INTERVIEWER: Marvin Minsky, when did the idea of artificial intelligence present itself to you as a young person growing up?

MINSKY: I think the first time I started to think about that was when I was an undergraduate at Harvard and I was looking through Widener Library and I ran across a big thick book called 'Mathematical Biophysics'. And now we're talking the late 1940's and there weren't such words around. I opened it and it was full of strange little articles edited by a prodigious guy, Nicolas Rashevsky, at the University of Illinois, I think, maybe Chicago. And it had chapters about theories of how cells might divide and how populations grow. And maybe 40 or 50 little chapters. And one of the chapters was about simulated neural networks by McCulloch and Pitts in 1941.

And I had been curious about psychology because-- that's a long story, but I was trying to decide what to do and one thing that seemed interesting to do was mathematics. But there were other people who were good at mathematics, very good, and in mathematics there's no point in being second best because it's different from other fields. And I was interested in biology and there seemed to be people pretty good at that.

And chemistry, I had a professor, Louis Fieser, and it looked like that was under control. And then there was psychology and as far as I could see there wasn't anyone good at that except maybe Sigmund Freud 50 years before. But what to do about it because people didn't seem to have any theories of how thinking worked. And here was this strange paper with ideas that in fact were completely new about finite state machines and things like that. And I got very excited.

INTERVIEWER: Is it fair to say that it that moment you glimpsed an enormous field that would eventually open up and become an almost uncountable number of disciplines.

MINSKY: I don't think I saw where this field could go, but I had been reading psychology and I did see that nobody had a theory, for example, about how learning works. They had philosophical theories: well you have ideas and somehow they get connected and when something happens you make a new idea and you put it somewhere in your mind and later you fish it out. And no one had any good theory-- excuse me-- there didn't seem to be any theories of how this knowledge could be represented or retrieved or how you could rub two of them together and get a third one. And I could see in McCulloch-Pitts ideas, which were just little switches connected to each other-- that there was a way that perhaps information or knowledge could be represented. And the paper was in three parts. And I couldn't understand the third one. And after a long time I decided that whatever it was, it must be wrong.

And that's very important because that gives you something to do. And I worked on various ways to fix it and I couldn't. It finally got fixed much later in 1956 by a mathematician named Stephen Kleene who also read the paper and said this doesn't seem right and he knew exactly what to do about it.

And incidentally, his theory of how to represent finite state machines in switches and so forth was exactly the same as another theory that two professors-- I'm trying to remember their names-- at MIT had made for calculating the impedance of a complicated electrical circuit.

And I don't know if anyone had noticed that but these two theories-- one is called Regular Expressions by Stephen Kleene and his theory was so nice and elegant that it's used in all search engines today almost exactly the form that he invented. And the Mason and Zimmerman-- I think were the professors-- and the signal flow graphs are used everywhere for calculating how electrical circuits would work. But those are 10 years apart in my experience. Interesting to see two theories in completely different fields that are exactly the same.

INTERVIEWER: In your childhood growing up-- let's just step back for a bit-- what encouraged you to think that you were capable solving problems or at least seeing insights into existing problems that no one else saw? What kinds of experiences did you have as a child?

MINSKY: I think when I was a child I didn't have the feeling that I could solve problems that other people could solve. On the contrary, I found things were quite difficult. And when I tried to read mathematics it would take an hour a page and I'd get some of the ideas but not others. And usually it would be six months later that suddenly it would click. And so I think I thought of myself as sort of slow. On the other hand, I thought of everyone else as incredibly slow. But I didn't think of myself as particularly creative.

And it's just-- but I never grew up in some sense. And as far as I can tell, I've been getting better at things slowly and steadily. It was only when I was older that I noticed that most people work on something, they do something wonderful, and then they get stuck. And I started to make theories of why do people get stuck and how to avoid it. And the best thing is if you've done something, you should be ashamed of it instead of proud of it. And I notice a lot of people keep saying, well I thought of that a long time ago and that sort of thing and they keep trying to get recognition and why bother?

INTERVIEWER: You went to Bronx Science and Andover and then you went to Harvard?

MINSKY: Right

INTERVIEWER: Which of those institutions was more liberating in terms of your being able to think broadly about subjects as opposed to just learning what was given to you?

MINSKY: I think I was incredibly lucky all my life because when I went in grade school I had interesting teachers. And there was a school for unusual students in New York then opened up called the Speyer School. And it had lots of unusual children. And then we-- and so I in that environment I was with people who were more or less my own age but who knew a lot. And I've had the good fortune always to be in that situation by one accident or maybe my father's clever planning or something. And then I went to Fieldston school in New York-- Ethical Culture School, which was a wonderfully smart place with extraordinary teachers. In fourth grade I had somebody who saw that I had read a chemistry book and he gave me the laboratory, and I was allowed to synthesize things and so forth. And I had some friends there who I'm still in contact with because they were good thinkers.

Then their High School of Science was a miraculous place because most of the high school teachers were PhD refugees. Because we're talking about the early 1940s, and all the smart people in the world who didn't get killed came to America, as far as I could see. In fact, when I got to Harvard the first thing was, gee these kids are not nearly as smart as the ones at the High School of Science. And they're finding these courses hard and it seemed like a step back. Andover, I was there just for a year and I had a calculus teacher who was also the wrestling teacher and that was pretty good. But most of the year was exceedingly-- and a great english teacher, Dudley Fitts, who translated plays from Greek and back-- but the kids were mostly jocks of various sorts.

But then Harvard again was a world of good fortune because I met a great mathematician shortly after I got there named Andrew Gleason. And I didn't know it at the time, or maybe he didn't either, but he was one of the greatest mathematicians in the world. And the math department had 10 or 20 people, each of whom had created some field and so forth. I met a great psychologist, young assistant professor named George Miller, who is now recognized as one of the pioneers of cognitive psychology. And when I met him, he knew I had read this McCulloch-Pitts paper and he said he couldn't understand chapter three also. And I said, well don't worry about it, it looks like it's wrong.

And we became great friends because no one else could-- and in fact, when I did invent learning machine, George Miller-- first he gave me a laboratory in the psychology department when I was an undergraduate. And then he got money from the Air Force or somewhere to actually build this machine so forth. So when I was an undergraduate I actually had a couple of laboratories. Another professor, Welsh, John Welsh, when I told them I was interested in neurons, he gave me a big laboratory because Harvard had just built this gigantic new biology building with hundreds of rooms. And it had been designed with some foresight so that it was more than twice as large as anyone needed. And I happened to go there and say, I would like to do some experiments.

