

INTERVIEWER: This is the MIT150 Oral History Project. The date is August 15, 2007. Brian Keegan is interviewing professor Robert Solow. Thank you for taking the time to be with us today. We just want to start off and go over biographical background before we get to your more academic accomplishments and your reflections. So I thought that maybe we'll start right from the beginning-- tell me about where you grew up, what you were like as a child, and what your parents did.

SOLOW: Well, I was born in and grew up in Brooklyn, New York which was one time a favorite-- it would bring a laugh from an audience, if I said I came from Brooklyn. But I did, in 1924, and I stayed there until I came to college 16 years later. We were a sort of lower- middle class family. My father was in the fur business. And the only piece of occupational advice he ever gave me was to say once, you know, Bob, I don't care what you do with your life, but if you go into the fur business, I'll kill you. So I didn't go into the fur business.

And I was a perfectly normal middle class kid. Played ball a lot, hung out with my friends, we lived-- it was a nice neighborhood. Always good in school, from the very beginning I got good grades and got them easily, liked to read. But given a choice between reading and playing stick ball or softball or football or whatever, I would choose the games any time. And I had two sisters, both younger than I. And I guess the only other important thing to say is that my mother and father, I think of as having been extremely intelligent people, but neither of them went beyond high school. They could not afford it. They had to go to work as soon as they got out of high school. But that they had the capacity to do a lot more, both of them, I have no doubt at all. So all you have there is a normal urban childhood in a lower- middle class part of Brooklyn. I went to the local public school, elementary school, PS 197, it was called through sixth grade.

Then I transferred to a junior high school-- a little different from what a junior high or middle school would be now. It was a distance away, so I had to take a bus. Not a school bus, just a regular bus, on the street for a nickel to and from school. But it ran-- it did seventh, eighth, and ninth grades in two years. So it was sort of a selective school. And I got through ninth grade there, and then went to the local high school-- James Madison High School, which was no more than a 1/4 mile from where we lived. And graduated from Madison when I was 16.

I had skipped a couple of grades in elementary school-- I learned to read at a very, very early age, not being taught. I don't have any recollection of my parents trying to get me to read, but I was curious, I guess, and so I could probably read by the time I was four- years- old or something like that. And so I got out with the three times-- grades I skipped in grade school-- and extra year saved at junior high school, instead of graduating from high school at 18, I graduated at 16.

INTERVIEWER: Now did the Great Depression-- how did that affect your family or growing up? Was it particularly onerous?

SOLOW: It was an important thing in my life and probably has a lot to do with attitudes I have, even now. My father was never unemployed for any length of time-- he always made a living. And we didn't live in any hardship at all. But, as a kid, say from the age of eight on, I was conscious of the fact that all my parents did in their free time was worry. And their worries were purely economic, what was going to happen? Could they continue to make ends meet in a satisfactory way? They talked about it lot, and I could overhear them talking about it with their friends, and so I was very much aware, even as a kid, that something bad had happened and that it was called the Depression. And it meant that there were a lot of people out of work and a lot of people were poor and hungry, and that stuck with me.

INTERVIEWER: Do you have any memories of FDR and his fireside chats? Or talk about economic issues?

SOLOW: I have clear recollection of-- in fact I even have some recollection of the election of 1928, when I would have been four years old-- because I remember the name Al Smith who was the Democratic candidate and lost the election to Herbert Hoover. But when it came to 1932 and the first Roosevelt election, we were very aware of Roosevelt. I heard his voice on the radio. I'm not sure that I listened to the early fireside chats because I might have been in bed by the time that those came on. But Roosevelt was a hero to my parents and their friends and we, I mean I and my friends in the neighborhood, were completely aware of him. We knew his voice, we heard speeches occasionally, and we knew that he was trying to lick the Depression.

INTERVIEWER: Tell me about your scholarship to Harvard, or how you came to go to Harvard.

SOLOW: I've said that I graduated from high school at 16. In my last year there, my senior year there, a person who was of considerable importance in my life was a teacher I had. Her name was Mrs. Towster and she and her husband sort of befriended me. She taught a wonderful elective course for seniors at Madison High School in which we read great novels. We read-- we started with *Candide* by Voltaire. We read a Balzac novel, we read Stendhal, we read Flaubert, *Madame Bovary*, we read Turgenev and Tolstoy. And I think that was the first experience I'd ever had when I was interested in something for purely intellectual reasons. I said I always got A's school, but I didn't care about it. I did it because it came easily to me and there was no reason not to do it. But that course and Mrs. Towster and her husband got me interested in classical music for the first time, I'd never heard anything like that. And there was-- I spent a lot of time with them after school, and they were always talking about serious things. I guess that was the first really intellectual experience I'd ever had.

So, here I am getting out of high school at the age of 16. And I sort of took it for granted as an act of nature that I would go to Brooklyn College, which was the branch of the City College-- City University of New York in Brooklyn, not far from where I lived. And there were no guidance counselors or anything like that at the school. I might mention that Madison High School when I went to it had 10,000 students. It was not a small institution. It was in two sessions-- you win either from 8:00 to 1:00 or from 1:00 to 6:00. Freshmen and sophomores at one time, juniors and seniors at another-- I don't remember which was which. But I just sort of took it as hardly a decision, I would go to Brooklyn College. And that teacher, Mrs. Towster she said nope, you should apply for some scholarships. You should-- you can get a Regents Scholarship from New York, there are a lot of those-- New York State, I mean. And you should apply to Columbia and you should apply to Harvard. I think those are the only two she mentioned. So I did it. And I got-- actually, there's another thing worth saying.

There was a scholarship offered by the Harvard Club of Long Island. If you know your geography, you know that Brooklyn is actually part of an island called Long Island. And I applied for that and it was a nice scholarship-- just for one year, freshman year. And I was interviewed and I was turned down. And it was pretty clear from the context that I was being turned down because I was Jewish. There was-- I remember one of them asking some sort of unkind question. And I don't remember what I would have said, but I didn't get that and I'm pretty sure that's why.

There was also a Harvard Club of New York Scholarship that I had applied for, and that I got. And it was worth \$600 for one year. Tuition at Harvard was \$400 and \$200 would almost pay for room and board. And so I went. And actually, I managed to support myself totally. I don't-- from the age of 16 on, I've been, I was totally self-supporting. Never took a dollar from my mother and father-- I didn't want to. I had two younger sisters that had to go to college. There wasn't a lot of money to go around, as I said. I figured they'd need it. Also, I had this feeling that if I was getting money from home, then I would be a little bit under the control of my parents. And I think from probably-- I don't know at what age, but a very early age long before that, I was very independent. Never wanted anybody to tell me what to do, I liked to do it myself.

So I went to Harvard College, age 16. I had this \$600 scholarship. I got a part time job to fill out the rest of the money and just went. I got straight A's in my freshman year and so my scholarship was reduced from \$600 to \$400 because it was no longer on the Harvard Club of New York but on Harvard University. So, I got a second job and was still able to support myself. I worked in one of the libraries and I was a busboy in a restaurant in Harvard Square. And the truth of the matter is, I never-- didn't have the any trouble getting A's in those courses. It just came easy. So I did.

INTERVIEWER: So you entered Harvard in the fall of 1940. This is obviously, the war in Europe had already increased in tempo by then. But maybe you could tell me about your awareness of the events going on in Europe were or America's coming involvement.

SOLOW: Yeah, that's sort of interesting. I knew about the war, of course. I was interested in politics, as were most of the people I knew both at home and at Harvard College. And we knew about Hitler and all that. But I have to say, I know exactly where I was when I heard about the attack on Pearl Harbor. I was in-- so this would have been in December '41-- I was already a sophomore. I was in taking a course in psychology with a professor named Henry Murray in a small building at Harvard Square and I was sitting there. And it must have been December 7 or 8 of 1941 and we heard about the attack on Pearl Harbor. That came as a total surprise. I hadn't really thought much about whether the U.S. would end up in the war in Europe. I was certainly not an isolationist at that time-- or ever-- but I don't think that it preoccupied me or my contemporaries then. So this Pearl Harbor came as a shock. And then from that time on, of course, we knew that we were going to be involved in Europe. We weren't just going to fight Japan alone. So from that point on, the war was a really very important thing. And I decided very soon after that-- maybe at about that time-- that I was going to join the Army. I was going to volunteer for the Army. And I've never been sure exactly why-- it was for two reasons I suppose. One reason was that I really was conscious of the fact that Hitler had to be beaten. I was like a lot of my contemporaries then, sort of left wing. Never a communist, actually, never remotely. But on the left, definitely, and I knew that Hitler had to go and if Hitler had to go, I wanted to be part of it. And the second reason was, I think I was bored out of my brains at Harvard College and I needed to do something else.

So I-- at some point, I don't know exactly when-- probably in the spring of 1942-- I volunteered, I joined the Army. And I was told that I would be inducted sometime toward the end of 1942. Why that time lapse, I don't know. But that's what I did.

INTERVIEWER: Did it ever pass through your mind that you could have gotten a deferment? Or served stateside?

SOLOW: Oh, I knew that--

INTERVIEWER: Or, you wanted to be in Europe?

SOLOW: I knew that perfectly well because even after the draft was instituted, I knew that most of my friends-- not all, but probably the majority of the people I knew at Harvard-- were going to get some kind of deferment. There were various programs you could join with names like V-12 or something like that, which were in effect a glorified ROTC and meant that you would be able to finish college or at least go on for two years or three years without actually being in the service. And I definitely did not want to do that.

INTERVIEWER: So you served in the Italian campaign. Tell me what you were involved in.

