**ANDREW SCHULTZ:** Last summer I received a letter from Cecil Johnson, explaining what he wished the Fellows to do, and I have found this an especially difficult thing to do. My name is Andrew Schultz, I am in my 79th year, and my memory for past times has always been poor, but it has not improved as the years have passed, for sure. So, one of the problems in doing this is to recall exactly when things happened, and I may be wrong on some of the dates I give you, and I wish you would understand that. The three aspects to the, my experiences with American institute of Industrial Engineers, and I think they are related, in general, to the elements of some importance in any profession. And the first one of these is the substance of the profession, the second one is the profession itself, and how it is defined, and the third is the environment in which it operates.

And at various times in 30 or 40 years, these basic factors have changed somewhat. When I first became aware of the institute, industrial engineering was largely a matter of time study and methods, and really related to individual work and workplace design and definition. I think that it would be very well if I reviewed some aspects of my own education to explain the background from which I came. My education was at Cornell University, within the Sibley School of Mechanical Engineering, which awarded two different degrees, the Bachelor of Mechanical - a basic BME degree - Bachelor in Mechanical Engineering, or an ME degree, as such, and the Bachelor of Science in Administrative Engineering. When I'm matriculated at Cornell in 1932, I had in my mind certain objectives, and one of those was to make my career in an area that involved not merely engineering, but also business type of activities, and that was the interest that I brought to the institution.

I also came at that time convinced, that there would shortly be a war. And I had gone to secondary school in Andover, Massachusetts, and the mathematics teacher there, who had spent some considerable time Germany after the First World War, and had many friends who would come from Germany to visit him, frequently asked students to a picnic when these people visited, which gave them a chance to meet students and students a chance to meet outsiders. And it was as result of those events that I was convinced that war would come after Hitler took over in Germany.

At Cornell in Sibley School of Mechanical Engineering, we had a number of departments. One was industrial engineering, and a department that had been started by, later Dean, Dexter X.

Kimball, who had created the department in the early 1900s, in which area they taught methods engineering, time study, plant layout, things of that nature. Other departments were heat, power, mechanics, mech lab, machine design, the typical mechanical engineering departments in any large school. When I matriculated in 1932, they had just started a new program in administrative engineering, which had been created by a professor named Johnny R. Bangs, Jr. And Johnny had been away getting a PhD at another institution, when Kimball solicited him to come to Cornell and develop and operate such a curriculum, which he did.

And I found it very attractive and became a student in that degree program, which was basically a mechanical engineering program, somewhat slimmed down by the elimination of a few courses, and substituting for the various options within the mechanical program, the administrative engineering courses, which Bangs instituted. He searched throughout the university and brought people in to teach these. And included in these courses were, of course, the typical industrial engineering courses taught in that department, plus economics, public speaking, technical writing, corporation finance, and similar courses having to do with human relations, psychology, and that type of thing. It's based upon his belief this was a very important part of an engineering education, and also very early, I think this program included, as I've already said, probably the first course in industrial marketing in the country, as well as another course in statistics, which he thought was extremely important for engineers.

As I completed that curriculum, I was exposed to all of these areas, and received what I thought was a very good education, and in fact this was justified by later studies that were made of students who received that degree in the early years, between 1935 and 1941 or 42, before the war started. And most of those students went on to significant jobs in industry, a good many of them becoming chief executive officers of the organizations in which they were placed.

I went to work in 1936, and worked for a year, when I received a telegram from Bangs offering me a job as an instructor, with the possibility of getting a PhD. And I grabbed at that, at that time and returned to Cornell, and started out in that kind of a program, concentrating in, my major was actually in quality control and statistics, with minors in economics and other topics of that nature. I was able to receive that degree in 1941, before being ordered to active duty. At Cornell, we had ROTC, the basic two years of which were required all graduates. And I completed the officers program by going to summer camp in 1934, so the end of my sophomore year, which was a year in advance of the time when most did. And then, so that I

could go to Europe in the summer of 1935, which I also did. My summer camp in ROTC was interesting, because for the first time I came in contact with large numbers of southern students. Georgia Tech, at that time, had an ordinance program, and their ordinance programs, as ours at Cornell, and a number of others, were all sent that summer to Raritan Arsenal in place of the Aberdeen Proving Ground, where they were rebuilding the quarters used for such ROTC students.

In 1935, I did go to Europe, and we bicycled throughout Germany, and we were quite aware, in our travels, of exactly what Hitler was doing and how he was doing it. And one thing was of great interest to me in subsequent years. In one of the towns in the Black Forest, we went into a hotel to have lunch, and in going to the laboratories, we found instructions for the nearest Air Raid Shelters. And lo and behold, many years later when I was in the army during World War Ii, we found out in some other recovered material from the bombing of London, that one of the incendiaries which had been manufactured, had been manufactured in that community at the time I was there in 1935, which gives one some idea of how well planned the German war effort was.

