

**Leonard M. Rieser '44**  
**Provost and Dean of the Faculty Emeritus**

An Interview Conducted by  
Jane Carroll

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Dartmouth College  
Hanover, New Hampshire

INTERVIEW: Leonard Rieser  
INTERVIEWED BY: Jane Carroll  
PLACE: Leonard Rieser's office  
Hanover, NH  
DATE: August 16, 1996

CARROLL: Today is the 16<sup>th</sup> of August 1996, and I'm speaking with former Provost and Dean of the Faculty Leonard Rieser here in his office in Hanover, New Hampshire. I was curious when you first came to Dartmouth. That was 1940?

RIESER: As an undergraduate.

CARROLL: As an undergraduate. How did you choose Dartmouth?

RIESER: Your question's very perceptive, as you'll see from your answer. It was certainly my intention to go to Harvard, and my family's intention; and as late as July of 1940 I was sitting at the camp where I was a counselor, talking to a friend with whom I planned to room in freshman dorms. We were picking a room. And I had a phone call from my home that a telegram had come saying something about "Harvard is sorry, but your score on your recent English exam meant that you would have to wait a year to come to Harvard." That set in motion a search for an alternative. In retrospect, I'm surprised that I wasn't more discouraged by that, or depressed, but it's because I really hadn't thought much about alternatives. I may have, earlier, applied to Reed College, I don't remember, or whether I did it then. I certainly was admitted to Reed. I had a nice letter from the president of the student body, welcoming me.

And I think what my mother must have done is talk to a distinguished alumnus who lived down the street about this dilemma. But also, I had had a teacher of Latin in high school who was a graduate of Dartmouth, and in his era a winner of what's called the Barrett Cup. He was one of those many, many people whose height of achievement was as a Dartmouth undergraduate, and he had encouraged me to apply, and I

hadn't applied before. The idea of going to a men's college had never occurred to me. At any rate, within a couple of weeks I was admitted to Dartmouth. The process was quite different. I'm sure this alumnus called President Hopkins (I don't know it as fact, but I have to assume that) and I then had to choose between Reed and Dartmouth. My best friend from high school, who was not a stellar scholar in any way but had been my next-door neighbor since childhood, had decided to go to Dartmouth with no good reason either. So I said, "I'll do that." He had already decided much earlier.

So, maybe early in August, that matter was settled. And in September--it was hard to get--I think I got a small single dormitory room on the ground floor of Streeter Hall. But I had no sense about Dartmouth at all. Well, I had friends who had gone there, and a teacher who had obviously been there. So the way we traveled then was to get on a train in Chicago. The train was switched someplace, maybe at Albany, and ended up in White River Junction--actually, ended up in Lewiston, Vermont, as I recall, which no longer is a station. But we got there, and there were--by then there were real cars and not horse-drawn carriages, but you can find pictures of horse-drawn carriages meeting students in an earlier era. And I arrived at Dartmouth College. And it was lovely, fall.

And then all of a sudden you realized you were in a place where there was a great presence of what were then, and now do not exist, "big men on campus," in big green sweaters or white sweaters with big "D"s on them; where freshmen were hazed in a very--not degrading like VMI or a Citadel, but you wore little hats which you had to always wear; you were at the beck and call of upperclassmen when it came to moving furniture; and of course I thought about Reed College, which was a very different kind of place. So that at that moment, that's when two paths diverged in the wood, and I went to Dartmouth, which is why we're talking here today. Had that not been the case, I never would have known Dartmouth as a place where you might go after a Ph.D. in physics.

CARROLL: Was it hard to make a transition from being a city, or suburban child in Chicago to what was really a very remote area here in 1940?

RIESER: That wasn't hard, because we lived out in a rural suburb north of Chicago. It was hard when you couldn't get anywhere. There were no planes to speak of. There were trains; travel to Boston took four or five hours by bus and car, although there was a little train that went between White River and Boston. And the tradition was that on the weekend you should get in a car and go to Smith College. I didn't really like that very much. The level of drinking took me by surprise. But the utterly macho

environment—it took a long time to find fellow students that had a real interest in the intellectual life of the place. And there were some. It was--one of the things one did then, and I've seen it now too--one way you escaped then was to, say, join the Outing Club, which happened to be a different kind of sensitive person. Still physical, and I liked that, but the overriding athletic aura and the formality--you were called "Mr. Rieser" or whatever--I think the only manifestation of that in the current time was Vincent Starzinger, who would always address the students that way.

And it was not a particularly happy time, though I enjoyed the courses I took. I began taking physics and so on. I had been by the end of high school a straight "A" student in things, math, physics, Latin and so on. So it was a bit of a surprise to find oneself getting "B"s and "C"s; and to organize oneself to be a different kind of student. Also there was a surprising line, in a funny way, between scholarship students and non-scholarship students. That is, to--in the dining service there were several options, but one of them was a room (it was Freshman Commons, as I recall), it had tablecloths and you were waited on by those freshmen who were on financial aid. And often those freshmen with financial aid were outstanding students, whether they were athletes or, in some cases, scholars, or both. So that was a funny class situation. That is, on the one hand, that made them, that defined them as poor, and on the other hand that defined them as talented. And so it was a funny perplexity.

CARROLL: Where was Freshman Commons at that point?

RIESER: It was what is now called Collis. The big room in Collis was Freshman Commons. And then there was a room off that which was then called the Colonial Room, and I think as an upperclassman you could buy a meal from there. And that, again, was tablecloth service, waited on by other students. If I had been running it I would have followed the St. Paul's tradition, where every student has a job, without regard to income. That's how Putney School operated; I was Chairman of Putney board for many years. So there was that.

Most of all, for a person whose interest was intellectual, who was not built as a football player, there was a certain unsureness, and you tried to act in a way which would be respected by them. The attitude toward girls (we'd call them women now, but they were, at best, called girls then) was, in many cases, quite derogatory. It was a very sexist place. Some of the teachers were wonderful, but they too were teachers who had chosen a male institution. And that, as I think back on it, is sort of a continuation, I suspect, of the prep school tradition but at a higher level. On the other hand, when you went home at vacations, you were "at Dartmouth," and

that gave you credit; and during the summer after freshman year I worked and raised, made enough money to get a car, so sophomore year I had a car, which was a huge difference.

So I had no regrets, I'm sure (at least I never thought about it), not being at Harvard. It never occurred to me. I always had this lingering question about Reed College. And while it's a nonsequitur in terms of this discussion, it will come up again later. There was a moment when the Chairman of the Board of Reed flew out to Norwich to ask if I'd consider being president, and the only temptation was that that would settle my curiosity about Reed College. We didn't do that. So Reed's always been in the back of my mind, but not--with no regrets.

CARROLL: Do you remember when World War II was declared?

RIESER: Of course.

CARROLL: You were here on campus at that time?

RIESER: Yes. That is, sophomore year (I don't want to drag this out)--sophomore year was certainly more comfortable than freshman year. It had one attribute that, again, I think of now, hadn't thought of before--the fraternities (which are still not first class examples of meaningful residential life, in my judgment) were very dominant then, and sophomore year was when people competed to join fraternities, and fraternities competed for people. I had a very negative feeling about fraternities from the beginning, so therefore you felt out of that part. And that, again, is where, then (and, I believe, now, though it's changed a great deal) the Outing Club provided this alternative. Because in general there were a lot of men then who did not want to join fraternities, but that was another kind of camaraderie, and a kinder, gentler camaraderie--Canoe Club, Outing Club and so on. And I know that that is still a resource to many people.

So as sophomore year began there was that noise in the system, but I enjoyed my courses, and I had picked up a roommate and got a lovely corner double room, still in Streeter Hall, and it had the advantage (what was his name, [Francis S.] Fran Dougherty ['44], I think)--it had the extra advantage that his father worked for the Hormel Company in Minnesota, and we had a fireplace in that room. So that every so often we would have shipments of things, and we could roast little sausages in the fireplace. And compared to the prior year of being in a single room in the basement, having a choice living place with a guy who was really fun made that second year much more interesting. And I do remember on, it must have been Sunday, December 7, listening to President Roosevelt.

He may have spoken on Monday, but Sunday--I believe it was a Sunday that--because I remember being in that dorm room and hearing of the start of World War II. And slowly, but not too slowly, the full implications of that for all of us became clear.

CARROLL: Did you start looking around at the people in an all-male institution, wondering who was going to go and who was going to stay, and begin to question that?

RIESER: No, I think that the first order, you had to assume everyone was going to go. I had a different decision to make. Dartmouth did not have a regular summer term then, and I decided that I'd have to--I'd hoped to finish college first, I'd only done two years--so I decided to enroll in the summer at the University of Chicago, with the possibility of coming back in the fall. And that was a decisive change, I didn't come back. And toward the end of that summer, where I took primarily physics courses, I enlisted in the Army, in the Signal Corps of the Army, and by that enlistment was given one year beyond that summer to complete the Bachelor's degree. So I was--it was called "inactive duty."

CARROLL: And you did that at Chicago?

RIESER: Yes, I didn't come back to Dartmouth.

CARROLL: So you really were able, in three semesters, to cram two years of...

RIESER: No, it wasn't quite that. It was in four semesters plus, which I will explain. Because obviously--I mean, I left Dartmouth, I had no regrets about leaving it, but there was always something about it that must have been in my mind, or I wouldn't be here now. So I took primarily physics courses in Chicago, though I had to meet some of their requirements, which involved--there's something called a biological sciences survey course, and I had to take at least two terms of that. And that's only relevant because in that particular year, 1942-43, a person to whom I'm now married, who went to Goddard College, they had a rule there that you had to spend one year away at a major institution, coming from a little place like Goddard.

And I won't dwell on that, other than her mother--some of the work for her mother, who was editor of Parents Magazine, founding editor, was a cousin of my mother, and my mother had been urged to be sure that I brought Rosemary Littledale out to the house, which was in the country, an hour plus from the University. Past experience with such arrangements led me to avoid it at all costs. And just by chance we met in the hall, that is in the fall of 1942, now, at Chicago, I lived at what's called International

House with another roommate. The best living ever, in the tower; we had the tower suite in International House for one term; and by a process I won't bore you with, I ran into this woman whose name I knew because my mother had said "you must look for her." And that was November, and finally my mother, not letting up on the pressure, feeling a little embarrassed, said, "you must invite her." So I think it was March, probably, and I did invite her, and the rest is history, actually. And that helped make life rich at the university, although she was only there for a year.

CARROLL: Were you married before you went into the Signal Corps, then?

RIESER: No. As you'll see, I never went into the Signal Corps. I finished all my courses, but had one English requirement to fulfill, which was required of all students there, which could be met by writing ten or more term papers--by taking a writing course. So in the fall, when I normally would have gone on active duty, I spent another term taking this English course with a woman (what was then called a home-study course, because it wasn't their English course). A woman whose name, I think, was Catherine Cue Baskerville or something like that (it's not relevant, really). And once a week we would meet, I would then write my paper, we'd meet again; maybe it was twice a week. And I completed a series of such papers, and in December '43, which therefore is six months prior to what it would have been at Dartmouth, so I did accelerate some, I graduated from Chicago. Chicago graduates every quarter. This was really wartime, remember now this was December '43. There were a lot of military training programs at Chicago, as did develop at Dartmouth too. Because those who stayed at Dartmouth in that class went into Naval or other training.

CARROLL: The V-12?

RIESER: The V-12.

CARROLL: So the Signal Corps loomed.

RIESER: Right, it loomed, and it's what I expected to do, I'd done it with my eyes open. Because having a background in physics, the Signal Corps, I naively thought, perhaps maybe there would be a place where at least you might do some interesting things. You didn't think much about being killed or injured at that point. But because I'd taken these courses at Chicago with one or more people who were then working on the Manhattan Project, which was going on in the Physics lab--and indeed when I lived at International House with that roommate (this was in the fall of '42), we regularly played squash together, and only later, of course, did I learn that

in the doubles court next door was the group putting together the first chain reactor. And in December of 1942 was when that went critical, December 2<sup>nd</sup>. So that before war was declared--let's see now, by the time--yes, because war was declared in '41. After war was declared, but in December 2<sup>nd</sup>, 1942, the first chain reactor went critical. We had, prior to this, and presumably afterward--because we still played squash, we were next door--but we knew nothing about it, I knew nothing about the fact that part of the lab where I took courses was closed for what was called "war work." Several of the labs. But that didn't seem abnormal.

But then, one professor said, "Would you be willing to work on this project?" He explained to me as much as he could, but I was too naïve to even understand when he explained about getting pieces of uranium together, and so forth. I didn't really know what I was doing. And I said, "Well, I'm enlisted in the Army, so that you'd have to work that out with somebody." And just to close that period--let's see, on December 30<sup>th</sup> I then reported to Fort Sheridan, Illinois, to enter the Army and go on active duty in the Signal Corps. And my then fiancée, but not wife, came from the east coast to--I guess I'd gone there for Christmas, and then we came back to Highland Park, we'd been there for New Years. But then on the 30<sup>th</sup> my mother and she dropped me off at the fort. And I had dog tags, all of that. And next day we went through various physical tests, because I remember between the eye test and the ear test, when the loudspeaker asked that I report someplace, and they simply said, "You're to go home and report on January 2<sup>nd</sup> to the laboratory in Chicago." And I was given a little pass, I remember, and walked out the gate, and the sentry said, "How do you get these?" and I said "I don't know, but there it is." And I had to hitchhike into town from the fort, it was about ten miles, and then call up home. They picked me up. And that was a very unusual New Year's Eve, because that was December 31. And Rosemary had still stayed on; she was going home shortly afterward. And then on January 2<sup>nd</sup> I showed up back at the University, and began working there.

CARROLL: How quickly did you understand that it was working on the atomic bomb?

RIESER: That you understand real quick. The group I was drawn into was involved in many aspects of instrumentation, and the first part of the Chicago assignment was to get the large plutonium-producing reactors, in Hanford, Washington, in operation. But that involved a lot of instrumentation work; it involved frequent trips to Oak Ridge. There were times when I would be--because of what we were developing, which was to measure neutrons in the ongoing reactor, deep inside--I would be given several hours of the reactor to myself, and I would ride up and down on the loading elevator, poking our instrument various places. It was radiologically kind of a stupid



activity, in retrospect. But it felt pretty important, and I learned a tremendous amount. And then we had decided to get married in July of that year. I remember getting cold feet about it. I just came back from Oak Ridge in June...I'm not sure this is relevant to your tape...

CARROLL: That's all right.

RIESER: ...but it was a Vanguard helicopter. And the man with whom I worked who was sort of a fuzzy philosophical physicist as well as a very hard-nosed instrument person, and who knew Rosemary. And one day when I came back with gorgeous data recorded from my work in Oak Ridge, and said, "By the way, I've decided to put off getting married." And I remember he pushed all the material aside, saying, "You just don't understand what's important." And he reached on the shelf, gave me a copy of The Little Prince by St.-Exupéry, and said, "Go home and read this." Which I did. I did try--I remember walking up and down Michigan Avenue in Chicago one time, and using the phone in the theater that still exists, to tell Rosemary that I thought we shouldn't get married right now, life was so uncertain. And it was uncertain...but the nickel kept coming back. So I called up my favorite aunt and uncle who lived in Chicago. I said, "How about dinner?" "Fine." So I went over and I said, "Before supper I have to make a phone call." And I explained why I was calling; and they were terribly fond of this girl, and I remember my aunt saying, "You make that call, there's no dinner." Simple as that. So we ate dinner.

CARROLL: So your stomach won out!

RIESER: Well, the influence of those people won out. And I've been eternally grateful. Though, again, it's not relevant, that aunt and uncle, long after the aunt died we became sort of close friends of the uncle who was a good deal older, and came here frequently. I'm only mentioning it because it's so relevant to today. He was in the movie theater business, etc., very close to Notre Dame, and it was he among others who decided they should make a movie about Knute Rockne. And because Mrs. Rockne had so little money. And he read the script, didn't like it, because it made him this guy who was kind of a drunkard but who was famous-- Let's see, is Knute Rockne the coach or the player?

CARROLL: He's the coach.

RIESER: He's the coach. But the player, he's the one who was called The Gipper. And my uncle said, "I know where I can get a grade B actor for this movie." Well, I put--he told us that story in great detail, at length, and then I had him do it again so I had it on tape. And I've never forgiven him for Ronald

Reagan, and it was crucial. I mean, those funny things in history. So he died a couple years ago, but he knows we were very fond of him, but we were not forgiving. It's just such a funny click after last night's reference to The Gipper.

CARROLL: That's right! Well, you were out there and actually saw the atomic bomb to be tested as I understand it, is that right?

RIESER: Well, after a year at Chicago...

CARROLL: You went to Los Alamos?

RIESER: We went to Los Alamos. The mission in Chicago was to get the plutonium production going. There were other things, but that was fundamentally it. And you have to understand, I was very young, 22 years old; I was not an important presence. There were a lot of important presences there. But it was so interesting, and lethal in terms of what we were trying to do, but it was historically unique: hadn't happened before and I don't think it could happen again. And I've lectured on the subject a lot but I don't want to take your time on that, but I'll just say it was such a micro-manifestation of the initiatives which drove Jewish scientists out of Germany. And all of a sudden all these people one had read about and so forth were around, much more at Los Alamos than in Chicago.

And after a year at Chicago and after the reactors were going in Hanford, we decided to--we had the opportunity or, I suppose I sort of thought it was an obligation--here I was on inactive duty, my friends were all in the Army, you wanted to be where the action was. So Los Alamos suddenly had had to expand over the initial group when it was realized that they couldn't use the plutonium in a weapon in the same method that you could use uranium. It's a technical point which Rich [Richard "Rich" Kremer, Jane Carroll's husband] understands. So they had to create a whole new division called the Gadget Division (G Division, it's called), and had to bring in some additional people, younger workers; not Alphas in Aldous Huxley's sense, but enlightened Betas, at least.

So we went; we were told we would have to live in separate male/female dormitories and so on. I'd been married six months, but housing was very tight at Los Alamos. And when it came time to move I really regretted it. My wife thought being near Santa Fe would be fun; and it was also clear that she had to work at whatever job there was. I won't give you the whole Los Alamos lecture, it's out of style here, but that did result in--but she became, in due course, after the real mundane job, the director of the nursery school at Los Alamos--and the group I was in was at a very high

priority in the series of things that had to be accomplished before testing of the plutonium implosion weapon could be done. So that people like Niels Bohr and [Isidor] Rabi would come by our group, because [Robert] Oppenheimer had them roving around. Rabi would sleep; Bohr was more interested.

CARROLL: And were you there until the end of the war?

RIESER: Yes, and beyond. I was at Alamogordo [New Mexico] on the 16<sup>th</sup> of July, getting there at least on the 14<sup>th</sup>, I don't know. The 16<sup>th</sup> of July happens to be our wedding anniversary, and so I began to get rather paranoid about the fact that something would happen and I'd be--God had given us one year together, and then bingo, [inaudible]. Rosemary meanwhile was in Los Alamos, and they could--since they knew, roughly, when it was going to be, they could--I don't know whether she saw the explosion 200 miles away, or not. Because you had to get up early. But the night before the explosion (this is all part of another lecture of this series) I walked up and down the desert around midnight with Victor Weisskopf, who was one of the leading physicists, while he explained to me why we weren't going to get blown up, why the atmosphere was not going to ignite, etc. And Victor and I have been good friends ever since.

And then, curiously, after the shot--after the war ended, there were a couple of us who were then sent on active duty, having been, having volunteered, having been inactive throughout the war, but working. And the argument was that you couldn't ever get out of the Army without going on active duty in it. So then I went off to basic training in Fort Leonard Wood, Missouri, or someplace, and was at the point where they were going to put real ammunition in our rifles after dry-firing, when I was called out of line, a little bit like the experience at Fort Sheridan. And the lieutenant, who had been very gruff, sort of put his arm around me and said, "You have orders to go to Los Alamos." I said, "Oh?" I actually knew--it was clear I was to go back. And he began to tell me what an interesting place it would be, and he went on and on, and I finally said, "Well, I have to tell you, I live there, I mean, my wife is there."

