Russell Hobbie

Interview with Russell Hobbie

Interviewed by Professor Clarke A. Chambers
University of Minnesota

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University of Minnesota Campus

Russell Hobbie - RH
Clarke A. Chambers - CAC

CAC: This is Clarke Chambers. I am conducting an interview with Russell Hobbie, Physics Department, associate dean for students in the Institute of Technology [IT] for many years. It is the September 19, 1994, in the afternoon. The interview is being conducted in his office in Lind Hall.

With that elegant introduction, Russ, can you share something about your early education, your intellectual development, what turned you on to physics, why you went to the university as you did, how you got to the University of Minnesota, etcetera.

RH: I grew up as a college brat. My parents both taught at Skidmore College in upstate New York. I was born in 1934. One of my earliest recollections is at age three or four falling in the college fishpond and being fished out by some of the Skidmore students. My father taught physics there.

CAC: Ahhh.

RH: He never had any physics majors; so, he was also the director of buildings and grounds. My mother was the college librarian. When I was just finishing seventh grade, they moved from Saratoga Springs to Springfield, Massachusetts, to teach at American International College because they felt that I should be going to high school in a larger school district. The thing that I found fascinating about that—which is irrelevant to this interview—is the fact that in eighth grade in Springfield, Massachusetts, I had exactly the same textbook for English that I had had in seventh grade in Saratoga Springs, New York . . .

CAC: [laughter]

RH: . . . something which I have always attributed to the New York state regents' exams and regard as a great strength.

CAC: Were you an only child?

RH: Yes. My father had been married earlier and I have three step-sisters, but I've never really interacted with them; so, essentially, I was an only child. I chose, instead of going to the classical high school where I would have had to take Latin, to go to the technical high school which had a college preparatory program, but you also took machine shop and mechanical drawing. In my career as an experimental physicist and, also, in my evening career as a homemaker, I have found that experience to be invaluable. I became very active in amateur radio when I was in school and I was a Washington finalist in the Westinghouse science talent search in 1952. It seemed as though I had always wanted to do physics.

CAC: There must have been good basic science at this high school?

RH: Oh, yes, there was. It was excellent. I had three years of French. It was a standard college preparatory course except that they had machine shop and mechanical drawing. I don't know how I decided it, but I decided that I wanted to go to college at MIT [Massachusetts Institute of Technology], which I did and I thoroughly enjoyed that experience. I liked Cambridge so much that I wanted to stay in Cambridge. Everybody told me that I ought to change schools for graduate school; so, I went up the river to Harvard as a graduate student.

Something that was very interesting that I had not thought about much lately, that I said to the first year TAs [teaching assistants] who were involved in the IT-wide TA training a couple of weeks ago, was that I made a very big mistake my first year in graduate school. I was given a fellowship by Harvard; so, I did not have to be a TA and I lived in an apartment near Central Square rather than living on campus. The result of this was, that for my entire first year of graduate school, I did not form any friendships and I did not study physics with anybody. That was a disaster.

CAC: You must have met them in the laboratory?

RH: But, I didn't ever get to know anybody. I took a quantum mechanics course from Julian Schwinger, which it turned out later, I found out was populated by all of the people who were almost getting their Ph.D.s because they wanted to see how he taught the course. I basically failed it. I got a *C* in it; but, as a result of that, two very important things happened. The first was that they made me take my pre-lims the fall of my second year in graduate school. I studied for them that summer. I passed them and had that hurdle behind me. The second thing was they took away my fellowship and made me a TA instead. As a result of that, my TA assignment was to work with Ed[ward Mills] Purcell, who is a Nobel Prize winning physicist, a very wonderful and humble person, redesigning the junior electricity and magnetism lab. That was a very great experience.

I should back up and say that I had never, in either high school or college, had a course in biology; but, for my senior thesis at MIT, I drifted into a project in the acoustics lab making measurements of ultrasound attenuation and living tissue as part of the basic research for a group that was trying to do neurosurgery using ultrasound at the Mass[achusetts] General Hospital. That was kind of interesting; so, I went back and worked with them . . .

CAC: That was a brand new technology at that time for surgery.

RH: Yes, it never really did amount to much; but, people were trying it. I worked there then the summer after graduating from college and, also, the summer after my first year in graduate school at Harvard. I ended up going in the direction of nuclear physics at Harvard. After my first year as a TA, at the beginning of my third year in graduate school, I became an RA [research assistant] at the Harvard cyclotron and drifted into doing my thesis in experimental nuclear physics on the cyclotron. I took my final Ph.D. oral exam on April Fool's Day in 1960, which means that I had done my Ph.D. in slightly under four years because I graduated from college in 1956.

I had been a member of the American Association of Physics Teachers [AAPT] since 1957, since I started graduate school. I had seen names like Al Nier, and Ed Nye, and Frank Verbrugge . . . Verbrugge being then, I think, the president of the AAPT and Nier and Nye both having written articles in the *American Journal of Physics.*

CAC: Nier must have had a international reputation by then?

RH: Oh, he did; but, the point is that here were two of our most stalwart and active researchers who were writing articles for the *American Journal of Physics*, which is essentially the college and university teaching journal.

CAC: I see.

RH: That gave me a very favorable disposition toward Minnesota.

CAC: Heavens.

RH: I had met my wife, a public health nurse, in Boston. She had grown up in Iowa, had gone to the University of Iowa, and had then come out to work for the Visiting Nurses' Association. She had some interest in moving closer to her home and I had seen this about Minnesota; so, when I was finishing up my Ph.D., I applied at a consulting firm in Boston for a post-doc[torate] at the University of Chicago and had written a letter to the Physics Department here just completely cold. We came out for Christmas in 1959 and drove up here. We interviewed at Chicago on the way, and I didn't like it there, and they didn't like me. I had received an offer from Bolt, Beranek, & Newman [BB&N] but was waiting on it until we came out for this interview. I got out here and discovered that I was actually being interviewed by Morris Blair

to be a post-doc on the old Van de Graaff generator. I spent the day with him. About two o'clock that afternoon, he offered me a job and I accepted. All of this is a little bit different from the way things are done now.

CAC: [laughter] I'm going to back up just a minute because it was intriguing that our three most distinguished scholars are writing for a professional journal engaged in the teaching of physics. What would be the nature of an article that Al Nier or Eddie Nye would write for that kind of a journal as distinguished from their pure research?

RH: It would be something about, say, the pedagogy of teaching about cosmic rays or teaching about mass spectroscopy. I do not remember what it was that I had seen; but, I know that I saw their names in the journal. It would be fairly easy to go back to the late 1950s in the index and find out what it was they actually wrote.

CAC: This means that you were attracted by the teaching dimension?

