Oral History of Martin Graham

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Cameraman: Paul McJones

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<START TAPE 1>

Peuto: This is tape one of an interview of Martin Graham made in Berkeley, California, on the 15th of November, 2011. It is 10:25. Martin Graham was the principal investigator of the Rice Computer R1, and this interview will be primarily focused on that design. Hello, Marty.

Graham: Good morning. Nice to see you again.

Peuto: Yes, we have known each other since 1968. You were my Ph.D. advisor, and I realize there are lots of nice questions that I never asked you before, so we're going to go deal with some of those questions.

Graham: Well, since you've gotten your degree officially, you can ask me anything now.

Peuto: That's good. There are three sections pretty much to this discussion. One is we will talk about early years, family background, up to university, typically. And then we're going to talk about your period of time for you from the navy in '44/'46 to Rice University, where you joined to work, I guess, on the R1 project. So and then the third section is going to be the R1 project itself.

Graham: Okay.

Peuto: We'll kind of stopover at the end of the R1 project, which, depending on the counting, is either '66 when you leave for Berkeley or '71 when the R1 is decommissioned because there's plenty of other material for another interview.

Graham: The thing that's remarkable is that it was used by users …

Peuto: Yes.

Graham: … after it was finished.

Peuto: Yes, and I hope we both talk to that because that's a pretty noticeable fact about the R1. Let's talk about your family background. Where were you born, and where did you grow up?

Graham: I was born in Jamaica. That's Jamaica, Long Island, in 1926. So it was just the tail end of the prosperous years.

Peuto: When you were a kid, what did you do for fun?

Graham: We played stickball, stoop ball. I built model airplanes. And when I was very young, I used to play around with the electrical outlets in the house and things like that.

Peuto: And what were the best and worst subjects from your viewpoint when you were in school?

Graham: The schools were quite different at that time. It was an upper middle class neighborhood. It was about a 15-minute walk to school generally. I would not know any poor people, but there was a orphanage next to the school, and their people went to the same school, so at least I had some contact with people, not in that upper/middle-class. And thinking back, there were courses that seemed in some way strange, like how to set a table, where the forks should go and the knife and the spoon, and I remember telling us using a knuckle and a fingertip to measure a distance to get the distance from the edge of the table to the eating implements, and that seemed so out of the current curricula and so unimportant social thing. And I was particularly good at math and so on, but I did find at all the courses that they were giving, the one that gave me trouble was music appreciation. I'm tone deaf, and I remember we were supposed to recognize pieces that were played, and then the teacher would call on the person in the class to identify the piece. And I still remember to this day they played *Humoresque* and she called on me, and I couldn't identify it, so she got me up in front of the class, took me from behind, and played it again with me rocking to the music. That one I recognize to this day.

Peuto: What did your parents want you to do when you grow up?

Graham: Well, they knew I was interested in electricity. And when I was quite young, I said I wanted to be an electrical engineer, and that was fine with them. My father did not want me to be in the business he was in, which was dressmaking, manufacturing dresses.

Peuto: And when you look back on those high school and other years, maybe even further to university, do you remember teachers that had a big impact on you?

Graham: I remember the lady who helped me recognize *Humoresque* to this day. It was Miss Jones. She was one of the younger teachers in that school, and it was that kind of an incident that had an effect, but it wasn't that they told me I should go into this field or that field.

Peuto: When you went to college, did you choose a specific college?

Graham: Yes.

Peuto: Which one?

Graham: It was Brooklyn Polytech or Polytechnic Institute of Brooklyn, which is predominantly engineering. It was founded in the era of the civil war in the United States. The school colors were blue and gray, and the tuition was $600 a year, which we couldn't afford at that time. We got some help from one of my aunts, who was well to do, and I think to this day, it's still not one of the very top schools, but it was good. And on occasion, they would hire a person who was retiring from Bell Labs and came, joined the staff. And quite a few of the students who worked for the telephone company when they graduated, and quite a few, I think, got their degrees going to school at night.

Peuto: Now, you alluded to it, but when was your first exposure to electrical engineering as something that you wanted to have as a career?

Graham: I don't remember it as being real engineering, just being interested in things that worked. I guess it was when I was around 10 or 12 that I got around to building a radio, an AM radio, and not from a kit but from parts from a radio supply retail store. I made the mistake of taking every part and cutting the leads short so that I had the problem of getting them longer again so I could get them wired into the chassis, and it didn't work at first, and the fellow that owned the store helped me get it hooked up and working. And then I worked for that store.

Peuto: But your exposure to computers was much later.

Graham: Much later.

Peuto: We'll come back on that at that point. One of my other questions about your family is did you have brothers and sisters?

Graham: I have a sister eight years older than me who also went to the same grade school. So when I came, people recognized me as being her brother. And she was very bright, so they expected a great deal from me at the time. And she died just a few months ago.

Peuto: Sorry about that. You mentioned this radio. Was that the first electronic device you ever built?

Graham: It depends what you call electronic. For example, I remember that I didn't like to fall asleep in a dark room, and I wanted to have my lights stay on till after I fell asleep. And you could buy a timer if you had a gas engine-powered model airplane, and I remember working on using that timer, which is usually made for a few-second flight to run for like 20 minutes so I could use that to turn off my light. That's not current-day electronics, but it was in that general area.

Peuto: Now, you went to work after high school? Or how did it go?

Graham: Well, I worked while I was going to school. I was interested in building airplanes, models, and ship models, and there was a local store that sold those things to mostly kids. They got their supplies from the largest store in Manhattan called Polks, P-O-L-K-S. And I worked for that store part-time after school a few days a week, mostly going into New York to pick up their orders and bring them back to the store. And Polks did not only things that youngsters would work on, but they had a big department. The model railroad supplies that was a very popular hobby for wealthy older men. So I got immersed into a much broader group of customers and what they were doing, and I remember I had to get a work permit in order to handle that job.

Peuto: My next question is I understand that you were in the navy from '44 to '46.

Graham: Yes.

Peuto: But I don't know if at that time you had finished your schooling or what was the case about this, being drafted.

Graham: Well, it's an interesting story. World War II was going on and there was a draft, and I had skipped two grades in grade school, so I started college when I was 16. I didn't quite fit in socially as a mature student, with all the other people being 18, but at any rate, so I wasn't eligible to be drafted. But a notice had come out that you could enlist in a program and become a naval officer, so I went down and applied for that before I was 18. And then I was rejected from that program because my eyesight wasn't good enough. So then time went by and I became 18, and I received a draft notice, and when I went down to be drafted, there was one place that drafted you whether you went into the army, the navy, the marines, or so, and you came in not knowing which one you were going to be in. But when I got there after I did the initial entry, they hung a tag around my neck saying I was going to be in the navy, and that was because of my having applied for the officer training. So by the time I was drafted into the navy, I had done two years of college.

Peuto: Out of four, I guess?

Graham: Out of four. And the navy had a program. They needed radio technicians desperately, and they had a program called the Eddy program, and an Eddy Test, so you could take that test when you got drafted, and if you passed it, you immediately got promoted from seaman third class to seaman first class, which really made a difference because when you went to boot camp, everybody was seaman third class, even the navy person that was actually doing the running of the troop. The people who ran the classes and were nominally in charge were all high school teachers of physical education that had gotten into the navy. So anyhow, it was sort of an elevated status right from the beginning, which was kind of nice.

Peuto: So basically you were in the navy from '44 to '46.

Graham: Yes.

Peuto: What you were doing was a radio operator and then …

Graham: No, radio technician.

Peuto: Radio technician.

Graham: Because if you passed the test, you went to a one-month school to bring it sort of up to a certain category. But first you went to boot camp for a month. Then you went to radio tech school for three months. That was in Chicago, and it was always in a high school gym that you lived. And then you would get sent to a secondary … well, let me get it straight. I think it's a month in Chicago, then it was three months in a first training school, and then something like seven months in learning the technical end, and there were different schools for being aircraft, electronic technicians, so shipboard or so on. And then when you went to that school, you would learn radios, transmitters, radar, sonar, and at the end, it included labs of not so much building equipment but repairing it, and then you would be shipped off to a navy base or ship.

Peuto: I guess you were discharged in '46? I don't know if that is the right word.

Graham: Discharged, yes. Of course, I went through all of that, and then I was assigned to a naval air station at French Morocco, and I served there about six months and then was discharged.

Peuto: And that was pretty much past the end of the war because …

Graham: Yes, the war ended while I was in school, so they shut down the school for a week or two, and then they decided to train people to the end. So I went to the end of the training and served, as I say, in French Morocco.

Peuto: I suspect after '46 you went back to school?

Graham: I went back to school, and there was the GI Bill of Rights. So I finished my bachelor's degree at Brooklyn Polytech.

Peuto: If you don't mind, I'll hold you there. We're going to ask a question and come back. So if you look back on your life through college, including the last two years that you did after '46, what was the greatest influence to you? What is it that made you take the path you took?

Graham: Well, it may seem strange, but the education in the navy was very, very useful. And it wasn't the education in the electronics. It was the education of how you survive in a society that's very class-oriented, that a naval officer has a much greater power than an enlisted man in the navy, in general. So you could just be in training and see how everybody was being treated and know who you should not pick a fight with because you were sure to lose it, and that's particularly useful regardless of what profession you're in. So there was that. Also, when you live in the same Quonset hut with, say, six other people, you learn a lot about their religious feelings, what foods they like, what they like to do, and just arranging to get along individually, person to person, with people that you're not always choosing yourself the way you want.

Peuto: So are you …

Graham: It's important.

Peuto: Excuse me. Are you telling me that it helps how to negotiate with your colleagues when you were at Rice University?

Graham: It happened even before that, yes. And it still does.

Peuto: Let's go back, and I had interrupted you, but I just wanted to find our common thing. So you got your bachelor in '47 at the Polytechnic Institute?

Graham: Yes.

Peuto: What was the subject or what was the essence of your studies?

Graham: Let me add a piece that didn't come up yet. When I was an undergraduate, I was working for one professor at Brooklyn Polytech, and he was a professor that graduated from MIT in 1929 during the Depression and had a job at Bell Labs, and he was very bright and very creative and very off the middle path. For example, he and his wife met at a Trotskyite cell. And as I say, he was off the beaten path, and he treated me very nicely. We had coffee together even when I was an undergraduate and before I went into the navy, and I got a lot of my advice that I found useful for the rest of my career from him. For example, he told me once that his daughter was complaining to him that he didn't belong like to the National Academy, and he said, "Oh, that's a mutual admiration society." So that was very helpful to me when I wasn't admitted to some places. And there were things that he said like, "Not every problem has a good solution." I never heard that from another faculty member, and it's something really worthwhile knowing. And, also, when you're given a problem and they're in a textbook and the solution is in the back of the book, that's a whole different world than being given a problem and not knowing whether the problem is correct or they didn't consider something.

Peuto: So he was a big influence?

Graham: He was a big influence, and also, when I applied, I wanted to get a master's degree in mathematics because I felt I wasn't particularly strong in theory, and I applied to four schools for that. Applied to Princeton, MIT, Harvard, and Caltech, and I was still quite young, and I guess I always felt a little inadequate because I was short and not very powerful physically. I was good enough for stickball and stoopball but not for football. So at any rate, I wouldn't have gotten admitted to some, but he wrote a very strong letter of recommendation. He taught a course in acoustics. That was his specialty. His name was MacLean.[1]

Peuto: MacLean?

