MIT 150 | Jay Forrester SM '45 (Part 1)

INTERVIEWER: This is the 150th Anniversary Interview with professor Jay Forrestor. Can I start by asking where you were born and what it was like growing up in Nebraska?

FORRESTER: I was born on a cattle ranch. I spent my youth on a cattle ranch where my father was the original homesteader, the first private owner of the land. So we're that close to the American frontier. I think the ranch was a very good launching point for my career, which in academia has not been geared to the academic audience, but always to the outside practical world, and one started with a very practical world on a cattle ranch.

INTERVIEWER: What did your parents do?

FORRESTER: My father had graduated from college. My mother had three years of college, which was very unusual in our community. They both taught school in one-room country school houses when they first went to the ranch in order to earn a little bit of extra money. My father was still teaching at my earliest recollection. My mother had stopped teaching to take care of me and my sister. My mother taught me the equivalent of the first two grades, first and second grade. Then I rode a horse to a one-room country school house where my father was a teacher for the third and fourth grades. For the fifth and sixth grades a woman was a teacher who had been my father's student. Seventh and eighth by that woman's sister who had been my father's student.

INTERVIEWER: Kept it kind of in the family?

FORRESTER: Yes, it was almost home schooling.

INTERVIEWER: What impact do you think growing up on the ranch had on you?

FORRESTER: I had never been very interested in taking care of cattle, especially in the middle of a winter blizzard. I'd always leaned toward doing the mechanical things and then doing electrical experiments of various sorts that I was led into by reading books from what was called the Nebraska Travelling Library. Every winter the state of Nebraska would send a big wooden box of books to each country school. A wide variety of books and they would have ones on electrical experiments, door bells and batteries and various things. I usually studied those and did various experiments. By the time I was a senior in high school I built a wind-driven electric generator that was the first electricity that we'd had on the ranch.

INTERVIEWER: Very forward thinking of you.

FORRESTER: Yes. I was in wind power long before most people knew anything about it.

INTERVIEWER: That wind-driven electric plant got you a scholarship to agricultural college, which you quickly dropped out of and switched. Tell me about that.

FORRESTER: I don't know why I got the scholarship, but I did have a $300 scholarship, I think it was from Union Pacific Railroad I believe for the agricultural college and about two or three weeks before enrolling I really decided that the agricultural college was not what I wanted to do, and I enrolled instead in the electrical engineering department at the University of Nebraska. A lot of people don't realize how one got into college then. Any citizen of the state could enroll in the university. You couldn't necessarily stay unless your grades were good, but anyone could enroll and the tuition was $35 per term. So I started down the road of electrical engineering.

INTERVIEWER: What was it when you graduated from University of Nebraska that drew you to MIT?

FORRESTER: When I graduated from University of Nebraska a lot of the students went on to industry, working for General Electric and such places. I was encouraged I think to consider graduate school by the faculty. My mother, when she graduated from college had been a librarian in Springfield, Massachusetts for a period of time and knew what MIT was. Actually, in the western states at that time MIT meant the Massachusetts Investors Trust because of investment salesman that had gone through the country. I applied to several places. I think I was turned down by Cornell. I received minimum scholarships. I came to MIT partly because I was going to be paid $100 a month for a research assistantship. So I came here. As I tell people, I came in 1939 for one year of graduate study and I just haven't gotten away yet. Or put it another way, I came to MIT in the year 9 BC, that is before computers. A lot of people don't realize there was a time before digital computers, but there was.

INTERVIEWER: What were your first impressions of MIT when you got here? You know, coming from the Midwest. The campus, the students, the culture.

FORRESTER: My first impressions, I'm not sure that I have a very clear recollection. I thought it was a pretty crowded place arriving through Central Square. But I found it a very congenial place. Something like 75 percent of the electrical engineering faculty were from outside of New England. So it wasn't exactly a narrowly focused community. At that time the electrical engineering department was small compared to what it is now. Every week there was tea for graduate students and faculty with a seminar talk by somebody. This was well attended, and the senior secretaries of the department served tea in china cups and silver urns and it was a very formal affair. I'm not sure that the departments do anything like that anymore.

But it was a place where everybody got acquainted, got to know the faculty and hear what was going on in various people's research. I met early on, Harold Edgerton, who had also come from Nebraska and was enlisted not long after I arrived to carry his battery packs in the days when you didn't carry flash units on your shoulder. Battery packs over to a theater in Boston where he was taking flash pictures of Gjon Mili in his dance routines.

INTERVIEWER: Did you have any mentors in graduate school that played a big role?

FORRESTER: Yes, Gordon Brown was very much a major mentor that was responsible for a great deal of my career. I was commandeered by him in the second year that I was here. At the beginning of war research to work on some remote controls for gun mounts where MIT had a contract, I think from Sperry Gyroscope, to develop some gun mounts that would respond to the directions from interception computers. So I became involved in that and I was sort of one of the founders, one of the four or five founders of the Servomechanisms laboratory, where Gordon Brown was the head of it. That developed to a fairly substantial establishment through World War II where we were developing control units for radar antennas and for gun mounts. I look upon Gordon Brown as my very influential mentor.

Gordon Brown ran a very different sort of laboratory from some other people. He gave very young people a great deal of responsibility. He expected a lot from them. He backed up those that he had confidence in. So we took on some rather major projects you would not find graduate students probably doing at this stage, because at that time there was the wartime pressure to get it done regardless. The bureaucracy was very much less than it is now.

I remember that we got to a point where we had some classified work going on. We had to have a laboratory with controlled access. We needed officially to have guards. There were no guard services at MIT. I had an auxiliary police badge from the Cambridge Police Department. I was officially a member of the Cambridge Police Department with a pistol permit because I had to double as a guard. Also, when it came to ordering things that you wanted you didn't put in requisitions for it. You just called up the place that might supply it verbally, placed an order, took a purchase order out my desk drawer, filled it out and sent it to them. That's all there was to the process of purchasing, so it was very much more informal than it would be at the present time.

INTERVIEWER: Things have changed.

FORRESTER: Things have changed a great deal. Overhead on research contracts were probably down in the range if 30 percent instead of 100 percent .

INTERVIEWER: So the work that you were doing at the Servomechanisms lab, so not all of it was done in the lab because somehow you wound up being on the USS Lexington when it was hit by a torpedo. How did that happen?

FORRESTER: I had designed the hydraulic controls. That's a separate issue. We found that the military at that stage did not trust electronics except in radios where they had no choice. So they very much preferred mechanical things. I had designed a hydraulic system, a hydraulic pump, a hydraulic valving system and a hydraulic motor about a one horsepower capacity with a one stage hydraulic amplifier that had about a 10,000 to 1 force gain, which you can hardly get out of electronics.

I had put this on a radar set that the Radiation Laboratory had designed. They had an experimental radar set that was intended for looking for oncoming torpedo bombers. It turned out that the Captain of the Lexington came to the Lincoln Laboratory, he saw this experimental unit. It was scheduled for production within a year or so, and he said, I want that on my ship. He didn't mean the production ones. He said, I want that particular one on my ship.

He got it and it had on it the hydraulic controls that had been designed in our laboratory. I had some part of it, but not all of it. It had been out in service for a considerable period of time when the control systems began to stop functioning. So they wanted to know if somebody would come out to Pearl Harbor and try for a repair.

Not knowing what the problem was, I packed everything I could think of that I wanted. I had a substantial wooden foot locker full of tools and spare parts. It weighed 250 pounds as luggage on a DC-3. It was very amusing to watch the luggage handlers come over, look at it, said it was stenciled on it 250 pounds. They laughed, didn't believe it. Would grab it and nothing at all would happen. Those were the days when you went to San Francisco by DC-3, stopping six or eight times along the way. One learned to sleep right through landings and take-offs on such a flight, and went then to Pearl Harbor on one of the Pan American flying boats, which had sleeping berths on it. Rather elegant way to travel in those days. Also one of the two most memorable steaks I've ever had was on that flight. Maybe just because of the circumstances rather than the steak, I'm not sure.

