



**Interview with Myron Tribus, Ph.D.
Former Professor
January 21, 1996**

Interview conducted by Professor William Van Vorst

Van Vorst: Myron, I'd like to start by getting you to give us a little background of yourself, your high school days, college. Why you went to Berkeley, why you came to UCLA.

Tribus: Well, I was born and mostly raised in San Francisco, went to the high school of Commence and graduated in 1938. I had, at that time, fallen in love with the field of chemistry and intended to be a chemist. Frankly, I went to Berkeley because it was the only school I could afford. Also, I won a scholarship to go to Berkeley. Now you have to remember in those days, the economy was different. At UC Berkeley I lived on \$425 a year: room, board, books, tuition, dates, entertainment and clothing. So a scholarship of \$150 or \$200 was very significant in my life.

I studied in chemistry and I was a good student. I've always been a good student, and at the beginning of my junior year I went to my advisor who was Professor [Wendell] Latimer and told him I thought I ought to broaden myself—that I was doing well in chemistry, but I didn't really know which electives to take. He said I should take something in engineering. So I took a course in fluid mechanics—ME 103—I was already required to take a course in mechanics. He said something about Boelter being a good teacher and I might enjoy heat transfer.

So I took ME 151, Boelter's famous heat transfer course. I don't believe I had been in that course more than two or three weeks before I found there was something about the way he taught and what he did that was so exciting to me. Suddenly, this is what I wanted. I didn't want chemistry anymore! I went to him and I said, "This is great, what else can I take?" By then, I think it was the second half of my junior year. He told me about a course in radiation, a course in heat and mass transfer, and I immediately signed up for those. I can't remember all the details, but he allowed

me to take a graduate course even though I was an undergraduate, because by then it was clear I would have to go into the military upon graduate. He also hired me on a project having to do with heat transfer in heat exchangers. He also put me to work on some papers that he coauthored with Norris and Stride on high efficiency fins for heat transfer. I don't think I contributed anything, but to my surprise he put my name on as a coauthor.

Also, Boelter had started a series of evening gatherings at his house. There were several other people there and we would talk about many things. I can't remember the topics, but they were what today we would call "socio-technical" topics. They were technical topics and I think you went to some of those meetings, didn't you?

Van Vorst: Not at Berkeley.

Tribus: At any rate, there were other people like Warren Geicht who attended. Dean Boelter hired me on his research project. We went to ASME [American Society of Mechanical Engineers] conferences; I was now taking all those courses from Boelter. A heavy, heavy dose. Also some taught by Joe Gier and Ray Martinelli and Earl Morrin. I thought Boelter's approach was fascinating. For example, I remember his first exam because out of 100, I got a score of 17! Now, I had never had that kind of experience before in my life. Then I discovered that I was high man in the class! Boelter explained that he never gave problems to which he knew the answers! He wanted us to teach him! His problem sets were all so practical. You were given a picture of a glue pot from a catalogue, and you were to look at the picture, the stated voltage, and so forth, and asked to calculate at what temperature the glue pot would come to equilibrium. Or how short can a welding rod be before the welder can't hold it anymore. He gave us problems which were so different than anything that I had ever experienced up to that time. I was absolutely hooked by him, studying everything he gave.

In fact, for the research project I had to go to the library to find and organize information. I was so determined to look good in his eyes. I think that's part of his strength as a teacher. You just felt that you had to do a good job. He had a famous phrase. If you were doing something he'd say, "Well, what is the contribution? When you are finished doing this, why does it matter?" Most people didn't challenge you the way he did.

Van Vorst: And you felt you were letting him down if you didn't. [laughs]

Tribus: That's right, and I worked so hard at the library doing my first full-blown paper in my own name on the properties of gas mixtures. Remember, I was only an undergraduate at the university, but I was going to publish a paper based on my own research. It had to do with the viscosity of gasses and gaseous mixtures. I went to the library and studied the theory, got all the references and cross-plotted everything. I actually went blind. That is, I had to go to the hospital for about four or five days because I couldn't see!

Van Vorst: Really?

Tribus: I worked so hard for this guy. In fact, my eye troubles began there—I didn't wear glasses before then. Boelter just inspired me. Well, I graduated in May 1942 and on July ninth I was called into service. Behind the scenes some letters went back and forth. Almost everybody in my class went into the field. I was in the ROTC [Reserve Officers' Training Corps]. Everybody in my class went to real combat, but I was sent to Wright Field in Dayton, Ohio.