Graham: Yes. It was an elective course, and it was known by the students as "Sudden Death." But he was well known, particularly for some advances in acoustics, for example, how you can do an absolute calibration in microphones when you have a loudspeaker and a microphone, and a speaker can be either a speaker or a microphone. So there's a reciprocity relationship that can allow you to make an absolute calibration. So he was very bright, and I learned, as I say, a great deal from him, and he was very helpful in writing recommendations. Anyhow, Princeton just rejected me. MIT accepted me for a master's degree, but it would take two-and-a-half years to get the degree. That was because under the GI bill, the government will pay you up to a certain amount, and beyond that amount, they wouldn't pay; the school had to pay. So MIT was using that overall system to essentially get, I considered, slave labor, and I didn't

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[1] William R. MacLean (1908–?).

think I wanted to spend two-and-a-half years for a master's degree. So as I said, I was accepted there but chose not to go to MIT. See, some of what I learned in the navy affected that decision.

Peuto: But you had an offer from the Polytechnic Institute?

Graham: I didn't apply.

Peuto: Oh, okay.

Graham: And I was accepted at Caltech for a doctorate, so I'm sure they wrote very good recommendations. But I felt, as I say, young socially, and that was 3,000 miles away. It's one thing to be far away with the navy looking out for you, but it's a different thing to be 3,000 miles away by yourself. So that was the reason I didn't go to Caltech. In retrospect, I think it was probably one of my poorest decisions. I think I really would have done well there, judging from students and people I know who went there. They really looked out for young, let's say, unusual students that they had. And I was accepted at Harvard, but Harvard had no department of electrical engineering, but they accepted me for engineering science and applied physics, and I went there. That's one of the best decisions I ever made. Of course, it was a different social and ethnic background for many of the students, and yet there were a very large number of students that came from, say, lower-class families that were very, very bright, and you went to mixed. So eating with them and spending time with people, and the way Harvard treated their students, when you entered, you had to have somebody sign, I guess, a bond that if you had any debts that you didn't pay, they would pay. So MacLean signed mine, and then Harvard never bothered you about having to do something and getting an approval. It was pretty much you did what you wanted to do, and they had protected themselves against at least financial losses on your account. There was quite a different overall atmosphere from Brooklyn Poly.

Peuto: So you got your master’s?

Graham: I got a master's degree in engineering science and applied physics.

Peuto: And what was more specifically the subject?

Graham: You didn't need to do a thesis for a master's.

Peuto: Oh, so it was taking classes?

Graham: Taking classes. One or two of the classes were in subjects that I had essentially taught at Brooklyn Polytech, and some of the courses were taught by Nobel Laureates, and in retrospect, they were extraordinarily good courses.

Peuto: And then what happened after the master?

Graham: You should talk to my wife about that.

Peuto: Why? In '49, you got married?

Graham: No, we were keeping company, and I commuted from Boston to New York. We met shortly after I got out of the navy. I had a birthday party for myself, a surprise party. I wasn't surprised, but the guests were surprised. And one of my best friends from the fraternity at Brooklyn Poly when I was an undergraduate was in the navy. His fiancée was studying to be a nurse and had to work that day, so he brought his sister, who is my wife now, and they thought that she and I should meet, and we met that way, and that was before I went off to Harvard. But then if I had gone on for a doctorate at Harvard, I would've had to pass two language exams. You had asked earlier about what subjects I was not good in, and the subjects I'd say I dislike most were languages, and I had taken Latin and German in high school. Normally, you would take German as a language when you're doing a doctorate, but I didn't want to do two languages, and Brooklyn Poly, you're required only one. And at that time, I felt strongly enough that I would go back to Brooklyn Poly and I would have MacLean as my advisor, who I'd respected greatly. It worked out very well for me professionally, but it's a peculiar reason to not continue with Harvard. But anyway, it was my reason. My wife disagreed. She's very good at languages. So then I went back to Brooklyn Poly, and I taught, took some courses, and then started working at Brookhaven National Laboratory, and I did my thesis at Brookhaven National Laboratory with Brooklyn Poly.

Peuto: And the subject was?

Graham: The subject was a vacuum tube. Tells you how far back all of this was. And the thing that was important in a lot of the nuclear instrumentation was to build counters because if you had an experiment and the reactions were rapid, and a lot of them happened in a second, in the early days, you had a mechanical counter and you'd click. You'd click up a number for each of them. But as things got faster, the mechanical counters couldn't keep up and you needed an electronic circuit to divide such that you got a pulse out of that circuit to do the mechanical thing, let's say, for every 100 pulses that came in. And what mattered in doing it with vacuum tubes, how fast you could do it depended on the transconductance and the capacitance. Because you were charging the capacitance, you needed a certain voltage to control the vacuum tube current, and so there was a problem in making the capacitance low and the current change high, and that's true if the tube is a grid and a plate: a triode or a pentode or so on. But if you have a tube where you have a large current in the beam and you deflect the beam, that ratio doesn't hold the same way. So I worked on a vacuum tube that formed a beam, a flat beam of current with two deflection plates. It was like a cathode ray tube but not a dot but a line, and that going from one plate to another of a secondary emission material, and then it got collected and the idea was to be able to build a very fast counter.

Now, at Brookhaven, there were people that could do vacuum systems, all kinds of the physical end, and there were machinists there that had worked at Sperry Gyroscope during the war that were really very, very good machinists and would do things that normal machinists were not used to doing. For example, if there was a metal part that had to be done very, very accurately, they would cool it with liquid hydrogen and then they would machine it and it stayed cold and dimensionally stable. And that's an unusual procedure and everything, but they had experience in all of that. So there was that background. There was a problem in building the right shape electrodes, and I built the punch and die system essentially, or die-forming master so that I could form the plates. So I did some of that myself. And then we'd evacuate the tubes. You'd have a getter in it to get the vacuum down to a really good vacuum, and I guess I spent a year or two on that. I was doing other instruments at the same time, but that was the thesis. And when I had my thesis exam, there was one man at Brooklyn Poly who was consulting for RCA, and he said that they could do that job in a week, which I did for my thesis. And then there was also a question about could I get the thesis typed up in time because I procrastinated, and it had to be handed in shortly after my exam. So there was an objection about how could I possibly get it typed up in time. I said I had it already typed up. And the overall result was they accepted it, and you're supposed to have it published, and there was a deposit you put up, which was refunded when you got it published, and I think it was $100 or $300. It wasn't worth it to me. That thesis was never published, but I did my degree.

Peuto: I understand you were working at Brookhaven from 1950s to '57, maybe?

Graham: Yes, 1957 is when I went to Rice.

Peuto: Yes. And then you went to Brookhaven in 1950, something like that?

Graham: Something like that, yes.

Peuto: You already alluded to that because some of your work there was your Ph.D. dissertation.

Graham: Yes.

Peuto: But beyond that, what then did you do in the next six years or so?

Graham: Well, it sort of overlapped with the dissertation. The instrumentation division built instruments for the theoretical physicists and chemists doing experiments at Brookhaven. A lot of them had worked on the Manhattan project and so on. It was a very bright group of scientists, and it was nice to work directly with them on what projects they were doing. There was one technique for telling, I think, an atomic weight of a gas by having it diffuse through an orifice, and you could tell by the rate at which the pressure decayed what the atomic weight was. So there was an instrument, you essentially had an orifice in it, and you needed a pressure gauge that you could track and measure the time interval. So you could build the instrument but you'd wanted to have a plate that would deflect, that you put a voltage on it, and you'd see its deflection change. And I remember building one instrument that would take the parts that were already available and combining them so you could do that measurement, and that got used continually by people working in that area.

Another instrument that I worked on was they were going to build an accelerator. If you do it with a magnetic field that confines the path to be a circle, then it's a cyclotron. And as the energy goes up, you make the magnetic field stronger to keep it going in the same size circle. And you sort of run out of iron and power because the width of the circle is not zero, but you have to control things accurately to keep the beam focused. So the idea came up that if you build the magnetic fields so that instead of just decreasing with the radius there's a section that decreases with the radius getting bigger and then there's a section that increases as a radius gets bigger, the field gradient is alternating, and you can confine the field better and you can build a higher-energy machine for a reasonable amount of money. So they were building a AGS, an alternating gradient synchrotron, and that could be 100 feet in diameter and a lot of money involved, and they wanted to build an electrostatically focused machine; as the energy went up and the speed went up, the frequency that you needed to apply to your accelerating plates changed. So it was a question of could you go through that change without the thing not working from beginning to the end. So they built the thing, I guess, about 50 feet in diameter with electrostatic focusing, and in order to do that experiment, they had to have no magnetic field, really essentially no. So I worked on a probe that would measure magnetic field down into the low milligauss. It was the size of a cigarette, and you used a strip of magnetic material that saturated very, very hard with very little magnetic field, and you put on an AC field and you saw if the phase changed at the crossing point. So I worked at building that kind of an instrument. They were drastically different. I learned a great deal from the people that would ask you to build something. There was a guy in charge of that division whose father worked for an electrical utility. He was an electrical engineer. And he kept saying, "It's easy to measure something to 1%. It's very hard to measure it to a tenth of a percent." To this day, I find it's pretty true.

Peuto: As a side question, you were at one point a teacher at Polytechnic Institute in the period '48 to '50?

Graham: Yes.

Peuto: What did you teach?

Graham: Usually it was electronic circuits. There was one required course in field theory, Maxwell's equations and so on. People hated to take it. It's basically hard because it's a different math. It's vector analysis. You're working in three dimensions, and with things that change with time and the equations interact. One thing will show up in two equations. And there was one semester I was assigned that course, and I taught it doing a lot of calculations of real cases, something so and so big, the kinds of things that come up in the real world in that area when you're doing problems that aren't really problems in that area. That's particularly true if you're dealing with electrical interference, a radio interfering with another radio, electromagnetic interference, which is a very important area today. Teaching that course was one of the best things that happened to me because I had to learn [the material]. It was arranged that you could take half a semester of vector equations and then the other half in that course because you didn't know enough math to do those problems. But teaching it, I learned it well enough so that I could use it myself, and I've used it over and over in the years since.

Peuto: Was it the beginning of your interest in an academic career, or when did that start? Or it was the consequences of going to Rice University?

Graham: No, I taught part-time. Well, I would help other students, particularly in the fraternity. I would tutor them in calculus so they could get past that point in their [studies]. I apparently did well in teaching because students that were having trouble in those areas that I had no trouble with at all learned it well enough to pass the course, as well. So the teaching part, I don't think I ever said to myself, "I want to be a university professor." That just came because the people I ran into all along the way doing things I was interested in involved a teacher.

Peuto: We're getting close to the section that starts with Rice University.

Graham: Yes.

Peuto: There are a few questions I still would like to ask you.

Graham: Sure.

Peuto: One of them is, are there any comments you want to make about all this period that we've covered, from your birth to 1957? Anything that you think was important to you?