Anyway, I found out what the problem had been. It required rewinding the coils that had been in the valving system, the electronic coils, the coils that were driven by electronics. The problem had been that they had been wound up and then the lead from the inner side had just been wrapped around the rest of the coil and brought out. What it meant what was that the input lead and the output lead we're probably touching. An electric circuit can shut off the current very quickly and when it does it induces a high voltage in the coils and it had broken down the insulation so I had to devise a way of winding them where one could not get that sort of proximity between coils. I was partway through the process when the Executive Officer of the Lexington came around and said they were planning to leave port, would I like to come along and finish my job? So yes I would.

I sometimes worked on this unit at the top of the mast in the absolute darkness at night because you weren't allowed to have any lights showing. I continued to rewind the coils. The ship was equipped with really a good shop that could do that sort of work. So I did get them working and we stood offshore during the invasion of Tarawa. I didn't see really any action there, except for the fighter planes coming and going. Then after that we took a turn through the chain of islands north of Tarawa. I'm always confused as to whether it's the Marianas or the Gilberts. I think Marianas, maybe.

The Sunrise and Sunset Chain had Japanese airports on them. The task force came right down through the middle between them to bomb the airports, which the Japanese did not like. So they kept sending out torpedo bombers to try to attack the ships. At one stage I remember one of them had been shot down ahead of the ship. You could feel a little shudder as the carrier hit the airplane that was in the water. At that time, Stark Draper's gyro gunsights were very much in use. They were on the guns that were providing antiaircraft protection.

The whole day went by on that sort of thing with nothing very dramatic happening. Then the task force was leaving about 11 o'clock that night in the dark. The Japanese came in and dropped flares down the whole back behind one side to silhouette the ships and then came in with torpedo bombers from the other side. One of them hit the Lexington. It tore a big hole in it and cut off one of the four propeller shafts. And it was in a tight turn because at the moment that it happened it was in a turn, so it was circling out there in the water with the Japanese around, which is not exactly where you wanted to be.

I generally spent my time in the main control center because I was there to look after the radars, and that's where the activity was, so I could hear all of the communications. The admiral in charge of the task force, which I think he was on the Yorktown asked for a volunteer destroyer to stay with the Lexington. He was going to take everybody else out of range. There was no answer, so he assigned one.

The story was that the Captain, the last time in port had insisted that some manually controlled hydraulic rams, pistons had been put in for manual control of a rudder, be that as it may anyway they managed to bring the rudder back to near neutral. Just enough to offset the fact that there were two propellers on one side and one on the other. After that they steered with variable propeller speeds instead of the rudder. Brought it back to Pearl Harbor. The torpedo took out the refrigeration systems, so all of the meat began to rot and you could smell that throughout the ship. They put it in dry dock in Pearl Harbor just for temporary repairs before sending it on to Washington state for real repairs.

INTERVIEWER: You came back to Boston with a story to tell?

FORRESTER: I had come back to Boston. I had been very forcefully coached that I should say nothing at all to anybody about this episode. So I got back to the Radiation Laboratory and all of the leading people came to hear what I had to say about the equipment. I made no mention of the torpedoings. Then one of them said, now tell us about the torpedoing that you were in. Because by then they'd had all the information.

INTERVIEWER: After World War II how was it that you got the project to come up with a flight simulator?

FORRESTER: That was again Gordon Brown's intervention. I was thinking of how I got started on the flight simulator. I was thinking of leaving and maybe working for a company in feedback control systems that we'd been involved in. Maybe even starting a small company. I had Denver in mind as a place that I might try this out. When he called me in and said he had this list of 10 or so projects that I might find one of interest. As I looked down the list and heard more about the background, I decided the flight simulator would be the most important one.

This was an idea being backed by Luis de Florez. I always refer to him as Admiral de Florez, although I think maybe he was only a Captain at that stage. But he was head of the Special Devices Center in Port Washington, Long Island. Special Devices Center of the Navy. He had this idea that he would like to have a simulator a little bit in the spirit of the Link trainers that train pilots, except that this was to be so precise that you would put in it the data that you would get from wind tunnel tests of models of proposed airplanes and would be able to try out the behavior of the airplane. How did it feel and could a pilot control it well and did the pilots like it before in fact, the airplane had been built? That's a very substantial problem in aerodynamics and behavior.

At that time computers were analog computers. Any computers that existed were analog computers. MIT had been in analog computers from way back. Bush's mechanical differential analyzer was one, but even I think before that there were simulators for power systems made up of just little units that represented power lines and generators to find out what you needed to know about the stability of a power network. So the idea was that we would build an analog computer to do this. It became clear to me fairly early on during the first year that we would not succeed in making an analog computer that would do it because they had a limited scale of variables because it depended on the physical equipment of the components. It was almost certain that what you would get out were the solutions to the idiosyncrasies of the device and not the problem that you wanted to solve.

At that point then a number of changes began to revolve around Perry Crawford, who had been an MIT graduate in electrical engineering, I think. About a year ahead of me. By that time he was working for Luis de Florez in Port Washington, in the Special Devices Center. He is the person who first suggested to me that there was a field, that there were digital computers. At that stage I think the Harvard Mark I, an electromechanical machine, not an electronic one, an electromechanical computer was running or near running. There were ideas floating around about digital computers. I hadn't paid any attention to them. Perry Crawford is the one, standing on the front steps at 77 Memorial Drive one day, who suggested I really ought to look into digital computers as something to use for this aircraft stability analyzer.

We did that. Found there was a fair bit of thinking going on. Fair bit of confusion as to what a computer might be, and certainly confusion about how to design it. There being a lot of alternatives at the time. Most of which have fallen by the wayside, except for what we did in the Whirlwind computer.

So for about a year I expect, we we're thinking of digital computers for the stability analyzer when Perry Crawford essentially made another intervention. He was a visionary with some very good visions. Although sometimes not exactly knowing how to carry them out. But he brought into me the idea that we shouldn't really spend our time on a digital computer for organizing military combat information. By that time I think maybe Admiral de Florez might have been losing interest in the aircraft analyzer anyway. That we should turn our attention to the idea of using a digital computer as what would now be called a combat information center for the Navy. To go on the Navy ship to receive the information from the air, from the surface and from submarines, to essentially analyze and perhaps even run a Naval task operation from the computer. Bear in mind there was no high speed, general purpose, reliable computer in operation at that time. It was very daring. Nevertheless, Perry Crawford was the kind of person who was absolutely uninhibited in who we talked to. So he would carry this idea up and down the chain from the Chief of Naval Operations everywhere sufficiently to get money to start us down that track. So that's how we got from Gordon Browns list of things into a channel that lead to a lot of change at MIT.

Going back to Admiral de Florez for a moment. As I say, he was of Spanish extraction. I don't know literally where he was born. He had a waxed mustache that was quite distinguished. It came to points on each side. He's the only person that as I know has ever had permission to land a sea plane out here on the basin in front of MIT. I don't know how he did it, but the MDC, the police would clear the basin and on alumni day he would come in, land his sea plane, come to the alumni lunch and then when the speeches afterward got a little bit boring he would go up, rev up this airplane, drowned out everything else that was going on and take-off.

INTERVIEWER: He liked to make entrances and exits

FORRESTER: Yes, I think he liked to make notable entrances and exits.

INTERVIEWER: Let's talk about the Whirlwind I, and the changes that it wound up making.