SOLOW: Well actually, it was very interesting. One of the things that I suppose rarely happens in the Army. After I was actually inducted and I was at Fort Devens, not far from Boston, which was the induction center. I knew two things amongst other-- there were two minor skills I had picked up. One skill, I knew German. I could speak German pretty fluently, actually. Because when I came to Harvard in September 1940, my randomly assigned roommate was a German refugee, Gerhard Nellhaus. And I was just clever enough to say to myself, this is a good opportunity. You're going to take elementary German as one of your freshman courses and you're going to have Gerhard to talk to at night and evening and you can learn German pretty well that way. And so I did. And by-- after two years, by the end of my sophomore year-- in June '42, I was practically fluent in German. So I had that skill.

Also, I knew Morse code. Because at some point, the psychology department at Harvard had some kind of contract with the military to improve the teaching of Morse code, to train radio operators. And so there was an ad somewhere that if you came twice a week in the basement of Memorial Hall as a guinea pig for this, you could learn Morse code. That struck me as a cool thing to do, so I did it. And I was pretty good at Morse code.

So here I am, I know German and I know Morse code. And I get assigned-- ta- da, I mean this hardly ever happens-- to a signal intelligence, a radio intelligence unit, whose job-- I don't know if anybody does this anymore-- whose job was to listen to very low level German radio traffic. I don't mean, not like, bright people did in England and break codes at very high level. This was platoon headquarters, to company headquarters, company headquarters to battalion, battalion to regiment. I don't think we ever listened to traffic from any unit bigger than a regimental headquarters.

In order to do this, you had-- it was, these were the early days, by the way, there were-- it was line of sight. You could not hear what you could not see. Now you didn't have to be as far from me as I am from you. Signal would carry a distance. But almost any obstacle would block it. So we-- after we learned our trade-- our job was to find a place as close to the Germans as we could manage without being overexposed. Because we had to do this stuff in a completely closed-in truck. Which had eight or nine radios operating and a place for people like me-- I'll tell you about that in a second. And so, if we were clearly visible, we're dead. So what usually-- the idea was to get just behind the brow of a hill with the Germans on the other side and so you could poke an antenna up over the hill. And they would send this traffic sometimes in plain language, but more often in elementary codes. And my particular job, along with some others, was to try to break those codes in real time. They were very simple codes, but you had to understand them very soon because a message might say, you're going to get a delivery of machine gun ammunition at such and such a map coordinate at 7 o'clock this evening. And if the message came at noon, it had to be read or else it was useless. But more often than not, we were able to crack those codes and read them, and the delivery of machine gun ammunition would be met with a fighter- bomber or some mortar or whatever. And score one for our side. So I did that for-- in Italy, for two years. And from the bottom of Italy to the top, actually. And we were a very successful unit. We were cited by the Corps' headquarters or whatever a couple of times. We were very, very good at it, I have to say. And it was also a marvelous experience for me.

First of all, it was dangerous enough so I didn't feel like I was sitting out the war in comfort. But it wasn't like being a rifleman-- I had an excellent chance of surviving. So I didn't have the guilt of being safe, but I had a little bit of-- a good shot. And it was a small group-- a company of about 120 men doing various functions and extremely well led. And since it was successful, doing good, there was very high morale. And we were buddies. And I just enjoyed that. I must have been the youngest person in the unit since I was-- I started off, I was well before my 19th birthday. And these were, I thought, terrific guys. And they were not all college students. Most of them were-- the radio operators themselves, the guys who could do Morse code very fast and with a lot of noise-- most of those were high school graduates, if that. The drivers, the men who laid wire, all that. So I got to be a very close friend with a cross section of people. And I liked that a lot.

And I also acquired a real taste for small high morale groups doing a job. And the MIT economics department is a prize example of that for me. It was the same sort of feeling although, of course, the personnel was a little different. So, we worked our way from the bottom of Italy up to the top when the Germans surrendered in May 1945.

INTERVIEWER: Now you refused any promotion beyond the rank of sergeant, is that correct?

SOLOW: I got to be technical sergeant-- and if the war had lasted another few months, I'd have made master sergeant, but I'm just as glad it didn't. I could have had a commission at one point. My company commander was a marvelous man-- died a few years ago, we stayed friends until he died. He offered to-- he wanted to put me up for a battlefield commission-- make me a second lieutenant. And I thought about it for about 30 seconds and said, gee no, I don't want to do that. I can remember pretty clearly what went through my mind was that if I became an officer, I'd have two choices. I could stay in the unit, in which case my relationship with all these guys who were my buddies would change. They would be supposed to say, Sir. The alternative was to join another unit. I didn't want to do that either. So I said no, no thanks. I'll just stick it out where I am. And so I got my promotions and I was a high-ranking non-com.

INTERVIEWER: Now you were discharged in August 1945 and you decided to return to Harvard. And tell me how you finished out your last couple years at Harvard and what made you decide to pursue economics?

SOLOW: That's a funny story, too. At Harvard, before I left, in 1942 I was in, I had finished a fraction of my junior year before I was actually in the Army, probably in November '42, something like that. I had gone to Harvard College-- if I had any thought at all about what I would study, it was botany.

Why botany? Because I had heard that there were jobs to be had in the Forest Service. This was 1940, the Depression was not really yet over. The unemployment rate in the US 1940 was about 14 percent. And being able to have a job was a very important thing. And someone told me there were always jobs in the US Forest Service. And I said, well, then maybe I'll study botany and I'll be a prize pick up for the Forest Service. But I took a biology course my freshman year-- and I got an A in it-- but I knew it was not for me. I really couldn't do it. I was just not good at it. I could master it, but it was clearly not right for me.

So I didn't know what to do. And they had a sort of catch-all major at Harvard then called the Area of the Social Sciences. And I said, that I and all my contemporaries knew that our society had broken down pretty badly. Meaning not just American society, but on a wider scale than that. The Depression had made a deep impression on me, and the coming of Hitler and the Nazis and the war was yet another sign that society wasn't functioning very well. And so, it seemed to me very important to learn something about how society works. And maybe be able to do some good about it. So I signed up for this Area of Social Sciences thing and I pursued it for the rest of my time until 1942. And I had a couple courses in economics, I had a course in political science, took some sociology, took some anthropology. Was able to go amongst all the social sciences-- and the variety was nice.

Then I go to the Army, I'm in the Army for just about three years-- a few months short of three years. And I come out-- I get home in July of 1945. And the first thing I do is get married because I had a girl and we'd been writing letters to one another. As she always says, her father must have near died. Here's this guy she had known for maybe six months or something like that back in Cambridge, Mass. And he goes away for three years and then comes back and seven days later-- which was the time it took to get it done-- we were married. But we got married, we've been married for 62 years now, so it must have been okay. And we were on our honeymoon, we decided to have-- I was still in the Army. I'd come back in July and I got a-- I can't remember, a 30 day leave or-- everyone did. I had a leave, I got my-- I was still in uniform. I was still in the Army.

And at that time, for all I knew, I'd be heading back-- heading off to Japan. This was before-- oh, I have to tell you. That on the troop ship, coming back from Naples to Newport News, Virginia this would have been toward the-- well the date will come out, I don't remember the exact date, but we'll know. This is a, like, seven or eight days on a troop ship, heading back. We were all fully expecting to have our month's leave and then get shipped off to Japan. We heard over the boat's-- the ship's-- PA system, the news of the Hiroshima bomb. And we all cheered. There's this ship chock-a-block with returning-- with guys like me-- and we cheered. And I have never held it against Harry Truman for a second that he dropped that bomb.

So by that time-- by the time we landed, it was probable that the war was going to end. I remember hearing on the radio the announcement that Japan had surrendered. But anyhow we-- so Bobby and I got married, and we thought we'd have a honeymoon before I had to go back to Fort Dix. And so we were going out on the Cape somewhere, on Cape Cod. Falmouth, I think. And we got as far as Cambridge and had to spend the night in Cambridge-- or in Boston-- to get a bus the next morning. And we stayed in a little inn near Harvard Square. I do remember-- boy, how times have changed-- we showed up to stay at that inn and, you know, we're 21 years old and I'm in uniform. And the innkeeper wanted to see a marriage certificate. He wouldn't contemplate renting a room for a night to some soldier and a broad-- they might not even be married. Fortunately, I had a marriage certificate-- we'd been married the day before.

So we were staying in that inn, and we were listening to the radio. And they announced the point system for discharges very soon after the Japanese surrendered. You got a point for every month you were in the service, you got an extra five points or one point for every month you'd been in a combat zone. You got five points for this, five points for that, and if it added up to 85 points, you were immediately eligible for discharge. So I'm sitting there adding, and finally it added up, and I said, I'm out.

And there I am near Harvard Square, it's-- we were married on the 19th of August. So it must have been the 20th or 21st of August, and I figure, I've got to go back to school in September, I'm going to be discharged right away. And so then I figure I'd better go tell them that. And then, I had to decide what I was going to study. And I didn't know. So I said to my wife, now you were an economics major. Was it interesting? And she said, yeah, it was very interesting. So I said, all right. We'll give it a try. And I went over to the economics department and told them I was coming back in September. And that's how I became an economist.

Now the interesting story is how my wife decided to study economics. She was a Radcliffe student and she was taking-- she was, I think, a literature or an English major, something like that. And she was taking a course, and one of the books they had to read was a novel by Charles Dickens. *Bleak House*, maybe. And she hadn't read it and she had a paper due the next day. So in her dormitory, she asked if any of the girls had read *Bleak House*, and one of them had. She said, tell me the story, tell me the plot. Girl told her the plot. She went up to her room, wrote the paper, turned it in the next morning, and got a B+. And she said to herself, there's got to be something wrong with a subject when you can get a B+ on a paper under those circumstances. So she did economics. And that's how I became an economist. And I came back in September '45-- we came back-- and I had to finish my undergraduate degree. And then naturally, go on to graduate school.

INTERVIEWER: What compelled you to attend graduate school? Was it a natural progression that you had--?

SOLOW: Well, yeah I did very well. If I have any talent at all, it's for getting high grades in courses. I did very well, I liked it, it was interesting. It was really quite fascinating. But then I graduated, in 1947 or something-- yeah, I had two more years to go-- in 1947. Had to decide what to do next. So I consulted my wife and she said, I think you ought to go to graduate school. And I said, well, I will if you will. So we both registered for a PhD in economics. It just seemed like the natural thing to do. I was enjoying it. And so that takes me through graduate school.