RH: I was attracted by the fact that it appeared to me at that time that at the University of Minnesota, research was important. It was a research university. Having grown up seeing a small college, I thought that that was a bit stifling and I didn't want that. I thought that one could combine teaching and a research career here; whereas, I was seeing incredibly cutthroat competition among the post-doc and the assistant professors at Harvard. I knew that I didn't want any part of that. I sensed in my interview at the University of Chicago, the same thing.

CAC: For a lay person, would the articles that we're talking about have been more accessible to a lay person than . . . ?

RH: No, but they would have been accessible to a senior physics major or a first year graduate student.

CAC: Okay.

RH: They were quite technical articles.

CAC: That's an engaging thought.

RH: You said that later on in this interview, we would come back to this dichotomy between teaching and research . . .

CAC: Yes.

RH: . . . so, I won't talk about that now. I spent two years as a post-doc in Blair's lab and, then, the second year, I wanted to teach; so, they had me teaching the senior quantum mechanics

course, which I enjoyed thoroughly. I think I had my first Ph.D. student—we could go back and look at this—Rumana Fan Jamboonifan, about 1962.

CAC: That's early on.

RH: The second one then was Dave Wiser. The timing is a bit jumbled in my mind here. The university got funding for the MP, or Emperor Tandem Van de Graaff, that was built down on the river bank to sort of replace the linear accelerator, which I had not worked on, but which was becoming obsolete as well as the old Van de Graaff. I had a large number of Ph.D. students in a very short time. It was Jamboo and, then, Jim Morgan, Dave Wiser, and Clark Bergman all doing their Ph.D.s on the new Tandem Van de Graaff. It was, by that time, the late 1960s and it was the first real crunch in the Ph.D. physics market. What I saw happening here was something that . . . I had to write something like ninety-five letters for each of these last three people to get them jobs.

CAC: Oh, boy!

RH: I got quite concerned about the fact that I was now in a state where I was producing one or two Ph.D.s per year and I understood the problem of exponential growth and did not really want to contribute thirty to sixty Ph.D.s, I have sometimes cynically said, as a waste product in order to keep my own career going.

CAC: [laughter]

RH: I also saw coming up the fact that nuclear physics was going to change and that the experiments were going to have to be done at national laboratories because individual universities could not afford to keep these things going; so, I made a conscious decision about that time—I was now an associate professor—that I wanted not to continue in nuclear physics. The department was fairly supportive of that and I became the director of undergraduate studies in physics for a few years.

CAC: Excuse me. Jim Werntz was a colleague of yours?

RH: Jim Werntz was here as a colleague, yes.

CAC: He had some of the same disposition, perhaps?

RH: That's right. He was a little older than I was. Here, I saw someone who had been tenured in the department, who had this interest in combining research with teaching; so, it felt at that time like it was a very supportive environment.

CAC: Jim himself moved into administration to facilitate teaching.

RH: He moved into administration and, then, went to [University of North Carolina-] Chapel Hill.

CAC: Yes.

RH: I had forgotten about Jim. Yes, that's right, he was definitely an influence on me then. I had used a computer only peripherally in my Ph.D thesis at Harvard, had started to learn how to use computers here; and it was the time when one was just about to start using computers for taking data online, automatically, in experiments. I was put in charge of acquiring a computer for the Tandem Lab to take all of their data.

CAC: Computers were at a rather, as we look at it now, primitive level at that time?

RH: What we ended up buying was a Control Data 3100 computer that was $200,000 or$300,000 and, then, had to build our own interface from it. I had been stewing about—this is sort of a eureka! episode that happened—how to use the computer and how to design the interface for it. I remember going home one day with a fever because I had a cold, going to bed, waking up from a little nap, knowing exactly how to do it and getting out of bed . . .

CAC: [laughter]

RH: . . . and writing it all down . . . the logic flow of how the registers were going to be connected to each other.

CAC: How do you suppose that process operates? Almost everyone I've talked to has had a similar story.

RH: It sticks in your subconscious. Your subconscious is stewing on it and, then, you have a Eureka! episode.

CAC: Yes. They're quite common.

RH: That's the way the mind works. By the way, this work that I had done at MIT the two summers at Mass General . . . I had turned down this opportunity to work for BB&N doing some kind of . . .

CAC: What's BB&N?

RH: Bolt, Beranek, & Newman, the consulting firm in Cambridge, the other job offer that I had.

CAC: All right.

RH: That would have been a medical physics or medical engineering type of thing. I'd come out here and happened, at some point along here, to be invited to the neighbors for a dinner party at which I met a pathologist named Richard Riess, who was a pathologist at, then, St. Barnabas Hospital, a principal in Lufkin Medical Laboratories, and who was interested in using computers for interpreting lab test results. He wasn't necessarily using computers; but, it was the time when these sequential multichannel analyzers, these things that could do first six and then twelve and then twenty blood tests for the price of one, were first coming into vogue. What was happening there was that people were finding abnormal chemistry results that they couldn't explain. For example, take uric acid. You would normally measure uric acid in a patient if you thought the patient had gout. Nobody had ever noticed before that you can also get elevated uric acid from drinking too much coffee and a few other things. So, there was a lot of bewilderment going on.

CAC: It comes from just crude empiricism. You just start putting these things on a computer and suddenly different things show up?

RH: No, you're not on the computer yet.

CAC: Oh, okay.

RH: You had never bothered to measure uric acid in these others diseases; so, you didn't know that it was elevated in those other diseases.

CAC: It comes not from a hypothesis but from just a chance looking at . . .

RH: Being hit over the head by nature.

CAC: Yes.

RH: So, Riess was making things that he called diagnotes, which were just lists of what could cause an elevated uric acid, or a low calcium, or a high calcium, and so on. At that dinner party, he started asking, "Was there any way that one could computerize this?" Having just put in this online computer at the Tandem Lab, I started using it to try to do some pattern matching. This, then, led to, for several years, my working with Riess as a collaborator and Lufkin Medical Labs having a research contract with the University of Minnesota that supported a couple of students over the years. We did a lot of work on developing automatic interpretation of the clinical laboratory results and published several papers in this area. I can remember still being the director of undergraduate studies in Physics and Mort[on] Hamermesh, who was the department head, coming in one day to tell me that they were promoting me to full professor based on the work that I had done at the Tandem in nuclear physics and the online computer. As Mort put it, "We thought we'd better promote you while we still understood what you were doing."