I had this intensive training in advanced heat transfer. Very quickly, I became the major consultant in heat transfer at Wright Field. There were about 2,200 officers there, all technical people. This was the center for the design of new aircraft and monitoring of all the contracts in aircraft. I was heavily involved in that, traveling to different parts of the country, talking about heating in cooling and ventilation of aircraft, deicing of aircraft, and things like that.

It was clear that what Boelter had taught was extraordinary. I didn't understand at that time, but there were no true centers of heat transfer research other than [William H.] McAdams' group at MIT, [Allan] Colburn's group in Delaware, and a very new beginning group under Mike Shahin at the Illinois Institute of Technology, and then Boelter's group. Now there are other places where people talked about heat transfer, but there was no center of excellence. These were people who had written fundamental papers whose names were known internationally. There just wasn't anybody else at Wright Field with the training I received from Boelter.

I got involved in engine cooling. I got involved in oil coolers on aircraft, even flame suppressors, heating and ventilation. I even designed the first heated deicing systems for helicopter blades. It was an extraordinary opportunity to go to all aircraft companies to review their work in heat transfer. So I came out of the War with a tremendous boost in understanding as to how to put the theories to work.

I got involved in problems of aircraft deicing, which was led by Lewis Rodert at NACA, now called NASA. I visited the Ames Laboratory at Moffett Field, where Lewis Rodert had been championing the idea of heated wing deicing systems. His first design was simply to run the exhaust through a pipe in the wing, but that wasn't very practical. He was pushing the design of exhaust gas heaters. As soon as I saw what was going on, based on Boelter's training in heat and mass transfer and boundary layer, and all those things I learned from Boelter, I sat down and figured out how you ought to design such systems. There were lots of unknowns about the design conditions. Like how much water would you have to take care of, and where would it go, and what would be the heat transfer characteristics over the rear of an airfoil and inside the airfoil?

It was stuff that was pretty complicated, so I arranged for contracts to be let. We let a contract with Irving Langmuir and his associates at the research laboratory of General Electric, because Langmuir had worked out the theory of drop dynamics in moving streams of air. He and Katherine Blodgett worked with the differential analyzer to calculate how fast ice would build up on a wing. I negotiated an Air Force contract for Boelter and his gang at Berkeley, including Earl Morrin, Ray Martinelli, and Armand Guibert, and many other people who were on that contract. I monitored the work and called meetings. We would have the people from General Electric, representatives from Lockheed and Douglas, and Vega, and all the airplane companies would come to a big meeting and we would discuss the design of the deicing system.

As a result of that, we made some models and put them on the summit of Mount Washington, where they have natural wind tunnels, icing wind tunnels. All the results were brought together and provided the basic data on which to design deicing equipment. And then we built some experimental aircraft. They got information from Boelter's people, put it all together, designed, instrumented, and then tested actual aircraft.

After the War, we got some of the German literature, and we had a chance to compare what we had done with the Germans. And we were light years ahead of them. As a matter of fact, in 1950 I received a letter from a Russian engineer who told me that they were designing the Tupolev aircraft deicing system using our work. It was amazing. He sent me a report and I recognized that in the equation that he had carried over a typographical error that we had made!

Well, I tell that to make the point that Boelter's influence on me was extraordinary. But he also had this gift of organizing a team to get things done. During the War it was profound. And today, aircraft icing prevention systems are designed based on that work. I am concerned because I fly a lot and I have talked to pilots and I find that they don't understand what's in there, how it works. And about five years ago, some people from Boeing sent me some reports because they said that they were not confident that the manuals and things they were putting out were right, and they had gone over the literature and found that I was still alive [laughs] and they wanted to have me check it over. So that was a contribution that still stands, and it's used everywhere.

When the War was over, I had to decide where to go.

Van Vorst: I was going to ask how you happened to come to UCLA.

Tribus: Well my heart was in Berkeley, tell you the truth. But I didn't know what I could do. During the War, I had a chance to meet Bill McAdams, Alan Colburn, and I also sought out Max Jacob. I had realized we were in terrible shape with respect to having people who understood heat transfer, and so I did two things. One, I asked these great men that I met—Boelter's name was a door opener. You know if you

went to W. H. McAdams and said, “I am a student of Boelter’s,” cooperation was swift. Right away Colburn told me that Max Jacob was not busy. Jacob was a refugee from Hitler who had come to the United States, and he didn’t really know much about how to get a government contract. So I looked at what needed to be done, and I went to the Illinois Institute of Technology and found Max Jacob. I laid in front of him five or six problems that I thought were urgent. I said, “Max, which one would you work on?” That way we became friends.