Graham: Well, there have been one or two professors. There's one prof, I think, from Carnegie Mellon who had cancer and knew he was going to die within a few months and talked to his class about his bringing up and how his parents treated him, and it was different than in many of their cases, but it helped them understand what motivated him and how much effect his parents had had.[2] And that started me thinking more about what I remembered. I remembered I was a finicky eater that I went to bed very late, and I liked to build the model airplanes. So there was a card table in the dining room that I could work on my airplanes at, which didn't fit in with a well cared for dining room and so on. And we had a table, dining room table, and I worked on it sometime, and there was one time I put in a gouge like a 16th of an inch deep, and my mother was very upset, and my father said it didn't matter, and I realized that I was being treated unusually. And my mother's family had three daughters and three sons, and in those days, you lived very close to your relatives. You could walk there or it'd be a short car ride as you got further into the 1930s. But at any rate, you'd run into the relatives quite often. And there were uncles that kept telling my mother that they were not bringing me up correctly and I should have to go to bed by nine because an hour of sleep before midnight is worth two after. There were all those kinds of things. Generally considering that they all came from Europe when they were young, they really had an appreciation of what you could aspire to do in the US, and they all ran successful businesses. But at any rate, I tell people these days that my father would tell my uncles to fuck off and leave my mother alone. That's putting it in today's succinct language.

Peuto: Yes.

Graham: But in the long run, it made a very big difference.

Peuto: Given this punch line in a way of making a transition now, a nice segue, somehow you got attracted to go to Rice University …

Graham: Oh, yes.

Peuto: … to do a project. I believe the project was not yet done. So tell me about what made you leave Brookhaven; I'm assuming you'll tell me.

Graham: No, the two go together. Generally, I told you in the navy I learned who not to fight with. Now, when I was at Brookhaven, I got my doctor's degree. There was another guy there named Bob Chase who didn't have a doctorate, who was in the same instrumentation group, and he was very bright. I don't think I even got a raise after I got my doctorate, and I felt I was being treated unfairly. The guy in charge of the electronic instrumentation division had worked on the Manhattan project. His name was Higinbotham,[3] and sometimes I would have technical arguments with him. I remember one time he said, "I finally won an argument with you." So the relationship was a little awkward. I wanted to change jobs, and I was up for jury duty. You know, you'd go down and you wouldn't be picked, so you'd have the day

[2] Randolph Frederick "Randy" Pausch (1960–2008).
[3] William A. Higinbotham (1910–1994).

all off. So I went and interviewed at Sperry Gyroscope because they built that general kind of instrument. When I went and applied for a job, they didn't have anybody that could interview me, but I didn't want to leave. Generally, when I reach a point where I couldn't get things sort of straightened out and I had a problem, that it was between me and the person that was my immediate supervisor, and I couldn't see it getting better over time, I always figured that I should change jobs.

Well, around that time, but let's say before it, there was a thing at Brookhaven. They didn't have a big computer. This was a long time ago, in the early 1950s. And one physicist needed a computer, so he bought one for his project. It was a Remington Rand vacuum tube thing, very primitive. And then they started talking at the higher levels that he shouldn't have his own computer. It got to the point where they wanted to get a big computer, and it was going to cost a lot of money, and maybe they should build one. And at that time, they were building the ILLIAC I, which is one of the few that was built at a university and worked and was used for years. So they were looking into building their own, and they decided to build it. And I got involved in building the large computer they were going to build at Brookhaven[4], and they were going to copy one that was built at Los Alamos.

Peuto: That was MANIAC?

Graham: That was MANIAC …

Peuto: II?

Graham: … I.

Peuto: Oh, MANIAC I.

Graham: They were in process of building MANIAC II, and the guy in charge, I don't believe, was a trained electrical engineer with a graduate degree, but he had worked on MANIAC I. So I met people at Los Alamos, and I met this fellow I mentioned, Zevi Salsburg, who was at Rice, and I met him at Los Alamos. And he had grown up in New York, again from a middle class family, so naturally personal friends and acquaintances, and he said that they had put in an application to the federal government to build a computer, and one of the main activities for a large computer was for designing reactors. So that was a kind of problem that took a big memory, you wanted a fast machine, and the places that were using them were General Electric and people who were building and designing reactors, and they had annual meetings.So I got to know a number of those people. And that was an AEC operation, Atomic Energy

[4] Merlin was the name of the computer built at Brookhaven that Graham was involved with. See: Y. Shimamoto. Merlin. Internal Report, Applied Mathematics Division, Brookhaven National Laboratory, September 1959. http://www.bitsavers.org/pdf/brookhaven/merlin/Merlin_Provisional_Report_Sep59.pdf

Commission. There was one being built by one of the labs at Oak Ridge and so on. And so Rice had applied for the money to build a computer, and the group that put in the application were two theoretical chemists and one physicist.[5] The physicist was from a fairly wealthy family. I think they owned the Coca-Cola distribution system in the state of Georgia. And I don't think either of the other two were relatively wealthy, but in any event they wrote up a proposal and nobody did anything with it at the AEC. And then John von Neumann died, and he was deep into computers, and the question came up, what research was the AEC supporting that he would be interested in, and the answer was nothing. So I looked at the proposal from Rice, and the three faculty members were very well respected, [but the AEC] said, "We won't fund it because you don't have an engineer." The guy running this sort of stuff in the AEC was John Pasta[6], and I got to know him through the meetings. So one reason they desperately wanted me was I was an engineer, and they looked at it, and they told them that if I came to Rice, they would fund it. So I think it got funded eventually for $450,000 from the AEC and $150,000 from Shell Oil because a lot of these guys consulted for Shell Oil.

Peuto: Hold it because there's so many questions to ask at this point.

Graham: It's a very interesting point.

Peuto: You kind of alluded to why Rice got that contract.

Graham: Yes.

Peuto: One of my question is: was it common at that time? I mean everybody seems at that time to be not affording to buy the computer but willing to build the computer. So I understand there were several locations that, quote, unquote, might be in competition with Rice. Can you elaborate?

Graham: Oh, yes. The one fellow that was deep into using computers at Los Alamos on calculations where you do things that occur at random and you look at the statistics was a fellow named Nick Metropolis[7], and he had just changed jobs, was going to the University of Chicago, and he was going to be in charge of building a new computer there. He wanted me to go to the University of Chicago. And this gets weirder and more interesting as it goes on. Brookhaven is about an hour or an hour-and-a-half drive from the city, and there was one fellow in the fraternity whose parents owned a factory in Pennsylvania. And there was a family, just a man and woman, refugees from Germany, that had a dog kennel. And then they had moved the dog kennel to right near Brookhaven. This fellow that I knew from

[5] Graham said "It was Zevi Salsburg and John Kilpatrick who were the physical chemists, and a physicist named Larry Biedenharn." in: Martin Graham, an oral history conducted in 1991 by Andrew Goldstein, IEEE History Center, New Brunswick, NJ, USA. http://www.ieeeghn.org/wiki/index.php/Oral-History:Martin_Graham
[6] John R. Pasta (1918–1984).
[7] Nicholas Constantine Metropolis (1915–1999).

the fraternity, and who had been in the US 101st Airborne, came back a different person right after the war. He was friends with [the couple with the kennel] and told me about them. Selma [Graham's wife] and I became very good friends with them. And if you used the bathroom [in Graham's house], there's a bird, a ceramic bird. We got that when they died. I think the last one, and then they died. But they were like parents to us. And they would talk with us and would talk to people that they knew that boarded dogs there, which was a very wide range of fairly well-to-do people from all kinds of occupations. And when I told them that I had offers from Chicago and Rice, they said they'd spoke to so and so, who is from Chicago, and he said, "People exist in Chicago, but they live in Houston." And that went a very long way to my deciding to go. Rice is a poor university, of course, compared to the University of Chicago, but Chicago didn't have any engineering. That particular comment turned out to be very accurate. The people … we just fit in very comfortably.

Peuto: We have to switch tapes, so let me ask you a quick question and then switch tape. There is a paper by a person named Adam Thornton called *A Brief History of the Rice Computer, 1959-1971*[8]. It was written not too long ago. I don't know when it was written. But are you familiar with that documentation of the history of the Rice computer?

Graham: I don't think I've ever read the whole thing. I'm familiar with it because at Rice, it was a very small group, and there were two students, electrical engineering majors, that worked on it essentially as technicians, and at Rice, if you graduated in electrical engineering, you got two degrees. It was a five-year program, and you took naval ROTC. So when you finally graduated, you went to work. You were a naval officer, and you spent the year working in the navy. And one of the two guys was named Joel Cyprus, and he did a doctorate under me at Rice on majority logic; he worked out every possible case for three variables doing it with majority logic. I remember he wanted to make it a little more complicated and do it for four variables. I told him it didn't go up linearly. It was variables that he would never finish. Then he came back and he said, yes, that I was right. So he did it for the three and got his degree, and he taught at Rice, and he consulted for Texas Instruments, and he developed some games people could play on little computers that were patented and that TI sold. I'm pretty sure they sold them. But at any rate, he was deeply embedded in the whole computer business, and when he was, I guess, about ready to stop teaching, he got this person [Adam Thornton] to document the whole history, and that guy knew Cyprus and he could talk to many of the faculty that were involved with the machine and so on. And so as I say, I'm quite sure I never read the whole thing, but I know what was going on.

<END TAPE 1>

<START TAPE 2>

[8] http://www.cs.rice.edu/History/R1/

Peuto: This is tape two of the interview of Martin Graham on the 15th of November 2011, and it is 11:45. Marty, we were leading to the reason, somewhat complex, why Rice University got the contract for the development of this new computer. So can you summarize a little what you were starting to say?

Graham: Well, it's peculiar in that I didn't write the proposal. So I don't know the exact wording, but it must have been that the three of them [Salsburg, Kilpatrick, and Biedenharn] had been doing work for the Atomic Energy Commission as consultants at Los Alamos and they could do more and have their students more involved if they had a computer comparable to what they had at Los Alamos. And they were already familiar with using it and working on problems that the AEC wanted solved. So it'd be a natural place to have three of them that the AEC's already using, and if it didn't cost too much, they liked to have the computer built at Rice.

Peuto: So it was the victory of locality and access to students of having to ____.

Graham: The reality of already they were in there and doing it, so there shouldn't be many surprises of things going wrong. And …

Peuto: I understand that you didn't write the proposal?

Graham: I didn't write it. I mentioned John Pasta, and he was making decisions of this type, and I was told—I don't remember how much later—that John Pasta had said to these three guys that if they knew that I was coming to be the engineer, he would've given them more money.

Peuto: But that's good.

Graham: Yes, the fact that the $450,000 was from the AEC. The $150,000 from Shell did not have the same kind of restrictions. I didn't need approval to do certain things, so I was able to do things generally as quickly as I wanted and then later get the approval and have money from the AEC go back in to the Shell money. One of the problems was the accounting department at Rice had never handled a deal like this before, so they had to broaden their abilities in handling that kind of an account because Rice had to specify what category of expense the money was spent on. And there was one case, they had an order to come to check that things were being handled okay, and the item, as I remember, there was a lady, sort of older, not really elderly but an older lady that came and did the accounting verification, and I had bought wrapping paper, and I needed it in actually constructing a part of the machine. And she said wrapping paper was an expense of operation and it shouldn't be included that way. So we backed it out, and it wasn't probably $100 or $200 at the most, but there was somebody looking at it and there were complications with the accounting department. We got to know them and the problems they had and so on.

Peuto: So to the best of your knowledge, the goal of the R1 project, as it's called later, was to build a computer, or did that computer have some characteristic that you guys had to deliver on?