INTERVIEWER: What was the intellectual climate like?

SOLOW:

It was actually-- it had pluses and minuses. This was the beginnings of the flood of returning veterans with the GI Bill-- that's how we paid for it. It meant that the university was jam-packed with students. But they were very good. These were guys-- most of them had graduated and then been in the service-- whether like me as a volunteer or just as a draftee. And it was now two, three years later. And they were more mature than your basic graduate student is-- they were older and they had seen a little more of life, at least military life. And they were in a hurry. They knew they were older than they would have been-- for many of them like me, two or three years out of your life. So they were dead serious about getting their degrees.

And so the atmosphere was really very good although it was very crowded. And it meant that on the whole you didn't get a lot of quality time with the faculty. I did, actually, by good fortune. While I was still an undergraduate, I had come to know Wassily Leontief, who was a professor of economics at Harvard. And because every junior or senior-- at least senior, and I think junior and senior-- had what was called a tutor and you were supposed to meet with them once a week and he'd give you something to read and then talk about it the next week. And as luck would have it-- probably wasn't luck, it was probably the fact that they knew perfectly well that I had been a straight-A student when I was-- in '40, '41, '42. And so they assigned me to a good person. And Wassily was a wonderful teacher and he did a marvelous thing for me. I had, at that stage, never taken a college level course in mathematics. I was extremely good in high school math, but I just had no reason to go on with it, and didn't. And Wassily Leontief would say to me-- well, here's an interesting thing for you to read. But no, you can't read that because you don't have any mathematics to speak of. So why don't you read this other thing instead?

Well, it took only two weeks like that and I figured, I want to read the good stuff. So I started with elementary calculus and did the whole bit. And that was an enormous favor that Wassily did for me. So it was-- but then I was lucky as a graduate student even in those crowded days I believe the number that sticks in my mind that there was something like 280 graduate students in economics at Harvard, in those years from '47 to '49. That's an enormous number. And the courses were all packed. But I was lucky since I knew Wassily Leontief, and he knew me. And I worked as his research assistant part-time, and so I had a very good education there.

INTERVIEWER: What was your dissertation on? In lay or so many words?

SOLOW:

I got interested in the size distribution of income. You know if you array annual incomes of families, or individuals, in a country like the US, you get not exactly a standard bell curve. But you get a skewed bell curve with a long tail on the right. There is a very small number of very high incomes. And then as you go to lower and lower incomes, a bigger and bigger fraction of the families earn that much. But then eventually it reaches a peak. And then there's a fairly sharp decline as you go to very low incomes.

And I was interested in studying that. And studying the process that gives rise to that. And I was also influenced by the fact that having come just fairly new to any kind of serious college level study of math, I had learned probability and statistics. And I was fascinated by probability. So I thought maybe I could use what I have learned about probability to study how this frequency distribution gets formed. And so that's what I did. I wrote a thesis on the dynamic process that gives rise to this not bell-shaped curve for incomes. And how it varies from year to year. And how an individual family travels through the income distribution. You know, there's a normal age thing. You get started at a lower income, get higher incomes as you get older. And so on. And that's the kind of thing I did. It was actually-- I didn't realize it at the time --but it was a very new thing to do. I don't think anybody had used those mathematical probability tools, stochastic process tools, in economics until then.

INTERVIEWER: Now, you were finishing up here your PhD when Paul Samuelson published his famous paper, *Fundamentals of Economic Analysis*. How aware were you of Paul Samuelson before MIT or his work?

SOLOW: I knew Samuelson's name and I had read some of his well known papers. I think that the *Foundations of Economic Analysis* came out just about the time I was finishing my graduate work. And I gobbled it up. There weren't many of us in that large crowd at Harvard who were doing this kind of mathematical theory. But those of us who were just grabbed it, and it was like nothing we'd ever seen before. The standard book, if you were, when I was a graduate student, if you were doing some mathematical economics was a book by an Englishman named John R Hicks called *Value and Capital* that had a mathematical appendix. So we learned a lot from that mathematical appendix. But the Samuelson foundations were so superior, so much clearer, so much better, so much deeper, in getting at the essence of economic theory that it made a big difference to my generation of people.

INTERVIEWER: What had you heard of MIT economics, or just MIT in general when you were a graduate student at Harvard?

SOLOW: Nothing! I knew absolutely nothing. I was totally ignorant about things like that. But in those days-- this would be like 1949. In those days it was not rare for someone like me who had finished his course work but had not yet finished the PhD thesis to be appointed an assistant professor in a decent department. But I had no idea what the decent departments were, or what to do. The only place I knew, the only academic place I knew was Harvard. Harvard wasn't much interested in me. It wasn't much interested in people who did theory mathematically. All I knew was that I had a telephone call or letter, I don't remember which, almost certainly from Harold Freeman at MIT asking if I might be interested in a job. You only had to ask me that question, and the answer was, "Yes." But I knew nothing about the place. I mean, I knew what it was. I knew there was this reputation of nerdy students and no girls. And all that. But I didn't know anything about it at all. But I sure was interested in the job, especially since I wasn't going to be offered anything worth having at Harvard.

So I went over to the interview. And I met a lot of the people. I suppose that must have been the first time I ever met Paul Samuelson face to face. I might have heard him give a talk or something like that. But I met Harold Freeman. I met Ralph Freeman who was the head of the department then. And I met a couple of the other faculty. And they seemed to be interested in me. I must have had very good recommendations from my teachers at Harvard. From Leontief, from Sidney Alexander, from any number of people. I remember that Ralph Freeman said to me, well what sort of salary were you thinking of? And, you know I'm very sophisticated. I said to him, this is part of the legend I guess. I said, what's the lowest salary you pay assistant professors? And he said \$4,400. And I said, that sounds pretty good to me. So I was hired for \$4,400 a year, which was more money than I had ever thought of in one place at one time. So I grabbed it.

And of course, at that time most of my teaching was in statistics. Because I said, I'd written this probabilistic thesis. Oh! This is part of the story. I had also put in, when I finished my exams at Harvard, I had put in for a Ford Foundation Thesis Writing Fellowship. They had them then. And if you won one of these, they would cover all your expenses for one year while you wrote a PhD thesis. And I wanted to do probability and statistics. Harvard had zilch in probability and statistics. There were one or two decent courses taught by a guy named Fred Mosteller a well known statistician, died a couple years ago. I had taken those. And he had done a reading course for me to teach me something. And then he said, look if you want to learn more statistics, you should go to some university that takes it seriously. And the two possibilities were Columbia and Berkeley. So I got this Ford Foundation Fellowship for a year. My wife got a job at the Federal Reserve Bank of New York for that year. And I decided to go to Columbia, spend a year at Columbia, which had an excellent mathematical statistics department.

And I spent half my time writing my thesis, and the other half of my time taking courses in mathematical statistics. But I had already got the job at MIT. I said I want to take my first year on unpaid leave. I have the finance, and I'm going to make a really good statistician out of myself. And they were willing to do that. So I went off to Columbia, did that just for a year. And when I came back and started teaching in the fall of 1950, that would be, most of my teaching was statistics. I taught elementary statistics for all of MIT, because MIT was almost as bad at statistics as Harvard. They couldn't be as bad, because Harvard was as bad as it gets. But MIT had little or nothing. And the elementary statistics courses that I taught were taken by undergraduate from all over the Institute. And some graduate students as well. And I also taught specialized graduate courses in statistical methods for the economics graduate students. And I only sort of gradually began shifting over to teach economics. I never lost my interest in it.

INTERVIEWER: What were your initial impressions about campus? How did it match your expectations? Was there anything that struck you about the campus, or the atmosphere when you arrived?

SOLOW: Well, of course it was very different from Harvard. Of course even Harvard is not like Williams or something like that. It's not your idyllic grass and rolling hills. But the main building at MIT, the great Mausoleum that creates a great impression, the first time you see it. I've never been in a place where you could walk from one end to the other indoors like that. I liked the students instantly. I thought they were just terrific. They were a little nerdish in those days. And there were almost no girls. But MIT students first of all had no affectations. They were who they were. They had an enormous appetite for work. And they were good humored about it.

I'll tell you something about the girl business. As the junior member of the department of course, I got all the scut work to do. That happens all the time. And one of my jobs was being the person an undergraduate came to see if they wanted to change their major. If they wanted to, in particular, come and be a major in the then-called Department of Economics and Social Science, because there was some psychology and there was some political science. And it was all wrapped up into this one department. But I was the new assistant professor, so anyone who wanted to major in course 14 came to see me. And there was a not large, but fairly steady flow of people like that.

It soon occurred to me that I was getting quite a few girls. And then it occurred to me that the girls all had names like Phillippa or Georgia or Wilhemina. Then I would discover typically that they were only children, and their fathers were MIT alumni. And then it became clear, the father says to himself, all I've got is this girl. But at least I can make an engineer out of her. And he'd ship her off to MIT. She was named Georgia or Phillipa, half as if she were a boy. And they'd ship her off, and the poor woman would be registered in mechanical or electrical or civil or what have you. By the time she got to the middle of her sophomore year, she'd say to herself, I don't have to do this. And what else could you do? If you didn't want to be an engineering or science major, the only thing you could do was come to Course 14. So these were very bright young women. But it's not like that anymore. It has changed. In a way, that's the biggest single change at MIT in those 50 years, the role of women in the student body.

INTERVIEWER: Where was your office?

SOLOW: I was in Building 14, which is the office building right back of Dewey Library. And I don't remember the number anymore, but I remember the office. It was right next to the elevator and stairwell. And across from Paul's office, across from Samuelson's office. And we shared a secretary, Gloria Wiggin. And that began our friendship.

INTERVIEWER: Who else was in the department when you arrived?