Well, after the end of the War, I was invited by McAdams, Colburn, Jacob and Boelter to come to get my doctorate and I thought about it a lot. Then Boelter started talking to me about the unified curriculum, and by now with my experience as an engineer, I saw that he was right—the engineers I met during the War were too narrow. They could work on what they knew or they could continue to do what they had been doing. But they couldn’t do something new. They just didn’t have the fundamentals, and I saw that he was on the right path.

So I agreed to study with him—I thought I had signed up with Berkeley. I had gotten married and I came out to San Francisco. I roamed around Berkeley looking for a place to live. I couldn’t find a house. So I went to the engineering department on the Berkeley campus and said, “I am looking for a house. Can you help me?” And they said, “What are you doing here? You’re supposed to be in Los Angeles.” Then I went back and checked, and lo and behold I had really agreed to teach at UCLA. I think that I would not have come if I had known that was the game, because I was in love with Berkeley. My years at Berkeley had been wonderful. You know how you mature on a campus, had wonderful friends and experiences and all that.

Anyway, I came down to Los Angeles, and I joined the teaching staff. Boelter immediately put me in charge of thermodynamics. I told him that I didn’t know anything about engineering thermodynamics. He said, “That’s why I’m putting you in charge. I want it changed.” I read the books that were used in engineering thermodynamics. They were all published before the War, and I saw that they were hopelessly out of date. They were inconsistent with the unified curriculum. In fact, it was about that time I think I met you, Bill.

Van Vorst: Yes, I remember the text that we had, the book by Bernard, Ellewood, and Hirschfield. I think my father had that text when he was at Cornell.

Tribus: I’m sure. I remember when we took over the course they had two weeks devoted to the use of a flyball governor on steam engines, and we knew that didn’t make sense in the modern era. The two of us set out to right notes. We tried all kinds of things. I even taught one year from Mark’s Handbook! We were grasping for new ideas in thermodynamics. I introduced some ideas about statistical inference as I worked toward my doctorate and did research.

Boelter insisted that I teach just about every course in the curriculum. I am sorry now that he didn't make me teach solid mechanics or something like that, because I was weak in that and still am. But I taught everything else. Even in electronic labs, and so on. I tried to stay one day ahead of the students. It was a great experience, and the students were fantastic because they were the veterans coming back from the War. They were eager to learn. They were highly motivated. They wanted to make up for lost time. They wanted engineering jobs. They saw what technology could do. A great bunch of students. I taught all different kinds of courses, but always coming back to thermodynamics and heat transfer. I taught graduate level heat transfer, and had wonderful students.

I still have the wonderful notes that I think are as good as what people teach today, even though that's 60 years ago. I say that because I've gone to technical meetings and elsewhere and have seen the level at which people are functioning and it's not up to the level Boelter taught us. As a matter of fact, there is a fellow in Houston who has written a number of papers on the history of heat transfer. He identifies Boelter's contributions as changing the nature of heat transfer education in the United States. He tracks Boelter's students, where they went and what they did, and so forth.

Van Vorst: I know him. I can't think of his name either, but it starts with an l.

Tribus: Lienhard, yes!

Van Vorst: And he has a time chart of great events and names of great people. Boelter is the only American on that chart.

Tribus: Yes. A lot of great things came out of those years. One of them I have to mention is Novak Zuber. This is one of those curious things. Novak was a refugee from Yugoslavia. He walked off the boat, came to UCLA, studied very seriously. Interesting about Novak is that during World War II he had been in Italy in the underground, primarily working on rescuing downed American pilots. So he developed almost a paranoid personality. I mean everybody was under suspicion by him because he had to be like that to survive. Well, he had studied some works of Yan-Po Chang who had gotten some ideas about boiling heat transfer. He had taken those ideas and added a lot of his own, and as a result he had solved a problem which had been baffling everybody in the world. This had to do with what is called the crisis in boiling heat transfer.

