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MD INTERVIEW

AN INTERVIEW WITH KARL SHELL

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Karl Shell is without a doubt one of the central players in the development of economic theory and macroeconomics in the latter part of the twentieth century. He has made important contributions on topics ranging from growth theory, to overlapping generations, to extrinsic uncertainty, to monetary economics, to market games, and to technological innovation. His collaborations with Dave Cass are legend, and include the seminal formulation of the concept of sunspot equilibria. His "Notes on the Economics of Infinity," and his papers with Yves Balasko are overlapping-generations classics. Shell's many coauthors read like a Who's Who in economics, and include (in no particular order) Joe Stiglitz, Franklin Fisher, Miguel Sidrauski, Ned Phelps, Duncan Foley, Walt Heller, Albert Ando, Jim Peck, Rod Garratt, Aditya Goenka, Christian Ghiglino, and Todd Keister. This is also a club to which we are also proud to belong.

Shell's research is first rate and highly innovative, but his full contribution to economic theory must also be judged by his long service as the editor of one of the profession's premier journals, the *Journal of Economic Theory*. As the founding editor of *JET*, Shell took a small upstart journal, originally envisioned as a niche outlet for papers on mathematical economics, and turned it into one of the best in the profession. *JET* has published papers that have had major impacts on the development of economic theory in all its various flavors.

Because of Shell's long stewardship of *JET*, the story of his professional life—which we hope we have captured in this interview—is also a history of the evolution of economic theory over the past quarter century. We spoke with Karl in his office at Cornell University and over lunch at the faculty club in the Statler, and the interview tapes we made were very much a three-way conversation between Karl and the two of us. In editing the transcripts of these tapes for publication, we wished to keep the focus of the conversation on Shell, and have therefore adopted

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the same anonymous "MD" moniker we used in our interview of Dave Cass to mask the identity of the questioner. We hope you enjoy the conversation as much as we did.

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MD: How did you become an economist? Didn't you start out in mathematics?

Shell: Actually, I enrolled in Princeton [in the fall of 1956] as an engineer in deference to my parents. They had not gone to college and they wanted me to have a marketable skill. Engineering did not suit me. I transferred to math as soon as I could. I also took courses in economics. And some of the math I did at Princeton is close to economics—things like linear programming and game theory. So my transition from college to grad school was relatively smooth. Probably smoother than if I had been an economics major. Certainly much smoother than the transition from my high school to Princeton.

MD: Tell us more about Princeton.

Shell: My life centered around Fine Hall, which houses the math department and the math/physics library, which were open all hours. I took classes from Feller, Tucker, Wheeler, Wilks, and other greats. I wrote my junior paper for Ralph Gomory, the integer-programming wiz, and I did my senior thesis for Harold Kuhn, the game theorist.

I took economic theory from Will Baumol. I met Dan Orr in Baumol's excellent grad course. Dan became my friend and mentor. He got me a summer [1959] job doing inventory control for Procter & Gamble, the Cincinnati soap company. He took me on a tour of Oskar Morgenstern's research project on Nassau Street, where I met Clive Granger. Clive gave me some of his technical reports, which contained reviews of the time-series literature on the possible economic effects of real-world sunspots *à la* Jevons. Perhaps this was the seed that germinated into the limiting case of purely extrinsic uncertainty, i.e., stylized sunspots *à la* Cass–Shell.

As part of Baumol's course, I was exposed to Arrow's monograph and papers. Arrow was then listed in the Stanford catalogue under three academic departments, so I sought his advice on which program to apply to. He advised me to apply to the Economics Department.

MD: Tell us about the people at Stanford when you were a grad student.

Shell: The first economist I met at Stanford was Kenneth Arrow. He made a tremendous impression on me—as he does on everyone. I have been in awe of him since. He is a warm and gentle man, but—as you know—his intellect is like lightning. I was assigned space at Serra House, the quarters of the IMSSS [the Institute for Mathematical Studies in the Social Sciences], then the combined research group for the mathematical economists (led by Ken Arrow) and mathematical psychologists (led by Pat Suppes). The house itself sat off in the woods. It had been the

FIGURE 1. Karl Shell, Cornell University, about 1990.

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residence of Stanford's first president (raided, by the way, from Cornell). I spent four happy years [1960–1964] in Serra House. I shared an office with (among others) Menahem Yaari. Manny was a role model, mentor, and friend. I participated from the start in the Serra House workshop on mathematical economics and econometrics run by Ken and Marc Nerlove. Manny and Ken-ichi Inada were among the regular participants during my first year. Herb Scarf was an occasional participant. Hiro Uzawa was on leave at the CASBS [Center for Advanced Study in the Behavioral Sciences in Stanford, California]. At the time, I found the workshop to be very stimulating. In retrospect, it was sensational! Manny presented some of his work on the consumption-loan model, my first exposure to OG [overlapping generations]. Marc presented a wide review of work on expectations, including a careful rendition of Muth's paper on rational expectations. This was before RE had become widely known. Ken gave a first draft of learning by doing. His seminar was my first exposure to modern growth theory, certainly my first exposure to endogenous technical change, and my first serious exposure to the calculus of variations.

Meanwhile, at History Corner (where the Economics Department was based), I was taking from Bob Slighton a cutting-edge Patinkin-driven course in macro. From Slighton and Patinkin, one clearly got the idea that macro and micro were related subjects. Ed Shaw taught a low-tech, but very stimulating, follow-up macro course based on the Gurley–Shaw financial stuff.

During our first week of classes, I met David Donaldson, a fellow graduate student. We became friends. We shared an apartment. We had wonderful conversations about political economy, philosophy, and economic theory. David, George Fishman (another grad student), and I organized an informal weekly seminar with Arrow based on Herman Kahn's book on thermonuclear war.

During my second year at Stanford, I took courses in public finance and the economics of uncertainty from Ken and econometrics from Marc. I also met and courted Susan [Schulze] that year. We were married at All Saints' Church, Palo Alto. David Donaldson was my best man. Ken Arrow and Marc Nerlove were there. Winning over Susan was the best move of my life.

At about this time, Arrow set off for Washington to serve a stint as the senior staff member at the Council of Economic Advisers. He invited me to join him after the end of spring classes as a CEA summer intern. Of course, I accepted.

MD: Was Serra House where you and Dave [Cass] first met?

Shell: Dave and I were together in some classes, but somehow not that many. At any rate, we didn't do much together during our first 2 years at Stanford. Sometime during our third year, I think it was, we stopped to talk when our paths crossed at a relatively remote part of the campus. Dave complained about the lack of rigor in some of our courses. He was looking for a way to do more rigorous economics. I promised that I would mention his name at Serra House. I did. Herb or someone found a desk for him. I got to know Dave much better. We shared ideas, beers, and laughs. It was fun to be with a smart guy focused on economic theory.

At about the same time, probably earlier, Arrow was planning a year at Churchill College. He invited me to join him in England [for academic year 1963–1964].

I was deeply honored and excited about the possibility, but I was too fiscally conservative for my own good. Susan had taken out a personal loan to buy a used Volkswagen. We couldn't figure out a way to finance the trip to England and pay off the loan, so I stayed behind at Stanford to teach intermediate macro (Larry Lau took this course), mathematical programming, and political economy. I could kick myself for being such a jerk!

Meanwhile Hiro Uzawa had formed a group of students—including Dave, Steve Goldman, and Harl Ryder—working on optimal growth. Dave—after checking with Hiro—invited me to meet with them. I am grateful to Dave and Hiro. This is where I learned two-sector growth, Pontryagin's maximum principle, and more. Hiro's marathon seminars were memorable, the stuff of great stories that improved over time. There was usually mandatory after-seminar beer drinking at the Oasis (a bar in Menlo Park, near the Stanford campus). When the seminar moved with Hiro to the University of Chicago [in summer 1964], the beer drinking moved to Jimmy's (a Hyde Park bar near the Chicago campus).

MD: Tell us about your thesis and how it relates to your published articles. You wrote on inventive activity and growth. Were you doing new growth theory before new growth theory?

Shell: My dissertation was inspired by two Arrow articles—learning-by-doing and especially his chapter in the NBER volume on inventive activity. I put forward a macroeconomic model of inventive activity and growth, with two conventional factors, capital and labor, and one, new, nonconventional factor, technological knowledge. If there are constant returns to scale in the conventional factors, then, unless there are perfectly offsetting external dis-economies, there will be increasing returns to scale in all factors—conventional and nonconventional—taken together. This has two important implications: (1) Economic development is likely to be history dependent. (2) Purely competitive finance of inventive activity is not possible since the competitive rewards to factors overexhaust output. For the 1966 *AER* paper and Chapter IV in the 1967 MIT Press book, both drawn from my dissertation, I assumed that R&D is financed by the government—although not necessarily undertaken by the government. Depending on initial conditions and other parameters of the model, the economy will enjoy explosive growth, suffer from implosive contraction, tend to a steady state, or tend to a limit cycle.

In the chapter in the 1973 Mirrless–Stern IEA Jerusalem conference volume, which is suggested by my dissertation but goes beyond it, I focused on the role of industrial organization. One extreme is the purely competitive case with government finance of R&D—my thesis. The other extreme, a monopolist who controls the production of ordinary goods and research, is analyzed in the 1973 work. Sometimes it is optimal for the full-commitment monopolist to withhold capital and/or technology from production. The most important case treated in the 1973 paper is the intermediate case in which monopolistically competitive firms finance their R&D from the quasi rents they earn from their (temporary) technological advantages. Restrictions on the financing of R&D play an important role in the 1973 work.

MD: Let's talk some about what has happened since then in this area. What do you think of the new growth theory? Do Romer and Lucas acknowledge your early contributions?

Shell: I love the new growth theory. I applaud the focus on technological evolution. I like the emphasis on empirical facts.

Romer's three most important papers draw heavily on my work. Paul very clearly cites my three papers. I don't think that Lucas has cited this work much. One always likes a cite from Bob, but there are substantial differences between his stuff and mine. His models are based on Uzawa-like "human capital" as opposed to "technological knowledge." So Bob's focus was on education broadly defined, while mine was on invention. External effects are important in both the education and invention models. For Romer and me, issues about the appropriability, excludability, and nonrivalry of technological knowledge are basic. The effects of technology on industrial organization and the effects of industrial organization on technology are also central for Paul and me. Bob's problem is simpler: There can be positive spillovers from private human capital formation. There are no deep increasing-returns problems. Industrial organization is not central.