FORRESTER: Essentially nothing was known about the electronics that you would need to use. It was fairly clear that-- well, it was going to be from vacuum tubes. The transistors hadn't yet made their appearance on the scene. Vacuum tube life of 500 hours clearly wasn't going to provide you a machine that would work for very long or very well. If you had thousands of vacuum tubes in it. So we had the problem of reliability, we had the problem of designing electronic circuits. The laboratory was staffed by graduate students, men coming back from service in World War II on the GI Bill of Rights coming to graduate school. I had access to all the applications in the electro engineering department and I would skim off those that looked like the most promising ones for the laboratory and offer them research assistantships to work on what was in due course going to be an emerging, major field. We had very, very skillful and very motivated people. We had this vision of where we were going that was I think quite sharply etched. So we essentially knew what components we were going to have, and different people would be working on different components. There was always the uncertainty as to whether or not any of it could or would work. At one stage we were uncertain about random noise. To what extent it would intrude on the computation. The computations suggested it would be low enough not to matter, but we didn't have any real concrete evidence of that. So at one stage we made what was called the five-digit multiplier. A five binary bit multiplier that ran continuously multiplying and then checking the result and running all the time to see how often it would make a mistake. We discovered it often made mistakes about 2 o'clock in the morning. That eventually was traced to the elevator in the next building where the custodians used the freight elevator to go from one floor to another while they were cleaning the building. Which suggested that we were not going to be able to run a machine with reliability if it really were connected directly to the power system. That led us to isolating Whirlwind I with a synchronous motor and a synchronous generator so that it had a complete mechanical separation from the actual power system to isolate it from any transience of that sort.

Then, for those electronically inclined, the power demand pattern from a computer is unpredictable because it depends on what the computing numbers are in the machine. So you can have pulses of power, very rapid changes in power demand, which suggests you could not filter the power with inductors, as it was commonly done, because they will not respond to the high frequency demands.

So the filtering was all done with electrolytic capacitors in banks. Most people at that time measured their capacitance in microfarads, millionths of a farad. We had banks of one farad capacitors, made up of banks of electrolytic capacitors. As I have told people, it was self-correcting for failures, because if one of those capacitors short-circuited, all the rest of them in the back would discharge through it and blow it right out of the socket.

Anyway, as we began to see that we were headed down the road of military combat information systems it's clear that we were really going to put a high emphasis on reliability. I would say we had a laboratory absolutely dedicated to Murphy's Law. If anything can go wrong it will. You'll have to make sure it can't. And of course with vacuum tubes having a nominal life of 500 hours that would've been quite unacceptable so we found out why they had been failing in radios. That's basically where they had been used. Why they'd been failing and took away the cause and we raised the life of vacuum tubes in one design step from about 500 hours to 500,000 hours. A 1,000 fold increase in the life of tubes in one design step by finding out why they'd been failing and taking away the reason for it.

INTERVIEWER: What was the reason?

FORRESTER: The reason for it will get you into a bit of technical discussion. But I had observed, using standard tubes in a oscilloscope, I had observed-- well first of all, they were normally discarded because they had low emission, which presumably was interpreted as the cathode coating on the emitting oxides had stopped functioning. I observed in the oscilloscope test that you could get vastly more current than they were rated for if you put a positive grid on. But that peak dropped off very fast. Like, very fast exponential, down to an extremely low value.

I said to myself, that's interesting. That looks like the behavior of what was called a cathode follower circuit. The cathode circuit contains a capacitor bypassed by a resistor. There are reasons why you use that, but it had that characteristic that I was seeing except there wasn't any such circuit in the test. So we began to wonder, where is that capacitor and where is that resistor? It turned out that the nickel of the cathode on top of which the oxides had been coated intentionally had silicon in it because it made the nickel easier to shape and form. After about 500 hours this hot metal, the silicon would migrate to the surface and give a monomolecular layer of silicon oxide or some form of silicon on the surface of that nickel. Very thin, that's the capacitor. Very high resistance, that's the bypassing resistor. So what we did was simply use nickel without the silicon and that was sufficient to go up almost a 1,000 fold increase. Probably a thousandfold increase in life. Another interesting--

INTERVIEWER: That was a good day's work.

FORRESTER: Excuse me?

INTERVIEWER: That was a good day's work.

FORRESTER: Yeah, that was. Getting a thousandfold increase in one step is a good day's work. But there was an interesting manufacturing sidelight to that. We'd been working with an engineer at Sylvania on getting experimental tubes. When it came to producing them -- this now is later. Producing them for Whirlwind and later for the SAGE system. When it came to producing them, he made a very interesting observation. He said, you will never make those tubes in any city where vacuum tubes have been made before. He wanted to start a little factory out in, I think western Pennsylvania, where nobody knew anything about vacuum tubes, as a place to put it. He didn't want anybody who thought they could eat jelly sandwiches while they were working on the close tubes. All the things that had become careless in the production tubes, which were only going to go in radios and only going to last 500 hours and it didn't really matter all that much. So he had a separate factory with an absolutely fresh group of people to make them, which was an interesting and I think, rather profound idea.

INTERVIEWER: The work on the Whirlwind led you to the work at Lincoln Laboratory?

FORRESTER: Whirlwind led directly to the Lincoln Laboratory and I think through a very checkered history. The whole history of Whirlwind was a matter of changing objectives, changing visions and changing goals. After it had shifted over to Perry Crawford's vision of a Naval task force control there probably wasn't any real inclination in the Navy for that sort of thing. For a time we had a contract to apply the ideas to air traffic control. Then Russia exploded its first atomic bomb and that lead to the possible threat of Russian bombers with atomic bombs. The Air Force knew that their air defense system could not cope with that situation. At that stage people at radar sets would phone information. Somebody would stand behind a transparent plexiglas board and right backward the numbers and location so that the people on the other side could read what they were recording manually as airplane tracks. From there on they would telephone orders and this whole process was very limited in capacity. With lots of time lags.

Probably everybody knew that it wouldn't work in the modern era. But then the question was, what do you do? The result of that was to create Project Charles. Now projects of that sort have become somewhat routine. Some major problem, usually a military problem would be addressed by calling together 20 or 30 people who might be considered experts in it and trying to advise the military or the government on what to do. There's been several of those previously. I think there'd been one looking into using atomic power for driving airplanes. An idea that certainly did not have any future.

But following that pattern the military services set up Project Charles. Had a very interesting group of people in it. Leslie Groves, who had been Executive Director of the Manhattan Project, was a member. Nyquist, who had been very influential in feedback systems in the Bell telephone system, was a member. The heads of the Air Force in Canada and of Great Britain were members, about 30 people, and it turned out that I and George Valley were also members, because we had some ideas about what could be done.

It's always my feeling that those study groups seldom made any progress unless within them somewhere was an idea already formed as to what they should do. In this case, from my viewpoint, Project Charles was assembled to endorse what we were proposing. Now they did not see it that way, but in effect I think that's what turned out to be. There were no proposals in the group for how to deal with an air threat from advancing numbers of bombers at one time.

By that time I'd already met up with George Valley, or he had met up with me. He had been chairman of an Air Force committee looking into air defense. They had begun to have some ideas about tying together radar sets and how they needed some kind of computation to analyze the data. Except they didn't have any real answer to that question. Someone else at MIT, probably Jerry Weisner suggested to George Valley that he come up and see us, which he did. We had already done computer programming for how you would carry out an interception, and had some ideas about the whole process of how it could be done as a result of Perry Crawford and the work we'd been doing in the Navy and in air traffic.

So we'd begun to work on this with small amounts of Air Force money. We were always limited on money compared to what we wanted to spend. We had essentially an annual inquisition assigned by the Navy to look into how we could possibly be spending so much money and how our ideas where so far out from all the other people that were doing digital computers. But we had gotten to the point where the local Air Force laboratory had provided data links that could connect a radar set to Whirlwind. Whirlwind could analyze this data, throw away the junk that a radar set generates, find the airplane tracks in it, smooth those, project them, calculate the flight for an interceptor. That had been done ahead, really, of the convening of Project Charles.