And then we moved from a sort of cold-water quonset hut where we lived with outdoor plumbing, to a dorm room. And I won't go into all those details, but there I began working with a man named Otto Robert Frisch, whom you wouldn't know, but he's on my mind because I'm reading the biography of Lise Meitner, who was his aunt. And Frisch and Meitner were the ones who figured out what nuclear fission was, but Meitner, by virtue of having been driven out of Germany, continued to correspond with [Otto] Hahn and [Fritz] Strassman, nevertheless they and others cut her

out of the Nobel Prize. This is a dramatic book; I hope Rich [Kremer] has read it.

CARROLL: I'll have to tell him about it.

RIESER: He must read the Meitner biography. But for the next four or five months I worked with Frisch, which was really interesting, and he and I were good friends. He came to Dartmouth often as my guest, and lectured here. And then we were told that if we were admitted to graduate school, in science I presume, we could apply for discharge. And I decided the simplest way was to write the University of Chicago, where I'd been, saying, "What do I have to do to get admitted to graduate school in physics?" The letter came back saying, "You're automatically admitted, because at Chicago you were in the College for two years, and then you went into the Division." So I was in the Division of Physical Sciences, which offered the Bachelor of Science degree, the Master's degree, the Ph.D. degree. So therefore I applied for discharge. But I remember when the lists were posted, I was really depressed, I didn't see my name on it. And someone else came up to me and congratulated me, and I'd overlooked it. I was so dubious that I would get out of the Army ever, that I'd missed my own name on the list.

CARROLL: I thought your Ph.D. was from Stanford?

RIESER: Oh, it was. We then returned to Chicago. I went first; my wife had obligations as director of the nursery school. Indeed, they held her departure up for several weeks because she couldn't account for one cot that the kids used at rest time. That's a whole other... But we had a little--we were given a little house on the south side of the university. I was there in the spring and she arrived late spring, and the summer was really hot. And we'd bought a couple of new rugs, and we put one on the floor, both of them in this little prefab, and cinders were everywhere, and we decided that we couldn't spend five years here. I knew Chicago was a very, very tough graduate school. It's a fine university, I see it often.

And again by a quirk, and that's the only reason I'm mentioning this, because of the way paths diverge in the wood: because Rosemary was director of the nursery school she had hired Mici Teller as her assistant. And the Tellers--we actually babysat for the Tellers for a couple of weeks at Los Alamos, because Edward was invited to the University of Chicago, and they went out to look for a house. So we--I didn't see them very often, Rosemary saw Mici. And one day Mici said, "Our friend Felix Bloch from Stanford is coming to visit Edward." And she knew from Rosemary that we were unhappy. So they arranged for me to have coffee with Felix

Bloch, who later did receive the Nobel Prize: a very typical Swiss, very unusual. And he encouraged me to apply, and I did all that, and then received a letter admitting me and giving me a teaching fellowship. It was written on stationery that said "Leland Stanford Junior University." So we were very perplexed. We were so naïve, I can't get over it!

So we decided that before we agreed to leave the great university--even though I saw that five years at Chicago would be a struggle; people like [Enrico] Fermi who were there, marvelous teachers, nevertheless they only wanted one or two graduate students at a time, very competitive. So we decided to take the little money we had, and took a train from Chicago to what we thought would be San Francisco, and to find ourselves in Oakland. We had one lower berth, and packed all our food. We looked like peasants. And couldn't find a hotel room in San Francisco. We figured out you took the ferry across; there wasn't a bridge. And by the time nightfall came we did find a hotel room. And the next day went out to Stanford and met the chairman of the department, with whom I later did the Ph.D. degree, and realized that, as any fool would have earlier, that the university was founded by Jane and Leland Stanford, in memory of their son Leland Stanford, Jr. So its formal name is Leland Stanford Junior University. For that I made a train trip.

CARROLL: That is a great story!

RIESER: And to people who are from an area that is green and lush much of the time, Stanford, as you approach summer and early fall, is very brown. The buildings are sandstone; the whole place has a brown look about it. But it wasn't hard to make that decision. So that's how we happened to go to Stanford. But just the quirk of Mici having worked for Rosemary in the nursery school. Incidentally, Mici also called me once, I remember on a Saturday, at Los Alamos, and said, "I'm going skiing with our friend Klaus Fuchs; do you want to join us?" I always enjoyed the fact that I was introduced to Klaus Fuchs by Mrs. Teller, of all people.

CARROLL: When you got done with your degree, did you contact Dartmouth, or did they contact you, or was this just a general job search that brought you back to Dartmouth then as a professor of physics?

RIESER: Well that begins, certainly, the next chapter of the Dartmouth connection. I didn't say more about the graduate things, and I might think of them, but I don't normally think much about that. As I was finishing my degree, there were various options, because you couldn't not get a job if you were a physicist then. And because of a professor who had been at Stanford and who had been moved, and a friend who had gone to work with him at what

was called the Cornell Aeronautic Laboratory in Buffalo, I applied there, and I applied to the Brookhaven Laboratory on Long Island. I remember I applied to St. Andrew's in Scotland because I had seen an ad for it in a science magazine someplace. And I had learned--I presume I must have learned of an opening at Dartmouth, because of the death of a professor. And I had sort of this nostalgia. I must have seen in an Alumni Magazine a picture of the new wing on Wilder Laboratory, with the intention they were going to have an accelerator there, or something of the sort. Obviously I must have had a good enough time at Dartmouth to be very nostalgic about it. So I did apply, one way or another.

I was invited to visit Brookhaven and Cornell. I don't think I visited Dartmouth at that time, I can't remember for sure. But it wasn't hard to know you didn't want to be at Cornell Aeronautic Laboratory, which was a government type thing. Brookhaven was too, but it was much more academic, as those national labs are. But I decided on the Dartmouth thing, sort of decided. I was wavering. Because anyone I worked with thought that that was the oddest thing in the world. I mean, why would you leave Stanford to go to Dartmouth? And the man, the major professor who was a distinguished teacher, he was the only one who understood; he encouraged me. And I was debating this matter, and finally [inaudible], Chairman of the Physics Department at Dartmouth, got me on the phone, because I remember that phone call. And I had to say yes or no. I said yes.

And selling our little shack of a house and moving to New England was very difficult. And arriving here, renting sight unseen a house owned by the College (it wasn't a house, it was a building which has since been torn down and replaced by the Catholic Center on Occom Ridge there), it was called the Dow House, there were five families. And we had two children, one of them seven months old. But the fall is a lovely time to arrive here. The fall is the time to arrive, and spring is when you should recruit faculty. As Dean I would avoid every way I could having faculty candidates come early in the winter. I wasn't uniformly successful. Just to finish that: I came as an instructor in physics, with a salary of \$3,900, I remember. And I believe I taught six courses, three each semester. Some of them were courses like X-Rays, and

**[End of Tape 1, Side A -- Beginning of Tape 1, Side B]**

RIESER: There were about two people in each of the courses, and I said to the chairman, "Why do we do these courses?" Because in the X-ray course the two people were residents in Radiology at the hospital. "Why am I

giving these courses? There are a lot of other things I could do.” I was quite capable of [emissions?] and research things, and so on. And he turned to me and said, “What would you do?” And I said, “I’m sure I could find something to do.” Well, there was no helping that. But I learned a lot about photography, about X-rays.

For my wife it was a harder transition, although she was the one, because she is a much sharper decision-maker than I--having said we would go, we would go, and make the most of it. But the house where we lived was basically a bacteria house. There were five families, four of them with children; I forget, maybe there were 13 children all together. And kids were sick much of the time.

I remember that fall also because that was the fall in which the--it was 1952--in which the election between Stevenson and Eisenhower...And I realized the next day after we had lost, if I can put it that way, my colleagues in Physics all seemed so chipper. And I realized that this was a very Republican, albeit New England Republican, environment. It is so different now from what it was, you couldn’t begin to describe it. But it was not hard right at all, it was just pleasant New England conservatism.

That is, when we first arrived, Professor [Allen] King and his wife came to our house, brought us vegetables from their garden [inaudible]; there were Physics Department social gatherings at different people’s houses, usually serving home-made tomato juice. I remember in one case the Chairman of the department, a man named Leslie Murch [’22 A.M.], who was the senior member of the department, a bachelor--we invited him and a young instructor (that was the second year) to our house before one of these parties, and had drinks, because we knew what the parties were like. But they were cordial. They were nice people. There were seven of us; I was the youngest in every way, rank and so on. There was one instructor and I was he.

CARROLL: Was it a different university, having Dickey as the president, than it had been with Hopkins?

RIESER: Oh, decisively different. Now first of all, the difference in an institution in the last years of a 29-year presidency and the first years of a 25-year presidency can be a huge discontinuity. And John Dickey had arrived in November 1945, so he had already been here seven years when I arrived. As one of my colleagues said when we were walking the golf course once, in the winter, John and, I guess, Chris--no, I guess John just walked by with his golden Labrador, and he turned to me and said, “Well, there’s Himself.” I hadn’t had much contact with him.

I hadn't had, as a student, I think I encountered Hopkins twice, when there was something called Matriculation, where the President signed the document when you finished your Freshman tests and so forth, making you a Dartmouth undergraduate. And the second time, I remember all but killing him when I was on my bicycle and he was walking across--All of a sudden I looked up and there was the President of the College and I was headed right toward him, and veered off. The third time I met Hopkins was years later when we were writing a book, and [Sanborn Conner] Sandy Brown ['35], my senior co-author, and I, decided we should interview Hopkins. So it was three encounters with President Hopkins...

But the person you dealt with when you arrived, when I arrived, was a man named Donald ["Don"] Morrison--he was the Dean of the Faculty, and a heavy-duty character. And so I began teaching at Dartmouth, and realized it was a much better way to be at Dartmouth than as a student, because so many of the anxieties and pressures were not there, you lived with your family, etc. The winters were hard. The, as I say, children were sick a lot, it's still true of kids who move here from California.

And it--let's see, what happened? We decided we probably should leave; the middle of the winter was too much. And two things came up. One is, I somehow became aware of a job in the medical school at Stanford, which was related to X-rays and radiation and all sorts of things, and applied for that, I guess. I can't remember how, whether they asked me or I asked them, whether someone who knew a man I'd worked for, Ed [inaudible]. So in the early spring we started that process of getting the hell out of here. I also was approached by Los Alamos, at their initiative, I think, I know it wasn't mine, about coming back there. And I wasn't enthusiastic, but I filled out the forms. And this is again just a cul-de-sac, not irrelevant. And that had progressed, but then a letter came saying, "There's been some problem about your security clearance, so we would like you to withdraw your application." And I said, well, at that point I can't withdraw my application. Had you simply said the job didn't exist--punt! I wasn't looking for a job like that. But I said you can't stop there, that's a whole other saga.

But the Stanford job developed, and we were offered it. I was still an instructor then, though...let's see. I can't remember whether I discussed this job with the Dean or not; in any case I knew I was going to be promoted to Assistant Professor. Today you wouldn't hire a Ph.D. as Instructor, except in Mathematics. And the Stanford job looked interesting, and in due course, probably in May, I telephoned and said we would take it. And then I said I'd be sending a letter, which I did, and I drafted a letter.



And this very difficult debate went on for weeks, because the weather started getting better. May was lovely, and we became nostalgic, and I wrote the letter, and then on that same day went down to the Post Office and said, "I'd like to recover a letter I've mailed." And the guy said, "You have to fill out this form," which I did, and he handed me back the letter. And I'm quite sure I was in the Stanford catalogue that year; but we decided we ought to give a place two years, but that we would not stay in Hanover.

We were going to go either to Norwich or to Palo Alto, but we were not going to have the College as a landlord. We were going to pack a truck, but before we finished we had to know whether we were going to head across the river or across the country. And we did stay, and for a while I felt terrible about that decision too, but I don't now. But that certainly continued to be a somewhat less traveled road, to stay at Dartmouth when Stanford offered me a job. My salary was raised from \$3900 to \$4200. We moved into half of a--of the northernmost of the three brick houses in Norwich. When you go into Norwich, on the left there are three houses which were built in the era of the Norwich Military Academy, and we shared that house with different families, three different ones. And that was--living really improved. And I forget how long we lived there, but I therefore was commuting to Dartmouth, but it wasn't a difficult commute. And the winters still were harsh, but we had a lovely place to live, which is really critical. I've dealt with, as Dean, so often, knowing that housing was going to be the issue of any appointment, and Dartmouth had to be more imaginative about that.

CARROLL: When I look at your appointment, there's a whole series of appointments that come in the '50's, in the early '50's, that Dickey made, that when you look at it, it really seems to me he was professionalizing the faculty. They were very young people who were very bright, who were brought in to bring a sort of shot of life into an institution that had not had a lot of change through World War II. And I think of you and I think of John Kemeny as being two good examples of that. Is that an accurate view of it?

RIESER: Yes, probably an understatement. That is, when John Dickey arrived, I would guess there were, in the college faculty, as it must have been called then, 180 faculty members, of whom my estimate was 160 were on tenure. There were only three ranks: Instructor, Assistant Professor, Professor, no Associate Professor. Tenure was quite likely. The process was a bit informal. There was what we still have, a Committee Advisory to the President on appointments. Members of the committee would, best I can

remember from talking to Don Morrison, come in a bit before the meeting and sit in a corner or someplace and read some folders.

It was...I should explain about the deanship. When John Dickey came, either before or immediately when he came, the then Dean of the Faculty, Gordon Bill, shot himself. And he was not Dean of the Faculty with anything like the responsibility that Morrison had, and all the subsequent Deans had. They then had a triumvirate of three professors, of whom one was Bancroft [H.] Brown, a mathematician--I forget the other two [W. Stuart Messer and Andrew G. Truxal]<sup>1</sup>. So there was a funny interim solution. John was then looking for a new Dean, and Don Morrison had come as Assistant Professor of Government, from Princeton, although there was a whole crowd of people who had worked in one way or another in the State Department and/or the U.N. Founding. Betty Morrison, Don's then wife, was the secretary to Edward Stettinius at the San Francisco conferences. John Dickey, prior to Dartmouth, had been head of that part of the State Department activity having to do with the public relationships with what would happen in San Francisco. Richard Morin, who became Librarian in a surprise move later on, had also been a colleague of Dickey's, perhaps in the State Department, I don't know; all of that is available.

But John came to an institution. He was essentially picked by his predecessor. There was no search to speak of. I believe I'm correct in that Hopkins' announcement of his resignation and Dickey's appointment were simultaneous, which is always amusing. And let's see: there is one episode which you know well, but in order to be sure you have it recorded, I'm sure you know it. John Dickey told me, that's how I know: he had begged Hopkins to give him guidance, you know, "I'm just arriving." John was very young, 37 as I recall, arriving after the start of the year, he arrived in November. He was such an extraordinary person, most people wouldn't know, because he had no huge ego. And--but the story, Hopkins' story was, he begged Hopkins to tell him--You must have something you can tell to a new guy so he doesn't make all the mistakes that you made. Do you know this story?

CARROLL: No.

RIESER: And Hopkins said, "No, you're on your own." Dickey kept pressing him, and Hopkins finally said, "All right, I'll give you one piece of advice. Don't

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<sup>1</sup> Bill was on special leave due to ill health before retiring in 1947 when Morrison was hired. Brown, Messer, and Truxal handled Bill's duties while he was on leave. His suicide occurred after his retirement, in November 1947. *The Dartmouth* April 24, 1977 p. 1, 3. *The Dartmouth* December 1, 1947 p. 1.

have anything to do with murals.” [laughter] And that, of course--you understand what...

CARROLL: Yes, yes! The Orozco murals...

RIESER: Well, the Orozco murals...

CARROLL: ...and then the Hovey...

RIESER: And the Hovey ones. And I happened to be reading some of the material in the Archives on Orozco's visit, because I--the reason for which was something else...and the newsclips, the newspaper article, the Alumni Magazine clips. And Hopkins really stood tall. You may remember, at the same time they had decided that the Rivera murals at Rockefeller Center were not tolerable, and they were covered over, painted over, I think.

At any rate, John arrived with a faculty, most of whom would retire within a decade. And the first thing he had to do was to find academic leadership, and a lot of people were obvious candidates. They were senior members of the faculty, a few of them really outstanding like Arthur Wilson, but Arthur wouldn't have been a good Dean. I shouldn't say that; Arthur might well have been a good Dean, I knew Arthur well, but he was an extraordinary scholar, and extraordinary teacher. And the story I've heard, but I never looked into it, was that he invited people to comment on it, and nominate people. I believe Don Morrison wrote him a letter, indicating what he thought Dartmouth should be like; maybe even what the Dean should do, etc. And John surprised everybody by appointing this Assistant Professor of Government as Dean of the Faculty.

CARROLL: Which proved to be a wise choice.

RIESER: Oh, beyond belief. This was an extraordinary person. Not--John came from the Dartmouth tradition. He was not a great athlete himself, but he was six feet plus, of course, and hiked and snowshoed, not a skier. Very much an out-of-doors person. And Don was really his academic colleague. They were--how far apart in years? They weren't that far apart. Let's see, when Don was appointed--he was 44 when he died in '59. So he was born in 1915, so he was seven years older than I am, and in 1945 or 6 when he became Dean he must have been 32 or 33.

CARROLL: That's very young.

RIESER: And he really took over. Maybe it wasn't that early, it was in those first couple of years. And that's what dramatically changed Dartmouth,

although John Dickey was unambiguously at the helm, having great priority on making it an international place. His first Convocation address-- I don't know if you ever looked at those addresses. There's a book...

CARROLL: A collection.

RIESER: Yes. The first one, in the fall of '46, was entitled "The World's Troubles are Your Troubles," which I had used as the background for material in founding the Dickey Endowment, now Dickey Center. Because that is so much of what we were trying to do. And that first Convocation speech was where he first defined himself.

CARROLL: Now he's the one who started the Great Issues courses, is that right?

RIESER: You have to remember, that was a time when a student was very unlikely to read the New York Times, etc. And the Great Issues course was started by John. It was dramatically successful. The alumni of the period still begrudge the fact that it doesn't exist.

CARROLL: That's right, I've heard that.

RIESER: They don't remember the complaints, and we have to come back to the demise of Great Issues later. But Great Issues--do you know how it worked?

CARROLL: Why don't you explain it? It will be good to have on tape.

RIESER: As I recall it was a Monday night lecture, I think. Coat and tie were required. The entire senior class had to go. And they all fitted into Dartmouth Hall, 105 Dartmouth, which was considerably larger because the upper balconies--they were benches, they weren't... Then Tuesday there would be a question period. Every student kept a journal. People like [Alexander] Alex Fanelli, I think, helped in that course, as did [Thomas] Tom Braden ['40] and many others. Will you be interviewing Tom Braden, the Trustee?

CARROLL: Yes. And it seems to me that Dickey pulled on a lot of his international contacts, and people came.

RIESER: Oh, national and international.

CARROLL: Yes, names that are just amazing.

RIESER: Initially, I mean Atcheson, all these people. Because I used to attend a number of those. And then there would be another meeting, in addition to the Monday night and Tuesday morning, there would be another meeting of the seniors, at which a Dartmouth faculty member, or the director of the course (John Dickey initially was director) would provide preparatory material in anticipation of the next meeting. Gene Lyons, who normally is here, was the last director of Great Issues. He knows much more of the history, but [Franklin] Frank Smallwood ['51] would know a lot, too. [Martin] Marty Sherwin ['59] also, who was Director of the Dickey Center for two years, has lectured about that. He was a student then.

CARROLL: When I look at this, Dickey really opened up this campus to the world.

RIESER: Oh, it was a huge discontinuity. I'm sure that an awful lot of people here who would never have been here if he didn't have the aspiration of making Dartmouth a not only national but international institution. He felt very strongly about that; that was his whole commitment, having been in the State Department, the U.N. and so on.

CARROLL: Did you feel yourself to be part of this movement for change?

RIESER: Well, I was in the Physics Department, but a large number of us were younger faculty members. John Kemeny was appointed in '53, I in '52. But he negotiated a one-year sabbatical first, so he didn't show until '54.

CARROLL: I didn't realize that.

RIESER: Yes. And he was appointed to become Chair of the Math Department, although his position at Princeton was as a--something, I forget what it's called, but in Philosophy. He did spend, as he always pointed out, a year as Einstein's assistant, but--I enjoyed talking with Kemeny about Einstein. I never had a chance to talk to Einstein about Kemeny. But I think that there was a very unequal experience to John. But in my second year here, which was 1942, my salary hadn't been raised to...