MD: How does your early work differ from Paul's, then?

Shell: The Romer 1990 model is based on patents. My 1973 model is driven by the short-run technological advantages enjoyed by advanced technologies. Most students of technological evolution do not think that patents are the basis for the lion's share of technological improvements. On the other hand, Romer combines patents and vintages to make a fully Walrasian theory. My early work on monopolistic competition is a bit too "Marshallian" at the edges. Romer was not alone in this period. Phillipe Aghion and Peter Howitt, Gene Grossman and Elhanan Helpman, and others also did very important work that refers back to my stuff from the sixties and seventies.

I do believe I was doing new growth theory before new growth theory. Why isn't my stuff better known? There are several reasons: (1) Most of the growth guys in the sixties were looking for Solow stability. I was pushing morphogenetic (history-dependent) models. (2) It certainly didn't help that two of my three papers were published in places that were hard to find in the eighties and nineties. (They are now available at www.karlshell.com.) (3) I dropped out of endogenous growth to work on other topics. Good ideas do not make it on their own. A continued presence—which I did not provide—is important. (4) Some plausible versions of my models allow for overly optimistic (contraempirical) growth. To temper this, one must add some antigrowth "frictions." For this, Romer introduced effort wasted on copying and near-copying. There are also plausible political-economy frictions that one might employ: Governments see in new technologies new sources of political power and revenue, thus dissipating the potential productivity gains.

MD: Have you done anything in this general area since the seventies?

Shell: Two things. One is a macrodynamics exercise based on the technologies I studied in the sixties and seventies. The second is an attempt to build a useful micro model of technological evolution.

MD: Is the micro paper the one on production recipes?

Shell: Yes. The micro theory of technological evolution has not kept up with the macro. This is because the neoclassical theory of production is incomplete. In a recent *JEDC* paper with Phil Auerswald, Stu Kauffman [the evolutionary biologist], and José Lobo, we put forward a micro theory of technological evolution in which production is described by inputs, outputs, *and recipes* (or blueprints). Our recipes are in the spirit of Dick Nelson, Ned Phelps, and Sid Winter. We use our recipes model along with some combinatorial math from biology to explain observed empirical learning curves. This requires extensive computation. Production runs for particular products are not very long. Hence asymptotic results, even when they can be derived, do not apply to this situation. Todd Keister, Yaw Nyarko, and I are investigating the Bayesian adoption of production recipes.

MD: Is the macro paper the one on the degree of returns to scale?

Shell: Yes. It is a joint exercise with Gaetano Antinolfi and Todd Keister. We analyze an OG economy with two reproducible factors—capital and technological knowledge—and labor. The key parameter is the degree of returns to scale in *the reproducible factors.* If the degree is greater than unity, the dynamics are qualitatively the same as in my dissertation: history-dependent growth that can be explosive or implosive. If the degree is less than unity, there is global stability of growth. If the degree is unity, we have an economy that is like the AK model of the new growth theory. This economy represents a bifurcation. It and the AK model are mere razor edges among the more general techonologies.

MD: You mentioned that you were a summer intern at the Council of Economic Advisers. When was this? Who was there? What did you do?

Shell: It was summer of 1962. Kennedy was President, Walter Heller (the senior) was Chairman of the Council, Jim Tobin and Kermit Gordon were the other members. By the end of the summer, Gardner Ackley had replaced Jim. Ken, the senior staff member (replacing, I think, Bob Solow), had brought me there. It was part of a unique period in the history of the Council. No political scientist would predict that such a tiny agency could be so influential. I can think of two contributing factors. The first is Heller. Walter had the support of important economists. He was persistent yet diplomatic. He knew the political map . He was there, ready and reliable, whenever his boss, the President, needed him. The second is Tobin. Jim and JFK seemed to be on the same personal wavelength. More than once, I happened to see Jim walking from Old State (the Executive Office Building) to the White House lawn, almost surely to visit the Oval Office.

The CEA had worked for a tax cut to stimulate demand. In the spring of 1962, the tax cut became law. This was a heady time for the economists in Washington. The tax cut of 1962 was also a turning point in U.S. political economy. After 1962, the fiscal policy debate was somewhat elevated. Deficits could be good or bad in given circumstances, but for the first time in a while, they could be open for discussion. In this very limited sense, most of the country had become Keynesian.

But the closest that I got to fiscal policy was in drafting letters for the Chairman's or President's signature in response to businessmen's and politicians' worries about budget policy. Nonetheless, it was a very exciting time for me. I was the lowest man on the CEA totem pole (perhaps nonuniquely). I had no settled office, so I shared offices. Mostly with Ken, whom I got to know better. My admiration for him increased, if this is possible. I also shared an office with Dick Nelson, who had edited an NBER volume on inventive activity that includes several classic papers on the subject, including the one by Ken. I was influenced by Dick's volume and Ken's paper. I learned from talking to and working with Dick.

MD: What was your connection to Tobin?

Shell: Jim was an important mentor at the CEA. He and Art Okun tutored me in CEA fiscal analysis. They assigned to me doable short-term projects and then critiqued my results.

MD: Did you like being a summer intern?

Shell: It was fun, but it was not a Monica Lewinsky type of job. I learned a lot of economics that summer. I don't know much about Washington, D.C. today. In 1962, it was a relatively small, relatively Southern city. There seemed to be a danger then that the economists and other scientists would be seduced into the political game. Tobin resisted this. Heller's job was in large part political, but I think the rest of us put too much weight on a lunch with a Deputy Secretary of This or That. I guess that the likelihood of being co-opted is even greater today than it was then.

MD: Your first job out of grad school was at MIT?

Shell: Yes, 1964–1968. Four great years in Cambridge. Stephanie was born in Boston in 1964. Jason was born in Boston in 1968 before our decampment to Squam Lake and Penn.

MD: Let's talk a little bit about your days at MIT.

Shell: The atmosphere was intense. The MIT economics faculty had lunch at a big table, at which Paul [Samuelson] held court. I was dazzled by Paul—as I expected to be—but I was also impressed by the breadth of general competence in the MIT department. Bob [Solow] was on leave during my first year. When we finally met, I was equally dazzled by Bob—a very deep guy, who tries to make us believe that it all came easily. Bob was splendid at getting the most out of others, always making MIT a happy place. I learned so much from Bob, Paul, Franco [Modigliani], and—of course—my suitemate, coauthor, and friend, Frank Fisher.

The MIT social pyramid was inverted. At times I was the sole member below full rank. At other times, I was joined by a few other junior colleagues. Among these were Steve Marglin, Pranab Bardhan, Duncan Foley, and the late Miguel Sidrauski. I wrote two papers with Duncan and two with Miguel. I did two books and a paper with Frank. Since the senior faculty was so busy (in Washington and in those capital debates with Cambridge, England), the graduate students had a lot of time for me. In my first year, Joe Stiglitz, George Akerlof, and several other top guys took my seminar course. I wrote two papers with Joe. Joe also helped me to model the role played by imperfect competition in technological evolution. George introduced me to asymmetric information and confirmed my

Figure 2. Karl Shell, Cambridge, Massachusetts, about 1966.

own penchant for using simple models for "counterexamples" and for starting new ideas. There were also Mrinal Datta-Chaudhuri, Avinash Dixit, Stan Fischer, Bill Nordhaus, Mike Rothschild, Eytan Sheshinski, Marty Weitzman, and others. What a splendid group! One summer—during the time that my family and I were at Squam Lake [New Hampshire]—Hiro borrowed a dozen students, taking them

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to Chicago. Friendships and rivalries were strengthened in the Chicago heat. In my third year, Peter Diamond moved to MIT. Jim Mirrless and Christian von Weizsäcker were MIT visitors. Tony Atkinson spent a year at MIT as a special student.

MD: What did you work on at MIT? Was there one particular area or was there a constellation of topics.

Shell: I was busy. I began my research on the OG model. I worked with Joe and Miguel on multi-asset accumulation. Frank and I published our first paper on index numbers. I led a group that included Frank, Duncan, Stan, and the late Nan Friedlander, which did a large applied project on education finance. I delivered my so-called Varenna [Italy] lectures on applications of Pontryagin's maximum principle to economics, the so-called "transversality condition," bubbles, non-inner-product price systems, and so forth. The *Journal of Economic Theory* was born.

The four years at MIT came to a sad end. Miguel died in Boston during our drive to Philadelphia. There wasn't a proper good-bye. Galleys from the *REStud* for Shell–Sidrauski–Stiglitz arrived in Philadelphia within a day of the terrible news. Miguel became a figure in monetary economics in the few years given to him.

MD: One of the workhorses of modern macro is the overlapping generations model. Your 1971 *JPE* article is one of the key OG papers. Tell us about this paper and your later work with Balasko.

Shell: The OG model *is* important for macro. In the OG framework, money can bear a positive price even if the public debt is never retired. The OG model is easy to work with: Aggregate wealth might be infinite, but individual wealth is always finite. The OG model is realistic: People *are* born and *do* die. In fact, I think that many macro policy issues that we will be facing in the twenty-first century involve demography and will require OG analysis. I think that Paul Samuelson's consumption-loan paper in the *JPE* is his best among many top papers.

My 1971 *JPE Notes* began the formal general-equilibrium analysis of OG models (just as Diamond's 1965 *AER* paper and Gale's 1972 *JET* paper began the dynamic analysis of OG models). The *JPE* paper was nearly completed at MIT, but its publication was delayed because of a rejection from the *AER*. There were two very positive referee reports, but the *AER* editor said that "with these reports" he could not accept my submission. *Notes* benefited from important suggestions from Avinash Dixit and Peter Diamond—aimed at helping to clarify the nature of the "infinities" involved. Avinash reminded me of a (slightly) different "double infinity": Hilbert's Infinite Hotel, in which there is a countably infinite number of hotel beds to be allocated to a countably infinite number of travelers. Peter pointed out the contrasts and connections with Debreu's *NAS* paper on economies with a finite number of consumers, but a countably infinite number of commodities.

