

INTERVIEWER: So this is the 150th anniversary interview with professor Peter Diamond. Let me begin by asking, where were you born and where did you grow up?

DIAMOND: I was born in New York City. I did completely public schools. Started in the Bronx. In November of 2nd grade the family moved to Woodmere on Long Island and did the rest through high school there.

INTERVIEWER: Okay, and tell me a little about your family.

DIAMOND: My grandparents were all immigrants. My parents were both born in New York. Deeply and thoroughly New Yorkers. My mother was one of eight. Six of her siblings were born in Poland before the family came. Her father, as is typical came ahead and when there was enough money the rest of the family came. My father's parents met in New York. For them New York was the center of the universe and the best place for anything.

INTERVIEWER: Do you think, as you look back are there any particular experiences from your childhood, which you think were very influential in terms of your professional life?

DIAMOND: The answer that leaps to my lips is no, but let me think for a moment. I think the answer stays no. I certainly, particularly in the current environment do remember conversations connected to the Great Depression. My father graduated law school. Same law school my son has just graduated in 1929, which was not a great job market to get started in. My mother was a bookkeeper at the time. He was making \$5 a week. She was making \$15. She stopped working. Not sure exactly when. My brother, my late brother, was six years older than me so probably around the time he was born. Which again, I've been very aware of part of a generation that didn't have employment opportunities and boy, she should have.

INTERVIEWER: Why did you decide to go to Yale as an undergraduate?

DIAMOND: We had a guidance counselor in the high school who, the way admissions were done then rather different from now, called three of the higher ranking male boys into his office and said, I've been thinking about where you'd like to go to college and he made a suggestion to each of us. His suggestion to me was Yale and it sounded like a good idea. I applied to Yale and Cornell, where there would be a Regents scholarship available in New York State as my backup. Those were my two applications. I don't think I gave it more than 15 seconds thought. Seemed like a good idea at the time.

INTERVIEWER: Can you imagine our children applying to only two schools? So, as an undergraduate, what drew you to mathematics?

DIAMOND: I had no idea what I was interested in. There was nothing in my high school that was particularly exciting. The math education was okay, nothing great. I did well in the sciences, but there was again, nothing that particularly attracted me. I was also growing up in a standard suburb where most of the fathers commuted on the Long Island Railroad. Really ignorant about the range of different things. So I started college with the idea of being an engineer without any clue whatsoever what that meant. At that time Yale had all the freshman living in the same quad and there were graduate students as resident advisors there. The roommate of my particular adviser right there near by was actually studying electrical engineering, which I had picked out for the sound reason that I knew I couldn't ever do anything convenient with my hands and I thought with electrical engineering was probably less important. As you can see I knew nothing. He decided to convince me that I wouldn't like electrical engineering because of the math. So once I thought, okay, now I'm in college. I'll stop and think about what I might want to major in and I looked around and what was the class I was enjoying most? It was a no-brainer, it was calculus. So I became a math major. Again, I had no idea what kind of job opportunities would follow from that, but I enjoyed it.

INTERVIEWER: After you graduated from Yale, what made you think about heading to MIT?

DIAMOND: Okay, well I think I have to back it up a little bit. I took introductory economics in my second year at Yale and got friendly with the teacher. Probably the only teacher outside the math department I ever got friendly with, and it's Charles Berry. In my third year, and I had the opportunity of graduating after just three, which I hadn't yet committed to, but I was certainly setting myself up so that I could if I wanted to. I wasn't in any hurry. I was enjoying undergraduate life. I didn't have a whole lot of electives doing the math major in that period and the kind of other requirements there were, so I went to see him because I had made a list of what I might want to take as a free elective meeting no requirements. The ones that were on the top of my list, the famous professors that had put them on the top of the list all happened to be on leave that year. I wasn't interested in the classes without the famous professor.

So I went to him and said, what do you think I ought to take? He said *intermediate economics* for honors majors. So in the fall I took *micro* and I liked it a lot. In the spring I took *macro*, and also took the graduate class in *theory of value mathematical economics* taught by Gerard Debreu. The book, *Theory of Value*, had just been published. He was an outstanding teacher. I realized I liked economics. I also still liked math, so as I prepared for graduate school. I realized I didn't know whether I wanted to go on in math or economics. I also realized that I had completed the half of the first year standard graduate program in math. So I could start graduate school.

So I was planning to set myself up to put off the decision as to which I was going to do. I had landed a summer job working as a research assistant for Tjalling Koopmans in the Yale economics department. I applied to more graduate schools than I had undergraduate. Three instead of two. I applied to the Yale economics department and Harvard and MIT math departments and decided leaving New Haven had certain appeal. The MIT fellowship was very handsome. So I came to MIT.

George Thomas was the graduate registration officer. I came up during spring break and met with him. He took me to lunch at the faculty club. At that point in the faculty club, adjoining the dining room was a bar where they also setup for coffee after lunch. So we went in for coffee. He asked me if I played bridge, and I said yes. We sat down to a quick game of bridge over coffee after lunch. He warned me on the way in that I'd be playing with Norbert Wiener, and he was known for overbidding.

So we had a nice bridge game, and I then went and met with Bob Solow, who was the graduate registration officer in economics, and explained that I was admitted to the math department. I was coming. I wasn't sure what I wanted to major in, and I would be taking some economics classes, and he said fine. When the schedules came out, I was planning to take *real and complex variables* in the math department and *micro* and *macro* in the economics department. And *complex variables* conflicted with both *micro* and *macro*. So I decided to do the economics, skip one of the math classes, which I could make up in the summer if I decided math was the way to go.

George Thomas said given the unbalanced program, I should be registered in the economics department. He handed me in an envelope my folder, and I walked down and handed it to Bob Solow. The absence of bureaucracy was one of the real pleasures. I'm aware of that because one of my Yale math colleagues, Dick Beals, made the same switch at Harvard, and it was so bureaucratic -- and also had something to do, perhaps, with the quality of a Harvard education -- he decided to go back to math. He decided it was easier to just go to Yale and get his PhD rather than going back through the bureaucratic process.

