MD INTERVIEW

AN INTERVIEW WITH PETER DIAMOND

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Peter Diamond is one of the major contributors to economics during the last half century. His many contributions include research on growth, Social Security, public finance more generally, the economics of uncertainty, search theory, in particular, and economic dynamics, in general. This work has shaped the way we think about many economic problems, and the way in which we formalize them. Among his long list of honors and awards, he is a fellow of the Econometric Society, a Guggenheim Fellow, a Fellow of the American Academy of Arts and Sciences, a Member of the National Academy of Sciences, and a Nemmers Prize winner. The National Science Foundation has supported his work for the last 40 years. A recent check indicated 9 books and 132 published articles, and there are few signs of any slowdown.

In this interview, which took place in his MIT office one morning during the 2006 NBER Summer Institute, we tried to learn how Peter became an economist, who his influences were, and what he thinks about some of the big issues in economics. We also talked about how he started working on several of his projects, and also how and why he sometimes stopped. Interestingly, as we all know, Peter is not only one of the leading economic theorists of his generation, he also takes policy issues very seriously. A leading example is provided by his interest in Social Security. As he has participated extensively in the discussion of such matters, we also tried to talk a bit about this aspect of his career. The entire process was a fascinating

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FIGURE 1. Photo taken at MIT in 2003. Credit: Donna Coveney/MIT.

and pleasant experience for us, as interviewers, and we hope that readers get as much out of it as we did.

MD: How did you get into economics in the first place, where did you go to school, and what was your earliest development as an economist?

PD: I had to have a planned major when I started college at Yale University. I wrote down electrical engineering, for no good reason; the bad reason was that I was aware that engineers made a decent living, I knew that I was good in math and science and knew that I was incompetent with my hands and I thought in the other parts of engineering that might be a problem. At Yale at the time, all the freshmen were on Old Campus and graduate students were residents in the dorms and advisors of the freshmen. My advisor's roommate was an electrical engineer, who for reasons I never understood decided to convince me that the math was, in his words, "too hairy" and wasn't for me. Those long equations didn't appeal to me; long equations still don't. So I decided I wouldn't major in electrical engineering. Then I thought about what my major should be and it became obvious I should be a math major. And I still remember the look of surprise on his face when I told him.

I took Introductory Economics . . .

MD: Do you remember who was teaching it?

PD: Absolutely, I took it in my residential college as a sophomore seminar with only a dozen students or so, in a lovely room with great big chairs it was easy to fall asleep in. It was taught by Charlie Berry. We got friendly outside of class. I enjoyed economics, I liked him. I even had my mother knit me a sweater that was a copy of a sweater of his that I admired. The following year, which turned out to be my last at Yale, I had a totally free elective, so I made a short list of famous Yale classes. Shakespeare was on it, some particular history class was on it, and it turned out, and this is the only piece that was real serendipity, that for the classes I wanted, the famous teachers were on leave.

So I went to Charlie Berry and said this is what happened to my plans. I've got to pick something, so tell me what would be a nice class to take and he said right away: Honors Intermediate Micro and Macro, which was taught by Ed Budd. I said OK! Ed Budd was a terrific teacher. I learned a lot of economics and I liked it enough that in the Spring I dropped French and instead took a graduate mathematical economics class based on "Theory of Value," which had just been published. At the time there were a bunch of undergraduate math majors who were exploring different fields. Dick Beals was one of my class mates. He started economics at Harvard and then went back to math. I do not know what would have happened if he had tried that at MIT. Dave Krantz started the process of switching from math to psychology. So there were a bunch of us who were in Gerard Debreu's class, it was fabulous teaching. The clarity of "Theory of Value" is a wonderful way of thinking about a lot of things in economics.

The summer after my senior year, I became a Research Assistant for Tjalling Koopmans, who was looking primarily for math—he wasn't looking for me to do anything in economics. I shared an office with T. N. Srinivasan, who was writing his thesis under Tjalling's supervision. Tjalling was a fabulous person to work for.

He thought my contribution was large enough that I became a coauthor, but even apart from that, just as someone to deal with, he was a great pleasure.

MD: Was that your first publication?

PD: I guess it must have been, yes. Then the question was graduate school. Going to graduate school was a no-brainer because the alternative was the draft, even though it was not yet the Vietnam War. The draft did not seem like a good idea even in peace time. So the question was, what subject to go to graduate school in, and I applied to both math and economics departments.

MD: Where did you apply?

PD: Yale economics, Harvard, and MIT math. I started at MIT in the math department. I had already completed half the first year curriculum in math, so my plan was to take the other half, take graduate Micro & Macro, and at the end of the year decide which subject to go on in. It turned out that Complex Variables conflicted with both Micro & Macro, so my fall back plan was to do Real Variables, Micro, Macro and another economics class and if I decided to do math I'd do Complex Variables over the summer. I found Real Variables to be a total bore. From my perspective—which is very much an application perspective—you just keep proving the same theorem over and over again. I didn't see the point in doing that.