Before *Notes*, the folklore was that the suboptimality observed in the OG model was due to the natural restrictions imposed on market participation. After all, Buck Rogers cannot trade with George Washington. Such market-participation

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restrictions are valid and they turn out to be a source of sunspot equilibria, but they play no role in perfect-foresight, nonstochastic OG economies. I showed that the so-called Samuelson friction is due solely to the "double infinity" of dated commodities and individuals. The usual proof of Pareto optimality does not apply to OG models because (discounted) social wealth can be unbounded. Don Brown and John Geanakoplos tidied up this argument using nonstandard analysis. The nonstandard-analysis reinterpretation of the double-infinity story is that in the OG world there are missing markets at infinity.

MD: We now refer to the model as the OG, or OLG, model. Samuelson called it the consumption-loan model. Were you the first to call it the overlapping generations model?

Shell: Maybe. At any rate, the phrase appears in my Malinvaud lecture, my joint papers with Yves, and the propaganda paper with Dave [in the Kareken/Wallace volume]. "Overlapping generations" underlines the basic demographic structure, while "consumption-loan" suggests a particular remedy to the suboptimality. "Consumption-loan" is too suggestive of the George Washington/Buck Rogers restriction on market participation.

MD: So, the claim before your *JPE* piece was that restricted participation leads to suboptimality, when in fact it doesn't. It's the infinity of dated commodities in conjunction with the infinity of finite-lived individuals.

Shell: Yes. Buck Rogers and George Washington cannot trade. Pretend that they can. With perfect foresight, the equilibrium is unchanged by this thought experiment. After all, Buck Rogers and George Washington do not have anything that the other guy wants. Looking at the OG model from a GE [general equilibrium] viewpoint, one immediately notices that the very overlap of the generations makes this a well-behaved GE model. Overlap ensures Arrow–Hahn "resource relatedness" and McKenzie "irreducibility," either of which ensures existence of competitive equilibrium. This is at the heart of the Balasko–Shell existence proof. The double infinity allows there to be competitive equilibrium allocations that are not Pareto optimal (even though they must be weakly optimal). The usual first welfare theorem proof for finite economies takes the individual budget constraints and sums them. Then one argues that if a better allocation were available, it would cost more and hence would not be feasible. This relies on the right-hand side—the present value of aggregate endowments—being finite. Since this can be infinite in the OG model, the proof of optimality fails in this case. (This also applies to Hilbert's Infinite Hotel.)

MD: When Neil [Wallace] taught the overlapping generations model, he used a sequence-of-markets approach, which made us think that the inefficiencies that appear in the model did have something to do with the fact that Buck Rogers couldn't trade with George Washington. Anything coming out of Penn—including Shell, Balasko and Shell, and Balasko, Cass, and Shell—specified the budget constraint as $p \cdot x$. And the graduate students at Minnesota couldn't understand how you guys could have one budget constraint when all these people couldn't trade with each other.

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Shell: The single budget constraint per individual is a consequence of perfect borrowing-and-lending markets. Even if the money markets are the only place forward transactions are made, the period-by-period liquidity constraints reduce to a single lifetime budget constraint. This is pure Irving Fisher. Yves and I reproduce the Fisher proof in our 1981 OG monetary paper in *JET*.

MD: This is closely related to Arrow's result, that with Arrow securities and spot markets you don't need contingent-claims markets as long as you have as many securities as states of nature.

Shell: Yes. For Irving Fisher, one uses *t* for time, where for Arrow, one uses *s* for state of nature. Dave and I reproduce in our 1983 *JPE* piece the Arrow argument for a single budget constraint per individual when the securities markets are perfect. Yves and I add a caveat missing from Irving Fisher: the equilibrium price of money can be zero, effectively closing down monetary borrowing and lending markets, making the separate liquidity constraints binding. Dave and I add a similar caveat to Arrow: the price of some or all Arrow securities can be zero, closing down the corresponding insurance markets.

MD: It was interesting to me as a graduate student to see these two very different approaches. The notation suggested to me at the time that limited participation was critical.

Shell: Let me go on. My preceding argument is a defense of one-budget-constraint-per-individual based on perfect borrowing-and-lending markets. If these markets are not perfect, there is an additional source of suboptimality, but this is well known and arises in finite economies as well as in OG economies. Even if there are credit restrictions, they don't change the argument of my 1971 *JPE Notes*. There is now a double-infinity source of suboptimality and a credit-restriction source of suboptimality, but in perfect-foresight models, there is no separate source of suboptimality caused by restricted participation. The Penn group was into credit restrictions in OG models. They are treated in my 1977 Malinvaud lecture. Suchan Chae (now at Rice) did a good thesis chapter on them. And more.

MD: So, much of what you've been talking about so far involves conceptual and theoretical issues. But you weren't beyond doing propaganda either, were you? A case in point would be the conference where you, Dave, and Neil [Wallace] pushed the overlapping generations model there as *the* model for macro and monetary economics, not just for interesting theorems about existence or welfare. How do you feel about that now? Could we get you to comment on your so-called propaganda piece?

Shell: I believe even more strongly than I did then that the OG model is a very important tool for money and macro.

I would not say it is the sole tool. It certainly doesn't hurt to understand the analogous finite model. Two quite separate things are central to the Samuelson classic paper: (1) the OG demography and (2) taxes denominated in money. Yves [Balasko] and I study taxes denominated in money in finite economies. This serves as a baseline case for our OG monetary analysis.

I think you have to understand the spirit of the times. I was getting, and I'm sure my colleagues were as well—we were all getting—a lot of flak from the established macroeconomics profession.

MD: That was the Keynesians at the time or the neoclassicals—who was "the establishment"?

Shell: The older guys in macro—many of them my heroes—were predominately Keynesian. But Keynesianism versus non- or anti-Keynesianism was not, as far as I could see, the basic issue. The basic issue was intellectual change. The Old Guard did not welcome the new ideas. They were especially averse to high tech, sort of "Math is for Micro; Macro is based on Common Sense." The OG model is, after all, quite congenial to Keynesian application. Indeterminacy, sunspots, and self-fulfilling beliefs in general would seem to offer room for Keynesians. I believe it was the novelty and the math that the macro establishment in the two Cambridges *and* Chicago disliked the most.

Back to the Minneapolis Fed conference: Neil might have taken some sort of line close to the "overlapping generations model is what you need for virtually everything." And then there was a tone possibly supported by Tobin, Brainard, Hahn, and several others that the OG model was good for nothing. And, given this binary choice, I was in the camp with Neil.

MD: What was the complaint of Tobin and the others?

Shell: Some claimed that the OG advocates were unable to differentiate between money and bonds. I think that they were partially correct, but they were completely unfair. We could—and did—include liquidity, cash-in-advance, or Clower constraints in our papers. We could accommodate this but we, like everyone else, could do nothing more to elucidate the economic sources of these mechanical constraints. Of course, if there are no such constraints, money and T-bills are perfect substitutes. The OG group understood this perfectly.

I don't believe that our differences could be described by labels like "Keynesian" or "new classical." All macro today is Keynesian in the sense that we think about the macroeconomy through the filter of national income accounts. Output, consumption, investment, and employment are (among others) key variables. Many of us do want to go beyond the useful IS-LM insights. The early Keynesians (and the early non-Keynesians) did not have the tools (dynamics mostly) to probe as deeply as we can today.

MD: Tell us more about the OG model.

Shell: In the spring of 1977, as part of a joint OG research project with Dave, I presented a series of seminars on the OG economy. The model was based on my 1971 *Notes*. The first presentation was in the Penn theory workshop. The second was at an MSSB [Mathematical Social Science Board] seminar at Squam Lake. I wrote up this lecture and used it as the basis of my November 1977 Malinvaud lecture in Paris. [*MD* will publish this as a "Vintage Unpublished Paper."] In these lectures, stylized "sunspots" (i.e., purely extrinsic uncertainty) appeared for the first time. In that particular model, sunspots affect the general price level. The allocation of resources is sunspot dependent. Optimal (contrasunspot) fiscal policy is active. Passive fiscal policies are not optimal. During the Squam Lake conference, Dave and I extended the (linear) model of my lectures to include nonlinearites and Gale-like dynamics. (This work was the basis of our application to the NSF in late 1977.) There are three sources of sunspot equilibrium in the OG model: (1) restricted market participation, naturally arising in OG economies; (2) the OG "double infinity" of my *JPE* note; and (3) incomplete markets (there is no state-dependent money, or Arrow security). My Malinvaud lecture was also meant to be a challenge to the simplest quantity theories. The logical quantity theory is about the relationship among *sets* of competitive equilibria. It is relatively weak: it limits rational expectations beliefs, but it does not tie them down.

MD: Your four papers with Balasko are OG classics. Did you hook up with Yves at this time?

Shell: By the time of my Malinvaud lecture [November 1977], Yves and I had begun talking about existence and welfare analysis for the nonsunspots OG economy. We discovered the right truncation and limit argument to establish existence of perfect-foresight competitive equilibrium. (Hiro Okuno-Fujiwara and Itzhak Zilcha got independently a similar result.) All subsequent papers on OG existence use the Balasko–Shell method. Perhaps the most innovative of these is Chuck Wilson's 1981 *JET* paper, which allows for finite-lived *and* infinite-lived consumers. A paper by Yves, Dave, and me that postdates Chuck's is possibly the final word on existence with bounded life lengths.

Yves and I also did the welfare analysis. We studied both Pareto-optimal allocations and weakly (or short-run) Pareto-optimal [WPO] allocations. WPO allocations are those that can only be "blocked" (if at all) by allocations that differ from the original one in more than a finite number of components. Roughly, WPO allocations can be blocked only by going to "chain letters." Of course, every PO allocation is a WPO allocation. For OG economies, the welfare theorems are: (I) Every competitive equilibrium allocation is *weakly* Pareto optimal. (II) Every weakly Pareto-optimal allocation can be supported as a competitive equilibrium.

Yves and I recast the monetary aspects of the OG model. Samuelson and others who followed him allowed for only a first-period injection of money. We permit a fully active fiscal policy. Taxes and transfers denominated in money can be made to each individual during each period of his life. We analyzed in detail the set of bonafide fiscal policies—those policies allowing for money with a positive price. In finite economies, there is (near) equivalence of the bonafide fiscal policies and the fiscal policies in which the debt is retired, i.e., the balanced fiscal policies. In the infinite economy, things become more interesting. If the public debt is permanently retired in finite time, then the fiscal policy is bonafide. If the debt is only asymptotically retired, then the policy might or might not be bonafide. Retirement of the public debt is neither necessary nor sufficient for bonafidelity in the OG economy.