Bob Solow promptly gave me the complete standard first year load plus *real variables*. I didn't like *real variables*. It's not my taste in math because as far as I could tell you're just proving the same theorem over and over again. It was getting more and more general. But I didn't have any basic interest in the generality. I had interest in the basic idea behind the theorem, so I found *real variables* kind of boring. I didn't find economics boring, so I made the switch.

INTERVIEWER: Can you talk a little bit about your first impressions of MIT? Of the faculty, the environment, the culture?

DIAMOND: The economics department is at the far end of campus. It's with additional buildings that have gone up since then. I started in 1960. It was considerably more isolated then than it is now. So the culture of MIT for me was rather narrowly the culture of the economics department. The economics department was and remains a wonderful place to be a graduate student. The faculty were very friendly. The doors were open. The faculty encouraged the students to interact and work together. Everything was done to hold down any sense that the students were competing with each other. Because the idea the students would all do well, the students would all get good jobs. It wasn't a competitive element. The department still bends over backwards to cut down the possibility that students might be competing with each other for a better fellowship or something like that. The idea is to organize those things ahead of time, so the students merely focus on doing well and collaborating with each other. So I liked it a great deal.

INTERVIEWER: Where there any particular you mentioned a couple of people-- any influential professors or fellow students who were influential to you in your studies?

DIAMOND: Bob Solow was my thesis supervisor and in every sense of the word, was and remains my mentor. He is a terrific economist and he's just an absolutely splendid person. I'll tell you a little Bob Solow anecdote from considerably later. My younger son Andy, was in I think it was fourth grade and the teacher had a project each year for the students that involved the whole class and the project involved, that year, the environment and politics. So they picked an environmental issue, which was the non-biodegradable six pack plastic rings, and pursued the idea of trying to get the legislature to mandate that they be biodegradable. She managed, I don't know how she did it, to get one of the committees in the state legislature to have a hearing and invite the class to come testify. Andy came home and he said, we've been learning about lobbyists and what lobbyists do and maybe we could get somebody to come with us when we testify. Andy said, do you think Bob Solow would do it? I said, call him and see. Andy called Bob Solow and Bob Solow, as I knew he would, said yes of course. So a hearing, which probably would have had one member of the legislature there for the kids, with a Nobel Laureate showing up was packed. Was on all the TV news stations and the kids had quite a time.

INTERVIEWER: It's all about networking. Once you got your doctorate how did you wind up moving to Berkeley?

DIAMOND: I was in the job market, standard job market. Not nearly as organized as it is now. Where the AEA puts out a little pamphlet of job opportunities for economists and obviously there were all sorts of government rules on the need to advertise and all of that. It was much more informal then. But it still involved primarily, but not exclusively interviews at the meetings to set up fly outs and the like. Meetings that year were in Pittsburgh. I believe the last time they ever were in Pittsburgh. The association has gotten too big for what the Pittsburgh hotels could handle, I believe. So I had a bunch of interviews. I got a bunch of offers. I visited the different places. Berkeley was the one that really appealed to me.

INTERVIEWER: That must have been quite a place in the early and mid 60s.

DIAMOND: Two things. One was that Berkeley had as the vice chair, who was in charge of recruiting at the time, Andy Papandreou, who left Berkeley and returned to Greece and became prime minister thereafter. Andy had a whole bunch of slots, and he hired at the same time Sid Winter, who started one semester before us, Dan McFadden, Olly Williamson, David Laidler, and me. The existing junior faculty there were Bernie Saffran and Al Fishlow. So first of all, it was just a group that coalesced both professionally and on the personal side. Some of my best friends remain the fellow assistant professors as is not unusual.

So it was intellectually very exciting, and the first year there it was just a fabulous research environment. I was single, and Berkeley was a nice place to be. The second year I was there it was the free speech movement, and Berkeley stopped being a good place to do research for awhile. Even classes got interrupted. It really was a shock to the system. Quite a number of the senior people left shortly thereafter. What would have been my third year at Berkeley, I actually had leave and went to Cambridge, England. Came back from there after I had accepted the MIT offer because my wife, Kate, was in Berkeley while I was in England, and it seemed to make sense to go back to Berkeley in March rather than just moving on. We got married in October.

INTERVIEWER: So did the MIT offer come to you or were you looking to leave?

DIAMOND: No, I was perfectly happy at Berkeley. I'm not sure there's any other offer I would have considered at the time.

INTERVIEWER: Okay, well can you tell me a little bit about that and why MIT was such a draw?

DIAMOND: It was the combination of it being the number one department unquestioned at the time. Afterwards, on the MIT model a number of other places have built up much better programs. Not just the MIT model, but MIT PhDs doing it. But at the time, it was clearly the number one department. I knew what an extremely pleasant place it was. How good the graduate students were. It had all the attractions for me in the abstract, which overlaid with all these positive feelings I had at MIT from my three years as a graduate student. So for me it was a no-brainer. But it was not in any way a knock on Berkeley because I was liking it thoroughly in every dimension.

INTERVIEWER: At that time, what do you think it was that made MIT's economics department the best in the country?

DIAMOND: Well, the obvious answer starts with Samuelson and Solow. So when you had two of the handful of recognized outstanding leaders in the profession you had a leg up on the couple of schools that had one. The rest of the schools that didn't have such a visible leader. Beyond that it was the fact that the MIT curriculum had focused on bringing math into economics earlier than most other places did, but did it not with mathematical analysis, for its own sake. But always with mathematical analysis to connect to economics questions. So the place had an outstanding faculty. Had the reputation, which was true of looking after graduate students and had the best graduate student body of any school. Obviously, being a faculty member in a place where the graduate students are terrific is a plus and that helps you attract good faculty and then having good faculty makes it easier to attract the good graduate students. So it's a nice equilibrium, which has been sustained for a very long time.