So as they milled around in the matter of really not having an idea, we invited them over to observe interceptions using Whirlwind. This was done before we had magnetic core storage. This was when we were still using some electrostatic storage tubes of our own design. Whirlwind was not reliable. We would work all the preceding night sometimes, making sure everything was going to work for a demonstration. Whirlwind was very accommodating. It would run through a demonstration and stop when they walked out the door.

Anyway, with World War II radar, that had probably good resolution and azimuth, but very poor, maybe one mile resolution -- I mean, good in distance, but probably one mile in azimuth. But by smoothing it and taking multiple scans and combining them we would get enough accuracy that we would have to run our fighter plane at a different altitude from the bomber or we would be likely to crash them into each other. Where the computer was feeding data to the auto pilot of the fighter plane.

I ask people these days, how much random access memory do you think it would take to bring in radar data from two airplanes, analyze it, throw away the trash that the radar provides, compute directions and send them off to the auto pilot of the airplane? They say, oh, 10, 15, 20 megabytes at least. We tell them we did all of that in 800 bytes. Not megabytes, just plain 800 bytes. Because that was a time when the real restriction was the availability of random access memory, and it was worth paying somebody for two or three months to take one order out of a program. Now of course, it's the other way around. People are extravagant in the overuse and inefficient use of programs because memory is so cheap. It was a very different technical environment. So that led to the creation of the Lincoln Laboratory for designing an air defense system.

INTERVIEWER: How did the concept of magnetic core memory evolve?

FORRESTER: One way is to say that it was a clear case of necessity being the mother of invention. I had a group 200 or 300 engineers building a system that we knew wasn't going to be reliable enough with essentially a commitment to military reliable equipment. What we were building or what we had operating was clearly not sufficient. The electrostatic storage tubes that we had designed here, I think they were perhaps better than what was otherwise being used at the time, but it cost about $1,000 to make one. They stored about 1,000 binary bits. They lasted about a month. We were paying about $1 per month per binary bit to maintain storage. If you take your 500 megabyte computer that's $500,000 a month to maintain it. So obviously it's an entirely different world.

I had devised in 1947 a concept of a three-dimensional storage. Maybe one should back off. At that time there were one-dimensional computer storages. These were tubes of mercury, about a meter long. A crystal at this end would send shock waves down the tube. Shock wave for a one, no shock wave for a zero. Send a stream of binary digits down the tube. They would be picked up by a crystal at the other end. They would be re-timed, reshaped and re-circulated.

This was reliable, but it was slow. You could not get at, this was a millisecond or so. You could not get anything that was in the tube until it came out the end. So you were limited in how quickly you could get access. Again, relatively expensive, but reliable nevertheless. That was being used at the University of Pennsylvania as storage. Other people were using something called a Williams storage tube. This was a plain, ordinary cathode ray tube with aluminum foil cemented to the face of it, and arranged so you would scan on the inside and pulse the beam and leave little dots on the inside. You could tell from the picking up signals from the foil whether there those dots were charged or not charged. Had to be continuously scanned to refresh the points. The whole thing was rather treacherous.

We thought we would improve on it with the storage tubes that we designed, which were a very different sort of thing. Much more expensive, much more complicated. They worked, but I'm not sure that in retrospect they made any tremendously great advance. We had to have something better. I had started, as I'd begun to say, I had started with the idea of using glow discharge tubes in a three-dimensional array. Glow discharge tubes represent a nonlinear element. That's the essence of storing. You've got to have some kind of a highly nonlinear element and a glow tube you have to raise the voltage to about 70 to ignite it. It'll stay ignited until maybe you get down to 10 volts. So you have a big range here where if you are operating below the ignition point it won't ignite and if you get above it, it will. That gave the idea of the coincident current storage. You would raise, in the x-axis you might go halfway up, in the y-axis you'd go another halfway and then the tube would fire, and then it would say fired on the basis of just one axis activated. It gave you the possibility of igniting individual tubes and keeping them ignited until you extinguished them by the reverse process.

That idea conceptually was what eventually we used. But I don't think I ever had any great confidence in the implementation. We did run some tests on individual glow tubes. These are glow tubes like you would see in instruments or on door bells and things. But they generate a lot of heat. They would be sensitive to heat. They depended on secondary emission, which shifts with time and they were slow. So it wasn't really promising except for the geometric concept. Then in reading the electric engineering magazine I opened up to an article-- well, to an advertisement. An advertisement of magnetic materials for magnetic amplifiers. Rectangular hysteresis loop. Magnetic materials for magnetic amplifiers. Now the material had been developed by the Germans in World War II and I believe it had been used in their army tanks as amplifiers in their control of the tank.

Here was a nonlinear device. It had a rectangular hysteresis loop like this. I thought, well there should be some way to use it. I was living in Wellesley at the time. I went out and walked the streets in the evening, in the dark to just think about how to use this and came up with an arrangement that followed right straight through to the first wave of computers of how to arrange them to trigger this magnetic material and come back and find out whether or not it had been triggered. But we didn't know really.

First of all, those units were again, conceptually satisfactory, but operationally not very satisfactory. The magnetic material itself was so sensitive to pressure and stress that it had to be put inside of a plastic case in order to avoid any pressure on it. Again, it would be expensive, bulky and in any case the magnetic materials were relatively slow. We ran experiments with some of those and the idea would work, but again, it was not exactly what you would want in a way of cost or practicality.

The next step, then, was coming to an advertisement again in the magazine for magnetic transformer-- I mean, ceramic transformer cores that were being used in television sets. The article by the author who was a ceramicist. I think, probably a man from Germany. Very likely someone who had made pottery as his career was making cores for the television industry. The problem was that these cores had a fat magnetic loop, which represents power loss. So he wanted a straight line, magnetic, coming and going in the magnetic field. We wanted it just the reverse of what he was trying for. We wanted something that would take this bulge and square it up and make it totally different.

There were some interesting sidelights. The early work on ferrites had been done by Philips Labs in Eindhoven, Holland. We wrote to them to say we would like to go in the other direction, could they give us any advice on how you would get a rectangular hysteresis loop? They wrote back and said enough was known about the theory that they knew it couldn't be done.

But we were talking to this experimentalist in New Jersey. As I say, a man whose career had probably been making pottery. Occasionally he would get a core that came close to being what we wanted. Now if you get one you know it can be done. After that it's a matter of learning how. For a long time we got our cores from him. A very small yield, but out of a batch, there'd be some that would work. I never was there myself, but some of my people went down always to work with him. They said he would mix up this black powder out of various things he had ground up and run his fingers through it and say that feels square to me and fire it, and he would-- every once in awhile-- get some satisfactory cores.

I think MIT spent close to $1 million finding out what he was doing. In other words, there are maybe as many 50 variables in that process. There is first of all, all the choices you have as to what elements you use in the powder. All the mixtures and characteristics. How you grind the material, how hard you press, how fast you fire it, how long you fire it, what temperature you fire it at, how do you cool it. By the time we had nailed down all these variables you could get essentially 100 percent yield from making these cores. But it was a long, expensive process and nobody knew whether it could succeed. We did not know for sure that this material wouldn't change its characteristics under the magnetic beating of the circuits that wanted to keep reversing it's magnetic field.

Eric von Hippel-- no it's not Eric. Anyway, professor Arthur von Hippel in the MIT electrical department had a laboratory on materials. We worked with him. There wasn't anybody that could be positive what this material would do in terms of life or what would happen if you kept using it in the environment that a computer would require. So after we had a couple of years of running experiments and getting control of the material we decided to make-- or two of my staff decided to make-- a computer to test the magnetic core storage. I did not think they could get it done. They did it somewhat against my better judgement. But it turned out they made a computer almost equivalent to Whirlwind in a few months out of the test equipment that we normally use for testing devices, and demonstrated that the magnetic core memory really did work. Then we moved the first bank from the memory test computer over to Whirlwind I think in 1953.