CAC: [laughter]

RH: One of the things that happened—this is a real interesting example of serendipity—there was that Riess, as a pathologist at St. Barnabas, was always being asked why such and such happens by the physicians. He was sort of physician of reference. So, he was a voracious reader, and he would always subscribe to a lot of journals, and he would always tear out articles and file them away. My father-in-law, at this point, had kidney disease and was on dialysis and they were having a lot of trouble managing his . . . he was clotting where he had his shunt in and before he had the shunt, when they had the cannula in his arm. I borrowed Dick Riess's folder on blood clotting. Riess had misfiled in that folder an article about survival in certain diseases that showed an exponential decay like you get in nuclear physics. This got me to thinking that it might be interesting to put some examples in the pre-med physics course; so, I wrote Al Sullivan who was the assistant dean of the Medical School asking if it was possible to snoop around over there. Al asked me to have lunch with him one day—it was in October—and said, "What you really ought to do is to attend Medical School." I said, "I can't. I'm director of undergraduate studies in Physics. I'm teaching a full load, which is a course each quarter. There's just no time to do that." He said, "You could just audit things and skip the labs." So, for two years, I did that.

CAC: Good grief.

RH: This was very useful to me both to find more physics and to give me enough knowledge so that in my work with Riess, I wasn't just a high-classed programmer; but, I actually knew enough medicine so that I could contribute intellectually to the project.

CAC: These were the basic academic courses not the clinical courses?

RH: The Phase A and Phase B courses. I wasn't doing any rotations at any of the hospitals or anything; but, I sat through the remainder of the year in embryology, and biochemistry, and anatomy, and pathology, and physiology, and then, in the second year, the organ systems, the neuro psych, the cardiovascular, the pulmonary, the renal, the dermatology, the bones, the GI [gastrointestinal].

CAC: You were nimble enough to pick this up?

RH: Yes. What happened then was that I found that there was so much physics that I started writing some articles about what I was learning in the *American Journal of Physics*, that journal that I referred to earlier. I can remember going over to Owre [Hall] 15 or someplace like that a few minutes before class and I'm doing the final read through of this manuscript I'm about to hand in when some uptight medical student from two rows behind me leans over and says, "Did I miss a handout?"

CAC: [laughter]

RH: I really got a fairly good knowledge there and found that there was just too much physics ever to fit it into the pre-med physics course. I also found that there was a tremendous gap between what we teach the pre-meds, who will take one year of physics and that's it, and what you found in the physiology and biophysics research literature. I convinced the Physics Department that I ought to try teaching a course to try to fill that gap, a 5000 level course that has a year of general physics and a year of calculus as a prereq[uisite] that would appeal to the physiologists and so on. Probably around 1972 or 1973, I started teaching that course, developing it as I went. That turned into a book [*Intermediate Physics for Medicine and Biology*] that was published by Wiley in 1978 with a second edition about 1988. I'm trying, without much success, to do a third edition right now.

CAC: You had the encouragement of your own department but also the encouragement of the Medical School to engage in this kind of cross work?

RH: Yes. In fact, after I started teaching the course, I can remember Professor Jack Johnson from Physiology wanted to come and sit it in; and I was quite nervous about this because I was afraid I might get some of the physiology wrong. He reminded me, in no uncertain terms, that I'd been sitting through his course and turnabout really was fair play.

CAC: [laughter] It's impossible to make a comparative statement or guess, but, again, with so many of the interviews I've had in different ways, an individual has reached out, as you're explaining here, and the university provided an hospitable environment for doing it. I don't know whether that same hospitality would have existed at many other universities . . . some? Do you have any sense of Minnesota?

RH: I have a sense that it would not exist at many and that it probably doesn't exist here now.

CAC: We may come back to that; but, why don't you just say a few more things about that now?

RH: I think that the pressure on an assistant professor for tenure has become so much greater in the twenty-five to thirty years since I was having all of these experiences that were I to come to Minnesota as an assistant professor now, I would not make tenure.

CAC: And you would not take a dare and do this thing because that would subvert tenure?

RH: That's right.

CAC: Do you have any idea why there is this change in culture?

RH: I think that what has happened is that the federal funding and the desire of every university to compete strongly has just driven it and I think it's happened every place. I've seen it happening at four-year colleges. I think it's going to swing back; but, not all of our colleagues

believe that yet because right now there is an oversupply of Ph.D. physicists that is far more profound and far worse than the one that I saw in 1969 or so that made me quit nuclear physics to the point that we know—I've been on a national committee looking at this so I know the numbers—we are producing about 1300 Ph.D. physicists a year. About 800 of them are from foreign countries.

CAC: And many of them wish to stay in the states?

RH: About 200 of those 800 go home; the other 600 wish to stay. So, we have 1100 new Ph.D.s trying to get into the job market here and there are 800 new jobs a year, only 200 of which might lead to a permanent career. That's a fairly big mismatch. Cornell University has already, in their physics department, severely restricted the number of students that will admit to graduate school. Not many other places have had the courage to do that.

CAC: Is there an interchange on the job market of persons trained in basic sciences in the Institute of Technology and those being trained in engineering? Is there a job differential there?

RH: The physics community is sufficiently arrogant that they are convinced that there is. I'm able to see what's going on in engineering as well and the market for Ph.D.s and engineers has softened considerably also.

CAC: This would be across the board of electrical engineering, mechanical engineering, chemical engineering?

RH: Yes. What has happened is the decline in the defense industry.

CAC: Ahhh.

RH: That is really where all the Ph.D. engineers and physicists were employed, or most of them. What I see is a glut of Ph.D.s in the job market combined with not sufficient federal funding to allow things to continue to grow exponentially like they have been. David Goodstein at Cal Tech [California Institute of Technology] has a very interesting article that he points to about this that shows that it was growing exponentially since the time of [Sir Isaac] Newton by several different measures; and it was about thirty years ago that this exponential growth in science, in general, started to level off.

CAC: But it picked up in the life sciences, the biological sciences, didn't it?

RH: Yes. It's not as hard there. He was looking at science over all and it still levels off. The current issue of *Science Magazine* has a long section at the back about careers. It's starting to be a problem in the life sciences as well.

CAC: While we're kind of the subject, could you say something about being in an Institute of Technology where there are strong engineering departments and strong basic disciplines?

RH: I have found it to be a very hospitable place to be and a very good linkage. My own interests are, as you can gather, more applied. In fact, some of my physics colleagues would say, "They are, in fact, engineering." I have felt quite comfortable working with both engineers and with basic scientists.

CAC: There are no problems of curriculum, of dividing students?

RH: I would say with my engineering dean's hat on now, it is far easier here for the engineering departments to have a meaningful discussion with the Psychics, or the Math, or the Chemistry Department than it is when they're in separate colleges. I like this structure.

CAC: By general ascription, Chemical Engineering has been, at Minnesota, one of the strongest departments in the whole university?

RH: It is probably *the* strongest department.