INTERVIEWER: You've said that you tend to work in the MIT style, can you talk about what that means?

DIAMOND: Okay, a part of it I've already touched on. The MIT style is the connection between what you're doing and the uses of economics given my focus in what was then called public finance and is now called public economics, it's very much policy-oriented. But you can't do policy without a good picture of the actual functioning of the economy. The MIT style was for an applied theorist like me, you do rigorous theory. So it passes the standards of being mathematically correct, but you do it for what you learn from doing the modeling in order to understand more about the economy and understand more about the design of policy in the economy.

The other part of it is that the process of going from basic research to either empirical understanding or policy recommendations is something that will draw on many different pieces of theory, because the economy is very complicated. Much more so than a subject where you could assume that every electron will be like every other electron. It doesn't work for people, it doesn't work for people across different people, it doesn't work for people across time. So the underlying complexity of economics is enormous, and what you try to do building a model is understand one aspect of how things interact. Then when you come to think about how the economy actually functions you draw on a lot of them.

So that leads you to do what some people disparagingly, some people favorably referred to as toy models. The idea is not to pursue generality beyond what's appropriate for checking the robustness of your findings, rather than looking for the ability to show that something can be proven in much more abstract settings. This goes back to the way I described why real analysis didn't turn me on. When you have a good result you know it can be generalized. For me, it was no fun to do it. So the idea was to have enough generality in the model to be strongly suggestive of greater robustness in the lack of sensitivity to narrow parametric structure. Instead, to have something that looked like it would be a usable tool for a number of different issues, because it had on the one hand, enough robustness, enough generality and on the other, not so much that it was hard to use as an analytical tool.

So that's the style I've been in, and I do very much go back and forth between doing basic research and getting involved in the public policy debate and occasionally, advising governments. Not much of that much more on the public side. Around primarily social security programs, both in the US and abroad. A bit around tax issues and for me, it's a back and forth between the two. My policy recommendations always have a theoretical underpinning that I have an idea where they're coming from. I mean, not have sat down and actually checked and derived, but I know where it's coming from.

The flip, when I get involved in a policy question, I suddenly spot there's an issue out there that no model has ever addressed and well, I may have an intuition it's a good thing to sit down and address. I got caught up in social security policy issues starting in the mid '70s when Social Security was in crisis. One of issues I recognized as having no theoretical underpinning was the issue of how much you should change the level of a monthly benefit when somebody starts the benefits a year later. They call these actuarial reductions in Social Security, or the delayed retirement credit if you're at an older age.

I thought, what an interesting question. so I went to Jim Mirrlees, with whom I'd already been collaborating for a decade. We have collaborated continuously since 1967 and still have a couple of things that aren't quite done. But he's in Hong Kong, I'm here. It's a lot easier to communicate now with internet than it was back in 60s when we were working on optimal taxation.

But anyway we've wandered off in somewhat different intellectual directions. So I put the problem to him and we started working at it and it led to a series of papers. The first one published in '78 that opened up a whole bunch of additional things because a good policy question leads you to a model that will often have a range of different implications. That's the fun of the back and forth between the two.

INTERVIEWER: Could you summarize for me the early work that you did in commodity taxation?

DIAMOND: I'll start with the history of that. I was teaching graduate public finance, together with Cary Brown, and making use of as the mathematical tools around duality, which was for economists a hot new tool. People were using it and I got exposed to it in Berkeley in different ways. So using that I would be redoing for class well known things in order to do them in this style and to see whether it was actually better pedagogically and whether it also opened up research opportunities. So I was explaining in the classroom the measurement, the precise measurement of deadweight burden, which is what happens when you have a tax system that decreases some of the efficiency in the economy as part of raising the revenue and doing what else it's trying to do.

There was a very famous article by Al Harberger and that had been the standard way of presenting it. I wasn't completely happy with it. There was this little bit of fuzziness about exactly how you were supposed to measure part of it. So I wanted to redo it in this form, and I had prepared lecture notes, and I went in and was delivering the lecture. I was literally in the classroom presenting this when I said oh, I could look to see what taxes would minimize the deadweight burden. It would be very simple to do given the structure I had just put on the board. So I finished the lecture, went to my office and derived it. Or I should say, re-derived it, because Ramsey had done it in the late '20s, and I was unaware of that.

I don't have a concrete memory of what happened next, but it must have been that I went and showed it to Paul Samuelson, and he clued me in to Ramsey, and he also pointed out that he, in 1949 or something like that, had written a memo for the US Treasury explaining Ramsey to them. He actually started using that in class a year or so later, as Avinash Dixit showed up at the memorial for Paul in Atlanta with a copy of what was distributed, the purple mimeograph printout of Paul's earlier piece.

But in addition to re-deriving the first order conditions for taxes I had realized there was a set of implications for government production that came from it that were interesting on their own count. That's now referred to as the Aggregate Efficiency Theorem. I went back to Cambridge, England to spend the summer and there I was doing a seminar on what I had done and Jim Mirrlees came up to me and said, you know given that you've set this up in a dual fashion while you've interpreted it as a one consumer economy with no income distribution issues. In fact, with the tools at hand you can interpret it as a many consumer economy and then explore how income distribution would affect the kinds of taxes you want to do. So we set to work on it together because that seemed like a good idea and it was fun, and by the end of the summer we actually had papers. It was presented in 1967 at both the American and European meetings of the Econometric Society. We had a fair bit of difficulty with the AER and also I was in Africa for a year and it was very hard to get joint work pushed along while we were doing that. So that then lent itself to a way of thinking about what's referred to as the equity-efficiency trade-off, which is when you make more use of taxes in order to have a better income distribution, you're also having more deadweight burdens. So a bit less efficiency in the economy and how do you want to trade those off? We extended in the paper as it exists beyond just commodity taxes to look at income taxation, but only in the context of a piecewise linear finite parameter income tax. While it was easy to get out the first order conditions and get a few inferences from that it didn't go very far. Then Jim, by himself, did the big breakthrough of the totally flexible income tax, which then set off a large literature. So that as a way of thinking about the trade-off between efficiency and equity was really a changing point in public finance.