Somebody said, well here's this suite of rooms. Which included a black photographic laboratory where you could go in and experience sensory deprivation. And all sorts of equipment. And so I got to-- I was interested in the inhibitory nerve of the crayfish claw. It turns out this is a wonderful animal. Welsh recommended it because the nerves in this thing are so big that you can see them. And if you have a magnifying glass, you can really see them. And you can move them around with the tweezers and connect them to your alligator clamp. And here you're doing neurophysiology with a screwdriver. And the crayfish doesn't seem to mind having it claw snipped off because it snaps off. It has a detachable joint. And it sits in its tank for the rest of the year and grows another one. So I didn't feel any ethical problems about this wonderful animal.

INTERVIEWER: Did some of that work-- did some of that work motivate your development of optical devices?

MINSKY: Well maybe the most important experience I had was meeting Warren McCulloch. And Warren McCulloch was a philosopher and physiologist and great poet. I think maybe a hundred years from now, he will be seen as one of the great philosophers of the 20th century. Right now, he was sort of well known in the early days of cybernetics, Norbert Wiener and gadgets. But he's been forgotten. And he would look at a problem and think of some new way to do something with it.

And one thing he was I believe the first person to invent circuits that would work if you break any part of them. How do you make a reliable circuit that's redundant enough that it will correct some errors at least? And he wasn't good at mathematics but he just worked out all the possible simple examples of this and found one which was self-repairing. Did many things like that. And I followed him around for couple of years and I think the reason I developed so well in this field is that I didn't listen so much to what Andrew Gleason said when he showed me how to prove some theorem or what McCulloch said about his particular theory but I was always asking, how did he think of that? And sometimes I'd ask him and he'd tell me some wrong theory because nobody knows how they think of things.

But the idea that what students should learn from their teachers is how they work, not the subject they're teaching. And I think it was just a great accident that I encountered this Warren McCulloch who is interested in that as much as-- he was interested in how he would think and tried to explain it.

And even today, which is 40 years later, sometimes when I'm stuck writing something, for example, I could hear his voice. He's saying, oh that's too pretentious or that's not pretentious enough. I think I've accumulated a cloud of these people. There are four or five people that I worked with for several years and whenever I'm stuck, I can hear Oliver Selfridge or Dick Feynman or Andy Gleason saying, oh you're wasting your time, why didn't you look at it this way? It's almost as though I'd made little copies of these guys.

INTERVIEWER: So there's a Richard Feynman in your brain?

MINSKY: There's a little Richard Feynman and there's a Theodore Sturgeon, the science fiction writer who I tracked around with. Because his science wasn't very good but his intuitions about it were good, and he could write these wonderful things and create these images. And I just wanted to know how to do that. Never got very good at it but I can sometimes say, what would Richard Sturgeon say or what would Isaac Asimov say? I have about ten of these characters that I can exploit.

INTERVIEWER: What did you want to learn from Richard Feynman?

MINSKY: How he got such good examples of things and then made theories.

INTERVIEWER: What period did you work with him?

MINSKY: I think I had met him in the '70s, early '70s. It was interesting. I was traveling around Los Angeles with a friend of mine, Edward Fredkin, who was a incredibly innovative thinker. Ed has discovered all sorts of little theories of things. He started the first company that did image processing and word processing and that sort of thing. And he was one of three or four people I've known who we're never afraid to do something. Normally when you say let's do this or that, somebody will say, well that would be very hard and so forth. But there are three or four people that I've known-- John Mccarthy, Oliver Selfridge, Feynman-- if you think of doing something he'll say, let's do it. And then we'll do it the next day or right away. And usually get anything done-- to get anything done-- you have to convince a lot of people and make a plan and so forth. But I've worked with a few of these people who say, well if that's a good idea maybe, we can do it tonight instead of next year. So until the 1980's, I never wrote a proposal. I just was always in the environment where there would be somebody like Jerry Wiesner of MIT.

John Mccarthy and I had started working on artificial intelligence in about 1958, or '59, when we both came to MIT. And we had a couple of students working on it. And Jerry Wiesner came by once and said, how are you doing? And we said, we're doing fine, but it would be nice if we could support three or four more graduate students. And he said, well go over and see Henry Zimmerman-- or I think it Zimmerman-- and say I said that he should give you a lab. And two days later we had this little lab of three or four rooms. And a large pile of money which IBM had given to MIT for the advancement of computer science and nobody knew what to do with it. So they gave it to us. And--

INTERVIEWER: Not a bad move.

MINSKY: Right. And for many years that kept happening. We'd think of something to do and-- I had a great teacher in college, Joe Licklider. Licklider and Miller were assistant professors when I was an undergraduate. And we did a lot of little experiments together. And then about 1962, I think, Licklider went to Washington to start a new advanced research project that was called ARPA. Just started up. And he said to them, well there some people at MIT who have all sorts of nice ideas. One of our students have built a good robot, another had built a machine that showed promise of doing some good vision research, Larry Roberts. And then Larry Roberts also had this idea of an intranet. There were a few-- I mean that I idea came from several people. But Licklider got them him and Ivan Sutherland to come to Washington to help run this department. So now I was in this situation that Licklider had sent a big budget to MIT to do time sharing. Which had been invented by John McCarthy and Ed Fredkin and a few other people. Also it'd been invented in England about the same time.

INTERVIEWER: Time sharing computing that was different than batch processing?

MINSKY: Yes, using a computer with multiple terminals. In fact, we went to visit IBM about that. and we went to Bell Labs also to suggest that they be working on that. And the research director at IBM thought that was a really bad idea. We explained the idea, which is that each time somebody presses a key on a terminal it would interrupt the program that the computer was running and jump over to switch over to the program that was not running for this particular person. And if you had 10 people typing on these terminals at five or 10 characters a second that would mean the poor computer was being interrupted 100 times per second to switch programs.

And this research director said, well why would you want to do that? We would say, well it take six months to develop a program because you run a batch and then it doesn't work. And you get the results back and you see it stopped at instruction 94. And you figure out why. And then you punch a new deck of cards and put it in and the next day you try again. Whereas with time sharing you could correct it-- you could change this instruction right now and try it again. And so in one day you could do 50 of these instead of 100 days. And he said, well that's terrible. Why don't people just think more carefully and write the program so they're not full of bugs?

And so IBM didn't get into time sharing until after MIT successfully made such systems. And years later I had a sudden flash of really what was bothering this research director. I think he said, well if somebody interrupted me 100 times per second, I would never get anything done. But--

INTERVIEWER: Identifying an important difference between humans and computers that IBM apparently didn't fully grasp.

MINSKY: Right. And making computers easier to use I suppose wasn't in their business interest anyway. Making them solve problems faster, that is. I'm sure making them easy to use was. But that came from-- none of the large companies ever did very much for computers. It was all hackers here and there and their ideas gradually filtered up.