MD: Tell us about your work on multi-asset accumulation.

Shell: Multi-asset models are important—partly because generality has value, but mostly because of the importance in dynamic market economies of capital gains. My work began with the 1965 Shell–Stiglitz *QJE* paper on the allocation of investment in a dynamic economy. It was in part as an answer to the so-called [Frank] Hahn problem: In multicapital, perfect-foresight market economies, the dynamical system "tends" to be unstable—often the saddlepoint instability frequently encountered in optimal growth models. Most paths in the market economy lead to ruin (unlike the optimal growth models in which the best of these paths is always chosen). A nail in the coffin of capitalism?

Joe [Stiglitz] and I analyzed a market, growth model with two capitals and hence capital gains. The laws of motion, driven by perfect foresight in the asset market do yield a saddlepoint equilibrium, but we discovered that adding the natural restriction to nonnegative prices yields global asymptotic stability. For this reason and others, perfect foresight does suggest efficient allocation of resources, but the attendant capital gains suggest (as Keynes stressed) "instability." Insensitive price forecasts *à la* Cagan tend to stabilize matters in the Keynesian sense. Joe and I did not get to the (game-theoretic) analysis of short-run equilibrium without complete futures markets. It is still a project very much worth doing!

In our particular model, errant trajectories (paths satisfying short-run maximization but very inefficient in the long run) are not competitive equilibrium paths. It is price nonnegativity that rules out these bad paths, but without an infinity of futures markets, it is not clear that the economy will always stick to good paths. More work needs to be done on this. Perhaps some game theory ideas can bridge the gap between the "instability" of Keynes and the "stability" of the Walras-like equilibrium.

There was a companion paper, with Miguel [Sidrauski] and Joe [Stiglitz], in which we analyzed the dynamical system in which the second asset isn't capital but is instead money and—as is now well known—there is also typically a saddle equilibrium, but unlike the case where there are only real assets, only "half" of the paths turn out to be disequilibrium paths. The rest, except for the razor's edge (that tends to the saddlepoint), are hyperinflationary paths.

I now see these money-and-capital phase diagrams—or their cousins—in graduate textbooks. I grind my teeth. The textbook authors invariably focus on the stable manifold, the set of paths tending to the rest point, citing perhaps "the transversality condition" or something. This requires much more care than the textbooks give. Usually it is boundary conditions that close the model. The model is not always closed. Hyperinflation is hard to exclude, for example, and—as I show in my Varenna lectures—the so-called transversality condition is not necessarily a property of every optimal growth solution. And, as I said before, the Walras-like equilibrium concept might not be appropriate to the dynamic analysis of capital gains in economies with incomplete markets and/or recontracting.

MD: Are you referring to the "Capital Gains, Income and Saving" paper in the *REStud*?

Shell: Yes. If you were to ask how that paper should have been titled, I now think it should have included something about "money and growth," and I would now ask the reader to begin with Section 2, with the one-sector monetary model. There is a lot of two-sector stuff in there that is not so important. The savings behavior was primitive. It was a constant fraction of perceived income, where perceived income included capital gains, and it would probably not be up to today's standards to look at a model with simplistic saving behavior like that, but, in fact, all the themes of this hold for, say, the overlapping generations model. The same themes have reappeared in work by Jean Tirole in the OG environment. This—along with optimal growth—is the background for my work with Dave [Cass] on Hamiltonian dynamics.

MD: Hamiltonians have been a prominant feature of your work. Tell us about them.

Shell: A perfect-foresight economy (whether market or centralized) can be described as a Hamiltonian dynamical system [HDS]. The Hamiltonian function (think of it as, say, national output as a function of the input stocks) that generates the dynamical system is itself a full description of the technology. A lot is known about *autonomous* Hamiltonian systems. There is root splitting by a simple theorem of Poincaré. Adding convexity (in prices) and concavity (in stocks) yields a saddlepoint system. Boundary conditions from economics then are likely to ensure global asymptotic stability. But only special (zero-interest-rate) HDS's in economics are autonomous. On the other hand, any HDS can be represented as a perturbation of an autonomous HDS. If the convexity–concavity of the Hamiltonian function is strengthened (by, say, the discount factor), then saddlepoint dynamics leading to global asymptotic stability (with boundary conditions) are maintained. In fact, Dave and I weakened the critical condition from curvature to steepness, relating our work to McKenzie's work on value loss. Jess Benhabib and Kazuo Nishimura used the Hamiltonian approach to develop sufficient conditions for indeterminacy or history dependence. The Benhabib/Nishimura analysis is important for indeterminacy and sunspots.

MD: Were you at MIT when the *Journal of Economic Theory* got off the ground?

Shell: *JET* began when I was at MIT. Edwin Beschler, the math/physics editor from Academic Press, proposed that I edit a specialty journal that he wanted to call the "Journal of Mathematical Economics." I was intrigued, but I was cool to the proposed title. Edwin consulted the senior economists who had been advising him. They went along, and so Academic Press agreed to a new name and the implied acronym: *JET*.

MD: It's extremely rare for someone that young to be involved in such an important and big project, isn't it? Given the time and effort required to run something like that, was it at the right stage of your career to take on something like this? One could imagine Academic Press approaching Kenneth Arrow, but they approached a relatively young guy.

Shell: You have posed two good questions: (1) Why would Academic Press approach me? After all, I was not yet 30. (2) Why would I take on this responsibility at this early stage of my career?

I imagine that Arrow and other very senior guys would not accept the job. But he and several other very senior guys were prepared to serve as associate editors.

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Still—this is not a full explanation. It suggests that Academic Press might not be able to attract a very senior guy. It does not say why they chose a very young guy.

MD: One theory might be that they did ask Arrow and he said why don't you check with Karl?

Shell: Something like that might have happened. Arrow and/or others such as David Gale, Harold Kuhn, Leo Hurwicz, Lionel McKenzie, Roy Radner, Paul Samuelson, and Bob Solow might have been contacted by the Press. These guys were very supportive of *JET*. I was active in professional matters when I was at MIT—refereeing, seminars, lectures, etc. I was a known quantity. Perhaps the senior guys trusted me—especially since the original (and subsequent) editorial boards were loaded with diligent, senior guys, who could guide me.

I had the energy to do this. One reason one seeks an appointment in an excellent department is to have excellent colleagues. Through *JET*, I have had the privilege of knowing and working with many of the best economists of the twentieth century. I don't think that my research has suffered because of the editorship. On the other hand, turning down papers is certainly not the best way to make friends.

MD: Was it difficult getting the new journal off the ground? How did it achieve its current high rank, both in theory and in economics generally?

Shell: As I said, we began with a stellar group of associate editors, each of whom worked very hard for the *Journal*. Several from the original group are now Nobels. Many of the rest deserve Nobels. *JET* began to organize in 1968. We began publication in 1969. During our first year, we worked at soliciting and attracting the right papers. It is virtually impossible for a journal to recover from a mediocre start. *JET* is the top pure specialty journal in economic theory. In surveys, it also ranks fourth or fifth among the so-called "core" journals in all of economics. The editorial board has sought to be broad and inclusive—favoring no schools or topics. For example, we were early in game theory and financial economics, we have published over the years some of the best political science, and we stuck with social choice and intertemporal economics when they were out of favor. Currently, we are making initiatives in political economy, macro, monetary economics, and computation.

MD: You sell yourself short because it takes quite a bit of talent to get to know the large pool of talent you've drawn your associate editors from. It must be hard keeping on top of who's who in the profession, particularly with the new scholars coming up the ranks.

Shell: Well, thank you for the compliment, but in fact working with the associate editors has been an enormous source of satisfaction. Working with them has also been broadening.

Working with Academic Press—especially in the days before it moved from 111 Fifth Avenue in New York's publishing district—was a lot of fun. In the beginning, *JET* came under the editorship for math and physics. This was the perfect culture for *JET*. Two magnificent AP editors stand out—Edwin Beschler and the late David Swanson. Each had done graduate work in math. David was an AbD [All-but-Dissertation]. They were steeped in the culture of scientific publishing.

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AP's roots go back to Leipzig. Academic Press, along with such companies as the Interscience division of Wiley, Dekker, and Frederick Prager, had been founded by refugees from Leipzig, for some centuries the primary scientific publishing center of Europe. (The English university presses, Springer-Verlag in Heidelberg and Berlin, Elsevier in the Netherlands, and Birkhäuser in Switzerland were also major sources of science publishing.) A vigorous rivalry existed among all of them, resulting in an astounding rate of growth for American science publishing, which had been relatively weak before the 1950's and 1960's. I had the pleasure of witnessing this robust competition among the Leipzig houses. For example, when AP lost a journal project to Interscience, one of these guys muttered: I told so-and-so not to marry him. "So-and-so" was from an AP family. "Him" was from an Interscience family. Every AP editor was schooled in the old Leipzig adage: Don't lecture the professors; they lecture you.

MD: One of the more important articles in macroeconomics appeared in *JET*: Lucas's 1972 "Expectations and the Neutrality of Money." Can you tell us about your interactions with Bob, and how this paper came to appear in a theory journal?

Shell: Bob submitted the paper to *JET* after it was rejected by *AER*. I don't remember whether I saw the *AER* editor's reasons or not. Too much theory? Too much math? *JET* snapped up the article. In my mind, it is Bob's best. It is a major macro paper. It is an excellent theory paper. It is built on three essential ingredients: (1) the OG model of Samuelson, very slightly adapted for macro; (2) asymmetric information modeled through the "islands" of Ned Phelps, and (3) rational expectations of Muth. This is a seminal paper even though its conclusions can properly be questioned. This article stimulated my own research, especially about sunspots.