INTERVIEWER: But you ran into some patent disputes, too, didn't you?

FORRESTER: Yes, we had patent disputes on the magnetic core memory. It took us about seven years to convince industry that it would work and was a good idea. It took us the next seven years to convince them that they had not all thought of it first. I got a very ample and extensive education in the patent law through all of that. As one patent attorney told me, if all the facts are on your side and everything is perfect and the logic is indisputable you stand one chance in four of losing.

INTERVIEWER: What impact did the work that you did in digital computing and random access memory have initially?

FORRESTER: It made the idea of a computer controlled air defense system practical, reasonable. Obviously we would have to have a manufacturer for making the computers. So I and three of my colleagues set out to decide who should be the manufacturer. So we toured various-- well, we first of all sent out memos to a lot of people to see who wanted to be considered. Maybe four companies did want to be considered. They included Sperry Rand, Raytheon, I think and IBM, maybe one or two others. This was not a matter of putting things out for a low bid. This was a matter of our deciding who really showed promise of being able to do it. That lead to a number of interesting sidelights.

We visited Sperry Rand at the time that MacArthur had already been fired by Truman from his military role in the invasion of North Korea. So he was Chairman of the Board at that stage of Sperry Rand. I remember the extent to which they were trying to impress us. The president of Sperry Rand entertained us on his yacht. As we walked down the stairway into the living room he apologized for the rose petal that had fallen on the floor from the bouquet. That evening we were driven back to New York by General MacArthur. Actually, his three or four star general was his driver, but we all sat in the back of a limousine, three of us and MacArthur for the trip from Norwalk, I think it was back to New York where he was living at the Waldorf Astoria. That was very interesting because I have never encountered anybody who could carry on a conversation in absolutely perfect publishable English. Every sentence complete, no hesitations, the logic absolutely organized. If his conversation could've been recorded it would be ready for publication without a change. I was much impressed and furthermore, turned out he was essentially right. He was discussing the current United States' preoccupation with Russia taking over the world and moving out from its border. He said, they're just going to be too busy with all of those satellite countries they already have. They can't handle it. They won't. And of course in the long run they didn't. He had very pointed comments about people. He talked about Marshall, who I think was Secretary of State then coming over to Japan when MacArthur was in charge of rebuilding the country. He said, looking down his long aristocratic nose at these people that he was trying to build into a country.

Anyway, it was clear that IBM was in the best position to do what we wanted. Sperry Rand had three factories. A manufacturing factory, and they had two computer factories. They had the Research Associates in Minneapolis, and they had the one that Eckhart and Mauchly started in Philadelphia. It was clear to us that these three were fighting with each other and they weren't really a team. One couldn't count on them. The IBM people were a team. That was quite clear. They would respond to what the company wanted to do. So we chose them. It was as simple as that. Then I wrote the contract between the Air Force and IBM. I put in it that IBM could not put a single drawing in the factory till we had signed it, which was very galling to them because they imagined themselves as the experts in the field. We had been, by that time well, as all out that evolved we were doing the designing and working with them on the design. They would want to do a circuit or something in a certain way and we would say no, no, that's not going to be reliable. No, that's not the way to do it. They would say, but we've always done it this other way in our commercial equipment. We would have to be in a position of saying, yes, but we've looked at your commercial equipment in the field. It's not that way anymore. Somebody changed it since you last saw it so it would work. So it went almost every circuit of the way, an argument about how to do it.

It was quite clear to me that those 200 or 300 engineers thought they were delivering their computer to the shipping room platform. We had the attitude they were delivering it to the country with the expectation it would work. I told them, from time to time, that if I had my way IBM would be responsible for maintaining their equipment in the field. They assured me their company would never take the responsibility for military equipment in the field. Well, come the day when I had gotten the Watson's and Wright Field to sign a contract for IBM to do perpetual maintenance on this equipment. You have never seen an organization turn around so fast. Those 200 or 300 engineers, every one of them could imagine himself and well first of all, they would say, and how can our company do it? Everyone realized they were the ones that were going to be out there doing it because the company didn't have anybody else. So everyone could see themselves as being in Saskatchewan some winter morning at 2 a.m. with what he was designing, and within a week they wanted exactly what we wanted. There was no argument about how to do it. It demonstrates the importance of incentives and what the goals are. They changed from the goal of delivering to shipping platform to delivering to themselves. That made a complete reversal of attitudes.

INTERVIEWER: The years that you were at Lincoln Laboratory and working in the development of digital computing, you were working with Robert Everett? What become of that work? What did that lead to?

FORRESTER: Robert Everett had been my Associate Director all the way through World War II. He came probably in 1940 or so to the Servo lab. Again, probably as one of these graduate students. So he had worked with me in the days of the hydraulic control equipment and he continued with me throughout the aircraft analyzer and on into the Whirlwind I computern and continued as associate director of Division 6 of Lincoln Laboratory where I was the director. It was in that set up that together we created the SAGE air defense computers and launched the installation of them. Robert Everett was a Master's degree graduate of MIT. I think he went to Duke University before that. He's perhaps the only person that I know who if he and I differ I will give him more than 50 percent odds of being right. He was a very, very effective colleague. After I left Lincoln in 1956 to come to the Sloan School or what was then called the School of Industrial Management, he stayed, became head of the division that he and I had headed. Then not long after that MIT spun that whole division off to become the MITRE Corporation. So my organization that started as the computer lab became Division 6 of Lincoln. Then became the MITRE Corporation. I guess for political reasons or something they appointed a man from Bell Labs to be director for awhile, but very soon after this Robert Everett became president of MITRE and was president for quite a number of years. MITRE has grown continuously in military and governmental electronic systems design and management.

INTERVIEWER: How old was Lincoln Laboratory when you got there? Wasn't it just forming?

FORRESTER: It was just forming. There wasn't any before. I suppose you could say it was a few months because we weren't part of the first month or two or three or five because we continued here as the Digital Computer laboratory. Then it became clear that since they were dependent on Whirlwind and our activity it began to move out into the buildings out in Bedford, and became Division 6 of Lincoln.

INTERVIEWER: How did your interest in engineering and the work you were doing at Lincoln Lab become an interest in joining the faculty at the Sloan School when it was forming?

FORRESTER: There were several things that led me to change from Lincoln to the Sloan School. By 1956 I felt the pioneering days for computers were largely over. People have a hard time understanding it these days, but I think that in terms of speed, reliability, storage capacity, the things that make a computer more happened in the decade from 1946 to '56 than probably any other decade since. Indeed every decade has brought major advances. But the nature of computers was essentially established pretty much in the pattern that still exists. So that was one thing. I was ready for a new career. Another was that the organizational structure of the Lincoln Laboratory changed and I found myself in a position that I didn't especially care for, which was another reason for leaving. The third was that Jim Killian who was then President of MIT brought a group of dignitaries out to the Lincoln Laboratory to show them around. I was walking down the hall beside him and he said to me that MIT was starting a new management school and there might be something there that I would be interested in taking part in. So that was a door opening to the possibilities. What happened then was a year of discussion with Eli Shapiro who was Associate Dean of the School of Industrial Management, which has become the Sloan School. Discussing what might be done. I think it was more or less in the context of a vision that Alfred Sloan had had when he gave $10 million to start a management school. He thought a management school in a technical engineering environment would develop differently from one in a liberal arts environment like Chicago or Columbia or the other places that had management schools. It was worth $10 million to run the experiment and see what would happen. Maybe better, maybe worse, but anyway it might be different.