SOLOW: It was much smaller than it is now. Bob Bishop was in the department. And Cary Brown was in the department. And if I remember correctly Morris Adelman was in the department. And Ralph and Harold Freeman. Harold Freeman was the statistician. Harold Freeman had been the only teacher of statistics and practically the only teacher of statistics at MIT, before I got there. So once I got there, we were able to divide up to work. And he could stick to the things he was most interested in, which included industrial statistics, quality control, product testing and things of that sort. And I could do the kind of thing that had more application to science and to economics. And Ralph Freeman who was the head of the department and had been for a long time, was really, very, very good at it. He was very understanding, knowledgeable. Not so much about economics but knowledgeable about MIT. And what people needed. And he regarded it as his job to make the department function well and be happy. And he succeeded.

INTERVIEWER: You've already mentioned that you hadn't thought that there was any other job. Once you got the call from MIT, that there was no other job. Once you had started establishing yourself at MIT, and realizing the milieu that MIT was in, were you at all attracted to other departments elsewhere?

SOLOW: No. The simple truth of the matter is I loved being at MIT. I loved the department. These were the best people you could imagine. For much of our time at MIT we did most of our social life with my colleagues and their wives and families. There was no stuffiness in the department. There was no hierarchy in the department. everybody was friends with everybody else. Everybody was doing his work. We all had lunch together everyday because we enjoyed each other's company. And some of that I always think must be due to Samuelson. Because by that time, the early 1950s, Paul is obviously the leading economist in the United States or the world. With hardly any question. Everybody knows that. And many people in that position would insist on privileges, or at least even if they didn't insist they would somehow get around that they need privileges. But Paul's not like that at all. And if you have the best economist in the world in the department, and he's not being stuffy about anything it must be hard for anybody else to be stuffy.

So it was a very wonderful place to be. And I loved it. Hollis Chenery who was a graduate student with me at Harvard and someone I knew very well, and he was chief economist at the World Bank later. And a good friend all his life, a good friend of ours. And he came and did a visiting course at MIT sometime in the 1950s. He was on the Harvard faculty then. And we were told that when he returned he said, well I'll tell you that MIT may not be the best economics department. But it's sure the happiest economics department. And that was true. So yes, once I began publishing some serious economics I got offers from other departments. And I really had no interest. I never even bothered to go to Ralph Freeman and say I've been offered such and such a salary by the University of Chicago. Why don't you give me a raise? I never once did anything like that. The only thing I ever did was tell one university, yes I might like to join your faculty, but I have only one condition. You've got to move Paul at the same time. And that was not going to be possible either. So I never felt tempted at all to leave. Never.

INTERVIEWER: Now in 1956 and 1957, you published two fairly substantial important papers. One on steady state growth. And the other on the technical change in aggregate production functions. Maybe you could explain what the theory is behind that, and what they are?

SOLOW: Well if you think back, this is still a decade, 10 years after the end of the war. And one of the great phenomena in the world is the fact that after a lot of post-war hassle, the European economies are beginning to grow rapidly. The US did not really have a post-war recession as everyone feared. And is growing. But these economies are growing at different speeds. There is also the problem of the developing countries. Although that didn't concern me so much. But it was in the air. It was sort of one of the outstanding problems. How to account for, how to provide an explanation of the growth paths of a modern economy, and why some grow faster than others. What characteristics is it that do that?

And there were some attempts to work that out. And the best known were papers by an English economist named Roy Harrod and an American economist named Evsey Domar who later joined the MIT faculty, but was then I think working in Washington. He may have been working in Washington at the Federal Reserve Board or something like that. So the Harrod and Domar papers were what there was fundamentally. And it seemed to me that they couldn't be right. They couldn't have the thing right. Because one of the characteristics that came out from them was-- When I say the growth path, I mean washing out the fluctuations, the business cycles. Ignore them. Imagine they're not there. Or get rid of them statistically, or do something like that.

But the Harrod and Domar stories had the implication that that growth path was unstable. That a modern industrial capitalist economy would forever be on the verge of falling off that path. And if it fell off it would fall further and further. And the history of the modern world-- what little I knew of it --wasn't like that. The Depression of the 1930s that we talked about before was so important precisely because it was such an exception. It was not the routine thing. So it seemed to me that there had to be a way of modeling, of analyzing in simplified form, the growth of a modern industrial economy which wouldn't have that unfactual characteristic. Would look more like the economies we know, where there are deviations, occasionally bursts of rapid growth. Occasionally bursts of slow growth or even a slight decline. But there's a tendency to get back. So there's some stability to that growth path. And so I worked on that. And I found that essentially by dealing with technology differently from the way Harrod and Domar had done, and making more out of the possibility of substituting capital for labor, and labor for capital, you could get a better model. A model that would actually work, and not have the instability characteristic. And be usable. You could use it to interpret facts, data. And it turned out to be a good thing. It was wonderful for me. But it was a good model. I wrote that paper in 1956. It's now 50 years later, and it still gets cited in the journals. It's still the fundamental model of growth for an advanced economy, for a capitalist economy.

So I wrote that paper. It was nice. I knew that. I knew it was nice. But I didn't know it was going to be an important paper when I wrote it. But I published it in 1956. Then I got interested of course in how could we do this empirically? The whole purpose of this is to explain what's out there. Let's try to look at the history of the US. And I knew where to get data from the US such as it were. This was in the 1950s. I could go back to 1909, the census year of 1909, so I could have almost 50 years of data. And so I started working on that. How to make an empirical implementation of the model. And I discovered something that never entered my mind, which was that-- I had thought like everybody else, thought is maybe excessive. I had taken it for granted like everybody else, that the main thing that pushed an economy to grow was the increase in population and the accumulation of capital goods. The building of factories. The building up of the stock of machinery and equipment, and stuff like that.

And I found that if you looked at the data with all of the theory you could bring to bear, you could not make that story hold water. And the only way of accounting for what we'd seen in the US between 1909 and 1950-- something was if the main driving force for growth had been what I chose to call technical progress. Meaning what was in my mind was improving technology, but also included improving skills. It had to include building up the skills that are necessary for more advanced technology and so on. And so I devised a way of estimating the contribution of technological change-- meaning this broad concept of technological change --estimating its contribution to the growth of the US economy. And I found amazingly that something like 80 percent of the 20th century growth of the US economy had to be imputed to the advance of this broad concept of technical progress.

And that conclusion too has more or less stood up over the last 50 years. And this next thing was a desirable thing. I realized all the time that as you got more measurements, you could try to explain a little more, a little more, a little more, and leave less to be explained by technological progress. And that's happened. But it's still true, I would think, that today a good estimate of the fraction of aggregate economic growth in the US and probably most European countries as well, that has to be imputed to the advance of technology is of the order of 60- 70 percent. So I did that paper in 1957. And it's remarkable how long they've held up. And I get my name in the libraries, because one is called the Solow Model. And the technical progress story gave rise to something called the Solow Residual. So as long as there are library catalogs, the name is there.

And I was doing this work in 1955 and 1956. I was 31, 32- years- old. And I was having the time of my life. Because I hit on something that was obviously good. And every day opened up another direction in which to carry. But what I didn't know was that this would become a whole industry. The number of papers taking off from those two papers over the next 10, 20 years is very large.

INTERVIEWER: So you were already having the time of your life in the economics department in the late 50s?

SOLOW: And by the way, during this period the department is getting better and better. And this is important. I think what differentiated it from the other major economics departments was that we paid attention to our students. It was a very good place to be a student. Because for instance, I-- and I think I was typical --I never had formal office hours. Never. If I was there, any student was entitled to come in. If I was doing something and very busy, I'd say could you come back in 10 minutes or something like that. But for one thing, we had sort of evolved the principle that you did not buy off your time with research funds. If you were a member of the faculty of the MIT economics department, you taught. And it went. People took it for granted and did it.

Nobody wanted to, or certainly didn't try more than once to say oh, I have this research grant. I'd like to teach only half- time. Because that was looked down upon. And it was a very student-focused department. And that sort of thing gets around. And we got marvelous PhD students. We got the best there was I think because they knew that it wasn't a big bureaucracy. It was not a mill. You would be taught, not just lectured at, but taught. And that also made teaching some of those people another source of real pleasure.

Beginning I would say in the 1960s we had this influx of absolutely superb students. I had the wonderful experience a couple years ago that two of my PhD students won Nobel Prizes in economics. And not only that, they're friends of mine. They've been friends of mine ever since their student days . So this happy, cooperative, friendly sort of place mattered.

I have to say, I don't know whether this was a good thing to do or not. But when we made faculty appointments, we did think whether this was a person we'd like to have lunch with everyday. And if someone had a reputation as being a son of a bitch or a self-centered person, we were less likely to want to appoint that sort of person. And so it really was a good place. It still is I think.

INTERVIEWER: Now in 1961, you were awarded the John Bates Clark Award for your research. And for being under 40, as a young economist. Did that in anyway change the formalities or the expectation? Did the recognition launch you to the national or international scene, or change you at all?

SOLOW: No I don't think it did. I suppose it can't have done my reputation any harm. But I didn't think of that as anything that was going to change anything I did. Or anything I thought. I was in Washington that year at the Council of Economic Advisers. Which was another one in my time, in 61, 62, was another one of these small, happy, high morale, everybody's in this together organizations. I think that's the greatest thing in the world. And the Council was like that in those years. I don't know what it's like now.

But no, the Clark Medal was a nice thing, because all of the people who had won it before were people I admired and respected and liked. So I felt good about it, but that was not going to do anything to my life.

INTERVIEWER: You can tell me about what purpose you were serving on the Council of Economic Advisers, or how are you were appointed there?

SOLOW:

Oh, well when Kennedy was elected. I didn't play much of a role in the Kennedy campaign. Paul Samuelson did a lot. But I didn't. I was just doing my teaching and my work. But Kennedy was elected. And then, it probably would have been after he was elected, and he had named as the members, the chairman and the two other members of the Council of Economic Advisers, Walter Heller as the chairman and Jim Tobin of Yale as one of the members, and Kermit Gordon of Williams as another one of the members. I knew Tobin better than the others.