Bob's was not the only *JET* home run. For example, in the same issue with Bob's paper we also published Dave Starrett's classic on pollution, Malinvaud's important paper on risks in large markets, and Dave Cass's classic on intertemporal efficiency prices. Ned [Phelps] showed that, if an economy is forever bounded above the golden-rule capital level, it is oversaving. Dave discovered a formula in terms of intertemporal shadow prices that provides a complete characterization of oversaving. Yves [Balasko] and I in our 1980 *JET* paper showed that Dave's method extends to OG models. Weakly Pareto-optimal allocations are supported by intertemporal shadow prices. If those prices satisfy the Cass criterion, the allocation is not Pareto optimal. If they do not, the WPO allocation is PO.

MD: One of the things that's made *JET* so successful has been its track record of publishing important papers. Is there any area where you think *JET* missed out?

Shell: Probably we haven't done nearly as much in macro and monetary theory as in micro, despite publishing the Lucas article. We are working on this. I am optimistic, especially since growth theory and general equilibrium theory—two biggies at *JET*—are so basic to macro. We are, I think, in the center of political science, but we could do more in political economy. We are working on this. More in computational and experimental economics would be desirable. In the beginning, we got the best papers in financial economics, but now there is very stiff competition. We would like to gain back ground. There are other important fields—such as development and international economics—where we could do more. There is a lot of good economics being done these days. We are very international in scope, but we must be more so. Journals are changing rapidly in the face of the electronic technologies. It is important for the editorial office to have up-to-date electronic capabilities.

The evolution of our discipline is leading *JET* to expand its scope and to further internationalize its board. I am very pleased that Jess Benhabib has joined me in the *JET* editorship. Jess is terrific in many ways. I am confident that he will provide the leadership necessary for modernization of *JET*. Our first priority is to expand our scope, while maintaining our quality. The second priority is to be fully electronic in our services to authors and readers.

MD: Tell us about index numbers. What attracted Fisher and you to this subject?

Shell: Frank and I did two things at MIT: We extended the Könus theory of the true cost-of-living index to include quality change, new goods, and disappearing goods. Following Debreu and others, we developed a theory of the GDP deflator isomorphic to the Könus theory. In our long-delayed second book, we put forward a more general theory of production price indexes, which is only in very special cases isomorphic to the Könus theory. The 1972 book is quite well known. The 1998 Cambridge University Press book is not. This is a shame. It is the better work.

What got us into this? Support from the Board of Governors and the BLS was helpful, but measurement issues are clearly important for applied and pure economics. The market, after all, aggregates by reducing to a few the number of asset types for sale, the number of time periods for time-of-day pricing, and so forth. Understanding aggregation is worthwhile, even for a pure theorist. Macro is in part about interpreting aggregate data. Sticky prices are in part about aggregating over time, menu pricing, etc. And so on.

MD: What led you to leave for Penn after 4 years at MIT?

Shell: An excellent job offer from Penn. Ned Phelps and Albert Ando invited me to move to Philadelphia. I was an untenured associate at MIT. Penn offered a good salary, tenure, and a promise of a full professorship within 2 years. It was a very good move. I was happy during most of my 18 years at Penn [1968–1986].

The timing was good. Ned was doing important work on the microfoundations of macro. His ideas were very new. It might be difficult now to imagine how the profession was then. Micro and macro, despite Patinkin, were mostly put in separate compartments. It took me a while to break this old habit. Ned taught me a lot. Sometimes it took years for his lessons to sink in. Ned and I wrote a paper on the burden of the debt.

MD: Who else was there at the time?

Shell: The central business at Penn back then was macroeconometrics. Lawrie Klein ran a large, professional shop. Albert Ando ran a first-rate boutique. Albert and I extended the Baumol-Tobin transactions-demand model to include equities as well as money and T-bills.

There was also an important Kuznets wing of the department. The late Irv Kravis, an international comparisons numbers man, built the Penn department. He brought Lawrie from Oxford during the Red scares. Dick Easterlin, the imaginative demographer, and Al Phillips were also followers of Kuznets. Oliver Williamson was a serious, independent force bringing transactions costs to the analysis of the firm, IO, and the law. Olly is a good friend.

Sidney Weintraub was the oldest theorist. He was a Cambridge, England (anti-Cambridge, Massachusetts) Keynesian. Roy Harrod visited Penn during my first year or so. So, I had some link to this type of Keynesianism but Sir Roy did not reveal too many secrets. Among the theorists, there was Ed Burmeister, author with Dobell of an influential text on growth. We did some work together that didn't lead to publication. And there was Bob Pollak, with whom I had discussions about index number problems. Tom Sargent, Ed Prescott, Walt Heller (the younger), Steve Ross, Dave Cass, Jim Jordan, Costas Azariadis, Jacques Crémer, Andy Postlewaite, and Marc Nerlove came later. There were many excellent economists at and visiting Penn during my 18 years!

MD: Tell us more about these guys.

Shell: Tom and Ed were probably hired by Ned. It was very clear that these guys were going somewhere but I don't think they were treated well by Albert or Lawrie. I worked some with Walt—especially on optimal taxation and costly tax administration. We are friends. Steve was a nearby neighbor. We met in the hood, sometimes to discuss kids, sometimes to discuss science. Jim [Jordan] and Jacques [Crémer] were also neighbors. In our walks home, they taught me many things including mechanism design, learning, contracts, and petroleum economics.

Ned left for Columbia in, I think, 1972. He is perfectly suited to New York life. Columbia offered me a position in 1973 after our return from a year at Stanford. I was tempted, but our style of child-rearing was based on the ample space we then had in West Philadelphia. We feared that we might not be able to adapt to New York.

Dave's first job was at Cowles. Mine was at MIT. We visited each other's institutions regularly. I moved from MIT to Penn. Then Dave moved to Carnegie. We continued to visit each other's institutions. I persuaded our colleagues to invite Dave to Penn. We now had the critical mass to build a full-scale theory group. Together Dave and I built and co-directed CARESS [the Center for Analytic Research in Economics and the Social Sciences], which is an important chapter in the history of the Penn department. It was useful at the time for the theorists to have some institutional weight relative to the large-scale and successful activities then going on in macroeconometrics.

Costas Azariadis came from Brown to Penn. He and I chatted about macro in our frequent walks from office to home. I think the modern Penn department was born when Andy Postlewaite moved from Wharton to be the chair of the department. Andy is smart and innovative. He was an excellent chairman.

There were some very good Penn students. Steve Salant did a self-directed dissertation on search, which introduced me to the subject. John Weymark began with me on optimal taxation, but went far beyond to become a figure in social choice.

Jim Peck finished his Penn Ph.D. with me at the CASBS. Jim is a figure in OG, sunspots, market games, asymmetric information, exchange rate determination, and "impulse demand" including movie attendance and bank runs. You [Steve Spear] worked with Dave, Yves, and me. You have made quite a name for yourself in OG, sunspots, externalities, market games, experimental economics, and more. There were several others.

MD: Let's back up and talk about sunspots. In the early stages, your OLG work and your sunspots work seem to have been intertwined. The original reference on sunspots is your 1977 Malinvaud lecture "Monnaie et Allocation Intertemporelle." This must have been during your year in Paris. Tell us more about the "Monnaie" paper and where the sunspots idea came from.

Shell: The sunspots idea arose as part of a research project on OG economies. There were for me at least two inspirations for the sunspots idea—one distant, one proximate. The proximate inspiration was Lucas's 1972 *JET* paper. I was impressed by the tools in that paper—RE, OG, and AI [asymmetric information]—that serve as a platform for a concise money-macro model. On the other hand, I was skeptical of what I took to be Bob's conclusions: The best policy (within the model) is one of constant money growth. From what I knew about OG models, I mistrusted this. I was trying to formalize why. The distant inspiration was perhaps "Keynesian," although one also sees this at least formally in non-Keynesians like, for example, Cagan. The idea is that reduction of friction in the economy can be destabilizing. One sees this in models of multi-asset accumulation. Perfect-foresight models tend to be less "stable" than static expectations models. Why shouldn't an RE model behave like its special case, the PF [perfect foresight] model? In this, I was running against the macroeconomics establishments of the time. People like Ando and Tobin believed that the differences between them and the new freshwater economists were based on the freshwaters' use of unrealistically sophisticated models of markets and expectations. The freshwaters agreed—except for the "unrealistically."

I disagreed with both sides. I saw no proof that RE models would necessarily yield Chicago policy prescriptions. Perhaps the opposite. I made a trial run on sunspots at the Penn theory workshop during—I think—the Spring of 1977. In May 1977, I gave the core of what became the Malinvaud lecture at the Dartmouth conference center on Squam Lake. (Thanks to an introduction from George Akerlof, my family and I spent a month each summer during our MIT and Penn years at this beautiful, quiet lake in New Hampshire. One year, we sublet "our" cottage for the making of the movie *On Golden Pond*.) It was at an MSSB conference organized by Dave and me. Buz Brock and José Scheinkman were there. They gave a paper in which low- or zero-variance monetary policies are optimal. In my lecture, optimal contrasunspot policies were always active, i.e., had positive variance. Lionel McKenzie was also there. My lecture was in part a follow-up to my 1971 *JPE* OG *Notes*. Hence the focus was on the simple linear case. Immediately after the lecture, while still at the conference, Dave and I did an analysis of the nonlinear case, using Gale-like phase diagrams.

Figure 3. Paris 1978 (left to right): Jean-Michel Grandmont, Jason Shell, Joss Bitan, Stephanie Shell, Karl Shell.

I had a Guggenheim for 1977–1978 at CEPREMAP, the French government research bureau in the 13th arrondisement of Paris. Edmond Malinvaud was the senior economist in France. I was honored to receive his invitation to speak at his famous Monday evening seminar. I wrote out the notes for the seminar in English. Pascal Mazodier helped me to translate the title page and the abstract into French. We had trouble translating "overlapping generations." *Generations successives* doesn't capture the idea. Years later, when Manuel Santos translated my 1971 *Notes* and Balasko–Shell into Spanish, he also faced trouble with this part of the translation. Is "overlap" an Anglo-Saxon idea?

I had several goals for this lecture: (1) to familiarize the group with OG problems (although Jean-Michel, Yves Younès, Yves Balasko, and others were at or near the cutting edge); (2) to introduce sunspots; (3) to question, in several ways, the simple "quantity theory"; (4) to show situations in which high-variance contrasunspot fiscal policies could be desirable. I cannot say that the seminar went over well in the short run. Malinvaud found sunspots bizarre. I was left to go back in the metro alone. Nonetheless, Jean-Michel followed up with good questions the next day. Yves [Balasko] continued to be keen on our joint OG project, and Roger Guesnerie and Jean-Jacques Laffont took me to lunch, questioning me at length about sunspots and OG over *cassoulet*.