And having this background, and having a PhD In the area, I then went on to spend five years in the Army, and I was ordered to active duty in Washington, DC, in the industrial division in the Army Ordnance Department, in the ammunition portion, where we were responsible for the manufacture and distribution of explosives and chemicals. In that, I was exposed to a great deal of a great many problems, and we have tremendous responsibility and authority for such young individuals. There were many of us Reserve Officers in that unit. When I first reported, there were about 13 or 14 people in our division, within a year we had thousands. And it was a very interesting development for a young man. And I had the responsibility, ultimately, of maintaining liaison with a great many different people in coordinating and scheduling the productive activities of many plants and arsenals.

In 1946 I was discharged from the army of the United States and returned to,... left Washington to go south to Florida, since it was in March, to get a little sunshine and recover. I had some health problems at that time, and the army insisted I take care of them immediately. So we wanted to do that in Florida and went down there, and were there about three weeks before my wife's mother had a heart attack. So, we drove north overnight, and she dropped off at the hospital where her mother was, and I went on back to Ithaca, where we left a lot of goods in the place we rented, to see if they were still in the attic and still available, which they

were.

Then I went over to the offices of the Engineering College to see what was going on. And it turned out that a few days before, one of the professors, who was elderly, had had a heart attack going up one of the long flights of stairs and had dropped dead. And several of his courses were topics that I was familiar with, and they asked me if I would plug-in and teach those. And I had not yet decided what I would do at the end of the war, since it was all over, and I was a free man again. And so I began to teach. In short order they offered me the job of an associate professor, which at that time carried tenure, and I ultimately accepted. And I took over his courses which included statistics, and began to carry on as a professor. I question a decision that was solved more or less fortuitously for me.

In that period of time, from '46 to 1953, I was quite busy mastering the new aspects of my career. And I tried to find out as much as I could about the mechanisms of the educational process, as they were operating at Cornell. We had in that time some very exceptional students. And we faced some difficult problems in the faculty had, soon after I had gone into the army, adopted the five year curriculum, and along with four or five other institutions, planned to institute that program at the end of the war. So, we needed to adjust our curriculum from four to five undergraduate years. And one of the new aspects of it was the institution of a project, which could be either individual or group projects, but projects focused in some way on the design activity of the engineer. And one of the jobs that I was given was to work on the new five year curriculum for the mechanical engineering school. And I spent a great deal of time on that, that first semester that I was there in the spring of 1946, and in the ensuing summer, and helped develop that curriculum. And we ended up absorbing, within the same curriculum, and awarding the same degree of both what had been the administrative engineers and the mechanical engineers.

Mechanical engineering at that time had about 20 options, which included industrial, heat, power, and various other things of that nature, various specialties. And these were very, no longer formalized, and within the five year program it was possible to adapt the later years to do a variety of things. We ended up with, what were basically called the option A and the option B, and the option A was a straight mechanical, and the option with all their various specialties, and the option B was the development from the administrative engineering, but which is made possible a significant project somewhere in the latter years.

Shortly after that, the head of the department had an illness, and I took over as acting head for

a semester while he recovered from that, and during that time, I had the job of working towards the design of these projects. So I instituted staff meetings with faculty, at which our purpose each week was to design the program for the project. And I learned then something about faculties, many of them approached the project with great reluctance. And we decided that in our department, which was called industrial engineering administration, or a description I never fully understood, the reasons for which I never comprehend, but which were undoubtedly political in some aspect.

In that department we spent a lot of time designing industrial, real life industrial, on-site projects for our students. And as a result, when the first class came into it's fifth year, which was in the year 1950-51, why, we have a program already adopted and planed, and were able to start off with our first projects. Now, Cornell had given priority on the matriculation, or admissions, to anyone who ever set foot there prior to the end of the war. And as a result, a good many students who had dropped out before the war to go into the services returned, and we could admit very few freshman that fall of 1946, it was that very small, highly selected class, which made up our first project group. And we chose for that group, an industrial project in one of the corporations not more than 60 or 70 miles from Ithaca, and used that as our ground for developing further this technique. And we had, I believe, a successful year at that. Byron Saunders, who later played a major role in accreditation process of the American Institute, directed the project, and it ran very, very well I thought. Soon after that, we would take our students and have anywhere from five to 20 projects, depending upon their size of the problems and the numbers of students participating in each.

During this period from 1946 to 1953, we had a great many activities underway. One of the things we did was to start an American Society for Quality Control Section, using a technique that had been used by several of our staff early in the war to start another society in the area of cost accounting. And we set this up as a Southern Tier of New York section, which surprised the cities of Towanda Pennsylvania, Elmira and Corning New York, Binghamton, Endicott, and Johnson City, all within about 50 miles of each other. We did not go much farther north than Cortland, New York, which was just north of Ithaca. And this gave it a large enough industrial base to support various types of sections in various professional and technical groups.