So one day, in December 1960 I suppose this is, we're home in Concord, where we live with our three children, and the phone rang. I think I was already in bed. The phone rang sometime after 11 o'clock at night. And I get out of bed and pad out to the kitchen, and pick up the phone, and it's Walter Heller and Jim Tobin and Kermit Gordon on the other end. And my first words were, what are you guys doing up this late? But they said they were planning the incoming Kennedy administration, and would I be willing to come to Washington to be on the staff of the Council of Economic Advisers. It's 1960, I'm 36- years- old. Yeah, 36- years- old. And I really had no particular desire to do that. I had just finished saying I was having the time of my life, why would I want to do anything else? So I talked to them for a bit. I said, you don't want me, I'm a theorist, not a practical person. And they said, well you can be the council's ivory tower theorist if you want. And so I said, well I'll think about it. And I went back to bed, and I said to my wife, I said who it was. And I said I need a job in Washington like I need a hole in my head. I remember the phrase exactly. And my wife said, you know, all during the Eisenhower administration, I've heard you complain about their economic policy. Why don't you put your money where your mouth is? I had no good answer.

So the next day I called the department. It might still have been Ralph Freeman as the chairman, I don't remember, and said what had happened, and asked if I could have a year's leave to go to Washington. And the answer was yes. So I called them up and said I'd go. And I went and arrived in January '61 when the new administration took office. And of course, after about a week, I said this ivory tower stuff is for the birds. I want to get in on the every day stuff. And so I did. I was the senior main staff person. And again it was a very non-hierarchical place. And I functioned for much of that time as if I was a member, not a staff person. That is as if Kennedy had appointed me. Because Walter wanted it that way. I remember, there was one time during that year, I was in my office in the Executive Office Building, and the phone rang. And my secretary came in and said, it's the Swiss Embassy. They want to know what your protocol rank is, because they want to invite you to a dinner. And I said, tell them that I am a full professor at MIT, and they don't have any rank that high in the government.

And I'd intended for her to tell them that. The secretary was too smart. She went across to Walter's office and said what had happened. Then Walter said tell the Swiss Embassy to treat them like me if I'm not there. And that's the kind of thing it was. Walter was the boss but it was a very good thing. And I was actually there in Washington for a year. I had one year's leave. But then the first year I was back at MIT, I would spend two, three days a week in Washington helping out, helping the group. Because I'd been involved in a lot of stuff and I wanted to finish it. And what it did for me was give me a dose of practical macroeconomics, policy making. On a day- to- day basis. Where something comes up that you have to think about and come to an opinion about. And back up the opinion. And do it by tomorrow or the next day or Monday, or something like that. And I learned to do that kind of thing. And I learned whole parts of economics I'd never been involved in.

For various reasons it was very important to do some good labor economics. We were dealing with what we thought was a very high unemployment rate, between six and seven percent. And there were arguments current in universities and in the congress that said you can't do anything about that. It's a kind of structural phenomenon that's built into the economy, and there's no policy we can do that will respond to it. And our gut feeling was that that probably wasn't right. But I had two or three months to give myself a crash course in the economics of the labor market, and come to an opinion. And convince my colleagues. And then be prepared to defend it within the executive branch and within Congress. And I kept that up. And that has stayed with me up until now. I still work occasionally, more than occasionally on the labor market. And I picked that up because that was just an assignment that came my way. So that was a very good time.

Kennedy was a wonderful president from the point of view of someone at the Council of Economic Advisers, because when he got a memo, he read it! And not only did he read it, but he wanted to understand it. He always would begin conversations by reminding whoever he was talking to that he got a C in economics when he was an undergraduate at Harvard. But he wanted to understand things. And Walter Heller did something that no chairman of the council had ever done before. And I doubt that anyone ever has since. When he sent a memo to the president, every memo was from him, from Walter Heller. But Walter would sort of put a little note at the bottom and say who it was who actually wrote this memo. So it wasn't common, but it was not a rare thing for my phone to ring or Art Ochen's phone to ring. He was the other main staff person. And the voice would say, the president wants to talk to you. And so you would drop what you were doing and say yes sir. And Kennedy would say, I'm reading this memo that I understand you wrote. And I've come to the second paragraph on page two, and there's this sentence and I don't understand it. Can you explain it to me? You know, you would give your life for someone who would actually want to know what you meant when you wrote something like that. So that was good! That was a fine experience. It stayed with me. And I came back to MIT loaded with economic questions that needed answers. And it provided me with a lot of things to do.

INTERVIEWER: When you were at MIT, you mentioned already that until the mid 1960s, it was the Department of Economics and Social Sciences. And then in the mid- 60s, the political science and psychology were spun off. How much did you interact with the psychologists or political scientists before that?

SOLOW: Somewhat, yes. I can only give you instances because there's no point in trying to judge percentages. But for instance Karl Deutsch was a political scientist in the department. And Karl was writing a book about ethnic minorities, and the politics of countries that have ethnic minorities, like the Swedes in Finland, the Swedish-speaking part. And he got interested in linguistic minorities. The Swedes in Finland aren't Swedes, but there is a large Swedish-speaking minority. It's often thought to be the most catered to minority in the whole wide world. So Karl Deutsch got interested in how languages spread. And what would determine whether the use of Swedish, or the use of a particular language in a multi-language country would rise or fall. And he had some ideas about that. And he was telling me about them once. And I said, you know, what you're describing sounds a lot like a bunch of differential equations. And he said oh yes? I said sure, do you want me to write some down for you? And I did. And I think there's an appendix in the book which gives the mathematical model that has roughly the kinds of results he was looking for.

So that's an interaction. And there were others. Alex Bavelas was a psychologist who was interested in small group behavior. And he had really interesting problems, and some of them had a statistical character. So I could talk to him, and I did over lunch or coffee or something like that. So there was non-trivial interaction. But I would say that mostly-- and it's perfectly natural --mostly the economists talked to each other. And the political science people to each other. But I don't think there was any friction at all. And there certainly was some collaboration. But the political scientists and the psychologists, they were the ethnic minority in the department. And I'm sure they were better off to have their own.

INTERVIEWER: Now throughout the 60s when you were building the powerhouse as it were, did you run into resistance for the Schools of Science and Engineering. They're always called the barrens?

SOLOW: At the start, you have to keep in mind that in the early years, and even extending into my early years, into the 50s, the economics department was a service department primarily. Very few people came, almost nobody, came to MIT as an undergraduate intending to major in economics. Maybe nobody. But the engineering departments wanted people to know some economics. They still do. They should. And I already mentioned that I taught an introductory statistics course for the whole Institute because there was nobody else to teach it. So it was a service department. We did have a PhD program. Small, but some very good students. They were attracted primarily by Samuelson, by Samuelson's name in the beginning.

And there was a period when I think the powers that be at MIT, I'm not so much thinking of the deans and the president, but the science departments and the engineering department, thought of economics as just a service department. If we had needs, they should fill them. And I don't know how it happened exactly, but I think it reflects very well on the kind of open institution MIT has always been in my time. That as we accumulated really good faculty, and the department had begun to have a reputation amongst economists, the big part, the major part of MIT got to understand that. And to my knowledge, I don't remember anyone ever digging in his heels, and saying come on, what are we spending money on an economics department for? When for the price of one economist we could get half a civil engineer or something like that. I don't remember that.

On the other hand, you really should talk to people who were department heads and see with them. I would urge you to get a hold of Bob Bishop who still has every one of his marbles. And he would have known. He would probably have a better idea than I. My picture is that once the department had genuine quality, really good faculty, MIT was prepared to experiment. Fortunately it's not an expensive department. It doesn't bring in a lot of research funds, that's true. And we never wanted to bring in a lot of research funds, because I at least felt that the teaching was the preeminent thing to do. So maybe from an administrator's point of view it's not such a cheap department, as I regard it. But we just improved ourselves. And as we improved ourselves, our role at MIT became better and better. So I think it worked out.

I really do think that MIT as an institution lives up to its self- image better than most places I know. The self- image is, if you've got an idea, if you have a good idea, we'll do our best to let you run with it. And that goes for departments as well. So I think it worked extremely well.

INTERVIEWER: In terms of building the faculty, so many notable scholars either ended up at MIT or at least spent considerable time at MIT, who would go on to win Nobel Prizes or other awards, like I've got Franco Modigliani or Scholes or Merton or McFadden or Engle. What was the common trait between all of them? Was it a shared background, a theoretical paradigm, or just brilliance?

SOLOW:

No, I don't think it was that. First of all, those in the group who had been students at MIT, like Bob Merton knew that they liked the place, that it was a good place to work, and it really was. And I think that its reputation as a good place to work is mostly what attracted people. Franco Modigliani knew Paul, he knew me. But he too, long after he'd come, he would say things like, even when I was at Carnegie Mellon, Carnegie Tech I guess it was then, I realized that I was really an MIT- type person. And the atmosphere of that department was different from any other economics department I ever knew. Primarily because there was no ideology. There was very little or no hierarchy. And everybody pitched in. And I think Franco was paying himself a compliment when he said that he was really an MIT- type person at heart. And I think that's the sort of thing that attracted them.

But it was certainly not a shared paradigm except, I mean the one thing that there is-- Well this is sort of interesting. I think that amongst American universities, MIT probably was the one that domesticated mathematical methods in economics. On the other hand, today and 20 years ago for that matter, 30 years ago, the mathematical methods permeated MIT but there were other departments around the country that pushed to more and more sophisticated mathematics. Aimed more and more at methodological purity. And we did less of that than others. The difference was that if you were teaching a course in international trade, you would make use of mathematical modeling where it was useful. Wouldn't have to be terribly sophisticated, mathematically sophisticated. In fact, it ought to be economically sophisticated. So while I think that our department was kind of the one that domesticated mathematical methods in economics; it was other departments that got fancy with it. And that may have helped attract some of people who didn't want to focus on that kind of very sophisticated, very delicate mathematical stuff. But wanted really to do economic modeling.