INTERVIEWER: Two questions. You described that your calculus teacher at Andover was also wrestling coach?

MINSKY: Yes.

INTERVIEWER: Did you try out for the wrestling team?

MINSKY: I was in wrestling class and it was surprisingly interesting. But one day-- in fact I got to be where I thought was pretty good-- but I was in the class of people from some weight up to 137. And then one day they weighed us and he said, well you weigh 138 so now you have to be in this class-- the next class-- which is from 138 to 147 or something. And then I was the worst one. And I decided there was nothing to this skill.

And generally I developed an attitude toward sports which is that there's absolutely no point to it. The people who are good at it are maybe 2% faster in some reflexes and they're a lot slower in others like worrying about whether they're going to get hurt. And there's just no point. And when I see 20,000 in a stadium watching them, my first thought or last thought is, why don't they hire one very good critic to watch them? What's all-- it's a waste of time to have 20,000 people having mediocre thoughts about it. Why don't they just hire someone who will evaluate it. And basketball is the best example because it isn't even statistically significant. If you see a score like 103 to 97, that's less than one sigma and one shouldn't regard that as a victory at all. So it's very unscientific.

INTERVIEWER: Right. And I think it's safe to say these ideas here are probably unlikely to catch on in any big way.

MINSKY: Well eventually they'll catch on, but it might be a couple of hundred years before the culture has adapted.

INTERVIEWER: Describe how you came to MIT. What motivated you or what reputation about MIT made this place a home for you?

MINSKY: Oh. Well when-- as I said, I went to Harvard as an undergraduate. And it was wonderful. And I had a neurology lab and a psychology lab and that was great. Most students never got that. I happened to be at the right place at the right time. And I was majoring in physics for awhile and my grades weren't particularly good. So I thought I should make up for that by writing a good thesis. And it turned out you couldn't write a thesis in physics. They just didn't have a bachelor's thesis. So Gleason said-- my mathematician friend-- said, why don't you just switch to the math department? You can write a thesis there. So I wrote a nice these about fixed points on spheres. It was pretty exotic. I sort of-- there was an unsolved problem and I solved half of it. Got some really striking results.

INTERVIEWER: What was the problem?

MINSKY: The problem was: it was known that if you have three points on a sphere-- supposed you have a sphere. It's like the earth, it has an altitude. It turns out that if you take three points around the equator, equally spaced, there's some place you can put them where the altitude of all three points will be equal. In other words, if you had a three--

Well, that's the theorem. It was proved by a Professor Kakutani at Yale. But it was only true for three points on a great circle. And it seemed to me this ought to be true for any triangle. That is, you ought to be able to put it somewhere and rotate it so that-- like a three-legged stool-- so it would stand straight up. I couldn't quite prove that, but I proved that it was true for several different shapes of triangles. And then I got stuck and a couple of years later, this strange person I never heard of named Freeman Dyson proved the thing in general. And he sent me this paper and I didn't believe anyone could possibly be that smart. Because it was just so strange.

But anyway, Gleason said I should just switch to major in mathematics. And I could write a thesis and I did. And then I said, this is-- and I was a senior-- so I said, well I'd like to stay here. And Gleason said, no. You should not stay here. You've been here four years and you've observed a lot of what we have to teach you here and now you must go to Princeton. So I felt very rejected. It turns out Princeton was the other place that had the other half of the great-- well, they were all over the place, but Princeton had another full set of great mathematicians who had-- Sure. Von Neumann. Von Neumann who became my thesis adviser. Not quite my thesis adviser. But anyway, I felt rejected but I said, well, okay. And I went to Princeton.

INTERVIEWER: Godel.

MINSKY: There was Princeton and Godel. I had lunch with Godel once. He was wearing gloves because he was afraid of germs. Einstein, who I couldn't understand because I wasn't used to German accents. Anyway, that was a great place. And then I met Oliver Selfridge who was this pioneering researcher who had known McCulloch and Pitts. In fact, he had been Walter Pitts' roommate. And he was at Lincoln Lab and he invited me to join his research group there who were inventing all sorts of things. and then I got a message from the MIT math department inviting me to come and be a professor. So all this happened. I was just being pushed around. I never actually made any long range plans or applied for anything.

INTERVIEWER: I suspect that when future historians look at the significance of your work, random is probably not a word they would use to describe your achievements and the places you've gone and the things that you've done, even though you sort of naughtily describe it is as random.

MINSKY: Well I was always trying to find the simplest way to solve a problem. And I think that could lead anywhere because nobody knows what the simplest solution is.

INTERVIEWER: What was the opportunity at MIT when you arrived in the math department. What did you see needed to be done, and were there rooms and labs? You described what Wiesner got you there right off the bat, but what sort of things were yours for the taking?

MINSKY: Well, of course they did. When I arrived at MIT, McCarthy I think had been there for a year. And he already had laid the groundwork for catching students and potentially good mathematicians and perfecting them into being computer science. Computer science was just growing in the-- when you're talking about 1960 weren't very many series at all. And today I'd say that computer science is a whole new area science that was never even imagined for except by a few pioneers like Godel and Turing and Post. A handful of people had had visions that there would be something like mathematics for complicated processes. Mathematics itself is really only good for very simple things. Because if you have 10 or 20 equations of different times, there's nothing you can do. What you can do is take one or two equations and study them very thoroughly and build great towers of theories about those things. But if there are ten different things interacting mathematics is helpless. Computers are helpless at understanding them but in some sense you can-- they let you experiment with things you could never do by hand or in your head. And so then you could discover phenomena and then simplify things down to see, well where does this new phenomenon come from? And what part of the system caused it? And progress comes from taking a complicated thing with behavior you can't understand gradually breaking it down. Of course some things no one's ever broken down, and we don't understand them.

INTERVIEWER: Is it fair to say in the beginning before your artificial intelligence lab got started that sort of high road of mathematics was algebra and the sort of abstract complex mathematics geometry? And that computer science was view it as more of an applied science, more of an engineering kind of low-road?

MINSKY: Yes. Applied mathematics was not very-- was not filled with so many great ideas as pure mathematics, which took very simple sets of axioms or assumptions and build huge towers. In fact when I was in graduate school the most exciting thing in the world was this-- to me-- was this field called algebraic topology.

Topology which is the principals of geometry where you don't actually care about the shapes of things but just the properties of the shapes. Like are all the parts connected in a simple way or are there holes in it or is it twisted or things like that. And strangely, the hardest problem-- it was a hundred year old problem practically-- was this: suppose you have a plain two dimensional surface and you draw a curve that never crosses itself but it closes. So in topology that's considered a circle because you just care about how it's connected and you don't care about it's shape.