The other thing that happened was George Thomas (author of the Calculus text book) was the graduate student advisor in the math department. When I explained my plan and what the dilemma was he said under the circumstances I should continue in the economics department. Bureaucracy at MIT was so trivial, it was just a matter of his making a phone call to Bob Solow, who was the graduate student advisor, and my file was transferred. So in my first year as an economics graduate student I was financed by the math department. Whether I would have found my way to economics without that serendipity, I have no idea.

MD: Who did you work with in Graduate School?

PD: I will give you a two-part answer. As a second year graduate student in the spring, Steve Goldfeld, who was a classmate of mine, and I thought it would be a good idea to start thinking about thesis topics. For that purpose we went to Frank Fisher and asked for a reading course of interesting papers in theory for us to read and expand beyond what was offered in the advanced theory courses. In that sense, I started with Frank. When I actually went to work on my thesis Frank was on leave for the year. I don't know what role he would have played if he had been there, some role I'm sure, so it was Bob Solow that I worked with.

There is another Frank Fisher story I enjoy telling—particularly to students who see things disappear in their theses. I was browsing in mathematics and turned up a theorem on matrices that I saw instantly was usable for interesting results on convergence of general equilibrium. So I went to Frank, and showed him, and described how I would use it. He got very excited and took me to Paul Samuelson's office. Paul walked over to a shelf, pulled out a conference volume, opened it, and showed me a paper by Lionel McKenzie that used the theorem exactly the way I meant to. So my first piece of independent work vanished. On the other hand I felt that I was in very good company.

MD: It sounds like you were doing theory and math econ in those days?

PD: That's right. The generals at the time had written exams in three fields, oral exams in two. My oral exams were in Advanced Theory and Public Finance, my third written exam was in Money. My thesis was totally Theory.

MD: What was your thesis about?

PD: They were three essays on economic growth. I spent the summer between second and third year at Rand. Some of the time I spent playing war game and having a good time, and some of the time I was free to work on anything I wanted. So I thought about the infinite horizon utility problem that I had worked on with Koopmans and thought of another way of approaching the problem in getting properties of that utility function out of topological considerations and some assumptions. That was my first essay, which was essentially finished in the first or second week of the semester, as I had started it during the summer, and that became my job market paper.

Then, T. N. Srinivasan, with whom I had become very friendly after sharing an office (we drove cross-country together), sent me what was part of his thesis. He hadn't fully solved the problem he wanted to solve, a problem in optimal growth theory. I saw a trick that would permit me to solve the problem that he had first set up, so that was essay number 2. For essay number 3, Bob Solow gave me a book by Salter[1] on how technical progress reduces costs for a firm. It was a way of modeling the impact of technical progress on the cost function of the firm, and I saw that I could turn it around, and it became a growth model. So my third essay was on technical progress and economic growth.

MD: From your description, there appears to have been no sharp distinction between Micro & Macro. The Intermediate Micro & Macro courses at Yale were taught by the same instructor. You worked as a theorist with Koopmans, Solow, Samuelson and Fisher. Some of these names are traditionally associated to macroeconomics.

PD: Growth Theory was part of Advanced Theory. It was both Micro & Macro, as it is today.

MD: Did you interact at all with Jim Tobin?

PD: During the summer I spent at Cowles working for Koopmans I would go for coffee. Tobin was there, Okun was there, Brainard was there. So, yes, over coffee I was certainly picking up the Yale macro vibe.

MD: Tell us about your first job.

PD: First of all the person in charge of recruiting at Berkeley was Andy Papandreou, before he left and went back to Greece. He had hired Sid Winter starting in the spring semester before I started. And simultaneously hired along with me were Ollie Williamson, Dan McFadden, and David Laidler. The assistant profs already there were Al Fishlow and Bernie Saffran. Berkeley was a very exciting place to be an economist, particularly a theorist. Debreu and Radner were there. My first year I taught the Graduate Micro/Macro Sequence, together with Scitovsky, and I taught an undergraduate Public Finance class for majors, that was a year-long class.

MD: So this was your first venture in Public Finance.

FIGURE 2. Diamond in the Sinai desert during his visiting appointment at Hebrew University, Jerusalem, 1969.

PD: It was my second graduate field, but I hadn't done any research. I did spend the summer of 1963 as an intern at the Council of Economic Advisors, working primarily with George Perry. Walter Heller was the Chairman. Again I had some exposure to public policy issues. That is actually the only time I've ever been on salary for the federal government—a question that comes up from time to time when doing presentations on a panel—people identify their professional credentials with the government, those are my professional credentials.

MD: How long were you at Berkeley?

PD: I started in 1963, I taught two years full time, and then I took leave in my third year. That leave was at Churchill College in Cambridge, and then I returned to MIT, but I spent part of that year back at Berkeley. The first year I was there was just an unbelievably exciting time to do research on campus.[2] The department was an exciting and an up-and-coming place. The interaction was with young people who have remained close friends ever since, the kind of bond you form as Assistant Professors. My wife-to-be Kate also got friendly with the wives of my young colleagues. It was teaching that Public Finance class that got me very interested in the field. My paper on the national debt started as a handout to the undergraduates when I came to the public debt part of this year-long course, because I didn't like the rest of the literature. I thought Modigliani and Buchanan said relevant things, but I didn't like the models, so I sat down to build a different model to explain the topic to the undergraduates. I taught the course both years.