INTERVIEWER:

Now in a sort of ironic turn of events, in the 60s and 70s, Harvard's Department of Economics made a fairly concerted effort to recruit graduates as well as other faculty, and some would characterize it as raiding or poaching, that the fact that they--

SOLOW:

You betcha! And I resented it bitterly and complained about it a lot. You know I'm bragging now, so maybe it has to be discounted. I think we did a really good job of recruiting new faculty. We were willing to take a chance. We were willing to take young people who looked really very good. A lot of them of course had been our own students. We had the habit of never hiring our own PhDs directly after their PhD. We thought that would lead to inbreeding of a bad kind. So all of our best PhDs had to go and get a job somewhere else. But once they'd been away for two or three years, we felt perfectly free about trying to recruit them back if we wanted to. And we had a lot of luck that way. And a lot of people in the department even now, are of that vintage.

But I think we did it a terrific job because we were willing to take chances. We had enough confidence in our own judgment of how good somebody would be. And I was pissed off when Harvard, which has lots of money, would then try to pick off the people from our faculty. And offer them more money than we could afford. And-- and this bugged the hell out of me --a lower teaching load than our people did. You got a call, you don't even have to move. You're in the same community. And here's this great university that is willing to pay you more than you're getting now to do less teaching. And it's very tempting. And I think it speaks a lot that we lost almost nobody to Harvard. In fact we tried, Paul and I in particular, to inculcate the view that you're not a man until you've turned down an offer from Harvard University.

But I resented that. I still resent it. It still happens occasionally. In fact, just a couple of years ago, two years ago, Larry Summers, who should not have done it, let Harvard try to recruit three people from MIT, from our department. And one of them went. And two of them said no thank you. So I don't like that. And I have sometimes insulted my Harvard friends by telling them that in no uncertain terms. That what they should do is get a little imagination, and a little intelligence, and do their own recruiting.

INTERVIEWER: You have perhaps one of the most famous quotes popularly in economics, when you said, everything reminds Milton of the money supply. Well everything reminds me of sex, but I keep it out of the paper. Maybe you can explain what the background of that comment was?

SOLOW: Yeah, well it was actually spur of the moment to tell you the truth. There was a conference at the University of Chicago, I think on whether informal wage controls might help stave off inflation. The Council of Economic Advisers in my time had written a famous report about wage guidelines. What constituted good, average wage behavior, from the point of view of preserving high employment and staving off inflation. And we were not trying to get wage controls at all, but we did want to push this idea. And the University of Chicago-- which is a very laissez-faire kind of institution; it has very strong ideology, along those lines--ran this conference which hovered around this topic.

And I think I gave one of the papers. And I had been instrumental, I'd been part of the group that had worked this out at the Council of Economic Advisers. So I did the paper. And Milton Friedman was the discussant, was the commentator on it. Milton Friedman's particular thing was a doctrine roughly called Monetarism, which-- I'm over simplifying this but it doesn't matter --held that the only possible source of inflation is excessive increase in the money supply. And the only possible cure for inflation, the only possible, feasible or desirable cure for inflation, is not to have excess growth of the money supply.

So Milton was the commentator on my paper. And of course, I could have written the comment for him. Because anyone, not only I, anyone would have known more or less exactly what he would say. Somehow he got from the content of my paper to the money supply, and then talked for the rest of the time about the money supply. So it came my turn to respond, and I opened my mouth and that's what came out. I hadn't thought about it for a minute in advance, but I was just listening to Milton and thinking about what he had said. And so what I was trying to say was, whatever you're talking about, Milton will talk about the money supply. So it came out that way.

INTERVIEWER: What kinds of other interactions or relationships did you have with other prominent economists? I know that Kenneth Galbraith and you had a falling out of sorts for a number of years but--

SOLOW: Yes. Well, first let me say I was a friend of Milton Friedman's. We got along well. We knew we disagreed with each other intensely. We spent a year, the academic year 1957 and 58 at a place called the Center for Advanced Study in the Behavioral Sciences in Palo Alto, attached to Stanford. It's sort of a very nice place where you get an office, and there are 40 or 50 people there from all sorts of fields, but behavioral science is the label. And they're all on leave, and they're all doing their own work. And Milton was there that year, as well as I. And as was another University of Chicago economist named George Stigler who also became a very dear friend of mine. George Stigler was also a very conservative anti-public policy sort of person.

But we were all friends. In fact I carpoled with Milton that year. So five mornings a week for a whole academic year, we would drive from the neighborhood where we lived to this place, arguing all the way. And then when we got out of the car, we'd continue the discussion for another few minutes. And so it went on and on and on. But we were perfectly good friends. And George Stigler was not particularly a Monetarist, but was a very conservative economist in his own way. He and I were extremely close friends. Bobby and I played bridge with George and his wife. And I knew his kids. I still know his older son who teaches statistics at University of Chicago now. So it's perfectly possible to be very friendly, have real affection, and still disagree intensely.

Ken Galbraith was a different matter, which also worked out reasonably well. I wrote a review of a book of his called *The New Industrial State*. And it was a very negative review. I did not like the book. I thought the book was all wrong. But I did something that Ken Galbraith disliked very much. I wrote a humorous review. I made sort of jokes. Now that's his stock in trade. Galbraith does this kind of writing all the time, making jokes and making fun of people. And I made fun of his book. And he was genuinely angry, so angry that he immediately wrote a reply which is fine, but he wanted it to appear in the same issue as the review. Obviously they had sent a copy of the review to him before it was in print. Or maybe I had sent it to him, as a matter of course. He said, I want my reply to appear in the same issue. And the editor, said no we can't do that, because it's already ready to go. It would be terribly expensive. And Galbraith said I'll pay for it!

And he did. He paid the extra print costs from shoving this in at the last minute. And I got to write a short rejoinder. And he was very, very angry. And it lasted for quite a long time. But on most current policy issues, we tend to be more nearly on the same side. It was just the analysis that I objected to. And it wore off eventually. And we got to be on good terms again, to such an extent I have to say, that when he died a year or so ago, his sons asked me to speak at the memorial service, which I did. But I suppose it was partly my fault. But you know, the guy makes fun of everybody else. Why shouldn't you make fun of him? I did.

INTERVIEWER: Many labels have always been applied to you. You're a Keynesian or you're a Centrist, or you're Neoclassical. What accuracy, what usefulness do these kinds of labels have?

SOLOW: None at all. None at all. The plain fact is that I am what is generally called a Keynesian for a certain class of problems. And I am generally what is called a Neoclassical economist for another class of problems. And I don't think that there is any conflict there. And it's a label that I don't-- I just think that kind of labeling doesn't make a lot of sense. I'm a Keynesian in the short run, and a Neoclassical economist in the long run. Although it's generally thought that those are two sets with no overlap. But I am. And I said short run and long one. There is a real intellectual problem about the medium run, about the periods which are a little too long for the normal Keynesian assumptions to be right, but maybe a little too short for the normal Neoclassical assumptions to be the right assumptions to make. And how to deal with that medium run is as far as I'm concerned, an unsolved problem by me or by anybody else. But I think that kind of labeling just doesn't make a lot of good sense. I wish people would talk about particular ideas rather than attaching them to people.

INTERVIEWER: Have you seen this sort of labeling increase in recent years? Or that the importance placed upon adhering to a label increased? Either in the policy or the academic realms?

SOLOW: No, I don't think it has increased, but I don't think it has decreased.

INTERVIEWER: Okay. But it was still prevalent when you were younger?

SOLOW: It was still prevalent then. And it still goes on now. The labels have changed slightly. But the same kind of thing happens in economics. And it has a bad consequence. One of the consequences is that people do not take seriously the work of the other ideology. If you identify with school A, then you don't pay serious attention to what people in school B do, and vice versa. And that's too bad.

INTERVIEWER: In the 1970s, 1980s, there was diminishing federal support for graduate education and universities in general. And there was increasing dependence upon fellowship support, the department increased substantially. And maybe you could talk about what the kind of physical conditions of the department were, and the kind of problems or concerns of the department in the 70s and 80s.

SOLOW: Actually, I do think that the economics departments probably suffered less from that than the science and engineering departments, because apart from decent computers, there's no expensive laboratory stuff to be done, and that's a big help. Because we were, by the 1980s, clearly established as sort of the best department in the country, and had very, very good people, we were able to get adequate research funds out of NSF, and a couple of other agencies, because we'd hardly ever needed a lot, needed a big chunk. Never had to buy an expensive spectrometer or anything like that. And the members of the department, faculty-- life must have been a lot harder then especially for the junior people. And that I think is true, that the senior people who had quite established reputations could almost always get the relatively small sums as these things go that they need for their research.

Remember, we never went in for buying time during the school year. Never had to raise funds to pay salaries. I'm sure that the engineering departments and the science departments think that was a ghastly thing. We shouldn't have been allowed to do that. But the focus on teaching was such that we really didn't want that to happen. So that's another way in which we saved money, at the expense of the institution. MIT had to pay the salaries for all of these people. But I think that we managed to survive, the senior people did, without too much difficulty. For junior people it was very difficult getting research funds.

The tradition in economics again is very different from the tradition in engineering and science. One of the advantages in fundraising I suppose that junior people in science and engineering have, is that they are associated with a lab or a center which has a couple of big shots, large caliber cannon at the head of it. And the money flows to the lab or the group, and some of it gets down to the senior people. The tradition in economics is not that way. If I wanted a grant from the NSF, I might have a graduate student research assistant to pay for, but never an assistant professor, a junior faculty. Junior faculty was expected to fend for itself. And they try.

INTERVIEWER: Now you were named Institute Professor in 1973. Did that come as a great surprise to you?