Well everybody knows that if you do that then there's exactly one inside and one outside it divides the plane into these two sets of things and not three and no one had ever proved that. The first proof was around 1935. So this was called the Jordan curve theorem. I'm not sure whether Jordan had the first solution or was the first one to state the problem quickly. And it's sort of obvious that it's true. And the reason why it's hard isn't it what if the curve isn't really smooth but it wiggles a lot. If it wiggles an infinite number of times before it gets here, maybe there could be some little part of the plane that you sort of almost outlined it. That's a very strange-- there is a-- in three dimensions there is a strange phenomenon that I think they're called Antoine sets. There was a mathematician named Antoine who discovered this very simple example. Imagine a regular chain-- bicycle chain-- not a bicycle. Yeah.

Anyway, you have a link like this. So here's two links. So now let's have another link and another link and that's called a change. Now close it by putting a last link so now are you have a ring of links. Now if you put a string through the middle of that, you can't get it out. There's nothing stopping it. But you bump into this chain and well you could try to push it through but that wouldn't help.

And so here's an Antoine set. Each of these links does not divide space up very much. But it does have the property that, if something's going through it, you can't get it out. Because it hits the walls. Now here's this chain where there are a lot of links, and none of them are touching each other. So that shouldn't make much difference, should it, if they're not touching each other. And yet, if you put a string through-- if you had one link here and a string you could just go around it. With this Antoine chain, a regular chain is closed, if you have a string you still can't get it out. But it's not being stop by any particular link. I mean, it hits this link so you go here.

So what's that? Somehow this said in three dimensions it's dividing space up in some queer way. And there's nothing like it in two dimensions. Two dimensions if you have a-- you can't have a lot of little circles room because you can go around them. In three dimensions you can. In four dimensions it's much more complicated and nobody knows what happens really. So I got fascinated with that.

And this is a long story but after awhile I finally understood a proof by a Czech named Cech-- C-e-c-h-- of the Jordan curve theorem. And of all the experiences I can remember in mathematics, my feeling of accomplishment was greater of understanding Cech's proof than anything I ever proved myself. It is a strange thing, but it was that this proof occurs in a sort of infinite space of things all messing around. And step by step he shows that something happens.

And so here's a case-- it's like appreciating a Shakespeare play when you can't write your own, but you still might say, oh I understood something about this play that nobody else did or something like that. It's not that I created it. So that's always bothered me that if I do something myself and other people admire it, I regard it, well I couldn't have done it unless it was obvious. So that's another way not to get stuck. If you have a theory and it turns out to be wrong, great. Now is a chance to do something better. And you run into people who don't like their theories being proved wrong.

INTERVIEWER: Not a problem for you.

MINSKY: Well, no, because then now you've got another problem to solve. Can I make a theory that includes this and the exceptions to it and so forth.

INTERVIEWER: How did-- you mentioned that Wiesner was the sort of impetuous behind the formation of your lab McCarthy. What happened in the beginning and how did that become a mission to really specify what's going on in the human brain?

MINSKY: Well when I built this learning machine-- that had really started before-- this was a machine that made connections between things. If you would give it a little problem-- usually the problem was something like a little rat, simulated rat, in a maze and if it managed to make the right turns to get the cheese, then you reward the machine. And it changed the probability that it would take the-- it would increase the probability of taking the same paths again. And so it did in fact learn to solve some simple problems. But after awhile, I could see that it wasn't going to understand how it solved them. So it couldn't-- what was missing is it could accumulate conditioned reflexes all right, but it couldn't say, oh I've learned a lot about this and I still haven't done this and what's the reason?

And I might have gotten stuck with this machine for a long time except another friend of mine named Ray Solomonoff had found a different theory of how to take a bunch of data and make good generalizations from it. Because you can think of any theory of learning as saying you've had a lot of experiences is there a sort of higher level, simple thing that they're all examples of. And Ray Solomonoff invented this other way of making generalizations. And I instantly said this is much better than everything the psychologists have done since Pavlov and Watson and so forth in 1900. And so suddenly seeing this new idea of Ray Solomonoff-- which also occurred to a Russian named Kolmogorov about the same time and later independently a guy named Gregory Chaitin in I think he was in Argentina-- but when I first saw this new idea by Ray Solomonoff I got the idea that everything in psychology was too low level and couldn't handle the right kinds of abstractions. Ray Solomonoff's theory said if something happens you must make a lot of descriptions of it and see which description is shortest because it must have the most significant abstractions in it. Like if you could have a short description that gives us the same results as a long one it must be better or whatever. And over the last 40 years that-- because I think that discovery was around 1957--

INTERVIEWER: So from the beginning the search for artificial intelligence was also a search to define processes that were fundamentally in psychology.

MINSKY: Yes, right. Why do descriptions work? And how do you make descriptions? And if something happens, in the old psychology you would somehow make some very crude representation, an image, a record of exactly what happened and connected to another one. And that might work to explain how maybe some fish or simple mammals think, but it doesn't explain how you could think about what you've recently been thinking. And the-- so although I was very deeply involved in trying to improve reinforcement theory and the traditional statistical theories of psychology, almost the moment I saw Solomonoff's idea realized all this stuff could never get anywhere. And that was the reason why my learning machine couldn't transfer what it had learned from this maze to another maze that was similar and so forth.

INTERVIEWER: What kind of machines did you work with in those early days to do your experiments and to demonstrate your results?

MINSKY: Well in the early days there were relays and vacuum tubes. And you could build almost anything out of relays although it was slow. Because Claude Shannon had published a master's thesis in I think 1947 giving-- Shannon's was a remarkable discovery. He made two major discoveries, each of which started a whole new field and solved almost all the problems in it. So in 1947 I think it was, he published this master's thesis about switching circuits. And nothing much has happened since then. That was the whole thing. And then 1950 he published this theory of the amount of number of symbols it takes to represent some information. And for about 10 years people worked on various aspects of theories and found better proofs for the ones in Shannon's 1950. But essentially he had solved all the important problems.

INTERVIEWER: So vacuum tubes, relays?

MINSKY: Right. And the SNARC machine-- this neural analog reinforcement machine-- it had about 400 vacuum tubes and about a couple of hundred relays and a bicycle chain. So that when something happened and you want to increase a probability, a little motor would turn on and a bicycle train would turn a volume control. It was all electromechanical. You could now write a description of such a machine with a hundred computer instructions I suppose and make it run a billion times faster.

INTERVIEWER: What was the biggest technological breakthrough in those early years that allows you much deeper access to the kinds of questions that maybe you were limited in exploring with the early computers?