MD: What led you to ever think, in your wildest imagination, that random variables that don't affect preferences or technology, can have effects on the allocation? It turns out that you were right, but that was a pretty crazy idea at the

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time, wasn't it? What did your colleagues think of this? Did they think you were off your rocker?

Shell: Yeah.

MD: Let's talk about that. What led you to it, and what were the reactions?

Shell: In my 1971 *JPE Notes* and my 1969 "Varenna lectures," so-called "bubbles" play an important role, as they do in the Samuelson cases in the Gale 1972 *JET* paper. If there can be nonstochastic bubbles in OG models, then why could there not be *stochastic* bubbles in OG models? The "double infinity" allows for money to have positive (bubble) value. Why couldn't the same double infinity allow for a random price level?

The question is: How does uncertainty enter the model? Think about Jevons sunspots. Jevons (i.e., real-world) sunspots are a form of intrinsic uncertainty. Real-world sunspots do affect plant growth, telecommunications, and incidence of cancer, but generally not all that much. Imagine the limiting case—the case of extrinsic uncertainty, or Cass–Shell sunspots—in which the random variable has no effect at all on the fundamentals. Model securities markets and other markets in the same way that one would model them with intrinsic uncertainty. Then ask: Do the stylized sunspots matter? We now know that there were three separate sources of sunspots in the Malinvaud lecture: (1) the double infinity, (2) the restrictions on (security) market participation naturally arising in the OG economy because of the different birthdates, and (3) the incompleteness of the (security) markets (there was only one money instead of sunspot-dependent Arrow securities). Each of these is by itself a source of sunspot equilibria, but my intuition was initially driven by the double infinity and bubbles.

We did not merely show that sunspots matter. That would have been relatively easy. What we did show is that sunspots can matter even in economies with rational expectations. Individual rationality does not ensure collective rationality. Many economists (and others) had the notion of market-generated, or endogenous, volatility. This goes back at least to the early nineteenth century idea that waves of optimism and pessimism might interact with bank lending to generate business cycles or to Keynes's emphasis on "animal spirits" as a determinant of investment. But what we did was to formally model endogenous uncertainty and show that it is consistent with (a very strong version of) rational expectations.

MD: Were sunspots a reaction to rational expectations? You seemed to be saying that it was a reaction to the claim of the RE school that the combination of market clearing and RE pins down outcomes.

Shell: I am certainly not against rational expectations. In fact, in the theoretical literature SE is usually a type of REE. RE may not be fully realistic, but it is probably the best expectations concept we now have for policy analysis. I lived through the era of pre-Lucas-critique policy models, in which the government finds the glitch in individual expectations and drives the economy from this glitch. The prescribed policies are unlikely to work in practice as individuals catch on to the obvious tricks of the government that are permitted in these models. But this does not mean that RE cannot be conditioned on sunspots, on calendar time, on near sunspots like freight-car loadings or "consumer confidence," and so forth. Nor does it mean that RE implies the quantity theory of money. There *is* a quantity-theory-like result, more properly called the absence of money illusion, which relates one set of equilibria to another. It says *nothing* about how the economy selects from the set of equilibria. Assume that there are two economies, A and B, that are identical except for government policy. Assume that money taxes and transfers are always 10% higher in B. It is true that the price level could be 10% higher in B, but it is conceivable that under RE the price level is the same as for A, 20% higher than in A, or even that sunspot effects (or nonstochastic cycles) are observed in one economy but not the other. So the quantity theory is not so much a theory, but a possible empirical regularity (or not). This does not challenge the rational expectations concept itself, but it does raise questions about many policy models of rational expectations.

MD: What kinds of reactions have you gotten to these ideas?

Shell: Notice that what I have said was not likely to please. One good friend, a distinguished macroeconomist, wrote to try to persuade me to give up sunspots on grounds of their unpopularity. The saltwater economists were math averse, even though some of them had made important math-intensive contributions (usually in micro) of their own. They were in a box. They had criticized the freshwater economists for modeling individuals as too rational and markets as too efficient. Otherwise, they might have taken sunspots and other indeterminacies as what they were looking for. The reaction from the freshwater economists was also nonpositive. Bob [Lucas] thought we were kidding. He could instead have viewed sunspots as offering a new RE tool in attempting to explain business cycles.

The sunspot revolution is disquieting. The set of equilibria is large. If economists don't pin down economic outcomes, argue many economists, we will be letting psychologists or somebody else do our work. I agree that determinacy is better than indeterminacy if it can be gotten realistically and honestly. If not, other approaches are necessary.

Not that the RE concept is perfect. People are not fully rational. Furthermore, one believes that in dynamic economies—in which the future can create its own uncertainty—the RE concept implies too much coordination. You [Steve Spear] have tackled this in your *JPE* "airport" paper on "sunspot equilibrium without sunspots." Matt Jackson and Jim Peck have done this in game theory settings. These are very tough questions. Dynamic games are very difficult, even when—as usual—the game theorists overemphasize "good " solutions. This is a wonderfully open area. Aditya Goenka is doing intriguing things on bounded rationality and sunspots. He and I are investigating the sunspot effects of Kahnemann–Tversky-like cookie-jar budgeting.

MD: Tell us about the work that led up to the Cass–Shell 1983 *JPE* article and about Azariadis' role in the development of the sunspots idea.

Shell: Dave and I are the co-inventors of the sunspot equilibrium concept. Our original invention was explicitly based on OG dynamics. We later introduced sunspots into other general equilibrium models. I returned from Paris in June

1978—full of my OG work with Yves and my sunspots work with Dave. Dave picked us up at the Philadelphia airport, but was out of circulation for the rest of the summer. I gave Costas a copy of the Malinvaud lecture, and filled him in on what Dave and I had done beyond this. Costas was, for quite some time, resistant to the ideas, but we had a lot of time together and eventually he was persuaded. Costas did something that Dave and I did not do: He found *stationary* (Markovian) sunspot effects (in the Lucas model). He and Roger Guesnerie later showed that there is a relationship between these sunspot cycles and long-run deterministic cycles. So Costas and Roger are important sunspots guys.

MD: You and Dave stress that SE are typically not mere randomizations over certainty equilibria.

Shell: The method used to construct the SE in the Malinvaud lecture is *not* mere randomization over the equilibrium sequence. That would have been silly. It does, however, involve some randomization over segments of the equilibrium sequence. This bootstrap method from the Malinvaud lectures of constructing SE was used by Costas. Jim Peck used the same method to show that if there is a continuum of monetary equilibria, then there will be an SE in which sunspots are effective in every period—even in models with nonstationary preferences and endowments. You [Steve Spear] used this technique to good effect in your dissertation. Roger Farmer and Mike Woodford extended Jim's analysis to more general situations. This has led some of the applied sunspots types to believe that SE are mere randomizations over CE [certainty equilibria]. This is incorrect. Often, it is correct to say that indeterminacy of CE is sufficient for the existence of SE.

In our 1983 *JPE* paper (delayed because the editor at first asked us to drop the appendix), Dave and I took a (very) finite "OG" model with restricted market participation. One polar case is that no consumer is restricted. This reduces to a simple Arrow–Debreu model. If the economy is convex, then in the sunspots economy, the allocation of resources is independent of sunspots and is the same as it is in the certainty economy. This is the Cass–Shell Immunity Theorem. The other polar case is that every consumer is restricted from participating in the securities market. Then an SE *is* a mere randomization over CE. Obviously in this case, SE exist if and only if there are multiple certainty competitive equilibria. For the intermediate case (some restricted consumers, some unrestricted consumers), Dave and I construct an example of an SE that is not a randomization over CE's. The literature now abounds (in different contexts) with sunspot equilibria that are not mere randomizations over the certainty equilibria.

MD: You mentioned that there were three separate sources of sunspot equilibria in the OG model. Tell us about these.

Shell: Restricted participation is one of the three sources of SE from the Malinvaud lecture. Another is, of course, the double infinity, my prime intuition for sunspot effects. Dave and I wrote a paper for a 1989 Texas conference volume in which it is assumed that individuals can trade on all markets (meeting at the beginning of time) even for goods delivered before their births and after their deaths. (Remember that I showed in 1971 that easing the natural restrictions on market

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participation does not affect the set of perfect-foresight equilibria.) Dave and I showed that sunspots matter in double-infinity economies even in the contrafactual world with no restrictions on participation nor restrictions on the range of markets. The third source of SE in the model of the Malinvaud lecture is market incompleteness. The literature on general equilibrium with market incompleteness—known was GEI—goes back to Roy Radner, Oliver Hart, and Doug McManus. Dave, his students, and Yves [Balasko] enriched the GEI literature by making sunspots the leading example.

MD: Earlier, you described Cass–Shell sunspots as the limiting case of intrinsic uncertainty that has only very small effects on the economic fundamentals. What do you mean by this?

Shell: One interpretation of the sunspots economy is that it is the limit of an economy with intrinsic uncertainty as the effect on the fundamentals of that uncertainty vanishes. The idea was formalized by Rody Manueli and Jim Peck in the *JEDC*, based on results in the Spear, Srivastava, and Woodford paper published in *JET* in 1990. It is shown that the limiting equilibria are indeed SE.

One reason one might consider sunspots is to get at excess volatility *à la* Bob Shiller. How do we recognize when volatility is "excess"? Shiller finds that the variance in share prices is about three times the variance in "share fundamentals." The implication is that 3 is a big number, but some elasticities intervene, so one cannot be sure. In the pure sunspots case, however, the denominator (variability of the fundamentals) is by definition zero. Hence if the outcomes (prices and quantities) are variable, you have a positive number divided by zero. There can then be no dispute about whether this volatility is excess (although it is not necessarily a bad thing).

MD: Electrical engineers call this gain, i.e., the ratio of output (or signal) noise to input noise.

Shell: Yes.

MD: Your work on sunspot equilibria has generated quite a few papers. Could you tell us something about these and how they related to the overall issue of sunspot equilibria?