Now that image was still unfulfilled. The school had officially started probably in 1954. It was built on the Department of Management, which had existed for a few decades. But as a school, as one of the five schools of MIT it had started about 1954, it had been in existence officially a couple of years. But there wasn't anybody in it to represent the technical side of MIT. So I was the first one in the School coming from of the science engineering side of MIT. It was a very interesting environment at that time. Some very able people who had assembled to see what they could do in a new management school. Douglas McGregor was there. He'd been president I think of Antioch College before that. Warren Bennis was there. Eli-- it's not coming to me, anyway was a member of the faculty and the activities at the school were just beginning to take shape. I had a year to decide why I was there was essentially nothing to do except to decide why I was there. Which probably would not be open to people at this stage. Also I probably could not get an appointment there with the credentials I had then. I had no earned doctorate. I had an honorary doctorate from University of Nebraska at that stage. I had published one important paper in a second level physics journal. That was about the extent of my publications, and I was given a full professorship with tenure as my first academic appointment. Of course, I had created the computer industry and a few other things, but still those I don't think would count today in academia like they did at that time.

INTERVIEWER: I'm not sure about that.

FORRESTER: Let's hope that's not true. But the evidence doesn't seem to be very obvious that it's wrong.

INTERVIEWER: What do you think your more technical background brought to the beginnings of the Sloan School?

FORRESTER: I had the background and feedbacks systems for I mean, throughout World War II we were in feedback systems, which we, like all engineers thought of as applying only to physical equipment without realizing that they applied to everything that was going on around us. I brought a tremendous amount of management experience because I had been running a personal budget of several million dollars a year at Lincoln and controlling a budget of probably $1 or $2 billion outside in terms of the control that we had over IBM, the Air Defense Command and the Air Material Command because they were building and installing a system over which they had essentially no expertise. So we really were running it. I defended the Air Forces budget before the Bureau of the Budget, because they didn't have anybody that would be able to do it. I wrote-- as I said-- the contract between the Air Force and IBM. So I had that very considerable amount of management background. It was not the break that a lot of people imagine when they think of going from designing computers to the management school. It was a very practical management. It was management in the real world, not in theory or concepts. So I had a very realistic background of the nature of management.

INTERVIEWER: Do you think your maybe more practical experience, was that well received?

FORRESTER: My more practical experience was I think well received at that time because the School was I think considerably more open and more outside oriented than academia becomes after time. So I think the emphasis on research for the professional journals wasn't as strong then as it probably is now. I never felt a part of academia, my constituency was always on the outside. Always had been and continued to be. So I got outside funding for the research that I was doing in developing the new field of system dynamics. That field did not get a widespread acceptance in academia for a very long time. It's infiltrating, it's getting there. But not very open at first to a completely different way of looking at social systems and doing modeling of social systems and economics.

INTERVIEWER: Why do you think that is?

FORRESTER: Everything about it is different. The source of information for modeling is different. I take the position that there are-- let's say there are three bodies of information you can draw on. There is information people have in their heads. There's information that's written down in newspapers and the books and there's the information for which numerical measurements have been made. Just for the sake of discussion, I would say the mental database has a million times as much information as the written database. The written database at least from the standpoint of dynamic behavior probably has a million times as much information as the numerical measured database. Yet, the social sciences have tended to focus on what they can do with that numerically measured set of data, which gives them some security. I will analyze it in a certain way and I'll get the same results that somebody else does when they analyze it in that same way. But it's devoid of real contact with the outside world. So system dynamics models draw on all three of those databases: the mental database, the written and the numerical, but the overwhelming percentage of what you use is out of a mental database. Because that's what the world runs on. I challenge people to think of their own institution. MIT or any other place else. Everybody in it gets up at 10 o'clock in the morning, walks out and your place is taken by somebody with no experience in that setting. You say, read the orders, read the policies in my office, look at the data you'll find in my office and carry on for me. Obviously, it would be chaos because the world runs on experience, apprenticeship and being there. If you want to model the dynamics of such an organization you have to understand who has what information, what it is they're trying to do at different points in the system, who they're afraid of, what they're doing for the benefit of the organization, what they're doing for their own benefit and you can get that out of talking to people if you persist long enough. That's an approach to where models come from. It's totally different from what's been current in academia or in economics in particular. So that's one part of it, and out of it you don't get the kinds of articles that will fit into the publication framework of the academic publications and so on the whole there's a difficulty of even publishing it because you're dealing with highly nonlinear complex feedback systems and you're doing it essentially on experimental basis. The modeling represents a role playing structure of the real system and you're running experiments. There's no general theory. You don't come out with a general theory. You come out with impressions and a degree of understanding of what the system is like, but you don't come out with a clear-cut, final set of answers. There are no such. So the whole thing is very, very different in terms of where it comes from, and also the practical use to which it's addressed.

INTERVIEWER: So as we start getting into the system dynamics discussion I guess I should start out by asking you if you can give me a definition of it and then start to tell me how it originated.

FORRESTER: As a definition, there's never been a good definition. It's like asking, what's the definition of engineering? Or what's the definition of law? Or what's your definition of medicine? There are no sweeping, general definitions of any of these professions. It is a profession in that sense.

INTERVIEWER: I just want you to say system dynamics because you haven't said it yet.

FORRESTER: System dynamics evolved initially as being industrial dynamics. I'll come back in a moment to how it got started. But the best definition is that we I think have in a few sentences is that it is computer simulation modeling of how things change through time. What is it that is causing change? When you begin to look at all change everywhere, of every sort, it all is controlled by feedback loops. This is an idea that's not generally recognized. But if you ask, what happens when you fill a water glass with water? People say, the water runs out of the faucet into the glass. But that's only half the story. The other half is that the level of water is shutting off the faucet because you're watching the water level and when it gets to where you want you shut off the faucet, so there is a feedback loop there of the water level controlling the faucet. So it goes with everything that changes through time. This is also true of social systems and physical systems. So we're really talking about a field that melds the two, brings physical systems and social systems into the same framework. It brings most disciplines into that framework because most disciplines would like to deal with how things change through time, but they do it with varying degrees of success.

Just to show what kind of effects you see, I had two doctoral students in our Department of Electrical Engineering and Computer Science who had had all the theory about solid state physics and everything. They came over and built a one or two level, simple system dynamics model of what's happening to the electron cloud at the contact point of a transistor. They said it's the first time they ever understood what was happening. They had seen it in terms of mathematical equations, but they had never seen it as a physical process, and when we may get later to the idea of system dynamics as a foundation for kindergarten through 12th grade education, but through this process one can deal at the level of fifth grade with kinds of problems that academia and graduate students have been afraid to try.

But, you asked how the field got started. Again, I would say it was this practical approach. I had a year here to kind of figure out what I was going to do. I think people expected and maybe I did myself that I would be devoted to how computers should be used in management information systems. Or possibly, pushing forward the field of operations research, which was already defined as a field, pretty much as it has been through the years. I looked at each of these and I came to a conclusion that they were not the right thing for me anyway. Operations research was well-defined. It was making significant contributions to various management problems, but it was not dealing with the big issues that made the difference between corporate success and failure. On the side of using computers in management it seems to me we had done our part on that. We had developed the field of digital computers. We had shown that they were possible and they were already being manufactured and were being used in banks and insurance companies.

So there were computer companies and big financial companies in particular that were working with how to use them. It did not seem like one or two or three people here were going to have much impact on the momentum that was already going on. It was then out of discussions with some people from General Electric that the field really began to evolve. They were very troubled by why their manufacturing facilities, I think in Kentucky, for household appliances would be working seven days a week, three shifts, one year and three or four years later half the people would be laid off. It was easy enough to blame it on the economic business cycle, except that wasn't entirely persuasive.

I suppose it was my background in engineering feedback systems that led me to take one notebook page. Across the top were columns for inventory, order backlog, employees, production rate, and a line, week by week down the page. Then using the policies that I'd already discovered in discussions with them that they were using, in terms of production and employment as a consequence of orders and backlog, you'd come to the next week. Now given those backlogs and inventories and production rate will you increase or decrease by how much in the next week. So now you would hire more people and you'd fill that one in. As a result you'd have more production, and you would change the inventories. Then you would to do it again in the following week.