CAC: Yet, whatever strength the Chemistry Department as such has, it wouldn't seem that that quality, or at least the ascribed quality, would float over. What is happening here?

RH: The place where it's useful is if you're, for example, are getting into solid-state physics where in order to . . . The borders between material science and electrical engineering and solid-state physics are extremely blurred; so, you'll find somebody like Allen Goldman working in all of them and collaborating with people.

CAC: But, the Department of Chemical Engineering itself has a very special reputation and probably authentically well-earned.

RH: That's right, and it's different from the Chemistry Department; although, there are faculty who hold faculty appointments in both.

CAC: You've been on the inside for twenty-five years. To what factors do you ascribe the really outstanding excellence of Chemical Engineering—you said you had no doubt in the whole university. Where does that come from? Is it leadership? Is it chance? How does one put together a really high quality department?

RH: Neal Amundson built the department.

CAC: He built it by taking on young persons who were promising, right?

RH: Yes. The Chemical Engineering Department has an excellent reputation both in graduate and undergraduate work. Sometimes, certain individuals don't spend as much time on teaching as I wish they did. I think it's probably inevitable. I think for being the number one research department in the United States, they probably do a much better job of teaching than other places would if they were to have that same position. A lot of administrators will try to say that good teaching and good research go hand in hand; but, there are only twenty-four hours a day and you can only spend your time doing so much and there is a trade-off.

CAC: Yes. Neal Amundson, however, has been gone a long time. So, do these things have momentum or are they picked up by other leaders?

RH: They were picked up by Ted Davis. There's an esprit in the department and it's kept itself going. I have no idea how quickly a department could go downhill. My suspicion is that you could lose it fairly quickly.

CAC: I was told by a former dean of the graduate school that some of the most interesting seminars and workshops, not necessarily in Chemical Engineering but on topics that might seem to be historical or humanistic, were sponsored by Chemical Engineering.

RH: That's because of Gus [Rutherford] Aris.

CAC: Okay. Again, we come back to individuals and their influence on this big institution?

RH: Yes.

CAC: While we're kind of on this subject, let me backup a bit. You came in 1960, which was very soon after the basic sciences had left the former College of Science, Literature and the Arts and came into the institute. You said, informally off record, earlier that the conversation about that, at least within the institute, was not very extensive by the time you got here.

RH: I did not hear very much. I heard a few comments from people that this had happened recently; although, I still could not tell you exactly what year it happened. The comments that I heard were that it was because the, then, dean of CLA [College of Liberal Arts] could not understand the need for the cost of laboratories. When I came, we still had a CLA and an IT Math Department and those were rather forcibly merged.

CAC: Right.

RH: It seems as though there were bruises and ill will from that merger for a long time and there might still be some; although, I haven't heard much recently.

CAC: It's interesting that you would perceive it as largely salutary from the point of the institute and, now, it is thirty-three years since that event and it's still perceived as the beginning of the end for the whole idea of the liberal arts by persons in the liberal arts.

RH: [pause] I don't know how strongly I believe what I'm going to say now because you've triggered something that I'm sort of trying to think out in my own mind. A few years ago, I had a chance to be on a panel that was put together by the Association of American Colleges [AAC]. It was about the nature of the major in the liberal arts curriculum. Let me backup. When I was the director of undergraduate studies in physics and back during the 1970s at least, Physics, I think felt that, although it was budgetarily in IT, it was certainly a part of the Arts College and that's how we got to know each other. I used to joke that Physics, Chemistry, Math, and so on were at double jeopardy because they had to serve on committees in both colleges. Certainly, at that time, I felt a good sense of connectedness of these. Now, it may be that that has worsened. Since, I've not had a Physics Department . . .

CAC: I'm sure not worsened, but weakened I'm sure.

RH: . . . position, I've haven't paid much attention to that. Getting back to the AAC, I was invited to be on the physics panel for this thing, about the nature of the B.A., the baccalaureate degree. They had panels in physics, in math, in religious studies, and I think in history. There were several things. We mostly met as individual panels; but, we had two joint meetings of all the panels. I was very struck, during those joint meetings, at how fundamentally different the physics curriculum—by this, I mean the physics curriculum at a college that gives a B.A. degree—and the math curriculum were from, say, the history, or the religious studies, or the women's studies curriculum. In physics and math, there is a very distinct hierarchy of courses. You cannot do the calculus level physics until you've had calculus. You cannot do the junior physics until you've had the first physics and you cannot do the graduate course until you've had several of the junior/senior courses. That was true of all of the physics departments that were on that panel. It's the nature of the discipline and it was true in mathematics as well. We used to make some jokes about the fact that, in virtually every other major that was represented at that AAC panel thing, the major consisted of taking these six out of twenty courses or whatever. There was no prerequisite structure. What I don't know is how much the weakening of the Arts College is due to the moving of the sciences out . . .

CAC: Ah!

RH: . . . or how much of it is due to this fundamental split in the disciplines.

CAC: That's a very intuitive speculation. I don't know either. I want to back up just a minute, also, to your interest, not of research again, in medical physics. It led to this course, which I think was the last thing you were talking about there; but, it led also to other kinds of research projects you had.

RH: I needed to finish my random walk through life.

CAC: Yes.

RH: I continued working with Dick Riess and we published several papers; but, I had one master's student and two post-doc sabbatical visitors, which Lufkin Lab was funding. Somehow . . . someplace along there, I was asked to be an administrator again. I was asked to be the director of the Space Science Center. I had done that for about five years and, then, in 1984 [V.] Rama Murthy, who was then the acting dean in IT . . . When Roger Staley had come, he had done a national search—Paul Cartwright was retiring—for an associate dean for Student Affairs and they had hired Ed Steuben from the Illinois Institute of Technology [IIT]. Then, Staley was no longer dean. Rama was the acting dean. Steuben was offered a position to go back to IIT as a vice-president, so he left. In the fall of 1983, I was approached by Rama to see if I would be interested in this job. Now, I realize that I've got to go back and pick up another whole thread of my academic career that I had completely forgotten about.

CAC: Okay.

RH: Sometime around 1963 or 1964, Frank Verbrugge was running NSF [National Science Foundation] summer institutes for high school physics teachers. Warren Cheston was his associate director. Cheston came to me and asked me if I would like, during the summer, to teach a course for the high school physics teachers; so, I said, "Yes." For several summers, part of my summer salary came from the Tandem Lab, the old Van de Graaff Lab, and part of it came from these NSF summer institutes. At that point, I got interested in the problems of the high school teachers. Then, for two or three years, I forget which, when I was still over in the Physics Department as director of undergraduate studies, I became the PI on these; so, I was the principal investigator on these NSF contracts. Then, NSF got out of the summer institute game. That was probably around 1968, 1969 or so . . . 1970, maybe 1971.

