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MD INTERVIEW

INTERVIEW WITH DAVID CASS

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and

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David Cass is undoubtedly one of the central contributors to modern dynamic economics. His fundamental contributions include work on optimal growth problems, overlapping-generations models, sunspot equilibria, and general equilibrium models with incomplete markets. His research has shaped in profound ways the manner in which we do both micro- and macroeconomics. From laying the foundations of real business-cycle theory via the Cass-Koopmans model, to providing us with general tools and techniques to analyze dynamic economic models, to furthering our understanding of monetary economics, to making fundamental contributions to the economics of extrinsic uncertainty, Cass's work has played a major part in the development of much of modern macroeconomic theory. In addition to being a first-class scholar, Cass is also truly his own man and a free spirit of the highest order.

In this interview, we tried to gain some insights into the story of David Cass and his approach to economic theory. Also, given the title as well as the intended readership of *Macroeconomic Dynamics*, we made a real effort to get him to discuss modern macroeconomics and the influence his work has had on its development. We edited out some parts of the discussion in the interests of space, but what remains is essentially unedited. As most readers will know, David Cass has collaborated extensively with Karl Shell over the years. We talked to Shell a few weeks after talking to Cass, and that interview is scheduled to appear in a future issue.

We met with Dave in his office at the University of Pennsylvania's Economics Department just before noon. Amid the boxes and piles of articles, books and CD's, he sat in his standard jeans and T-shirt, looking about as disheveled as he usually does. We chatted there for a while, went out and continued over lunch, and

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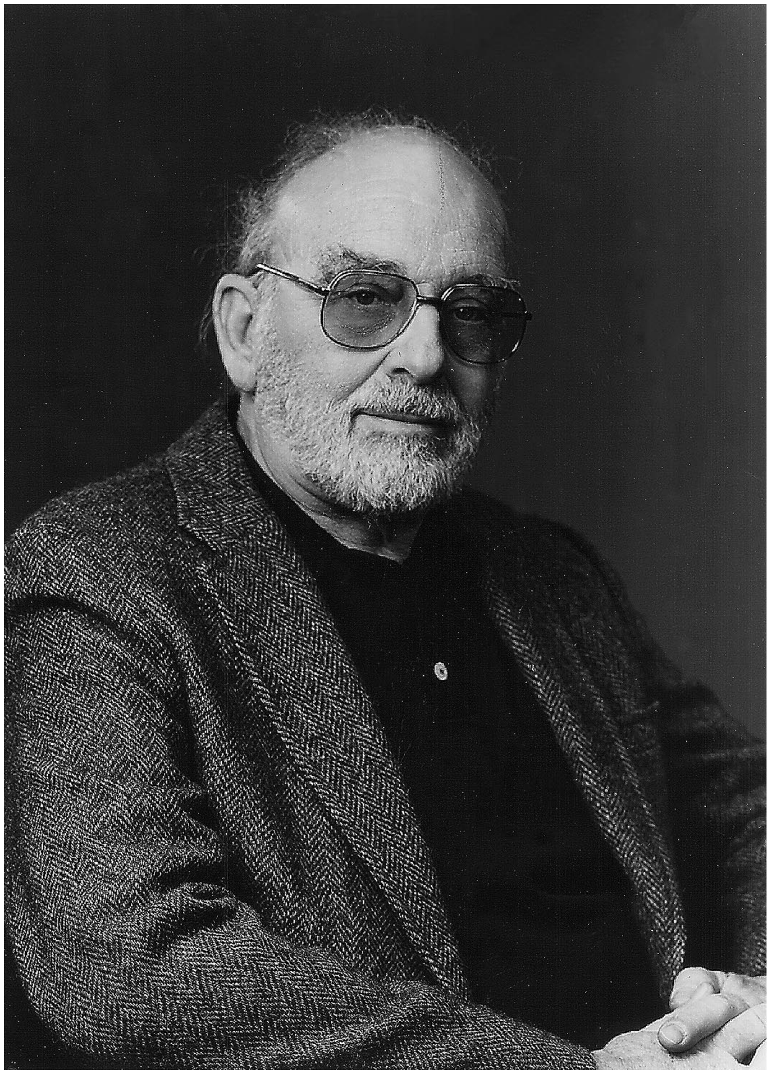

Figure 1. David Cass, June 3, 1994, on the occasion of receiving an honorary degree ("docteur ès sciences economiques honoris causa") from the University of Geneva.

then returned to complete the interview several hours later. It was an unseasonably warm day in February, and Friday the 13th to be exact. That is traditionally an unlucky day, but one that turned out in this case to be a real treat, at least for us! We hope that you get as much out of this conversation with Dave as we did.

Keywords: Optimal Growth, Overlapping Generations, Sunspot Equilibria, General Equilibrium Models

MD: Let's begin by talking about graduate school and your advisor, [Hirofumi] Uzawa. How did you first hook up with him?

Cass: OK. I viewed Stanford's graduate program as being completely chaotic. I'll give you an example. The first year I went to Stanford, they had a qualifying oral in the first semester and everybody had realized that this was patently absurd. So they had abolished the requirement, but they'd scheduled the orals already, so they decided to hold them. My oral—and I didn't even know people on the Stanford faculty very well at the time—my oral was composed of Ken Arrow and somebody else. When I found out about Arrow, I was terrified. So I went in and Ken asked me a question and I gave some half-assed answer, and he has this capability of taking someone's answer and then reframing it in a way that makes a lot of sense. So, my qualifying exams consisted of my short responses to Arrow and then him elaborating to make sense of them.

But the point is, they had this requirement that they abolished but they scheduled, and that was typical. So basically, at Stanford you were kind of left on your own as a graduate student. There was just no coherence in the program. Now, I don't remember exactly how I first met Uzawa, but there was a mathematical economics group who had offices separate from the department in a little house on campus called Serra House, and that is where what I consider the really good people at Stanford were: Arrow, Uzawa , Scarf. We had other kinds of mathematical social scientists there. And somehow, Karl [Shell] knew about Serra House right away, and we had our offices there.

MD: Did you and Karl enter in the same year?

Cass: Yeah, and somehow Karl introduced me to Serra house. I don't remember how we got involved with Uzawa, but we just got involved with him. Maybe he ran a seminar or something. I don't really remember how we met, but it was clear that this guy was really into research and very good at directing people, so we hooked up with him. Then, the last two years at Stanford (I stayed four years), I basically spent at Serra House working with Uzawa. He always had seminars going. Uzawa, in my view, by conventional standards, is a terrible lecturer, but he is an awesome teacher. His greatest virtue is that when he lectures he shows you how he does research. If he doesn't prepare, he will tell you about a paper he is working on, and he gets up and basically re-creates the mistakes that he made and corrects them. He explains why he decided to do this and that, and it is just like you are taught by doing research.

So, I took a couple of courses from him and found them great, but from conventional standards they were probably a disaster. He taught econometrics, and he wanted to calculate some estimator, probably a limited information maximum likelihood estimator, but he didn't really remember anything about it. Half of the course consisted of Uzawa coming in and starting to prove a theorem about this estimator, and he would go on for about an hour or an hour and a half, and then he would realize that he had gone off on the wrong track again and he would say "Oh, sorry." Next time he would start up again—it was really incredible! But it was interesting. He has a really good mind for working from first principles and for working out how you solve a problem.

