Frederick C. Lindvall

—How It Was

Frederick C. Lindvall, professor of engineering emeritus, was interviewed by Ann Underleak Scheid for the Oral History Program of the Caltech Archives. E&S *has made a shortened version of the original transcript and presents here Part Two (of two parts).*

Ann Scheid: Could we talk about what you did during the war years?

Frederick Lindvall: Well, shortly before Pearl Harbor, I got involved with Dr. Charles Lauritsen's group. He was much impressed with what rockets might do. He had a chance to learn something of the English experience with rockets, and he came here and got projects started on them. It was all very hush-hush. I was asked to help on this, and I started devoting part time to it. Then when Pearl Harbor came, I went full time on it.

My particular responsibility was rocket launchers, and at that time we had them for land use, ships, and aircraft. Later on, Carl Anderson, who was also working on the project, took over the aircraft-type launchers, and I continued with the shipboard launchers, particularly those for landing craft and some for land and amphibious vehicles. We would get up early in the morning, drive out to Goldstone Lake and do our test firing, and then drive home again, getting home after dark.

Then the Navy came to Dr. Lauritsen with torpedo problems. The Mark 13 aircraft torpedo was not performing as it should. I was asked to form a group to work on the torpedo, so I dropped out of the rocket business. The big problem with the Mark 13 aircraft torpedo was that it was dropped from aircraft, but there were such limitations on the speed at which it could be dropped and the altitude from which it could be dropped that the torpedo planes were virtually sitting ducks for the anti-aircraft fire from the ships being attacked. Also the torpedoes wouldn't run properly after they got into the water. They would suffer internal damage and would broach the surface of the water and run in a crazy path. We built a launching facility behind Morris Dam to simulate water entry. It was literally a long tube down the hillside, and we blew the torpedoes out with compressed air so they would enter the water at whatever speed we wanted, depending on the amount of air pressure we put behind them. We would study the underwater trajectory and examine the works afterward to see what the internal damage was. We also developed instruments to determine the kind of accelerations that were occurring in different parts of the torpedo at water entry. We explored various head shapes too, to see if anything better for water entry was possible. It turned out that the existing head shape was pretty fair, but we discovered that the tail structure exerted quite an influence in controlling the entry. The more tail structure we had on the torpedo, the better it behaved. So we came up with the idea of a shroud ring that went on the tail of the torpedo.

Another group at Caltech was working in the water tunnel, and they found a good profile for this ringtail. We machined these, put them on torpedoes, and stabilized the water entry. We also made certain improvements in the way of the mounting of the equipment that kept things running, as the term goes, "hot, straight, and normal." Then we conducted tests at sea from a carrier that let us use its planes. We loaded them with these modified torpedoes, and then the torpedo planes fired them at their own ship. Of course, there was no explosive in them. They were set to run deep, so if they ran properly, they would run under the ship. The performance so impressed the skipper that he wanted all the modified ringtail torpedoes he could get his hands on to take out to Pearl Harbor, which he did. He demonstrated them to Admiral Nimitz, and Nimitz ordered the Bureau of Ordnance to modify some thousand Mark 13 torpedoes. Since the Bureau of Ordnance hadn't heard much about what we were doing, it was quite upset, but we were able to work directly with the fleet here at Caltech and not have to go through the cumbersome Washington machinery.

Later, I was asked to split my engineering group to assist the Manhattan Project people. So we divided the group and added some more engineering people to it and took over part of the manufacturing facilities we had acquired for rocket work out on East Foothill Boulevard. Our principal mission was to develop a backup fusing system for the A-bombs. Our version didn't have to be used because the Los Alamos version worked, but nobody knew until it was tried, and they had to be sure.

AS: So actually Caltech was involved in manufacturing at this time?

FL: Oh, yes. We modified several hundred torpedoes before the Navy got its own production going, and Caltech manufactured well over a million rockets that went into service. It was done in shops all over the place; wherever we could get any machine time, we would contract for parts. The Caltech people — principally our chemical engineers Bruce Sage and Will Lacey — set up the powder extrusion facility in Eaton Canyon to make powder grains for the rockets, and that was a big operation.