CAC: These things happen fast and, then, it requires physics departments all across the country to adjust very quickly?

RH: Yes. There was a resurgence of interest in training high school teachers in around 1982 or 1983. I was still in the Space Science Center. We were able to get Samuel Devins from Barnard College to come out and do a summer workshop for high school teachers, which we held at the Bakken Library for Electricity in Life [Minneapolis]. Roger Steuer was involved in that. I guess that they are still doing summer workshops at the Bakken to this day. Then, there was a chance to apply for an NSF one, which I did, and we were awarded this—I applied while I was in the Space Science Center—and we had the teachers here on campus the summer of 1984, which was my first year in the dean's office. The reason I digressed into that was that Staley, when he was dean, and Richard Green had set up something called the Minnesota Alliance for Science. They had hired somebody to write a proposal, and the thing had gotten funded, and Stueben was one of the co-PIs and Chick [Charles] Algren, over in the College of Education, was

the other co-PI. The reason that Rama had approached me to become associate dean was that they wanted me to take that over when Steuben left to go back to IIT. I was interested in doing it only if it had summer support because Riess at Lufkin Lab was much more interested in issues of medical economics now than he was in the clinical chemistry stuff and Lufkin Lab was about to be swallowed up and absorbed as all of these labs have; so, I needed to be doing something else.

CAC: I don't understand that. Absorbed by whom?

RH: They were bought by Smith, Cline, and French.

CAC: Ah!

RH: We now have clinical chemistry being done by huge national laboratories and the places in different towns are simply the collecting stations, and they fly the samples back to New Jersey, and they run them there, and they send the results back by computer.

CAC: Physically, the facilities are still based in various research universities?

RH: I'm talking about the stuff for day-to-day lab tests for patients that's done in private laboratories.

CAC: All right.

RH: I was still looking for summer support. The Alliance for Science thing was coupled with the associate dean. I turned Rama down about three times and the fourth time, I said, "Yes." That was really a big change in career for me. I've been here ever since.

CAC: Nearly ten years.

RH: I've been in the dean's office. As I mentioned to you before, somehow, during the first couple of years, I was able to find enough time to do the second edition on my book; although, I don't think the quality was as good as it would have been had I been in the Physics Department and had more time to work on it. I like to sometimes say that what I did with this book was to corner the market on an empty set. Sales have not been terribly high. Wiley [Publishing] is in the process of letting it go out of print. Since it's really a reference book as well as a textbook, they've never quite understood in their college physics division how to market it. The American Institute of Physics [AIP] has a press that is trying to get titles. They have a series in biophysics and I'm just in the process of finishing negotiations with them so that the third edition would actually be a different title and it would switch from Wiley to AIP.

CAC: This would suggest there are not a large number of courses like this?

RH: That's right. I know of one at Madison. I know of one at Michigan and I know of one at Johns Hopkins and that's about it. Part of the trouble is that as budget's have tightened . . . First off, you have to have somebody with a biological or medical as well as a physics background to teach it. A few years ago, there was a year when the three associate deans had to act collectively as the department head in Computer Science. It was the year that the course was supposed to be offered; so, they had some other Physics faculty teach it. My understanding is that it didn't go very well because they didn't have the depth to bring to it.

CAC: Sure.

RH: I'm way behind right now on the schedule for producing the new book . . .

[End of Tape 1, Side 1]

[Tape 1, Side 2]

RH: . . . the day-to-day stuff in this office takes so much more time now than it did ten years ago—well, maybe, I do. Part of it is that there is a greater flow of documents. Part of it is that the university has decided to do a lot more with the recruiting of students. We have to because the student population is down; so, I'm involved in that. I will be involved in Campus Preview Days three Saturdays this fall, which ten years ago didn't even exist.

CAC: But, that's an essential thing to do?

RH: Oh, that's right. Everything seems to be essential. It's just that there's too much that's essential.

CAC: How do agendas get added onto in that fashion?

RH: Here I am . . . responsible for student affairs in IT. They start doing Campus Preview Days and call up and say, "Can you organize something on engineering, and find a faculty member to do it, and, then, will you serve on a panel for parents this afternoon?" You don't say, "No."

CAC: These are items that are generated by Central Administration, in this case, for a good cause?

RH: Yes.

CAC: Or by your administration?

RH: Probably everything is for a good cause; it's just very hard to say, "No," to people.

CAC: Or to set priorities as you're down the line?

RH: Yes. Another example is that I was involved for three summers in a project that the American Association of Physics Teachers had with the National Science Foundation to develop a new high school physics curriculum for the students who do not normally take physics, called active physics. I was working with two high school teachers here in town on four chapters having to do with the physics of the body. Somehow, even as late as a year ago this summer, the summer of 1993, I was able to find time to get work done on that. This last summer, I was supposed to finish chapter seven through twelve of my revision and I am still, at this point, in chapter seven. The reason for that is because when we went into the provost's office to talk about budget late last March, things in IT were in such desperate straights in terms of computing that the provost told IT to come in with a proposal for a mandatory fee for its students to cover computing. So, the spring was spent on selling that to the students so that it was passed by the Regents and, then, the summer was spent . . .

CAC: For undergraduates?

RH: Undergraduates and graduate students . . . making sure that the public labs were up and running and properly managed so that the students who have paid this fee wouldn't come in and kill me.

CAC: This because of the shortfall in general monies to underwrite . . . ?

RH: Anything, yes.

CAC: What kind of a fee does it amount to?

RH: It's $50 a quarter this year. It will $100 a quarter next year. It's being used both to provide proper management of the public labs as well as to provide equipment in both public labs and in departmental teaching labs. So, that just got added on top and nothing went away.

CAC: Yes.

RH: [pause] This is exhausting.

CAC: The work you're doing is exhausting. There is a kind of finality in your observations that makes it difficult to lead somewhere else. Let me try this though because we're now talking about your role as associate dean for undergraduate studies. Have you been able, the ten years you've been in the office and for longer than that, to see basic changes in the curriculum, in the expectations, in the levels of the courses, in the quality of the students—there's a lot of items there—or is this pretty consistent?

RH: I think it's been pretty consistent. I, perhaps, am becoming a grumpy old man; but, I do have a sense that the students are not willing to work as hard now as they were even ten years ago.

CAC: Is this, in some part, because they're working harder off campus to make money?

RH: It could be. I certainly know that they are working off campus. I am not one of these people who has a fixation on having students graduate in four years.

CAC: Yes.