Consider what happens if you take an electrically heated plate or a nuclear heated surface and blanket it in water. As you increase the temperature or increase the heat throughput at first it boils, but at some point the heat transfer is too great and the liquid is lifted from the surface and it is then blanketed with the vapor. The heat transfer conductance drops dramatically. This is called the burnout phenomenon. Everybody wanted to know at which point it occurs. If you build a nuclear reactor, you want to have the temperature come up to just below that. Anything too far

below, you're wasting surface and if you go too far above it the surface will burn out. And so the burnout crisis was a very serious thing. People all over the world were investigating this phenomenon and the methods they were using just weren't working. I had been given a Q clearance from the nuclear program because I was going to work in that field. Novak as a foreigner wasn't cleared. One day he came into my office. He had had a fight with Kurt Forster and he came to my office. He was wild-eyed and he started babbling on about this crisis in heat transfer. I listened to him and saw what he was doing, and I thought, "My God, this guy has solved the problem. He really has!" For example, the Russian Kutamadze and some other Russians had taken a lot of data on boiling heat transfer. They found a non-dimensional group that almost correlated their data. They plotted a line and there were data scattered all around the line. Novak analyzed that and discovered there was a coefficient of pi over 24 that went right through the middle of all the data from the Russians. It was right on the money. He couldn't go to the meetings because he was not cleared to go, so I went to the Atomic Energy Commission secret meetings. Everybody stood around talking this way, that way, and the other way. Finally, I got the floor and I presented Novak's treatment, showed data that fits it.

That became the way things got done. It was the influence of Boelter in that, the way he taught people to reason and to think and so forth.

Now when I got my doctorate, I left UCLA for a while. I went to join General Electric and became a gas turbine design engineer. My doctoral thesis had been on deicing of jet engine inlets, and I worked up a theory that turned out to fit the data from NACA. At General Electric I redesigned the inlet deicing system for them. I wasn't really happy in industry, I wanted to come back to education.

I went to the University of Michigan for several years and organized an aircraft icing research establishment patterned after the work I had been doing during the War. I persuaded aircraft companies to send representatives because I had started a kind of running battle with the old NACA. I felt their method of design and their recommendations for designing deicing systems were really not right. The people in industry favored what I was doing. So we set up our own independent laboratory in competition with NACA sponsored by Wright Field.

After a while, I felt the need to come back to teaching. I came back to UCLA and continued to teach and do research and study. During my doctoral examination I was asked what was the connection between Claude Shannon's idea about entropy in communication and entropy in thermodynamics and I didn't really know. I read Shannon's stuff, but I didn't quite understand it. My answer to the committee was, "I don't know." It was a pitiful reply, but none of them understood either. I got my doctorate, but it bugged me. It really did. It bugged me that I didn't know, and during the time I was teaching thermodynamics I kept coming back to that problem. When I started teaching graduate-level thermodynamics I would ask my students. This is what I learned from Boelter—if you don't know, ask your students.

I taught this course in thermodynamics with a faculty member from geology, George Tunnel, who was a great thermodynamicist and a great admirer of J.W. Gibbs. We taught from Gibbs and that was tough on the students. I remember the first time I would have them get the book, and I'd say, "Now your assignment is to go home and before the next class read page one." They all laughed at that, but they came back to say, "We couldn't get through with this." When they go to the point that they could read Gibbs at a rate of one page every two hours, they felt very accomplished. It was very hard reading. That was a wonderful experience with Tunnel because he was thoroughly logical.

Well, during that time another of my students, Zissimos Typaldos, came to me one day and said, "You're asking students about entropy and thermodynamics. That's been solved." He referred me to a paper by Edwin Jaynes who then was at Stanford. So I went to the library, read his paper, and my God, there it was—the Rosetta Stone. Now you have to understand when studying classical thermodynamics from 1941 continuously trying to improve it, and now we are at 1958. So it's a lot of time, and I had not been able to make a logical connection between Claude Shannon and Rudolf Clausius. There was something there. Brillwin had written this book on the subject but it was only about an analogy. It wasn't a proof that they were identical.

For example, we use the word "tangent." We take the tangent of an angle and we use it in surveying, and we used the tangent in AC motors and things like that. We understand that we mean the same tangent while applying it in different ways, but it is the same idea of a tangent. On the the other hand, we would speak of entropy in thermodynamics and entropy in communication theory, but it was not clear that it was the same idea. It was the same word. In fact, somewhere around 1957 I wrote a paper in which I said it was a pity that Claude Shannon had treated them as the same and he shouldn't have done it because it was going to confuse people. Some day we were going to be building devices in which the dimensions were so small that the entropy in communication and the entropy of thermodynamics would get mixed up with one another, and that was a bad thing to do. I published that paper, I'm sorry to say.