MD: There weren't many overlapping generations models in the literature back then. What led you to work on that environment? You knew the Samuelson's piece?

PD: I was not aware of anything else other than Samuelson's article. Buchanan, who had a book on the public debt, and Modigliani's piece on the public debt made it clear that the intergenerational dimension of the public debt was absolutely essential for analyzing it. So it was one of these things that happens when you're young, happens less when you get older—when it's just obvious what you ought to be doing. So I just set up the model and it was straightforward and quick to do the analysis.

MD: You were on leave in Cambridge for a year. Did you meet Jim Mirrlees then and there?

PD: I met him for the first time in 1964 at the Econometric Society meetings in Zurich. And then, when I showed up in 1965, we became very friendly. We didn't actually start collaborating until the summer of 1967. We have been collaborating continuously since then.

MD: What were your main intellectual influences in your college, graduate school, and junior faculty years?

PD: I definitely work in a MIT style and I don't know that I have to pick and choose among the MIT faculty of the time. I was certainly strongly influenced by Debreu to be more rigorous, more mathematical than what at least appears to me to be the MIT style of the time. Then my colleagues at Berkeley were very much a reinforcement of doing MIT-style things with the kind of rigor that Debreu stood for. And McFadden—I wrote a joint paper with McFadden while I was there—and Saffran's comments on everything I worked on were influential. There were some passes at starting things with others that did not work out. But I would certainly put the two and a half years at Berkeley high up. And obviously the Yale background, both with Ed Budd as a teacher and Koopmans for whom I was a research assistant.

MD: Let's talk about Diamond-Mirrlees.

PD: To go back a step, when I came back to MIT I started teaching graduate Public Finance, sharing the sequence with Cary Brown. At Berkeley I only taught undergraduates—the graduate courses were taught by Rolph and Break. Important in the profession generally, and particularly at Berkeley at the time, was the use of duality. The McFadden/Winter development of teaching micro out of duality, which influenced how Varian wrote his textbook, was something I was immersed in at the time. So when I started teaching Harberger's deadweight burden in the public finance, I naturally thought of doing it differently. The paper I have with McFadden on the use of the expenditure function in public finance was really stapling together different things that he and I had worked up that used expenditure functions, that is, the duality tools that I picked up at Berkeley. So, I'm teaching how to measure dead weight burden using the expenditure function in the public finance class at MIT and I'm literally at the blackboard thinking: I could minimize this! So I finished class, went back to my office and reinvented Ramsey, because with a dual approach it is a piece of cake.

MD: Were you aware of Ramsey at the time?

PD: No, nor of Samuelson's exposition of Ramsey, which has since appeared in the *Journal of Public Economics*. Beyond that I also obtained, in that one-consumer context, the aggregate production efficiency result, which I don't think is in Ramsey. At some point, I became familiar with Boiteux's work from Dreze's survey in the AER where he talked about it from the French perspective on pricing with a budget constraint. He had many consumers, but in the background there was lump sum redistribution across consumers which couldn't be used for the resource constraint for the planner, which of course makes it equivalent to a one-consumer economy. So there was no equity efficiency trade-off.

In the summer of 1967 I went back to Cambridge with my wife Kate—we had recently been married—and I gave a seminar based on the Ramsey optimal tax, done dually, and the efficiency result. At the end of the seminar, Jim came up to me and said: Since you set it up with prices you can straightforwardly do many consumers. We started working on that together and by that winter the heart of the paper was done. In fact, we each presented it at our respective Econometric Society meetings in 1967. The long lag from then to publication has multiple causes. Lack of e-mail was certainly one of them, particularly while I was eight months in Nairobi.

MD: What were you doing in Nairobi?

PD: Teaching economics at the university and consulting for the Ministry of Planning under a Rockefeller program. It was fascinating. I went from there to Jerusalem, which was a bit closer to England, but still not easy to get things done, and I then went back there. That summer, I spent a month in Oxford and one in Scotland working with Jim, with our families along. We started putting the pieces together again. There were two long referee lags in the process. So that came out much after it began circulating.

MD: Shall we fast forward to "New Public Finance"?

PD: I guess if I'm going to find a precursor of "New Public Finance," it's my 1978 paper with Mirrlees and the still forthcoming paper, which was actually circulating as a working paper in 1982, at the same time, I think, when it got rejected by *Econometrica* after, if memory serves me four years of refereeing. I agreed to be on a panel consulting the Congress about the U.S. Social Security system, back when it was overindexed and there was an obvious need to change it. Congress was looking for some outside advice on, first of all, the description of whether this is a huge problem, and how to correct it. I served on two different panels headed by Bill Hsiao, who is now at the Harvard School of Public Health, and got very interested in issues of Social Security.