Uzawa was a marvelous person to work with. I model my career in terms of working with graduate students after the experience I had working with Uzawa. He treats them exactly as equals and he spends a hell of a lot of time one-on-one with them in all kinds of situations. Don't think it was only in the office—it could be going to a bar, or any of that. He just spent an enormous amount of time. Now Uzawa probably never read anything that I wrote. I am sure he didn't. But he always wanted to talk about it. He'd always force his students to deal with that, and he had a group of students in seminars, so that all the students knew what the other students were doing. Of course, we had a focused subject—growth theory and, more particularly, applications of a fancy version of the calculus of variations, the maximum principle, to growth models. So we all had a common background but, actually, that personal thing is one of the reasons I got into trouble with the Penn administration. One of the basic issues I had about dealing with graduate students here was that somehow the administration wanted me to distinguish very carefully between my professional activities and my social activities, and I told them that wasn't consistent with my idea of how you deal with graduate students, and it isn't. So this is all in response to the question of how did you meet Uzawa—and the actual details about meeting Uzawa I do not remember.

MD: Did you know that you wanted to do growth theory?

Cass: Not at all.

MD: What was your undergraduate training?

Cass: I was a joint Economics–Russian Studies major.

MD: Russian Studies?

Cass: Yeah. Very anomalous because languages were probably my weakest suit.

MD: Where was that?

Cass: The University of Oregon. I always thought that I was to become a lawyer because that's a tradition in my family. I spent a year at the Harvard Law School and hated every minute of it. I spent most of my time re-reading great Russian literature, and I learned how to answer exams just by deductive logic. I'd memorize a few definitions and go from there, which got me through. Then I went in the army and I decided that what I really wanted to do was to go back to graduate school in Economics, and I decided to stay on the West Coast. I was very lucky. I didn't know anything about graduate schools but it seemed like the major choices were Berkeley and Stanford. Just by chance I decided Stanford rather than Berkeley, and I think it was a hell of a good decision, because the faculty that I got inspired by are really world class.

MD: So why did you decide to go to graduate school in Economics?

Cass: I liked economics, and I realized that my undergraduate degree was the tip of the iceberg. They were just barely getting into the use of equations in class and it was kind of fascinating to me—the idea of being formal about a social science.

MD: So you probably had very little mathematics training when you went to grad school.

Cass: I had virtually no math training. I had taken college Trig, Algebra, Geometry, and that was it. In fact, I remember the first day of class at Stanford there was a guy teaching a macro course; his name was Bob Slighton. The first day of class he wrote down a general equilibrium model and decided that he was going to calculate a multiplier, which is just a derivative of the model. He filled the blackboard in the front of the room and the side of the room and I didn't understand a word of this. I knew what a derivative was but I didn't know what a partial derivative was. He was doing all this partial differentiation. Of course, in those days, in terms of partial differentiation, people didn't really understand what they were doing. They would write down differential forms, what in differential topology are called the tangent spaces, and they'd be dealing with calculus on manifolds but didn't really understand it. The technique was kind of incomprehensible.

I went home from that class and I said, "Well, you're not really prepared to sit in graduate classes in economics," so I basically re-registered for calculus and statistics and I think I sat through Slighton's class, which was excellent. The micro class was more problematic. It was taught by a guy named Melvin Reder, a labor economist, and he came in the first day of class and put his feet on the chalkboard and said something deprecating about economic theory, so I never went back. The first term I spent learning introductory calculus and probability theory. The probability theory was actually taught by a guy whom I later learned was a world-class probabilist. It was marvelous because he introduced everything via examples, and then you could study for the exam because you had a good feel for what probability theory was all about.

MD: Did you and Karl work together at Stanford?

Cass: No. One of the funny things about Stanford, and this may be true in other graduate programs too, they preselected people that they assumed were going to be stars. Karl was an undergraduate math major at Princeton who basically went to Stanford because he knew about Ken Arrow, and Karl was a preselected star. (He was not selected as top star of the class; I forgot the name of the guy who was, but he turned out to be a real bust.) Since I decided not to take economics the first year I was there, I really didn't have much to do with economics students, and I only got to know Karl probably toward the end of my second year. He introduced me to Serra house, and there Uzawa's students all worked together because we all knew each others' problems, so we could communicate very quickly. But none of us actually wrote papers together. Karl and I started collaborating on papers much later, in the early 1970's, but, again, Karl knew my thesis and I knew Karl's thesis throughout the whole development phase, so basically who contributed what to what as graduate students with Uzawa was always up in the air.

MD: And how were you led to your thesis topic?

Cass: Optimal growth? Well, it was basically the fascination Uzawa had with the maximum principle.

MD: What growth theory did you know before that?

Cass: At that period there was a distinction between what we wanted to do, optimal or prescriptive growth, and descriptive growth a la Solow. He wrote a ton of papers, starting with the very famous paper on the one-sector model, and then he wrote many others describing competitive growth models that had more goods, and maybe some specialized technology, and he kept repeating how you describe a competitive equilibrium and its efficiency properties (something Malinvaud did much more elegantly in his justifiably famous *Econometrica* paper). So that's descriptive growth theory. Then there was this famous paper by Ramsey, and Uzawa was clearly fascinated by two-sector versions of the neoclassical growth model. He wrote several papers on that. Then he decided to go into optimal growth theory and produced a paper which was essentially a two-sector model with a linear objective function. Basically, he re-created the calculus of variations himself—he is a very original guy—and then he discovered the maximum principle and became fascinated with it. Uzawa also gave a seminar on economic history in which he went back and took all the great names in economics, starting with Ricardo, Marx, . . . and reproduced what they were doing as a growth model. I was very influenced by Uzawa's work. I didn't even know about Ramsey at the time.

MD: That is interesting because sometimes one hears about Ramsey as this hidden classic. But didn't some people know about Ramsey? Didn't Uzawa?

Cass: No, I don't think so, because I didn't find out about Ramsey until after I had written the first chapter of my thesis on optimum growth. And then I was, to be perfectly honest, I was a bit embarrassed about it.

MD: How did you discover Ramsey?

Cass: I don't remember now. Maybe somebody mentioned it; maybe Uzawa knew about it, but not really, because he thought my contribution was absolutely seminal. In a way it is not at all. In fact I always have been kind of embarrassed because that paper is always cited although now I think of it as an exercise, almost re-creating and going a little beyond the Ramsey model.

MD: Ramsey had no discounting and you did have discounting. That's one difference, right?

Cass: Ramsey had no discounting. He made a big point of talking about the correctness of the social welfare function from a moral viewpoint, I believe, maybe in his side remarks. Tjalling Koopmans was very sensitive to this issue, too, when he wrote a paper of this sort. It turns out to be much harder to solve the problem with no discounting because, even if the objective is written as a function of a functional, it is not well-defined because it may be infinite-valued, and you have to use a trick to make sense out of it. You have to take the difference between utility of consumption and utility of the golden rule consumption so that you get a function that is well defined. As a technical aside, it is very interesting that the Ramsey problem is a counterexample to something which people now always do. I think they do it in macro without even thinking about it, when they do dynamic optimization, and they write down transversality conditions as necessary, which I also said something about in my thesis, and this is dead wrong. The Ramsey

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problem is a counterexample to this: You have an optimum, but it doesn't satisfy the transversality condition.

MD: Is this an issue only in the no-discounting case?

Cass: Yeah, that's in the undiscounted case. It has to do with the condition in capital theory that is called nontightness, which is a sufficient condition for the transversality condition to be necessary, and basically is an interiority condition that enables you to use a separating hyperplane theorem. Now I have forgotten what the original question was!

MD: How you came to the optimal growth problem.