SOLOW: It came as a surprise. I don't think anyone had told me in advance that it was going to happen. Yeah, so I guess it was a big surprise. And I loved it, because in fact that was a very important thing for me, because it came not from the department, not from economists, but from the Institute as a whole. And I took a lot of pleasure in that. I didn't, however use it to diminish my teaching at all. An Institute Professor can-- in fact that's part of the point of the rank --can sort of do what he or she wants. Teach, not teach, teach across departments and what not. I didn't want to diminish my teaching. So I just kept doing exactly what I had done before. But I had the pleasure of having that kind of honor from the whole of the faculty. And that meant a lot.

INTERVIEWER: Now tell me about the process of you being informed that you'd won the Nobel Prize.

SOLOW:

Well, I'm sure typical of anybody. There's a six- hour time difference between Stockholm and Boston. And I think they make this announcement at noon or 11 o'clock in the morning in Stockholm, and so it's five or six o'clock in the morning here. So what happened is we were asleep in our bed, and the phone rings. You're too young, but this is 1987. I'm 63- years- old. If the phone rings at five o'clock in the morning, your first thought is uh oh, something's happened to one of our children. So I got out of bed and answered the phone. And I recognized-- I won't say recognized, but the voice on the other end introduced itself. It was a Swedish voice. And it was a guy I had actually met sometime. And so he said, I'm so and so. I'm the secretary of the Swedish Academy. I'm calling to ask if you would be willing to accept the Nobel Prize in Economics next-- this is October --in December, whenever they do it. And I had no difficulty saying yes, thank you. And then I must have uttered the stupidest thing I ever said in my life. I said to my wife, let's go back to sleep. Because there was no chance. Then the phone started ringing everywhere. The phone just rang and rang. And by six or six thirty, people were knocking on the door and that was very exciting. And it is very exciting.

I also got the best piece of wonderfully timed advice I ever got in my life, finally in one of these phone calls in the morning. I knew there was going to be a press thing at MIT at 10 o'clock or 11 o'clock. One of the phone calls was my son John, who is an economist and who teaches at the University of Iowa. And he was born in '54, so in 1987 he was 33- years- old. So he called and said he heard the news, and he thought it was a great thing. And his parting words were, "Try not to say anything stupid about the stock market." So not only was it a good warning, but it gave me the perfect out. Anytime anyone asked me a question about the stock market, I could say, well I'd really like to tell you, but my son's advice was to try not to say anything stupid about the stock market, so I won't. And there was a press conference at MIT. And Paul Gray was there. And Jerry Wiesner and a lot of my colleagues. And it was really very nice.

And that does change your life. Not always for the good actually. The way it changes your life is first of all, I decided right away or very soon, that I was not going to accept any invitations for money. Of which there were plenty. But I would accept invitations from worthy causes. I never dreamed how many worthy causes there are. So I found myself doing a lot of things that I've never done before, that are time consuming. But I couldn't think of any good reason not to do it. And amongst other things that people want to do is they'll interview you or ask you to write something about subjects that you know nothing about. Somehow the notion that if you've got a Nobel Prize in economics, you're entitled to be opinionated about every subject including physics. And I presume that people with Nobel Prizes in Physics feel entitled-- or it's believed that they're entitled to be opinionated about economics. And that's not a good thing. And I used to say that there isn't anything I can say that's so stupid that a magazine won't print it. And it's true!

And it does it in a way take an awful lot of time away from research, from actually doing your job. I never let it interfere with the teaching, but I got less work done after I won the prize than I did before. Well, I was in my 60s, so I probably wasn't going to do that much good anyhow. But that is a big change. And they're very practiced at that in Stockholm. The week or 10 days in Stockholm in December when they do this is really as close to royalty as a normal human being ever gets. Just very good and very nice. They make you feel good.

INTERVIEWER: Was it something you had been expecting deep down based on other laureates and their work?

SOLOW: Not really. I had no reason to think. There were other people who in my mind were just as worthy of that kind of thing as I. In fact it was a little embarrassing because, you know, I'd be chatting with friends and they would say, you're bound to be the next Nobel Prize in economics. And I'd say, well that would be very nice. But maybe it didn't happen that year. And it's a little embarrassing. You feel guilty for all your friends expecting you to do this, and you don't. But I thought in 1987 that I could name at least half a dozen people who would be as worthy of that as I. And why should I-- But I guess I knew that I had done work that was in that league. I certainly did know that.

INTERVIEWER: I just want to move on to some issues at MIT. And some reflection on your prospectus. One that the Institute often has grappled with is what role do the humanities, arts and social sciences have within an institute of technology that emphasizes engineering and science. So you had come to the Institute in 1949, 1950, right after the Lewis Report was published, which founded the School of Humanities. Maybe you can describe or give your perspective on what role the humanities have at MIT.

SOLOW: Well, it varies. And I think this reflects in some ways credit on MIT and in some ways not. MIT has been very good at targets of opportunity. In a way purely by accident, it developed an excellent economics department. If I had to say why, it was because Harvard University never hired Paul Samuelson and never hired me until we had already been at MIT for a long time and had established ourselves. And I think particularly if Paul hadn't come at the very beginning-- Paul was at MIT before the Second World War really --it could've gone entirely differently. But never mind. There we were. We created a really good department. And MIT could have buried it by negligence. But it didn't. It realized that there was something good here, and it's worth cultivating. Even if nobody thinks about MIT as a center for economics. By now people do. But in those days they didn't. But the administration and the faculty at large was willing to give it a try.

Same thing with linguistics. We had found ourselves at MIT by accident with a top- notch linguistics department. The accident was the government was trying to find a technology for the machine translation of language. Could you translate from German to Spanish or English to Russian through a computer, by a machine? And they had a large project on the machine translation of language. It failed. They never and still can't do it very well. And certainly in the 1950s and '60s, couldn't do it at all. It failed. But there they were at MIT, a bunch of really good linguists, Noam Chomsky, Morris Halle and some others. And the Institute was willing and able to say, all right we didn't ask for this collection of people, but now that we've got it, let's go with it. And created what is one of the world's leading Departments of Linguistics. It's a lot harder to do that with English literature, because there are too many established Departments of English Literature around the country. It would be very difficult to create a world- class English literature department at MIT. You could spend enough money but it probably wouldn't pay off. And it's very, very unlikely that anyone by accident is going to accumulate the makings of an English literature department of really world- class people. We have very good people in those departments, but you need that mass of them. And the accident didn't occur.

So I think that Tech does as well as circumstances permit, combining these two things. It understands that you do not want to, even if you could, turn out engineering graduates and science graduates who don't know anything else. They can do that in English universities because the secondary schools teach to a much higher level in the last few years. But for MIT it's very important that everybody have some experience that you can actually do good intellectual work on a literary subject, or on film, or on a social science.

INTERVIEWER: That leads into my panel question. It's that what sort of opportunity cost if any does a requirement that MIT students who are technological innovators be required to take classes in the humanities?

SOLOW: I don't think that the opportunity cost in that sense is large at all. The notion that if an undergraduate at MIT spent the hours that he or she spends on humanities, arts, social science subjects, doing aeronautical engineering, they would be much better aeronautical engineers, I simply think that's false. My experience is first of all that the last little bit of an undergraduate education does not add that much in the major. Does not add that much to graduate work. You make those really first class scientists and engineers in the graduate school. And by exposing the undergraduates to--

It's not so much that they should read some novels and be able at a dinner party or something to talk about novels. I think what's extremely important is to realize that there is good, hard intellectual work to be done on subjects other than science and engineering. I think that's really very important, and it probably makes for a better all around scientist, engineer. Certainly I think it adds more than is subtracted from having one or two less subjects in aeronautical or chemistry or something like that.

INTERVIEWER: I also wanted to ask, what are your thoughts about the epiphany that it seems that Wall Street finance firms have had about recruiting MIT students. In recent years, as many as 50 percent of the graduating class has gone off into finance or consulting. Is that an efficient allocation?

SOLOW: Well I'm supposed to believe that if that's what the market does, then it must be efficient. I'm less sure than that. But the uncertainty I have that that's a good idea relates not particularly to the MIT students or their training, but to Wall Street. We like to think, and we should think of the capital markets, of the securities markets-- let's call it Wall Street --as that part of the economy that allocates capital efficiently, that makes sure that the savings of the US and what we borrow from foreigners, a large amount these days, gets allocated to the right industries and the right places and right activities. And the capital market does that. But an awful lot of the activity of--

Oh and another thing that the capital market does. And this is something where all those MIT students with their ability to do mathematical modeling and computational modeling in particular. That's where it pays off. We have learned what's called financial engineering, to take bundles of securities and create new securities, so you can separate out the risk and peddle it separately if you want. You can sort of take some activities that are uncertain and involve risk. And by financial engineering, hive off the risky part, and sell it to someone who wants risk. And then have the less or non-risky part, sell that to someone who wants a low return without the risk. And that's something that MIT people are very good at doing. Not necessarily because they learned to do it at MIT, but because they have the mindset of an engineer, or physicist. Less so, chemistry or biology of course. To do that sort of thing.

The reason why I wonder whether it's a good allocation is that the other thing that the financial services industry does is create opportunities to speculate. And while some speculation is probably necessary-- by speculation I mean simply betting on a rising price, or falling price. While some speculation is probably necessary for an economy to allocate risk efficiently, some of it I think has no economic function at all. Doesn't make the economy as a whole richer, but transfers income from people who are bad at it to people who are good at it. And that I think is less useful to the economy as a whole. And I'm sure that MIT graduates contribute to that too. But of all the problems of American society, the fact that Wall Street hires so many MIT graduates is very far down on my list.

INTERVIEWER: Walter Rosenbluth once characterized the years between 1950 and 1990 as Camelot at MIT, because you had these leaders, with Stratton, and Killian, and Johnson and Wiesner and Gray. There was this continuity and predictability in MIT's leadership. Maybe you can share your experience that you had with some of these presidents, or with the administration?