Shell: The macroeconomists and general-equilibrium guys were usually surprised by the first sunspots papers. For game theorists, however, the general idea was not surprising. After all, stochastic solutions to nonstochastic games are run of the mill. I was asked by Roger Myerson and others at MEDS about the relationship between the SE concept and Bob Aumann's correlated equilibrium concept. Jim Peck (then a student) and I turned our attention to this question during my fellowship year at the CASBS. We had to find the correct model for making the comparison. Obviously, a game is needed. Equally obviously, markets are needed. Put this way, it is then obvious that the platform for our analysis should be the market game of Shapley and Shubik. To be able to do justice to correlated equilibrium there must be asymmetric information. In this environment, Jim and I found that, unless the initial allocation is Pareto optimal, there will always be SE; all correlated equilibria are sunspot equilibria; not all sunspot equilibria are correlated

Figure 4. Karl Shell and Jim Peck in Philadelphia, November 1985.

equilibria (because correlated equilibria are self-enforcing, while SE allow transfers of income across states of nature).

So Jim and I have shown that imperfect competition is a source of sunspot equilibria. We also extended the sunspot equilibrium concept to the case of asymmetric information. Taking the market game to the limit as the number of traders becomes large provides excellent foundations for Walrasian equilibrium, sunspot equilibrium, and the endogenously determined spectrum of open and closed markets, including securities markets.

MD: What are some other sources of sunspot equilibria?

Shell: The most dramatic addition to the sunspots literature in the last decade was the incorporation of indivisibilities and other nonconvexities. Our [Shell and Wright] joint 1993 *ET* paper opened the field. Remember that, in finite, convex economies, the certainty equilibria reappear as nonsunspot equilibria in the sunspots economy. The sunspot equilibria expand the set of equilibria. Proper sunspot equilibria are not Pareto optimal (although they are not usually Pareto dominated by the nonsunspot equilibria). Hence if markets are perfect and the economy is finite, sunspots do not matter in convex economies.

All this is changed with nonconvexities. The certainty equilibria might not reappear as equilibria in the sunspots economy. If markets are perfect (and the economy is finite), the certainty equilibria are Pareto optimal in the certainty economy, but this does *not* lead to an immunity theorem because the CE [certainty equilibria] allocations can be Pareto dominated by feasible sunspot-dependent allocations. With perfect markets, sunspots can matter if there are nonconvexities. This is a subtle

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idea since we use the same term "Pareto optimal" to mean two different things: (1) Pareto optimal among certainty allocations or (2) Pareto optimal among random allocations based on a given sunspots device. This ambiguity of language has led to some serious confusion by others in dealing with sunspots and nonconvexities.

MD: Tell us about the connections between sunspot equilibria and lottery equilibria.

Shell: Ed Prescott and Rob Townsend introduced the notion of lottery equilibrium [LE] in 1984. According to Ed and Rob, Arrow indicated the need for decentralizing LE and conjectured that sunspots might do the job. Using a macro model due to Richard Rogerson, we [Shell and Wright] show that the LE *can* be decentralized as SE. There is some recent work relating LE and SE for the important case in which the sunspot device is continuous: Rod [Garratt], Todd [Keister], Cheng-Zhong [Qin], and I have shown that if there are perfect markets and symmetric information, then the set of LE allocations and SE allocations are equivalent. This is not necessarily true unless the randomizing device is continuous. Tim Kehoe, David Levine, and Ed Prescott have extended this equivalence result to the important case of asymmetric information to handle cases in which the "nonconvexity" arises from moral hazard constraints. Aditya [Goenka], Rod, Todd, and I have investigated the effect of the randomizing device on the equilibrium allocation of resources. It is impractical to insure separately against a large number (certainly a continuum) of contingencies. For reasons of transaction costs and potential moral hazards, risk classes are often restricted. Public policy considerations factor in. The effects of risk classes on SE is an important topic for further research.

MD: Can sunspots help us to understand empirical aspects of economic fluctuations?

Shell: I am very interested in the role played by sunspots in explaining business-cycle facts. Your [Steve Spear] theoretical paper on externalities (a type of missing market) and sunspots was a precursor. Increasing returns and capacity utilization have also played important roles. People engaged in this effort include Jess [Benhabib], Roger Farmer, Stephanie Schmitt-Grohé, Roberto Perlí, Larry Christiano, and Yi Wen. As far as I can tell, the adoption of sunspots (in addition to real technology and consumption shocks) improves the fit with business-cycle facts. These models to date are necessarily very conservative in that they do not stray far from the RBC approach or the RBC "etiquette." I would, of course, welcome OG models (as well as or instead of infinite-lifetime models) in attempting to explain the facts of economic fluctuations and growth. I would also welcome more complicated mechanisms for transmission of the "sunspot shocks." Some differences of terminology between sunspots theorists and sunspots calibrators must be worked out. The calibrators have a notion of "near sunspots" that seems to differ in interesting ways from the Spear–Srivastrava–Woodford and Manueli–Peck approach. To my knowledge, there are not as yet any calibrations in which all the randomness is purely extrinsic.

Empirical sunspots is good stuff. I look forward to seeing more.

MD: During your Penn years, you had some sabbaticals that were crucial to your career. Tell us more about Stanford, CEPREMAP, and the CASBS.

Shell: For 1977–1978, I had a Guggenheim to travel to Paris with my family. I was the guest of Jean-Michel Grandmont and CEPREMAP. It was a wonderful time. Jean-Michel and Joss (and all of CEPREMAP) gave us royal treatment. Jean-Michel and I are close friends. At CEPREMAP, I met Yves Balasko, Roger Guesnerie, and Jean-Pascal Benassy. I had met the late Yves Younès earlier (in Varenna, Italy). Yves [Balakso] and I became very close. We worked closely over several years on the OG project. Yves is a brilliant mathematician and a brilliant economist. Yves visited Penn several times to advance the OG project. We did the final write-up of our first three *JET* papers at a hotel in Bivigliano, a small Tuscan town north of Florence. Yves views sunspots (in convex economies) as an instance of symmetry breaking. The equilibrium equations are symmetric across states of nature. There are symmetric solutions—the nonsunspot equilibria—but there are also asymmetric solutions (to the symmetric equations), the sunspot equilibria. Rod [Garratt] has suggested that, in nonconvex models, sunspots can be symmetry *making*. The stochastic allocation, for example, provides a method for "sharing" indivisible goods. Yves has an important 1982 piece in *JET* formally defining "extrinsic uncertainty." Rod, Todd, Cheng-Zhong, and I simplify this idea for the special case of continuous sunspot devices.

While we were in Paris, Wayne Shafer was the acting editor of *JET*. The administrative editor's job was vacant after our year in Paris. Susan [Schulze] agreed to take this post and has held it since—doing a great job.

In 1984–1985, I was a fellow at the CASBS in Stanford. The CASBS is close to the Stanford campus, but it is separate from the university. This place is Shangri-la. From my CASBS study, I had clear views of both Hoover Tower [Stanford] and the Campanile [Berkeley]. Jim Peck was then my student. We spent hours together every day at the CASBS. We grew to be very close. Jim is a very smart and deep guy. Thanks to him and Shangri-la, it was a spectacular year. Jim and I did imperfect competition and sunspots there. I also completed papers with Yves on retirement of the public debt.

My 1972–1973 visit to Stanford was not a sabbatical. I was a visiting professor on unpaid leave from Penn. Mordecai Kurz was a terrific host. He found room for me and *JET*. Thanks to Mordecai, Stanford was our home away from home during most of the Penn years. The theory profession is in Mordecai's debt for his valuable summer workshops. If it hasn't been done already, someone should write a chapter on the IMSSS/Israeli connection and its role in bringing game theory into economics and economics into game theory. During this time, I became close to Bob Aumann. Bob, Jim [Peck], and I wrote a CAE working paper on sunspots and asymmetric information. I also became close to Paul David, an excellent observer of history-dependent technological evolution, and Michael Maschler, who with Ariel Rubinstein taught me modern game theory.

MD: Did you have sabbaticals from Cornell?

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Shell: Yves and I joined Walt Heller and the others at UCSD [San Diego] for Fall Quarter 1992. I spent a leave as Jess's [Benhabib] guest at NYU in 1999–2000. Jess and I organized at NYU in May 2000 a successful conference on extrinsic uncertainty in economics, in which the theorists and the applied guys exchanged ideas. Susan and I spent one June on a Fulbright at the IAE in Barcelona as the guest of Joan Esteban. We spent one May with Osamu and Yoshiko Nishimura, living in a beautiful, old house on the Doshisha University campus near the imperial palace in Kyoto. We spent another May visiting Churchill College and the Faculty of Economics in Cambridge, England.

MD: Tell us more about your joint work on market games.

Shell: As I said, Jim [Peck] and I picked up on market games in our study of sunspots and imperfect competition. Jim pointed us to market games. He had been an undergraduate student of Martin Shubik at Yale. I love market games. Neil [Wallace] was right to teach them to Minnesota freshmen. In an unpublished appendix to our 1991 *REStud* sunspots paper, Jim and I establish the existence of pure-strategy Nash equilibrium in which all markets are open. (Closed markets are easy.) The best GE-style paper on market games is the one in the 1992 *Journal of Mathematical Economics* with you [Steve Spear] and Jim [Peck]. We provided a complete analysis of the structure of the equilibrium set. Jim and I in the 1990 *GEB* used the market-game model to show how short sales can create liquidity. With sufficiently large short selling, there is a Nash equilibrium as close as you like to some competitive equilibrium even if there are few consumers. Jim and I (in the 1989 Texas volume) show that, with imperfect competition, market structure matters and there is no applicable concept of "complete" markets. In particular, the Arrow securities game and the Arrow–Debreu contingent-claims game *never* achieve the same Nash equilibrium allocation if any income is transferred across states of nature. A corollary to this is that these models have no proper SE allocations in common. You [Steve Spear], Aditya [Goenka], and Dave Kelly have done very good work in generating deterministic business cycles in market-game macroeconomies. I encourage you to continue this as a callibration exercise and to include sunspots.

MD: Why did you leave Penn and come to Cornell?