RH: I really am a grumpy old man. I really have a sense that, probably because of television, students cannot read as carefully or as long as they used to. There's probably a very good reason in nature why we grow old and die and are replaced by other people and why plants die and are replaced by other plants and, that is, that one organism can probably only deal with so much change.

CAC: [laughter] The surrounding culture certainly created enormous pressures on young people, age eighteen to twenty-three . . . there's no question about that. On a related matter though, in what ways has the institute changed its undergraduate curriculum or is that pretty stable also or more stable than it would be in the humanities where things are up for grabs all the time.

RH: It has been pretty stable. There are a couple of observations that I want to make. One thing that we did do in about 1990 was to change the freshman math, physics, and chemistry courses from being 5-credit courses to being 4-credit courses so that the normal load would be four courses at a time, which is what it needs to be in upper division.

CAC: Four fours instead of three fives?

RH: Yes. There were two things that were happening. The first was that a full load for a quarter in order to graduate in four years was fifteen credits in CLA and sixteen credits in IT. Yet, for financial aid, we try to be kind to the students and say, "You're eligible for financial aid if you take twelve credits." Pretty soon the students think that twelve credits is a full-time load and, then, they all of a sudden wonder why they can't graduate in four years. The other thing was that we were getting them in the habit during the freshman year of taking only three courses. We weren't sure whether the fact that they were taking only three courses as juniors and seniors was because they had learned that three was the magic number or whether it was because of their jobs or what. We tried to change it to really get our students to take four courses during the freshman year each quarter.

CAC: Do you have any sense of whether the deeper immersion in fewer courses is a better learning experience or the other way around?

RH: It's somewhat less immersion in one more course. What I think had happened was that, in some cases, there had been inflation of credit and that, although, five credit were being given, it really was no different than if four were being given. In other cases, we had to cut out material. In Physics, I know we cut out material and the Physics faculty are sort of still quite

unhappy about that because they feel like they're not teaching a real Physics course because they're only lecturing three days a week instead of four days a week.

CAC: I want to come back to another part of your career. It would be apparent from what you have said and from the CV [Curriculum Vitae] that I have that you were a troubleshooter, that your quality as an administrator came to be recognized. The Space Center would be one example. This receivership in Computer Science would be another. Can you say something about the administrative problems of the Space Center?

RH: There really weren't any problems at the Space Science Center. There was this building which was not really all devoted to space but was a multidisciplinary, very high quality lab facility that reported to the Graduate School. Gerry Shepherd had been the director of it.

CAC: It goes that far back?

RH: Yes, it goes way back . . . even before that. I'm not sure who was director of it before Gerry; but, he was selfish enough to decide to retire. So, then they had a search committee looking for somebody to replace him. There was some discussion when I was approached about that job because I really viewed the space as . . . It was not all space physics and I said very clearly, when I took it, to the committee and to the dean of the Graduate School that I was not a space physicist and I was not going to spend a lot of my time trying to pump up money for the space program or anything like that. Hence, I think this was understood by the administration; but although, I never heard anything formal about this, I think there were some space scientists who wished that I had spent more time trying to make the space things more visible.

CAC: Is there a curve in NASA [National Aeronautics and Space Administration] grants, for example? I would assume that would have a major source?

RH: Absolutely.

CAC: When does it crest at this university?

RH: I don't know. I do know that right now the Physics Department is looking to probably close their electronic shop, which, had for the last twenty-five years, lived on NASA projects. I don't know whether the demise of the electronic shop is due to retirements and to people like Conrad Mauersberger returning to Germany or whether it is because NASA's funding has changed. I'm just not close enough to it to know the answer.

CAC: What proportion of the institute's research commitment would have come from NASA?

RH: I haven't the foggiest idea.

CAC: Okay. National Science . . .

RH: Understand that I can tell you about students; but, I've just not paid attention to those numbers.

CAC: Okay. Say something about Computer Science then. Then, I want to come back to the larger issue of how a new technology gets introduced into a large instituion, not only in teaching and curriculum but as . . .

RH: I'm not sure what to say about the Computer Science Department. There were some factions within the department. There was some squabbling. There were some rather unhappy things that got spread all over the *Daily*. Dean [Ettore "Jim"] Infante was forced to put the department into receivership and the only way one of his associate deans would go serve for a year as department head was if we all did it together; so, we all did it together. That worked fairly well because Gordon Beavers was the budget expert, and Sally Kohlstedt dealt with the graduate students, and I dealt with the undergraduate curriculum.

CAC: So, for a year, you had troika running the joint?

RH: That is correct.

CAC: Which is not an administrative strategy that's highly recommended, generally.

RH: It was also pointed out to us that troika is a masculine noun and ought not be used. [laughter]

CAC: [laughter] That kind of a [unclear] is masculine, is it, in the Russian language? All right.

RH: This was originally attributed to Phil Hodge [Jr.] in Aero[space Engineering and Mechanics]. When we first went in, the question was, how many deans does it take to run a department? The answer was, it was known to be three, and the reason was, one to hear no evil, one to see no evil, and one to speak no evil.

CAC: [laughter]

RH: I understand that when we left, there was an underground joke going around in the department which is, how many deans does it take to run a department? The answer was, more than three.

CAC: [laughter] You wanted to say something more—I would interested, too—again reflecting on a long sweep of twenty-five, thirty years within the institute generally not only in Physics or, perhaps, Computer Science, or perhaps Space Science, of the tension between a commitment to

research, which surely is expected, and the teaching mission. We've talked about it briefly before.

RH: Yes, I know; I'm just trying to marshal my thoughts. I may be wrong, but my impression was that, at the time when I was not sitting down and grinding nuclear physicists left and right, that this was supported or at least tolerated in the department. Here I was, at this point, newly an associate professor who did ultimately, within the department, get promoted to full professor. My feeling is that if I were to try to do the same kind of diversity of things that I have during the time that I was associate professor that I probably would not get promoted and I would end up being one of these associate professors who becomes very bitter and retires that way. I don't know whether that's true or not. It's hard to judge yourself. This may be something that you ought to ask somebody else because it is true that I have continued to do a lot of things and been visible nationally; but, I don't know whether that would have been enough or not. I just sense that there is a lot more pressure. I was part of a group . . . I did not write the proposal that funded the Tandem Lab; although, I contribute in terms of the computer stuff and stuff like that. I was part of a group. I brought in a lot of money from Lufkin Lab when I was doing the biomedical computing kind of stuff. It was not peer reviewed funds; although, it did benefit the university. I think that the quality of the research that we did was reasonably high. It only supported one graduate student and two post-docs over the ten or twelve years that I was doing that. I think that, as I look back at my own career, the thing that I think that was most important, that has certainly given me the greatest intellectual satisfaction is *the book*; but, I think that would be viewed as not research . . . scholarship, or pedagogy, or something. So, if someone were coming along doing what I did, I'm just not sure that they would get promoted. As I talk to assistant professors, I really feel that they have a lot more pressures on them to get funding than I did. I think that's bad. I think that we would be a healthier university if we could somehow return to the kind of situation that we had in the 1960s. This may just again be a grumpy old man looking nostalgically back; but, I do think that there appear on the horizon to be a lot of pressures on universities in general that are going to cause a change in the way this research activity takes place.