The quality control group was quite successful and within a few years, we had two sections, one focused in the Binghamton area, and one focused in the Corning and Elmira area, where there were several plants, not merely the Corning glass works, but we had a number of other

plants in Elmira which were of some size and importance. As a result of this, I came in contact with a good many industries. In several of them, I offered in-house programs in quality control and statistics, for three or four of them, as a matter of fact, on Saturdays, which I'm sure was not always popular with the staff, who were forced to come in an extra day a week, but which seem to go very satisfactorily.

Other things we did at that time were to try to maintain closer contact and work out techniques of bringing industry onto the campus when that was necessary. We had, for instance, I remember we put on the plant managers conference one Easter recess, and brought on a good many plant managers from that general area and from a broader areas, for a couple of days' session.

Also during that time, I had our first exposure to the computer, as we got a, a tank, I called it, it was a card program calculator, manufactured by IBM, with whom I'd been doing some work. And this came to us via the Cornell Aeronautical Laboratory, which was located Buffalo, New York. And they had one and we're getting a larger computer, an IBM computer, I think the 301, which was the first sizable computer, if I have the number correctly in mind.

And this computer was an illustration of something else was going on at the university. Many of these veterans who had returned to Cornell, came under the GI bill, they could not otherwise have afforded, been able to afford completing their college education. One of them was Jerry Haddad, an electrical engineering graduate, who went on to IBM and designed that computer for them, soon after he was recruited from Cornell. One of the other man in his group was a fellow who went on up to Buffalo, to work for a firm up there. And then set up his own firm, and he design the first pacemaker for hearts. And actually, made very deal of money when he sold his company, which he started, for a great deal of money to another medical supply operation. And a great many of these people were exceptionally able, yet, they were not in the normal string of being educated in our universities and technical colleges. And it illustrated to me the importance of the Community College as a source of individuals, and when I subsequently became dean of the college of Engineering. I make every effort to establish relationships with the Community Colleges and draw these students into the mainstream of engineering education.

In 1953, I had my first sabbatical. At that time my wife was pregnant with our second child, and we went to Washington, to Bethesda, to the Operations Research Office of the Army, which was managed by Johns Hopkins University, and was located in a girl's school in Chevy Chase,

Maryland, which had been wired for security. And I spent a full semester there, and continued to consult with them throughout the summer which followed, but my wife was able to go home and have our second child in Ithaca at that time, in that summer of 1953.

That experience was a great experience for me, and extremely beneficial, because it enabled me for the first time to organize in my own mind, what the substance of industrial engineering should be. The critical element in this was the computer. As some of you old timers, and they're not too many left, may remember, performing a multivariate analysis was almost impossible prior to World War II. And anyone who's ever run Friden, Monroe or any of Marchant calculators, trying to do a four factor multiple regression analysis, a four variable multiple regression analysis, or a complex analysis of variance, realizes if you have 200 or more measurements, this becomes almost impossible on such machines. And many of the statistical manipulations that are so obviously mathematically oriented, and so obviously correct, the performance of these manipulations was very onerous and had to be done many, many times in order to ensure that there were no errors. And this exposure at the Operations Research Office to the most modern computers available at that time, and the subsequent, and subsequently through many of the projects in which they worked, opened up an opportunity for me to reappraise the whole field.

The greatest development, in my mind, was the potential of the automatic digital computer. And while at ORO, I realized that this made possible simulation of the various systems, period. At that time, when one designed a manufacturing system, there was no way of operating it in the abstract, before the system was completed. The first, one of the early issues of the Industrial Engineering Journal had an excellent article in it by a young man who had worked at a Michigan piston ring company, I think it was, during the Second World War. And he, in designing a new plant layout, had used Dixie cups, had used the planned times and determined the machine-- between machines -- storage required for that kind of an operation. Unfortunately, that's a very limited way of doing it, and the automatic digital computer opened up the full possibilities of simulation for industrial processes. Obviously, many of the problems would have to be solved by simulation or by heuristics, rather than by firm, mechanistic, mathematical models. Because the amount of computation for the simplest production control problems involved raising factorials to powers, and this was still not within sight, as far as the capacity -- or potential short term developments -- in the capacity of digital automatic, digital computers.

The second factor that was enthralling to me at that time had to do with the rationalization of these new developments in a fashion that we could comprehend, and develop further, and identify as far as the environment is concerned, and as far as the profession is concerned. The Operations Research Society of America had been started by that date, and I became somewhat involved with that at that time, more or less inevitably, because of my interest. The following year, in October of 1954, we had a symposium at Ithaca, at which, I'm not quite sure what the organization was, but the purpose of the symposium was define criteria for education in operations research. And the attendees, and I looked this up, it's interesting how much they overlapped industrial engineering. The attendees were Russ Ackoff of Case Institute, who moved on to Wharton, John Brainerd of Penn, one of the originators of computers, Camp from Melpar, a Westinghouse air brake subsidiary in Alexandria, which was early into industrial operations research, Cunningham from the Naval postgraduate school at Monterey, Davidson from Johns Hopkins University, Ellis Johnson from operations research office of Johns Hopkins along with Joe McCloskey, Merrill Flood who was then at Colombia and went on later to Michigan and did a lot of work in hospitals and health systems, Mel Salveson from GE, he had introduced the first large digital computer into an industrial environment for General Electric in the major appliance facility, Rob Roy of Johns Hopkins, he was currently dean of engineering there, Russ O'Neill from UCLA, Jack Mitten Ohio State, Whiten of MIT, Quaid from RAND, and Bob McGarraugh and, I, and Pete Morton from Cornell University.