As you went down the page you find that there's an inherent, internal instability to that system. That even with constant incoming orders they would have this trouble. Never mind the business cycle. But those policies lead to an internally unstable system.

Now that particular system was converted in our very first summer session, probably about 1958 into a role playing game that used to be called the Refrigerator Game. It's now worldwide known as The BeerGame. But it's a game in which a group of people down the side of a table, one is the retailer, one is the distributor, one is the factory warehouse manager, next one is the factory manager. They pass orders up and they receive goods coming down. Everything is on the table. They can see the backlogs, they can see the goods, this is more information there then you would have in real life. They start out with this system in equilibrium. There's the right number of orders going up and the right number of goods coming back down. Over here's a deck of cards face down with the orders every week that the retailer turns over and that's what he has to supply and based on that he orders.

Ordinarily, to run with that deck of cards being constant in keeping with what's been set up as an equilibrium system for three or four weeks. Then he'll turn over one that maybe is 5 or 10 percent bigger than before. Now he has to order more. After that everyone's the same. Everyone is the same 5 percent higher. Constant. Every week is the same. What you'll find is this huge instability that the factory down there will be working overtime and then shutting down. You will plot this, and you have this huge business cycle instability going on in the group.

That is very dramatic really, and hundreds of thousands of people around the world in all cultures have run it. At all ages and all kinds of people. From children to managers to Chinese to Japanese. It's available commercially, or distributed by the System Dynamics Society. The whole set up is available. You can get it and have people play the game and it's a powerful demonstration that systems matter. Also, that it's not obvious to people how to manage them.

That was really the beginning of the field. That game, which was the very first thing that we did is still used everywhere. So that evolved into looking at the rest of corporations and the modeling of things outside of the physical system, outside of the inventory and backlogs and production rate, and moving on into the psychology of managers and how the history of an organization affects its current decision-making, things of that sort.

INTERVIEWER: Along the way you started working with former Mayor John Collins?

FORRESTER: Yes, my whole career has been one of being willing to walk through an open door. Not knowing what's on the other side, but being ready to explore something that looks like it might be interesting. This was another one of those. John Collins had been a Democratic mayor of Boston for eight years. He had had polio in the big polio epidemic of the mid 1950s. He walked with two arm canes. He needed to have an office in a building-- well, and when he decided not to run for mayor again MIT offered him a one year appointment as visiting professor of urban affairs. MIT and Harvard often have done things like this to bring an outsider in to interact with faculty and students. They offered Mayor Collins this one year appointment. In order to deal with his infirmity the Sloan building at that stage was almost the only building that had automobile access to the elevator level. So he was given an office next to mine because the professor next to me was on leave that year. So through this combination of circumstances he ended up in the office next to me. In visiting with him he was telling me about the problems with cities. This was in 1968 approximately. The headline news were the urban crises. There were fires, there were riots. The cities were in turmoil and he'd been coping with it for years and explaining what they were doing. I began to get the feeling that I'd come to recognize in corporations. You go in, there's some major problem, you talk to people. Everyone knows what the problem is, everyone is trying to do something about it, everyone knows what policies they're following and what they would do in different crises. You quiz them on what would you do if, even if it has never happened and they'll know. So out of this you get the rules that they follow. They may not think of them as rules, but there are rules that basically they're following. Those rules then become the structure of a simulation model. The rules allow the model to make the decisions that the people would make under the circumstances they're in, but where all of the inputs in the computer are changing just as they do in the real life situation. Then you come back and you take these policies that people are following, you put them together in a simulation model and you find the policies they know they're following, which they believe are solutions will in fact produce the problem. In other words, what they are trying to do is a cause of exactly what they're trying to avoid. This is true throughout our social systems, and as a consequence leads to most of our problems in society. That the efforts to solve problems really are putting more and more pressure to do the very things that are causing the problem. Anyway, I began to get the same feeling in talking to Mayor Collins about cities. I didn't know what it would be. I just felt that his story didn't hang together.

So I said to him that maybe we should combine his background in cities and mine in modeling and see if we could shed some light on it. I had no idea where it would lead. He said, well what do we have to do? I said, we're not going to find out what we want to know from the urban studies department. We're not going to find the information we need in the urban studies library. We're going to have to have a group of six or eight people who have really lived with these problems in cities. We'll need them for a half a day a week for we don't know how long. Collins was very much a man of action. He said, they will be here Wednesday afternoon. His position in Boston at that time was that he could ask for almost anybody in politics or in business for their Wednesday afternoon for a year and get them.

INTERVIEWER: So what types of people showed up on Wednesday?

FORRESTER: Various people from different sectors of Boston. Various people modestly high up in politics, but not the top people. We probably had some out of industry. It was a small group. Six people or so, but sufficiently large. We begun to hold these Wednesday afternoon meetings. Long meetings, 1 o'clock till 5 or 6 discussing the problems of cities, and if you had sat in and recorded it, it would have been a standard aimless discontinuous discussion. It's the only time I've ever gone into modeling where I had no idea what would come out. No idea whether anything would. No vision of how it would possibly evolve. Everything else I've done, I've known how to approach it. Known we could. Here it was an absolutely open slate.

We were having these meetings every Wednesday afternoon and I was working probably as much as 30 hours a week trying to see if there was any sense coming out of that discussion. I would sort of lay out an outline of some relationships and decide that didn't seem to be going anywhere. Abandon it before I'd done much with it. Until I think it was a Sunday, I made a sketch. It was a rectangular grid of corporations across the top, new ones, aging ones, old buildings. Buildings that were new, buildings that were aging, buildings that were rundown. Across the middle was housing. New housing, middle income housing, decaying rundown housing. Across the bottom three kinds of people, managerial professional, skilled worker, and under employed. Into each there's a flow. You build into housing. People move from one category to another. So now you have nine stocks, and you have flows in and out. The next step is to decide what it is that controls each of the flows. When you look at it almost every one of those stocks has something to say about the flow. So now you have a social structure of relationships and even from the set of stocks one can immediately see what kind of thing's going to happen. As the buildings age, the employment declines. As the housing ages, the population concentration increases. There are more people per square foot in slum housing than there is in premium housing. There are fewer people in decayed buildings than there are in vibrant buildings. So now you had a situation where you can almost see it from that diagram before you've connected it all up dynamically. But basically, you go through a long period of 100 years or more in which the city is growing from empty land and in which the population balance is maintained primarily by economic opportunity being higher than normal and the availability of housing being lower than normal. You've got housing shortage and economic opportunity and that's maintaining the level of population . The growing level is maintained in such a way that it's a vibrant economic activity. But then as the land fills up the expansion, the building slows down in both housing and business, and now the aging process starts. As it does you come to a peak of population and now you hit a new balance. This new balance is less economic activity and excess housing, and you're pulling people in by the availability of housing and you're keeping them out by the lack of opportunity. Now you've produced a stagnation of people who need jobs and can't find them. So we saw that whole transition going on. Which I think very much stood the test of time. A result of that was that I would be invited-- I think my *Urban Dynamics* book was the result of that-- and I would be invited almost anywhere in the world where a conference on cities was being discussed. My view of why is that I could be the only person present who could talk for 20 minutes about cities and not contradict myself. Within this model I knew all the assumptions. I knew its behavior. I knew how the behavior would change if you followed different policies. Staying within that scope one can be absolutely 100 percent internally consistent with what you're saying. Now that doesn't make it right. I mean, that does not mean that it fits the real world. But on the other hand, if everything you say matches the reality known to different people, maybe no one has the whole story, but if everything you say does not get debated by an audience because basically they're agreeing with all the assumptions then it becomes a powerful argument.