I mentioned before this glut of Ph.D. physicists and I said that I'd been involved in a national effort that was looking at that. What happened was that I was asked, a little over a year ago, to chair a committee that did a review of the programs of the American Institute of Physics. The American Institute of Physics is the umbrella organization for the American Physical Society, the Acoustical Society of America, the American Optical Society, the American Association of Physics Teachers, the American Association of Physicists . . . there are about ten of them. I got a chance to serve on a committee that . . . It was some very good people, one of whom was David Goodstein who is the provost at Cal Tech. Dave and I turned out to really be on the wave length in terms of this oversupply of Ph.D. physicists. What I see happening right now is that the funding agencies like NSF don't have enough money to go around. What's happened is that they have had, basically, only a slight increase in budget; but, we've been producing all of these new scientists at an exponential rate with a doubling time of, perhaps, six to eight years and their budget hasn't been going up that fast. Pretty soon there's not enough to go around. What they

have been doing is to come back to the universities and ask for more and more in the way of cost sharing. We don't have the money to do it. We are getting pressure from the public who don't give a hoot about what research we do. All they want to do is to have their sons and daughters taught well. I think a lot of people, including Jim Infante, think there are going to be far fewer research universities ten or fifteen years from now. One of the problems will be, can we position the University of Minnesota to be one of them? One way you go at that problem is to pretend that nothing is changing and just put more and more and more pressure on your young faculty. The other way to do it is . . . I need to say that in some of these conversations where David Goodstein and I agreed with each other about what was happening, I can remember him saying that fifteen years from now Cal Tech will still be a premier research institution; I don't know about the University of Minnesota. I think one of the things that Jim Infante is trying to do is to figure out how to keep us still in the front line. If we need to find more money to supplement diminishing state resources, and if there is not enough federal money to go around and they ask for more cost sharing, and if there are societal demands for such things as more masters of engineering degrees for students who are in the work force and are place bound, it seems to me that something has got to change. The only way that I can see that changing is if we do recognize something in the way of greater ecological diversity among our faculty. The way I would do that is probably not to change the way we grant tenure in the first place, but to say that once we have given somebody tenure, we're stuck with them and what we need to do is, as their interests change and they wander through their career, that we find somewhat different ways to keep them happy, productive, and useful to the university.

CAC: And renewed.

RH: And renewed. The tenure code right now says that essentially the promotion to full professor is based on the same criteria as tenure. I've been saying for six or eight years now that I think we've got to change that to recognize some kind of significant contribution but over a more diverse area. Faculty that I talk to will privately agree with me; but, nobody has the guts to come out and try to make any kind of a change.

CAC: All cultures are resistant to that kind of change, particularly if it involves values and the value of pure research has been a very heavy one, particularly in the sciences. On a related aspect of the issue that you've raised, a very large number of the persons I've talked with, regardless of what college or what department they have come from, have commented in their own instance and, then, there are twenty-five instances, on the increasing specialization of research, which then manifests itself in a specialization in teaching, particularly at the 3- and 5-level courses and that as a consequence—this is the link—there has been in many places a loss of a sense of collegiality of faculty being engaged in a common enterprise. Most of them recite this very poignantly that they know that their research was important and that the specialization was required by the intellectual demands of their own discipline, but that the downside, the cost, they see as being very severe and an accelerated cost the last ten, twelve, fifteen years.

RH: I'm not sure. It used to be that in the Physics Department people rotated through courses. You'd teach a course three years, three times, and then do something else; but, this was back in the 1960s and the early 1970s.

CAC: You had to be something of a general practitioner?

RH: That's right. I would like to say that that still exists; but, now, I have to say that I've not looked carefully at the teaching assignments. There does seem to be, now, a group of people who are the ones who teach the introductory courses and that it doesn't seem to rotate around as much as it used to. I have not looked at the teaching patterns to see if people are teaching the same course. I would have said that I was teaching many different courses, but the point of fact is that since 1972 or 1973, I have only taught my biophysics course. But, since 1979, I have been an administrator of one kind or another and, now, basically, even though I'm sort of full-time in the dean's office, I teach my course, fifteen credits, every other year really as an overload; although, some of the other associate deans do the same thing . . . some don't. That may be a special case. I am sure that if I were not in the dean's office, I would probably be teaching my biophysics course every other year and teaching the general physics every other year. I would like to think that I could go back and teach any one of the undergraduate courses; although, it would be a bit of work. I was surprised to discover that that is not true in Computer Science and that that is not true in Electrical Engineering and so on. Maybe it's happened in Physics, too, and I'm just not willing to admit it. That's the kind of thing that somebody could sit down, and look at the data, and figure out.

CAC: Yes. I talked with a chemist the other evening and he said that when he entered the profession—he's a bit older than we and has been retired for some years—that in the 1950s, most good research chemistry departments in research universities had an introductory course for beginning graduate students in chemistry and that by the early 1960s, there were three or four such introductory seminars, becoming more specialized, and by 1970, each person had one of his own and that there was, then, a loss of a general practice and this kind of intense specialization which meant, as he saw it, that in chemistry at least, the beginning graduate students were socialized to this kind of specialization from the very beginning.

RH: That's not true in Physics because we still require of our Ph.D. students a year-long sequence in quantum mechanics and a year-long sequence in classical physics, mechanics and electrodynamics, that everybody has to take. We still have a departmental written exam that is quite broad. We have just recently changed the Ph.D. preliminary oral exam from being an examination on all of physics to being something on the preparation of the student to do the thesis work. My, again, grumpy old man feeling is that this has been a relaxation of standards.

CAC: But, you don't have a sense within the institute whether physics is a sport or whether physics is the model, a prevailing model?

RH: I don't know.

CAC: Okay. A friend spoke—a colleague from the College of Education—much the same story. When he came the college met as a full faculty quarterly and talked about their mission, their teaching, their research, priorities, what places to hire anew, and so forth and that by the mid 1960s, because of this kind of specialization which was encouraged by the availability of national foundation grants for the first time in education in large amounts, the specialization was so great that the college faculty met pro forma once a year and did no business and the business was done where the teaching and research was going on at the departmental level.

RH: Yes. The IT faculty meetings are once a quarter, and are pretty pro forma, and are attended by thirty people out of 400.