Graham: No, I would say that the most important things we did and delivered were not required in the agreement because they were things that weren't thought of at the time. Let me give you a particular example. They were things to help science for people at Rice that needed that kind of help. There was a fellow that was a geophysicist and was interested in earthquakes. And normally the machine you use to measure earthquakes is a horizontal pendulum, and it's pivoted so its period is infinite. As if you move it to one place, it just stays there. So if the paper moves and the weight is staying fixed, you see the relative motion. That's a difficult mechanical accomplishment. Now, since you're only really interested in the movement, it doesn't have to be an infinite period, but it should be a long period. And once you build the pendulum and the support post and the guy wire, then the problem is measuring its relative position of, say, the post and the weight. So you do that usually with an optical beam. Then you can get magnification with a mirror and so on and a long path length. This fellow had built the sensor, and he had a plate hooked to the earth and a plate hooked to the weight and used it as a capacitor in an oscillator circuit. So the frequency would change with the position, and there was no light beam, and you didn't need an A to D converter because you were converting the frequency, and that's a standard procedure. And there was one big effect that was particularly interesting. The cold front from Dallas to Houston changes very rapidly in location as a cold wave comes in from Dallas into Houston, and it actually shows up as a seismic movement. So he could watch the cold front come in with his seismograph. And then he wanted to do computing on it, and that's what the computer is for. So there's a question of how do you get his data into the computer. And one problem is he can't do the computing whenever he wants. The earthquake happens whenever it wants, and he can use the computer when it's available. So we had four tape decks, transports, that we bought that were made for computers. I think they were eight-track tape units. So we fixed up a tape unit so we could read its output into the computer. In fact, that happened regardless of him because you're using that for temporary storage, your big amounts of data. The problem that comes up is when you put data on a tape and you store in big blocks, they're in blocks, and you somehow have the header identifying a block number, so you can search for that, see the block, and then read it. But in the geophysical work, time is continuous. It doesn't come in blocks. So we worked out a scheme that there were two kinds of blocks, headings, and one you could read and tell what block it was and the other read continuously but with a different coding on the eight bits. So you can tell the real time that it happened, so you could record continuously and then read off any chunk you wanted. That was certainly no objective involved when we started. So the objectives changed as the thing got used.

Peuto: I knew the thing that I have read in various comments about the R1 was that there was the funders, AEC and others, wanted you to be innovative, to try some new ideas, which is why you ended up having so many interesting capability-like features.

Graham: Yes.

Peuto: Tell me more about how you felt you were encouraged to be innovative, or were you?

Graham: Well, back at Brookhaven, when they were going to build a copy of MANIAC II, I didn't think they should build an exact copy. And certainly the people that had written the initial proposal were not ones who would be putting in an innovation. They just wanted the thing to be built and to work. But I'd say they were in favor of trying things, and it went clear into how you wrote the software, even though that wasn't my main bag, I was interested because sometimes you can do something in hardware that's very difficult to do, like this tape business with that source. I think it's interesting that I go to the EE and CS department faculty meetings at Berkeley. They have a meeting usually every Monday at lunch, and they discuss curriculum, what should be taught, what innovations in research should be done. So there's a whole issue of computers embedded in computers, where you've got a system and you want to do a measurement and it involves computation. Can you take the computer and build it into the system? So that's called embedded computers, and there's a whole group working on that kind of stuff. And the place you really see it is in automobiles. You don't build an automobile into a computer, but you build a number of computers into the automobiles. In fact, when they had this tsunami, the number of plants went down, and automobile production stopped on certain cars because of the computers not being available. So I remember the guy in charge of that section was talking about it and what they were going to do, and one of the really older professors was sitting right near her, and he said, "Marty Graham did that years ago." Because from my point of view, a computer was part of an instrument, not the other way around.

Peuto: That's a very interesting comment in the sense that I was going to ask you how come you started as an instrument person and you ended up making a pretty significant achievement in the computer business.

Graham: Well, I made contribution in doing electronic circuits in the instruments. The computer is just, to my mind, even now sort of a special aspect of electrical engineering and circuits. Now, furthermore, yesterday, a very unusual thing happened to me. I had gone to the department meeting because it was Monday. And I was going back to my office and met one professor who came 13 years ago, and we sat and talked. Usually, I don't have a chance to talk to these people. And I told him I remembered when he came and that I thought he was very good, and I still think that he's very good and doing interesting things, although I didn't know exactly what he's doing. And he said, "One person is describing his thesis in about 20 minutes, like a thesis exam, and I think you'd be interested in the topic." And I remembered reading the e-mail saying it was going to be given and what the topic was, and I thought I might be interested in it. But because we met and talked, I actually went and listened to it for an hour. And the topic was how can you reduce the battery drain power required in a communication link, a wireless link that's 10 meters, and it matters because it's a nuisance if the battery goes dead too soon. Well, the way you do it is you get every block to use less power. Well, they've already done that with cell phones. So now the question, is there something you can do that will use less power when it's possible to get by with less power? So it involved looking at the noise problem, and the noise problem is not the noise that's coming in, which is what I usually work on. On wired systems, it's the noise coming in on the twisted pair.

Used to be telephones, analog, then <inaudible>, telephones, digital, but it came in on wires, and I was always concerned about the trash that you got from wiring in the building, that kind of stuff. But if it's wireless, it's not the noise unless somebody's sort of accidentally putting stuff in on top. The noise is coming from your amplifier. So it's white Gaussian noise that dominates a large part of the field. And so how do you design the electronics in your link to get by with the poorest white Gaussian noise? And part of the answer is you go back to analog if it's a different kind of problem, but that takes more power. So this guy talked for an hour, and at the end, I realized and was told that what was really unusual is the stuff he developed was being put in production by Sony and he had done two tapes to fabricate two chips to test the whole thing out into the operational region. But the guy that I thought was so good and had been talking to, he asked the fellow some questions, and there was one question that was just great. He said, "How come what you're doing was known 50 years ago and nobody did it?" Because the technique of using two alternate things in the one approach and then using the one that gives you the less power had never come up. And what's so unusual is when I started doing stuff for Tut Systems it was the same deal, that you have a system and the noise is the white Gaussian noise in your receiver. So what I've been working on for a while is this is a very peculiar noise because you're generating it, and usually you don't want to generate noise, so you work your best to make no noise. But there's a whole thing about white Gaussian noise. It's only bad at certain times. It's just statistically. Most of the time it works fine, and once in a great while, you'll make some mistake. So the effort's all been on when you have a mistake once in a great while, how can you put in an error detection and correction? So for 50 years, the major effort is error detection and a pure mathematical thing essentially. What I've been working on is if you put in two amplifier stages, each one puts out the signal but with different white Gaussian noise because you're not controlling it. So it turns out most of the time the noise is so low it works fine, and on rare occasions, because otherwise the systems wouldn't have gotten this far, you put in enough power and it works okay. This thing, if you have two of them and only one is working, for it to not work, it's two very low probabilities occurring at the same time that makes it not work. So I've been working on how can you tell cheaply and easily that at least one is working? And I don't have to know how often it's not working. What I'm working on is building it, so, whichever one is better gets used. So, I don’t know statistically exactly how often it will go bad ‘cause the probability theory gets messy. But, this would be a very big improvement and it would reduce the noise battery power. So, the problem I’m working on, and I’ve wondered often, that what I’m working on people should have realized 50 years ago. It’s right there in the papers, that it’s this way and they never changed the drawing to be a little different.

Peuto: Very interesting. I have a question coming back to Brookhaven and copying the MANIAC. Is that the two computers that influenced the design of the Rice computer, the Brookhaven one, which is MANIAC I and MANIAC II, or are there other computers that influenced the design?

Graham: The design at Brookhaven architecturally, not the circuits themselves …

Peuto: Yes.

Graham: The MANIAC II influenced [the Merlin computer at] Brookhaven, but did not influence the Rice computer in the same way.

Peuto: So, there is a connection between MANIAC I, Brookhaven, MANIAC II, but it is not something that changed the architecture of Rice. It's kind of independent in some sense.

Graham: Yes. Now, the Brookhaven machine circuitry, some of the circuitry I had developed in connection with other measurement problems. Los Alamos was having trouble with the electronic circuits in the control section of MANIAC II. During the month I was there, I told them my circuit would work such and such a way better and they actually used the circuit from Brookhaven before Brookhaven put it completely in the computer. But, the Rice machine, the architecture, probably one of the most drastic differences is almost every large computer has a fixed word length and they've taken that word length and split it into two halves and an instruction is half of the full word. So, when you start doing programming, you have to say, "Go to this word and pick the left half or the right half." So, transfer instructions, the addressing for instructions is more complicated than the instructions for the data and the number was typically like for 48-bit word, 24-bit instruction. And if you're old enough, those numbers sound very familiar. It's the sort of thing like the ILLIAC, like probably the IBM 7090 and 7044. Well, it seemed to me when I started looking at all of this in connection with the Rice machine, what kind of information is there in the instruction, in 24 bits? Well, there's 2^{24} possible things and the instructions are almost always involving at least one chunk of data, 48 bits. So, you can take this 48-bit chunk, but the number of things you can do with it are limited by the 24, but subtracting off from the 24 the number of bits it takes to identify that chunk. And it gets a little confusing because a bit here is worth doubling something somewhere else. One is geometric scale; the other is arithmetic and when you think about it, you tend to think about one and you're not always sure which of the two you're talking about at the time. So, I was thinking, suppose we only put one instruction in one 48-bit word? Well, you certainly can do that, but then the question is what's the trade-off for these extra 24 bits that are now put in it? How many more choices can I give that would really make it worthwhile putting that? And it's not quite in the scale of the geometric or arithmetic. It's over into the philosophical because the question is how many things do you have to do until you get to the end? And if I had 48 bits and the memory is not very much different in size, then I have to take as many bits out of that 48 to handle the memory address. But, it still leaves me a lot more than if it was only 24 bits that I had and I took a chunk out. So, the issue was is there something I can do that really makes a basic difference about the way you think about the whole thing? So, that got me to thinking about when you write a program, for example, are you going to do it on fixed-point numbers or floating point, or not on neither, but where if it's 24 bits, it's 24 bits. Where you're looking at each bit having a meaning because of where it is in the work. Now, that's a whole different ballgame and that was already being done where they take a few bits and use it for indirect addressing. In the IEEE interview[9], I mentioned suppose they had three bits they were doing. Well, three bits lets you put in three different ones and do one thing and do the second and do the third or do none. You have a choice of doing or not doing each of three things, or you could look at the three and say it's one of eight

[9] See footnote 5 on page 16.

possible combinations and doing eight different things or not. It doesn't sound like that's a tremendous difference, but after I thought about that, we ended up in the Rice machine taking eight bits. You had a choice of doing each of eight things or not doing it. That's a lot of different choices. Then, the question that comes up is suppose I've written this program and I want to be able to run it in fixed point or floating point. Well, what you mean by an "add" in the two is essentially the same thing conceptually, but if it's in a computer, what you actually do is two different things. So, maybe if you took every word that was a data word and you took two bits out of it and you could say, "If this bit is a one, do this in floating point and if it's a zero, do it in fixed point." That means I'm throwing away two bits in every data word, but you very seldom have a program when you want to do one order in fixed point and one order in floating point. So, the innovation was to take a bunch of bits and that would be used to tell what the address was. And the address would be a relative address where your data is. But then, you could put a bit or two there to tell whether that block you have put in in fixed point or floating point. So, by taking quite a few of the bits and using it in things that decide you're going to do it this way or this way, you make it so your machine does it whatever way is appropriate for the way you're doing everything else. See, this mattered a lot to the physical chemists because they wanted to take 24 bits and have each bit represent a location in a 24x24 array. So, you would look at 24 words in a row, you'd have 24 squared sites where a molecule could have something or not and it would be a map in space and of course, you could do it in three dimensions also. So, by putting in that kind of information associated with the address and information as to where the address is plus iterating one step, this extra information about where the address takes you, you can also say something, "I'm going to do an additional calculation as to what row and bit number it is and go there." So, you can embed a whole computation dealing only with the address and you're putting all that in, but it's worth it to put it in because it saves on how many 24-bit orders you need. So, that was very innovative and that's in the basic, philosophical structure of what computing is all about.