INTERVIEWER: The name of that theorem?

DIAMOND: It's called the Aggregate Efficiency Theorem and the paper is called *Optimal Tax*. I forget the exact title.

INTERVIEWER: Different than the Diamond Mirrlees?

DIAMOND: Yeah, it's the Diamond Mirrlees Aggregate Efficiency Theorem.

INTERVIEWER: Okay because that's how I read about it.

DIAMOND: Just to finish the story we submitted it to the AER. The AER liked it, sent back some referees comments and said more or less we like the paper, here are some referees comments. Please send back a revised version. And it took us awhile because of being in different places without the internet it was a slow process of doing that. We sent it back in to the AER and by then the AER had changed editors. The new editor decided to send it out for full refereeing again and that took awhile. The new referees also liked it, but the editor said it was too long. If we would cut it in half, he would be happy to publish it. We were not willing to do that.

At this point it must have been around 1970, so this was three years old. It had been circulating as a working paper. People had been working on it and citing it. It just didn't sense. And in fact, two different journals, knowing we were hung up with the AER, offered to publish it in the next available issue without any further refereeing. Jagdish Bhagwati was on the board of editors of the AER, and he went to the editor and said, you're making a mistake. So the editor came back and said, okay. But I'm not willing to devote that much of one issue to one article.

I mean, the logic of that escapes me to this day. If we would split it into two separate articles he would put it into successive issues. Totally pointless as far as I was concerned, but he was the editor, so we did that. So that involved another delay while we were trying to rewrite the paper, to split it up into two parts. So it eventually got published long after it had been widely circulated.

INTERVIEWER: You know what happened to that editor?

DIAMOND: He went on being the editor.

INTERVIEWER: How about a brief explanation of the labor market search and match?

DIAMOND: I've done two different search items. The labor market search and match is the later one. I did an earlier one on what happens in retail markets with search. I think in terms of the intellectual history I'll start back then. One of the research areas that attracted some attention at the time was how an economy would get to a competitive equilibrium.

So we had the conditions for having a competitive equilibrium, what it would mean to have all the markets clear simultaneously and all the income distribution determined in that way. We had all the beautiful theorems, what's called the Arrow-Debreu model of how to interpret it, what its properties were, how it had been extended in many ways. The question of how the economy got there had been pursued in what's called tatonnement, which was a hypothetical auctioneer that experiments with prices, trying to find the prices that would clear all the markets at once. The question was, is there some kind of differential equation algorithm, which will converge? The answer is not always. The famous Herb Scarf example of the usual approach leading to a cycling rather than actually converging.

But that was trying to understand how that might happen and it seemed to me that that was the wrong question. That the question, how do you get to a competitive equilibrium with something that resembles the way an actual economy in fact fumbles around with some excess demand, some excess supply, some price adjustments that the right question was if you had a model of how the economy fumbled around, what would it converge to assuming it converged? So I was working on that and at a time when information and how information affected consumers was a serious issue and the idea of search was out there although the famous Stigler article was simultaneous search rather than sequential search, which gives you a very different picture.

So I set up a simple model. Again, the MIT tradition, trying to ask the question so that it would be crystal clear what was driving it, what would happen in a single market if you had frictions that the only way you could find out the price in some other store was to go to another store? That had a cost to it. It might not be much, but it had a cost. In a simple setting where all the consumers were the same and all the firms were the same, even though you had lots of firms and lots of consumers and they would go searching, it turned out the economy converged to the price that a monopolist would set. This was true no matter how small these search frictions were, and there was a discontinuity at zero.

That became known as the Diamond Paradox. It came from an interest on my part in understanding the consequences of having the resource allocation process play out in real time, rather than having a set of equilibrium conditions for successive periods. So it was the real time issue there that I was interested in and that led to a rather stark result. The result obviously was not meant to be realistic, but what is realistic is you don't get convergence to price equal to marginal cost.

So literature that came up afterwards focused in part on when you get a distribution of prices rather than just a single price and what kind of prices happen in the equilibrium. It depends a whole lot on details of what kind of information people get and how they get it and things like that. I went on being interested in this basic issue of the process playing out in real time and people adapting to the fact that they knew they were in a process that was playing out in real time. I made several unsuccessful attempts to find some modeling.

Then I read a paper by Dale Mortensen using Poisson processes to model some contracting issues. I realized -- I must have been exposed to Poisson processes as a graduate student, but I never made the connection on my own. I realized with this mathematical tool that I could readily start solving for actual equilibria, and not just the behavior of a single person, or two people writing a contract in an environment with Poisson process mathematics describing how things happen and how people learn about things happening. That, in fact, lent itself to solving for an equilibrium.

Dale was doing the same sort of thing in a related way on his own at the same time. It was a very quick process from finding the right mathematical tools to using it to ask several different questions around how the labor market works when you have to look for a job and what that means for unemployment, for wages, for the incentive to invest, things like that. I worked on and off on that for awhile including some wonderful work with Olivier Blanchard that was related in part to our wanting to do something to honor Bob Solow at the celebration of his 65th birthday as it turned out.

INTERVIEWER: I do want you to sort of as briefly as you can summarize the sort of three point plan that you proposed for fixing Social Security.