MINSKY: I think a very simple one, namely the invention of the language Lisp-- L-i-s-p-- by John McCarthy. Lisp is a language-- computer language-- where there are only about eight or nine basic instructions. But these instructions are arranged in the structure called a List. And most of the instructions are on how to change a List. So in this language, you can write a computer program. And then you can write another computer program that will edit and modify the first one.

Now we have languages-- the popular languages today are still--- Lisp is 1960 I would say. The great languages today are to a fairly large extent pre-1960. Because they can't understand their own instruct-- it's hard to write a program in C that can understand a C program and say, oh if this happens I should modify the program to do this or that. Now you can in fact do that. And people become so expert at this that they can write Lisp-like programs in C or Java or these other programs. But it's hard. And I shouldn't knock them because they're easier to learn to be good, to write complicated programs with.

But the big change was going from thinking of a program as a sequence of commands to thinking of a program as structure sitting in the computer that another program can manipulate. So this made it possible in principle to make a program that could even think about itself. Now no one actually did that much. And that's what I'm still trying to start: a project which has programs that mostly spend their time thinking about why those programs themselves succeeded or failed to do something else and have access to a lot of advice about how can I change myself to be better doing. And this is just not catching on right now and I'm having trouble convincing other people to go in that direction.

INTERVIEWER: Yeah. You're stuck for the moment.

MINSKY: Right. But I think I'll write a book about something else until they get ready for it.

INTERVIEWER: Your book, 'Society of Mind' presents a matter for the operation of the brain that is fundamentally different from traditional psychology. What is that? MINKSY: Well most of traditional psychology tried to imitate physics and physics had a wonderful modus operandi. You see a phenomenon, what should you do? And one thing you could do is find the simplest sets of laws that would predict that.

And for example Ptolemy tried to explain the behavior of planets. And he said, well they were going almost in circles it seems but they're not quite circles. So what could we do? We could-- maybe we could combine a little circle that's turning faster with a big circle. And so if the planet looks like it's a circle only it's going a little too far here and there, let's add another circle which is added to the first one but it goes happens fast. So then it'll bulge out here. So that was called epicycles. And the ancients use that too-- this idea by saying well everything's made of circles but their circles are rotating at different speeds and different sizes. And it takes quite a few circles to match the description of a planet, planet's motion.

And Kepler discovered that you could do it much better with just one ellipse. Ellipse is slightly more complicated than a circle because it's like it has two radii rather than one. So it's little worse. And that made a tremendous difference because that explained the behavior of planets to great precision. Eventually you discovered that the orbit of Mars is a little bit affected by the orbit of Jupiter so it's not quite an ellipse, and Newton discovered the right law which was even simpler. Which is that planets attract each other with a force that's one over the squares the distance times the mass.

And Newton discovered three laws which did almost everything for mechanics. It wasn't-- it explained everything except electricity. And Maxwell added a couple more laws-- four more laws-- so now we had seven laws. And Einstein discovered that Maxwell laws could be reduced to much less. And so physics ends up in Einstein's time with about five laws. And then things got worse because quantum mechanics was just being discovered-- partly Einstein's fault.

INTERVIEWER: Psychology was attempting to mirror--

MINSKY: Psychology was-- I called it physics envy in honor of Freud. Psychology said, well we've got to find four or five simple laws that explain learning. And if you look at old psychology textbooks everybody has a little set of laws. Like the most obvious phenomena that everybody observed is if you ask a person to remember a list of ten items, you say I went to the store and I bought some soda and cabbage and spinach and chicken wings and so forth. If you say 10 of those and you say, what did I buy? The other person will say, well I know that you bought cabbage and a soda and chicken wings and they won't remember the ones in between.

So people made up a law of recency which says that you remember the most recent thing most. And then they made up a law of primacy which is you remember the first thing most. And maybe there's another law which is you remember the loudest thing most. And for many years psychologists tried to imitate Newton to get a small handful of laws to explain how memory works or perception works or this or that. There was one honest psychologist named Hall who did the same thing, only he got 120 laws.

INTERVIEWER: How do you develop an artificial intelligence slash psychology theory that doesn't have physics envy?

MINSKY: Oh well, yes, the last paragraph would have been-- so this Occam's razor or finding the simplest theory worked tremendously well in physics and it worked pretty well in some other sciences. But in psychology it does nothing but harm. And a simple reason is that we already know that the brain has 300 or 400 different kinds of machinery. And they're each somewhat different. We know how three or four of them work like a little bit about the visual system and the cerebellum. But we don't know how any of the dozens of brain centers work in the frontal lobe and the parietal cortex and the language areas and so forth. But one thing we can be sure of is the brain wouldn't have 400 different kinds of machines unless they were specialized and behave in different ways and do different things.

So 'The Society of Mind' starts out pretty much in the opposite way and says, let's take all those things or a lot of the things that people do and try to find simple explanations for how each of those could be done. And then let's not try to find a simple machine that does all of those but let's try to find some way that a few hundred of these different things could be organized so that the whole thing would work. And in fact the book didn't get-- my first book, 'The Society of Mind'-- didn't come up with a good theory of that. So it's basically what we call a bottom-up theory which takes a lot of things and different phenomena and explains them in different ways.

INTERVIEWER: You know it was a bottom up theory-- how do you figure out how these work?

MINSKY: Right. So 'The Society of Mind' had four or five ideas about how these would be organized. But most of the book is-- it has I think great many good ideas about how different aspects of thinking work, but it doesn't have a picture of how they're organized. And the new book, which took another 20 years, is top-down.

And it says that, let's start by imagining that the mind has a lot of different things it can do-- resources like a room full of different computers-- then which should you use and when? And the answer is that-- or the answer I proposed-- is that you have to have some goals. So there's a chapter on what goals are and how they work. And that chapter comes from some research in the early 1960's by my colleagues Alan Newell and Herbert Simon.

In fact, if I can interrupt myself, for many years there were just two major centers of research in artificial intelligence. One was the group with McCarthy and me and later Papert at MIT. And the other was Simon and Newell at Carnegie Mellon. And they also had a crowd of students who produced great little theories. And their students went other places and the field spread.

Anyway, one thing that Newell and Simon did was to develop a theory of how would a machine have a goal? And their answer was-- it's sort of like the kind of feedback things that Norbert Wiener described in cybernetics, but it's different. In order for a machine to have a goal, it has to have some kind of picture or representation or description of a future situation. Now you'd say informally and it would like to have that situation. But like is no good, because that's burying -- that's what you're trying to explain. However, what it could have is a machine that also has a description of what you have now and this other description of what you would "want". And it find differences between these and eliminates them. So having an active goal is to have a description of a future situation and the present situation. And then a process which does things to make them the same by changing the present situation. Now another thing you might do is change your goal of the future one and that's if you're really uncomfortable and that you want something and can't have it then you could always just change, edit the goal and settle for something less. But that's another story.