AS: Were you getting contracts from the government?

FL: Yes, we worked under the National Defense Research Committee (NDRC) and later on for something called OSRD,

Fred Lindvall and students at the weekly Winnett Center coffee hour in 1964

which was the Office of Scientific Research and Development. At that time, Richard Tolman was one of the principal people in that operation in Washington, along with Vannevar Bush.

AS: What was happening to the regular activities of the Institute?

FL: Well, there were special war training programs, in which people taught things that were perhaps a little elementary — drafting, elementary electronics, and the like. We got instructors wherever we could, people who were teaching in high schools and junior colleges.

AS: So the training became less academic?

FL: Yes, but there were, of course, a few of our regular students, and then we got the Navy V-12 program, and a whole group of students transferred from Stanford to Caltech, so there was a sudden increase in the student body. The V-12 program was mostly engineering with some work that was relevant to Navy things — some on ordnance, some on navigation principles, things of that sort. These students were going to be commissioned directly into the Navy.

AS: How did this affect your teaching?

FL: I wasn't doing any. I was working on rockets and torpedoes at this time, but Professor Sorensen kept on teaching, and Professors Maxstadt and Robert Daugherty, and a number of other people who, for one reason or another, didn't want to or didn't fit into what was going on in the Caltech contracts. They kept on teaching and doing almost double duty.

AS: Did you do quite a bit of traveling in this period?

FL: Oh, yes, to make installations of rocket launchers on support boats that were going to take part in the African invasion. I went to Norfolk, Virginia, for that. We conducted some test firings with these special rockets — barrage rockets. They were quite inaccurate as far as trying to hit a target, but for barrage purposes — bombarding a beach prior to landing — they were quite effective.

AS: Did you really go out on a carrier for your torpedo testing?

FL: Well, actually, we were in a Navy blimp looking down to watch the torpedoes running. That was off the coast of southern California. And there were lots of trips to Washington and some to Los Alamos.

AS: Were you at Los Alamos when the bomb tests were made?

FL: No, but one day while I was on the Manhattan Project I got a call from a Caltech graduate who had been a Naval reserve officer and was back in uniform. He was with Navy procurement in Los Angeles, and he wanted to know if I knew a Dr. Benioff in the Seismological Laboratory. "Oh, yes," I said, "I know Benioff." "Well, I have a secret dispatch to deliver to him, and would you mind coming along and identifying him to me?" So we rode up to the lab in his Navy car; he, incidentally, was wearing side arms. He made me describe Benioff before we got there. Fortunately, this was one of the times Benioff was wearing a mustache.

AS: He kept shaving it off and growing it back?

FL: Off and on. But the officer delivered the message and got the signature for it. Actually, the message was a request for Benioff to watch at the time of the Trinity shot to see if there was a recordable bump on the seismological record. And there was. And that's as close as I got to the actual blast.

One of the things we were asked to do at Caltech was to make replicas of the A-bomb, and we built them from scratch. They were simply TNT bombs, which would be dropped as decoys, and the real one might come down right along with them — if they wanted to play it that way. We manufactured quite a bunch of those, and they turned out to be pretty potent TNT bombs in their own right.

AS: Millikan was still head of Caltech. Was he really running things in this period?

FL: He began to sort of lose touch with things because the business office was so filled up with contracts. After all, we were handling millions of dollars worth of procurement contracts, and scads of non-Caltech people were on the payroll. Right after the war a couple of business types that had come in on the rocket procurement moved over into the business office and began to bring order out of things. Originally, Millikan and Ned Barrett, the secretary-treasurer, ran things pretty much out of their pockets. There weren't good records or good systematic dealings with the faculty. Once in a while I might get a letter saying that my salary had been increased, but a couple of times without a letter I discovered that I had had an increase because the deposits they were making for me at the bank were bigger than they had been.