CARROLL: Was that 1954?

RIESER: Let's see, '52-'53, '53-'54. In '53-'54 I was an Assistant Professor in Physics at a salary which had gone up from \$3900 to \$4200. I don't quite know how we lived on it. We'd got through graduate school with the G.I. Bill. And then I was approached by Swarthmore College to teach as Assistant Professor of Physics, I think because the head of the department had been someone I knew at Los Alamos, very interested. So I went down to Swarthmore College, and was royally treated, even served

sherry in the President's house, which was considered way out for that Quaker institution, and I was offered the job. I found that we spent all of our time when I met with the faculty, talking about this student or that student, etc., and I didn't have a good feel about the zest for physics there, and yet Swarthmore is a very good place. And I didn't fully appreciate Swarthmore, Bryn Mawr, or Haverford. But also it's much smaller than Dartmouth. I decided Dartmouth could only get better. Swarthmore, which had been--had seen great days under a man named [Frank] Aydelotte, was going to struggle to get back to where it was. His presidency was famous at Swarthmore; the subsequent president, whom I met, died not too long afterward.

So I decided that I would not go to Swarthmore, though I had talked about Swarthmore with a man who was a distinguished philosopher here, named [Maurice] Maury Mandelbaum ['29], a classmate of John Dickey's, who had come from Swarthmore back to Dartmouth. So I thought, "Gee, I'll talk to him about it." And he understood my decision, but he told me something I hadn't thought of, which was, "You really should tell the Dean that you've been offered this job, because he may be interested in your leaving." The thought had never occurred to me. "And it is proper, for you and for him, for him to know that now and not later. You can't later say, 'Gee, why didn't you tell me? I was holding this offer.'" So I went to the Dean, Don Morrison. Don was not an easy person, at first; we became quite close, very close. And I said, "I want to tell you I've been offered the job, and I talked to Professor Mandelbaum and he said I should tell you that, because if you felt that I should take it, this would be a convenient time for me, thus for you." And he said, "Oh, no, I don't think so." I said, "I should tell you that the salary would be \$4800, but I'm not interested in bargaining, because I do not want to take it, but I would if you encourage me to take it." He said, "Oh no, don't do that." And that year my salary jumped from \$4200 to \$4800, which began to be livable, but it doesn't seem livable now.

CARROLL: Not in retrospect, but at the time...

RIESER: There is an inflationary factor. And that began a close relationship with Morrison.

CARROLL: Well, you had--it seems to me, when I look at your career, you were very quickly, then, made Deputy Provost for Sciences and Engineering, under John Masland?

RIESER: Yes, but that--there is space in between, you would have to understand, before that. I don't mean to...I have to go put some money in that meter.

[pause]

CARROLL: You were saying you wanted to go back to your first year on the faculty.

RIESER: Yes, we had got to the point where I had been approached by Swarthmore, and reported to Dean Morrison and was encouraged not to go. But that reminded me of something I failed to say which is crucial to a number of things. Incidentally, I was later approached quite seriously about being President of Swarthmore also, but that's for the future. It seems to me, places I didn't go, I'm always amazed I was given a second chance there.

The Physics Department I had joined had four full Professors and two Assistant Professors, or maybe one Assistant Professor and one Instructor. The Assistant Professor was named Lawrence Hadley. And there was an Instructor, and myself, that was a total of seven. The four senior ones included Mr. Murch, the Chairman; a man, Gordon Ferrie Hull, Jr. ['33], whose father had been a very distinguished albeit very difficult member of this department; we always teased that Gordon had simply succeeded him as a prince would succeed a king; Allen King, who lives in Hanover and is retired from Dartmouth, one of the faculty; and Willis Rayton, also a faculty member, as in Rayton Road.

The person just above me in terms of service (Instructor, Assistant Professor, I don't remember) decided to resign, and it was necessary to recruit someone else. And Morrison obviously had little faith in the four senior people, so he did something which could only make them angry at this. He didn't do it to make them angry, but he knew it would make them angry. And he asked Lawrence Hadley, an Assistant Professor, and me, to work at recruiting a new member of the department. That was rather heady for an instructor, and our number one candidate was graduating from the University of Chicago, named Robert ["Bob"] Christy. So we flew out to Chicago and interviewed him, and persuaded him to come to Dartmouth, which was quite surprising for him to do, as it was for me to come from Stanford. But he came and he retired a short time ago.

That began what was obviously stress in the department. Then in the fall, the Dean of the Faculty said he wanted to meet with the department. And Don Morrison had a mandate from the President, you know, department by department you have to rejuvenate them. And Physics, like others, was very heavily tenured; maybe Physics less so. But I think Don had tipped me off about what he was going to do, I don't recall. He sat down and the Chairman of the department was with him. The Deans asked for

this meeting. The friendship between Deans, the Dean and the Department, was not very great, especially this was a total change in Deans. There really hadn't been a Dean of the Faculty at Dartmouth at all. I know the history of it, so I can say that with assurance. So this was a real discontinuity. He was the President's man, with a great deal of power, and he began after pleasantries, limited pleasantries (and I'm telling you this so you'll understand about Morrison), and he had a chin that juts out the way it juts out in that picture. He said, "This department is rock-bottom, and we have to do something about it." I'm sure he had tipped me off, because he wouldn't have wanted to make me feel that bad.

The four members, the four senior members, were outraged. My new colleague whom I had recruited from Chicago, Bob Christy, came in and said, "You didn't tell me this!" And I said, "Well, I really didn't know it in these terms." And then the senior faculty members--well, the only one who really started to try to foment revolution against the Dean was Gordon Hull, Jr., whose father had been very--who had grown up in Hanover. And I remember our phone ringing all the time in our apartment, as Gordon explained the latest machination. And I guess I was in touch with the Dean on occasion. And surprisingly, and it certainly was a lesson to me for subsequent years, Gordon Hull said, "I'm really outraged by this, and I resign." I said to myself, "Boy, that was easy!" You know, really quite surprising. Because Gordon Hull, Jr. really was a difficult man, and the son of a difficult man, and had a proprietary sense about the department. So Gordon left. Murch, I guess the following year, perhaps, retired as Chairman. Rayton became Chairman. I'd have to get the exact years. And we recruited several additional people.

Lawrence Hadley came up for tenure and was denied tenure, which was the right thing to do, given Morrison's position, but I felt he had used Hadley, and I was pretty sensitive to that too. But by then we had three senior members of the department and four younger people, of whom I was senior among the younger. And shortly Murch retired, so there were two senior people, Rayton and King. And I guess probably a year later--and when Murch retired, Rayton was made Chairman. And that was good; we began to change courses.

I, meanwhile, remembering that picture I had seen at Dartmouth with that two-storey empty room which, in the Dartmouth Alumni Magazine had described a plan to build some sort of a nuclear accelerator there. For some reason I always said, "Gee, you ought to do that." And a member of the faculty named Bill Doyle, who is near retirement now, came from Yale, and I think had done his Ph.D. in nuclear physics, and he and I decided we would build an accelerator. I forget where we got the money; we got



an X-ray voltage generator, 250,000 volts, and we made our own vacuum equipment, and got a source of deuterium. And by using--having a deuterium target (that's heavy hydrogen) and deuterium projectiles, you could bombard deuterium with deuterium and you could produce neutrons. So we really began to generate neutrons in the basement of Wilder Hall. And every so often the voltage would be too much, in the sense that the insulation--we didn't make it ourselves, but if it were moist, if it were in the summer it could be humid, and it automatically shut down. But when it did there was a tremendous clang, because there were disks which made contact and somehow, when it was told to shut down because of danger, you'd hear this rattling metal [makes a sound with diving pitch], and everyone in the building would wonder...

And we did a few interesting things. But the amusing thing (amusing means interesting [inaudible])--the amusing thing was that the cover of New Hampshire Life and New Hampshire Profiles in that year had a picture of me next to New Hampshire's first nuclear accelerator, which shows what a small corner of the world I was in, having come from a place--you know, having worked on cyclotrons in Chicago in, what, 1943 or 44--yeah, '44. Here I was a decade later with this modest little effort, but for New Hampshire it was the first accelerator in New Hampshire. I've often wondered where that is. I think I must have a copy of it somewhere, but I'm sure I can find it. So those were fun times. And the department was really beginning to gel.

CARROLL: Within five years you really had had a complete sort of housecleaning in that department.

RIESER: Just about. But that wasn't even five years at that point. That was short of that. Because in five years I was chairman. That is, in 1955 that was three years. Let's see, Rayton became Chairman. In his second year as Chair, or third year, I don't know which it was--it was in the year 1956-57--he developed cancer and I guess--first of all he had to give up the Chairmanship. Before that though, in 19--two things, one is--Don Morrison, and I forget the year it happened--let's see--I'll come to that in a minute. I think I was right about the dates. I decided that I should take half the term off, or do something else for a while. I don't know why, in retrospect. I may have had a talk with Morrison once, in that period, where he said, "You know, you shouldn't plan to spend your whole professional life here." He did it in a nice way, but I was surprised. I hadn't planned to spend--initially I hadn't planned to even spend a second year here, but I hadn't planned to spend my whole professional life. I expected I would have a permanent position, given all the assignments I'd

had. And I think that may have...what led me to apply for a fellowship in biophysics at Cal Tech.

Now let's parenthetically go back to one point after the Swarthmore thing, and my colleagues knew about the Swarthmore thing, and people like Allen King were not very comfortable with me. Obviously I was a favorite of the Dean and they were non-favorite of the Dean. And Allen wouldn't remember it today (we're quite good friends, we don't see each other often). I remember walking with him, and he's saying, "You know, I don't like bright young guys who think they know it all." This was in my second year, this was after having recruited--after that "rock-bottom department." And I said, "Professor King, I don't feel that way." I said, "You know, when you come from Stanford where I come from, when you come here you have a lot of ideas about things." It didn't smooth it over, but we became friends.

I did leave out one point that I wanted to mention because it's historically interesting. When I arrived, Gordon Hull, Sr. was still--he was emeritus, but he was very much a presence. And he had done the experiments with [Ernest Fox] Nichols, in 1900, on measuring the radiation pressure of light. That is what Dartmouth--that was the most significant experiment done at Dartmouth, except for some of the astronomy work earlier. And that was done in the basement of Wilder laboratory, and involved--it was in a year when you used what's called glyptol and wax, and sealing wax, and all that. But it was a magnificent experiment. I think the results probably were never exactly right, I don't know, but it certainly demonstrated that the--it is related to Einstein's formulations about energy and momentum.

I remember when I first arrived here I was given an office on the third floor, a lovely office, the one previously occupied by the man I replaced, named [Arthur Bond] Meservey ['06]. And I remember when Hull stopped in to see me, and said, "I want to show you something." And he pulled from my shelf a book by [Robert Andrews] Millikan at Cal Tech, subsequently president of Cal Tech [inaudible] plus or minus. But there's a little paragraph there about the discovery of the radiation pressure and so on. And Hull always felt he should have gotten the Nobel Prize for that; and I think, though I have no evidence beyond myself, that he made it a custom when a new person came, to make sure we understood who he was. And he was ahead of his time. He'd gone to Germany to study, postdoctoral, and so forth; he may have got his Ph.D. there, I don't know. So he was very with it at that point, but he was very difficult.

CARROLL: Now would he have--I'm trying to tie this back to Dartmouth's scientific history. Would he have then known the man who did the first x-ray here in the United States, here at Dartmouth?

RIESER: That X-ray was done in February of 1896. I would not have thought so, but I could be wrong. Do you know--I must have given Rich a copy of the book that [Sanborn Conner] Sandy Brown and I wrote, about the history of natural philosophy at Dartmouth. Some stuff is in there that would be relevant. At any rate, Hull was a presence, and it was a rather professional presence. More professional, really, than those who were active members, although he had had an argument with a man named [Norman Everett] Gilbert who had been a fellow early professor, and the two of them had stopped talking for years.

And I remember when Nobel Prizes were announced, there was one in particular. It doesn't matter which. We had a weekly colloquium, and I remember Hull spoke, and he said--I forget who the two were, one was--it wouldn't matter, both of them [inaudible]--and he explained why the first one should have gotten it, and the second one, he just shook his head: "Why would he get the Nobel Prize for that?" That is, this nostalgia about the Nobel Prize was with him forever. But it did--I mention it only because Hull historically was a real presence here, and I rather enjoyed the fact that, grumpy as he was, that he could talk about physics the way he did. He really followed it. Now, back to...

CARROLL: Now, you were made Chair of the department very soon.

RIESER: Well, what happened in 1955 was that Morrison was named Provost, and that was a real discontinuity. There had never been a Provost; nobody understood why Morrison was named Provost. But it was related to the professional schools initially. Hopkins had told Dickey another thing, and that was with respect to the associated schools (that's what they were called, I changed the name to professional), with respect to the associated schools, he said, keep them at a distance. And Dickey became aware that he couldn't do the kind of rejuvenation he was aiming for in what we subsequently, but not then, called the Arts and Science faculty, without addressing the Medical School, the Tuck School, and the Thayer School.

Most seriously, the Medical School and the Thayer School. Because they were much older and much longer [tradition?] than Tuck which, although the first graduate school of business in the United States, was founded around 1900, and the others were much earlier. And Morrison set up a committee to review the Engineering Science program and asked John Kemeny and me to be on it. And I believe that was before I was Chairman

of the department. John was chairman of Math. Rayton, as I say, became Chairman in '54 or '55, then became ill with cancer.

I was still an Assistant Professor, and I said that I had applied for and received a fellowship to Cal Tech, because I had decided that I didn't want to feel that I could only be at Dartmouth, only knew people at Dartmouth. I'd been away from Stanford for four years. And this was a fellowship from the March of Dimes; it was called something else by then. And I was put onto it by a professor at Harvard in biophysics. And I said, "Well, where would you go if you wanted to learn something about biophysics?" I knew something about it, and liked it. And he said, "Well, the best person would be Max Delbrück at Cal Tech." Max Delbrück was a physicist, trained as a physicist, and was early on in the group that Meitner and others were in. He always explained to me that the reason he didn't figure out about fission was that he became too interested in biology and wasn't paying attention. But he went into biology, went to Cal Tech, and did famous work for which he received a Nobel Prize in the bacteria-virus relationship. Bacteriophage, it was called. And I don't know why he agreed to my coming; he didn't know me from Adam. But I applied and had this fellowship. And...

**[End of Tape 1, B -- Beginning of Tape 2, Side A]**

CARROLL: ...doctors?

RIESER: For this foundation. It was called the March of Dimes, but then Basil O'Connor founded it and it was called something else later. And the brief c.v. which I had then included my work at Chicago and Los Alamos, I mean I hadn't been out for long, and the subject of that work had been something like measurement of neutron density and it was either chain reacting or operating piles, what we called a reactor pile, because Fermi had used that name, so it was really just a heap, speaking of [inaudible]. And one of the doctors, I remember, said, "Tell me a little more about your work on the hemorrhoidectomy problem." And I said, "Boy, I am in a different culture!" I couldn't believe it! But I got the fellowship, and probably, whenever there's an embarrassment on the part of a questioner in an exam like that, it's an asset.

And--so I did go to Cal Tech. I was delayed because my wife's mother, who as I say was a well known editor and so on, became ill with cancer, and she died in January, I guess or early February, I don't remember quite when. But finally it was decided we shouldn't let the fellowship go by, so I left early in February. And I arrived at Cal Tech, I didn't know anything

about Cal Tech either, I just knew it wasn't Junior Cal Tech. And Max Delbrück met me in Pasadena at the Huntington Library where the airport bus left you. And he was a very unusual fellow, tall, thin German, and the first thing he said to me was, "Do you play the cello?" I said, "No." He said, "Oh, our cellist has just left." And I felt terrible. I mean, we had never had any discussion, but he was just addicted to music. He had built a house with tiny bedrooms but one gigantic living room, enough for two pianos and you hardly noticed them. And there was music all the time. So I felt I had an uphill battle with him from then on, because I was totally inadequate in an area where I didn't even know I could be adequate. But that was a very invigorating experience, being at Cal Tech, which is a science mecca beyond belief. I was there alone for about six weeks, so I worked very hard. The experiment we were doing wasn't that work for which he became famous, it was something else. It seemed to me very unlikely to prove results, but I didn't care, I was taking a course and working on anything with him was interesting. But I asked him, "Why do you do this? Here you are where you are, and this is an experiment which is low probability." He said, "I'll tell you. I'm a tenured professor. This experiment is not one I could ask a graduate student to work on"--though he did, actually--"I certainly wouldn't give this to a young assistant professor who has to make his way, her way, to be a professor. And when you're a tenured professor that's what you should do." I never forgot that lesson. And that's part of another story as to what tenure was about. But that was an insight that I never forgot as Dean, and used it to the--some were distraught by the way I used that phrase, but...

In the course of that I met another young person named David Dennison, who was a student of Delbrück's, and who actually then, through that contact, joined the Dartmouth faculty and retired--I think he's retired from Dartmouth now, but just recently. And my family did fly out later, and I bought a convertible Chevrolet. I met them at the Los Angeles airport, these three children, one sort of first-grade age, another one in nursery school, the other a tiny child who didn't walk, all in snowsuits, and a lovely warm March day, and me in my convertible, late in the day. And they looked so strange. And I had--I had found a house, and it was a wonderful period, scientifically. And for us too.

So in returning to Dartmouth in '56, in the fall of '56, I knew there was life beyond Dartmouth, and I really felt very pleased with myself. I didn't have another job, but I was able to get a small research grant and start research and so on. And then in the spring of '57, Rayton being ill beyond recovery, they had to make a decision about the Chairmanship. Arthur Jensen had been appointed Dean of the Faculty in '55 when Don was made Provost, but Don was such a strong personality at that--there was

no ambiguity there about the Dean reporting to the Provost, that's a whole other, I guess, picture. But the first time it was set up Dean and Provost, there was just no doubt that the Dean reported to the Provost.

And I should say in 1955 this study of the Medical School had been carried out, and the decision was made to re-found the Medical School as a two-year school, double its size. And that set in motion a great deal, which finally involves the hospital moving. Morrison was the architect of that, and together Morrison and Dickey brought in very, very top people from around the country as advisors, and that was a new approach to a Dartmouth problem, where you went to the top, nationally. And that's really quite different. And when it came to recruitment and so forth, when he went to Princeton and found John Kemeny he had been in touch with a number of people. It was a new self-esteem on the part of the leadership, that they didn't have to beg for faculty members, so it was a good opportunity.

They couldn't very well make an Assistant Professor Chairman of the Physics Department; they also knew that they were passing over Allen King, and that that was trouble. I don't remember what the cart and horse was. I do know that Arthur Jensen (we were at a meeting together in Amherst, and checking out everything), [inaudible] and he said, "I want you to be Chairman of the Physics Department." And I said, "Well, aren't there some problems there?" He said, "Yes, but Don Morrison and I have been talking about it." At any rate, I didn't even know I was being considered, as I can recall, for promotion, though I guess at one point--I guess Don must have said, "You're up for promotion...CAP." And I remember walking down the street with him shortly thereafter, and he said, "You've been promoted to Associate Professor." There were no Associate Professors then. I said, "What does that mean?" He said, "Well, we're re-introducing the rank of Associate Professor." And he said, "It's an appointment with tenure, and you and John Copenhaver and Harry Schultz and one other..." I forget. We became Associate Professors in the spring of 1957, and whether I was asked to Chair the department then, I suspect it was after resolving that issue that I was approached, but--all of that would be available if I wanted to figure it out.

That was a big discontinuity in our life, both because it settled the question of whether we would find a place to live, or buy a place to live, and being given the chance to chair a department which only had one way to go, was really a privilege. And knowing that everyone there, with the exception of the one senior professor, would be very pleased with that decision, without knowing what I'd do, but knowing that I, therefore we, were in charge.

I should say one last thing, before we stop, about housing. Because we lived in the north side of that brick house during this period, which was wonderful. The first year was with a family whose uncle was the Secretary of the Army, so that the Army-McCarthy hearings were going on at that time, and every evening we would all have a drink together and she would call the family to see how everyone was surviving, and that was really fun. They left after one year, and were followed by a member of the Math Department, [Gerald] Jerry Thompson, who's not here. They were there for a year, I guess. And I remember the day [Gerald] Jerry Thompson came to me and said, "I got a call from John Kemeny, I'm not being given tenure." He was utterly taken by surprise. John was not good at this. It isn't because he was brutal or anything. He was just--I think a personal problem with having to tell someone that, and I'll come back to...