Cass: Actually, even though Uzawa always went back and read literature and was always motivated by literature, I didn't pick that up from him at all; I just decided to work on this problem because the techniques were new and exciting and it seemed like an interesting problem. So I taught myself the maximum principle, some differential equations, and so on, by talking to people, seeing Uzawa working, and basically reading math books. Our bible at the time was Pontryagin's original book on the maximum principle. That is really interesting too, because that book is very geometric, and Pontryagin's blind.

MD: Was it in Russian? You would have had a natural advantage there.

Cass: I could have read a little bit of Russian, but it was translated. Anyway, he's blind, and yet all of his thinking is purely geometric; he pictures things. So I just put the two together, and then Uzawa thought this was great. I'm not sure why, I guess probably because Tjalling Koopmans was working on this problem and Tjalling was a bit of an idol for Uzawa. Actually, Uzawa liked to one-up people. At some point he was talking to Tjalling about the problem, and Tjalling was describing what he was doing and Uzawa interrupted and said, "Well, I have a graduate student who did that problem." Then Tjalling got very nervous about it, he was always very nervous about . . ., oh, authorship and who was first and that sort of thing, and we had some correspondence. Koopmans was also very interested in the no-discounting case, so he solved the much harder problem in some ways, in addition to solving the problem with discounting. Tjalling did all his analysis from first principles; he derived all of the conditions.

MD: Then you went on the job market.

Cass: I'll tell you a story about the job market that reflects the character, the idiosyncratic character, of Uzawa. Uzawa originally engineered for me to have a post-doc at Purdue, which was a pretty good department, but then he had contact with Koopmans at the Cowles Foundation, who were interested in doing some hiring. Uzawa decided that would be a better job, but his idea of supporting a student on the market was that it was immoral to have more than one offer. So I went to the winter meetings in Boston with just an interview with the Cowles Foundation, and a couple more that I had arranged that turned into disasters. I spent most of the time in the hotel room watching football, and I was rooming with Karl who had a million interviews! It came down to the last day of interviews and everything depended on my passing an interview with the Cowles Foundation, which was a lunch with Tjalling and Herb Scarf and I don't remember who else, very likely Jim Tobin. I talked a little about my thesis, but Tjalling already knew about it, and he decided to question me with "What will you be working on 10 years from now?" As with any graduate student, I couldn't even think two months ahead. I had no idea what I would be doing!

For some reason, they couldn't make me a regular appointment, and I remember Tjalling had obligated himself to make an appointment that it turned out he couldn't make, so he signed me as a research associate at the Cowles Foundation for one year, on a one-year appointment, with the promise that it would be extended and I'd become an assistant professor as well as research associate. You can't believe salaries in those days, even adjusted for inflation. My salary when I started was eight thousand dollars.

MD: Tell us about Yale.

Cass: Yale was a great postgraduate education. The Cowles Foundation, at that point, had a lot of money, and a policy of hiring or having in residence lots of junior faculty. The physical setting was in a separate building, in a separate little house. People like Tobin, in particular, really encouraged us. I really remember my days at Yale very fondly. When I was first there I talked a lot with Ned Phelps. Then of course I met Manny Yaari, and Manny and I talked a lot and ended up writing papers just based on these conversations. The consumption loan paper came about this way.

MD: And that was when you got into overlapping generations models?

Cass: Yeah, overlapping generations was with Manny.

MD: Was the overlapping generations model something many people were interested in then, in the late 1960's? Presumably not, since Samuelson's paper was published in the 1950's and then sat there for a long time without attracting much additional attention.

Cass: Yeah, it sat there for a long time. The Cass and Yaari paper used to have a lot of cites, and I think the main reason for that was it revivified interest in the overlapping-generations model. That's not a paper that I think of as a great paper because we were really struggling. I don't want to be quoted on this, but in my opinion I don't think that there is much in that paper that survives.

MD: So you and Yaari were chatting about things and began talking about overlapping generations. Had you and Karl talked about it previously?

Cass: No, I don't think so. My recollection is that I really first thought about it the first year I was at Cowles.

Going on about Cowles, my second year and into my third year, there was this big influx from MIT: Joe Stiglitz, Marty Weitzman, Bill Nordhaus, and others, too, and the environment was just great. Hell, I shared offices with Joe Stiglitz. I probably never would have gotten to know Joe and take him very seriously, because he is so quick and so sloppy, except that we shared an office together. Joe used to come in and sit down in the morning and say, "I am going to write a paper today." And he'd sit at his typewriter and write a paper. This just drove me nuts because I am very deliberate. So I got into a habit, when Joe would tell me he was going to write a paper about something, of talking with him about it. He would

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come up with some point and I would say, “Well, Joe, how do you know that’s true?” Actually, we ended up writing a lot of papers together based on the fact that I would ask Joe, “How do you know that that is true?” One of those papers is still cited a lot. It is about portfolio choice—the reduction to choice between two assets. I think that was a hard paper and we have really cool results from it, but it’s just to justify a simplification. In order to justify the simplification, it turns out you have to make extremely strong assumptions about preferences.

Anyway, Cowles was extraordinary. Very stimulating. For the most part, nobody was proprietary about sharing ideas. Nobody would try to protect their ideas. They talked about them.

MD: Was Yaari there as a visitor?

Cass: No, Manny had had his first appointment with Cowles. He was promoted to, I suppose, associate without tenure, a standard step. I was too, while I was there. Then he came up for tenure, and he had to decide whether he was going to go back to Hebrew University in Israel or stay at Cowles. Yale actually made some, I thought, really stupid personnel decisions. Partly it was motivated by the fact that they wanted to keep a through-put of junior faculty; they didn’t want to get a large senior group. So they turned down Manny. Another example of a serious mistake is that they turned down Ned Phelps. Hey, he has got an extraordinarily creative mind. So does Yaari. I have tremendous feeling and respect for both of them. And they were turned down flat.

I was also one of the young people that was through-put at Yale, if you will. I don’t think that they were seriously considering giving me tenure and, to be honest, I didn’t have a lot of publications when I would have come up for tenure, maybe a half dozen. I remember, I was on the market and I went to Johns Hopkins, and the department chair there told me they couldn’t seriously consider me because I didn’t have enough publications. But for some reason Dick Cyert had decided several years before that he really wanted me to come to Carnegie. Cyert is nothing if not tenacious—he kept after me every year. Originally (you probably don’t want to repeat this exactly), my view of Carnegie was that it was a serious place but that the typical paper was just to apply the Kuhn-Tucker Theorem to some problem, and I didn’t find that very exciting. But then I went to Carnegie and met some of the junior faculty. I knew Bob Lucas very well and he was a big draw.

MD: How did you know Lucas?

Cass: Well, Bob was at Chicago, probably just finishing up, when Uzawa moved from Stanford to Chicago. Bob never worked with Uzawa, but Bob’s work was probably also not particularly fashionable with any other faculty there because he was interested in doing the kind of things Uzawa did. So he became sort of a semiprotégée of Uzawa. I don’t want to exaggerate that, but anyway, I remember my first encounter with Bob intellectually. He gave some version of a dynamic IO problem, something about industrial structure, firms entering and exiting, as I remember. Anyway, I met Bob because Uzawa kept track of his graduate students and he used to hold conferences when he was in Chicago, where he had also built up a group of graduate students. One of the first conferences I went to, Bob Lucas was there and that is how I met him. He was obviously very smart, very serious, and we got along very well and so he was quite a draw to go to Carnegie. I knew he was not the kind of guy to just apply Kuhn-Tucker conditions, so he clearly did not fit my stereotype.