DIAMOND:

Little background on that perhaps first. President Bush appointed a commission. The commission came out with a report and a couple of alternative plans. While people were reacting and commenting on it, it was all done at a level that wasn't based on detailed analysis of what was there. I was on a conference call of similarly minded people talking about how to bring out the issues and I said I had already started writing something about what I didn't like in the plan and I would be happy to expand that and it would be good if I had somebody to work with who would be more on the numbers side and I was on the theoretical structure side and Peter Orszag who was on the conference call offered to do it. So the two of us got started analyzing with proposals from the commission appointed by President Bush and we completed that. We presented it and something that was a real shock at the time, we had a press conference in Washington to release it and within two hours the White House had issued an 18 page rebuttal. Our first reaction was, this must have leaked, but with a little research we concluded no. It was just somebody there who really wanted to get something out quickly. Then as the Social Security debate went on beyond that there was an argument being made that one couldn't restore what's called sustainable solvency. That is solvency that will be open-ended and not just fitting the 75 year traditional horizon, if you didn't have individual accounts. That seemed wrong. But unless you actually come up with a plan and the Office of the Actuary says, yes, it works, it's scoring points, but not actually proving it. So we decided it would really improve the general debate by having a concrete alternative plan.

The ground rules we set for ourselves was not to design the plan we would design if we were the Czars and could do whatever we wanted in Social Security. But to only put together pieces that really mattered for restoring solvency that fit within the income distribution concerns that had already been voiced and would be made up of parts that we would feel would not be extremely controversial. So in particular, in the book we did not talk about the current role of spouse benefits and survivor benefits and the relative treatment of one-earner couples and two-earner couples and single people. I think it's something that needs a major overhaul, but every time there's been a commission to look at it they've ended up in a fight between the people who want to encourage women to stay home and the people who want to encourage women to work. So we just would say nothing about subjects like that, that were orthogonal to our basic charge, which is design something that will work.

So we sat down and worked on it, and tweaked it and ran some informal seminars and then finally got it evaluated and put out the book. It served the purpose we had in mind, which was showing first of all that the Social Security problem was a lot smaller than the way the President would portray it as going bust because obviously payroll tax revenue continues to roll in and there's a lot of money there for benefits indefinitely. Secondly that it really didn't take a whole lot to fix it. Again, in a term that some people view as derogatory and some view as favorable, all it took were some nips and tucks and you could fix the whole thing. So we think it worked in the sense of showing you could do it. It wasn't a huge problem and it would work fine. If Congress weren't overloaded already this would be the ideal time to pass something like our plan because you don't want to do anything now that would cut benefits or raise taxes because of the state of the macro economy. On the other hand, people are very concerned about long run deficits and our plan sort of 45 years out knocks 25 percent of GDP off the debt to GDP ratio. Knocks 25 percent off the debt to GDP ratio for the debt that's in the hands of the public. That's the part that's really vulnerable to the bond market suddenly acting up. So it would be the kind of move that would fit with both short run and long run needs, but the President was obviously committed to addressing healthcare first and can't blame him for that and as we know that has tied everything up.

INTERVIEWER: Why have you stayed here for so long?

DIAMOND: I like it. I like a whole lot of things about MIT. The sense of excitement that's just around all the time. The sense of what's happening now, which is what you're really interested in. The sense that it's fun to work hard and the place MIT has in all sorts of things that affect the country and the world. They're all a source of pleasure. Then the particular context of the department. We continue to have outstanding students, outstanding colleagues. I love it when I interact with younger colleagues, when I interact with some very good graduate students. It's been very easy to enjoy staying here and thinking that there is just no other place I would like to be as much. There's also nothing wrong with living around here. I live in Lexington. I have a second home in Sunapee and the family has been very happy here.

INTERVIEWER: Are there specific ways in which being at MIT has helped you do your work?

DIAMOND: The obvious piece is the connection I have between teaching and research. Of late it's all been graduate teaching interacting with research, but early in my career it held for undergraduate teaching as well. When you have really bright students and first of all, you know you're standing up in front of them and if you say something wrong they'll notice. It gets the mental analog to adrenalin flowing as you prepare for a class and think about doing things. Secondly, it leads me to rethink what is it that we normally teach here? How is it taught? What's going on here? Just to pull an example back from my Berkeley time, my paper on the national debt, which was written before I came back to MIT, started out as a handout in an undergraduate public finance class. Now it was a year long class, so I could develop the theory that I wanted the students to understand so that they could understand how I was going to do a different presentation from what was in the literature about the effects of the national debt. But one of the glories of teaching economics at MIT is everyone has calculus from day one. So you've got students who are already geared up. But anyway, this happened at Berkeley and I realized after preparing for class that in fact this was a theoretical advance that was really worth publishing and it's had a big impact. So that kind of back and forth and it probably cuts into the quality of my teaching, except for the very best students, that I'm often looking for new ways of doing things, which particularly if I've done them the night before or in the morning, certainly aren't clear in my head and aren't all that clear in class. The students in some sense have to work harder at sorting it out. For the good students that's terrific. Particularly the ones who want to do the kind of theory I do. For the students who are more empirically oriented and are just looking for the understanding of the theory underpinnings, the lower level of clarity I do co-teaching in the graduate public finance class. I've done it with Amy Finkelstein and Jon Gruber and Jim Poterba. All of whom are outstanding teachers. In the typical year every single student will rate them higher than me. I get decent grades. I'm not a bad teacher, but I've seen what really terrific teaching is like and first of all, I probably couldn't do it anyway. But also I'm more distracted by the material and trying to understand it better, learn more about it, which may show up a year later in better teaching. But not at the time it's happening. So the direct feedback plays a role. The second thing is MIT and with the National Bureau here in Cambridge as well and the fact that we share seminars with Harvard means that the flow of people through here is just enormous. The ability to know what's going on in the profession without having to get on an airplane is just very valuable for being aware of what's going on. Then having younger colleagues because I've written papers with quite a number of colleagues means that I've stayed not as current as I was when I was younger, but I have stayed very much plugged in so that the basic theoretical research I'm doing is still breaking new ground. It's not okay, now I'm going to go back and clean up this old stuff that I never quite cleaned up that maybe nobody's interested in anymore, but has a place in my heart. I've got some things like that in my drawer that at some point I'm sure I will pull out. But instead I'm still doing new things and I think the MIT environment is a big part of why that's happening.