At any rate they left, and then for two more years we shared the house with an instructor in Russian, Jack Matlock, and Rebecca. Jack subsequently decided he didn't want to be an instructor in Russian at Dartmouth, entered the Foreign Service, and most recently, for four years, was Reagan's ambassador to Moscow. And Jack and Rebecca kept saying, "You must come visit! We have so much room in [inaudible] House," whatever it was called. We never found a convenient time...and they said, "We're not going to be here forever." But after he quit the ambassadorship, we got--I got him back with the Dickey Endowment for a summer. And he was working on a book, and he even sent me a copy of his book, which is an extraordinary history of that period, with a nice little note in it. But those two years--the four years in that house were rich. But we decided we...that's a whole nother story, I won't go into that now. But it's the way Gene Lyons and Micheline [Lyons], who are our long-term friends here and with whom I've worked off and on ever since, came into the picture. But at that point it's the spring of 1957, and maybe that's where we [should stop].

**End of Interview**

INTERVIEW: Leonard Rieser  
INTERVIEWED BY: Jane Carroll  
PLACE: Leonard Rieser's office  
Hanover, NH  
DATE: August 28, 1996

CARROLL: You had mentioned that you wanted to backtrack just a bit to '52 and '53 and mention something that had occurred then.

RIESER: Well, '52-'53 was the first year I was here. Late in the calendar year of 1953, the President decided to tackle the whole question of student life on the campus, and he created something called "Commission on Campus Life and Its Regulation." There is a long letter from John Dickey dated December 12, '53, which one might want to look at. On that committee were about five faculty members. I think I was half the age of most of the others. And it was surprising that hardly into my second year I had been asked to do that. I can't imagine that I asked to be on that committee, but I certainly agreed, and was honored to be asked in my second year at Dartmouth.

It included a number of representatives of undergraduate bodies, which I had forgotten, among them [David] Dave McLaughlin ['54] who was president of the Undergraduate Council; John Callahan ['55] with whom I've had discussions within the past couple of days, who was head of Green Key; [Joseph] Joe Mathewson ['55], who just, I think, has finished his term as a member of our current Board of Trustees, and some others. An extraordinary group of students, in a way more extraordinary than the group of faculty as a whole.

And this committee began in January 1954, and reported in...toward the end of the academic year 1955. It touched on everything, that's what was so surprising. The letter the President had written, in John Dickey's wonderful way, was followed by an address on the student radio system, informing all students of the creation of this commission. It was chaired by



Frank Ryder, a distinguished member of the German Department. It was called the Ryder Commission, and made pronouncements in the course of these two years, usually in a printed form, of what they were doing. What was striking is that they touched on everything: on sophomore dining (only freshmen were required, initially, to eat in the dining halls)--it was proposed that sophomores be included; the whole question of women guests: should they log into the dormitory? Must the doors be kept open? Things of this sort; the issue of driving and parking. There was a report from the Chairman of the Undergraduate Council Judiciary Committee on the whole question of driving. This was chaired by Jere Daniell at the time. There was a problem about gambling. There was a rule on automobiles, that freshmen couldn't have them but sophomores could, unless they were on financial aid. And that stood in place for years, until the period of the large influx of minority students, most of whom were scholarship students and who therefore, as a group, were denied cars, and that opened up the whole issue again.

I believe it was that committee which proposed the building of the Choate dorms, which was the first space where dorms were built in order to have faculty residence. There are apartments in those dorms, and I understand now there's a return to that in the providing of a house for two faculty members who happen to be married, in the new dorms on Wheelock Street. But the original move to have faculty members living in dorms goes back to the report of that committee.

And many of the recommendations were implemented. For me, the result was getting to know a lot of people I would not have known otherwise, and while that never has been my primary interest, nevertheless it was a reflection of John Dickey's concern about dormitory life, not as opposed to, but as a complement to fraternity life. I don't think he had any sense of eliminating fraternities, but rather had strong feelings about behavior in fraternities. He also was trying to enrich the campus for students who were not members of fraternities, and the appointment of that commission was the beginning of that.

Then I had talked about--I'm going to jump now to--back to the Physics Department and things which happened in the period of 1955, something of this sort. Actually it began before I went on leave to Cal Tech, which I think I mentioned in our earlier talk. In 1955, as I pointed out, Don Morrison was made Provost of Dartmouth; Arthur Jensen became Dean of the Faculty. One reason he was made Provost, one reason there was a need for Provost, was to assess the role of professional schools. The largest concern was the Medical School, and the debate was whether to keep it or get rid of it. And the decision was to keep it as a two-year

school and [Stephen] Marsh Tenney ['44 MS '44] was appointed Dean of that school, and I'm sure you have many people who have spoken to that. That was a complex time.

The one I was involved in was the review of the Thayer School. There had been a general review of engineering education outside of Dartmouth, but in 1954 the Board of Trustees established an ad hoc committee to evaluate engineering education at Dartmouth. And it was--that committee was chaired by Gordon Brown, as I recall, Gordon Brown at MIT, who, in terms of synchronicity, in which I don't have great faith--I read his obit yesterday, I've just been thinking about this and he just died in Tucson. That committee reported that there should be much more science in the engineering curriculum, and all of this is discussed briefly but rather completely in the book, The First Hundred Years of the Thayer School, by [William] Bill Kimball ['28].

But a committee was set up within the faculty to implement the recommendations, and this was the turning point in the Thayer School. Donald Morrison chaired the committee; there were several members of the Thayer School faculty; John Kemeny and another mathematician were on it; Francis Sears and I were on it; John Wolfenden from the Chemistry Department. That committee created what has since been called the Engineering Science Department, that is within the Thayer School. It has a separate chairman, who would not, probably, be the Dean of the Thayer School. And that was the beginning of a great change in the engineering education, and affected the curricula which the Math and Physics Department[s] provided, to make sure that they could implement that.

CARROLL: Did these changes in the Thayer School parallel similar changes in other engineering institutions at this time?

RIESER: I believe so. I believe that there is a sort of a parent report, and I think it may have been started by the engineering societies. Dartmouth was unique in that at one point it had civil engineering, mechanical engineering, electrical engineering. It didn't have an undergraduate engineering science program, and this program demanded a great deal of math and physics, and some chemistry, which hadn't been required before. And a little bit later we appointed a Dean of the Thayer School who complemented those strictly science requirements with the new program in engineering design, which is what makes--this was Myron Tribus--which is what makes this unique. But the appointment of Myron Tribus we can get back to. A number of members of the engineering school faculty I think were uncomfortable about this, because it did encourage much more research on the part of the engineering faculty, and

a few of them were very much involved in such research, and others were not. And that was about 1955.

Meanwhile, I wanted to come back to the Physics Department, which I joined in 1952. It was then chaired by a very senior member of the department named Leslie Murch; I'm not sure I spelled it to you right before, it's MURCH. I remind you that in the spring of that first year the Dean of the Faculty asked me and another person, Lawrence Hadley, to recruit a new member of the department, who turned out to be Robert Christy, and he joined in 1953. At this point a man named Willis Rayton was the Chair of the Department. There were seven people in the department, it was relatively small. And it was in the fall of 1953 the Dean of the Faculty came and announced that in his judgment the department was rock bottom, and that's what led to the departure of, by resignation, one of the four senior people, Gordon Hull, and let me explain [inaudible]. And Murch retired shortly thereafter, so that Rayton was then the Chair in 1953-54, and I think in '54-'55 also.

In 1955 there was a great change with the addition of three new young faculty members. There had been Christy and myself, now there were three more. Mr. Hull had left, and Mr. Hadley, and this was the first surprise to me, with whom I had gone out in my first year to recruit a faculty member, was not promoted. That reflected the Dean's determination to see that department rebuilt. So that in just a couple of years Mr. Murch had left, Mr. Hadley had left, Mr. Hull had left, and a number of young people had come in.

Also in that year, '55, we appointed as a visiting professor, and this is relevant, a man named Francis Sears. Francis Sears was at MIT. He had written what, for a number of years, was the most significant textbook in elementary physics. I remember parties in which we celebrated the sale of the millionth edition—sorry, the millionth copy—and while he was still in the department, the two millionth. The--he was for a year or two a visitor because Mr. Rayton went on leave for a year, and then in 1957 it was clear Mr. Rayton couldn't continue because of his illness, and I became Chair. So that there were two professors...

[telephone call interruption]

CARROLL: So what year did you actually become Chair?

RIESER: In 1957 I became Chair of the Department. It was then a department of eight people. But I think I should make clear that in 1956 I had gone off to Cal Tech, and I mentioned that when I got back I knew there was alternative life from Dartmouth. And also I had been told by the Dean that

I shouldn't necessarily plan to spend my life here. And that's a good thing to be told, especially if you think, "Well, I'd better start thinking of other things I might want to do." And I did when I came back from Cal Tech, that was in the fall of '56, I started a small research project based on the Cal Tech experience, with a grant from an organization called the Research Corporation. It might have been during that year, I don't know when. At any rate, in the spring of '57 it was clear they'd have to appoint a new Chair, and I was asked to do that, which meant they had decided not to appoint Professor King, who was the only long-term person there. At that point the department had grown from initially seven to about eight people, and I began being Chair, and one of my aims was to bring outside visitors in various ways.

CARROLL: Did you have tenure at that time?

RIESER: I forgot to mention that. It was not an important--I may have mentioned it before, but the question of tenure obviously was important, because you couldn't chair a department, especially if you had senior colleagues, without that. And in April of that year, '57, I remember the matter was under consideration, but going to the general problem of how tenure was considered then compared to now, it wasn't nearly the documented procedure that John Masland and I established later. And I encountered the Dean of the Faculty on the street and he said, "Well, you've been promoted." I said, "Oh?" He said, "Yes, you're going to be an Associate Professor." And I mentioned that to you before. And I said, "But there are no Associate Professors." He said, "There are now, there are four of you." And that was a tenure appointment.

And then it was later that spring when the Dean of the Faculty, I think it was just weeks later we were at some meeting and the Dean of the Faculty said, "I want you to be Chair of the Physics Department." And I hemmed and hawed about it because I saw the problem inherent in passing over someone. But I also knew that the department only had one way to go, and it would be a privilege to be there. And that's what I meant by saying, having come when I did, there were things that people were asked to do, so that when I was on the engineering review, I think it was just after I had become being Chair, and John Kemeny was the Chair of Mathematics, so that we were there in part in those roles.

I was faced with the problem of a shortage in the department because of the illness of Professor Rayton, and going over his recruiting notes I identified (and I'm recapitulating now) Agnar Pytte, who was [inaudible] degree at Harvard, and went down, and he agreed to come. As I said, he is now President of Case Western, and was Provost here.

During that period we made--we began expanding the size of the department. And also one of the first things I did was take advantage of some visitor program, I forget, where distinguished scientists would come for a few days. And I invited a man named Polycarp Kusch, who was a Nobel physicist for his work at Columbia University, and he made a big impression on the students, on a lot of the faculty, but also on the administration, in talking what a department had to be. There is some picture, I think in the *Alumni Magazine*, perhaps, of Kusch and myself with this accelerator that we built in the background, and it was clear that we were beginning to be in contact with the world of physics, with such top people. Having Sears come from MIT didn't upset MIT, because from their viewpoint he was of the old school. On the other hand, at that stage in his life he was probably the best-known physics teacher, certainly in the East, and brought to us a skill in teaching and in demonstration lecturing which was such that we would sit in on his lectures. And that's the kind of person you want doing that, someone who the faculty wants to be with I suppose.

So chairing was really fun. I'd encountered, somewhere along the way, a distinguished physicist that--he was French, his name was Alfred Landé--who I knew would be in this country and who agreed to come here for a term as a teacher, and he did, he made very important discoveries in theoretical physics. And also I brought another visitor under whom I got a Ph.D. at Stanford, who, at a time when Sears was unavailable because of a leave, actually rented Sears' house and did the elementary physics. And he was famed on the west coast for his teaching of physics. They both had won what is called the Oerstead Medal.

CARROLL: Oerstead?

RIESER: O-E-R-S-T-E-A-D. He was a famous physicist in the 19<sup>th</sup> century, as Rich knows. And Sears' Oerstead Medal, or a replica, I forget which, is now framed on the wall of the physics lecture room which, at his death, we named the Francis W. Sears Lecture Hall, or something. Sears and I, during my chairmanship when he was first here, decided to completely remodel the lecture hall, because he had come from the place where you had better facilities both for the student and the faculty member. And the current lecture hall, with a much bigger slope, much more useful seats with desks, demonstration capabilities which have now been expanded because of all of our electronic improvements, is one of the good places, and as I say they call it the Francis Sears Lecture Hall.

And again, getting ahead of myself, when Francis--Francis and his wife had no children, and she had gone to Simmons College and Francis had

gone to MIT--I don't know, I think he did--he went to Cornell for a while. At any rate, the bottom line was that they made a very substantial gift to Dartmouth. It's not usual that a faculty member essentially endows a chair, or that was a decision we made, so the result [was] the Francis Sears Professorship. All because MIT had made life sufficiently unpleasant, we were able to attract him to Dartmouth, and in a curious way, at the end he had paid his salary, that's what is so amazing. That doesn't happen very often. A wonderful, wonderful person.

CARROLL: After World War II was there a great influx of interest on the part of the students in physics?

RIESER: I think--I'm trying to think--I think not immediately. I think, and we'll come to it in a minute--I don't think the bombs and all of that did as much as the launching of Sputnik by the Russians. And I wanted to mention that because in 1957 I was teaching an intermediate physics course with Professor Christy, which anyone majoring in physics would take--this was in the fall. And one of the things you discussed in such a course is what's called planetary motion. That's really motion under gravitational fields, where one moves in some type of conic section, like an ellipse. And all of a sudden there appears an experimental planet. You know, in the past everything had to do with the work of Kepler and others in describing the planets and so forth, and here the Russians had launched one. And of course it had to behave as planets do except for one thing: the planets in general aren't slowed down by the atmosphere. And there's an atmosphere around them, but they're not swimming through it. We're not getting closer and closer to the sun, [inaudible].

But one of the things we did was, once we knew the parameters, students were able to calculate certain things about the orbit, and we knew the rate at which the satellite had to lose energy at each turn due to the drag of the atmosphere. Some things were announced, other things we figured out. So that students were able to follow week by week, and that satellite finally plunged back into the atmosphere and burned up.

But it was such a marvelous bit of timing to be teaching a course and then have the Russians launch such a satellite. I think that that stimulated the growth in physics and certainly very much the growth in support by the federal government in science and in science education and in mathematics education. Initially very much in physics, but of course it spread to biology, as it probably should have.

Parenthetically, there's one other thing that was happening in this period which comes up in your research. Don Morrison had taken the lead in

examining the issue of whether Dartmouth should remain on the two-semester system, or go to three terms: fall, winter, spring. You've come across this many times. I believe the matter was first discussed by the faculty in the spring of 1956, and the faculty voted it down. I remember getting a letter from the Dean of Freshmen, who was writing me about something, it said, "You are lucky to be away this term, you missed all these onerous faculty discussions." But the matter wasn't dead, it was just tabled, which is what faculties do (a lot of people do). So as I recall it was in 1957 that the faculty voted for the three terms; it was called 3-3, three course, three terms. And there was no mention, at the outset, of the summer.

But there is a book that is worth having in the record, because it showed the kind of person Morrison was. He and a Trustee named Beardsley Ruml wrote a book called Memo to a College Trustee, it's a paperback. And I believe (I may be wrong, but I believe) that this was posed then. Certainly the idea of operating year-round is part of that book: paying high salaries, having a limited-size faculty and operating year-round. The year-round part didn't come up in this--or if it did, in the discussion, I don't remember--in any case it wouldn't have been supported. That is, summers off at Dartmouth were uniquely a time for scholarship, since the teaching loads were pretty heavy.

CARROLL: How much did you teach? How many courses did you teach during the year?

RIESER: When I first came, I think I taught three courses each of two semesters, and that was onerous, except some of the courses were just a couple of students, as I mentioned, which I didn't think should be taught anyway. As time went on it dropped from three and three to two and three, and when we went to the term system one would tend to teach, as I recall, probably five courses a year. One of the things, and this is getting ahead, but in response to your question: I had never felt that the quantization of work by a faculty member could be mentioned only in courses. Courses are so different. Courses are large and they're small. Courses require a lot of preparation, or a little bit. Courses have labs and they don't have labs. So one of the things that we did was to reanalyze how you should measure a department's load, but not try to tell a department how they should distribute it among courses. That comes--we'll come back to that, because that became a crucial matter.

In any case, I believe that in the fall of '57--I may be wrong, maybe in '58 it was finally implemented--we were operating on a three-term system, and it's important that you know that our terms were different from

conventional quarters, because students took three courses a term rather than four, but the courses were one-third more intense in terms of the meeting times. For me--there was a great uneasiness about this in the humanities at one extreme; I think the sciences were the more comfortable with it. For me, I had only gone to places which were on a quarter system, as a student, except for Dartmouth. That is, the University of Chicago, Stanford, Cal Tech when I visited; so that the term system, in terms of calendar, compared to the quarter system is--compared to the semester system, was an easy adjustment, and I've always liked it. John Kemeny...

CARROLL: How do you...go on.

RIESER: I was going to say, John Kemeny, in a little book (I forget what it was) sort of typical of John, describes the role of a faculty member on the committee. He was on the committee having to do with people working three terms; I was not. And he was the faculty member who, I think, had pressed for this. He worked very closely with Don, and I think John was an important arguer in favor of three course-three terms, which worked perfectly for mathematics. I would say. I think John may not have realized that for those who think that, say, in a humanities course, you need more gestation time. If you're reading a book a week, to start reading one and a third books a week is a major change. I interrupted you, I'm sorry.

CARROLL: I was curious as to why you think that the shorter term works so well for the sciences.

RIESER: Let's see. Let me leave out "so well": why it works for the sciences. We used to teach elementary physics over a year, over three terms--sorry, over two semesters. Now it would be over two terms. What it meant was, you--in the past you would take introductory calculus and introductory physics simultaneously; it was possible, for example (this is just an example) to take introductory math your freshman year fall term, and then take physics winter and spring. There were--for hierarchical courses I think it was an opportunity. I think in some ways in the language

**[End of Tape 3, Side A -- Beginning of Tape 3, Side B]**

CARROLL: ...visitors and the change to a three-term calendar.

RIESER: One of the immediate effects of this calendar change was that every department chairman had to initiate a series of meetings to talk about when courses would be offered, what kind of sequences. Which became important especially in the sciences and required coordination among science departments, especially in the case of Math, Physics and



Engineering. Chemistry a little bit less, but nevertheless we now were in a new situation in terms of in what order one would take courses or, more importantly for departments, when we would offer courses. One didn't experience much opposition from science departments, that I remember, and as I pointed out there was understandable, and today understandable, concerns on the part of departments which--represented by Literature, History, other fields.

During this period, '57 through '59, my first two years as chairman, we did work to bring visitors, and we pressed to enlarge the department; I believe after those first two years we were on the way. I'm not sure it was settled in that second of two years to a department which had been seven people at the time I arrived in the fall of '52 to eleven in the fall of 1960. There also, during this period, one had the problem of deciding among new colleagues, who would stay. And that was a painful undertaking which stuck with me for a long time, because as I looked at the names of people who were here and are no longer here, I can recall explicitly the discussions that I had to have with such people.

In the fall of 1958, beginning of my second year as chair, I had begun discussions with Robert Oppenheimer, whom I'd known somewhat at Los Alamos, to come and spend a few days with us, and this was scheduled for May 1959, in the '58-'59 year. In March of 1959 life suddenly developed discontinuities. I was on my way to Seattle to interview some candidates for the Physics Department, in the first week in March. And I had a call when I arrived at the airport that my father was--I knew he was very ill, but they had decided to go from Chicago to Santa Barbara for the winter, and it was clear that his death was imminent. And my sister said, "You'd better just head straight south," which I did. It was not easy, because there's no flight directly. And he--I forget what day he died, but the day I arrived I saw him, and then by the end of the day he had died.