I can't remember whether it was this symposium or the one a year or two later, where the first industrial engineering symposium, that was followed by a hurricane, but I remember it destroyed the departure dinner of one of them. The OR Journal was begin to grow and have good content, properly refereed at that time, and my hope was that industrial engineering could have a similar publication. My own conclusions were that, not only did I think that I knew what operations research education then demanded, largely graduate, but that there was a great overlap between that and industrial engineering or engineering. And the large impact, as I've already emphasized, is the impact of the computer on systems design. And my own conclusions relative to educational programs and the impact on engineering or industry were fairly well developed at that time.

I did not think highly of the Institute of Industrial Engineers at the time, because of the journal, which did not include many high quality articles as far as research, but mostly practice and empirical articles. The almost total absence of any good research in academia explained that. There were few shining exceptions. Hal Davidson at Ohio State wrote an excellent doctoral

dissertation. Adam Abruzzi, I think he was at Columbia, but I'm not sure of that, wrote a very excellent critique of time study, Jack Mitten, and there were others, Mel Salveson was one of them. In our own group, Ed Crick was beginning to do some research, and pointed towards the development of adequate models. And what we really lacked were people working in the area who recognized the new tools and their capabilities, and therefore could structure models in such a way that they would take full advantage of them.

The tremendous computational capabilities becoming available were of tremendous promise, but we did not know how to utilize them effectively at the time. And the development of quantitative models, relative to the systems, which fairly represented them and did not yield to the inevitable pressures on the mathematician who developed models to simplify the model by making assumptions which fundamentally were invalid assumptions, or fundamentally excluded critical parameters from the results. The second problem that we faced, all of us at that time, was a retreading of our faculty, and the development of the type of graduate student program which was capable of exploiting, in an advanced way, and along with the frontiers of such developments, the possibilities of the mathematical and statistical techniques that were already developed and available to us. And the third challenge, of course, was the development of new courses needed for both undergraduates and graduate students.

And I would like to, a little anecdote may explain one of the problems we have. By that time, I had recruited three or four faculty members, but in academia we were not then clearly attuned to the research potential or support potential from the government. And we needed to develop those pipelines, so that we were able to exert adequate pressure on the government to modify its goals to fit what this nation really needed, as far as the development of industry. We are today paying the price for having failed to do that effectively long, long before we actually got started on it.

Now, my technique was kind of stupid. What I did was schedule, at the start of a recess, and I think it was probably Thanksgiving recess, I scheduled a trip to Washington in my car, and loaded it up with five other faculty members. I made reservations for us in a good hotel, where we had three beds in a room, and had two rooms, connecting rooms. And I've forgotten the name of the hotel, but it became quite famous, and was very excellent, subsequently. And we drove down one evening, and I'm not sure what day it was, and we had all of the next day in Washington, and I had called ahead and make appointments for us in various bureaus. Early the next morning , we checked out of the hotel and drove down to they the Naval Research

office, where I had a few friends, and we had specific individuals for these people to see. So, they spread out through the various agencies, and saw the people they were supposed to see, and I went to the NSF and talked to the man who was then operating that, as well as the person who was in charge of engineering, or under whose control was the engineering function.

I talked to him quite a bit, and I've forgotten his name now, but he was very accommodating and I finally said him, how many astronomers do you have? How much you're spending on astronomy? And he told me how many millions of dollars are going on astronomy. And I said, how much are you spending in engineering in total? And that was far, far less. I said there are in this country, and I was up to date on the statistics then, I've forgotten them now, but I think 10 times as many practicing, professional engineers as there were astronomers. And I said, it seems to me your emphases are completely misplaced in this, that you're putting your money into areas that are not of sufficient importance, either academically or economically, to justify the expenditure. And so I said, I think you ought to review that. I said, now where would we submit proposals, where would we submit proposals down here, and hope to receive some favorable reception if they were quality, good proposals. And I got that information. So we drove back to Ithaca that night, and we had made a lot of new acquaintances down there. Our faculty had learned the way, and subsequently we began to derive some benefits from this type of proselytization.