As I worked on policy analysis, it happened, as it always happens, you discover there are interesting policy issues on which there is no literature whatsoever. And one of those questions was how to adjust the level of benefits for the age at which they start. So, if somebody stops work but doesn't want to start benefits and wants to start them later, you have what is referred as an actuarial adjustment, but it may not be actuarially fair. At the time what they did did not make sense as an adjustment to inflation because it ignored the fact that inflation changed nominal

interest rates. I had some input into that. I realized this is an interesting question, which is: What is optimal way to adjust benefits for the age at which they start? So as Jim and I went on working together, I said "what do you think about this as a problem to work on?" He liked it and the two of us decided to work on it. We now have four different papers that are done or almost done and one more set of notes in the drawer, I don't know whether it will ever come out or not.

In the process of working on that we stumbled on a result that is of much interest in New Public Finance, which is the advantage of taxing savings as part of the design of the optimal labor market incentives. So the 1978 paper, like the current literature, assumes that the government controls everything. Obviously, we did not use a mechanism design vocabulary, but we had a model with the Social Security system and nothing else, and people did no savings, which you can think of as the government controlled their savings, so they consumed their after-tax wage when working, and they consumed their benefit after retiring. And the government by controlling those controlled everything, so it was very much equivalent to mechanism design, but we got at it by starting with a concrete problem in a simplified setting, as if there's nothing else going on in the economy. The companion paper was: What if savings are unobservable. So we then solved that mathematically much more difficult problem. It's forthcoming in the *Journal of Public Economics*, as it has been for about a decade now. That was done again in the late 1970s.

MD: So it occurred to you right then that hidden savings was a natural extension?

PD: Yes, it was the natural companion piece to the one we did with no savings. It also fit with my sense that we have a heterogeneous population and from the point of view of Social Security there are people who don't save and there are people who do save and we need a system that makes sense for both these groups. For other questions, not Social Security, there is a third group who save huge amounts, the Bill Gates of the world. They are not of any relevance for the design for Social Security, but of great relevance for the design of issues such as taxing capital and bequests. So, yes, the two of them were companion pieces that were on the agenda right from the start, but it's a much more difficult mathematical problem. It took a while to prove, it took a while to get it readable enough for the referees.

And then Jim did some simulations comparing two different optima to ask the question of what difference it makes, in the particular set up we were using, whether you controlled savings. Surprisingly, it's not terribly important. Then along the way we would solve two period models to illustrate things. Then we solved the continuous time model, and obviously the hard math was in the latter. We also had just a linear tax on savings, rather than the whole generality of the mechanism design framework, but we didn't push that. We wanted to show the flip of the fact that we would have, as the vocabulary is now, "wedges," if you had the full mechanism design, and you would like it if people saved less in the case where this is unobservable.

MD: Did you find a savings-dependent tax on labor income?

FIGURE 3. Four former Berkeley assistant professors and their families, New Haven, 1977: 1st row: Jenny Saffran, Matt Diamond; 2nd row: Tito Simboli, Kit Winter, Linnea Saffran, Ray McFadden, Rob McFadden; 3rd row: Peter Diamond, Ellie Saffran, Kate Diamond, Georgie Winter; 4th row: Jeff Winter, Dan McFadden, Bernie Saffran, Sid Winter. (Photo by Tito Simboli.)

PD: This was a Social Security analysis, so the question was, when your savings are not on the plate, then what do you do? No, it did not cross our mind to consider more complicated interactions, just as we did not think of the model where there were no savings as mechanism design. We said: How do you set it up if empirically people just don't save? That was the mind set in which we approached it.

MD: Let us move to search theory. How did you get started on it?

PD: There are actually two different starting places. One for my JET paper on the paradox of monopoly pricing, and a whole different start a decade later on the law and economics and labor market papers.

MD: Let's talk about the Diamond Paradox.

PD: I had been interested in the issues of convergence to competitive equilibrium, something Frank Fisher went on working with, and I was staying abreast with the literature. What struck me was the question being asked: let's explore mechanisms that have some plausibility and see whether they converge to competitive equilibrium. And it crossed my mind that that was the wrong question. The right question was: Let's set up a credible mechanism and see where it goes.

MD: So you took it from the other side.

PD: That's right. So I started fiddling around—if I remember correctly this was 1969—with models that I could solve that would have the property that there was some plausibility in the mechanism by which the process went on until you got to an equilibrium. And how I settled on this particular search model, I don't have any memory of any more. But, the paper was essentially done in the year while I was on leave, and then I came back, presented it at seminars and got it finished.

MD: There was no connection to the Phelps volume, which came out roughly at the same time?

PD: No, I was abroad, completely out of circulation, for the usual 15 months academic routine, eight months in Nairobi, five months in Jerusalem, then on to Oxford and Scotland.

MD: That paper stimulated a lot of people to develop some interesting papers and models, like Burdett-Judd, Butters, and a variety of other ways around the Paradox.