And meanwhile I had planned to go also to Pomona College to interview some people, and I had to put that off because I had to go back to Chicago with my mother and sister were there. And I then went back out to Pomona College (I believe this is the order) and got a telephone call from a colleague in the Physics Department saying that Don Morrison had died. And that was certainly a huge discontinuity in the lives of many people at Dartmouth, certainly my own. And in the same few days to have your father and the person who is your boss, the Provost of the institution, die, is a great shock.

I remember coming back and going immediately to the Morrison household to visit his wife Betty, and express my concerns. She immediately--she had known about my father--she said, "Oh, I feel so

badly about your father.” Well, my father was 65 years old. Don Morrison had died in his sleep at Princeton, visiting his mentor there; he was 44 years old. He’d been--he’d begun as Dean of the Faculty, I guess, in--probably ’46, or ’47, so he’d been Dean and then Provost for about a dozen years. So he was Provost from ’55 to ’59.

I was talking recently to Frank Smallwood about this, because Frank was the Executive Assistant, or Administrative Assistant, to the President, and we were both very fond of Don, indeed we both have the same picture of him in our offices. He’s retired from Dartmouth too. And he said that he remembers John Dickey coming into his office--no, he, John Dickey, was in his office talking; his secretary came in and said, “There’s a call for you from Princeton.” John left, and then not too long later came in ashen-faced, and he said, “Don died of a heart attack. I’m going home. Call the Trustees.” According to Frank he didn’t see him for three days. It was a tremendous blow, because together, over this period, they had effected so much positive change in the institution. Don was becoming very well known. I believe, though I have no direct knowledge, that other universities, Chicago among them, were interested in his possible presidency, but I don’t have direct evidence; it’s just a rumor in the back of my mind. And for quite a while things were very uncertain here.

I had invited (now you see the connection)--I had invited Robert Oppenheimer to come up, I think in April. He and John Dickey had known each other on some committee, I forget what it was, and I’d arranged for, in the course of Oppie’s stay here, a luncheon with the Physics Department at the Inn, and he and John were going to meet in the Inn bar. I guess at 11:30, I don’t know. And I remember when Oppenheimer came out, he said to me, “I’ve never seen your President in such a depressed state.” And I was aware of that.

Parenthetically, Oppenheimer gave a talk that evening in Webster Hall, which held 1200 people at that time, because the upper sides were there. And it was an extraordinary talk. People were sitting on the floor. It had to do with the universality of certain physical laws, like Heisenberg’s indeterminacy principle, and how finally the international relationships would have to be resolved, because there were certain things such as these fundamental laws of physics which everyone had in common. I wish I could remember more about that talk; I doubt that we have a record of it.

I had a reception afterward at my house in Norwich, and he came over--I remember opening a door and he looked at me sort of uncertain, and said, “Was that all right?” Well, I wasn’t--it was fine for me. I have a lot of feeling--I have a feeling a lot of people didn’t understand it. The applause

was overwhelming: they had sat at the feet of a mystic, because Oppie always was that way. I said, "Oh, I think it was fine." He said, "Well, I threw away my talk and decided I'd just talk about something else." And I said, "I suspected that too." I remember he had a bad back, because he--much of the evening he stood in a way that was against a wall, that was supported. But it was a grand evening, and having him as one of the series of short-term visitors, this inevitably added a connection. I mean, you have to have a science department that was known by people outside, and that they would be prepared to come to, to talk seriously about physics. He gave another talk about physics in a different part of his visit.

CARROLL: Is this before or after his trial, his notoriety?

RIESER: I believe it was after. I don't know whether it was before or after he was given the--I think it was the Fermi Award or the National Medal of Science, by President Johnson. But I wrote to him at that time and got a nice letter back, and goodness knows where all that material is; it's around here. It's either in those boxes or it's in the archives. I'll find it when I turn to it. But he was an elusive, complex character, just extraordinarily complicated.

Now I want to get back to the spring of '57--'59, I'm sorry--because it must have been in late April that I had a telephone call; it was about five in the afternoon. And a fellow said, "This is John Dickey, could you come over to my office?" Well, you never say "no" to that, and I went over, and it must have been toward the end of April--I could look all those things up. And he said--well, I remember, first John Kemeny and I had discussed the whole question of who would replace Don, and I think John, understandably, would very much like to have had that assignment. But at any rate, John Dickey said, "I've asked John Masland to be Provost at Dartmouth, and I would like you to be Deputy Provost." And I remember saying, "Well, what in the world is that?" It sounded very State Department-like, that being his--I think he picked up a lot of his ranking ideas there.

And I said, "Well, you know, I really like being Chair of the Physics Department. I know what I'm doing and I'm good at it, and it's going to take me some time to think about this." And I said, "You know, I'd be happy to give you advice any time you wanted," and he said, "You would be Deputy Provost, which means in the absence of the Provost you would be in charge, but on top of that I would want you to serve as Dean of the Sciences as the assignment of Deputy Provost," and I was Dean of Graduate School at that time too, Graduate Study. I said, "That's very complicated, because you have Dean of the Faculty, who today is responsible for the faculty, broadly, and what will he do?" And evidently, I guess, he had talked with Arthur Jensen, and he said, "Well, he would be

responsible for the Social Sciences and Humanities, and you'll be responsible for the Sciences.”

CARROLL: Is that the first time the two disciplines had been separated out from under the same head?

RIESER: Oh, they certainly--that is, the faculty regard [inaudible] the department [inaudible] all and the responsibility of the Dean of the Faculty, first under Dean Bill but that was irrelevant, but under Morrison until '55 when he became Provost, and Arthur Jensen became Dean of the Faculty. Arthur Jensen reported to Dean Morrison, to Don Morrison, who was Provost. An awful lot of us who had dealt with Don as Dean inevitably found ourselves talking to him about major issues. It must have been a very hard thing for Arthur Jensen, one of the most unusual people there is. I presume his oral history is someplace.

CARROLL: Yes, Jere Daniell.

RIESER: I see. Is it available?

CARROLL: Yes, it is. You [inaudible] and listen to this sometime.

RIESER: Yes, there are so many things I want to listen to. So I hemmed and hawed, as I always do with decisions like that. Whenever you go from a situation you know to one you don't know it's harder. And when I said "I'll give you advice" he said, "Look, I don't want advice, I can get advice." And I said, "You know, there are a lot of people who would like to do this job, and I like being Chairman of Physics." And he said, "I know, I've thought all about that. I'm asking you to do it." I said, "It's going to take me a while." Somehow I contacted a viral pneumonia, I think, something of the sort, and for a person who at times is known for having trouble making decisions, making them while you have pneumonia is harder. So I was recovering from that at home and John Masland came to see me, to talk about it, and I raised a lot of questions to how it would work.

And I also talked again, as I remember, to Maury Mandelbaum, the person to whom I had spoken about the Swarthmore job. And I said, "I have a quandary again, but it's different." And he said, "You have no choice." I said, "What do you mean I have no choice?" I remember being belligerent. He said, "If the President of the place asks you to do something like that, given the crisis we're in, you either have to do it, or go somewhere else. You can't just turn him down when you know the burden he's under."

And then I remember John Dickey coming as I was recovering, coming over to talk about it. He said, "You do have to reach a decision." I said, "Oh, absolutely." And he asked me if there was anything else. I said "No." And so I decided to do it. I remember a neighbor, kind of a nutty neighbor, saying, "I see the President has been visiting you." I said, "How do you know that?" And John Dickey--the Dickey's had a license plate, "99." And this was an old alumnus who kept track of things, and he'd seen "99" parked in the driveway. That just made it worse.

But I did agree to do it. I remember sweating afterwards, saying, "You know, it's going to be hard." I also had said to John, I needed to develop an office. There was no room in Parkhurst for an office for Deputy Provost. If he was Dean of the Sciences I would have to--I forget what I used for a Physics Chairman office, but I said I would have to take over an old classroom in Physics, and some construction would be needed so there could be a real office with meeting tables and so on. I threw out these things in part because I felt it was important, in part because I felt he might say "No" and that would solve my problem. But he didn't. I agreed to be Deputy Provost.

I remember going to see Arthur, and that's why I'm interested to read--and hear part of his tape--and saying something of the sort, "This is a screwy administrative arrangement." And he said, "Yes it is, and if the three of us weren't friends it couldn't possibly work." I said, "You've got it exactly right." And I never--the respect he and I had for each other was very great, and of course it came up--there were times when I would be able to operate quite independently, but when we talked about salaries and so forth we always felt that we shouldn't have one person dealing with a very different pool of money. So we had some system for dealing with that.

That was the beginning of changes in the way the Committee Advisory to the President [did its] business. One of John Masland's contributions was to go from the casual reading of folders a little bit before the meeting, by the faculty members of the committee, to developing notebooks, dossiers--where the notebook would have background of the half dozen or more cases indexed with copies of everything. So that everyone would have the chance at home, prior to the meeting--and in due course I elaborated on that too. And that's certainly the system now. The whole process of reappointment and promotion and tenure became much more rigorous in procedure than it had been before. Probably more rigorous in some decisions as well, but certainly the procedure was much less vulnerable to criticism.

CARROLL: At whose prompting was the process for tenure taken under consideration?

RIESER: The process of the CAP? I suspect that that was Masland's initiative as Provost. So, beginning in 1959 (here's where the ORC doesn't seem to jibe, but I know it was 1959 because I know when Morrison died, I know when my father died) I first had to get someone to chair the Physics Department. And I went to Francis Sears, who didn't want to chair the department any more than I wanted to leave it, leave the chair. But I said, "You don't have any choice, that's what I've been told and now I'm telling you," because I couldn't go back to the other senior person, and the next group wasn't ready to do that. So Francis did agree to chair it, and he chaired it well. His primary interest was not chairing the department or building a department, but he was a very fair and just person and had the respect of everyone. That had been my other concern, that an effort that I had started and could well have stayed with for a half dozen years, I had to pass on.

CARROLL: Did you continue to teach, as Deputy Provost?

RIESER: Some, yes. I might have been half time or something. It was the first-- another matter that was interesting: the President and I never discussed salary, I had never discussed salary with anyone. It was a year-round job, though. That was another aspect of it. And I remember he either verbally mentioned it or--I forget what the number was, something like \$13,000 or \$14,000--he said, "Is this all right with you?" and I said "Yes." But he didn't approach it at all by saying, "You know, this would mean an increase in salary" and so forth. He was very smart about those things.

And that was the beginning of working with a fellow who just enriched my life so much. I remember some of his first dictums. He said, "Now, a couple of things you're going to notice. First of all, in administration things move much faster." And I didn't know what he meant, but of course I do now. Things have to be dealt with, decided, etc., because a lot of people need the answers. And another one I remember was that "You have to remember that administration is walking around. You don't just sit." And he told me his famous story of when he started working in the State Department, he had a beautiful desk (he enjoyed telling this story) and he didn't know what to do. There was nothing in his "in" box, so he couldn't put anything in the "out" box! And he described how Secretary Hull came in and said, "How are you coming?" and John said, "Well, there's nothing in my 'in' box, so to speak!" I'm paraphrasing, but I'm pretty right. And Hull said to him, "Don't think your job is processing your 'in' box."

And I remember that--just, they weren't quick little--not a list of things all at one time, but he had thought out so many things of how one would conduct oneself. I remember, as I began to deal with things as Deputy Provost (and this happened a bit later, I only mention it now because I think of it), the first time I had to tell someone in another department that he would not be given tenure. He was a young chemist, a good teacher, the department had decided they weren't going to keep him. Because so many students took his course, there was a petition in Thayer Hall; I forget the number of names, far in excess of any you could have had in a class. This was typical, you know, someone had written a petition, "please help us." I have this feeling there were a thousand names or more, I don't remember.

But I finally had to meet him in my office. And he was a really nice person, I knew that, and he really had no deep interest in scholarship in chemistry. And he understood what I was telling him, but I was miserable. He left, and he thanked me. He didn't seem to hold a grudge: I don't know what he said when he was outside the door, but he also knew it wasn't my decision, that I wasn't prepared to reverse the department's decision.

I remember, it was late in the afternoon (one puts these things off) and I stopped over to the President's office. I said, "I've had a terrible day." He said, "Why?" I said, "It was the first time I had to tell someone..." I had told people in Physics, probably, but it was the first time I had to tell somebody that the department was not recommending him, and that's the way it would be. And I said to John, "And I did it so badly." And he smiled and said, "What made you think you could do that well?" I never forgot that.

CARROLL: What a good question that is, what a nice and easy way!

RIESER: This is all part of saying he had had a lot of experience, so that the growth that I felt in those years working with him was the same kind you feel when you're working for a doctor's degree with an established physicist, in my case. Here I was working with someone who just towered in the experience he had. I'll come back to that--and I'm sure anyone who has talked about John Dickey (who was not universally liked, but in retrospect I think he probably was). I then did the first of a series of office creations. I've created three or four elegant offices in this place--five, maybe--this one I had approval, but the others--and I redid that lecture room, made a fine office for what was in effect the Dean of

Science, we called deputy provost. And then began asking myself, "What do you do next?" I had no "in" box to even look at.

The first concern, for some reason, was the Chemistry Department. I believe that Don had initiated discussions with a person named Walter Stockmayer at MIT. And we had John Wolfenden as the senior professor, and Stockmayer was already a famous chemist, and for whatever reason Don had become aware of him. So I called up and asked him if I could meet with him. I remember we met for lunch at the Faculty Club. And I stayed overnight at their house, I remember--that's how I met his wife Sylvia. I remember it because there was lipstick on the sheet and I realized that Sylvia was not quite so fastidious about such things. But we came to know each other very well, and Stocky accepted our invitation to move from MIT, where he was a distinguished full professor, to Dartmouth.

The response at MIT was interesting. The Dean of Sciences there, a well-known man named [George Russell] Harrison (all this I get from Stocky), told him that "If you're making a decision like that to leave MIT and go to Dartmouth, you obviously need rest, so why don't you take a sabbatical, go to the Caribbean, think it over?" The president of MIT, [Julius Adams] Jay Stratton, a well-known physicist, understood Stockmayer's motives, and it's hard to guess another person's motive, but one of the things that became clear was that he was essentially turning out graduate students in polymer physics, he was consultant to DuPont, a very effective person.

He was a very outdoors person as well: he'd climbed every peak over 4,000. But he said to me once--I asked him, "How did we persuade you? Because it wasn't the money" (I forget, but it wasn't much)--and he said, "Well, I decided I didn't want to spend my life making replicas of myself." He was also a good musician. And there were other reasons. He liked the outdoors and so forth. So he and Sylvia came to Dartmouth. They had two sons, and it made me realize that if you wanted to change a department fundamentally you did it by the appointments you made. That is, a dean can't spend time thinking about this course or that course, how would you change it.

Don Morrison had changed the Physics Department by embarrassing the senior members, but that wouldn't have made sense because there were some very good people there. But with the appointment of Stockmayer it was on its way to becoming what it is now, a world-class department. And while I wasn't the one who discovered him, I was really



proud of having brought him here. And he just was--and is--a person of great strength.

I felt obligated to help them find a place to live. They wanted to live on a hill, picked a place in Norwich. I think it was land owned by Frank Ryder, who was the Chair of that Commission on Campus Life and its Regulation, who lived near there and had bought this land in Norwich. Walter Stockmayer is a very Germanic name and as a very Germanic person it's not surprising he came to know Frank Ryder, who was head of the German Department.

At any rate, he bought this land, and then the question was, who would he get to build a house? He had his eye on a very modern architect, a student of Frank Lloyd Wright, and he came to me, could I help him find a contractor? And I--there was a fellow who, legend has it, was left on the steps of the Norwich Inn in a basket, John Wilder, who was everyone's architect. So I said, "Would you be willing to talk to my friend Mr. Stockmayer about doing the house?" and he said "yes." He was very shy--big, shy man, Selectman, all of these things. I encountered him later, and I said--I didn't say anything, sometimes you just waited. And he came to me and he said, "I've talked to your red-headed friend." I said, "You have? Are you going to build it?" He said, "Yes. Won't be easy." He used words very sparingly. But I mention that because when you brought someone to Dartmouth, moreso when they came from not only a different climate, you did realize housing was everything. And I always felt it was worth--and I'm sure my successors have too, I don't say that's unique, but it was worth working at helping them get permanent...

**[End of Tape 3, Side B -- Beginning of Tape 4, Side A]**

RIESER: ...was saying, that you could afford to work very hard in trying to find people. Now, I felt with that I would turn my attention to somewhere else. The Physics Department was where it was going to be. The Chemistry Department, I felt we had made an important appointment. The year Stocky agreed to come but before he came, there was a hard tenure decision, and I became aware that hard tenure decisions weren't easy for Stockmayer; it's an important [inaudible].

The big challenge in the sciences was Biology, and I certainly put in a year of life on that. We had a Botany and a Zoology Department; this was a classical way of having departments. There was not much communication between them; they were both lodged in Silsby Hall. Some of them were very good teachers. The botanists always felt

besieged; there were maybe four or five of them compared to eight zoologists.

The zoologists were led by a man named [William] Bill Ballard, who just celebrated his ninetieth birthday, and never chaired the department. I talked to one of the people who Bill Ballard never was fond of, obviously, a man who felt deep down--but no one had ever asked his advice before--so I began going around talking to all senior members of both departments, and I talked to this fellow (I think his name was [W. Byers Unger]). And I said something about Bill, I don't know what it was, I may have even been asking Unger to chair for a short time, I don't know. I don't know how I got into it, but he said, "Chairmen will come and go here: Ballard will always be king." And that was clear.

There were some very good people, strong people, in the Botany Department: a man named Carl Wilson who was senior; a man named Herbert ["Herb"] Bormann, who finally left to go to Yale School of Forestry; and a couple of others. A man named [Augustus] Gus De Maggio who is now a very senior member of the faculty here, but a younger member then. And it was clear that the animosity between the two--"animosity" is too strong. They didn't really want to--the botanists certainly didn't want to be under the same roof. But there was a big revolution in America, or in the world, as biological sciences, which always requires that there be a taxonomic activity which does, in some ways, distinguish animals and plant life--with the discovery of DNA, it set into motion so much research in which the line between flora and fauna was just much harder to grab.

So I had two chores: one was to try to recruit some new people, and the other was to see if we could create a biology department. And Roy Forster, who's now retired and lives in Hanover, certainly encouraged this. I think John Copenhaver in the Biology Department--in the Zoology Department--encouraged it. The botanists were more or less against it. We talked about a department of molecular biology, a department of ecology--various ways of doing it--but to cut it simply by the taxonomic species was not going to lead to the kinds of courses or the kind of faculty we wanted to bring. And that was really a hard task, it took quite a while.

At one point I went to Harvard to talk to the Chairman, Carroll Williams, about possible people in the field of--I guess I was still talking zoology then. And he said, "The best person is Edward Wilson, but the chance of your getting him to leave Harvard is very slim." But I did talk to E. L. Wilson. I'm sure he doesn't remember, but I'm about to write him,

having finished his book, Naturalist, because he was willing to hear me out. He thought about it and then let me know by letter later that he was going to stick with Harvard, where his appointment was quite uncertain because usually you have to go away and perhaps be brought back to Harvard (I forget the details).

But it was a very difficult problem for several years, and we made certain concessions about what the people in plant science, what prerogatives they would have so they wouldn't be swallowed up. And I don't know, without looking at the catalog, exactly what year we cemented that, but we did create a Biology Department. And then we were able to go after resources for a new building, because they were in Silsby, but what we wanted to do was get them together in the same facility, sometimes different floors.

And we began talking with a gentleman named Gilman, who had two Dartmouth sons, Howard ['44 TU '45] and Chris [Charles Jr. '52 TT '53]. And we proposed a Biology Department, a building. We even went further and talked about the Gilman Biological Sciences Center, something of the sort, the Gilman Medical Center. That in itself didn't survive all the discussions, but we looked at rough plans, in terms of location, etc. I wanted it near the Medical School because there had been very limited interaction between the medical science departments. And there were then medical science departments, that is as a result of Morrison earlier, Medical School buildings were rebuilt, and there were medical science departments like Biochemistry, Physiology, and so on. And it was natural to have the Biology Department and a shared library. That was complicated; people get upset sometimes about books being combined, but it was obvious you have to have a shared library.