Shortly after the war was over, Dr. Millikan asked me if I would become chairman of the engineering division. My major responsibility was to build up graduate work in civil and mechanical engineering, which had not had the kind of development that electrical had had; nor had they gone the way aeronautics had under von Kármán. Von Kármán was then director of Guggenheim Laboratory, and he certainly didn't need me for a boss. Aeronautics ran practically as a little division by itself, though I had to see to it that they got their salaries, come budget time.

So there was the building up of mechanical and civil engineering, and, of course, in the meantime electrical engineering was growing, particularly in the direction of applied science. It became fairly clear to me that if engineering was going to survive at Caltech, it could not be second-rate. It would have to be pretty close to science. So I tried, rightly or wrongly, to steer things in the direction of applied science, and tended to appoint people whose research was of that nature rather than nuts-and-bolts engineering.

AS: I'd like to talk about engineering education a bit. You were president of the Society for Engineering Education, so you were quite involved in devising the engineering curriculum. When did you become interested in that?

FL: Well, on the campus it was a gradual transition as some subjects ceased to be of interest or relevance. We gradually shifted our emphasis here to put more stress on the theoretical and fundamental background of engineering. So one course after another was introduced that carried on this shift.

AS: Could you be more specific about the kinds of courses that were dropped?

FL: Well, for example, it was traditional over many years for engineers to take surveying. That was dropped. Mechanical drawing was also considered necessary, but we gradually phased it out as a requirement and made it optional. Engineering design, which was really machine design, no longer seemed to have a place here, so it was gradually phased out except as an elective subject.

Over the years undergraduate work absorbed more and more of what had been graduate work 15 or 20 years earlier. So the general level of mathematics competence had to be built up in the undergraduate work. Another aspect of the undergraduate program that eventually disappeared was shop work. Many engineering schools hung onto it much longer than Caltech did, but while it is good to know about manufacturing methods, how to run a lathe, build a machine, know a little something about a foundry, there isn't room in the curriculum for teaching it.

AS: Did the caliber of the students change?

FL: The level of the entering students was improving all along, particularly in mathematics. Many of them were able to start in with advanced placement. In the engineering curriculum we introduced some applied mathematics, which would carry on from the usual calculus and differential equations into applications of these subjects. We also introduced some new elements, such as La Place transforms and functions of a complex variable.

AS: Did you ever have trouble as head of the division recruiting faculty to Caltech?

FL: No, no. Always there was the problem of whether it was the right man. And, in general, we tried not to recruit a man for a specific teaching responsibility but to find a good man in a general area and then let him work out his own set of courses and his own research program. We've always wanted to look for outstanding people first because they turn out to be quite flexible.

AS: What did you see as your basic responsibilities as division head?

FL: To encourage people who came in with good ideas or a line of research that seemed promising. I tried to act in a permissive and encouraging way rather than trying to direct anything. There were a few key faculty people whose judgment I trusted, of whom I would ask questions. We didn't usually get together as a faculty and meet formally on things, nor did we have very many meetings of the division as a whole. And when we did, to talk about some curriculum or policy matter, we never voted. I would listen to the discussion and make up a kind of consensus.

AS: Is that typical, do you think, of the divisions here?

FL: Some were much more formal. But the more formality you have, I think, the more chance there is for divisiveness.

AS: You took a trip to the Soviet Union, I believe, and looked at their engineering education system. When was that trip?

FL: In 1958, right after Sputnik. In fact, as we were finishing up our visit, we had a session with the minister of higher education. I asked him, "What is the curriculum that trains people to produce Sputniks?" He shrugged his shoulders and said, "People like that just emerge."

AS: You didn't find that their curricula were that different or their method of selection was that different from ours?

FL: At that time they had their engineering broken into about 160 named curricula — as specialized as, for example, diesel engineering for stationary power plants and diesel engineering for locomotives; there was railway engineering, railway civil engineering, railway mechanical, railway electrical; there was also power engineering — the whole gamut of things. Students would try to get into the options that were the most glamorous — that is, communications and electronics, but only the very best students were selected for those. So there was a kind of built-in screening process, and the students who knew they weren't really tops would opt for one of the less rigorous disciplines. And each year the overall planning would specify that they needed to have so many new places in the curriculum in nonferrous metallurgy, for example. So the word would get around that there would be some openings in nonferrous metallurgy, and somebody who might have wanted to be in the steel business would decide, "Well, maybe I can get into nonferrous and maybe later on I can make a shift," or something of that sort. We also found that the schools we thought would be good, bad, or indifferent, were indeed that way. The farther from Moscow or Leningrad we went, the lower the general quality. The top professors wanted to be where the action was, namely, Leningrad, Moscow, or Kiev.