The Operations Research journal, as I said, was still proceeding satisfactorily, and I was hoping that we would see more information, similar types of publications, in our fields in engineering. Now, the problem-- this all relates of course to the educational and professional problems of the field -- as far as external and environmental expectations, we had some serious issues to face in our profession. The typical person's concept of an industrial engineer was a time study man with a stopwatch setting rates, and this was far from what it should be. Now, I've forgotten who, specifically, convinced me that I should attend the next annual meeting of the Institute. But, I did. It was held in 1955 in Saint Louis, and I've never forgotten that. I went to Saint Louis, and the hotel, I don't remember the name of that hotel, but it was a pleasant hotel, and we were there for three or four days. Don Malcolm was then the president, had just been elected president of the Institute, Fran Groseclose was the past president, Joe Southern was executive secretary, and Bob Lehrer had become editor in chief of the journal, which Frank had done before.

I don't know, I was invited to attend board meetings, the directors meetings, and I did for all the time I was there, because I was amazed at how they went at their problems. My impression was of deep, genuine concern, a willingness to work with the problem until it was solved, and solved conceptually and also, more or less, unanimously. Only one person provided a bad impression of the Institute, and that was a personal issue with him. Whether the thrust was OR or IE was really not clear. The Lehrer article, the reference in the Lehrer article that fall in the journal was very interesting. The references were Rob Roy, Ellis Johnson, Dantzig, R. G. Canning, Willie Feller, Mel Salveson, Roger Crane, H.H. Rubinstein, L.G. Mitten, Allen Manne, Gifford Simons, Andrew Vazsonyi, H.D. Amrine, Everett [INAUDIBLE] and William T. Morris. Now, of that group, Rob Roy was an industrial engineer, and dean of engineering at Johns Hopkins, Ellis Johnson was a leading operations research man and ran the ORO. Dantzig was the man who had perfected the simplex method of mathematical programming. R.G. Canning was a leading data processing person in the country, Willie Feller thought it was a leading probabilist in the country, and had been at Cornell at that time. Mel Salveson was a man I've spoken of, who was an engineer, and industrial type engineer from MIT, who had done graduate study in the west, and introduced computers to the team. Roger Crane was the Touche Ross office in Detroit. Rubenstein, I don't remember now, which one he was, Jack Mitten was at Ohio State in industrial engineering department, Alan Manne had to do with mathematical programming, Gifford Symonds was an Exxon employee, an an Esso employee at that time, who had introduced programming to that corporation, and had written a very nice manual on it. Andrew Vazsonyi on the west coast, was the men who developed the gozinto chart, which any self respecting industrial engineers should recognize as an assembly chart, which had been used for three generations. And how we got away with putting gozinto, and naming it in that regard, without any industrial engineering response, I never understood. Harold Amrine was from Purdue, and ran the industrial engineering department there. Everett [Inaudible] was an industrial engineer, as was William T. Morris.

So, we have here a mixture of these two things, and separating them was rather hard. Ed Slagle became the president of the Institute in 1955-1956 and his acceptance piece make clear to spread, the use of the organization, and it's great achievements in the short time of seven years it had existed. In the fall of 1955, in October, the long range planning committee held the first of the series of symposia, and that was held at Cornell in the Statler, a year after the ORSA meeting. The journal, in the April 1956 issue, had five articles on time standards. And the editorial staff of Lehrer, Cecil Johnson, [INAUDIBLE], and Howard Emerson began to change that emphasis as time passed.

The first symposium focused on the definition of industrial engineer. And the following topics were the role of the professional, the development of industrial engineering, and industrial engineering publicity. The symposium in Ithaca was attended by Hal Davidson, Paul DeGarmo from Berkeley, Johnny Now from the American Management Association, Dick Forbert from Proctor and Gamble, Frank Groseclose from Georgia Tech, Bill Hodson, who was then vice president of the Methods Engineering Council of Pittsburgh, W.R. W. R. Kelty from Johns Manville, Chuck Lake from R.R. Donnelley, Bob Lehrer from the journal, F.J. Tittler, Austin Weston, also from R.R. Donnelley, John MacIlwaine from Atlantic Refinery, Matt Payne from Lubrizol, Ralph Presgrave from Woods and Gordon, Alex Rath from New York, I've forgotten whether its NYU or CCNY, Bob Rohr from Eastman Kodak, Rob Roy, Ed Slagle, and McGarraugh and I attended from Cornell.

Not all members of the profession subsequently agreed with a definition that came out of that session, but it was adopted by the Institute at a subsequent annual meeting. And again, this was a session where everybody approached the problem with great seriousness, and very fine attempts to be tactful and to get everybody on board, So that we could come up with a common conclusion. For some years after that, I served as director of research of the Institute, and we had 10 committee chaired by competent and hardworking individuals, who dealt with the various topics, and we used this for two purposes. First, to encourage research in the field, and secondly-- and thereby produce more publications for the journal. And secondly, to try to develop and establish a better annual meeting, where we could have technical sessions addressed to specific topics.