PD: There is no question that I didn't think of the result as describing how things happen. What I thought was robust is that market power wouldn't go away, but as you started complicating models the full monopoly result would go away.

MD: So, you didn't continue on that because you thought the result is so robust in this particular structure?

PD: No, it was not that kind of explicit thinking. It was just that I wasn't interested in following up. It was out there and had a life of its own and I was interested in other things. At the time I was also pursuing law and economics. I took a bunch of classes at Harvard Law School to get the law stuff down, and the reason I was doing that was that I was very unhappy with, to put it in the starkest terms, the way the Arrow-Debreu model collapses the whole future into the present, and obviously search was one way of expressing my unhappiness with that. I had the sense that it was important for economics to have the future be different from the present. And the question was, how could I find a question in an economic environment in which I would find practical, interesting, things to say. And this was a problem I kept going back to repeatedly, and it crossed my mind there's a whole bunch of legal problems that very much have an intertemporal structure where time matters, and if I took classes at the Law School and started working with law and economics I would stumble over something, which would then lead me into an analysis that might prove to be more useful.

MD: Did this lead to the work on breach of contract?

PD: No, at the time I did some fairly straightforward work on torts, some alone, some again with Mirrlees, but it never connected to the work on time. So I went

back to thinking about time, and the problem I had was that I was so deeply immersed in Arrow-Debreu style thinking, that when I started thinking about a problem I would slide into Arrow-Debreu and lose the ability to do what it was I was trying to do. I spent a summer trying to get my mind functioning differently. It was not looking so much for output as for mental process change. I have a paper in the *Bell Journal*, which is results on things involving some intertemporal and uncertainty elements. None of the results were particularly interesting, but I was forcing myself to think through those issues; that led to the change in the way my subconscious worked.

MD: You didn't do it by reading other people's work, then, to try to remove yourself from the Arrow-Debreu tradition?

PD: Actually it was reading that was the turning point. It's when I saw Dale Mortensen's piece (on the optimal labor contract with a Poisson process of changes in the environment) that I realized that that was the tool I had been waiting for. My accumulated thoughts would now be modelable using these Poisson processes. That's how I got launched. I started with the law and economics contract kind of questions, and that is my work with Eric Maskin, and moved from that into more labor market focus. It was the coming together of a decade of dissatisfaction with a treatment of time in economics and the realization of the power of Poisson processes which Dale's paper introduced me to.

MD: Why did you take that direction—as opposed to, say, Lucas and Prescott's island model?

PD: The island model, I believe, as Lucas and Prescott set it up, fits the Welfare Theorem of Arrow-Debreu. They've got efficiency properties, and I believe the route into seeing that would be to think about it from an Arrow-Debreu perspective where the role of the island is a constraint on your consumption possibility set. There you have the property that I was trying to get away from—there is some central mechanism, something similar to the Walrasian auctioneer, which is controlling the flows between islands in a way that is a central mechanism. On the time thing, I'm still very unhappy with where we are. I wrote a book called *On Time,* but that was again a device, think of it as a commitment strategy, to try to wrestle with these classic problems. Some of the book I was pleased with, and some of the book I was not pleased with, particularly the macro side; but that was as far as I could get at the time.

MD: There were several ingredients to your search model. One, compared to, say, McCall or Mortensen, is the general equilibrium approach. Another main ingredient is the matching technology. How did you come up with that idea? Is it something that you had seen before?

PD: I do not know. Again, it is one of those things which I suspect was just obvious. You've got to get the right questions. There's a lot of questions in economics, and when you get at the right questions and the right kind of models, then there are a whole bunch of assumptions, and more or less anyone that is at that place will be making the same set of assumptions. I suspect the matching function is one of them.

MD: The beauty is that you don't have to worry about all the details of the strategic interaction, you just have these input and you get this output.

PD: In the work with Eric we had linear and quadratic [matching functions], and we were exploring the differences it made. And then I think it was just very simple to go from linear and quadratic to general functions. I never thought of that as something I struggled with. There were other things I struggled with; that wasn't one of them.

MD: It's certainly been an important ingredient in lots of modern macro and labor.

PD: I don't know from what it stems, if you go back to the Phelps volume, is the same basic idea there or not? I certainly knew the Phelps volume, if it's in there. Then, of course, the work Michael Rothschild did. Mike and I talked about search a lot, in general, and in the course of putting together a book of readings, because I was teaching an uncertainty course as an Advanced Theory course, and as far as I know there were no other similar courses or books of readings. I always thought that the kinds of problems I enjoyed working on were equilibrium problems, not analyses of properties of individual behavior, although obviously the profession needs both. And then the Yale and Debreu background had me thinking in general equilibrium, and the only kind of partial equilibrium that is legitimate is something that slides into a general equilibrium model. That's just the way I naturally think about all sorts of questions.

MD: It permeates your way of thinking, but you wanted to move away from it.

PD: I wanted to move away from one assumption.

MD: Do you think that part of Arrow-Debreu was in part a step back, that other people before them had thought about time in a richer way?