AS: Were there significant differences in the Russians' preparation before they got to the university?

FL: They had been pushed along a little more in mathematics than some of our engineering schools required. A foreign language was something that was encouraged. Of course, some of them automatically got two languages — their native language, which was, say Lithuanian, but they also had to have Russian for college. They had more training in drafting, shop work, and things of that sort. And they usually had to have at least one year of practical work in agriculture or factories before they could even go to college. Also the engineers had a design project, a design thesis. They would work part of the year in an industry, and then at the college they would work on completion of the project that they had started in industry. Then they would have to defend that design before a committee of faculty and outside engineers.

Their curricula were essentially five years long rather than our typical four-year program, and also the system provided carefully programmed correspondence work. It was possible for students in the correspondence courses to shift into regular academic programs at various stages in their development. The correspondence work might cover certain elementary things, such as basic physics, chemistry, and mathematics. And when a student had passed suitable examinations in those, he would be admissible to one of the colleges in a particular option.

AS: What was the group you went over with?

FL: After I retired as president of the Society for Engineering Education, I selected some people who were keenly interested in engineering education — a group of about eight — and we went on what was called a State Department Exchange Mission. It was funded by the National Science Foundation.

I might point out that the engineering in the Soviet Union was at that time, and I think is still, taught in engineering schools and not in universities. There were three or four engineering schools in Moscow, but not at the University of Moscow. That school had science and certain fundamental subjects, mathematics and so on, but not engineering. They had an engineering school for electrotechnics, for power, and for telecommunications, but I understand that since then some curricula have been developed that are quite broad and would be comparable to what we have in, say, Caltech and MIT. One thing was quite evident — all the engineering schools had excellent libraries of foreign books and current magazines, from the U.S. and the U.K. particularly. English was taught quite generally to engineering students.

AS: Was there anyone in your group who knew Russian?

FL: Yes. Leon Trilling, who was one of our Caltech PhDs, had grown up in Poland and had learned Russian as a young man. He was very helpful because the rest of us were dependent on interpreters. We'd sit around a table and talk, and if the interpretation was coming out the way Leon thought it should, he would sit quietly. On the other hand, if there was a misunderstanding or a misinterpretation, he would begin to fidget and burst in. But in general, we found that it was better to play down the fact that we had a fluent Russian speaker in our group.

AS: At Caltech you were also involved in committees on cooperation with industry and patents, weren't you?

FL: Yes, and in the early days of the patent committee, we had some real problems. This was because some interesting propositions were made to us by industry that would have tied us up in terms of secrecy. The people who would be working on some such project would not be able even in lunch table conversation to talk about what they were doing. It was almost as bad as having secret military work going on. So we established a policy that we would not take work that required any inhibition of what is normally called freedom of discussion. We wanted our students who'd worked on this research to be able to present it in seminars, write it up in their theses, and so on.

Also we had to work out a policy. We never had a patent policy before we had a patent committee. Was there any way in which a discovery of some sort could be assigned to the sponsor of the work? At first it seemed awfully difficult, and we were very stiff-necked about it, but over the years I believe that has relaxed somewhat. At that time our biologists were very strong in the belief that since many of their discoveries were health-related they really didn't want anybody to make a profit out of them — which was a little difficult to reconcile with the fact that the biologists would happily accept research grants from pharmaceutical houses.

AS: Do the patents ever accrue to Caltech as an institution rather than to a private individual?