So I went to visit and I met other people at Carnegie. Len Rapping, for example, was a very interesting person. He was originally a die-hard Chicago market-oriented person who had a complete change of heart during the Vietnam war, but still a very interesting and smart guy. The other person that I remember who really impressed me was Herb Simon, who was clearly a really interesting and creative individual. And I said, “Well, your stereotype is wrong, and that might be a very interesting place to go.” When I went to Carnegie it was a very good place. It was not a business school; even though they had a Master in Business Administration program, it was just not a traditional business school.

MD: This would have been around 1970?

Cass: Yes, this was in 1970. It was a small faculty and a relatively small number of MBA’s. We taught the MBA’s the same as we taught the Ph.D.’s almost, and at that point, unlike today, the MBA’s came and they were expected to perform, and they didn’t raise questions about whether the stuff was too hard or didn’t have anything to do with business. Carnegie was an absolute innovator in introducing quantitative techniques, and especially economics as kind of a broad basis for most fields in business. In fact, a great example of that was the development of finance as something serious. The finance people won’t like this but, to learn finance, you basically learn economic techniques, and that originally took place at Carnegie and took place in the standard way that Carnegie operated. If they had a course to teach, they would just assign somebody to teach the course. Merton Miller was one of the people assigned to teach a course on finance along with Franco Modigliani, and so: Modigliani-Miller. They were puzzled by something, and used economic methodology to solve it.

Carnegie was really a great place. They used the MBA program also to find good Ph.D. students. You didn’t mind teaching MBA’s at Carnegie because you could teach them a serious course. You didn’t have to pull your punches because they were expected to learn programming, expected to learn serious economics, serious econometrics, and so on. The other thing about Carnegie is that it had a very good system for supporting and encouraging young faculty to interact and to have time to do research. They were really good, as Cowles had been, about teaching loads, summer support, secretarial support, and support for travel until you were well enough recognized that you could go out and raise your own money. So I had nothing but respect for Carnegie Mellon, GSIA at Carnegie Mellon, and the team run by Dick Cyert. I have enormous respect for Dick.

MD: What were you working on in those days?

Cass: One of the nice things about Cyert was that he basically paid for a year’s leave between being at Cowles and being at Carnegie, so I spent that year in Tokyo, and I wrote several papers there. One I really liked the best, I think it is one of my best papers, and I don’t think that it is one that is very widely read. It

Figure 2. September 11, 1993, conference at Carnegie Mellon University in honor of Dick Cyert. Pictured (from left to right) are Dave Cass, Robert Lucas, Dick Cyert, Allan Meltzer, Edward Prescott, and Timothy McGuire.

is solving the following problem: In the neoclassical growth model, you can have competitive equilibria which are not optimal, not efficient, if you use consumption as the criterion. You can basically overaccumulate capital. The best example of that—an example by Ned Phelps—is if you look at the same neoclassical model and you look at a steady state that is above the golden-rule path, you can move from that steady state and take one step back to the golden-rule capital stock, and get a consumption bonus and have higher consumption ever after that. Being at the upper point you still have competitive prices, they are just not efficiency prices. If you look at those competitive paths, you can rule out the ones that are inefficient if you impose the transversality condition. So the transversality condition is a sufficient condition for ruling out capital overaccumulation.

I found this to be a very interesting problem: What is a necessary and sufficient condition? The transversality condition is a sufficient condition for efficiency, but is not necessary. The golden-rule path itself is a counterexample, as I said earlier. The golden-rule path is efficient, and for some criterion is also Pareto optimal, but there the transversality condition is not satisfied since the interest rate is identically zero. Manny Yaari and I had started working on this problem two years before, and we got one solution for it that was in terms of a condition that wasn't that interpretable. Now I know why I didn't like the condition. I wanted a condition on the price path itself that was necessary and sufficient, so I worked all year in Japan on that and got a complete solution. I really like that paper.

So I spent a year in Japan working on that problem, and then I wrote a couple of other papers on things I wanted to write about. One of them was actually very much Solow-like. I took the Wicksellian model, the point-input/point-output model and analyzed competitive equilibrium. I like that paper a lot, too, but it's very specialized. I doubt anyone has ever read it. And Joe and I finished our paper on portfolio choice that year. The biggest stumbling block for that was that in the paper itself there are computations for specific parametric forms and neither Joe nor I was that excited about, or that careful sometimes, dealing with parametric forms. So we really had a hell of a lot of trouble agreeing upon what was the correct way to write down these examples to illustrate our theorem. In the final version of that paper there were still algebraic errors; somehow neither of us took the responsibility for proofreading it.

Then I kind of fished around for a while. I worked more on growth theory. I got interested in the general problem, which was then very unfashionable because it was at the tail end of the neoclassical growth period, of the stability of competitive dynamical systems more generally. Karl and I produced a paper that I like a lot, although it might have been a little archaic even then, on this problem.

MD: Was that the first time you worked with Karl?

Cass: That was the first time Karl and I really worked together on a paper. Karl was at Penn at that time. Anyway, back at Carnegie I wrote some minor papers, like on the Hamiltonian representation of efficient production, a paper on duality; these were not major papers. Probably I got to talking with programmers and got back to doing things with programming at Carnegie. The guy I really talked to a lot was—I remember him well, in fact he died some years ago—a guy named Bob Jeroslow, who was really a mathematical logician turned programmer. He was extraordinarily clever. The big thing in programming then was integer programming, finding algorithms that would solve integer programming problems. There were lots of algorithms but people didn't have any idea of why they worked, and Bob was really good at constructing for any algorithm a counterexample that would never converge. I used to talk to him a lot. He started to get interested in economics and I got interested in programming again, so I wrote some programming papers.

After that, Karl and I got into the stability thing, which we spent a couple of years finishing up. Then at some point—I don't now how this should appear in this interview—the Dean, Cyert, became President and we had to hire a new Dean. At Carnegie the faculty was very involved in this process, and we actually talked a lot about the kind of person we wanted. For some reason we settled on Arnie Weber, who was a Chicago Business School labor economist. That turned out to be, from Carnegie's viewpoint and my own viewpoint, a disaster, because the guy had no feel for the Carnegie tradition at all. He did not understand the fact that Carnegie was quantitative, and that the quantitative emphasis was on economics, and that meant that you were going to have a lot of economists around. An example of this, a personal example, is that very early on Arnie called me into his office for some reason, and I had an interview with him. He told me that I was a luxury good and that I didn't do business. I did theoretical economics and it wasn't something that business schools could really support and he did it in a very obnoxious way that really pissed me off. And I said "f--- you, Arnie."

MD: Literally?

Cass: Yeah, I said "f--- you," and I decided that since I was working with Karl it might make sense to come to Penn, even though I had a few reservations about Penn because I knew it was very econometric-model oriented. But they made me a good offer so I couldn't turn it down.

MD: Could we stay on your time at Carnegie for a while?

Cass: Yeah, we could do that.

MD: Ok. Ph.D. students: One of the prominent ones you worked with there was Finn Kydland.

Cass: Finn Kydland, yeah, I was on his committee, and actually worked with him a lot on one or two chapters, but not most of his thesis. His thesis was all programming. Another one was Bill Barnett. I don't remember if I was formally on Barnett's committee. I know I talked to him a lot but I may have left before he finished or he may have just drifted off. The main group I worked with includes people who came my first year at Carnegie, such as John Donaldson and Bob Forsythe. Those two stand out in my mind.