Peuto: Focusing on what you just said, was that you or the team that came up with those views?

Graham: Those views about the trick? That's me because that's too big a change for anybody else to risk. But, there's one other point; that is, I already had the circuits that would do it economically.

Peuto: Yes. As the principal investigator for the machine and in this case, the conceptor, you were dealing with hardware for sure like you just explained, or were you also dealing with software, or was there somebody else that was a critical software person in coming up with those ideas?

Graham: They got a very good software person and he and I didn't get along particularly well to the extent when they wrote a recommendation for me and it's one of the people I was originally working with, they would write a very good recommendation about what I had done. And then, if somebody wanted to say I didn't do much and they would get a recommendation written by this guy, he would write, "The machine didn't do much in the way of hardware, but the software I built to run on that machine was really superb."

Peuto: So basically, there was tension between hardware and software if I may say so.

Graham: Not between me and the software people that were going to use it for their problems, but it's like medicine. The only thing that's important is the disease that you treat. Anything can help and you don't give a damn. If it's a disease you're treating, you don't want to cure it. I should put it this way. You don't want to prevent it because then you put yourself out of business. What you want to do is diagnose it and have a treatment that's expensive and takes a long time.

Peuto: Now, to come back to transferring these innovative ideas into products, I guess you started working on the computer in '57.

Graham: Yes, that's when I went to Rice.

Peuto: According to some other documents I've read, around '59 you had working parts of the machine. You were starting to have things working and I understand that the machine was more or less finished in '61.

Graham: It's a blur in my mind. If you're working on a machine and any of it works at all in a year, you tell the people who keep asking you, "How are you doing?" You say, "I got it working." For the people that are using it and it doesn't do what they want yet, it's not working. So, there's a substantial gray period over which you say it's working and you're not telling the truth. You're sort of telling it, but it's so widely accepted because everybody goes through it. So, for example, we said the machine was working when it didn't have any tape units working. So, there were complaints from people that needed the tape units for what they wanted to do and they said, "How can you say it's working? You don't even have the tape units working." But, if you had the arithmetic stuff doing arithmetic-- see, even if it did fixed point, but not floating point, but did it correctly, the fixed, you'd say it's working. We just have to do a little bit more to get the floating point. That's just software.

Peuto: One of the issues was clearly what kind of input/output devices were around …

Graham: See, that came later. The original is you want the same input/output devices as you used at the place you're trying to not have to go to. So, for years, the input was punch cards. Then it was punch paper tape. So, you bought somebody's punch paper tape reader and that was your input and you like to have printed output—not graphical for a long time, but just printed—which meant you got an electric typewriter. Now, it's interesting. At Brookhaven, on the machine that I built that was essentially an instrument that was measuring 1,024 channels of something and you wanted therefore 1,024 numbers printed out and the numbers were, let's say, ten decimal digits. No, it wouldn't be that long—say five—because the number of things you're counting were small. So, it would get printed out on an electric typewriter and before that, it would be printed out on a teletype machine before you had electric typewriters and you had teletypes and teletypes have a character set and it was five-bit characters. So, you would have to build something that would take the digital number in binary form, convert it to the right five-bit thing and print it. Well actually, what I did at Brookhaven was to take the teletype machine and it has a mechanical encoding. You have five bars. You move each of the five and the slots line up for the combination you chose. So, I had to make five new bars and decode it mechanically. Of course, I didn't want to be bothered and it's a mess to do it electrically. So, that worked out very nice. The output was a mechanical-- you raised the question input/output. So, you could get the output in the form that the user wanted and it was up to you to do a match somehow between the way they want it to the way you had it. And the connection with the geophysical guy at Rice, I told you he had a sensor that gave it out, digitized right off because he was doing that electrically and he didn't want to see numbers. He wanted to see the traces if he had a mechanical sensor with paper running and running a trace. So, we bought a strip chart analogue recorder and converted the numbers to analog into the recorder.

Peuto: One other thing that is noticeable about the history of the R1 is that it, as I understand it, was an asynchronous machine and there was a lot of ability in hardware to upgrade. So, you started, for example, with CRT storage tubes and at some point, there was a conversion to core memory and there was the addition of this A to D, D to A effort on the channels. So, can you talk a little about the design philosophy that made it possible to keep upgrading the machine until you couldn't do it anymore?

Graham: The upgrade from electrostatic to magnetic was after my time.

Peuto: Oh.

Graham: How you decide is, there people that are making money or hope to make money building core memories. So, when they get their price down low enough and offer to sell it, they have a problem. They don't want to sell it to their competitors who are trying to do the same thing, but if they can do it and they want to sell it to a big market, you just go to them and you say, "I want to buy it." That comes up with the printers. There was a kind of printer where the characters were all on a wheel and the wheel is rotating and you have a hammer that you can wallop. So, in an electric typewriter, you have 50 keys and you wallop one of the 50. In the printer that's a rotary printer, they guy that made them was Shepherd, you'd rotate it and you had a sync lock and it was timed and you walloped it at the right time. So, whichever one you wanted to use, you got him to tell him how he does it, at what two signals he needs. You give him either a coded thing and he picks one of 50 hammers to whack. In the electric typewriter it's nice. It has not on the wheel and it doesn't have 50; it's got them on a sphere and you rotate it in two directions in space to get the one you want and then you whack it. So, people that are good at the mechanical end go down one path. Eventually, you get to the point where you can make a dot with electrostatic means, laser printer. Then you forget about those two and you just buy the laser printer. So, in terms of a particular output, I'd say generally the primary consideration is what's available that works well at low cost. If you can manage from selecting from a group of things like that then you give the user whatever one he likes.

Peuto: One of the thing that you were alluding to minutes ago was the concept of tag bits.

Graham: Yes.

Peuto: Yes. You started off with talking about indirect addressing. So, why don't you give us more feedback on the indirect addressing issues?

Graham: Okay. Let's say you're working on a problem and there's a horrendous amount of data. Your memory is big enough to have it all stored in the computer and it's arranged in a certain size block of data. Let's say a thousand words. Now, if you have a million words of your data and in memory it's in blocks of a thousand, you have to identify which block of a thousand you want. So, you have to know where the data is in your million words of memory, or the computer has to know when you're telling a computation where to go to. So, if you say that this computation I'm going to do, I'm going to put these words in blocks 608 to 610. I've got two blocks of a thousand words each and I know that that's right because I just put them there. Then I want to write the code and I write the code so that that block of 2,000 words is numbered one to 1,999. I write my code so I do take word one, add it to word two to word three, whatever the computation is and that deals with mathematic computational technique. So, I have to match what one it should actually physically go to from the one that I've concocted to match my description of the computation. So, I give myself one more number. I know that if my code is written and it reads number one, I want it to change to number 6,001. So, that's a simple thing. If I know the number 6,000 is stored here and I say I want this added to that and then take that number and use it, I just tell it to add this to this and I have to get the right number here. So, I call that indirect addressing. It's address one, indirect to starting at 6,000. So, that's the indirect addressing part.

Now, that's such a neat idea is suppose I want to pick a thing that's related not to a block of a thousand and pick one of them, but I have a block, ten blocks like that and I want to pick the address for one in one part of the code one way and another part of the code the other way. Then, I'd like to do an indirect addressing, which says if this is number one, go here and it'll tell me where to start looking up, where to start in the next one and then that can go to another one, which has another piece of the code telling me how far to go in that. So, if I can do that twice, I can write a code where I do the indirect addressing twice and I store the blocks correctly. It's so good, why should I quit with two? So, in the Rice machine, we didn't quit at two. You can keep going. Now, suppose you make a slight mistake and it goes to the first place and it says go here and it'll tell you what to do next and it says do the same thing again. It's an iterative loop. So, it just squats and it's the kind of thing it's difficult to write a code for to tell that it's squatted at such and such a place. Then besides that, you have to do something to fix it, but at least you like to know it's grunting and doing nothing. So, once I decided we're going to put in multiple level and do this and if you can look up a starting address, you can look up an extra bit or two that tells you whether all of this is fixed point or floating point and that even lets you do a computation of part of it in fixed point and another part in floating point because you've got the tags, but it squats. So, we put in an extra hardware circuit that's counting how many times it's gone through this before it says, "I got it" and it's right. You can't tell that. The coder can screw it up. So, you wait until it is squatting because it's gone past and then <inaudible> it will just squat. Now, in a machine where the output is print and so on, it's printed out as it is on essentially all machines now. In the Rice machine, it had a neon bulb associated with every flip-flop that may flicker except when the machine squats. When it squats, you look up at the machine and you have it spread out over 20 feet and you can see that far. You make the doors transparent so you can see all the neon lights and you at least recognize right away that they're not flickering and you know which rack to look at. So, it's a hardware detection and a visual display directly to the operator.

Peuto: Now, I guess, you did not get involved too much with the software on the machine?

Graham: Occasionally, yes, I would write a test routine, where we'd iterate something and see if it came out the way it was supposed to.

Peuto: The programs that are associated with the Rice machine like SPIREL.

Graham: Yes, that was the compiler, right?

Peuto: Yes, EP1; all of those with …

Graham: I didn't do any of that.

Peuto: You didn't do anything about it.

Graham: No and they told me any program that I wrote for testing didn't read like anybody else's program because I was doing input and output visually.

Peuto: The other thing that I wanted to know is the notion of code words and the fact that essentially you were one of the first to design a machine with what is called capabilities, you know, type tags, and because of that, you had a big influence on Burroughs because some of the early people like Barton that were working with you guys on …

Graham: On time sharing.

Peuto: ...on the Rice computers left whatever they were doing at Shell and then probably got very influenced by the decisions that were made on R1. Am I in the right direction?

Graham: Yes. Generally, when there's a faculty meeting to discuss should someone get promoted from assistant professor to associate professor, to make sure that Berkeley remains a great university, they can't depend just on the department making the decision. Of course, I guess the thing they're worried about is two people are good friends. One is crummy, but that won't matter; the other one will still promote them. So, they want it done uniformly. Well, if you're going to do it uniformly, you have to write down what you're grading him on. So, they have a campus-wide committee that says, "This is how you should grade somebody that you're considering for promotion" and it's how many articles have they published. How many references have been made to them? Then you get letters from a number of people at different universities because usually you don't have very good friends at all of them. You have somebody that's honest at least at two or three and you can look at them. So, that gets around at least that part of it. So, you've gotten how many things they've published, how often, what period of time, and outside evaluations and citations. Then you want to know do they work and handle the load in the department that nobody wants to do like handling scheduling, that kind of stuff, and their involvement in running professional organizations related to that view. So, you get these letters and you have a committee in your department that goes through and says, "These are the things you look at and this is how we would grade them on each of them." Oh, and what do the students say about the teaching? Is it good or bad? And they say that, they give you that opinion at the end of every course they take with the professor. So, there's a lot of personal animosity or reward in what they say at that time. In general, I think it's very short on whether what they taught you was important or not. If they follow a particular syllabus exactly, right or wrong, that doesn't come through. Their judgment is lousy, but they do exactly what they're supposed to. The student can learn it even better if they, by the book, get together with three friends and talk to each other. So, my feeling is the list of criteria is very peculiar and not correct. So, the net effect of all of this is if they really enforce it, everybody on the faculty is a clone of everybody else. You must have some feelings from the time you spent getting to Greece is the way different people fit. So, my feeling is if you're very lucky and you're interested in doing something meaningful and new, if you're very lucky, you find an advisor that will go along with you, but the more usual thing is you'll do whatever one of them wants done and he's got money to do it from people that have been through that whole route. So, the overall feeling I have is one of pessimism.