So the new book is a little bit centered around this 1960 idea of Newell and Simon which I called a difference engine. And it says, in order to accomplish anything, the brain has to be full of difference engines of various sorts. In other words, there's no point in just reacting to things. You have to react to things in order to reduce a difference that you don't. Like if you're hungry you have to get food or reduce the absence of food. So you could always express things this way.

Okay. So, that's a simple picture of an animal. It has a set of goals, namely these difference reducing machines. And has to have some machinery to turn some goals on and off. So whatever those are, let's not worry about them because they would have-- whatever they are, they evolved because the creatures who didn't have them died out. Animals that lost the urge to eat, it doesn't matter what else they think, they'll go away. So what's the next step? Well the next step is what if you-- then you need another mechanism which says, well I've been pursuing this goal for a long time and nothing happened. So I call that a critic. The critic says, there's something wrong-- something has gone wrong with what you're doing.

The best example is, I kept doing things and I didn't achieve a goal. So what should I do? I should change my strategy. Of course I could change the goal too. And sometimes we do that. But the main idea is to think of the next level of the mind as a bunch of critics which are watching what's going on and looking for failure. If it's a success it's not important. Then you do something at low level psychology to make that more likely to happen. That's trivial. Obviously you want to learn what was successful, but it's not profound. However, if it fails, you have to do something really good. Namely do something new instead of-- so traditional learning is how to do something old again. That's why I discarded it in the 1960's because Solomonoff suggested another way.

So the top level of the new book called 'The Emotion Machine' is that the important thing that neurologists should look for are these things called critics which fire off when some effort to achieve a goal fails. And what should you do? Well you should think in a different way. You might change some of the goals or at least sub-goals and then you have the chance to succeed again. So that's the other idea. And that becomes a very rich idea because that leads you to ask, what kind of goals do people have? And there's two answers to that. One is that evolution provides the vitals ones like if you don't have-- if you don't drink enough, you'll die and so forth.

INTERVIEWER: And also ideas like consciousness become manifestations of these very complex critics interacting and reaching some sort of threshold.

MINSKY: Right because suppose some critics don't work either. Then you want to hire one which says what's wrong with the critics I've been using. And that's means you have to start thinking about your recent thoughts. You stop thinking about what you did and you think about how you were thinking. And to me the word consciousness is the name for-- there is no such thing as consciousness, but there's dozens of processes that involve memories of what you've been recently doing and new ways to represent things and describing things in language rather than images and so forth.

And in fact the existence of the word consciousness is the main reason why psychology-- you can ask why is Aristotle writing, and William James writing just as good as popular writing today in psychology? If you read William James you say, oh, he's better than this guy who's telling you how to think. If you look at Aristotle, you'll see his discussion of ethics is just as good is this President's ethics adviser and so forth. It's because they got stuck thinking that these words like consciousness and ethics are things rather than big complicated structures that might have a better explanation. So anyway, we have five or six levels of thinking. And the higher level is one where you make models of your whole self but they're simplified. And you say, what would happen if I did this without actually doing it. Stuff like that. Those are the things we call consciousness and I think there's about 50 of them.

INTERVIEWER: Let's talk a little bit about the span of time that you were at- you've been at MIT. It spans the late 1950's to the present day. Think for a moment about how students have changed in that time. But before you answer, tell me a little bit about the beginnings of the Media Lab and how you got involved?

MINSKY: That's in the middle of all this.

INTERVIEWER: Right.

MINSKY: Well when I came to MIT, the first thing is that it's hard for people today to imagine what it was like to be in a golden age. World War II was over. If you were a kid who wanted to learn about electronics, you could go to something called a surplus store-- which I did excessively-- and you could buy a gadget that today would have cost half a million dollars, full of parts and gears and take it apart and rearrange it. There are no surplus stores today. If there were, they would have these integrated circuit things that you can't understand and take apart. And you'd have parts with 80 pins or 200 pins. A computer processor is just intractable.

So first of all we had free laboratories. Also, we had companies like Heathkit which for a very small price would sell you the parts to make an oscilloscope. So tens of thousands of young people were making their own laboratories. What else about the golden age? Well, there were these great professors who came from Europe and in fact filled high schools.

Okay, when I got to MIT the wars were over-- the new one was about to start, I suppose, but that's another story-- and the universities were expanding. MIT was growing. It never-- it decided not to grow much. MIT has 4,000 undergraduates and it's going to stay that way. And Caltech has 1,000. Boston University has 50,000. So some universities grew. But what happened at MIT was that the faculty grew. So there were more graduate students than undergraduates. And there are more laboratories then you can imagine. So every student at MIT, if they want, can be in a laboratory. It's heaven. If you want to do something, the world is open to you.

And now, as an assistant professor, here I am. And I have the smartest people in the world, as far as I could tell-- I got some names. Ray Kurzweil wrote me from high school and he became one of the great adventures of the century. Gerry Sussman I knew, Danny Hillis from high school. These were kids who wrote and said, I hear you're working on making thinking machines. So I didn't do anything. I was just there in the right place. And the world was beginning to hear about cybernetics and AI and so forth. And the right people just came. And as I said before, Jerry Wiesner kept getting money and Larry Roberts and Joe Licklider. Every time we needed something or room for more students, somebody would hand it over. This stopped around 1980 by a strange political accident. Senator mansfield who was a great liberal decided that the defense department might be a dangerous influence and he got Congress to pass some rule that the defense department you can't do basic research. It should only support research with military application. It's a great example of whatever you want to call it.

INTERVIEWER: Unintended consequences--

MINSKY: Of shooting yourself in the head. And, but anyway--

INTERVIEWER: But right after that, the Media Lab.

MINSKY: Okay. Then now there was a thing-- we're doing this interview in this very building where Nicholas Negroponte-- who had had some training as an architect-- got this idea that computers were going to be important and media was going to change. And by the year 2000 there wouldn't be any paper anymore. And all sorts of ideas that were correct except that people didn't do the right thing. And so he had started this media lab. And in fact, some of the most exciting things in computers had happened-- not in the place you'd expect it to, but in the civil engineering where Professor Charlie Miller had developed graphics so that people could envision buildings and move them around. And Negroponte's, it was called the Architecture Machine Laboratory. And that was doing similar things-- finding new ways to improve communication. He invented something called zero bandwith-- what do you call, television by phone? There's a name. Anyway--

INTERVIEWER: Telemetry? Zero bandwith? No.