So Mr. Gilman was interested in this, and I remember sitting on the floor of the President's office with him. I don't recall that his sons were involved. And again, my memory is that not too long after, he said he was going to do it, and sent a check for a million dollars. That's when a million dollars was a million dollars. I, meanwhile, had crafted a proposal to the National Science Foundation, which was then supporting science buildings, asking for \$750,000, which I guess is all we could have gotten from them, and we got that too. So that we had just under two million to be going with that. And then subsequently (I think it was subsequent, I forget when it was built) the Dana Biomedical Library, which exists, made that a complex, and the top of the new Biology Department, Gilman Biological Sciences Department, has a greenhouse on top.

And I think there was increasing cooperation between the two, but a certain amount of blood was spilled, and someone who is in the Dean's position uses up some chips. On the other hand, most of the people there continue to be friends. One of the easy things about being Dean, of course, is that people who are most antagonistic to you probably have left, either at their volition or the department's, so the people who are here you probably recommended for tenure. That is one of the down sides. But achieving a biology department was something I very much wanted, and which has become very important, obviously.

CARROLL: I'm curious about the Hannah Croasdale story. Do you want to speak to that at all? She connects with the biology, which is what made me think of it.

RIESER: Sure, of course. I don't remember the dates without looking them up. I've talked, of course, to Mary Turco [1978]. I haven't read her thesis though she came and consulted me... Hannah was a Ph.D. biologist. Her work was in mycology, which has to do with fungi, especially Arctic work. I think she came to Dartmouth essentially as a technician.

CARROLL: Mm-hm, in the 30's, I believe.

RIESER: Yes. In the summers, I believe, she went off to the north. She--but there were no women in the Biology Department; there were hardly women in any department. I think that--I can come back to that. But I rather believe that she was not treated very well by the senior male faculty, and I think I'm understating it. And she was, in terms of publications, as well known if not better, than most of them. Roy Forster was quite well known; Ballard to some extent, and he did unusual work in embryology; I think Bill could have, if that had been his mode, pushed for her having some title other than "Research Associate" or whatever it was. And I talked to various people about her. The bottom line was, in due course, I pressed, I think, for her to be an Associate Professor and, I believe, finally Professor. And there is some correspondence between us, very nice letters from Hannah. But it was, in my judgment, a blatant example of difficulties that a woman scientist faced here or anywhere else.

CARROLL: Would it have been hard for her to teach male students? Would they have listened to her?

RIESER: She did some lecturing. I'm not sure (and this was true of a number of the initial female appointments to the faculty) that teaching the large introductory classes would have been easy at all. I think it would have been hard. I think teaching upperclass students in a field in which she

had a world-class reputation, there could be no doubt about her competence. And I'd have to go back to see what she did in that regard. I'm sure she must have had some thesis students. Is John Copenhaver on your list?

CARROLL: I've spoken to him already, yes.

RIESER: Did he mention Hannah?

CARROLL: Yes, he did, and--

RIESER: Because I recall that he was quite supportive of these efforts.

CARROLL: Right, and was rather more condemning of his fellow biologists. But she bears no malice, which I think is interesting.

RIESER: No, that's extraordinary, and I know that's true. You have to remember that John was a graduate of this department, and had the problem himself of coming back as an Assistant Professor with his teachers. He had been--Roy Forster had been his mentor as an undergraduate, and he went off, got his Ph.D. and came back, and he and Roy were always close. But I think he found Ballard and the others very chauvinistic to say the least. So the difference in our view of that is not really a difference in view but a difference in the way John expresses himself and the way I express myself, and you could touch a million items and find that was true. But certainly she was not treated well, and in this day and age it would have been thought of as flagrant, probably. But she was respected for her science. I've never heard anyone run that down.

CARROLL: Well, so you were making hires right and left in this position as Deputy Provost.

RIESER: We were adding a lot of scientists. I can't remember all the people we hired in Biology. I went through the catalog in Physics. I could do that in Biology, but it's not worth it.

CARROLL: Were these all new positions?

RIESER: There weren't many new positions. We had to sort of work that out. There were, as I say, four botanists here--Carl Wilson, Herb Bormann, a man named [William T.] Jackson, retired, and, let's see, this man named Charles Lyon, who was of the old school. But all of a sudden the work he did became of great interest to NASA [National Aeronautics and Space Administration], and in his last years he was consulting on the

whole question of what seedlings do in non-gravitational environments. He'd been doing all these experiments with centrifuges and so forth to show how the roots above and below reacted to different gravitational--different acceleration fields, and then that became known and he would have experiments aboard satellites. So a guy who had sort of been [inaudible] all of a sudden is of national consequence.

And then we began appointing some additional people in ecology: Gene Likens, I forget who the other was. They all became--a number of them have left since--Herb Bormann, who was very good and who took the lead in establishing what's known as the Hubbard Brook Project--and one thing I learned to do was to listen to people like Bormann telling me how very important it was to have a watershed system, in this case, which could be studied over the years to see what happens with the cutting of trees, etc. And not to think I had to know; that is, if someone that had this much respect as a colleague felt strongly, one should try to do it. I looked into things, but I mean I didn't--you can't try and learn the biology that would put you on an even plane in arguing. You can ask hard questions. And there's no reason for you to know, but Hubbard Brook has become quite famous as one of the controlled ecological sites. It's in the White Mountains, and it's a place where they can control the cutting of trees, the flow of water, the leaching of materials; and Bormann and Likens are famous for that.

I remember Herb being offered a job at--what was it, Wake Forest or Emory or something--and one of the problems you have as Dean, (or in this case, I don't know, I was probably still Deputy Provost) was figuring out who you wanted to work to keep and who you didn't, and how you would work to keep them. And you always felt badly when someone you really wanted chose to leave, but it also told you you had good people around you. And he kept me in suspense a long time, and then sent me a very brief note, something like, "Roses are red, violets are blue, if you'll stay here, I will too." And I called him up right away, and said, "At the moment you can count on me if I can count on you."

Those are great feelings of satisfaction: that is, recruiting people you really wanted, but also retaining people you really wanted, against the pressures of places inevitably wealthier than we were, and usually with more facilities, though building the new building helped a great deal. And finding a judicial--being judicial about the roles of those two disciplines, forcing them into one but making sure you were fair--well, in due course there was an electron microscope facility built between--actually it's between Dana Library and the Medical School, which is under the Provost's aegis now, the microscope facility. But it became

possible to have instrumentation which could be utilized by people in the different fields, chromatography and so forth, which it doesn't matter whether the material is from a plant or an animal, when you start analyzing its genetic structure and so on.

So the department has pretty good equipment, which was got often from research grants. And the introductory course, rather like the one I had at Chicago but for no reason, has participants from many departments--I mean, a top person trained in animal science and a top person trained in botany might give the course together. So that it was a much more efficient way for students to learn, than to have to choose, or take courses which inevitably had a certain redundancy, unnecessarily, because it was taught in different ways. So this is just summarizing the--first chemistry and then biology. Biology was a much harder problem.

CARROLL: Is this the time when computers begin to make an appearance on campus and begin to be pushed--the time-share?

RIESER: Yes. Let's see...what...?

CARROLL: [inaudible] '59, I think.

RIESER: That sounds right. John Kemeny and I, of course, were very closely together, and one had to have a certain respect for John. He had turned this Math Department around, and the whole computer thing, his concept was unique: namely, you develop something which will be available as a library is available to every student, free. And they wanted to set up (he and [Thomas] Tom Kurtz) the first time-sharing system. IBM didn't make a machine that would have worked. It turned out that General Electric, which later sold out, I guess, to Honeywell, did make a machine that could have 24 ports. And using old teletypes, that is, dumb workstations. "Dumb" isn't a pejorative...

CARROLL: I understand, yes.

RIESER: ... they began working at time-sharing. But I remember the decision, and I have to add a parenthetical remark: There was, and I think still is, though I've lost interest in it since I'm out of it, an organization called the Associated Colleges of New England. The acronym is ACNE. And there were 15 institutions, public and private--well, they were private except for places like University of Vermont, which had a private part, has a private section. Brown and Harvard and Yale and Dartmouth. It was concerned primarily with undergraduate, not graduate. The Ivy League institutions

were formed basically to deal with athletics, but this group was--it included Wesleyan, Williams--and we met once a year.

I remember we were meeting at Brown, and John Dickey and I must have got talking about the computer thing. I said, "I think we really have to go ahead and do this, and it's going to cost a quarter of a million dollars." And he wasn't prepared to figure it out himself, but he said, "Well, tell me the downside risks." I said, "If this system doesn't work, we have a \$250,000 computer and we can sell it for maybe not that much, but it's not a \$250,000 risk at all." And I was sort of John's emissary here; that is, a necessary condition for doing it is that I would be able to persuade--that I would understand it well enough to persuade the President in his terms. I had to understand it in John's terms, but you're always in that position. And that resulted in bringing the G.E. computer to the basement of College Hall, and started the time-sharing business.

CARROLL: And as I understand it, G.E. gave the equipment also with the understanding that Dartmouth would work to develop educational software, or educational programs, I guess they called it then.

RIESER: Yes, we had some obligation, but we didn't get it free. We had some money in it.

CARROLL: And there were 24 stations all together?

RIESER: That's my memory. But that was considered a lot. And what basically happened was that the computer programmed itself to keep going around to the 24 ports. It was slow, but it was--it was the first step. And the basic concept, as I said, which is John's--John was not a hardware person, but conceptually, and here his long training as a philosopher was important--I don't mean in the philosophy alone, but his training was in logic, and that was his license, but--still it's Mrs. Kemeny's license--they had to develop this computer language.

They first had to program the computer to operate, then they had to develop a language, which was called BASIC (I forget what that stands for) which recently has reappeared. I read an article, it was probably a week ago yesterday, it was in the Science section of the Times, about something called Quick Basic. John Kemeny and John [Tom] Kurtz much later formed a corporation to re-do BASIC, and that was True Basic. I don't know that that got very far. Quick Basic, I believe, is something that Bill Gates is involved in, I don't know. It will get far. At any rate, John became very well-known for this and I believe that that



was before we began the discussion of doctor's degrees. I think we started the computer business first. I'm glad you brought that up because it was critical.

CARROLL: When was Kiewit built, then, the building for computers? Is that at this time, or is it a little later?

RIESER: It was a little later, and again I don't know the exact year and I should, because then I'd know which hat I was wearing [Kiewit was dedicated in 1967]. Because I was Deputy Provost for five years, then became Dean of the Faculty, then Provost. And I don't know, without looking. You could find out. You might look that up. I will say one thing about it, there was no connection between Peter Kiewit ['22] and computers ab initio. We might as well insert this--everything will not be chronological, I'm sure you've dealt with that. Orton ["Ort"] Hicks ['21], who was Vice President of Development, with whom I worked closely for a long time, a wonderful person--he's--he lives at Kendal now, and he's in his upper nineties. We would go out to raise money often. We only failed once, with a guy who--he never has forgiven the man for outfoxing us--but we decided that Kiewit, being in the business he was in--I don't know how we decided, maybe he would be interested in coming about--he was in the earth-moving business, on a huge scale. He came from Omaha.

But during the period when we were building--the United States was building these huge electronic detection systems in the north, one was called the DEW Line, it was something--the EW of DEW was "Early Warning," I forget what the D was. And it was at a scale that, of course, has been equaled by other companies since, Bechtel and so on, but Kiewit was way ahead of the game. Peter Kiewit was a very mild-mannered, sweet person who had obviously made very good business decisions; was a great benefactor of Omaha; I think a deeply religious person; and he had gone to Dartmouth for, at most, one year. But in Ort Hicks' view, if you'd been at Dartmouth for a year you were an alumnus, very much so. I don't think I have it mixed up. I think Robert Frost was half a year and Kiewit a full year, but it could be the reverse.

So a date was made for us to have dinner with Peter Kiewit at the Chicago Athletic Club. Why it was there I don't know, but there it was. And Ort was asking for a million dollars--again, still big money. I thought half a million would be a tremendous achievement. But Kiewit had his cards close to his chest, and the dinner--I thought it was quite satisfactory conversation, but Mr. Hicks left very discouraged. I said, "What's wrong with you?" He said, "I don't think we're going to get the million dollars." I said, "I think we're going to get half a million dollars."

What have we done? We've had airfare to Chicago plus one meal. What do you want?" And we did get half a million dollars, and we made Kiewit famous.

That is, the bottom line here is that one thing led to another. There were more gifts from Kiewit, and later a much bigger commitment to work at these things in Nebraska and elsewhere, so--another story, way down the line. But the important thing was that this guy, who began with negligible interest in Dartmouth, and I think no preconceived notion that computers was his thing, was persuaded to put half a million dollars on this somewhat hare-brained scheme. And we both have to look up the date of the building of Kiewit. I think it is important to do.

CARROLL: I will do that. Now when did you move then to become Dean? How did that come about?

RIESER: What happened--and there are some other things that I want to mention, but let me answer your question. Let's see, I became Deputy Provost in 1959. In 1963, I believe it was, Arthur Jensen resigned as Dean of the Faculty. I suspect, though I don't have the exact facts but I'll check there, that John Masland the Provost may have taken initiative there.

The job of Provost here is very difficult. I've never watched a successor as Provost who didn't feel that, some more than others. And the reason is, we don't have a faculty, per se. I reported to John Masland, Arthur did, the Deans of the Medical School, Tuck School, Thayer School. And incidentally, one thing that I think occurred at that time was the--I think I have it right--the appointment of [Carleton] Carl Chapman as Dean of the Medical School. The exact date of that...

**[End of Tape 4, Side A -- Beginning of Tape 4, Side B]**

RIESER: ...wanted to be Dean of a four-year medical school or, let's say, a full medical school. It went from a two-year to a three-year, the idea being you would go eleven months a year. I think that was one of--probably one of the conditions of his coming, though I'd have to ask him. I do remember the Trustee meeting at which this was discussed, and I raised the--I think I was still Deputy Provost--the obvious question of endowment. I said, "You know, this medical school has negligible endowment. It can get by as a two-year school, partly because the cost per student for a two-year school is less than for a four-year school." I issued a warning, I remember, at that board meeting--it was easy to say [inaudible].

I think that the commitment to create a full medical school, which had existed here until 1914--it's important that it was created initially as a medical school in the 1790s--and it was only in 1914 when there was a famous report by a man named Flexner, which proposed that a number of schools become two-year schools, and Dartmouth was one of those. And then it was re-founded in '55 as a two-year school twice the size. And then in due course the question of making it a four-year, a full institution, came up. But it never had generated the kind of endowment through all those years as a two-year school that could easily sustain four-years. It's getting by now, but partly because of contractual relations with the Clinic, in terms of money.

You asked when I changed. Arthur Jensen resigned as Dean in 1963, and John Masland, in addition to being Provost, became Acting Dean of the Faculty for the year 1963-64. He had both jobs; I think he wanted to figure out what to do about it all. I also know from my own experience that being Dean of the Faculty is basically more fun than being Provost, depending on the President and the things you want to do. John had made a very important initiative when he first became Provost, in raising from the Ford Foundation \$750,000, and creating something he called the Comparative Studies Center. And I'm sure you've come across that in your tapes. I don't know whether [Lawrence] Larry Radway is on your list or not, but...

CARROLL: Yes.

RIESER: Have you talked with him?

CARROLL: Not yet.

RIESER: I see. But Larry and [Francis] Fran Gramlich became co-directors of the Comparative Studies Center, one a professor in the Humanities, one a professor in Government. And this provided opportunities, some of which were perhaps [inaudible], but in general was a huge opening up of the faculty activity into very much--to quite an extent, into the less developed countries of the world, but not exclusively that. And so, for a long period there was--it was a great enhancement of Dartmouth as a--with an international reach, which of course is what John Dickey wanted also.

John Masland carried on that job, carrying both positions, for one year, and then I guess the year John Dickey asked me to be Dean of the Faculty. And we just--I agreed to that with a horrendous number of conditions. I said, "I like being Dean of the Sciences." It's the same old

story. He said, "I know." I said, "But I have a couple of feelings about the faculty deanship. First of all, it can't just be located in an office in Parkhurst. It has to be located in the context of faculty departments, just as the Dean of the Medical School would be in the Medical School, etc." And that would mean finding some space in an old building, and I made a suggestion about Wentworth Hall, which was underutilized.

I said, "Secondly, we have chairmen of divisions--Dartmouth had three divisions during this period, Science, Social Science, Humanities--they weren't--we had division meetings a few times a year, but it wasn't--didn't have too much meaning, because the chairman of a division had no personnel responsibilities. So I said we would have to appoint three Associate Deans, one for each division, who would be relieved, maybe a third teaching, and who would be able to focus in as I've had the privilege of focusing in on disciplines that I understand, and be seen as advocate for departments. "Fine," John Dickey said. I said, "Another thing that's wrong" (with what we were then beginning, I guess, to call Arts and Sciences, I think John Masland introduced that notation from the college faculty or whatever it was, or undergraduate faculty, to Arts and Sciences)--I said, "There's no sophisticated budgeting, and I would have to be able to go after a budget officer who was trained in controlling budgets," and many other things. "Fine," he said.

I ran out of demands! And so it was agreed, and in the fall of '64 then, I had begun by renovating a little bit of space on the second floor of Wentworth Hall. Two of the three division chairmen were named Associate Deans; a third one, for whatever reason, we kept as Chairman. I think he didn't want to be; he was a very senior faculty member. But we included him that way. And then I started recruiting for an Executive Officer, as we call it. And this was when a man named [William] Bill Durant, who came from--he had been in the Navy, had been a Captain in the Navy. He had a lot of Supply Corps experience, hundreds of millions of dollars of activity, and we were talking one or two or three million. And he was offered a job also at Yale. Why he took this one, I don't know.

And so we set up an office structure with three little offices for Associate Deans, an office for Budget Officer, an officer for graduate study because the Associate Dean to the Sciences (I had also been Dean for Graduate Study). And we have to come back to the graduate study question because that preceded my becoming Dean. And then I made a very nice office for the Dean of the Faculty, currently occupied by Jim Wright, in the corner of Wentworth Hall. [Nice in that it] had two attributes: one is that you could see much of the campus out the

windows; it had a lot of built-in bookshelves and a huge built-in blackboard, which is there to this day. I once, liking scientific things, I may have mentioned, I once had that telescope, which is in the portrait in the hall, sitting as an artifact on a round table we sat around. And some faculty members who had used it assumed that I was using it to look in the--it had a very good view of Reed Hall and a few others. And I said, "Well, try it." It was one of the original objects brought over by the group that John Wentworth--John Wheelock--sent to England.

At any rate, at that point there was a Provost, John Masland, with his office; the Treasurer was John Meck. And that was another one of my arguments with the President, saying that the Dean of the Faculty has to be able to have a foot both in the Administration, but also to be seen as an advocate for the faculty, and I'd rather have the Treasurer across the Green. I don't mind walking. They also were tight for space in Parkhurst. It is now expanded much more than that, because the offices are much [inaudible]. So that was an easier decision than the decision to go from Chairman of Physics to Deputy Provost. I don't know why.

CARROLL: Let's back up just a bit and talk about the question about graduate studies, that you had to deal with as Deputy Provost.

RIESER: That was--initially, Dartmouth had--let me explain. The initial pressure in this time came from John Kemeny, who wanted--he had done many innovative things. He had established what were called the John Wesley Young Instructorship of Mathematics so people would come for two years but not stay; he was trying to deal with the fact that he had a pretty tenured department. It's important to give one piece of background, that you may know well. The Trustees had voted in the last decade of the previous century to offer graduate study. Not much had come of it. I think the first person to get a Master's degree was named Katharine Quint [1896], but I'm not sure. Do you know about Katharine? I wrote a paper on that which is in the *Alumni Magazine*. That's where I'd have to look for it, because it's also in these boxes. And then it was utilized by something called the Dartmouth Eye Institute, and a man named David Bisno--do you know David?

CARROLL: Yes, I know David well.