Peuto: Okay.

Graham: And if I want to know if the guy did something good, or I did something good, if I meet him 10 to 20 years later or even longer and he says, "I had a course with you. It was on such and such a topic and I've used the information professionally." I've had cases where a person will say I've had a course with you. For example, "It was in computer architecture and you talked so much about how much things cost and how much that mattered that I went into real estate." That's a true story.

Peuto: On the subject of the impact on the Rice computer on the Burroughs machine, was there an impact?

Graham: Yes. What got me started is there weren't papers published on this kind of stuff that you can see from [Thornton's] report and from the IEEE [interview that] has this kind of thing in it. We gave very few papers. I didn't like to write papers. Well, I still don't particularly care to. So, if you don't have papers, how does the information get around? Well, that's mouth-to-mouth and working together. So, the business from Shell, what's interesting is not only did they give money, that the people that learned things being involved with using and building the Rice machine conveyed that information to other places. The overall structure mattered in cases where the machine was being used in a somewhat different, more flexible way like in time sharing. Any of these features were important in getting the software to do what had to be done in the physical world with the parts that were available at the time. And they would either be working for a company and using a machine that they would like to have these capacities/capabilities in or they were working at designing a machine and it would be an advantage in selling it to have these features. As far as I could tell, this worked quite well in getting a lot of these conceptual things out from R1 into the computer people at Shell and probably through Shell to Burroughs. And they built the machine, but they didn't get the circuits right.

Peuto: I don't know about the circuits. One of the thing we alluded to and I would like to reemphasize was that you had a standard circuit that you used that were solving lots of the problem that existed with the circuit.

Graham: With the circuit.

Peuto: And in particular, you had the idea of having some transformers, you know, to adapt voltage of input versus output and that was very new and that's something that you started at Brookhaven. So, all of this is correct?

Graham: That occurred before R1. It occurred directly with Los Alamos, but it was not very new. It's very old stuff that people didn't know how to do.

Peuto: So, very quickly [before the tape ends], your machine also could be micro-programmed. Am I correct?

Graham: By the time you do all the indirect addressing and everything, yes. It could be micro-programmed; more than that in that if you now have 48 bits and people were getting by with 24, there were some bits that maybe you could do something with. So, we decoded a particular combination that's not already used and build a whole control unit section to do that. That's micro-programming.

Peuto: Yes, so technically you had, and I hope I'm correct in my example, you have 48 bits, okay, and you could have 24 bits for an instruction and then you could have essentially two instruction in parallel, you know.

Graham: Well, you do them in sequence.

Peuto: You do them in sequence, excuse me. You do them in sequence, but then there is so much-- thanks to the tag bit, the indirect addressing and a few other features you can essentially create new instruction equivalents.

Graham: Yes. The question about how long should an instruction be has come up not 20 or 30 years ago, but in the last, say 5 or 10 years, I think that Hewlett Packard's group actually got into making different length instructions for different purposes.

<END TAPE 2>

<START TAPE 3>

Peuto: Okay, this is tape number three of Martin Graham, an interview done on the 15th of November, 2011 and it's 3:05 p.m. Marty, you have said something very interesting, which you said you really view computers as being embedded. In sense the computer is embedded in some of the--

Graham: Oh yes.

Peuto: In some other tool, in some other device and much more than …

<interruption to adjust lighting for less glare>

Peuto: So in essence you were defining computers, maybe, not as the central part of the equipment or the device, but as a supporting part, maybe without the computer the device wouldn't exist. So could you talk more about this because this is intriguing?

Graham: Well, we're talking about if you go back to 1950, that's 60 years ago. That's a long time even just looking at it in normal times, but the rate of change in the electronics and information and high tech industry has been tremendous over that period. And I think if you go back to 1950, the computers were difficult to build, cost a great deal of money and relatively few people used them and could appreciate how important they were. So how significant the computer was compared to the rest of the world was determined in a fairly unbalanced way because it was such a unique and different item. Today, the very large computers still are to the rest of the population as they were in 1950, but the cost has plummeted, the number of people that use them and know about them is a very much larger percentage of the population and consequently their impact depends on their being used in so many varied ways and so many applications. So I would say today the difference is they're really appreciated, maybe a little incorrectly but mostly correctly about how important they are and how much they've really affected all our lives in many ways.

Peuto: Okay.

Graham: Let me add one piece to this on the change. About three months ago we went out with another couple to a very nice restaurant to celebrate my 85th birthday, and I realized …

Peuto: It was in April?

Graham: No, July.

Peuto: Oh, July.

Graham: And it was a later day, a week or so after my birthday, and I realized blowing out 85 candles is hard and getting 85 candles on a reasonable sized cake is hard and then I also realized that sitting in the restaurant and overhearing the conversations was very different than 85 years ago. People were talking about computers, binary systems and so on, real computer language as it was in a very small group, 50 years ago. So I made up a little candelabra, a strip of wood I think with six holes in it to hold normal birthday cake candles and labeled them one, two, four, eight, sixteen, thirty two, sixty four. One of those will last more than a normal life time. And it was very nice, we could blow them out, we had them at the table, we lit them and we told the restaurant they could keep them for other people celebrating anniversaries. So it's kind of a way of expressing how big the change has been, in answer to your question.

Peuto: Okay.

Graham: I told my attorney about that and said would that be patentable, and I asked him to check with other people in his office and when he called back a week or so later, he said it was patentable and he's going to give it to me as a birthday present.

Peuto: Why any physical device has a better chance of being patentable than software.

Graham: Yes.

Peuto: I wanted to get a clarification. If I understand correctly, Bob Barton of the Burroughs Computers worked for Shell, was involved very early in the Rice Computer R1 and left Shell to start working on the B5000 where his knowledge of what happened in the Rice Computer must have been useful to him despite the fact there were very few papers written about the technology from the Rice Computer. Did I get it right?

Graham: I think all of that's accurate. I don't recall dealing directly with him particularly, but what you said I think is accurate from what I'd heard.

Peuto: Okay. You alluded at one point again during the two previous tapes that you were doing some work in research and so and you were talking about, I think, Shannon's Theorem.

Graham: Yes.

Peuto: Without you saying it, because I happen to know what you have been doing. Could you talk a little about the kind of work you're doing, because I guess as a retired faculty you never retire, you continue working.

Graham: I continue working. At Berkeley, if you're retired you share an office with another retired person and you have access to secretarial help and so on. So it's a very nice arrangement and you can even buy a parking permit, which is significant.

Peuto: In Berkeley.