FL: Yes. Under government contracts, the government has the option of first refusal. If the government agency decides it does not want to prosecute a patent application on a particular discovery, the college is free to do so if it wishes. And in some instances, that has been done. Originally, there were patents on a vacuum switch of Professor Sorensen's, which were assigned to the college with certain rights granted to General Electric Company because it had sponsored some of the work. There was an orthodontist here in town who made a lot of inventions on his own — things that were useful in orthodontics — a little tiny spot welder and various braces and things of that sort. He turned those patents over to Caltech with no strings, and a company was organized on the outside that made these things and sold them to dentists. Royalties were collected over quite a few years on these particular patents. The vacuum switch patents just lay dormant until the state of the art in vacuum technology and materials developed to the point where the vacuum switch could be a commercial product. But by that time the basic patents had expired.

We tried to encourage industry to suggest lines of investigation that would have some value to them, preferably in a broad rather than in a specific sense. And we tried in all ways to improve relations with industry on recruitment, getting industry to send its representatives over here, encouraging seminar talks, encouraging student-society talks with industrial representatives. We did all of those things, recognizing that most of our engineering students went to work for industry. And, after all, we were always passing the hat to industry for funds for general purposes, such as the Industrial Associates program.

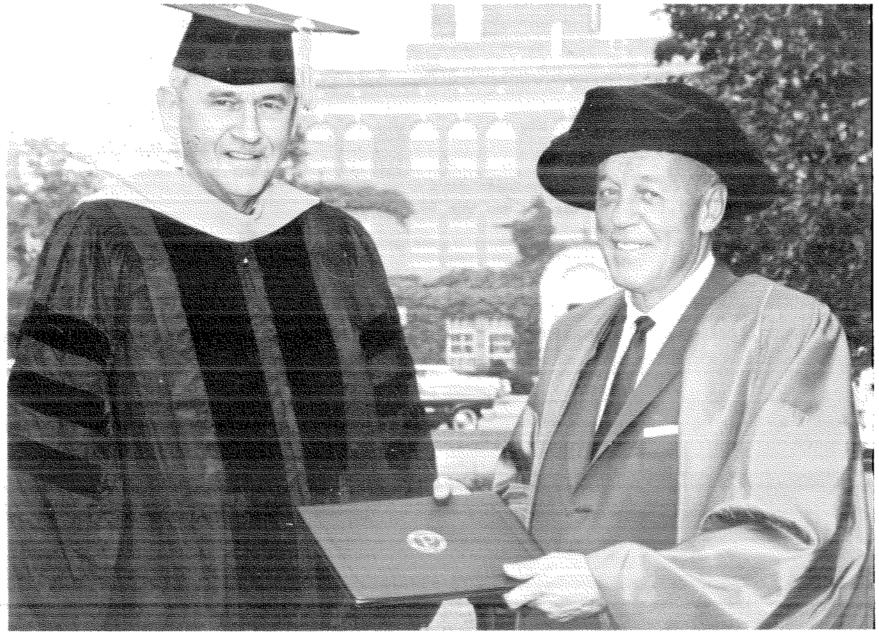
AS: You made a couple of other trips, particularly one to Africa, which sound interesting. What was your purpose there?

FL: I went along to see what the engineering education situation was in the countries we visited and to try to assess to what extent that kind of education and research was helpful in their economic development. We spent a lot of time in South Africa, where they were taking care of themselves quite nicely; but in some of the other countries what was going on was too much patterned on the old British colonial schemes.

Many of the engineering students were being taught an advanced type of engineering that was not immediately useful in their countries. Most of the developing countries needed a lot more of the how-to-do-it kind of engineering. They needed roads, railways, drainage systems, and safe water supplies — all the things that are just the necessary infrastructure of a country. Over there, the universities felt that they were a little above that sort of thing. They were teaching engineering with the object of having their students pass professional engineering society examinations that were set by people in London.

AS: Did they have trade schools too?

FL: They had some vocational type schools. In Kenya at that time, there was a quite good training school operated by the Department of Telecommunications, to train people to service telephone, telegraph, and radio systems as well as the signals of the railways. The man who showed me around there said, "One of our big problems is that our students are grabbed up by private industry to service radio and computer equipment, and we



Recipient of an honorary doctor of engineering degree from Purdue University in 1966

don't get them into the government service for which they've been trained."