MD: Kydland's work with Ed Prescott began the development of real business-cycle theory, and the workhorse model in real business-cycle theory is the Cass-Koopmans model. Did you and Finn ever talk about growth theory?

Cass: No, as I said, Finn when he was a graduate student was doing programming.

MD: Let's talk about Lucas's use of the overlapping generations model.

Cass: I'll tell you, that is an interesting paper we're talking about, in *Journal of Economic Theory*, an interesting paper. I wasn't so interested in the macro, but what struck me, and this is related to some of my later work, was the assumption that Bob made to solve for equilibrium, that the state variables were obvious (that is actually the first time that I thought about the sunspot idea). Bob and I had some long discussions, and I would say, "Well Bob, why is this the actual state space in this model?" That question came up—and now I am jumping ahead—after I came to Penn. At some point Karl and I started talking about that and we developed what we called the idea of sunspots. But the initial impetus toward that for me was talking to Lucas.

MD: Also, technically, Lucas's paper was one of the first uses in economics of contraction mappings.

Cass: Well, Bob was very fixated on using contraction mappings to get fixed points. I think maybe he always uses that technique. I don't think he even knows Brouwer's Theorem! No, actually he does. He just likes contraction mappings. Anyway, the view in capital theory, as I understood it, was that you could treat, from a fundamental state space, uncertainty as well as time. So a commodity index could represent time, uncertainty, and commodity characteristics like location, whatever you wanted. But the viewpoint in growth theory is precisely that equilibrium is just prices that depend on the underlying state space. Bob went a step further and—I'm not even sure how I would say it—it is more like a function of the underlying state variables, or to put it more accurately, the state space itself is generated via some underlying process through observed variables. So that's what the state space itself is, for instance, money and some actual random shocks. Money is one of the state variables, though it's actually defined on the underlying state space. The states of the world are described by money and a random variable that has to do with island-specific shocks.

The ultimate question is, "What is a state space?"

MD: Brock and Mirman was another seminal paper.

Cass: Brock and Mirman was kind of a milestone because they focused on introducing uncertainty into the neoclassical model. Where did I meet Buzz Brock? Somehow, Buzz was a student at Berkeley and I think his thesis had to do with optimal growth in a multisector model. That is probably when I first met him. Our careers overlapped in several dimensions, for instance, when Jan and I spent the year later visiting Cal Tech and Buzz spent part of the year there. We had quite a bit of contact when Brock was working on growth theory, and then we just kind of drifted apart. He is still very active. I just haven't kept up with him or much of his work.

MD: When you were at Carnegie and people like Lucas and Prescott were working on the new macro stuff, were you paying attention?

Cass: Not really.

MD: Or do you think that this work is more microeconomics?

Cass: It was clearly micro and was being called macro, and you know, actually, for some reason, I never talked a lot with Bob about it. I don't know why. We had a great personal relationship, but somehow we didn't talk much about that. Our styles are really different, so we didn't talk a lot about that work, except we would go to lunch together and we would talk about it more on a casual level, but it was not at the blackboard level. It probably had to do with the fact that Bob was in the Chicago tradition and was very concerned about empirical testing—whatever the hell that means—something that I have little sympathy for and very little interest in, to be perfectly honest. So there was quite a difference in viewpoints about why you did theory and what the relevance of theory is, and I am still of the opinion that theory is more a way of organizing your thoughts, how you think about the world. And it's strongest in providing counterexamples when people confidently claim that something is true in general. If you can construct a not-unreasonable model in which this phenomenon is not true, then [Bronx cheer]. You can't assert with any confidence that some proposition is true. Now this clearly does go over to the question of when an assertion is true or not true if you want to quantify it. You can stay at the qualitative level—like the Laffer curve, an idea that was by example. Then you can construct models, plausible models, where you can get either result, and that makes his proposition absolutely dubious. I don't know how the data look. Probably most regressions are very mixed: Take a bunch of data and fit some curve to it and then claim that you summarize the data with some curve and that's a dubious claim.

MD: This is probably a good point to ask you how you feel about calibration, as pushed by Prescott and others over the past decade or so.

Cass: The main problem I have with calibration is the level of abstraction of the models that are being calibrated. I mean, if you are calibrating something that is essentially like a neoclassical model, then I kind of wonder what the hell that means. I suppose when I thought about it (and I haven't thought in great detail about it, to be honest), the whole notion of calibration and how you say that you've got a model that fits the data well is pretty amorphous. For example, to say that it generates time series for certain parameter values that share certain characteristics with the observed time series, I think you have to have a formal methodology for talking about what it means for two time series to be close. I thought that when I paid attention to the real business-cycle stuff, the idea of what to calibrate or what a good model is was pretty vague. Now I am probably being unfair to the real business-cycle people, because there are some really smart people working in the area, and they've probably refined the idea of calibration and gone beyond the simple calculations of the original neoclassical growth model; but probably not very far beyond, because you're still dealing with aggregate time series. My student John Donaldson, who works in the area, is very good and I have a lot of respect for him.

But the thing about real business-cycle theory I suppose is that it is almost like a religion. I have talked quite a bit with Victor [Rios-Rull], whom I have a lot of respect for, who has this view, this view that he is convinced quite strongly about, that this is the only way to look at the world, to look at economics. When anybody tells me it's the only thing, I'm skeptical. I don't believe that using general equilibrium theory is the only way of looking at the world. I think I have learned a lot from game theory, focusing on strategic ideas, the importance of strategy, and imperfect information.

MD: Isn't that general equilibrium?

Cass: It can be, but there are other ways of looking at imperfect information and all these ways are important. But I also think that the general equilibrium model itself has a role, that it is still an important benchmark, and that there are still a lot of interesting things that can be done with that theory.

MD: That is one thing that you do have in common with Prescott.

Cass: Yeah, absolutely. But, if anything, maybe Prescott is more extreme. I have learned a hell of a lot being at Penn, where there are good game theorists. I mean, I have really learned a lot. I could probably teach a game theory course without ever having read more than a dozen articles, just from having been here.

MD: Let's talk more about the Penn years, which are tied in with overlapping generations models.

Cass: Karl and I got back, if my memory serves, into thinking about the overlapping generations model sometime in the middle to late 1970's. If I had to pinpoint a date I would peg the year about 1977. I'll tell you the genesis, to my recollection. Karl and I were having a discussion because there was a seminar here run by more junior faculty, and we participated, and we would go back and read some classics in macro that people wanted to read. One of the papers people wanted to talk about was Lucas 1972. I don't remember why one of us decided to present

FIGURE 3. Cass singing with Randy Wright's band (The Contractions) at the Penn Economics department's "skit night" at which economics graduate students lampoon the department faculty, and vice-versa, March 3, 1998. Pictured (from left to right) are Randy Wright, Dave Cass, Andrei Shevchenko (a Penn Economics graduate student), Gwen Eudey (Georgetown University, visiting at the Research Department, FRB of Philadelphia), and Boyen Jovanovic (at piano).

it, but it got me to thinking again about this issue I'd raised with Bob about the state space, and Karl and I talked about it. Karl was astute enough to observe that we could formalize the idea of having arbitrary variables in the state space. So Karl constructed the first example of sunspot equilibrium, and I think it is the one that appeared in his so-called Malinvaud lecture. It's a linear OLG model where households' allocations but not their welfare depend on sunspots, and so I objected to the example. I said, "Karl, that's not a convincing example. It doesn't matter from a welfare viewpoint."