RIESER: He's everywhere. Very interesting. That's another topic. The Dartmouth Eye Institute was established up the road. I know that my wife, who was a student at Goddard College, had come there because she had this disease called aniseikonia, which is a problem involving different perception in the two eyes, and other things. It's more complex

than astigmatism. And the head of--the creator of this was--what's his name? There were two people. Hadley Cantril, who went on to Princeton, but Adelbert Ames, a Dartmouth alumnus, I believe, whose house has become Montgomery House, but that's way down the pike--established the Dartmouth Eye Institute, made many models which probably are owned by the Psychology Department, where you can see the problem of perception. Over maybe a decade, and you can check Bisno's, but they awarded seven, I believe, Ph.D. degrees. And those were the only Ph.D. degrees, and that ceased, certainly, before the Second World War.

John then opened up the proposition that we create a new graduate degree in mathematics, not the Ph.D., but we'd call it Doctor of Arts. There had been a lot of discussion about this in various mathematics societies, the need to provide degrees for people who might not do original work in mathematics--which is terribly difficult to do, I must say, compared to physics, because in physics you have the whole experimental element--but whose theses would be expositions, sophisticated expositions, about mathematics. That would really train people who would be teachers of mathematics available in many colleges, which were having a hard time getting mathematicians. I think John really believed in that. I also think he believed--I knew the history, he didn't--I think he believed he might be able to get support for this very innovative graduate program, while he wouldn't get support for a conventional Ph.D. program. And he also had a sense, which I think was incorrect, that this was going to be a major U.S. direction for graduate study in mathematics (not the major, you don't beat the Ph.D.); but that we would be out ahead, but there would be many others.

And I remember the way things like this were considered is that you first go to the division involved; that's how we did business. This would require a vote of the Science Division, and if they agreed, a vote of the faculty, and certainly, of course, a vote of the Trustees. And the necessary condition for that would be that the Administration would take it to the Trustees.

I remember--I don't know--there are two episodes I'll mention to you. I remember--I don't know whether a meeting of the Division was called. As I recall--I think the Associate Dean was Chair of that Division, I'm not sure--and the subject was the Doctor of Arts program in mathematics. And this was a set of people all of whom had Ph.D.'s, all of whom, as a matter of principle, favored graduate study, though there were issues of the economics of it. And John was confident that, with a Doctor of Arts program, which was new and innovative, he would get a good deal of

outside support. And he made his presentation, the discussion went on, and as I recall became increasingly critical. John was an excellent parliamentarian. He could see that this was a meeting that would be better if it ended than if it went on. So I remember his saying, "I move the meeting be adjourned." Someone said, "Well, wait a minute, there are things I want to say!" He said, "It's not debatable," which is true. That is, a motion to adjourn is not debatable, for obvious reasons: if you could debate it, you haven't tested it. And the motion to adjourn won. And that was the end of that phase of the discussion.

CARROLL: That was clever!

RIESER: Oh, very--he was very good at that. Sometimes too good, because people of course left saying, "Oh, he doesn't want to hear [inaudible]." And another episode which is famous among a few of us: This was being discussed at other universities, and there was a meeting of a mathematical association at Yale, and John was to be a featured speaker. And we decided we'd both go down, because this was going to be a case where this discussion had a much broader audience, representing many places. John owned a--was it a Mercury or a Ford? Whatever it was, it had an engine much bigger than that car normally came with. And in we got, put on our seatbelts, he lit a cigarette, and off we went.

And we were never short of things to talk about. We were arguing about this, but of many things about Dartmouth. You know, I relished conversation with John. He was interesting, and I could challenge him, in part to see what he would do. And I remember we were due for lunch and then he was to lecture at 2:00. And I looked up and saw a sign, "Greenwich, Connecticut." I said, "John, I think New Haven is before Greenwich." He stopped this big red powerhouse and wheeled around and said, "You know, you're right." So we turned around and he said, "I think I'm out of gas, or almost." So then we managed to get to a gas station, but for some reason the car had heated up badly. One thing after another!

So finally we crawled back to New Haven, parked the car, made a guess at where the building was; we found it without trouble. We walked in and my friend (I'm not sure he was my friend then, but he became a good friend) Kingman Brewster, since it was his institution, was opening up the meeting, and I think was about to introduce John, though he wasn't apparent in the audience. But as the introduction ended, John simply walked down the aisle and gave his talk. We finally got back at the end

of that day: we were pretty tired, it was really something! It was one of our famous adventures.

One thing led to another with respect to this, and it was clear that the faculty was not going to support the Doctor of Arts program. I think they would support an option under the Ph.D. for a somewhat differently defined--I'd really have to look up and see how we resolved all that. But the bottom line was that the Board of Trustees voted a Ph.D. degree in Mathematics. The economics of it were not unfavorable, because you build in a certain amount of teaching assistants, and teaching done by those assistants. I forget--Oh, I know what happened. The Rockefeller Foundation was making a gift of a million dollars to the Medical School, I believe, and we had gone to the Rockefeller Foundation. I think Warren Weaver was still there, I don't think he had moved to Sloan [the Alfred P. Sloan Foundation]. And they didn't make the gift, but my memory is that they said the first year's interest on the million dollars (the \$50,000 we were asking for starting) would go to the Math Department, from the Medical School gift. I'd want to doubly check that out, but I couldn't have made it up.

And so it started. Obviously the physicists, the chemists, everyone said, "Well, gee, if we'd known we could do that we would have." So then a much bigger pressure started. And I was both Deputy Provost and then Dean of the Faculty, and for a while I was Dean of the Faculty and Dean of Graduate Study. But I shifted that back to the Associate Dean, in this case, I think, [James] Jim Hornig, because all of our graduate programs were in the natural sciences, and it made sense. And for quite a while that was a given, that you would be Associate Dean for the Sciences and Dean of Graduate Study. Now I think we should stop, because...

**End of Interview**



INTERVIEW: Leonard Rieser  
INTERVIEWED BY: Jane Carroll  
PLACE: Leonard Rieser's office  
Hanover, NH  
DATE: October 22, 1996

CARROLL: When we left off speaking last time, we'd really gotten to the point where you were Assistant Provost...

RIESER: Deputy Provost.

CARROLL: Deputy Provost. And that was a position you held from when to when?

RIESER: That was from July 1, 1959, the year in which Don Morrison died, until five years later, '64, when I became Dean of the Faculty.

CARROLL: And then from Dean of the Faculty you became Provost?

RIESER: I was Dean of the Faculty from '64 to '67, and then I continued to be Dean of the Faculty but I also held the title of Provost.

CARROLL: What I'd like you to talk about is, there seems to be a lot of changing of titles at this time, and I'm wondering: is this just semantics? Did the job change? And what the duties were, attached to each of the titles?

RIESER: Well, to do this fully runs a whole gamut in time, with a lot of material, peripheral but important in this period, omitted. Having said that, it is an interesting topic in itself, and relatively unique to Dartmouth. That is, Dartmouth did not have a Dean of the Faculty until a man named Gordon Bill was appointed to this by President Hopkins, and he was not a very effectual dean because he didn't have the assignment that could make him effectual. And toward the latter part of his period he (based on what others have said, I never met him) appears to have had emotional problems, and finally, I believe, destroyed himself.

When President Dickey came in, and I forget when this occurred with Dean Bill, but a triumvirate of three professors had been set up as the Dean, if you will, and President Dickey then had the problem of finding a new Dean of the Faculty and defining the job. Because until then the President was effectively Dean of the Faculty, though he had Gordon Bill. And despite some people, senior people, who could have been Dean, he chose Donald Morrison, an assistant professor of Government. That caused a certain amount of faculty resentment, but it was the kind of move that presidents make too infrequently, and still occurs too infrequently, of really working at who would be the dean they wanted, or whatever, and reaching through hierarchies to do that. Morrison then began, and I may have said some of these things before, there's a danger of repetition, so you tell me...

CARROLL: I will, don't worry.

RIESER: OK...essentially refounding the faculty. Almost all of them, almost the entire faculty would retire within a decade, so that he, both through lateral appointments such as that of John Kemeny in 1953 or through appointments at the introductory level, assistant professor, was re-founding a faculty, by and large, during his years as Dean. There's a lot to say about that, but jumping right to the issue, it became clear to the President and Dean Morrison that the professional schools, then called (I bet they're called associated schools) were being left behind in the Dickey-Morrison years, and that, beginning with the Medical School, it was essential to have leadership at the top administrative level to review the Medical School, the Tuck School and the Thayer School. And in order to do that, Don Morrison was named Provost. That was a name not unfamiliar in academic circles, totally unfamiliar at Dartmouth.

And then together, I assume, they appointed Arthur Jensen Dean of the Faculty. Arthur had been Chairman of the English Department and, I think, quite an effective person in his recruiting for that department. I sense that Morrison and Jensen worked very well together. Morrison had a capacity to, having given up some of the responsibilities to the Dean of the Faculty, to let him be very much on his own, I think. Although, as far as people like myself and John Kemeny (I had been brought here as an instructor by Morrison), we tended to go to Don quite a lot. It must have been hard for Arthur. But I suspect Don was always careful not to in any way end-run his Dean, not to listen to one of us and then take initiatives and then inform the Dean he'd done it. I don't think that was his style.

But during that period there were real significant changes in the mission of the professional schools, most notably Medicine, which involved

tremendous expenditure and building construction. A lot was done in the Thayer School, and I told you before that John Kemeny and I were on a committee along with others to, essentially, make a proposal to re-found the Thayer School, which was done with the establishment of an Engineering Science Department. And Morrison was the guiding hand in all that.

Then suddenly Morrison died. The President was badly shaken, and I remember speaking of that last time we met, including my experience with Robert Oppenheimer and his saying to me, "I've never seen John Dickey so shaken." And that happened in March, and it was many weeks, I think, before he decided what he was going to do. That happened the second week of March, so that--I think it wasn't until April or early May that he decided to appoint John Masland as Provost. And he could have stopped there, but he didn't. I haven't followed clearly enough why he did what he did, but he asked me to be Deputy Provost. I don't know how they picked the title, because I know that the title Associate Provost was discussed. Perhaps, because there was Dickey, John Masland, Arthur Jensen, and Arthur then had to live with the fact that in addition to a Provost with whom he hadn't worked, there was to be a Deputy Provost who was going to be responsible for a third of the faculty (not a third in number, the science faculty was smaller, but for the science faculty). And I reported to you that he said, "This will only work because we are friends."

Having, out of curiosity, listened to a bit of Arthur Jensen's reminiscences, they confirmed what I detected all along, that John Masland, not having appointed Arthur (Arthur was Don Morrison's Dean, he was not John Masland's Dean), and it was the first manifestation for me--not only did John and Arthur not get along very well because John, as a new Provost, wanted to be in on everything, and didn't yet have the established relationship and would not be satisfied with the three professional schools and some of the other, library and so on. Indeed, I think it was then that we defined the faculty as the Faculty of Arts and Sciences, and I believe that was John's initiative, as opposed to the phrase "The College Faculty." And graduate study came into being during the period when John Masland was Provost and I was Deputy Provost, and I think I'll have to go back to that another time.

Arthur Jensen, I guess, was not happy with the relationship with the Provost, although Jensen and I got along very well, no question about that. We would meet together--when we talked about salaries, we made sure that the science departments didn't have a different structure than the others. We would sit down together and go over them all together, and since he knew, having been in that role longer, he knew the background of

various salary situations, I wouldn't have wanted to operate without him. On the other hand, one thing it taught me, which I later acted on, is that one person setting salaries for a long time has its down sides, because one has biases. And I can remember that several times when I said, "But why, Arthur, is this person at this salary?" and he would argue, but he wasn't rigid, we'd talk about it.

In 19...let's see...Arthur was Dean in '59-60...he was Dean before that, but under Masland he was Dean '59-'60, '60-'61, '61-'62, '62-'63, I guess four years. And it was clear, I guess, that he wasn't too pleased with things. He had never, I guess, had a sabbatical, and I wouldn't have known this had I not listened to his tape. And he decided that not only was working with John not something he enjoyed enough, but also (and I know that only from what he said, I did not see that in some ways) he resigned as Dean of the Faculty and went on a year's sabbatical.

John Masland then became Acting Dean of the Faculty in the year '63-'64. And that...I think that wasn't simply...that wasn't alone so that he could figure out how to deal with the Dean of the Faculty, that's not enough. I think that what he realized, and this is what's critical in the Provost's job at Dartmouth, is that the Provost does not have a faculty. I think he missed very much never having been Dean of the Faculty, so he certainly wanted to try that. I continued as Deputy Provost, however, so that he and I would have to meet on many situations which he and Mr. Jensen had met on.

John, meanwhile, was taking new initiatives from the very beginning. One called the Comparative Studies Center which involved the Social Sciences and Humanities faculty, and involved a big grant from the Ford Foundation which then gave it the ability to offer faculty many opportunities which they didn't have before, for research and travel. And therefore, even when Jensen was Dean, the Comparative Studies Center under the co-leadership, I think, of two faculty members, Francis Gramlich and Larry Radway, had major resources to--patronage as a pejorative term, there wasn't patronage so much--they had major resources to effect faculty development and therefore curriculum and so forth. It certainly resulted in many new courses that weren't there before. John Masland was very much an internationalist.

After his year of Dean of the Faculty he realized that, at least from his point of view, you couldn't do both, and I was then asked to be Dean of the Faculty, effective July, 1964. So I had spent five years as Deputy Provost. Together John and I worked very hard at re-founding the Committee Advisory to the President. John developed the current system of providing books to the members of the CAP. He sat, of course, with the CAP. I do

not recall, I'd have to look up in the organization of the faculty to see if he had a vote. The Dean of the Faculty always has had a vote. And that's another example where the Provost found himself in a somewhat marginal position. As I say, I'm not sure whether he voted or not, but the Dean spoke. The Dean presented the case, the Dean explained to the CAP (I did it one way, it's done differently now) how he would view the future of the department, in the abstract as well as the individual. And votes were taken and so on.

I may have mentioned, but I would want to mention again, to be sure there was one case at least where the committee voted (close call, but voted) for a tenure appointment for someone that the President was very uncomfortable with. Now the President (this is a committee advisory to the President, it's important to know that)...and at most once a year could a President decide he wasn't going to accept the advice, or come back to the committee. In this case that, and I mentioned before, the President said he would accept the advice but he couldn't present it to the Trustees, and the Provost was asked to present it, and it was approved. A funny solution, I thought. I believe my memory's right on this, but I know how to find out.

Then in the period 1964 to '67, as I say, Masland was Provost and I was Dean, and I suspect maybe I could have been more generous to John Masland, who I liked, we were good friends of the family. But a Dean of the Faculty...I mean, having had that experience I know why Deans of the Faculty feel the way they do, that they do not want to be interfered with by the Provost. But I did report to the Provost, and to the President too, but there was no ambiguity. The Provost was the person to whom the Deans reported.

CARROLL: May I ask you something here?

RIESER: Yeah.

CARROLL: The person who is Provost therefore has to control the faculties of what were then the associated schools, the professional schools, plus the library, plus the Hopkins Center and museums. That's a very broad stretch. What kind of person has the background to do that?

RIESER: What kind of person has the background to be President?

CARROLL: I guess that's the same kind of thing.

RIESER: And I would also have to say that everything you say about the Provost at Dartmouth, you could say it in spades if it were Stanford, Chicago, or Yale. Harvard doesn't have a...well, Harvard has a Provost, but it's very ambiguous. You have to be willing to delegate some things, too. But John liked that arrangement, and I had no trouble with it. And yet he missed--having had that one year as Acting Dean, he realized what I always knew, that being Dean of the Faculty was more interesting intellectually, and the contacts you had outside the institution with your counterparts to me were more interesting.

There must have been a lot of personal reasons which led John, in 1967, to resign from Dartmouth to take a position at the Ford Foundation in India. And again, despite some redundancy perhaps, John Dickey then approached me in the spring of '67 about being Provost. And I was torn, because I'd only been Dean for three years; I thought it was going well; and for a reason which I understand so well now, and which anyone who has been involved in this would understand, I liked that job. Indeed, as I may have told you, I suggested that John might want to talk to John Kemeny about being Provost, and for reasons which he made clear, he still pressed me. And I said, "Well then, I would be reluctant, given the fact that within the next several years you're going to retire from this place, I'd be reluctant to go out and start, and pick a new Dean of the Faculty, etc. And I think it would be more efficient to combine these jobs for the next couple of years." Which he was very favorably inclined to, because it saved him some worry too.

I had told you earlier, when I agreed to be Dean it was with the understanding that the offices be set up outside of the Administration building, that there be Associate Deans. And I therefore had made the Dean's job easier, and in my judgment more sophisticated and fair, by having Associate Deans for each of the three divisions. And the Dean of the Sciences was Dean for Graduate Study as well, as I had been as Deputy Provost. So through that period, graduate study being confined to the sciences until Psychology was added, was managed as one of the responsibilities of the Associate Dean of the Faculty for the Natural Sciences.

CARROLL: Now this was when the faculty division chairs were made into Associate Deans?

RIESER: Correct.

CARROLL: Did the job change, or was it just a title change?

RIESER: The job changed.

CARROLL: How did it change?

RIESER: As Chairman of Division, you didn't do much. The divisions met a couple times a year, the faculty, and there were agendas to deal with, etc. You had no responsibility at all for appointment of faculty, and you really didn't have much curriculum influence either. So it was a drastic change: it essentially set up three sub-deanships, responsibility--it was like my being Deputy Provost, where I had the sciences, including the personnel. So that in the deanery, as we called it, there was an Associate Dean for each of the three divisions.

Recruitment, department analysis, close work with departments: one of the hardest jobs is, how many faculty there should be in a department, and how do you measure it. It's hard in the following sense: today there is some system, I guess, of a four-course load. I've never called teaching a load, I've always called it opportunity; I got snickered at, but nevertheless people who enjoy teaching, until they are denied that opportunity by taking on something else, don't know that it's an opportunity. I think John Kemeny was the first with whom I worked on this question of, What is a reasonable job in terms of a teaching assignment? And it certainly isn't just measured by courses. That's not a quantum that's reliable, because a section of Spanish 1 is very different from Freshman Chemistry with lecture and lab. And they can't be weighed the same. So that a chemist might have a very heavy job teaching just three courses, which would be comparable, perhaps, to someone in elementary languages teaching five or even more.

So that one real improvement was sitting around the table with a humanist, a social scientist, and a scientist, so that we discussed in the deanery these issues, not just department by department. But they had to make the case for their section. We also (and I don't know when that was, I have to look that up) established a fourth associate dean responsible for interdisciplinary programs. And that was a major change, and I forget...let's see, Gregory Prince ['63] came around 1970 or '71, I'd have to look that up. And he came initially as Director of Summer Programs, because we were offering programs in the summer, including some courses but primarily special programs, conferences and so on. And fortunately we had been working at that, because that then gave us a start on what eventually became a full-fledged summer term.

So that finally the deanery included the three disciplinary Associate Deans, a fourth Dean for whatever we called it, special programs, and an

Assistant Dean (at that time I think it was a person named Brunetta Wolfman, who was black, and who was in every sense a wild card). But during this transition period with respect to race, not gender but race, having in the deanery someone who could pound on the table in one's own establishment was very important. And of course there was an Office of Graduate Study and a director of that office who reported to the Dean of the Sciences. Thus I was Provost and Dean; I had two offices, one in Wentworth and the Provost's office in Parkhurst. It was very demanding job, but no time was lost in communication between the two offices, because it was done biochemically, lying in bed, you'd just think about it.

And I had also asked, and had done two things. I had, earlier as Dean, appointed Bill Durant as Executive Officer (I mentioned that before). And that was critical, because we had a large budget and in order to present ourselves in the budgetary competitions, which is not a bad thing to have, I had to have at my side someone who really knew all the data and to whom I could turn and have it. I also, when I agreed to be Provost, I told John Dickey I needed a budget officer for the Provost's office--this is in response--you asked how you deal with Hopkins Center, the Library, etc. So that while Mr. Durant continued as far as Arts and Sciences went, I had asked a physicist, [William] Bill Davis [Jr.] to be Associate Provost for budget. And he had his office in Parkhurst. So I had Bill Durant in Wentworth and Bill Davis in Parkhurst, and therefore was able to do both jobs. Some things may have fallen between the slats, in the cracks, but one thing that was mute during this period was the inevitable competition between Dean and Provost here, a competition which would not occur in certain other places, where the definitions were less ambiguous. So that was the plus side of that three-year period, '67 to '70, before John Dickey was replaced.