Well, when Typaldos told me this development, I sat down and with all those years of preparation, in two weeks I wrote a paper which showed how if you started with Shannon's information theory and the idea that states of energy are discrete (quantized), then you could drive everything that was said in classical thermodynamics.

I couldn't sleep. I was absolutely taken by his work. I called up Jaynes in Stanford and I went to his office. He must have thought I was a madman because I went to the board and I derived all of the stuff that flowed from his work. It just poured out. He got it! He was a brilliant physicist and he congratulated me and said I was right.

However, he couldn't understand what the fuss was all about. He didn't understand classical thermodynamics and the way it was taught. He knew statistical mechanics

Now this development was to me very important because in the unified curriculum Daniel Rosenthal and Al Rosenstein and I had been spending a lot of time thinking about the building blocks out of which the unified curriculum is built. We felt that understanding of materials was a cornerstone. Materials play a special role because you cannot begin to think about design if you do not have a good understanding of materials. We mean both the structural and electrical properties of materials. You really need that foundation and you couldn't get that straight if you didn't have at least elementary statistical mechanics. So my idea was, if we could figure out how to start thermodynamics in the first part of the junior year with some statistical mechanics, no matter how weak, it would enable, in the second half of the junior year, the materials people to have a different approach to the teaching of materials. Then we could have the senior year devoted to design in a very realistic way. And true, it would be amateurish, since there were a lot of things they didn't know; but we would have laid the foundation. Furthermore, Boelter was always talking about the importance of statistics, and there wasn't any place in engineering where we were using statistics. So if I could figure out how to get statistics and statistical mechanics into the first semester of the junior year, then it would open up the teaching of materials. That was my motivation. I really hadn't thought of anything else, but as it went on, suddenly the derivation of all this became beautiful. I started reading the history of thermodynamics thought and I saw how people over the years had grappled with the idea of Maxwell's demon. The demon had information. And then I found many people starting around 1910 had been writing about this. I realized Clausius did his work in roughly 1849. I remember it, because it's the time of the gold rush in California. Clausius provided a well laid out treatment of classical thermodynamics. In 1903, Gibbs had written statistical mechanics. Quantum mechanics hadn't been developed yet. Gibbs had hinted at his results being connected to information and at ten-year intervals, Gibbs wrote of this. The great man had talked about the connection. But it was not until Shannon quantified our ideas about information that the two entropies became one! I started writing about this identity and that's when I ran into the paradigm shift problem.

I have to backtrack a little bit and say when we introduced our work on heated wings, we were introducing a new paradigm. It was wartime and you could get people to change.

I had worked with Irving Langmuir on the problem of cloud seeding and nucleation. Again that turned out to be a heat and mass transfer problem. Icing clouds are made up of little drops of water. How fast would the drops grow? I remember I went through line-by-line Langmuir's derivation. In fact, I found an error of a factor of two, which I was very proud to be able to take to him.

That brought me into my first experience with a paradigm shift, and I learned that paradigm shifts occur with thermodynamics, the unified curriculum, and other attempts to get people to change.

Langmuir had gone to the Soviet Union during the War. As a prominent scientist he could go there, even though we distrusted the Russians. He flew over. He was an avid pilot. He rode in the bomb bay of a B17 or B24, I don't remember which it was. Anyway, he is flying above the clouds and looking down, observing the clouds. He saw things happening in the clouds' physics. He saw that there was a cirrus cloud way high up and below was a cloud deck where the clouds were breaking up. He reasoned that ice particles were falling from the cirrus deck and seeding the super cool water in the droplets of water in the clouds below. Later he went to Mount Washington and did some experiments on the clouds there. The clouds rise up from the cloud base and arrive super cooled. They turn to ice when they strike something, but they otherwise didn't turn to ice. He reasoned that if he put ice crystals into the clouds, he could trigger it and change the weather. He set out to do so. He flew over a stratus deck, dropping dry ice, and cut an "x" in the cloud.

That work was done under a contract that I had let from Wright Field. As a matter of fact, in order to let the contract (thank God the statute of limitations has gone by) I lied a little bit. Exaggerate, I would rather say. I wrote a contract saying that we wanted to investigate the possibility of shooting carbon dioxide pellets out ahead of an airplane so that by the time the airplane got there the cloud would be dissipated. The icing storm would be changed because it's super cooled. Water droplets strike the wing, freeze, and hang on, but if it was ice that would just bounce off. Of course, that was totally impractical but the guys letting the contract were not technically trained and didn't know, so that was the contract under which the first cloud seeding was done. It was done successfully.