INTERVIEWER: So one of the things that I've noticed during the conversation that isn't necessarily the case with everybody is you work very closely with colleagues. You're publishing with younger faculty members. You work closely with graduate students. You've worked with people across the ocean. Can you talk a little bit about the value of collaboration?

DIAMOND: There are three really different kinds of collaborations and I've had some of each. One collaboration is we've got this job to answer a question, we know pretty much what we're going to do, but there's a fair bit of work in getting it done. Let's divide it up. You do this part, I'll do that part and then we'll staple it together. I've done some things like that and it permits you to get the stuff out in less time so you can do more things. So that's useful, but it doesn't really change what you can do. The second kind of collaboration, which is what happens when I get involved in doing empirical work as I've done with Jerry Hausman and Jon Gruber and Olivier Blanchard is that I have the theoretical perspective and tools and I look to a colleague who has the econometric perspective and tools and we worked together doing something that's an interaction between theory and empiricals that in a sense neither of us could do alone. It requires bringing together two very different skill set and that is the kind of thing I couldn't do alone. But again, it's the kind of thing that fits with the sort of things somebody like me would do when combined with somebody like that. The third kind is where you're engaged in a conversation with a potential coauthor and you get an understanding of the problem that neither of you would have separately. This was particularly true at the time when I was working intensely with Jim Mirrlees. We thought enough alike that we were collaborating and communication was very simple, but we thought enough differently that occasionally we would misinterpret each other in ways that would add to the quality of the output. Occasionally we would bring a perspective that was just enough different from the other perspective that then we would go off and do something different from what we would've done. I think it is a sign of certainly the best of our papers, but quite a number of the others as well. That it is different from what either of us would have done alone although in terms of having the relative skills either of us could have done it alone, but neither of us would have. I think that that's a very interesting result of different forms of interaction.

INTERVIEWER: The last one must be the one that's most fun.

DIAMOND: Oh, yes.

INTERVIEWER: What were your thoughts when you were head of the economics department? How was that experience?

DIAMOND: It was awful. The first semester went fine because I was finishing up stuff and being department head. Being department head takes a lot of attention, and I'm a list maker, and I work off lists and things like that. I did, in my own view, a good job, but what I discovered starting the second semester was I was inadequately schizophrenic. My mind running over department issues made it impossible for me to do basic research. I missed it so much that I'm told I was showing signs of clinical depression. So I stopped being department head after I served for 18 months, but by the end of the first year I had already resigned. I was just filling in till the next one was done and filling in is a different argument.

Nan Friedlander was the dean. John Deutch was the provost. Both of them very good friends of mine. Given the description of what it was doing to me and the AO at the time, Pam Hart pointed out to me that my behavior was starting to change in a way she didn't like. This was clearly coming from this and they said, well of course you've got to stop because that's what friends are for. So I stopped. It actually took me a number of months afterwards of going back doing research and having it not go quite right. Then there was one day when I'm working away, and I said, oh, that's the way my mind used to work, jumping from here to there, it had gone. My wife said if I'd done the normal three years I probably would never have gotten it back because it was a way of approaching thinking about things that I just couldn't do while being department head.

INTERVIEWER: Well, you're not cut out to be an administrator.

DIAMOND: No.

INTERVIEWER: Do you have any thoughts about the School of Humanities Arts and Social Sciences and sort of how it fits into MIT?

DIAMOND: The School as a whole, there are two dimensions to this. One is the School has some internal tensions from the fact that there are departments that have PhD programs and departments that don't have graduate programs. So the operating, given how schools work at MIT as a single school, puts together in school council departments with just very different visions of what they do, who they try to hire, how they relate to other things. So I don't know that I have any answer to how one would restructure. Maybe combine urban studies and architecture with philosophy, political science and economics as one school and the rest as another.

I enjoyed getting to meet colleagues from the different humanities and arts departments. Got very friendly with some of them and that wasn't the issue. There was a certain level of not quite matching that made for some tensions. It may not in any way have interfered with the functioning as a school. From the point of view of MIT, I think it's really important for MIT's role. Not just in undergraduate education, that there be more than just science and engineering here. Obviously a lot of engineers are interested in things involving economics. That's an easy one. For the undergraduates it's just extremely important to get some breadth in your education, because you're going to live the rest of your life off it.

I love taking classes. I don't do it all that often, but I have from time to time gone back and taken classes. Stochastic processes, classes in the law school. Most people, when they leave school, they're done. The rest of their lives they're living off the intellectual capital they accumulated as a student. Our department has more breadth requirements in the PhD program than any of the other leading departments because the idea that this is a foundation for 30 or 40 years of research and you better recognize you may end up doing something different and having some background will give you a chance of actually making a move.

So I think it's extremely important that students have additional perspectives on the world, on life, on things to do, on things to enjoy, things to learn from. Then for the faculty again, you're in an environment where you're aware that there are these other things happening on campus. You go occasionally to a lecture, a seminar in all sorts of different things and it makes for a richer life and richer lives sometimes lead you to think better.

INTERVIEWER: Since you've been here for a number of years, what kinds of changes have you seen in the culture of MIT, in the student body, in the faculty?

DIAMOND: In terms of the changes of MIT while I've been here, as I said, right at the beginning the economics department is way off the end of campus and we mostly lead a departmentally focused life in teaching and research and don't interact all that much. What is stunning in the economics department is the continuity. Yes, we share the changes that are country-wide. We have a larger fraction of foreign students than we did when I was a graduate student in the early 60s. Yes, we have more women in the PhD program than we had in the early 60s. And yes, of the Americans we have there's obviously much more diversity, for lack of a better word of the different backgrounds of people. In the 70s when we realized how few black PhDs there were in economics and how in fact there were some schools that were turning them out, but not the elite schools. We ran a deliberate affirmative action program bringing in black graduate students. Holding them to the same standards we had, but with the expectation it would take an additional year to get to general exams depending on background. We produced a large fraction in, I think it was five years we had that program, of the top black PhDs in economics that are out there. After that it seemed to us less of an issue, that the other schools had gotten interested and because the undergraduate world was changing it was much easier to find black incoming students who had the kind of good background that were easy to evaluate and you didn't worry that they didn't have enough of a good enough undergraduate education and needed some remedial work as part of going ahead and reaching their full potential. So we dropped having a concrete program. So that's as it were, two changes. One as we went to having that and then we went to no longer needing it. Obviously with the undergraduate body there are far more women, far more foreign students, far more diversity. I don't, in my narrow set of interactions with students, get much of a sense of things having changed a whole lot. But then again, economics is sheltered from a lot of things that happen at the Institute, so I wouldn't view that as a really informed sense of what's going on across the Institute generally.