PD: No, I don't think it was a step back. There are properties in that model that can carry over to other models, and it is the experience of understanding the model that influences how you do other work. The earlier approach wasn't setting off a sequence of research advances. So, no, I don't think it was a step back.

MD: The "coconut model" then evolved not from a Keynesian macroeconomic tradition, from thinking about a role for macroeconomic policy, as it has been widely interpreted, but rather from this process of going beyond Arrow-Debreu.

PD: Part of my aspiration about getting time right was the belief that it matters a whole lot for policies in a number of dimensions. It mattered more in macro, it matters in the labor market. So it's not that I was ignoring that it would lead to a Keynesian type of result. It was the model I was starting with from the search and time and uncertainty perspective, and then exploring different ways of setting it up with different properties and looking for the properties that would be interesting, and interesting means they have robustness and some relevance.

MD: Then, in your *Econometrica* paper, you put a cash-in-advance constraint in the coconut model.

PD: The thing that was obviously missing in the coconut model was the nominal-real link, which is something that is obviously true—there is a nominal-real link. And the question is, how to think about it. I put cash-in-advance in there not with

FIGURE 4. Photo taken before a presentation to the Chinese government of a plan for pension reform, 2004. Nick Barr, Edwin Lim, Diamond, Caio Koch-Weser, Stan Fischer, Premier Wen Jibao, Jim Mirrlees, Teh Kok-Peng, and Xiang Huaicheng, Chairman, National Council for Social Security Fund.

an eye on monetary theory, as you [Randy] have done a great deal on, but, rather with the hope that it would be a route into something Keynesian, which it turned out not to be.

MD: One view is that in the coconut paper you wanted to talk about policy, and you could talk about fiscal policy, but half of macro is monetary policy. Did you want a monetary version so there could be central bank policy in addition to the fiscal policy?

PD: Yes.

MD: You did other work on search and money with Yellin. How did you meet him and how did that work out?

PD: At the time, Joel was at MIT and he started as a physicist, so basically he is widely read, but his real expertise is solving differential equations. I don't remember when I first met him, but he has done joint work with Paul Samuelson, he has done joint work with Paul Joskow, and with me. So he had a connection to the Economics department. And as I started thinking about trying to do better on the role of money in these kinds of models, I realized that solving differential equations was the way to go. In fact he published on his own in a math journal the solution to the mixed difference-differential equation we used. He figured out how to solve it. He also brought to the collaboration a wonderfully different perspective. Not trained as an economist, he pushed me to translate things into the kind of vocabulary he liked and then understood that to circulate this paper among economists it had to be published in an economist vocabulary. So he just wanted

it in his vocabulary as a way to advance the research process. He has a different mind, he has a powerful mind, and interacting with him was a great pleasure.

MD: Your continued working on search for quite a while, with Olivier Blanchard, for example, and that work influenced a cohort of students at MIT in the 1990s. But then at that point you stopped working on search. You moved on exactly when this was blossoming. Did you think that this was all done, and hence we're wasting our time? You have a record of choosing different topics and moving on to the next one.

PD: I do enjoy the "how do you set up a model to address a question or problem." To me, asking, "I got this model and now I have to work hard to make it more realistic and put in more features," I do not enjoy it as much as the thinking hard about how to get started. Some of that has to do with the laziness of doing hard math, that many people go through as they age. And of course you have to keep in mind that Social Security became a very hot topic.

MD: Speaking of Social Security, let's talk about your experience both in policy and research.

PD: The starting place as I indicated was back in the 1970s, which included a policy proposal for the needed reform, but which did not succeed. The wage-indexed system was adopted in the 1977 legislation, while the panel I was on recommended a price-indexed system. This is different from what the commission appointed by President Bush has called a price-indexed system, because theirs is a mixed wage-price index system. If you set up the system completely real and if you have economic growth and if you have progressivity then you will generate some surpluses. That factor alone tends to generate some surpluses, which can be used to offset demographic trends, or in a steady state could be used—I anticipated at the time would be used—by the political process to make the system more progressive. Social Security has a history of identifying problems and then putting in some wrinkle in the system to provide benefits relative to the problem.

So there are benefits for divorced spouses or divorcees whose ex-spouse is dead. If you looked at the condition of the elderly women who got divorced at a late stage, they were worse off than widows, so the system introduced a benefit for them as a way of addressing the social problem. The commission appointed by President Bush, in addition to their individual account part, and in addition to how to cut benefits to get the system toward balance without transfers, identifies vulnerable groups for whom benefits should be increased because there were social problems. So, I thought having a system that generated fiscal surpluses, the political process was such that, that they would be well used, so I liked that. I was involved some in the early 1980s, Jerry Hausman and I have some empirical work about retirement. I had a consulting contract for one of the government appointed commissions whose report never had much influence, and I stayed in touch with civil servants involved with Social Security. I did some technical panel work in the 1980s in the time of the first President Bush, I did two panels at that time. I stayed in touch with the policy issues.