INTERVIEWER: That series of books, the *Urban Dynamics* and *World Dynamics* and *Limits to Growth,* that series caused quite a sensation. Can you talk a little about that?

FORRESTER: Yes. You see again, we said earlier that the whole approach was different from what's going on in the social sciences. The urban dynamics model was built out of talking to urban people, not out of data. That by itself made it suspect. What we came up with in these systems you generally find that there are very few high leverage policies that can really change things. Most policies in a system are very low leverage. They don't really matter and most of them are what people debate. They debate things that don't matter.

When you find somebody that has latched on to a high leverage policy, the odds are very high that they're pushing that policy in the opposite direction from what they want to accomplish. This bias toward doing it wrong comes out of the fact that all of our intuition and experiences is built up out of dealing with simple systems and the lessons are diametrically wrong with complex systems. So we found only one high leverage policy in the model and that was the construction of low cost housing. Building more low cost housing was a popular urban and national goal at the time. What we showed was you should take it out, not put it in. This was not popular. One full professor of social science in our fine institution here came up to me after the book came out, he looked right at me and he said, I don't care whether you're right or wrong the results are unacceptable. So much for academic objectivity. That was the reaction the book got in some circles. Others said, somewhat more tenably, it doesn't matter whether you're right or wrong, those ideas will not be accepted by people in politics or the inner city. Those were the two groups we could absolutely count on for support if they got close enough to the ideas to understand them. Now that's an if that we never really found a good way to overcome. But it's really quite enlightening.

Shortly after the book came out-- well, the Sloan School had been running one-month seminars in management for executive level people in American cities. Department head level people. One of these groups was convening shortly after the *Urban Dynamics* book came out. I had never given a talk about it yet. I was asked if I would take a Monday afternoon and a Wednesday morning to discuss what we had done in *Urban Dynamics.* I have never had a lecture, on any subject, anytime, anyplace go as badly as that Monday afternoon. Because in the audience was a man named Gene Callender from New York. He was a member of Lindsey's Liberal Government. He was the department head level in New York. He was not accepting a thing I was saying and he was carrying the whole group with him. At one stage he said, this is just another way to trample on the rights of the poor people, and it's immoral. Another point he said, you aren't dealing with the black versus white problem, and if you don't deal with the black versus white problem you're not dealing with the urban problem.

So it went the whole afternoon. The intervening Tuesday evening there was a dinner where these people were staying out in the Endicott House, so there was a dinner out there. Neither John Collins nor I went or were able to, some of our students did. After dinner one of the students called me up at home to report what becoming fairly obvious anyway he said, the group is very hostile. With that encouragement I started Wednesday morning. About an hour into Wednesday morning Callender's questions began to change character. They were questions to get information. A couple of hours into the morning he said, we can't leave this here at the end of this morning. We've got to have another session. I heard him, but I didn't let on for I wanted to see what would happen next, and pleasantly he repeated it. I said, well fine. I would be happy to meet with you again. I don't see any place in the schedule, but if you can arrange it with the people that are running the program I'd be happy to meet with you. Usually that's the end of such an exchange. But it wasn't. He did get another session.

Then he came to my office. He sat there just as relaxed as he could be. He says, you know, it's not a race problem in New York at all. It's an economic problem. He had in his briefcase a report of every borough of New York, the amount of empty housing, and the rate at which it was decaying demonstrating the whole proposition that Harlem had too much housing for the economy. Too much housing compared to what it could support. What he wanted was, would I come down to New York and meet with his colleagues to discuss what we'd been talking about? Complete turn around from that Monday afternoon. I've seen that a time or two also with other people in urban dynamics. If you're watching you can see the instant it happens. They've got to be a captive audience because at that stage they would get so emotional and so negative, but if you were just casually talking to them they would walk away. But if that wasn't possible there would come a time when they just looked like they'd been hit. Their whole world turned inside out. Everything they had previously known as fact was still true and the meaning was totally different. I've never seen that in a corporation. People don't care about corporations like they care about cities. You can't really stir up much emotional reaction in a corporation about their policies like you can in social systems. It took two, three, four hours of exposure for people to really begin to understand *Urban Dynamics* and that's not easy to get from skeptics. *World Dynamics* and *Urban Dynamics* were on the list for discussion of League of Women Voters and Parent Teacher Associations and such, so they had very wide readership and a very, very wide acceptance in certain circles.

INTERVIEWER: Was there a point at which you realized that system dynamics was going to be as big as it really is? As all encompassing?

FORRESTER: I don't know that you would say there was any specific point. It has evolved, but certainly by the time we'd been through *Urban Dynamics* and *World Dynamics* and *Limits to Growth* it was quite clear that we could make major inroads into social systems and economics. That has mostly to evolve still.

INTERVIEWER: Is the theory gaining acceptance slower than you expected or wanted?

FORRESTER: A lot of people in the field are disappointed that it hasn't moved faster. But I point out that it's a profession. It's like engineering when MIT was founded in 1868 or whenever it was. It is like medicine when John Hopkins started the first medical school in the late 1880s. All of those have evolved and grown and developed and they're still developing and system dynamics is following along in that same evolution of a profession of considerable complexity and considerable promise that hasn't yet been tapped at all. So it's not growing at quite the speed that computers did, but the exponential growth rate is several percent a year. Eventually that gets to be more than a society can support.

So it's growing very well, but it's handicapped by lack of vision by a lot of even the participants. It's handicapped by the fact that the best people are doing invisible work. A lot of the work is going on in corporations and I had the interesting experience of being invited to lunch in Paris at the headquarters of a company by a leading person in system dynamics and we sat down to lunch and he said, I'm sorry. I'm not able to tell you anything we are doing in system dynamics. He said, we can go to the professional meetings and we can discuss anything we're doing in economics or in operations research and we're not allowed to even say that we're interested in system dynamics. That was the extent to which they felt it was proprietary and important. A lot of work is hidden that way. On the other hand--

INTERVIEWER: Why is that?

FORRESTER: Beg your pardon?

INTERVIEWER: Why is that?

FORRESTER: A misunderstanding by the people that others can follow quickly in their footsteps. In other words, they think that if they were to release this information they would be at a competitive disadvantage whereas, I think the situation is much more like a vice president of 3M once said in the speech. He said he would be perfectly willing to get up in front of any audience and tell them exactly how the company is succeeding better than they are because they won't believe a word I'm saying. I think they underestimate the difficulty it would be for their competitors to follow even if they gave the whole story. It's not an easy field.

INTERVIEWER: So they're afraid that if they are public with what they've learned about their own internal systems that somehow that will get duplicated by a competitor?

FORRESTER: I think that's the concern. Yes.

INTERVIEWER: Even though each system is unique?

FORRESTER: Systems are not as unique as people like to believe. I think that's one of the fallacies that in fact is afoot in the field. People are working on unique models whereas my advice to someone, even in business, is to model the industry of which you are a part or the companies who are in the same business. Because the generic model, the general purpose model, the thing that is common to all of them is probably simpler than the model they would make of the specific company because they would put in more than they need to, and what they really want to know is, why is company A doing better than B? You should have a model that will expose both of them based on the known policy differences. Very often the crucial policy differences are evident from the outside. You can show that A is succeeding because of the things we can observe about it compared to C. Some of my most important simple models I can tell if they're relevant almost in the first phone call with somebody from the company. Ask a few leading questions and you know if that model fits or not.

INTERVIEWER: So when you speak with people who are being very closed with the work they're doing on system dynamics does that mean they are studying it and making changes based on it?

FORRESTER: It's hard to know what people are doing with the information in the corporate setting. Presumably they ought to be changing the policies that govern what they do. It's hard to know whether they're making those changes or not from the outside. But there are situations I know of where those changes have beem made and they have been successful.