Graham: Yes, in Berkeley. But at any rate, I've been working on things that interest me and sometimes someone comes up with a problem that I know something about. Sometimes it's something I've just developed an interest in, but this particular issue is related to, let's say you have two wires connecting a transmitter to a receiver and you want to transmit digital data over it, and you're limited as Shannon analyzed it. The limitation is the random noise that is in. And it's not the random noise that comes in on the wires. It's the random noise that comes in from your amplifier stage. And in the physical world, this is all reasonable. You can shield your wires from one point to the other and really have no external noise and you can drive it with a clean signal, no noise added, and it comes out to the other end, out of the wires with no noise because there's no amplifier. The amplifier is dealing with very large numbers of very small things. It's electrons and many, many electrons at the current levels that are used, and consequently the noise has the character of white random Gaussian noise. In Shannon's paper, there's a picture of the analog link and it's always time-invariant analog, which is never true of any real link. If you put in enough, you burn it out. It's nonlinear, but in the operating region that's in common use it's linear. And if nothing's changing too fast, it's time invariant and if you do things quickly enough, it's time close to time invariant for that to be considered in the analysis. So it's kind of unique in terms of working on a system that's to work in the real world that your mathematical approximation is very close. And in the papers of Shannon and all the other stuff that I've seen in textbooks related to that, it's always one amplifier with one box giving the random white Gaussian noise with an arrow carrying it in, adding to the other and then coming out to the rest of the system, and that's true independent of what the modulation technique is. So you've gotten well, well into the problem under those conditions and as far as I could see there were two things that were peculiar to me. One is whenever I analyzed a circuit that took a signal from one point and took it to another and there was an amplifier involved and the impedance out of the thing going into the amplifier was different than the input impedance of the amplifier you matched the two for maximum power transfer, and that was the way that you could get the biggest power out or power in on a signal. And in the Shannon papers I didn't see any impedance matching network. Now, if you match with a transformer, you're not into the wideband Gaussian noise because the things in the transformer aren't moving around and they don't have the random variations. So you should at least do an impedance match to do the best with what you've got, and that didn't seem to be in any of the pictures. So I figure, well geeze, when they were doing this the amplifier impedance must have been close enough to the output impedance of the wires coming in so it didn't make much difference. And if it did, they should have put the transformer in and it would have been better, but the calculations would have been completely correct except for a constant due to accounting for the "you're not matching and how much better you could have done", and that seemed the case in all the pictures I looked at in the textbooks. And then it dawned on me that there was something really wild here, and that is if you put in two amplifiers and you input the signal into both, you would get the signal out of both and if the amplifiers were the same, you'd get the same signal out of both. But the white Gaussian noise would be white Gaussian noise out of both, but white Gaussian noise doesn't tell you what the signal looks like as a function of time. It only tells you the statistical property. So if you took two white Gaussian noise signals and looked at the two of them, they would be drastically different almost all the time. They would be the same under the rare conditions when you had no Gaussian noise or at least similar if the noise was so big that both of them clobbered the operation. And big enough for it to clobber it meant so big that the chance of that happening was low enough so the system was acceptable to you. If you could tolerate one error in a million, you wanted to only have the statistical properties such that each time you do it, it was only one time in a million times that it didn't work right, and the actual time depended, of course, on the bandwidth, but the percentage goes with this function of Gaussian amplitude being e^{-x^2} (e^(-x^2)), and that's a function that varies very rapidly with the square of x, and there's a coefficient for the x^2 (x^2) that's essentially describing the signal to noise ratio for that particular amplifier and signal. So here was a number that you knew a great deal about statistically, but didn't know what the real s(t) was. And you were the one that was generating it with this piece that you put in that generated it. So if you put in two of them, the likelihood was very, very high that they wouldn't be the same. While I have been looking at modulation techniques, and if you look at the amplitude of the signal at specific times, sampling times or clock (???) times, you control the picture, and if the picture that you're looking at, the signal can look like almost anything and all you care about is its amplitude. And when you start looking at how much power is involved in the amplitude, it depends on how many different amplitudes you're dealing with and your signal to noise ratio. So you had control as to what you were doing and the performance was determined by what you did at that one particular modulation with the Gaussian noise over all time and you were looking at the statistical property of that noise amplitude. And I'm used to doing the stuff where I know the signal is a function of time. So I was thinking back, and you really use Fourier analysis where the signal is a sine wave. And the reason you do that is I think in the 1700s, Fourier was looking at all different kinds of signals and he's a mathematician and they all looked like sine waves when he looked at musical instruments or a person singing, it was sine waves. And that was all tied up with the real world being composed of resonant components. So they generated sine waves, but if you're dealing with something that isn't resonant sources, the sine waves are because you're hooked onto one mathematical technique, and it's not the right one. Now that was apparent if you were going through electrical engineering and you did all the sine wave analysis, which is what you had in a power system. It was all 60 cycles or 50 or I think 13 1/3 for railroads, but it was sine waves and the frequency that they picked to use went with the way the real world worked, and they picked one frequency and you were really in great shape because the Fourier analysis made the mathematics easy. Now, when TV came along it was no longer resonant sources. So the curriculum got changed to include how you manage to use this great technique for non-resonant sources and the signal was: you took all the non-resonant sources, non-sine waves which you get from pictures, scanning them, and you change them to a representation and you make them the sum of different sine waves. So if you take any signal you got and you represent it as the sum of sine waves, different frequencies and amplitudes, then Fourier series is just great. Then you work the problem. And then you add your answer back to get a single answer: what all the sums are after they go through, and that's what we learned from about 1950 on, first in more advanced courses, the Fourier equivalent and so on. And it was sort of a standardized way of finding out what the representation was, and they would plot almost always only the amplitude representation, but not what happened to the phase at each frequency. And then when you got up and added them up, got to the end added, it didn't look like what you really saw unless you went back and included all the phases. If you don't go through that step you get the right answer right off by doing the straight algebra where things depend on rate of change of voltage, rate of change of current. So I went back and said if I had only one wave shape and I made it a cosine wave for a half cycle, it would go from a value of plus one to minus one. And if I at that exact time put in a second one that went half of a cosine wave from minus one to plus one, it would finish one cosine cycle and I'd have a problem that involved analyzing the circuit for only one frequency, but that was the only one I could work. I didn't put any information in. But if I made the interval from the end of one to the beginning of the next and made it one interval for a zero and a different interval for a one and then drew that wave form, I had a single wave form versus time to analyze and it had the information in it, and I had the option of measuring that time interval either by the end of one to the beginning of the next or the zero crossing of the first half to the zero crossing of the second half. If I'm looking only for a zero crossing I can even get the amplitude wrong and know what's going on. So I now had a system where I didn't have to know the game to make my stuff work, and in addition, I didn't have to know the frequency but I only had to know a frequency interval not the exact time of anything. So I sort of didn't have to know recovery of the synchronization signal or the clock. And a lot of work had been done on retrieving clocks, and basically the way you retrieve a clock signal and do it accurately is you take the signal that's varying with a varying amplitude and you can get a very accurate analysis of the amplitude of a Gaussian varying function by looking at it for a long enough time and averaging it. So the only thing I gave up is I couldn't get an answer quickly. I had to spend a lot of time for each bit retrieving the zero crossing time. But if it was always the same amplitude and the signal was big enough I should be able to do it with a good signal to noise ratio. And it turns out that the derivatives changing faster than the amplitude changes because of a big amplitude because the derivative goes as two pi times this frequency. So there was an inherent advantage it seemed to me. So I have been working on that, and you also have another difference in that the peak amplitude you have to handle if it's to be a linear signal doesn't change with the bits you're transmitting. The average power, it turns out, is higher the longer you take the transmitted bit, because it's at the value of plus one or minus one. If it's sine waves, the power is the square root of two times a plus one or a minus one. So just looking at the pure mathematical end, it looked like there was some advantages there. And then, my idea was that if you look at something statistically, how long is it this way or that way or what's the probability that it would be the way you liked it instead of the way that's really screwing it up for you, you should at least be able to make it work better part of the time. So I was looking into that. And I had already looked at this business of the sine and cosine and the attributes that went with that. And I wrote up a patent application—or the attorney did—saying if you do this and you modulate it with the time, you have all these advantages. So that had been filed, and it takes a long time for it to go through. So quite a bit of time went by and the patent office finally said they approved it. And then the Supreme Court heard one patent case and they were looking at what was meant by something being obvious. And the conclusion that they came up with was different than what the examiner had come up with or the patent office and a few examiners. So they gave us a notice that they had revoked the approval of that patent. And then recently, I got a notice that the patent was approved because they've rewritten the patent law and the overall view of things has changed, probably changed most because of what people were doing were software patents, but the most drastic change is that the time you start counting, is not when you invented it but when you filed. And if you're talking about when you invented it, you can do a lot of lying.

Peuto: Okay.

Graham: So I'm now at the point of how do you tell which is best. If you know exactly what the amplitude of the signal is, then you know the signal and anything wrong with it is the noise. But to know exactly what it should be everything has to be working perfectly. Suppose you almost know what it should be and you'd like to get an idea of how bad the noise error is. You don't have the right numbers to do that calculation, so I moved on. You do the calculation assuming that the numbers are just what you would like and you measure what the error is in your measurement. And then you put in the second amplifier and you do the same thing with it. So you get two different measurements, not over a very long time to get them accurately, but you get two inaccurately but at the same time. So I know it made an error or I know it made another error, but now I can look at the two errors, which I now know, not accurately, but I know two errors and I can see which one gives the smaller error. My original idea was to know what gave definitely an error and detect that, but then I have to tell the rest of the system, geeze I don't know what's right or wrong, but I know it's not right. But if I know two, I know one is more right than the other and if it turns out that the times that it's too wrong to be usable, which is when both of them are bad, really bad, if that happens way, way less in this system than with only one amplifier, I can build a better system and use less power. So now to look at the statistics of this and probability theorem and come up with a number of how much worse it is, what percent of the time is really beyond me as in that area, but I'm sure that it's better for some sets of numbers. And if you make the ratio such that you know it's better, you don't know exactly how many times it's better but you think it's quite a bit better, that I believe is patentable. So I filed my initial thoughts and in that write up I didn't say I put in two amplifiers, I put in N amplifiers. So if I'm doing roulette gambling and I'm placing 31 bets at a time, I'm pretty sure it will win. So the preliminary filing is that description and I have to write up the new description which I've only finally realized in the last week or two so that he can file an application. So that's very exciting and rewarding to me, tied up with why didn't anyone else come up with this in 50 years. And I've heard talks and judging by who's giving the talk and what they say I can understand why. It's usually because there's a solution to the problem. They know the solution. They spent years using the solution and the fact that it might be somewhat different has never occurred to them. So in terms of what I'm working on and why it excites me, it's just the interest of it being a case that I think is true and the fact that people tend not to think of problems that way. And in a lot of the things I've worked on, the problem has usually been solved by realizing that there's a constraint that's been included at the beginning which can be left out. And I think that's something that we don't teach at all. We don't even teach them to be inquisitive enough to ask what can be changed.

Peuto: Okay. I have a few more questions.

Graham: Okay.

Peuto: A correction, it was my impression from the Thornton paper that you left Rice University to go to Berkeley because of some dispute in Rice University and you told me in a private conversation a few hours ago that in fact at that time you were looking for a job. You were thinking of leaving Rice University, excuse me.

Graham: Yes, I wanted to leave Rice. My usual thing is I have a disagreement which I'm becoming more and more disgusted with my immediate supervisor, the one who's making decisions of what I can do and can't do and so on. And at Rice it was with the dean of engineering.

Peuto: Okay.

Graham: And I would say the culmination of it was that they wanted to hire a new chairman. The chairman at the time I went was I think a retired colonel in the Air Force and his field was power. And he didn't want me hired. And the people that were recruiting me, the three faculty knew that he would be opposed to me. So they arranged for me to come down and be interviewed by the president and other people on the faculty, but I never me the chairman of the department I was to be hired into. And I mentioned to you the disagreement that came up because I was paying my technician more than he was paying his technician. It was that kind of thing. So I knew I wanted to leave, but the culminating thing was they were looking for a new chairman for electrical engineering and the dean of engineering had come out to Berkeley to interview people and he spoke to one person who was an associate professor of electrical engineering and it had been made clear to him that at Berkeley he would never be promoted to professor. But he told the dean from Rice who was out here that he would be interested not only in telling him, telling the dean, who was good or who was bad. He said he would be interested in the position himself. And I was on the committee looking for who should be the next chairman and I heard from the students that the dean had hired the next chairman and he had never mentioned it to me. And I had a sabbatical coming and I took the sabbatical at Berkeley. That's because the guy[10] that they had hired to be head of the Computer Center was a mathematician from Illinois and he was willing to come because the chancellor at Berkeley who negotiated with him promised him that there would be a department of computer science in the College of Letters and Science, which houses statistics and mathematics. And the fellow that currently was associate director of the computer center and a professor of electrical engineering was leaving to go back to Utah. He was Mormon, and he was founding a company and going to be on the faculty of the University of Utah. His name was Evans[11] and he and Sutherland[12] founded a computer company specializing in computers for graphics. So I was a person, probably the only person in the country, that was acceptable to a person who was chairman of electrical engineering[13] and acceptable to a person who was going to be chairman of computer science, but not acceptable to the first one because he wanted computer science inside electrical engineering. So there was a fight going on where it should be. And the chancellor that promised the department in Letters and Science went out with the free speech movement. And the new chancellor at Berkeley had not made any commitment. But the way it finally got settled is they did form a new department, the CS in Letters and Science, and I was appointed to that joint position.

Peuto: Okay.

Graham: It's a wild story.

Peuto: Yes. And that's why I have a master of science in art and science. One of the first things you did at Berkeley, and this is at the edge of what we are dealing with is that you worked with a multiplexer that was being designed at the computer center?

[10] Abraham Haskel Taub (1911–1999).
[11] David Cannon Evans (1924–1998).
[12] Ivan Edward Sutherland (1938–).
[13] Lotfali Askar "Lotfi" Zadeh (1921–).

Graham: Yes. They were doing work at Berkeley on time sharing systems, and time sharing systems all involve a big central machine and a lot of smart enough terminals to interact with it. And there was a project going on there that was from DARPA[14] And it was kind of a diverse group. They had a number of very bright students working on it. And although DARPA was a great help, beyond getting this set up and they were also working on this kind of thing at MIT on time sharing. So the person in charge at Berkeley, I'm trying to think who it was.[15] It was somebody who wasn't quite up to that job. Also there was a fellow named Mel Pirtle that was studying for a doctorate in electrical engineering, and he was quite bright and energetic and Butler Lampson was involved, who was very bright and very energetic, and a few other people. Martin Deutsch[16] was a physicist at MIT that I had met and his son[17] was at Berkeley, and he was involved. So they had a good group and they needed the terminals to hook in, which wasn't their main bag. It was the system their people were involved in. And I guess at MIT they could just buy terminals and the hardware that would interface and it was how you managed the resources that was really important. Now, that's where you became involved.

Peuto: That's true.

Graham: Because it was a question how are you going to manage the resources and what other resources and who's going to make the decisions and there was no work done that really settled any of that, but it was handling a real estate deal with two people are fighting with each other and the best you can do is let them fight it out, decide what the agreement is and have a referee holding the money until they reach an agreement. So your thesis was on time sharing, but the practical aspects of it[18].