And then as communism was ending and as macroeconomists were rushing to Eastern Europe to advise everybody, like many people outside the system, I was envious. It just looked like so much fun. I also had a great deal of skepticism of whether the macroeconomists would get the details right. So I went to talk to Jeff Sachs and said I would like to play, and he said, well, all of these countries have Social Security problems, and are you interested in going to Poland and writing a chapter in a conference volume about Social Security problems in Poland. The major problem was not what an outside fiscal economist had a whole lot to say, which is that they had poverty and deficits and political problems on how to deal with that combination.

MD: But they did have a Social Security system in place.

PD: Yes, all communist countries had a system in place. This was more a political problem, not an economic problem. How much can we cut benefits? What will it do to poverty? How much can we generate money to cover the current deficits? But in the reform debate in Poland, there was somebody who was urging that Poland imitate Chile. So in the chapter I wrote for the conference, in addition to describing the circumstances in Poland, I discussed imitating Chile as an option, and that got me interested in Chile. Then there was a Brookings conference of various aspect of the Chilean experience. Rudi Dornbusch asked me to write an essay about Chilean Social Security. Salvador Valdés-Prieto, an MIT PhD and Chilean Social Security expert, agreed to be my coauthor. It helps to have somebody on the ground who gets it right. So we wrote a major essay on the whole history of Chilean reform. A lot of people got interested in Chile, it was one of those lucky things being in the right place at the right time, and that got me involved in exploring systems in many countries, most recently China.

That's been the policy side. At this point, though, I ran out of things that struck me as really interesting to work on in basic Social Security research. Of course, in the U.S. right now, this is not a hot topic. I am actually in the process of shifting gears to taxation issues. So when I was invited to coauthor a chapter on the roles of capital and labor income taxation for the Mirrlees Report, I was delighted to getting rolling on what I decided was going to be my next direction.

MD: Policy should use what we are really sure about, while basic research must push the envelope, and explore what we are not sure about. Do you share this view?

PD: I think what you want to bring to the policy debate is what you think is the best relevant analysis. Sometimes it's things you are sure of and sometimes it's things you are not so sure about, but, you think even with the uncertainty is worth having as an input into your thinking. Part of that relates to how I think about the connection between policy and research. Some people think what you really want to do is think about the model you believe in the most and take it literally. I think that's not a good way to do policy. I think what you want to do is to understand at an intuitive level the range of different models that address different parts of the question you're interested in. It is too complicated to address all of them in a single model. And then put together a policy discussion that also incorporates a

FIGURE 5. Diamond and son Andy on Green Monster at Fenway Park, 2006.

political-economy constraint and implications for the future, but also puts together intuitively what you've learned from the different models. So if you have a model and you're not quite sure how important it is, it's not going to be very important in your thinking.

There's a quote from Marshall that I like so much I used it repeatedly, including in the volume *On Time,* about using multiple models that are refined to get you to understand something small, but then you have to put the pieces back together. So I think anything you learn from there is potentially useful when thinking about a policy problem. I think it is a natural tendency to overweight what you just learned and you have to work at avoiding that.

MD: Do you find it easy to communicate this stance with nonspecialists, for example, when you worked on panels?

PD: I never find it easy to talk to non economists, at least until I get my patter down. I did a lot of public talks about the debate on the Bush Social Security proposal. Once my PowerPoint evolved, I found that I was quite capable of making the key points to those audiences. But part of the point is you don't try to convey the things that you cannot convey without the economics background and you don't try to convey the understanding of it. You can convey the implication. I try to convey the understanding of some of the critical dimensions of what's going on.

MD: Is the deficit the reason why you are now turning to work on taxation?

PD: I'm drawn into taxation more from the structure of taxation issues rather than "is the deficit too big" and "should we do something about it from the tax side?" I'm drawn into it by saying whatever you're going to do from the tax side, how should you do it and how are you going to design your taxes.

MD: For young economists, that seems like a promising area at the moment.

PD: I've learned over the years to feel strongly that someone of my age should not be giving advice to young economists on what's going to be interesting.

MD: Since we are interviewing for *Macroeconomic Dynamics*, maybe the readers would like to hear your comments on the state of macroeconomics, past, present, and future.

PD: I've already indicated that I think nominal stuff really matters. My other thoughts, reactive thoughts, are things that are widely held. Heterogeneity matters, uncertainty about the future matters. I am a card-carrying behavioral economist and I think that matters in both micro and macro. I became convinced that there is a lot of truth in Kahneman and Tversky's work a long time ago. I first got to know Tversky in 1969 when I was teaching in Jerusalem. I didn't meet Kahneman until somewhat later. I found that approach fascinating, plausible, and from time to time I would sit around to think about how to use it in economics, and not coming up with any way of doing it, because it is hard, and I admire the people who have made the breakthroughs and have turned it into a research community, as opposed to an anomaly that you identify and you know is something that ought to be addressed and, if you could, it would make economics better. So I think it matters in macro as well.