I don't remember when we started the Southern Tier chapter of the Institute, but I do remember the first meeting and it had been some years before that. We tried the same thing we did with statistical, with quality, American Society for Quality Control, and we did it in this fashion. We got a commitment from a restaurant, I think it was in Owego, New York, it was a very good restaurant. We got a speaker from Eastman Kodak to come, who was on a hot topic at that time, and had had received some publicity. And then we invited people from various corporations whose names we had, and we had access to them through the other societies, such as the ASQC, and were able to guarantee a pretty fair turnout. And, I've never forgotten it. We got to, for the technical talk after the dinner, we had everybody in a room, and I had two young men stationed at the exit to the restaurant and said, don't let any of them get out without paying $2 and leaving their name and address so we can get this thing on the road,

and they followed that very well. But when the technical session began, there was no blackboard. So, we took the table, turned it over, it fortunately had a green top, we found some chalk, and this gentleman from Eastman Kodak, very kindly, gave this talk and used the table as a blackboard to illustrate the technology he was talking about.

Now, I would like to say a few words about some of the major contributors, as I saw it, at that time. These Individuals were dedicated and participated fully, and brought... as far as I was concerned.... helped us build the industrial environment, the recognition of the Institute and of its members demanded. The first one was Warren Alberts, of the United Airlines, he was vice president of United Airlines. And I've never forgotten the first time I visited his office. One of our graduates worked, or several of them, worked for him, and when they'd come through Ithaca, they would say, some time when you fly from Chicago, stop off, we're right near the airport.

So, one day I did that. I went over to their headquarters and fortunately Warren was there, and I spent a little time with him. And I said, what are your problems now? He says, well our problem is we want good reservation system. And he said, we need a computing system to back it up, we know what we want in the reservation system. So he said, we have prepared this specification, and he showed me a specification which was as horrendous as a typical Pentagon request for proposals. And it told what the specs... the spec was a spec of what the system should do, and fully explain and fully explore. And I said, well who did you send it to? He said, we sent it to everybody that we thought would be interested. I said, do you have any responses? He said, we finally got one from the Bell System, but he said we've had nothing from IBM or any of the computing companies, and he said we must have that kind of input. And I said, well, what has Patterson done about it? Patterson was then the president of United, and he said he's going after Tom Watson. And after that, IBM made a proposal.

The fact of the matter was, that at that time, in my opinion, IBM did not understand what a system was, and had no interest in exploring such things as systems. Now, this was the heart of our definition, was the design of systems, and our problems were related to systems problems, where all of these interactions and probabilities were working. And, as I said before, the automatic digital computer provided not only ways for us to explore those and simulate those situations and develop the heuristics to solve them, but also the possibly of producing computing and memories which would handle them, handle an information system such as this satisfactorily.

Sometime after that, I went to see Chuck Lake, who had become president of R.R. Donnelley. And he is the gentleman who deserves responsibility, just as Warren Alberts and others did for developing large reservation systems, and thereby laying the base for large database systems. Chuck designed-- deserves recognition for the fact that he digitalized the printing of the world. And at that time, he was very concerned about doing everything digitally by wire. So, he was hiring. He was writing the specs, the specification of a system, and then was recruiting Kodak, Xerox, and IBM to solve those problems, and paying them to do the development work to produce the equipment which he needed.

A third star in our galaxy at that time, I always thought was Mel Salveson, who had done, who was one of the first men to really understand the modeling that underlies in industrial engineering, and the first to use large computers. Another one was Don Malcolm, for whom I've always had great admiration. Done was the gentleman who developed PERT for the Navy when he was at Booz, Allen, Hamilton and that was what enabled... and luckily he was working for men such as Admiral Rickover in that job, who had a problem and the PERT system solved problems scheduling the development of the reactors and so forth. And the other people that I had the greatest respect for were the individuals who first wrote good papers and theses, fully exploring the problem of work measurement, and how Hal Davidson and Adam Abruzzi spring to mind on that.

The earliest presidents of the organization that I had the pleasure of knowing were Frank Colonel, the colonel, Frank Groseclose, Don Malcom, Ed Slagle, Howard Emerson, Carson, and then George Gustat from Eastman Kodak. I've never forgotten the picture in my mind of the time we had a convention in Dallas, and we went out for dinner at some ranch near there, and George came riding in on a glorious white horse, and great cowboy hat, in the proper costume, I thought that was a great way for the president of the an Institute of Industrial Engineers to come in to a dinner. And, finally he was succeeded by Johnson, who built our student operations, our student chapters very well.