Oh, I guess the other thing I would identify, because I was on the committee that did, it is the obvious explosion of the importance of biology. I was on the committee that added the biology requirement to the undergraduate education. I played a very minor role in it, but I was very happy to be part of it because it seemed like exactly the right thing to be doing at the time.

INTERVIEWER: You mentioned a couple of prominent colleagues. Is there a particular story that you would have about, let's say, Paul Samuelson? **DIAMOND:** There's a Paul Samuelson anecdote that I enjoy telling, capturing the way he was committed to the MIT economics department as a teaching environment. That this was important in his self-descriptions and the many autobiographical things he's done. He's referred to himself as a researcher and a teacher, and obviously everyone knows about the principle text and it's role. But he was also a central part in having the graduate program be concerned that students get a good education, and an enjoyable education, rather than the graduate students being fodder for research assistants. Main focus was the quality of the education.

So in the spring of 1961, when Bob Solow went off to the Kennedy administration, he would otherwise have been teaching the second semester of the graduate year long macro course and Paul Samuelson was scheduled to teach the second semester of the the year long Microeconomics class. with Bob leaving, Paul ended up doing both of them. So we got a large dose of him there. He also taught a *mathematical economics* class that was for second year students, we took the following year.

I remember watching him on TV on a Sunday night being interviewed in Washington and explaining to the interviewers that he was a teacher, and he had a class the next morning at 9:00 a.m. and I could nudge my roommate and say that's our class. You know, he's talking about us on TV and then what often happened is he would be flying up in the morning. He would catch a very early plane out of Washington, which was a sign of his wanting to actually make the class, but occasionally with bad weather the planes would be a little bit late. at 9:05 his assistant, Inez Crandall would come into the classroom and say, I've just had a call from Professor Samuelson. The plane has landed at Logan, he's on his way, would you please wait? Of course, we would wait.

He took the attention to teaching as something very serious and something important. Given all the things that were going on that semester, the quality of the lectures were sometimes uneven, but he was unfailingly interesting.

INTERVIEWER: What is it that you think makes MIT unique?

DIAMOND: Given the science and engineering orientation of MIT there are only a few other schools around: Cal Tech, RPI, a scattering of others that have the same orientation. MIT though has in it at a very serious level, an economics department. So the general uniqueness is it's this outstanding quality and it's science and engineering primarily focused, but still having the range and reach of a university in terms of the range of things that it taught seriously. But you know, not with all of it at the top level, at the PhD teaching level, I should say. So that is an element. From the point of view of an economist teaching in a place like this there is such a difference compared to the economics departments at the other leading schools where economics is typically the largest undergraduate major and therefore the role of the department in the university is rather different. Economics is very popular as a concentration for undergraduates, but in terms of majors that's a small item. So our teaching is different on two grounds. One I mentioned earlier. Obviously the mathematical background of the students and the ability to move into more advanced things much more quickly. But secondly the fact that overwhelmingly we are teaching economics to non economists. Having a different relationship between graduate and undergraduate programs. That is different. What implications might follow from that I don't have a clue.

INTERVIEWER: Is there anything else about MIT in particular that you think is important to say before we go back to some of the other things?

DIAMOND: Yes. There's one thing that has always struck me as a real plus given the quality of leadership of MIT. It might be a real negative otherwise. I think of it relative to a short story by-- suddenly I can't remember-- anyway, it's one of the writers who wrote maybe in the 30s or the 20s short stories about baseball players and other things. Baseball players would gather in the lobby of the hotel where they were staying and play cards. People, fans, would naturally sit around and watch them and then they'd ask if they could play too. They would play and they would always lose money at these games. The game was called tegwar, which is an acronym for the exciting game without any rules. That's why the outsiders always lost because they would invent whatever rule they needed as they went along. I've always thought of MIT as a bit like tegwar. That the Institute leadership, you could go to them with some idea and there's flexibility. The flexibility of viewing every rule as an adjustable rule. That could be horrible with the wrong people on top, but with an administration-- and they've all been like this-- that is very sensitive to the idea that it's the faculty and students who make MIT great and then the administration helps. But it's not about the administration. It's about the faculty and students. And so having that level of flexibility, the sense that you can negotiate about almost anything, there are some serious hardlines where they ought to be. I think that's one of the things that makes MIT so good at sustaining its excellence.

INTERVIEWER: Certainly enhances academic freedom. You've advised the US Government and other governments around the world about social insurance and you've looked at lots of different systems, and I wonder if you could talk a little bit about what's internationally the same in terms of the issues and what's internationally the same in terms of the obstacles to fixing problems? Is that clear?

DIAMOND: Yes, so you want me to draw on the fact that I've been involved sometimes directly for a government, more commonly at an invitation to do some speaking and writing about what's going on in a country with a lot of different countries. The question of similarities and differences, what I'll tell you about is where my answer is coming from. I may or may not tell you the answer.

A few years ago I was invited to write a report as part of a small team for a think tank in China, government related think tank, in order to both make concrete policy recommendations about pension systems and also to lay out the analysis in an attempt to improve the quality of the public policy discussion that was going on in China. While there were a number of us there, the bulk of the work was done by Nick Barr of LSE and me. The others all played a role, particularly on the policy side, rather than the laying out the framework side.