Peuto: Let me mention my last two or three questions. Do you have any statement about the Computer History Museum that you'd like to make or you have no specific opinion or what?

Graham: Let me go back a little further. The computer business was dominated by IBM by giving universities a very big discount so other companies weren't able to compete and they had very good mathematical people in the software end that worked directly with people at universities on the applications. The other large computer company was … who did the 6600?

Peuto: Control Data.

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[14] At the time it was known as ARPA (Advanced Research Projects Administration).
[15] See http://coe.berkeley.edu/news-center/publications/forefront/archive/forefront-fall-2007/features/berkeley2019s-piece-of-the-computer-revolution, which says Dave Evans and Harry Huskey were the original principal investigators, and Evans passed direction to Wayne Lichtenberger, a new visiting assistant professor. Graham is most likely referring to Lichtenberger, since Evans and Huskey were clearly accomplished individuals.
[16] Martin Deutsch (1917 –2002).
[17] L Peter Deutsch (1946–).
[18] B.L. Peuto. Comparative study of real estate law and protection systems. Ph.D. thesis, University of California, Berkeley, 1974.

Graham: Control Data. So that was a very nice, big, large machine with an interesting architecture because it had embedded within it eight [actually 10] small machines for doing all the input/output. That was along the route of what do you embed in what. So they embedded the electronics in order to embed the applications. It was a developmental step along the way. And they gave Berkeley a price break, and that proposal had been written up not by Abe Taub and not by me at all. It was by people that wanted to do the time sharing at Berkeley, and nobody was giving them all the terminals hooked into any computer that would accept them. So I got involved in that part [building a multiplexor to connect terminals to the time-shared Control Data computer]. We made it that whatever we built would use one of the eight processors to handle the electronics of the system. So we can put out whatever we wanted on the telephone wires to the terminals and build the terminals and the software to work a mechanical typewriter and get that information from typewriter in to high speed electronics inside the 6600, except this was a smaller thing that the 6600. It was the 6400, and that's the kind of machine that they were giving a good price on and cooperating, except it needed an additional memory, big memory.[19] That interleaved nicely with its big high speed memory, but they had forgotten to include the price for that in the bid. So they had this big machine coming that would have been fine if it had the extra memory, so there was additional arrangement for the university to dig up the money for the memory, and that made the whole thing feasible. My vague recollection is they got the money by mortgaging the parking structure at UCLA. So anyhow, it went ahead under those conditions.

Peuto: Okay.

Graham: Very peculiar proposal in how it got organized. How it didn't actually get started as it should have, and that was also when there was a visitor at Berkeley from Cambridge. I keep forgetting names these days.

McJones: David Wheeler?

Graham: David Wheeler; he was interested in this and our work and I discussed things with him. It was particularly on electronics circuits and the problem of grounding things and a big rack at high speed, and he said, you should look out for this. So I looked out for that and did it just the way he suggested and the whole thing worked really quite well.

Peuto: So to summarize, you were adding some nice information on the multiplexer on the time sharing system.

Graham: Yes.

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[19] Graham is referring to ECS, or Extended Core Storage. The Central Processor could not execute programs residing in ECS, but could perform high-speed block transfers between ECS and main memory.

Peuto: To come back to the role of the Computer History Museum, do you have any opinion or you don't have any?

Graham: Well, I mentioned in the IEEE write up, I didn't go into some detail. It turned that they put Mel Pirtle in charge and he wasn't really appointed yet as a professor, even as an assistant professor, and I remember being at his qualifying exam, I think, for candidacy, I think on the route to being a professor and I had some question that I asked him about what would you do if you were planning to do this and it turns out that that wouldn't work and the system then wouldn't work, what would you do. And I was expecting an answer about what technology you would look at to see if that would take care of it or not. And his answer was, "I would punt." Well, it was with that group of people who were working on it left Berkeley who were graduate students or acting assistant professors. No, Butler Lampson was an assistant professor. He was coming up for tenure. And the discussion about should Butler Lampson be appointed to tenure or not, one of the faculty members in this area said that they didn't know if Butler Lampson was really very bright or just talked very fast and I was proposing that he should be promoted to professor, which didn't get done and he was one of the group that left and went to Xerox PARC [Palo Alto Research Center]. No, they didn't go to Xerox PARC.[20] They formed their own company.

Peuto: Yes. Berkeley Computer Corporation.

Graham: Yes, and they had problems in both management and ability in handling the technology.

Peuto: Okay. My last two questions are very general. Let me mention both of them together in some sense, although you may treat them differently. What are the things you are most proud of in terms of technical work you've done and if you were to summarize your career, to date obviously, what would you say about these guiding lights or whatever? So those are very open ended questions, but they try to deal with things you feel good about from both achievement and career.

Graham: Okay, let me add one piece to this last thing about the network. Butler Lampson was involved. And one of the problems that had been coming up when a number of terminals are competing for a single resource, you have a priority system and usually it's they take care of the last request, and then after you finish that you keep the list of the requests that come in and do the next one and the next one and the next one. But that doesn't always work out nicely from the user's viewpoint, because they can get stuck in a queue that stays long. So to make it more equitable and [provide] some feeling of hope, you try not to do that kind of a sequence. But you have a problem of what happens when a lot of people ask for something at the same time and you can't resolve which one to do. What you can do is turn everything down for so long that people know requests aren't being handled right away, and then they can request it again and this time instead of you making the decision and waiting only a certain time to get the requests,

[20] Actually, Butler Lampson and several other people from this group did go to Xerox PARC after the Berkeley Computer Corporation failed.

you make the time longer-- oh shoot, I forget now. He changed the time. So the likelihood of finding at least one request that meets your criteria to handle it right now, you handle. And that was Butler Lampson's contribution, and that was one that just got used after that whenever that kind of a problem came up.

Peuto: That's interesting.

Graham: The reason I bring it up, I was thinking if you ask Butler Lampson that question, would he say that's the thing he's most proud of, probably not, because he did a lot of other very good things. But coming back to me, the question is what criteria makes me proud. Well, one criteria I mentioned is you meet a student 10 or 20 years later, they recognize you. They remember they had a course with you and what they learned and tell you that it made a difference to them in either what they're able to do and are doing or in inducing them to look at something else of being of interest to them. The last time this happened to me, I had just gotten up to Blank Pointe (???) in the Cuban (???) barber shop and I had sat down in the chair, and somebody who was sitting in the chairs waiting said, "Aren't you Marty Graham?" He had been a graduate student 10 years earlier. It turns out Selma knew his aunt and relationships like that out of the blue are extraordinarily satisfying. So that one, meeting a student that you feel you made a difference to, and there were quite a few because in a number of cases there was a student who wasn't being treated fairly and I would stand up and argue for them, usually against almost all the other faculty members. And there were some cases when they didn't want to do what I thought was right and I told them I was going to finance the student's litigation against the university. And the attorney that I've had for many years arranges to call the department, tell them what I said. Often they say they can't give me the papers that I want to see because it's in some student's handwriting. So the attorney calls them and they have to transcribe all the comments from handwriting to typed form and give it to me. So being really nasty on a number of occasions, I feel very good about them, because I feel I helped get justice done.

Peuto: Okay.

Graham: And in these days, that's very unusual you can manage it. There was one time when we had a remedial course for a sophomore level course, because people who transferred in from the junior colleges didn't have the equivalent course that was given at Berkeley in the sophomore year. So they had a class where they could either self-study or take a class that would cover what they missed, and one year they decided they were going to discontinue doing that. I thought that was outrageous and the chairman didn't want to do that, and I told him I was going to finance litigation against their decision and then he changed his mind. And it's interesting, I run into him reasonably often now and I think we both feel good about what was done and how it came out because it was the right thing to do.

Peuto: Okay.

Graham: And he just needed a little extra push. So that's sort of in the same category as doing a good teaching job, if teaching is training people, giving them information that they don't always get, but is useful to them in their whole lives. Then in the technical ends, the architecture of the Rice machine I think is something to be quite proud of because there's so much innovative and at variance with what was being done at the time, and in the long run, pieces of it have become generally used and appreciated. Maybe 20 years ago I would feel that way. I don't feel it's one of the best and most important things that I've done. The idea of questioning the assumptions you're given at the beginning and telling people about that is one of the best things that I do, but it's not limited to the technical end.

Peuto: And we were not limited in those questions to the technical end.

Graham: The one about realizing that the Gaussian noise is one kind of noise that I can describe, but not a noise I can tell you what the wave form is at any time, that's really very profound. And as I say, that maybe one day I'll run across an article where a guy discusses this in 1965 and no one picks it up, but that hasn't happened. So as of now, that's one of the most exciting things to me.

Peuto: Okay.

Graham: The other one has to do with the health business and the noise getting into people. Electrical noise gets into people and it changes the way their body functions. And so in that one, I think I'm the first person that realized that it's high frequency noise getting into the body that causes trouble, not the 60 cycles that's doing it and it's because the high frequencies can couple in capacitively and magnetically in a way that 60 cycles does not. Now, that's a result of my teaching electromagnetic theory once 65 years or so ago and learning it well enough to feel that I can look at the equations and say something. But realizing that, and that it gets into people and working out a meter that measures that band on the wires and knowing that it gets capacitively coupled into the people because they're close to the wires with those voltages. I'm very proud of that and developing the meter. And the meter is interesting because normally you like to use an RMS meter, true RMS. If you have a number of frequencies present and you want to know the total power, well there are no inexpensive true RMS electrician meters that go to 100 kilohertz and I did measuring, not the RMS current, but the average current of the derivative. And RMS is peculiar because if you have two frequencies that are different and the amplitudes of what they're putting in are equal, the true RMS is 40% higher. So you have a 40% error, which is way too much if you're measuring 120 volts, but if you're measuring a different effect and different people respond in different ways with a 40% difference, you're not losing a tremendous amount. So the fact that it was a very inexpensive way of getting close enough to the overall effect and knowing what the frequency range was that I think is a great contribution. And that, there's a patent on the meter and the filter works with the inductance in the wiring in the house and make the two stage filter that gets it down and there are thousands of people that have benefited from it and essentially nothing published by me except one or two papers.

Peuto: Okay.

Graham: Mostly to the patent office.

Peuto: Yes.

Graham: But at any rate, I told you the fellow I've been working with on this, we started in working on dairy cows. And we went through a number of stages that didn't work right. I thought if I wanted to get the effect of the number of pulses, I should just measure the amplitude and it would be the same amplitude for the number, so I wouldn't know the number. So I built a meter that way. It sounded like a good idea, but it gave the same reading if you had only one pulse or 50 in the transient and the effect was drastically different, so going to the rectified average and so on. It's sort of like what's missing. And the question is how wrong can you be and still know a good answer, and that's also a question that's generally not raised and not taught. You get the answer to this question as I stated it to you and you get the same answer I got or you're wrong.

Peuto: Okay. Okay. Marty, it's 4:10, so we are stopping I guess tape three and by the way, there are other things that we will do but we need to regroup and figure …

Graham: You want to talk some more after you've looked at it?

Peuto: Yes, exactly. I need to prepare. I mean, basically …

Graham: Yes, no it seems to me a lot of this went way off and used a lot of time that neither of us expected would be talked about that much.

Peuto: Yes, well that's good.

Graham: But it's worthwhile.

Peuto: That's good because that is kind of showing it's interesting. By the way, we are going to have a computer club meeting …

[recording ends abruptly]

END OF INTERVIEW