There's one episode that I...I don't know if I've mentioned it. I think John Kemeny was disappointed when I agreed to be Provost, when it became public, and I couldn't tell him what I told John Dickey, that I thought he should consider John Kemeny. We were beginning a fundraising campaign, and I think my having the name Provost, plus my picking someone else to be Associate Provost (Bill Davis was not called Deputy Provost, but Associate Provost, which is different), John took me aside perhaps the very afternoon that the President announced it. Because he announced the Provost and Associate Provost at the same time. And John seemed very upset, and said, "I guess I should consider going to another university. I've been approached about being Dean of the Graduate School at Berkeley." Something...I'm pretty sure I have that right. And we agreed to have a long lunch out of state, in Norwich.



And I've never...I wouldn't say anything here that I wouldn't say to Jean [Kemeny] (I can't say it to John)...but he was so sad, and he said, "You know, I've worked so hard, I've contributed so much that I want some opportunity to play another role." Because he was really distinguished in his role as building a math department, but he couldn't do that all his life, he had done that. One of the unique things about John is the way he did things and then they were over. And he couldn't have a career if it only was chairing that department. He also, in my judgment, then hoped to be President some time. We never discussed that. But I said to him, "I'm going to do everything I can not to see you leave, obviously. We've been at this together too long." Because we had, in our own departments, played joint roles.

And he then indicated that he would like to be part of the fundraising effort. And the reason I think that was a general strategy was that a candidate for President has to know--he was a candidate in his own eyes; he assumed I was, which was not the case, and I'll come to that. So we had one section of the fundraising for which the Provost was responsible, and I think that's been true in subsequent campaigns, which had to do with foundations and corporations as opposed to wealthy individuals. And I'd agreed to play that role. And John said he would like to do that.

So I approached John Dickey and said, "I would like to give John that opportunity, both because I think he'd be very effective, and because he'd like to do that." That wasn't a full-time job. John Dickey was less convinced than I that it was a good thing; I don't know why, but I think he thought John Kemeny would be kind of a loose cannon, I don't know. But he agreed. He said, "If you will manage it, that's fine." You don't really "manage" John, I mean, I really admire John, so everything I say is positive. And I worked hard to have that happen. And he was very good. And he was good with foundations; I think he had an office he used part-time in maybe the third floor of Parkhurst. But he maintained his role in Math. I don't remember, but I doubt that he was still Chairman then.

And the next--there were two other things, one of which was--well, there were two other major concerns at that time, one of which was the...you might call it the equal opportunity, and the significant changes made in 1968-69 with respect to minority students; but black students, there were no Hispanic. And the faculty had created a Committee on Equal Opportunity, and John was named Chairman. I don't know that I named him. It may not have been the faculty; the President may have created the Committee on Equal Opportunity, and John very much wanted to chair that, which he did.

**[End of Tape 5, Side A -- Beginning of Tape 5, Side B]**

RIESER: So I have to repeat something about that, because it so involved John, and then I'll come back to this Provost/Dean. The National Academy of Sciences usually holds its fall meeting outside of Washington, its spring meeting in April in Washington. At Stockmayer's invitation (he was a member), they chose to hold their meeting here. And the agenda and papers were determined not by us at all but by the National Academy. And Shockley had one of his papers on eugenics--he was a famous physicist (whether he had the Nobel Prize by then I don't know).

CARROLL: I think he had.

RIESER: I think he did. And that incident--I'm sure I've told you about how the--that is, he was introduced by Stockmayer. We had a lot of plans, because we knew the SDS [Students for a Democratic Society] was going to do something. It was the October of the term in which a month before we had admitted 100 black freshmen. I think they were dazed, and I think the institution was dazed, because we had previously had the order for 35 black students in all classes. Some were better qualified than others. As the occasion approached, the Union Leader ran headlines, "There Will be More Than Fall Colors in Hanover Next Wednesday," whatever it was, trying to stir up as much as possible.

So we'd made a lot of contingency plans, and Stockmayer introduced Shockley, and the room applauded, Shockley got up to speak and the applause continued, until he sat down, when the applause stopped. I said something, he got up to speak again, the applause started. I know that one thing that happened is, the SDS at that time had said, "If you can't"--to the black students, "If you don't have the macho to prevent him from speaking, we will." Well, that was all that was needed. So then John Kemeny, in a manifestation for me of a lack of understanding of the whole racial situation in this country, got up and begged them to let Shockley speak, speaking as the Chair of the Committee on Equal Opportunity. And said, "When he's finished, I will debate him." Well, that was the last thing students wanted, because the words are painful. So it continued, and there was no speech. And it was a disappointment to John, but it was a very naïve notion.

And the bottom line was that Shockley made it clear to me that he'd be happy to be invited back at a later date. Faculty members were mixed: Erroll Hill, one of my favorites as I told you, came in and said I shouldn't have let him speak. At any rate, there was a question of what sanctions would be applied to the students who had interrupted, because it was

necessary to establish the principle that if someone is invited to speak, you can't prevent that. And the students were finally given some type of warning: it wasn't even probation, it was something. But they were punished more--it was more the fact that they were the guilty ones rather than heroes that upset several faculty members. One very distinguished faculty member, [Howard] Henry Erdman in Government, and if you look at John Dickey's tape (I don't know whether I heard it there or if someone told me) John was a great admirer of Henry's. I think he was terribly disappointed that he took the position he did. And of course Henry had narrowly escaped from the Nazis in Germany, through Spain, and I fully understand his position as well. But for John Dickey it was very difficult.

CARROLL: I do want to clear something up on the tape, and that is: Shockley had not been invited by Dartmouth.

RIESER: The National Academy had been invited...

CARROLL: The National Academy had invited...

RIESER: But when you invite the National Academy...

CARROLL: But as I understand it, he had not been on the slate of scheduled speakers, but came with a prepared speech and asked, as a member, which was within his right, to read a speech. That's how the papers reported it then.

RIESER: I thought he was on the National Academy agenda. We can clear that up unambiguously. That's important, to check out. Because the fact that the Manchester Union Leader knew of it--he of course would have told them--but I think that--I don't know that you--I think you had to have submitted a paper to be included in the thing, but we should clarify that<sup>2</sup>.

CARROLL: I'll check it.

RIESER: OK. Now that's a little far afield on the Provost/Dean evolution. But I mention it in this context to say something about my friend John Kemeny, first with respect to that fundraising matter, and then with respect to this. The other issue at that time, that will come up again when we talk about the coeducation issue, was convincing John Dickey that we had to start talking about it, and his willingness to have me co-chair a committee with Trustee Dudley ["Dud"] Orr ['29]. John very much wanted to be on that

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<sup>2</sup> Shockley was scheduled to present his paper on the final day of the conference. See *The Dartmouth* Oct. 13, 1969 p. 1.

committee; I wanted him on the committee; but I'm just describing to you this series of what I call "presidential readiness."

At any rate, during that period of 1969 and of course the end of 1969, the issue of replacing John Dickey was foremost. John Dickey had said the year before [that] he did not want to continue to chair the Board, because they were going through this crucial problem of finding his successor. I had written to the Chairman of the Board in October, probably (I can't find the letter, I know the briefcase it's in) and I think I planned to send John a copy. I knew how concerned he was about this contest, which he thought was with me, which it was not. And I made it clear to Lloyd Brace ['25] that being President was not something that would appeal to me, even if asked. They talked to me about it once, I remember, though I didn't know why. I was meeting with three or four Trustees at their request for lunch after a Board meeting, and I guess it was related to that. But they didn't tell me.

CARROLL: Why were you not interested in the position of President?

RIESER: Let's see. I had turned down several presidencies before that. It was more than inquiries in the other cases, but--and some afterward. I think it was basically that my nature and that of my wife is not to have the burden of that central public responsibility. And it was certainly very much true, and is very much true, of my wife. And I wasn't about to make a decision which would end up my being President but not married to that wife. I think it was after that that I agreed to talk a little bit about the Amherst presidency; I don't know if [inaudible] if I have discussed that before?

CARROLL: No.

RIESER: This may not be the--well maybe--I don't know whether it is the time to do it.

CARROLL: Well, because we've been talking about Kemeny moving toward the presidency, I think we should talk a bit about you too.

RIESER: You asked me, why didn't I want the job. I think I basically didn't want the responsibility for the whole institution, and the kind of activities that it involved, compared to the activity that you have when you are responsible for the academic side. You have to really want to spend a lot of time on alumni matters, in fundraising, in meeting with administrative colleagues all the time. And in exchange for that, you really have to care about being the front center. I think...I mean, I've been president of other organizations, I've had the title of president, but this is different.

And--let me see, it was in--I think at the time of the Bicentennial there had been a Trustee meeting in December, followed by a Bicentennial dinner, and the end of the fundraising, and very successful. And the Chairman of the Board approached me and (I don't think I've ever discussed it before) and said--well, he made it clear that they were probably going to ask John to be President, but would count on my staying as Provost. I said, "You can't make that deal either with me or with John." So I said, "You have to decide who you want as President, but don't assume that on my part or his part." At any rate, John was appointed in January at a board meeting, and the next day we had lunch and he asked me to be, I think--I don't know if it was Academic Vice President, or what. And I said, "I like the title Provost better," so we kept the title Provost and Dean of the Faculty. So I had that title and that responsibility during the last three years of John Dickey's reign and during the early years of John Kemeny's.

CARROLL: So you really wore two hats: you were both Dean of the Faculty and Provost.

RIESER: Yes.

CARROLL: Did you--this is maybe a silly question, but did you continue to teach while you were holding these positions?

RIESER: Not at that time. As Deputy Provost I did. I might have lectured in some courses, I don't know. And then I don't know exactly how we dealt with that. I don't remember whether it was in the spring of John's first year, or after going through a year, and I have to pin that down; but we decided to divide the jobs.

CARROLL: '72 was when the jobs were divided. So it would be in the fall--it would have been in the spring of '72, I believe. It would have been his second year.

RIESER: But when was [Lawrence] Larry Harvey named Dean of the Faculty, that's the question.

CARROLL: I don't know actually, I can look that up though [1970]. But from '72 to '79 there's a split.

RIESER: Right, but there's some subtleties there. I was Vice President and Dean of the Faculty, and so on. And how that came about is not unimportant, and I'll have to...

CARROLL: Talk about it now and we can fill in the dates then.

RIESER: Let's see. I carried both jobs for a while with John. We agreed that we ought to have a Dean of the Faculty. He wanted a humanist, which was fine by me, and we asked Larry Harvey, who was an outstanding person. But being Dean of the Faculty, he'd already served, I believe, as Associate Dean of the Faculty with me. And there was one year when he was on leave as Associate Dean and I had a pair of people, [James] Jim Cox and John Finch, sort of a "Cox and Box" thing, who served as--responsibility for the Humanities for a year. Having just visited Jim Cox last week in Virginia, as we reminisced over all this, it's clear that that wasn't his favorite year at Dartmouth.

But Harvey then had had some experience as Associate Dean for the Humanities, when we asked him to be Dean of the Faculty. He very much--he reported to me unambiguously, and his choice, I think, would have been very uncomfortable otherwise. And in the CAP--I'm trying, again, to remember what the voting arrangement was, I have to look that up--but we would both be there and we would both sit together, as I recall, in making presentations. There was no rivalry between the Dean and the Provost at that time, partly because we had worked together, and I think Larry felt much more secure if I was there too. Because being Dean really wasn't something he would have thought of, and it turned out after fifteen months, I believe he began perhaps at the beginning of a Spring term, he--it was clear he didn't want to do that.

And I believe then it was 1972 by then, and that's when we talked of dividing--we'd already divided the job, but with Larry now opting out we had to get a new Dean, and I...that's when I took the position I'd rather be Dean than Provost. John then asked [Louis] Lou Morton to be Acting Provost for one year, and to, in that year, make recommendations on what the future of that job should be. But he was not responsible for the Faculty of Arts and Sciences at all, and I continued to be responsible for the computing center, I think, and possibly the Hopkins Center, and perhaps even the two smaller professional schools, I don't recall. Professor Morton as Provost (wonderful man) asked Marilyn Baldwin to be Assistant Provost, which surprised me because I assumed we were--I mean, he was doing about one-fourth the job that I'd been doing, and I guess John and I had some discussion then, and I said, "It's really..." There was no question but that I reported to the President as Dean of the Faculty, and we added the title Vice President, so I was Vice President and Dean of the Faculty. He was Provost (Acting Provost, but that wasn't used very much) and he served for one year.

He didn't like it, for the obvious reason that if you're not going to be president of the institution, in my judgment, you want to be responsible for the academic side of it. And he was responsible for, as I recall, the libraries, Dean of the College, Dean of the Tucker Foundation, all of these things, Admissions, maybe Athletics, I don't know. And at the end of his year, I believe, it was decided that there should be, in addition to Vice President and Dean of the Faculty, a Vice President for Student Affairs. And I don't remember whether Frank Smallwood held that title first and [Donald] Don Kreider second, I think that's the way it went. And we made some changes. Don Kreider was responsible, I'm sure, for the computing center at that point, because he was trained substantively for that job.

Now, when John announced that I was going to return to being Dean of the Faculty, and it was clear that in that role I reported to the President. I think the faculty, understandably, at least based on the people who were most concerned. Marilyn Baldwin's then-husband David (they're not married now) I think took the lead, along with others, saying "We've got to define the relation of Dean of the Faculty to the President, the process by which the Dean is picked, not just by the President, etc. And that there should be no ambiguity in the future but the Dean report to the President."

That led to a whole new statement of the Organization of the Faculty, which will now be brought into play with the search for a successor to Jim Wright, whereby a committee consisting of three members of the CAP chosen by the COP, three members of the COP chosen by the CAP, and those six members will choose a seventh who is an Assistant Professor. I forget who chairs it, maybe they pick their own chair. And they will recommend candidates to the President.

I haven't looked at the recent statement of it, I don't know whether it's the same or different. They had been--they also are the body that reviews the performance of the Dean. And that was done. I had two experiences at that. One must have been in '76, I don't know. I'll look all that up.

And then when I...let's see...I'm trying to remember just what happened. I think in the last year of John's presidency he decided--I agreed to be Provost again.

CARROLL: '79.

RIESER: That sounds right. And I believe he appointed Hans Penner as Dean of the Faculty. And Hans clearly wanted to report to the President, and we worked out a modus vivendi whereby the three of us would meet (Hans,

myself, and John Kemeny), and I would sit with the CAP; I would not vote but I could comment. And that provided continuity.

CARROLL: Why did you decide to go back to being Provost?

RIESER: Rather than Dean? I think it was time, and the faculty committee may have felt that. I think it was time, after so many years as Dean. I forget the total number.

CARROLL: Seven, all together.

RIESER: It was more than that, somehow.

CARROLL: When you can do the combined, it's true: that goes back longer.

RIESER: Yes. It was important to have someone else be Dean of the Faculty. And for purposes of--at that point I'd been Dean of the Faculty a long time, I was quite willing for a year or two to be Provost. And we were in the middle of a fundraising effort at that time too. And it was all right. It wasn't--I mean, in that period the question of being president at several places came up, including Amherst and Swarthmore and other places. But except for the Amherst thing, where we carried it one step, of meeting with the search committee--I mean, that's another whole story, because it was written up in the New Yorker, but not with--it wasn't clear who the person was. We realized it wasn't, still was not what we wanted to do. We did it, inquired about it once more, but it just reconfirmed our feeling.

I should point out that in 1972, shortly into John's presidency, I was elected President of AAAS, which is the American Association of the Advancement of Science. I think that pleased John, I think it surprised him but it pleased him. So that--I had previously been a Director, a member of the Board of Directors, and before that I had chaired a committee on science education. All this is a separate career independent of Dartmouth. But that made it possible to be here this long in those jobs and to be very involved in another enterprise.

It involved a major science meeting, going to the AAAS and the--not the counterpart, but major--[indistinguishable], it was called: The Council on Science and Technology in Mexico. And we had an extraordinary meeting, called *Siencia y il Hombre en las Americas* [sp?]. It was the first time that the major scientific community of Mexico and the United States met for two weeks, with a couple of days off in between, in Mexico City. And that has led to some of the other things, including a trip two weeks ago to Argentina; it's gone on for over 20 years.



But to answer your question, what made you willing to go back to being Provost: I'd been--I was involved in an increasing number of things outside, and the combination was manageable. The combination of being Dean and Provost would not have been possible along with the AAAS.

CARROLL: In the jobs of Dean and Provost, did you have any fundraising responsibilities in the different campaigns?

RIESER: Yes, it was primarily in this whole area of foundations and so on. And with the government. That is, I told you that when we were going to build the Gilman building someone had to develop a proposal to the National Science Foundation, and that provided three-quarters of a million dollars for the building. In the case of the Fairchild Center, I began the negotiations with Walter Burke ['44] to go from his initial idea of a building, to a center. I told you of that before. And Jim Hornig, who was Dean for the Sciences, was very much involved. And then with respect to Rockefeller Center, the Associate Dean for the Social Sciences, [Donald] Don McNemar--and this was in 1978, I believe, I was then Vice President and Dean of the Faculty, and we had to find a way of bringing the social scientists together.

My overall strategy had always been to get physical proximity for the natural sciences through the Fairchild Center, for the social sciences in what is now Rockefeller Center, and for the Humanities using more and more of Dartmouth Row for them. And Carpenter. And I think of that because last week, ten days ago, I was at Guilford College in North Carolina to speak at the inauguration of Don McNemar, who had been my Associate Dean, as President of Guilford. Several of my Associate Deans: Agnar Pytte at Case Western, Don now at Guilford, Greg Prince at Hampshire--I have had vicarious pleasure in my associates being president. And I was also a trustee of Hampshire for a long time. That's a whole nother story.

At any rate, the fundraising for Rockefeller Center involved a lot of us: Paul Paganucci, who was then Treasurer, and I met sometimes with [Rodman] Rod Rockefeller ['54] and once with Laurance Rockefeller and others. But that fundraising effort was very much joint. John was critical to it. It all was the result of the death of Nelson Rockefeller ['31], and the brothers and the son, Rod, wanted to do something in his honor. Or we made them feel that they wanted to do something at Dartmouth in his honor. And John initially had hoped that they would buy the Medical School. He even offered them a lower price! It was ridiculously low. But Laurance had just put so much effort into a hospital in New York, a homeopathic hospital in

Nelson's honor, that was out of the question. The whole development of Rockefeller Center is a separate topic, and not unimportant, because of the people who were drawn into it. Frank Smallwood was also critical to that.

At any rate, just to finish the evolution of Provost and Dean and bring it up to the current date--and then we have to go back next time: What happened was that I agreed to be Provost, effective '79, and for the first year of McLaughlin's presidency. I didn't really ask him or he ask me. I had announced to the faculty initially that I would stay as Provost until that fundraising campaign was over, but then decided--John and I decided--I think John felt it was very important to be sure that his successor had freedom to choose. But that would be true in any case. And when Dave McLaughlin was campaigning for the Presidency he was--he several times was very much involved in conversations with me about what it was all about. At any rate, I agreed to be Provost for the year '81-'82.

CARROLL: And then take the transition into the new presidency? You would span the two presidencies?

RIESER: I would span them, but '81-'82 was McLaughlin's first year. John served eleven years as President, from 1970 to 1981. And I announced that I would be Provost for one year. I'd already had quite a retirement party as Dean of the Faculty [inaudible]. And after one year with Dave, another gala retirement party, with the understanding that I would have a sabbatical for a year and a half, in 1982.

CARROLL: And who succeeded you as Provost?

RIESER: My successor was Agnar Pytte, who had been Associate Dean of the Faculty, whom I had picked. And he served throughout the remaining five years of McLaughlin's term. And when McLaughlin announced his resignation (that's another story too, but I'm not sure that will be on this tape) Agnar also said he was going to resign as Provost. Subsequent to his decision, he was recruited as President of Case Western, and he's done wonderfully. But I don't think he would have been noticeable at all had he not agreed to be Associate Dean for the Sciences, a position in which he served for three years.

CARROLL: I think, if that's all right with you, we are almost at the end of this tape, and I think we'll call that quits.

RIESER: All right.

**End of Interview**