AS: You were with a group of people from Caltech who were in different fields?

FL: Let's see, we had Professor Munger, who was really our leader. He's a political geographer. And there was Horace Gilbert, in business economics; Robert Oliver, who was more in general economics; Thayer Scudder, who is a student of African culture and anthropology; and I went along to look at engineering and as much of industry as I had an opportunity to visit, and at industrial-type labs and research labs sponsored by governments in developing countries.

I found in two or three countries efforts being made to use waste materials to get by-products. I believe it was the waste material from the cashew nut that was capable of producing a fair amount of alcohol that was adequate for industrial purposes, and they were trying to make that economic. But it was very difficult to have people accept the concept of making do with what they have rather than hoping for money to buy something like equipment. Everywhere there was a big desire on the part of the ruling people to have a steel mill, for example. That was a big symbol, and often that kind of thing took priority over the infrastructure that was needed to support the economy.

AS: You also traveled to India, I think.

FL: Yes. Caltech was one of a consortium of about eight engineering schools that got together and established, and for a time helped staff, an engineering institute of technology at Kanpur, India. This was a program of the Indian government, and our own AID organization was backing it financially. Caltech had two or three people who were there over a period of time helping build labs, organizing and teaching courses. After about five years, the consortium thought that several people who had not been part of the Kanpur operation should go there and see how it looked, whether any progress had been made. I was asked to go on that mission.

AS: What did you try to evaluate?

FL: Basically, whether they were doing a good job. Had they been able to recruit and hold good faculty people or were they still too dependent on faculty from the States, and too dependent on the U.S. for equipment and supplies? We felt that Kanpur was capable of doing a somewhat better job than they were, but they were held down by the Minister of Education, who didn't want Kanpur to be better than any of the other institutes of technology. They couldn't get out of line with the others on salaries or equipment appropriations.

We were also interested in what they were doing to develop worthwhile relationships with industry. Where did their graduates go to work? Did industry employ them? Did industry sponsor any kind of research activities or specialized education? To a considerable extent, industry and the universities were miles apart, but we saw signs that they were coming together. Efforts were being made, at least at Kanpur, to make the work relevant to industrial needs. But there was always the problem of whether they should be helping on today's problems in industry or the problems industry will be facing later.

AS: You retired in 1970. What have you done since then?

FL: At the time I reached the age of administrative retirement here, I was offered the opportunity to go to Deere & Company to establish the position of vice president of engineering at the corporate level. In the three years I was there, I tried to make recommendations that would generally improve the engineering situation, particularly the interchange of engineering ideas among the different factories. Each factory did all its own engineering, even keeping secrets from other Deere & Company factories. Now at Deere they have a central engineering laboratory, which does not design new products or tell the factories what to do, but it solves problems that the factories have and are not equipped to handle, such as in materials or methods or design details, fracture mechanics, better methods of planting seeds — things of that kind.

AS: That was just a temporary position?

FL: Oh, yes. The chairman there, Mr. Hewitt, who's a Caltech trustee, said to me, "Fred, you're at an age when you would be retiring from Deere, but by just coming on board now, the young fellows won't regard you as a threat." And that was helpful because they did cooperate with me, knowing that I was not a threat to them in the internal corporate politics.

AS: What have you done since you came back from Moline?

FL: Well, my son started a consulting firm — Lindvall, Richter, and Associates — that advises its clients on the ground motion that can be expected in the event of an earthquake of a given size and location. Then the client can have his designers check out a proposed building design or dam design to see whether the specifications are adequate to meet the postulated design earthquake. We analyzed the Big Tujunga Dam for the Los Angeles Flood Control District, for example, and we've looked at other dams for the Metropolitan Water District. The Richter of the company is Charles Richter, also retired from Caltech, and nominally I'm the president. Since I don't pretend to be a structural engineer or a geologist or seismologist, about all I can do is talk with them along general lines. And I provide a shoulder on which people can cry, but I always did that when I was division chairman at Caltech. □