work of Ed Prescott and his collaborators.

My highest priority now is to help improve these tools of the trade to the point where they really connect with the "big facts." This point will be reached when we have built a simple refinement of the DGE structure that weakens one or two of the strong Arrow–Debreu assumptions in the current version, and succeeds in matching key facts. Examples of such facts are basic asset market anomalies, slow growth in Africa, enormous fluctuations about trend growth in less developed economies, the response functions of GDP to basic monetary and technology shocks, the operating characteristics of inflation targeting monetary policies, and a few others.

To carry out this agenda we'll need to identify *which particular* classical assumptions we choose to give up, to work through the changes the new set of assumptions will imply for the DGE structure, and to take the end product to the data. That's a pretty tall order and one that'll keep many of us occupied for a while yet. I don't know where to begin, but two changes that seem particularly promising to me are to move away from rational expectations (as suggested by Evans and Honkapohja, Bullard, and others), and from fully enforceable intertemporal trades (as suggested by Kehoe and Levine). Each of these twists will complicate the relation between demand and prices by allowing expectations or debt limits to shift when current prices change. My own current research with Leo Kaas bets on debt limits. However, there are *many* classical assumptions in DGE, and we don't know yet which one is responsible for the poor data fit of the current crop of models. Producing a superior second vintage of DGE models is sure to take a great deal of effort from all of us.

Durlauf: What advice would you like to communicate to aspiring economists, specifically current graduate students?

Azariadis: Academic economists are paid pretty well, work flexible hours, travel a lot, and deal with an intellectual subject matter. Young people choose our profession for many reasons: income, life style, or intellectualism. Most of those who do it for the money could probably do better consulting, working on Wall Street or for some other private employer. Our life style is good but not as exciting as that of a writer or artist. My real advice goes to the type of colleague I value most—the somewhat irrational group of intellectuals who are in the business because they *must know* how the world works, and are willing to spend endless hours figuring things out. A friend of mine once coined the term "ether-walkers" for this class of person. Well, if you are an ether-walker, think about important issues long and carefully, then take intellectual risks as big as these issues need and your talent can handle. Your profession will esteem you if you fail and will reward you handsomely, albeit not immediately, if you succeed. One question you'll be posing to yourself at the end of your career will be "Have I added anything to the tree of knowledge?" Do all you can to make the answer a yes.

Durlauf: Thank you, Costas.

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