So we set to work trying to lay out the underlying theory of how you ought to think about a pension system, in general and in China, specifically. Then we got the good news, bad news. The good news was, we were actually going to present our findings to the premier, Wen Jiabao. The bad news was that that meant that report was limited to 30 pages. Well, we probably had close to 100 pages of just background material at this point. It was clear that we would be picking out very little of that and incorporating it in a report. Nick suggested that-- that was a part just the two of us were working on-- that we had much of the work done for a book on the subject, and it would be useful to have a book like that. *Most of the work* was of course a gross error on both of our parts.

So after we finished the China project we went to work writing a book about pension systems. Drawing primarily on our experiences, which were the US and the UK, but also drawing on some of the other countries we'd been involved in. We'd both been involved in Chile. Obviously both been involved in China. I had done things, well, involved quite a bit in Sweden. Little bits with a number of other countries. Probably the Netherlands and Italy are the two that I put the most time in on.

So we wrote a book exactly addressing the issue of what are the elements that you're trying to accomplish with a pension system, a national pension system. Secondly, making the point that there is no single institution that is the right answer. There are good reasons why different countries would have different structures. Secondly, recognizing the historical process means that when you're doing reform, rarely are you doing radical reform. Usually you're building on an institution that is there and trying to fix it. So the basic issues of helping people have enough in retirement, what we refer to as consumption smoothing. Recognizing that there are a number of reasons to be particularly concerned about the poverty of the elderly just as there are particularly of children. But your options for doing things are different than when you're dealing with the working age population. Recognizing that the future is uncertain for everybody and your pension system can play a role in dealing with uncertainty and risks even though it will only play out when you're retired. But you can design the system so that people who end up with bad labor market outcomes don't hurt their retirement as much as if you didn't have attention to insurance. So these basic issues are what everyone, every system is trying to address in different ways. Then we explore in the book the different kinds of institutions and recognizing that any system impacts on the functioning of labor market and the capital market as well as just providing income to retirees. We've laid out theory. We surveyed what different countries do with some applause and some criticism. To have a book out there we liked it enough that we thought it was perhaps too long for reaching all of the audience we had hoped to reach, so we brought out a second version that was literally half the length of first. So that is the answer really to your question about similarities and differences. The goals are similar, the outcomes need not be and still can be quite satisfactory. The political process is inherently different country by country.

INTERVIEWER: What other things do you think are important to say? That you think would be of interest that I haven't asked you about?

DIAMOND: We've talked about the role of the MIT economics department in the economics profession and economics education and one of the striking elements of that role has come out with the financial crisis that has come. Because at MIT, we have never stopped teaching the original economic stabilization, Keynesian ideas, but also the ongoing research to explore them in more detail. What my colleague Ricardo Caballero refers to what's been happening at the periphery, as opposed to what has taken over the main part of macroeconomics.

With an event that simply cannot be, in any way, shape, or form, addressed by what has been the center of teaching in most of the other departments. Something that Bob Lucas, perhaps the leading intellectual that has led to this group, has acknowledged not after it happened, but before the fact. Identifying crises elsewhere that were smaller, and saying how the standard theory useful for some things isn't useful for that.

So the fact that we have kept alive different parts of the research focus and done that from what I think of as the MIT tradition of being very focused on the fact that you don't say a model is good or bad. You say it's good for this purpose and bad for that purpose, because it's in the nature of a model that it's a simplification of reality and therefore must be inaccurate in certain ways. So you've got to use it for thinking about what's going on in a way that's sensitive to the connection between what the model does well and what the model does badly in the question you're interested in. The idea that the department has always viewed theoretical developments in the context of how they relate to what goes on in the economy has meant that we've kept alive and taught things that have disappeared from a number of other curricula.

Now that there is enhanced interest in this sort of thing, we are actually in a position to be moving rapidly and trying to change what's there from what we have learned at the way the interactions were different from what we had imagined before. So I think there have been two times in my career when it was really interesting to be an economist. The first was when Communism ended and the issue was, how do you go from a Communist economy to a Capitalist economy? How does that happen? And the second is now, when we're having the financial crisis of time that is revealing details that people, by and large, were simply unaware of. The whole range of different interactions and how they play out in real time, which goes back to a theme I talked about earlier, understanding how the economy plays out in real time.

I'm interested, as I'm coming up on retirement actually, in picking that as my primary focus as I stop teaching and start working on something new and different, just for the fun of it. New and different, but connected to a prime concern I have had about how the economy works in real time. Had for decades.

INTERVIEWER: I didn't get to ask you about some of the awards and honors you have, but I wonder if we could sneak in one more and I could just ask, did your appointment to be an Institute Professor change your relationship at MIT in any interesting or significant way?

DIAMOND: Two things. First of all, obviously I was thrilled and delighted to have it happen and extremely pleased. I've enjoyed the interaction with the other Institute Professors and enjoyed the enhanced interaction with the administration that's come from that.

It's had two implications though. One is that when I'm asked by the administration to take on a task I kind of feel I have to say yes. There's some that have been interesting, fun and not too time consuming. Some that have been interesting, not so much fun and very time consuming. So that's one thing that changed.

One very delicate issue, when the provost was accused by a faculty member of having rigged a review. The conclusions from it. I'd chaired a committee to lay that out for the Institute community. But I didn't do much of that since I became an Institute Professor. I'm doing more on taking on issues. Within the department it's really made no change in my interaction with colleagues and research. It has changed my teaching a little bit. I did take advantage of the license to do whatever I wanted. I've made no change whatsoever in my graduate teaching. My undergraduate teaching, because of making room for my younger colleagues, I was no longer doing teaching in areas that were of research interest to me. So I was doing more service-related teaching. *Law and economics* was the primary undergraduate teaching I was doing towards the end. While that's a fun class to teach, it wasn't a whole lot of fun for me because I wasn't getting any intellectual feedback. So I stopped doing service teaching as a result of becoming an Institute Professor and I probably spent considerably more time doing administrative matters than I would have done teaching if it wasn't for that.