Another thing I've never forgotten was the convention in Atlantic City. This followed one of our symposia, which was held at Northwestern, and we ended up with some symposium with, at the company that was run by a Cornellian whom I had had in class, and I've never forgotten, that was the first time, he had a helipad on the top of his, on the roof of his headquarters building, which was quite high. And what I wanted to get a taxi to the airport, to get back to Ithaca he said, no Andy, its too late for that, you'll have to go by helicopter. He says, we have

a helipad up here, and I ordered one for you, and it'll be here in plenty of time to get you to the airplane. So, that was my first experience on a helicopter, and I've never liked the damn things, and I was not happy to be forced to do that, but it was either that or miss my flight. And I've never forgotten, I got up on the roof with this other guy took me up, and I was looking up the sky, and along, I said, there it comes, and the guy said, no that's just a crow. Well, pretty soon the helicopter appeared, and of course I crouched down and crawled into the thing and closed the door, and then we took off. And as we took all over the edge of the building, two things happened. One, the pilot said "boy, that's a thrill, isn't it, I haven't taken off from one of these tall buildings before." And I said, "yeah that is a thrill, and my door just came open, what do we do now?" So, he says, "slam it." So, I slammed it, and we went on to the airport. And there he dropped me right next to the plane. Of course, I had to go in and show them I had a ticket first, and I finally got on the plane, and we went off.

Now, at that meeting we had a lot of complaints about the journal becoming too technical. So, we had a convention at Atlantic City after that, and I remember at that time I was in Washington, doing something else. And I did go to that convention, and I think it was the last convention of the Institute that I attended. And I've never forgotten, it was held in the hotel on the boardwalk, and all the doors had screens in them, it was not air conditioned, and they had big slots underneath. And we heard about some, we got some inflammation of some flack that was being, would be flying at the annual conference meeting. So, and we knew that they were going to change the... had a motion, placing a motion before the conference to change the orientation very much away from the engineering, towards the management side. So, I remember talking to some..., and we were afraid the journal would suffer from this. So I've never forgotten Cecil Johnson, because this was worked out very effectively. Xerox had an exhibition there at that meeting, and they had their newest, high speed machines there. So, Cecil got typed up a statement, which cautioned everybody about the bad aspects of this resolution, or whatever, motion, or whatever it going to be. And we ran off a thousand of these, or quite a few hundred, and went around, slipped one under every door in the hotel. I thought that was an excellent way of dealing with the problem of that nature, and it was dealt with.

The long range planning committee of the Institute held a number of symposia. Harold Amrine hosted one at Purdue. Frank Groseclose hosted one at Georgia Tech. Wayne Deegan hosted one the at the University of Iowa, and Bob Lehrer hosted one at Northwestern University. I think I may have missed one at Georgia Tech, but I attended all of the others. In 1959, I made

a trip to Mexico along with Ralph Barnes, Ben Niebel, and Waite from Columbia University, to give some lectures in Mexico and Monterrey. And gained a little more knowledge about how some of the South American universities operated. It was an interesting trip, and gave us an opportunity to learn a lot about each other, and to enjoy a trip to Mexico. We met quite a few people, and get a lot of lectures and talks, and had a very good... I think the pleasant experience. I was very happy to meet Ralph Barnes, who I had never met before. At Cornell we're very proud of the fact that he was one of our first, received one of our first PhD's in industrial engineering. Harold Mogensen was also a Cornell graduate, and so was Mike Maynard. So the leading proponents of work measurement, of methods and of predetermined elemental times, all graduated from Cornell in our field.

I'm especially impressed by what Mogensen did, and much of the Japanese quality circles stemmed directly from Mogensen's techniques. The addition of the use of statistics is merely the addition of another tool in the kit of those that are concerned about methods and ways of doing work. Some time after that, I became a member of the board of directors of the Engineers Council for Professional Development, and represented the Institute on that board. And as my term was drawing to a close, Pete Lohmann became the president, and we had at that time on the board, Dick Forsberg from Proctor and Gamble, John [INAUDIBLE] from the American Management Association, Pete, and myself. So, we had a nucleus, which required recognition of the importance of industrial engineering in the spectrum of engineering education. The major function of the ECPD at that time, was the accreditation of engineering curricula across the country. And a number of questions arose. One, for example, was the conflict between industrial engineering and systems engineering. Not a conflict in reality, but a conflict in terms, and in some instances, praised by institutions which wanted one or not, but not the other. And I believe we dealt with that satisfactory.

But the biggest accomplishment at that time was Pete Lohmann's accomplishment in setting up the advanced accreditation system. At that time, the ECPD was fully responsible for all engineering accreditation. We held many meetings and very extensive discussions of this, a number of papers were written, and it ended up with Pete sending to every engineering dean in the country, a packet of papers, a bound volume of papers, about the size of the telephone book, with the proposals to accredit degrees, advanced accreditation, with the highest level named degree being the accredited degree. And the theory was that we needed, with the addition of the computer, of the new mathematics that had to be added because of the computer, of the new techniques that joined... and of the new methods that joined our

curricula, that we needed more time in order to have adequate design exposure. And that more of the undergraduate years would have to be required to lay the foundation, and I think the response of industry to this in general was very good. So, this was passed by the board, and we had a national convention, at which all of the voting people voted in favor of this advanced accreditation.