We were going to a conference that Karl and I had organized at Squam Lake in New Hampshire on growth theory, and after this discussion, I spent most of the conference closeted in my room trying to construct an example of a sunspot equilibrium in an overlapping generations model where sunspots mattered for allocations. The first example I came up with was with quadratic utility. It was laborious as holy hell! So Karl and I were going to talk about this at this conference, and nobody understood the idea at all. They just didn't understand, until that last day when we actually gave the paper, and Steve Salop was the only person who understood the idea. This was sort of discouraging.

MD: Was this an overlapping generations problem?

Cass: This was an overlapping generations model, but in the overlapping generations model (as Steve Spear will attest, because his thesis is about this), you have to be careful picking your utility function. I do remember that I decided that I couldn't get sunspot equilibria for the standard parametric forms, and I was going to need cross-product terms, so . . . anyway, Karl and I came back and we knew this was a great idea, but somehow the reception that it got was a little discouraging, so we didn't really start working on it until much later. Karl's enthusiasm for the idea was extraordinarily high, and he talked about it a lot. He went to Paris in the late 1970's where he gave his Malinvaud lecture, which he always cites because he wants us to claim priority, correctly.

One of the other people he talked to a lot about it was Costas Azariadis. My view is that Karl explained the idea to Costas a number of times, and Costas finally picked up on it and he wrote a paper about it. He realized, not from a utility approach, but by having a first-order Markov system of probabilities, that one can get sunspot equilibria. Steve's thesis actually develops the general story, and he solved that problem long before, for example, Azariadis and Guesnerie did. But I have to credit Costas with something. When Costas produced a working paper or maybe even before that we realized that if we were going to develop the idea we'd better get to it.

MD: And this led to the *Journal of Political Economy* paper?

Cass: The *JPE* paper constructs a standard simple example that didn't require using the overlapping generations structure, although we built on one of the properties of the overlapping generations model, the friction you get by restricting participation on certain markets. Much later we wrote a paper showing that there's another aspect of the overlapping generations model, that somehow the open-endedness of time also plays a role. We constructed an example where there were complete markets and unrestricted participation—it is something like the following. This is basically an overlapping generations model where the uncertainty is all in the first period, you either get an alpha or a beta, and you can buy insurance against that, but because of the infinite structure of the model, you would still have sunspot equilibria. So there are two causes for sunspot equilibria: One of them has to do with the time structure of overlapping generations; the other has to do with not having enough access to asset markets.

MD: Didn't Jim Peck pick up on the second thing in his thesis?

Cass: Yeah, yeah, that's right. I haven't thought much about sunspots, especially in the overlapping generations model, for quite a while, but he develops a generalization about nonstationary sunspot equilibria in the OLG model. I think sunspots are really interesting, but even when Karl and I wrote that *JPE* paper, my interests had already diverged to thinking the way I did on the general equilibrium problem, in which you can actually do a finite-dimensional model. Of course, we have this simple but important theorem which says that if you have all the hypotheses that are necessary, stated and unstated, to get the First Welfare Theorem, then you can't have sunspots. Then we have what Karl used to call the Philadelphia Pholk theorem, which is that if you violate any of these hypotheses you can get sunspot equilibria. It's not quite true, because all it is saying is that if you have a theorem that says A, B, and C imply D, it's likely to be the case that if you drop one of the assumptions the conclusion is not going to be true. But, of course, it may be that it can still be true. I guess that is where Karl and I diverged on this a little, but he's gotten very interested in the absence of convexity. Now, his examples are perfectly okay, but it is not quite true to say that if you have some nonconvexity then you have sunspots, because—as Heracles Polemarchakis and I pointed out—you can have nonconvexity in production and, since profit maximization is relative to a hyperplane, you can substitute everything under the hyperplane and call that the production set, and you get the first welfare theorem back.

The *JPE* example is a real simple example where there are two states of the world and we interpret it, in the structure of the overlapping generations model, as two classes of households. One class can trade assets against the state of the world, while the other can't because it is born later, so it has to trade just on the spot market. That is one kind of example. But I got interested in constructing other examples of sunspot equilibria. In particular, in the early 1980's, I went to spend a year in Paris, and the first project I wanted to work on was to construct a sunspot example where there was a missing market. Somehow I decided the way to do that was in a model where you had assets, and not enough assets to span the states of the world. That's how I got interested in incomplete markets. That's another paper I like a lot, "The Leading Example" paper. I had real trouble getting it published because I wrote it precisely in the *JPE* style, a kind of a followup to the first paper but, to the Chicago mind, sunspots are irrelevant, just not interesting. Ironic as holy hell.

MD: In the famous Kareken and Wallace volume, one thing Cass and Shell say is that by definition the overlapping generations model is the only dynamic disaggregated model, which one may take to mean it is the only interesting macro model.

Cass: I have to get back now to the train of thought about the overlapping generations model. I got interested in the overlapping generations model because of sunspots. And then Okuno and Zilcha—this may have even been at the same conference at Squam Lake—presented a paper which was an attempt to prove that if you introduced money into the overlapping generations model, then equilibrium where money had a nontrivial price would necessarily be Pareto optimal. There was a flaw in their proof.

MD: Neil Wallace was always inclined to say that in Minnesota.

Cass: Their work was based on trying to verify formally what Neil believed. I saw their proof, read their proof very carefully, and it had an error in it. I decided that it probably wasn't true, depending on some characteristics of the utility functions, and so on, so I decided to work on a counterexample. Basically, I constructed a lot of counterexamples, where you can introduce money and, for one reason or another—heterogeneity, nonstationarity, and so on—you will not get Pareto optimality. I got interested in the overlapping generations model again. Karl and I really did believe in it, and we started working more generally on the overlapping generations model after we'd worked on sunspots. We really did believe at that time that it was the only serious model where money played a role. Of course, subsequently you have some very famous papers which present other basic paradigms in which money plays a basic role.

MD: Although mathematically those structures maybe aren't so different?

Cass: Well, I was going to talk about that. The Kiyotaki-Wright model I like a lot, but as I have pointed out to you, Randy, I think that the ultimate principle in both of those models is that the horizon is indefinite. If you truncated your search model, you wouldn't get a role for money either. So even though we didn't have the imagination to think of another model, and this, for example, would be your model with search, in which there would be an infinite horizon, I think you were right in asserting that the underlying time structure of the overlapping generations model is what provides a reason for having money. I still think that the ultimate thing is that money has value because people believe it is going to have value, and the only way they'll consistently believe it will have value is if they're never forced to put up. And that's common to Kiyotaki-Wright and the overlapping generations model.

MD: Well, it's interesting, because there are some infinite horizon models in which money has no role. So the infinite horizon isn't a sufficient condition.

Cass: Just the infinite horizon does not necessarily give you a role for money. In addition, you have to have some type of imperfection, some violation of the hypotheses of the first welfare theorem, like restricted participation (overlapping generations), or noncompetitive behavior (the search model).

MD: Do you agree that there are still many issues in monetary economics that are yet to be sorted out.

Cass: Oh absolutely. It would be nice but probably impossible to have a consistent model where we could get away from having to have an indefinite future to give value to money, but it is hard to conceive of how you would do that. John Geanakoplos has a model, it's an incomplete markets model with money and cash-in-advance constraints, where he gets value for money because money is issued by a bank and you have to repay the bank. But, ultimately, the bank is just throwing money away at the end of the day, and somehow the model is not really closed. It's a little unsatisfying.

MD: What are the issues with an infinite horizon?