MINSKY: Well this is a joke. For a long time Bell Labs and other people had been trying to make television available over telephone so you could see who you were talking to. And they failed. The technology wasn't ready. It was too expensive. In fact, I had a video phone it was called. But no one else had one except Nicholas that Bell Labs gave us.

INTERVIEWER: So you used to call each other?

MINSKY: Yes. Anyway, zero bandwith television was a demonstration made by some students in the pre-media lab, which was very clever. It was a complicated sounding processing thing which sometimes could guess what emotion you had from the sound of your voice. It was better than chance. It could tell when you were laughing and it could guess when you were smiling. And if your voice lowered and slowed down it would guess that you had a less happy expression. So they managed to get a few graphics on the screen. You're talking to someone and you're not seeing-- there's no camera looking at the other person but its guess-- it has a cartoon face. And it was uncanny because it worked just well enough that it looked like you were looking at the person who was talking. It disappeared. They never even published it. And I just noticed last week some laboratory at MIT which said, oh we're going to make something that listens to the voice and shows the expression of the speaker. A little 30 year--

INTERVIEWER: Hiatus.

MINSKY: But it's the old timers' fault for not even publishing it. It was just such fun and they just showed it to each other.

INTERVIEWER: And was that the spirit of the Media Lab in the beginning? Fun, exciting, new ideas, cross-disciplinary?

MINSKY: It was exactly. Nicholas had the idea that he wanted to expand the laboratory because there was so many things that he couldn't do in the architecture department. And he got a wonderful idea, how would you fund this kind of research which nobody was doing? And he went to companies that didn't even know what research was. And they all piled in. They said, oh we're worried. Various newspaper chains for example heard the prediction there wouldn't be any paper pretty soon because everybody would have things like iPhones and they wouldn't need paper. Well that didn't happen for 30 years more than Nicholas expected but who cares. They were scared so they started giving his new Media Lab money to say what's-- at Steelcase-- wonderful company-- they began to realize that since you could work from home, maybe people wouldn't need office furniture. What will happen to them when the office disappears? So some visionary people at Steelcase gave us-- first they gave us a lot of office equipment. All the chairs-- not this one-- but all the chairs in the Media Lab were really deluxe Steelcase modern things.

But anyway, the nice thing about the media lab was Nicholas's inspiration to see yes you can fund research if you explain to people why they need it. And for almost 20 years it was just like the golden age that I got into when the AI Lab started. The Media Lab again started. Enough money poured in that we could do anything we wanted. It had enough sponsors that whatever you did, one of the sponsors would be pleased. And Nicholas invented a kind of sharing of property and rights and so forth that there was great happiness from-- I think it started in 1984 or 1985-- '84 probably.

INTERVIEWER: So during your period--

MINSKY: And for 20 years. Now it's getting harder to support because these ideas have spread and the Media Lab is working hard right now to-- what's the next revolution? Can the new director reproduce this? I certainly hope he can but it might have been a historic moment. Nicholas has said that he could never have started the lab 10 years later because they were doing things that the other departments were doing by then.

INTERVIEWER: So it sounds like what you're saying is that in your period at MIT you scored not one but two golden ages. A big one and a little one. Not bad.

MINSKY: And the second golden age, starting in about 1963, I started to work with Seymour Papert. And I'd never been interested in education and that side of engineering. So we worked together for 20 years. And then when Nicholas started the Media Lab he had some great engineers and great hackers of all sorts. And he also invited me and Papert. And we started to move our activities. So my artificial intelligence and Seymour's new ways in education started to develop here in this new environment. And again, it was a golden age in the sense that if we got an idea there would be someone to support it. Today things are different. The United States has very few basic research institutions. The government is broke. If you look at the National Science Foundation, they are now in the situation where they can barely fund one out of 100 proposals. Now suppose some scientist proposes something that's going to take two years. That's fine if he gets it. Suppose 200 scientists do that and one gets the support. They have spent probably 100 man years of wasted time writing these applications. And so now we might be better off if we closed the research facility completely. And the United States is headed down a drastically destructive track.

INTERVIEWER: What kind of students came to MIT in the late '50s and contrast that with the kinds of students you see today? And how broadly have the concerns and interests of students at MIT changed in the time you've been here?

MINSKY: I don't think I could say very much about-- are we recording your question?

INTERVIEWER: Yeah.

MINSKY: I don't think I have a very good picture of that. I have a qualitative sense that we're still getting wonderfully-- we're still getting some of the best possible students. But we have a lot of machinery for losing the best ones very rapidly because they take courses in business and they take course-- a lot of them will go into management science. As you know, when a field has the word science in it, it isn't. But it tries.

And also if a student gets a pretty good idea, then in spite of the great internet bubble of the year 2000 or whenever it was, they can get support to start a company. What this means is that the students who do the most exciting research as an undergraduate or even a graduate student are very likely not to become a professor. In the golden age, as I mentioned, almost all my graduate students became professors. Virtually everyone. Somewhere or other, usually a very good place. Now very few students become professors because they get jobs in start-ups or in the industrial research laboratories like Google and Yahoo and computer related places like that-- even Microsoft-- which employ thousands of people who eventually produce nothing in most cases. It disappears. They just-- sometimes they're just hired so that they won't go somewhere else. I don't know. But the future is fairly bleak for students now because they can't look forward to a career in research. It's just closed. Some are going to China. China is starting research laboratories where we're closing them.

INTERVIEWER: You invented a microscope. Why?

MINSKY: That was a great story. Well, one of the reasons that I hung around McCulloch the neurological community was to find out what they knew about how neurons worked and about how the brain works. It turns out that there is, to this day, a great gap in neuroscience. Because we know a great deal about how individual neurons work and how they connect to each other through these complicated little things called synapses. And when one neuron gets excited it sends some chemicals over to the next one and these chemicals start new activities. And a lot is known about how this works and the conditions under which these synapses grow and become stronger conductors or more quick to act and so forth.

Then we know a little bit about what happens in the relation between two cells. And almost nothing about what happens when there's 100 cells. And most of the brain of the human brain-- and mammalian brains in general-- most of the brains actually they're not really made of cells so much as columns of cells. These columns were discovered around 1950. And most of the brain-- there's this bunch of 500 or 1000 cells which acts as a functional unit. And we're just beginning to find out what these do. In the case of vision, we know a lot about what the columns do. In the case of the cerebellum and the hippocampus, we know a little bit. And in the case of the front lobes where reflective thinking goes on, we don't know anything at all. And-- well what was the question?

INTERVIEWER: You wanted a microscope.