We also know that the competitive model doesn't describe either labor markets or product markets. We know that it's a very handy tool to use because we have a great deal of understanding of it and it contains considerable empirical relevance. It also has a lot of nice properties for helping you isolate how something you're exploring in a model impacts equilibrium, because you know what equilibrium is like without that. So, I think it's very handy. The question for any applied research with an eye on some policy issues is how important are the deviations and to what extent can you build them in. Well, tax policy is a place where they matter and we don't have a good handle on how much they matter. Macro is another place where they really matter. Before I was describing how I think about policy. I think there are things you can learn from macro models that have the underlying market-clearing, competitive, structure, but you shouldn't weight them too heavily when thinking about policy because they are missing some key ingredients. I don't know if I have anything more to say, because it's not the case that I stay abreast with macro developments.

MD: Regarding behavioral economics, what persuaded you that this is important? Is it the evidence from psychological experiments?

PD: There are two separate pieces to this. One piece is, do you think the psychological evidence is credible as a basis for thinking that people behave in ways that are missing in a standard model. The second question you ask is what makes you think that it's important to economics. Some of these you don't even need the experience to say: yes this is okay. Thaler's piece on the Price of a Bottle of Beer is a classic. Two guys are on the beach, one guy says to the other, I'm going to get myself a beer. Do you want me to bring one for you? Second guy says, Yes please. First guys asks, What is the maximum you are willing to pay for the bottle of beer? If it's higher than that I won't bring it. There are two scenarios—scenario 1 is the

place to go for the bottle of beer is a bar in the hotel that's near the beach. Scenario 2 is the grocery store that has some beer in it. When you ask people how much they're willing to pay, people expect to pay more and they give answers that reflect that, so you get very different answers in these two scenarios and it can't be that they're both actually measuring what beer is really worth to you on the beach that day.

To turn it around, it's sellers who are setting prices who are very interested in these psychological phenomena, and it's not a game against nature, it's a game against people who are trying to behave on their side of the market in awareness of psychological aspects of the people on the other side of the market. So I think it matters for all sort of things. It's that view I have, that it ripples through all of economics, that had me agree to organize a conference for the Yrjö Jahnsson Foundation. The commissioned essays were thinking about behavioral issues in the context of different applied fields to stimulate applied work, not behavioral finance that doesn't need stimulus, but rather public finance, development, law and economics, health, organization theory, and some macro. We focused on how the applied fields are going to change when the behavioral ideas get adequately modeled and can influence more the way we think about policy.

We have lots of laws in this country that are built around understanding the psychology of behavior. The obvious one is the three-day cooling-off period for door-to-door sales. It's not a binding contract, you can get right out of it. You don't have a law like that if you have the rational meeting of the minds image of a contract. And, of course, in Social Security, key are the ideas that left to their own devices, people don't save enough and that people do not understand the value of real annuities enough. So Social Security has been a behavioral topic for a long time, but having formal models will refine how those insights feed into both positive and normal economics.

MD: Isn't the challenge precisely to model things properly? The skeptics describe behavioral economics as pigeon holing facts in ad hoc preferences.

PD: In behavioral economics as in every other branch of economics there is good work and bad work.

MD: But are you bothered by this aspect of the literature?

PD: First of all psychology doesn't have a unified model. Secondly, one of the main messages that comes out of this is context dependence. In fact, one of the things I don't like in some behavioral analysis is you do an experiment in one context and you act as if you can take the parameters from that and use it in different contexts. I don't think that is the case. So, the progress on getting usable implications can be very slow because one of the fundamental premises is you don't have the generalization that we are used to. This model sheds light on a certain intertemporal problem and it can carry over to a lot of other examples of problems. The heart of this is that—and this goes back to one of your earlier questions—you have to think, "What did I actually learn from this piece of analysis?" One of the things that I tell students is that it's the wrong question to ask if something is a good model or a bad model; something could be a good model for one question and a bad model for another question simultaneously.

MD: Final question: Before and during your tenure here, MIT became and stayed one of the very best PhD programs. What does it take to make a great economics graduate program?

PD: There are two parts. Obviously it's the faculty, you have to have faculty with the appropriate abilities. Secondly, it's the concept of the kind of program you are trying to have, because you don't have a good program unless you have good students. You get good students in part if you have a significant value added and that is related not just to the quality of the faculty as individual researchers, individual thesis supervisors, or individual lecturers, but to the extent to which you have a program that works as a program in educating and looking after students. We have a tradition that goes back to the 1950s, of this being a department where the faculty work together to make it a successful program. In part this comes out of the realization that we need to work collectively to get the good program that gets us the students that we really want to be teaching. People do the heavy lifting for the things that make the program function as a program. That kind of institutional culture is something that was here when I was a student. I started in 1960 and was here when I joined the faculty in 1966 and we have been sustaining it. But it's something you have to work on.

MD: Well, it looks like that wraps it up. Thank you!

NOTES

1. W. E. G. Salter, *Productivity and Technical Change* (Cambridge, 1960).
2. The following year was not as conducive to research as a consequence of the campus turmoil surrounding the Free Speech Movement.

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