I went down to the weather service to try to persuade Reichelderfer who was then the head of weather services. Another guy by the name of Wexler thought that they ought to take over this project because I was a first lieutenant—maybe captain, I don't remember now—working on weather modification, which was way out of the line and responsibility of the equipment laboratory where I was working. They said to me, "Lieutenant, we meet lots of nuts who think they can make it rain, but not very often in a uniform." I went back and told Langmuir I was ashamed. He laughed and he told me, "The hardest thing in the world to sell is a new good idea because if it was new people won't understand it and if it's good they will have to change but they won't want to." He had lots of experience with innovation.

That remark was very important for me because I began to see as I tried to push the information theory treatment of thermodynamics, I found the same resistance. I wrote a textbook. I went around lecturing on it and people were angry with me. I mean, they wrote letters behind my back saying nasty things. Fortunately, I don't

mind a good fight. I don't like a dirty fight, and I like to fight at an intellectual level. I don't like the personal attacks.

So suddenly, I found myself embroiled in several fights—one of them having to do with the defense of the unified curriculum against people like Dr. [W. Delmar] Hershberger who wanted to have specialized departments in which they could do specialized research, and do what I call “engineerics, mathematics, physics, chemics, dynamics, acoustics, analytics, scientifics, academics.” But I had lived in the real world of engineering and I had been involved with designing, manufacturing, servicing, distributing, planning and managing—all the “ings.” Doing, delivering, that's what I thought engineering ought to be able to do. That's why I joined so enthusiastically with Allen Rosenstein on his emphasis on design. He saw design at different places in the curriculum as providing a way of bringing it all together.

About that time I also had an interesting encounter. Martin Loeb, who's now deceased, was in the social welfare department at UCLA. While I was getting my doctorate in engineering, my wife was getting her master's degree in social work. To relieve some of the characteristics of studying only engineering, I started taking courses with her, I went to her classes as an auditor. I became kid of a mascot in the department of social welfare. They gained where an engineer would come, sit, and listen and then argue with them; I enjoyed all of that. Well, I got to know Martin Loeb, who was a consulting sociologist in a project. You have to remember we were deep into the Cold War, and there were a lot of war contracts being let. The Korean conflict was starting. Martin was hired by a group of people as a consultant on this issue: if you wanted to get government contract you had to have people on your staff who could perform. People wanted to hire fresh young engineers out of college so they could stockpile engineers and then bid on these contracts. They wanted to know what it is the graduate of engineering wanted out of life so they could advertise their job description. Maybe they wouldn't deliver on the advertisement, but they could at least hire!

Martin and his colleagues did a lot of testing on graduates of engineering schools all around the country. Then Martin came to see me. He was very excited. He said, “There is something wrong. The self-image these students have doesn't make sense and they're all alike. They're cut out like cookie cutters. They want to work for a company where they can sit in an office and somebody will hand them well-organized problems and they would send back well organized solutions. They dream no big dreams. They are eager youngsters begging to be exploited.” And he went on and on and on. I had not thought about that, but again thinking of my experiences in wartime, and by now I was doing a little consulting and also I had worked for several years at General Electric. I began to see all of these ideas coming together. So I felt, one, the unified curriculum was the right thing and, two, that we needed it to teach the “ings,” not just the “ics.” We had been teaching “engineerics” not engineering!

And that's when things began to get difficult because to make that proposal meant changing people who had come into engineering departments from physics and other areas. Remember that prior to World War II, the preparation in the fundamentals for engineering was very bad. So when we realized we needed more scientific bases for engineering, people like myself from chemistry with a certain amount of engineering were brought in. But many of the people who were brought in were people with interest primarily in physics or in some very narrow part of the spectrum of engineering activity. The people that we brought in who were broad in engineering were just simply too few. They didn't fare well with their promotion committee. They were not welcomed. Also, they tended to want to keep their hands in real engineering, so they were part timers. We had some good designers, but they didn't think and function like the rest of the academic types. I think I was still more of engineerics than engineering, but I had an appreciation for my experiences and I saw the unified curriculum as the right way to go. So there I was in 1960 or so, heavily embroiled in three projects: One, the writing of this book on information theory in thermodynamics. Two, the defense of the unified curriculum against the people who wanted to attack it. And three, I was working on a project with Rosenstein and Rosenthal to investigate where we should be going with the unified curriculum because we felt it needed to be improved.