Shortly after that was done, ABET took over the accreditation of all curricula in the country, and the introduction of that organization, along with a great turnover that seemed to take place in deans of engineering across the country, really put the kiss of death on the advanced accreditation. And this was a bad thing for the professional... for the recognition of professional engineering across the country, and especially for the distinction of engineering curricula. It was, in large measure, due to the objection of academic deans, who very often, were not engineers, but had been mathematicians, business scientists of various sorts, who had limited engineering experience or knowledge in industry. And we have been faced with the unfortunate situation this country, where most of our engineers are employed by corporate industry and manufacturing, and not by communities such as a civil engineering, where licensing is absolutely essential and necessary. So the accreditation is the only means of defining... the accrediting process, is the only means of defining and setting standards for the profession. And it's a shame that that did not work. In my opinion industrial engineering is systems engineering, and in our definition, we recognize that. And I noticed that many of the systems engineering programs in the straitened circumstances of current years, have disappeared, and it is being recognized in more and more institutions that it is the IE program that properly deals with these topics.

I would like to conclude with one comment. I said at the start there were three aspects to it. One is the substance of engineering. In my lifetime, in our field, it has changed drastically, and I have tried to outline how it has changed. It is much more quantitatively founded, we are using entirely different tools and techniques, and we have entirely different problems to deal with. The second aspect of it is the profession, and in that we have not done an outstanding job in defining the field, and at least, as far as getting it recognized by the environment. In a sense, it is this third element that seems to have failed the country. At one point we were turning... we were graduating from our institutions of higher learning, very competent engineers to deal with a manufacturing problems of American industry. Increasingly, engineers disappeared from the top ranks of executive officers, and marketers, and financial people, and lawyers took over. At one point, when I was very young, a majority of CEOs were graduate engineers. That is no

longer the case.

In that, as what followed along, at one point, industry began to be very concerned about computing. We were fortunate at Cornell, since we were, before we started our Department of Computer Sciences, we were the largest consumption of computing capacity at the University. And many of the early software programs, and others sense have been developed by the Department of Industrial Engineering and Operations Research, or Operations Research and Industrial Engineering, it is now termed. And this meets my conclusion that the two are very similar, and that one is much more important at the graduate level than at the undergraduate level, and that is true to today. Most of our students who go into the OR field, do take at least a master's degree, and some more. The typical graduate from the institution, in recent years, has not gone into manufacturing, because manufacturing corporations are not interested in paying and in giving those people the ability to deal with their problems. Instead, many of them have gone to Wall Street, and some of these so-called rocket scientist that you read about in the newspapers, who are working out these software programs to deal with various financial instruments, are our graduates, who leave with masters degree. A number of them have progressed tremendously, financially at any rate, in that field, and a good many of them operate it.

Now, we would much have many of those individuals, with their capabilities and intellects, operating in the manufacturing area. But in this case, industry has failed us, and I think that this is in one major, the cause of our failure in manufacturing in this country. The manufacturing economy of Japan does not utilize anything as far as technology is concerned, that was not developed in this country. Some of these things have been neglected for decades by American industry. Between 1942 in 1946, every American firm that did anything for the Defense Department, or produced anything of that nature was utilizing statistics and statistical quality control. They had to demonstrate a quality control process, otherwise they were not fulfilling the terms of the contract. Many of those firms dropped that, the minute the war was over, just when Japan was taking it up. There are many other examples of that nature that we can hit. And I think that there's no question that the failure of the manufacturing is much more than a failure of technique. The techniques and the technology have always been available, but most managements are completely unaware of that fact. Until they are aware, and do produce the jobs for the people who are competent in this, we are not going to see the numbers of them that are needed. Students vote with their feet. They go to where the rewards are greatest. They are very adaptable and very intelligent, and they move to where they can

do the best thing for themselves, which is an entirely understandable procedure.

And I think that until we solve some of these problems, which are basically in the engineering environment, that we are not going to deal effectively with this. Now, I will not say anything about unions, but they also are a part of the problem. And this applies not merely to industrial unions, but to government unions as well. And there are ample examples of this, and this is another part of the problem that is managerial in nature, and must be dealt with before we can deal with this. The third element in this problem is Congress, and the loadings that they place on our industries in this country, which increase the overhead cost per direct labor hours. And one of the factors that people don't seem to recognize, that inhibits against our competing effectively with countries the younger populations, who are capable of utilizing the same technologies we have developed, who could come in and use the same techniques that we use, but who do not have to carry the load that we must carry for social reasons in this country. And this is one of the things of beating England, and beating the United States, and will also beat Germany and France as well, as the years pass. When the Eastern countries, when they open up, where they expect these things to be done, why we're going to see some very interesting times. And I think the challenge for the industrial engineer is greater than it has ever been, but on the other hand the techniques that he has at his command are also much more effective than has ever been true in the past. I hope this fills your needs, Cecil.