SOLOW: Well, what I will say is that I and my colleagues-- and I don't mean only my economics colleagues --but the people in the other departments, in other schools that I knew around MIT. I've been around there a long time, and I know a lot of people. We-- I and they --always had the feeling that the MIT administration had our interests in mind. And that there was no pyramid building. No self-aggrandizement. None of that.

Jay Stratton once was in the process of meeting with every department in the Institute. And when he came around to meet with the economics department, he-- at some point, maybe near the beginning, I don't remember --he said what is it that we could do for you? And Bob Bishop said, I'd rather you thought about that than we thought about it. And what he was saying was, you think of what you can do. We're not asking for anything. You just decide. And you could feel safe saying that to Jay Stratton. Or to any one of the people you named. That is a hell of a collection of people, from Killian up to Susan Hockfield as far as I'm concerned. They are as trustworthy as you can possibly imagine. And that has meant an awful lot to faculty people like me. That you can do your work and be as sure as you're ever going to be, that if you do good work it'll be rewarded. There won't be somebody or some group that's favored, that cuts into what should be yours. And I'm sure Jay Stratton went back home and thought well, what could I do to be useful for the economics department? That's just fine.

I mention in passing that there was a time when Jim Killian and Jay Stratton and Howard Johnson and Jerry Wiesner and Paul Gray were all in Cambridge at the same time. And they got along extremely well.

INTERVIEWER: So we were talking about the continuity of presidents that we had certainly during your tenure. And I wondered if you perceived that there were any costs or benefits associated with that "inbreeding". Because many MIT departments discourage it. Yet this culture of MIT lifers, people who start out as undergraduate and become graduates and faculty is sort of celebrated at the same time.

SOLOW: Well, like everything in the world it has pluses and minuses. I think we've done very well. Of that group, the one name I didn't mention was Chuck Vest. And Chuck of course did come from outside MIT, as did Susan Hockfield. But that long continuity, on the whole I think it did the place a lot of good. Looking back with the benefits of hindsight, I don't think that we missed anything. The Institute now as a whole, missed anything that an outsider might have brought. And the fact that-- I know this sounds sort of fatuous --I do think that the MIT culture is a really good one. And I said that it's one of the few cultures that actually lives up to its own ideas. And I think it helps that this whole line of successful presidents were steeped in it. Knew about it. And were able to carry it on. On the other hand, I have to say that Chuck Vest-- I was the co-chairman of the committee that found Chuck Vest. We had a Faculty Committee, of which I was the chairman, and a Corporation Committee of which Carl Mueller was the chairman. And Carl and I liked each other so much that the two committees never met separately. They only met as one committee. And so I'm sort of pleased that Chuck Vest was such a wonderful president as I think he was. And at least we got the character right. He may not have spent his life at MIT, but he fit in very well. And I think it's going to work out beautifully with Susan as well.

INTERVIEWER: Now you never served as a department head or a chairman or any kind of administrative post like that. Was that out of a choice that you never wanted to be in that sort of position?

SOLOW: Well it's hard to say. I never wanted to be. I couldn't administer my way out of a paper bag. One of the things you want about a department chairman is that the faculty can go to sleep every night knowing that everything that needs to be done is going to be done. I'm afraid if I was the department head it might not be such a good idea. Now whether the department sensed that. But the truth is that if I had to do it I could do it. And I would do it well. I would rather not have. I don't take much pleasure in administering. And I was doing good work. And so nobody ever put any pressure on me. If anyone had put pressure on me, I would have said sure, it's my duty. And I'll do it. But they were nice enough not to.

INTERVIEWER: Now throughout the 1960s, certainly towards the end of the '60s, how did you see sentiment and the culture of the student body change or evolve, especially with regard to social movements?

SOLOW: Well, during the uproars around the Vietnam War in 1968 and '69 and thereabouts, MIT was a little less affected than other universities. I guess because science and engineering students were less involved in the Anti-War Movement and things like that. On the other hand, there was plenty of uproar around MIT. And I can remember there were two brothers. They weren't twins, although they looked rather alike, named Roger and Peter Bohmer, who had both been students of economics, undergraduate students of economics. And they were both deeply involved in the student movement and the student revolution if you want to call it that. Both. And there was a time and occasion, probably in the late '60s or early '70s when there was a big demonstration that took place around the Sloan Building and the Hermann Building next door.

And it was so chaotic that the members of the faculty were standing around in sensitive places trying to make sure that nobody broke into any place where they shouldn't break in. And I was doing that kind of duty too. And I happened to run into Peter Bohmer who was one of the student leaders. And we had always gotten along when he was a student. We got to chatting with one another, and I said where's Roger? And he said, well Roger is here too. He's part of this revolt. And I said yeah, I'd like to say hello to Roger. And Peter said oh no. He said Roger's too off the wall. I don't want you to talk to him!

And there was a little bit of that always. I'll tell you, here is a good concluding story. Because we should stop. We're going to get to that on time. At the time of what I think was called the Cambodian Incursion, there was a lot of feeling amongst the students, including the graduate students in economics. And I must say a lot of us sympathized with them generally. But the graduate students and some of the faculty thought, instead of going on strike or doing something useless like that, we conceived the plan that the next morning we wouldn't hold classes. But instead, as many graduate students in economics and as many of the faculty as wanted to join them, would all meet in Central Square. And we would bring brooms and brushes and plastic bags. And we would clean Central Square.

We'd just go from one end of Central Square to the other. Cross the street and then come back. And do the whole thing. And clean up every bit of trash in the street. And stuff it into our plastic bags. And the idea was this would attract a lot of attention. And people would come out of the bars in Central Square and the cafes and restaurants and stores. And then they'd ask us what we were doing, and we'd say that we were really so disturbed by what our government had done in Cambodia that we couldn't see carrying on the normal university life. But we thought we might as well do something constructive, so we decided to clean up Central Square. And we hoped to get these ordinary Central Square people involved in a conversation about what we thought was wrong with the war.

So the next morning I don't know, maybe 30 students and half a dozen the faculty including me, showed up and we started sweeping up Central Square. And it worked. People kept coming out of store fronts and asking us what was going on. And we'd explain what I just explained, and tell them why we thought what our government was doing in Vietnam was wrong. And so on. The whole thing worked beautifully. At the end, at about five o'clock, we're packing up, getting ready to go. And along comes the first Cambridge policeman who had been by, and he's looking for trouble. And some of our students are prepared to give him trouble. One of them. I won't mention the name. A woman. Excellent graduate student, has since risen to very important positions in the government and universities, was in particular starting to give some lip back to this cop. And I could see that this beautiful day that had worked so well was going to end badly. This cop was going to get annoyed, and he was going to arrest us.

And all I could think of, I did. I picked her up. The student fortunately hardly weighed anything. And I threw her over my shoulder and walked into the subway entrance, carrying her. So the conversation came to an end. And there are little things like that that made it seem less-- what happened at MIT bore no relation at all to sort of thing that went on at Columbia or Berkeley of course or at Harvard even.

So the students felt it. The faculty felt it. But whether it's from the kind of training they had, their interests are just less radicalized, or whether they liked MIT enough not really to want to disturb it, I don't know. But it worked out better.

INTERVIEWER: Just a few more questions.

SOLOW: Well listen. One or two more, but no more. Because I do want to get you on that five o'clock.

INTERVIEWER: Okay. In the 1980s and 1990s, MIT staked out positions that were ethically and morally justifiable, but socially and politically they were fairly controversial. Whether it was in regards to technology transfer in Japan, financial aid, the anti-trust suit, the report on women faculty and discrimination, the student housing and mental health. Do you believe that these lessons were at all influenced by what MIT learned in the 1960s and '70s, or just MIT's general philosophy?

SOLOW: I doubt that it had much to do with the '60s and '70s, but I do think it had a lot to do with the general philosophy and atmosphere around MIT. I think that Chuck Vest's decision to fight that anti-trust case was admirable. But it's just the sort of thing that an institution that feels comfortable in its own skin would do. And by the way, I have talked to other university presidents whom I happen to know for one reason or another, who regret bitterly that they didn't do what Chuck did. And they think that Chuck did the right thing. And I don't know if they didn't have the courage or it hadn't occurred to them or I don't know what. But I don't think that the '60s or '70s, I don't think that that period has much of a hangover. I think it's mostly the institution itself.

INTERVIEWER: Recently there's been an emergence of these new or integrated or interdisciplinary fields in bioengineering or neuroscience or information technology or nanotechnology. And there's been a corresponding decline in sort of traditional engineering and physical science subjects. Is this something that an institution like MIT should be concerned with? It is just a natural ecology of ideas?

SOLOW: No, I think it's the naturally ecology of ideas. My guess is. I'm speaking now as an outsider to those particular things. But my guess is it's exactly what MIT wants. It's MIT's comparative advantage to those things, because it is and has been for 60 years I've known, more flexible than any university I know.

INTERVIEWER: Alright, final question. What about MIT is the same as other universities? What sets MIT apart? Why haven't other schools been able to imitate MIT's successes? Or should MIT be concerned about other schools closing the gap?

SOLOW: Well, now I have to speak about economics. The rest of the American universities have gained on MIT economics. And I take that as inevitable. And who knows? Today maybe Princeton is as good as MIT. It could be. It's staffed by a lot of our PhDs there. But I think that's the way things go. I think that imitation is the sincerest form of flattery. And you always fight to keep ahead. But you have to recognize that other people are going to try to catch up. They will imitate when they can. They will do better if they can. They will try to outguess and come up with the wave of the next future. And that's the way life is. And you win some and you lose some. And I think MIT has won an awful lot.

I liked it in a certain way when we were clearly the best economics department around by a substantial margin. And everybody knew it. And that was a good feeling. But that sort of thing can't last and shouldn't last.

INTERVIEWER: All right.

SOLOW: Okay.

INTERVIEWER: Thank you. Interview ends at 4 o'clock.