Cass: I changed my mind sometime in the 1980's about the infinite horizon. I suppose ultimately the reason that I object to it relates to rational expectations, although I would define rational expectations in a more general equilibrium than a macro way. I define rational expectations to mean that you have a well-defined state space, and that in those states every individual has common beliefs about the prices that will prevail. For those beliefs about future prices, today's markets will clear, and when tomorrow's state rolls around, given the plans, one equilibrium in the realized spot market will be at the prices that they forecast. Now there's a little problem in that there could be other equilibria. No equilibrium model that

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I'm aware of has a sensible process for actually achieving equilibrium prices, so it's not clear why the particular prices they forecast are going to be the ones that occur. Getting back to the issue, I can kind of understand why I might want to use rational expectations as a benchmark when the predictions that we're making are not too far ahead. But this is generally a question of assuming that you know what the structure of the world is. There's a big difference in my mind between that and assuming implicitly that you know this forever. I have become very uncomfortable with that.

MD: Is your view that for some relevant questions it may be more appropriate to use a short-run model?

Cass: I think you can use a short-run model, but the objection there is exactly the motivation behind the overlapping generations model, that when you reach a certain period, if you reach that period, then it is reasonable for people to expect that there will be a period to follow. It's sort of like an induction argument. You can't cut the world off because, in the last period, people are still going to be looking ahead one period. I mean, I understand that argument, I'm just uncomfortable with the conclusion that the model has to be infinite dimensional. I guess in my experience, except for these paradoxes of infinity, I find that infinite-dimensional and finite-dimensional models are isomorphic. But they aren't isomorphic on this one dimension of providing a role for fiat money and I'm uncomfortable with that. So I'm willing to introduce one of the artifacts I used to scoff at, that people, for example, get utility from holding money, or that they're constrained to hold money, in order to close the model. I am more comfortable with that artifact than with the artifact of introducing the infinite horizon. I have come full circle. I am sure that Karl and I in our defense of the overlapping generations model scoffed a lot at these other artificial ways of closing the model, but I'm more sympathetic to that now.

MD: Continuing on with incomplete markets, one of the things that has been happening in macro is the integration of the finance side.

Cass: Introducing finance into macro more generally, I think, is key, and I also think that macro fundamentally is going to be dealing with missing markets.

MD: Some people find incomplete markets models to be a little ad hoc because some subset of the markets is simply shut down.

Cass: It is very ad hoc, but the first step to understand the problem is to build a model where you assumed it. There is a lot of work now going on in which you try to justify missing markets, for example, along the line of, if you have a complete state space then idiosyncratic variables should appear as part of the definition of the state space. Then you won't have markets for the idiosyncratic risk because of the problem of moral hazard. Small numbers is another possibility. People are now trying to build more formal models that start out with some kind of standard information imperfection that would drive you to have incomplete markets. They want to make the incompleteness endogenous.

Another way of doing this is to maintain the structure of the incomplete markets model, but then to introduce agents who are optimizing the structure of the assets. I

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don't think those models have been very successful, probably because they require, for example, that the agents who are going to create the instruments have to be able to forecast (since it's a Nash equilibrium) what the other agents are introducing and then doing, what the equilibria are. You have to make this very strong informational assumption in order to get a formal model. This is an example of the kinds of problems that occur. You know, people are very aware of that, although I still think that a lot of things that are true in the model where incomplete markets are simply assumed will then be true in models where you explain why you have incompleteness. I have this belief. One of the results in incomplete markets that I like a lot was a result that I worked out in kind of a crude way, and then Yves Balasko and I wrote a paper about, and John Geanakoplos and Andreu Mas-Colell wrote a paper about at the same time, that shows that with incomplete markets you get a huge indeterminacy of equilibria in a real sense. I think that result is going to be robust.

MD: And that actually feeds back into monetary models since it implies non-neutral monetary policies when markets are incomplete.

Cass: Yes, and I am going to go a step further than that. The simplest version of indeterminacy comes about because you can pick different price numeraires, like price numeraires period by period, with incomplete markets. But another cause of indeterminacy, which creates even more indeterminacy, is that you can make the asset structure a parameter of the model.

MD: Haven't you made the point that one of the things about sunspots or dynamics is that market clearing and rational expectations are not enough to pin down very much?

Cass: Right. This is kind of self-destructive in a way.

MD: Some people say similar things because of fundamental belief in Keynesian macro—is that why you do it?

Cass: No, it isn't. I have to admit that this is kind of an anomaly, because what it is ultimately is destructive. I've been using a competitive equilibrium model as a benchmark and it has no predictive power, so in a way it is kind of self-destructive. I'm very interested in that. Intellectually, it interests me to try to figure out what it is that will pin down equilibrium. I am still at the stage where I don't know what the answer is.

MD: It's certainly a clear intellectual challenge for the future.

Cass: Well, it is an intellectual puzzle. And I must admit that in my career in economics I have always been interested in an intellectual puzzle, even though it's not fashionable, it may have no practical relevance—God knows what—you can criticize it on a million grounds. A good example of that is spending a couple of years working on this problem of characterizing Pareto optimality and efficiency in an infinite-dimensional growth model.

MD: What is in the future for micro, macro, general equilibrium, game theory? What lies ahead?

Cass: I have a very short work horizon. I always have. I think ahead to the next problem I am going to work on. I have always been penalized greatly when applying for grants, because I haven't the foggiest idea of what I will be working on in the future!

MD: It goes back to the question you told us Koopmans asked on the job market, doesn't it?

Cass: Maybe that's the whole problem, yeah! We've come full circle. But I actually know that there is a big component of serendipity in research. I mean, if you told me 15 years ago that I would be doing general equilibrium with incomplete markets, I would have said "Are you crazy?" The serendipity there is that I wanted to construct examples of sunspot equilibria with missing markets, and I realized that there were a lot of interesting questions about the model that I wanted to use for that purpose. In particular, the reason that I got into indeterminacy is that, in the sunspot model, if you have a missing financial instrument, then you get a continuum of sunspot equilibria; that turns out to be a general property of incomplete markets. The question I am pursuing now is what will actually cut down the set of equilibria. The best you could hope for is a finite number of equilibria, and I don't think the answer is that you have to introduce money in a way that normalizes prices spot by spot, because there is still something that is given as a primitive in the model that should be endogenous, and that's the asset structure. That needs to be endogenized. Now, the question is, whether when you put things in that framework, you still get indeterminacy. I'm interested in that question.

MD: So you want to endogenize the asset structure.

Cass: Yeah, you endogenize the asset structure. There are examples when you endogenize the asset structure that you do pin down the equilibrium, in a sense, but you really don't. A good example is work by Alberto Bisin, in his thesis, where he introduces basically this game theoretic idea where some households introduce new financial instruments and the way that they do it is in the Nash way. They take as given what all the other households are doing and they look at how the equilibrium is going to vary across their actions and they optimize. Now the problem with that is that we know with Nash equilibrium typically there's a plethora. What this cuts down on is the number of equilibria after the set of financial instruments is determined. Somehow, in his model, there is a section which deals with real indeterminacy which shows that you don't have a lot of equilibria associated with a given asset structure. But you do have a lot of equilibria associated with the Nash equilibrium. You've just moved the indeterminacy back one step.

MD: You were saying something a few minutes ago about the way you do research—about looking at the model as well as the questions that you think the model may help us answer. Can you expand on that?

Cass: Well, what drives me to do research is not what drives an awful lot of people to do research. I mean, I'm never much motivated by what some people call real-world problems. I am much more of a structuralist. I have pursued some questions just because they are interesting puzzles to me, not because of any economic relevance.