Shell: There were compelling personal reasons to leave Philadelphia. I would rather not go into them. Fortunately, while I was out at the CASBS, Ken Burdett (the Cornell chairman), Mukul Majumdar, and Jan Svejnar offered me Cornell's oldest chair in economics, the one held by my friend Fred Kahn, the great deregulator. It was with regret that I left Penn. Penn had been extremely good to me. I was on good terms with the administration. I had friends in other departments. In the spring of 1986, I marched in the Penn academic procession to see Stephanie get her BA in French literature. Jason was admitted to Berkeley that year. So we all left West Philly together.

MD: Tell us about Cornell.

Shell: It was good to join my dear friend Henry Wan. As you know, Henry is working on the stages of growth and the role of trade in development. Henry

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FIGURE 5. Marching in the Penn graduation: Stephanie and Karl Shell, May 1986.

rekindled my interest in endogenous growth. I also had a very warm welcome from the Cornell mathematics community (including Anil Nerode, John Guckenheimer, Lars Wahlbin, Mike Todd, Gene Dynkin, and Terry Fine). This was a pleasant surprise, since I think of myself first as an economist. My first Cornell research assistant was Raghu Sundaram. Raghu and I spent late nights on tough economics problems that we didn't crack. Raghu is a good friend. It is good to be with Tapan Mitra. I have always admired Tapan. He uses simple math in a very deep and elegant way to crack important questions. He has a nice characterization of bonafide fiscal policies in a simple OG model.

MD: Is this when the CAE [the Center for Analytic Economics at Cornell] began? Is the CAE really CARESS-West?

Shell: Cornell had some excellent economists on the faculty and some superb students. But in 1986, core research in economics was limited to only a tiny fraction of the economists at Cornell. There was little research infrastructure. Under the leverage of my appointment, Ken [Burdett] (the chairman), Nick Kiefer, and I set up the CAE. I was the first director of the CAE. At first, the CAE had two programs: theory and econometrics. Now there are three: macro, micro, and econometrics. David Easley is the current CAE director. I am currently leading the macro group.

The CAE and CARESS are quite different organizations, reflecting the differences between Cornell and Penn. Penn was in 1968 a research department emphasizing macroeconometrics. CARESS was needed to provide infrastructure for the theorists at Penn. The CAE was needed in 1986 to provide infrastructure for core research in micro, macro, and econometrics at Cornell.

MD: Were there others at Cornell?

Shell: Of course! I met you [Randy] when we were colleagues at Cornell, but it wasn't until later—after you had moved to Penn—that our collaboration began. Here is how I remember it. You were giving a seminar at Cornell and I was being obnoxious. You had intrinsic uncertainty. I said, why not extrinsic uncertainty? You had N commodities. I said, why not 2? You said, why not 1 (and, say, two states of nature)? You were right.

My first student at Cornell was Aditya Goenka. Aditya has done work in an important area: the design of institutions and mechanisms that behave well in the face of sunspot shocks. Others with contributions to this area are Mike Woodford, Bruce Smith, and Todd Keister. My work in progress with Jim [Peck] on sunspots and bank runs also fits in. Following Aditya, there was Rod Garratt. Rod extended the Prescott/Townsend lottery model to economies with only a finite number of consumers. With only a finite number of consumers, there is no "law of large numbers." Hence coordination is required.

Yves [Balasko] visited Cornell for the Fall of 1987 as a guest of the CAM [Center for Applied Math] and the new CAE. He had a profound effect on our graduate students.

For several years, our macro program was led by Bruce Smith. Bruce and Valerie Bencevenga are dear friends. Bruce was—and is—incredibly productive, with a major presence in so many of the important areas (and some arcane historical ones) of macro and money. Bruce and I have good papers waiting to be written up on transaction costs, exchange rates, and money versus bonds. It was a sad day when Bruce and Valerie left for Texas. I miss them.

I "lost" two students at Cornell. Mike Kelly wrote (but did not publish) a near-classic thesis chapter on the effects of restrictions on government budget deficits. He then went to Wall Street. (Eric Fisher, then a Cornell colleague, did a related theoretical and empirical piece on the "irrelevance of current account deficit" in international economies.) Xuelin Yang wrote a splendid dissertation (related in part to my work with Bruce) on sunspots in dynamic economies with more than one asset. He analyzed blue money and red money, blue machines and red machines, and blue land and red land. Last I heard from Xuelin, he had gone to the Singapore equivalent of Wall Street.

Early in his career, Christian Ghiglino visited us at Cornell. He is a smart guy with an open mind. Christian and I did a paper on the effects of budget restrictions *à la* Maastricht. It has led us to rethink optimal taxation and all the so-called

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FIGURE 6. Karl and Susan Shell, Jada and Rod Garratt, San Diego, Fall 1992.

irrelevance results that require the government to be able to choose freely from the set of equilibria. This is not unrelated to sunspots.

Without the research assistance of Todd [Keister], Frank [Fisher] and I would not have finished our second book on index numbers. Todd is—as I indicated—an expert on the design of mechanisms in the face of sunspots. Todd and Gaetano [Antinolfi] did a piece for *ET* on sunspots and (real-world) options. If you have a continuous sunspots variable, perfect markets, and convexity, you will have the Cass–Shell Immunity Theorem in "finite" economies, but this requires a continuum of Arrow securities. Todd and Gaetano show that you can also get sunspots immunity if there are four properly chosen options with correct strike prices—two puts and two calls. Huberto Ennis has worked on the role of sunspots in decentralized trading *à la* Nobu Kiyotaki, Randy [Wright], and Neil [Wallace]. Huberto has also done good work on banking. Huberto and Todd have joined forces in building a computer simulation model to evaluate the robustness of government policy in the face of learning and extrinsic uncertainty. Luis Rivas worked on taxation and growth.

MD: What are your thoughts about the future of economics, macroeconomics, economic dynamics, and general equilibrium theory?

Shell: I think we have merely scratched the surface in building useful macro-policy models. Let me go slowly to my point. If you were building a policy model in the face of uncertain technology, you would not be wise to choose a policy that maximizes subject to the "best-fit" technology being the actual technology. You would maximize expected welfare or you might even max/min or something.

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FIGURE 7. Cornellians about to drive to the Rochester Macro Conference, about 1998: Huberto Ennis, Todd Keister, Karl Shell, and Christian Ghiglino.

The same would be true if you were uncertain about demand. Now what if we are uncertain about belief formation? One thing you might do is look for some empirical regularities in belief formation. Some of this, but not very much, has been done. Then you might close the model with one of these empirical regularities (e.g., the quantity theory of money). But the "empirical regularity" is not likely to be a perfect fit. There might be "shocks" to belief formation. There might be uncertainty about its global applicability. So, it would be better to maximize the expected welfare or max-min or something, but this can be tricky. Beliefs can depend directly on government policies. In the old policy literature (e.g., Diamond–Mirrless), it is implicitly assumed that the government can select from among the set of competitive equilibria. This is unrealistic. The government can affect the set of equilibria, but typically there will be many different self-fulfilling beliefs for each government policy. This is true in virtually every model, but especially in OG and monetary models. So it is not an easy matter. But we will have to work it out. Grandmont, Woodford, Smith, Goenka, Keister, Ghiglino, Peck, and others have already taken some steps in this direction.

MD: Today macro and general equilibrium are very close, closer than general equilibrium is to the rest of micro. Your scholarship has played an important role in bringing macro and GE together. *JET*, too, has contributed to bringing them together. What are your thoughts on this?

Shell: Your observation is correct, but there is a greater truth: Now there is one scientific economics. How we divide the subject is more a matter of politics

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Figure 8. Karl Shell, Jim Peck, and Christian Ghiglino at the General Equilibrium Conference in Venice, May 1996.

and sociology than science. Macro is altogether different from what it was in the sixties. It is theoretical, involving a lot of pure and applied general equilibrium. In fact, many of the most important advances in general equilibrium in the last 25 years were inspired by macro. I am thinking about OG and sunspots (including GEI). The structure of the equilibrium set (*à la* Debreu, Balasko, and Mas–Colell) has been heavily applied in OG, SE, and GEI. So there was this positive "pull" from macro and finance for GE.

On the other hand, pure mathematical GE—distant from applications—did get in a rut: existence and the welfare theorems in new mathematical spaces. This was great for spaces defined by interesting economic problems (OG is a good example), but it was pretty boring for most of us if we could not find the economic motivation.

Of course, there are also cycles in research. Growth theory was soaring in the sixties and into the early seventies. By the late seventies, too many obvious dissertations had been written and published. The profession lost interest in growth theory for a while. Now growth theory (and empirics!) are back. There is some reinventing the wheel, but there are also fresh emphases. GE will rally after reinvigoration from macro and game theory. GE will be somewhat less "Walrasian." It will have to cope with asymmetric information and the lack of coordination implicit in economies with money and securities markets, but without forward and contingent-claims markets. It will have to be more dynamic (*à la* game theory) than Arrow-Debreu or Irving Fisher. Recontracting and limited coordination must be accommodated.

The recent advances in noncooperative game theory have been astounding. Economics is much richer today because of game theory. Some of the split in the economics curriculum is along functional lines. Some of our students will teach in business schools, where there is little emphasis on macro and general equilibrium. Other students will work in Federal Reserve Banks, where—so far—there is less emphasis on game theory.

I personally think that a natural meeting place of GE and GT [game theory] is in the market-game model. Market games are the only way I know to do general equilibrium with imperfect competition. The market game is the best foundation for competitive equilibrium. It is the safest GE approach to modeling asymmetric information. It partially "explains" the spectrum of open and closed markets. It is an excellent tool for the analysis of (symmetric-information and asymmetric-information) sunspots. It will be useful in understanding when a publicly observed randomizing device is needed and when it is not.

MD: It is time for the workshop. Thank you for your ideas and your memories. It has been fun.

Shell: Thank you. It *has* been fun. More fun than I was expecting it to be. Economics has been very good to me. I hope to be able to do a lot more. I wish for the next generation of economists a good research environment. I am optimistic. They can build on the very substantial progress in economic theory and the reinvigoration of empirical economics. Of course, there are always storm clouds. Our subject is necessarily "political." One must always worry about political intervention in our lives, as Leo Hurwicz, Franco Modigliani, and Lawrie Klein know very well. For policy economists, there is always a fine line between being relevant and being co-opted. But, for now, I see more pluses than minuses.

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