MINSKY: Yes and one reason-- so one problem was that you could try to guess what these columns did but you couldn't find a wiring diagram of one. So I started to think about-- all we had were very thin sections with the people uses diamond knives or broken glass.

INTERVIEWER: Microscope slides.

MINSKY: Microscope slides.

INTERVIEWER: Two dimensional slices.

MINSKY: Now the interesting property of the brain is that if you-- of course it's pretty transparent, there's no pigment in the brain cells to speak of-- so you have to stain them. And there's no empty space. That tissue is full of cells. Many of them are nerve cells and others are other kinds of connective tissue cells and so forth. And the connective tissue cells in the brain look pretty much like what you'd think brain cells use. Each of them have thousands of fibers coming out and so forth.

Well, if you stained all of the nerve cells, the thing is black. There's a wonderful stain which uses osmium of all things-- rare metal-- and when it stains a neuron it stains the whole thing. And a neuron may be a whole millimeter or more in size. And some of its wires go 20 millimeters or more. And if you stain them all then if you take a section that's more than a thousandth of an inch thick, it'd be completely black. So nobody had three dimensional pictures of what happens. Because even a thin microscope slide is so dense that no light-- very little light-- gets through.

INTERVIEWER: So you were looking for a 3D brain viewer?

MINSKY: Yes. So the question is, if you can't get the light through then what can you do? And I thought of-- and one of the reasons is if-- of course you can get light through if your shine a bright enough light through. But then this light that comes through is pretty useless because it's bounced off something. It's called scattering and it's going this way and this way. Finally it comes out this way and you don't know where it came from.

And I figured out a very simple way by combining two microscopes back to back, looking at the same point, that this microscope-- if light got scattered before it reached the point you're looking at by something else into this, then that would be collected by the second one and it would be no good. But if you put a pinhole at each end, then any light that went the wrong way significantly would just get rejected. So now I could use an extremely bright light and just collect the rays that straight through and count how many there were. So now almost every laboratory in the world uses this thing. Unfortunately it took more than 20 years between the first one I built and the second one anyone else built so that the patent disappeared. But I get lots of letters and emails from people who say, thanks for making this gadget. Funny part is that by the time I finished it, that was exactly the time when I had read Ray Solomonoff's paper and decided it wouldn't help to know how the nervous system is wired until you have high level series to interpret it. So although I-- after building it, I used it to look at worms and blood cells and things like that. I never actually used it look at it a neuron.

INTERVIEWER: It's possible that not everyone at MIT would smile so broadly if they described one of their patents expiring before they could take advantage of it.

MINSKY: Well, it's too bad. I could have used-- if I had a billion dollars I could do my project now.

INTERVIEWER: There you go. I could do my project too. Last question: a theme of your story, in terms of how you describe your success, it seems to be that you were at the right place at the right time. That you entered into a golden age. And that-- MINSKY: One other thing is if somebody does something better then you, don't waste your time. Never compete.

INTERVIEWER: Right.

MINSKY: Always go away and do something that nobody else does better. So I kept moving around.

INTERVIEWER: If you'll indulge me for just a moment. With the sentimental possibility that there is something about Marvin Minksy that has been passed on to the many students that have encountered you at MIT, what would it be that you imparted to them other than be sure to be at the right place?

MINSKY: That's not a very useful one. I think the useful one is if you get stuck don't try too hard to fix it but find another way. Because if you get stuck it's because you're not good it good enough at that. And you probably can't fix that. So find someone else who can do it. But if you've got stuck it's probably because you found a really good problem. So find another really good problem.

INTERVIEWER: Is MIT a great place to find another way when you get stuck?

MINSKY: It certainly was. You know that the nature of things change gradually in the legal structure. When I was a-- I came as an assistant professor without applying because William Martin, who was chairman of the mathematics department, thought, hey I heard there's this good guy at Lincoln and maybe computers and things like that have a future. I never actually found out why.

And a couple of things happened. One thing happened is that I was teaching four courses as things were in those days. And I said I could teach-- no I wasn't. I was teaching two courses each term. But still if you're teaching every other day I found this hard. Because after I'd give a lecture it would take me a day to figure out what I did wrong with it. And I also needed a day to say, well what should I talk about tomorrow? And so I couldn't get any research done.

So one day I was walking down the hall and there was a great mathematician scientist Peter Elias who was in charge of the EE department. He said, how's it going? I said, well I wish I could teach all my four courses in one term and do research the other term. And he said, well why don't you come over to our department and we'll let you do that. Oh he said, well what happened when you asked them? And the math department said, well what if everyone did this? And I thought, well why not? But they thought it was bad. And then when I got there it turned out the EE department had so much money that professors only had to teach two courses.

But anyway one day, Peter came by and he said, oh we decided you should get tenure. And I never thought about that. First of all, I'd never had the idea of staying at MIT forever. In fact, I went to another couple of schools and hung around. And after awhile I didn't like it there because at MIT practically every student is really good. And that's just wonderful. Other schools, you'd have to search for-- anyway, that's beside the point. What's the difference is that today, when a student-- when somebody becomes an assistant professor, they have six years to make a reputation and get tenure and they think about it all the time. And they arrange their career so they do one big thing instead of several. They don't waste their time. They publish a lot of papers. A candidate for tenure here might have written 30 papers which are all almost the same. It's a scandal. They write slightly different papers. They somehow get them in journals. And they count them. What happens is these people are so narrow in a field because they're so desperate to make these points that-- I don't know how to conclude this paragraph.

So the situation is very different and this is all because of well-meaning civil rights laws and the promotion process has to be very open and it's bad if people promote their favorite friends and you shouldn't promote people from the same institution or it will get inbred and they have all sorts of rules. So they made this seven year rule. It's very rigid and turns out it's six years and it's really five. Because there's also another law which is if you fire somebody you have to pay their salary for a year. This has nothing to do with anything. So really you have to make the tenure decision pretty almost firm when they're in their fifth year. So the pressure is enormous. And I--

INTERVIEWER: So it's harder to get stuck in this era?

MINSKY: You're almost forced to get stuck. And finally the chance of getting tenure is small because they're not making many new professors. And you know there's another factor in all of this which is the longevity-- you're not allowed to fire people because of age in the United States pretty much. In England, professors have to retire at 60. But the age of the-- life expectancy has been growing three months per year for the last 50 years. So people are living 12 years longer now than when I started college. So the number of vacancies for new professors is slowly being eaten away by mere longevity besides everything else. And so the pressure --

INTERVIEWER: And it begs be said, another example of you being at the right place at the right time.

MINSKY: Well I'm also not-- I'm lucky not to have gotten old while I did it. But that's just luck too.

INTERVIEWER: Well, thank you, Marvin.