MD: One thing interesting about your career is that you may have worked on these things for whatever reason—independent of any interest in, say, real-world policy—and yet the Cass-Koopmans model is the foundation for modern business-cycle theory, your work on overlapping generations models is related to much practical research in monetary economics, and your sunspot stuff also has macro policy relevance.

Cass: That is the beauty of a true intellectual discipline. It has room for people like me.

MD: Somewhere down the food chain?

Cass: Well, no, . . . you just learn something! You should never scoff at an intellectual's looking at a question, because you never know when what they are going to come up with will be actually interesting for other reasons.

MD: It may take 20 or 30 years, too.

Cass: It may take forever. And it may not ever happen.

BIBLIOGRAPHY OF DAVID CASS

BOOKS

1974

Selected Readings in Macroeconomics from Econometrica (with L.W. McKenzie). Cambridge, MA, and London: North-Holland.

1976

The Hamiltonian Approach to Economic Dynamics (with K. Shell). Boston, MA: Academic Press.

ARTICLES

1965

Optimum growth in an aggregative model of capital accumulation. *Review of Economic Studies* 37, 233–240.

1966

A re-examination of the pure consumption loans model (with M.E. Yaari). *Journal of Political Economy* 74, 353–367.

A turnpike theorem. *Econometrica* 34, 838–850.

1967

Individual saving, aggregate capital accumulation and efficient growth (with M.E. Yaari). In K. Shell (ed.), *Essays in the Theory of Optimal Economic Growth.* Cambridge, MA: MIT Press.

1969

The implications of alternative saving and expectations hypotheses for the choice of techniques and patterns of growth (with J.E. Stiglitz). *Journal of Political Economy* 77, 586–627.

1970

The structure of investor preferences and asset returns, and separability in portfolio allocation (with J.E. Stiglitz). *Journal of Economic Theory* 2, 122–160.

1971

Present values playing the role of efficiency prices in the one-good growth model (with M.E. Yaari). *Review of Economic Studies* 38, 331–339.

1972

On capital overaccumulation in the aggregative, neoclassical model of economic growth: A complete characterization. *Journal of Economic Theory* 4, 200–223.

Distinguishing inefficient competitive growth paths: A note on capital overaccumulation and rapidly diminishing future value of consumption in a fairly general model of capitalistic production. *Journal of Economic Theory* 4, 224–240.

Risk aversion and wealth effects on portfolios with many assets (with J.E. Stiglitz). *Review of Economic Studies* 39, 331–354.

1973

On the Wicksellian point-input, point-output model of capital accumulation: A modern view (or neoclassicism slightly vindicated). *Journal of Political Economy* 81, 71–97.

1974

Duality: A symmetric approach from the economist's vantage point. *Journal of Economic Theory* 7, 272–295.

1976

The Hamiltonian representation of static competitive or efficient allocation. In M. Brown, K. Sato, & P. Zaremba (eds.), *Essays in Modern Capital Theory.* New York, Amsterdam, Oxford: North-Holland.

The structure and stability of competitive dynamical systems (with K. Shell). *Journal of Economic Theory* 12, 31–70.

1979

The role of money in supporting the Pareto optimality of competitive equilibrium in consumption-loan type models (with M. Okuno & I. Zilcha). *Journal of Economic Theory* 20, 41–80.

Efficient intertemporal allocation, consumption-value maximization and capital-value transversality: A unified view (with M. Majumdar). In J.R. Green & J.A. Scheinkman (eds.), *General Equilibrium,*

https://doi.org/10.1017/S1365100598009080 Published online by Cambridge University Press

Growth and Trade: Essays in Honor of Lionel McKenzie. New York, San Francisco, CA, London: Academic Press.

1980

Money in consumption-loan type models: An addendum. In J.H. Kareken & N. Wallace (eds.), *Models of Monetary Economies.* Minneapolis: Federal Reserve Bank.

In defense of a basic approach (with K. Shell). In J.H. Kareken & N. Wallace (eds.), *Models of Monetary Economies*. Minneapolis: Federal Reserve Bank.

Existence of competitive equilibrium in a general overlapping-generations model (with Y. Balasko & K. Shell). *Journal of Economic Theory* 23, 307–322.

1983

Do sunspots matter? (with K. Shell). *Journal of Political Economy* 91, 193–227.

1985

Optimality with unbounded numbers of households: I. Overlapping (or overlapping-generations) structure and the first basic theorem of welfare. In G. Ritschard & D. Royer (eds.), *Optimalité et Structures: Mélanges en Hommage à Edouard Roussier.* Paris: Economica.

1986

On the existence of optimal stationary equilibria with a fixed supply of fiat money: I. The case of a single consumer (with L.M. Benveniste). *Journal of Political Economy* 94, 402–417.

1989

Sunspots and incomplete financial markets: The leading example. In G. Feiwel (ed.), *The Economics of Imperfect Competition and Employment: Joan Robinson and Beyond*. London: Macmillan.

The structure of financial equilibrium with exogenous yields: The case of incomplete markets (with Y. Balasko). *Econometrica* 57, 135–162.

Sunspot equilibrium in an overlapping-generations economy with an idealized contingent claims market (with K. Shell). In W.A. Barnett, J. Geweke, & K. Shell (eds.), *Economic Complexity: Chaos, Sunspots, Bubbles and Nonlinearity.* Cambridge, England: Cambridge University Press,

1990

The structure of financial equilibrium with exogenous yields: The case of restricted participation (with Y. Balasko & P. Siconolfi). *Journal of Mathematical Economics* 19, 195–216.

Convexity and sunspots: A remark (with H. Polemarchakis). *Journal of Economic Theory* 52, 433–439.

1991

Perfect equilibrium with incomplete financial markets: An elementary exposition. In L.W. McKenzie & S. Zamagni (eds.), *Value and Capital, Fifty Years Later*. London: Macmillan.

Indefinitely sustained consumption despite exhaustible natural resources (with T. Mitra). *Economic Theory* 1, 119–146.

1992

Regular demand with several, general budget constraints (with Y. Balasko). In M. Majumdar (ed.), *Equilibrium and Dynamics: Essays in Honor of David Gale*. London: Macmillan.

Incomplete financial markets and indeterminacy of competitive equilibrium. In J.-J. Laffont (ed.), *Advances in Economic Theory, VI*. Cambridge, England: Cambridge University Press.

Sunspots and incomplete financial markets: The general case, in the Mini-Symposium on "The Structure of Sunspot Equilibria in the Presence of Incomplete Financial Markets." *Economic Theory* 2, 341–358.

Stationary equilibria with incomplete markets and overlapping generations (with R.C. Green & S.E. Spear). *International Economic Review* 33, 495–512.

1993

Real indeterminacy from imperfect financial markets: Two addenda. In R. Becker, M. Boldrin, R. Jones, & W. Thomson (eds.), *General Equilibrium, Growth and Trade II*. San Diego: Academic Press.

1994

Market participation and sunspot equilibrium (with Y. Balasko & K. Shell). *Review of Economic Studies* 62, 491–512.

Notes on Pareto improvement in incomplete financial markets. *Rivista di Matematica per le Scienze Economiche e Sociale* 18, 3–14.

1996

Individual risk and mutual insurance: a reformulation (with G. Chichilnisky & H.-M. Wu). *Econometrica* 64, 333–341.

1998

Pareto improving financial innovation in incomplete markets (with A. Citanna). *Economic Theory* 11, 467–494.

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