CAC: In the Institute of Technology, the significant province is the department, the discipline?

RH: Yes.

CAC: This seems to be a rule that reigns everywhere, but with increasing severity where there's been a specialization and creation of new departments, new specialities.

Dean Hobbie, do you have other reflections that we should . . .

RH: My only reflection . . . I never dreamed that sitting here and talking like this for however long it's been could be so exhausting.

CAC: It's exhausting to listen, too, but exhilarating at the same time. There's only one thing that I would like to come back to then—perhaps, only briefly—and that is the process as you observed it of the introduction of the technology of computers into all aspects: teaching, learning, payroll, enrollment, grading, everything. How does that take place? Boy! it happened fast.

RH: I saw the first wave of this during the 1970s when there were a lot of people who got interested in computer-assisted instruction. Computer-assisted instruction consisted of a teletype machine pounding away at ten characters a second. I found it to be terribly stifling, and boring, and user unfriendly, and frustrating, and had no great interest in it. We had one colloquy speaker who was very passionate about it and was so passionate about it and so absurd about it that I think he probably kept our Physics Department from looking at computer-assisted instruction for at least five years. I can remember going out to dinner—I shouldn't put this on tape—with a small group of people afterwards. He was so rabid on this that he thought that the computer simulation was better than the reality in all cases. I sat there biting my tongue and not asking him about sex.

CAC: [laughter]

RH: Where I see computers have really taken off in IT . . . I have written one computer program for doing a simulation in radiological physics. I did that while I was here in the dean's office,

too, and it's now published for commercial distribution. It was a tremendous amount of work. I had a graduate student do a master's thesis on it originally and, then, I spent two summers turning it into something, just putting the interface around it so that it would work right. It's something that you might use in one course for part of one hour. It was a lot of work. Some people are trying to develop complete curricula. There are these simulations of certain things that are around now and I think can be used in that kind of way. Let me back up again and show my age. I can remember when the Super 8 single concept film loop was going to be the answer to everything in the world. I can remember when the [Kenneth] Keller plan was going to be the answer to everything in the world. I can remember when—in fact we're still sort of in it—cooperative learning groups were going to be the answer to everything in the world. I've seen enough of these fads come and go again, that I'm slow to get excited about the latest new fad. Where I find computers very useful myself in my own understanding, and where I have my class use them, and where I see them being used in IT is when they are genuine tools for something. By that I mean, the word processor is a tool. It allows you to revise and to produce better quality written materials. The spreadsheet is a tool. It allows you very easily to make mathematical models of things whether it's budgets, or a flow of something through a tube in the kidney, or something like that. The computer-aided design and computerized drafting is a tool, an essential tool in industry. All of electrical engineering has been completely revolutionized because the way you design circuits now is with programs like *Spice*, which is a commercial program—there are others whose names I forget—that you will use as a practicing engineer. These are the things that require very expensive workstations to work on, not the word processor and the spreadsheet but these other professional engineering canned packages or electrical engineering design packages that have really changed the way the professional approaches the field and, therefore, have changed the way we've had to teach.

CAC: All your graduate students and undergraduates have to master this?

RH: All our undergraduates in engineering have to master that. Now, physics is not there yet; so, there are some very interesting politics and differences of opinion about this mandatory computer fee in IT between the engineering disciplines and the sciences . . .

CAC: I see.

RH: . . . which we could go into but it's so recent that it's not yet history, so I won't. From what I've seen, it's these commercial packages that are used as tools that have, for IT, really upped the usage of computers. CUFS . . . as my friends say, "Never forget that CUFS is a four-letter word." For those listening to this tape in the year 2025, CUFS was the Consolidated University Financial System or something like that we have installed. My perception of CUFS is that it produces better reports for management than we had in the past and we had lousy reports in the past; but, it has quadrupled or quintupled the amount of work that has to go on in the departments entering the data so that one can have these bloody reports.

CAC: I'm told that some departments have to farm it out, that their secretarial staff are not up to it.

RH: I can believe it. Sandy Hummel who used to sort of be in charge of this office and used to be my secretary, now spends full-time on the accounts and someone else is my secretary and it's because of CUFS.

CAC: Sometimes these interviews turn into conversations. I can report for posterity that I became skeptical even of the word processor when the two persons most enthusiastic in my own department went around saying how much time they saved, and how it was improving, and how it was working, and you just can't get along without it. I wrote one book and ten articles and neither of them wrote anything, so that I was kind of skeptical about the quality and the quantity that was going to emerge quickly. That's a bad example; but, I was made immediately skeptical.

RH: What I found in terms of the word process is that . . . I now like it. I'm left handed. My handwriting is atrocious and it is easier for me to just type it into the computer straightaway. I certainly do spend more time now doing my own word processing. My secretary no longer types for me. There has been a big change; but, I'm not sure that this university could any longer afford to have secretaries who would type for all the faculty. I did find, between the time that I wrote the first edition of the book and the time that I wrote the second edition of the book, that the word processor really changed the way I did it.

CAC: Of course.

RH: But, I found another thing. In the first edition of the book, I made sketches of my illustrations and there are lots of them. Wiley hired an artist to turn those into finished drawings. I now do the finished drawings myself. My solutions manual that I give to the class is camera ready copy and I discovered the other day that a drawing that had some vectors on it, suddenly, by the time it was printed and in the hands of the students, all of the sticks had disappeared from all of the arrows that represented the vectors because of a printing problem. Now, I had to go back and waste fifteen minutes figuring out why the heck this stupid thing didn't print; so, it's a mixed blessing.

CAC: Sure.

RH: Once you have something, it's much easier to modify it.

CAC: It certainly has taken over. The university couldn't do its business without it. Morrill Hall couldn't. You can't have a payroll or register students without it.

RH: That's right. Let me tell you one example. When I did the first edition of my book—it won't help the tape but I can show you this—I had the solutions manual and my daughter typed all of this. It took her ten weeks to do it.

CAC: I should think so.

RH: Wiley paid her $75 week. It is now time to do the second edition of my book. I have the solutions manual and the new problems, I have already put into the computer. The new problems are there, but the old ones aren't; so, Wiley agreed to hire my youngest daughter, Anne, to type all of the old solutions manual into the computer. She thought she had a summer job. She was done in two and half weeks.

CAC: [laughter]

RH: The reason was that it was just so much easier to make corrections to the status in these equations.

CAC: Yes.

RH: I guess that was probably an increase not in my productivity but in hers.

CAC: It was a joint project.

Toward the end of all interviews, we both slow down and, then, I say, "Anything else you want to share with posterity?"

RH: I can't think of it now.

[End of the Tape 1, Side 2]

[End of the Interview]

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