An interview with
GENE GOLUB

Conducted by Thomas Haigh

On
22 and 23 October, 2005

Stanford, California

Interview conducted by the Society for Industrial and Applied Mathematics, as part of grant # DE-FG02-01ER25547 awarded by the US Department of Energy.

Transcript and original tapes donated to the Computer History Museum by the Society for Industrial and Applied Mathematics

ABSTRACT

Gene Golub, a numerical analyst and Fletcher Jones Professor of Computer Science at Stanford University, discusses his career to date. Born in Chicago in 1932, Golub attended several colleges before graduating from the University of Illinois with a bachelors degree in mathematics in 1953. He stayed on for graduate study, receiving a Ph.D. in 1959 under the direction of Abe Taub. Golub discusses his involvement with the ILLIAC computer and its creators and users, including John Nash, Franz Hohn, David Wheeler and Bill Gear. He describes work on statistical applications with C.I. Rao and Charles Wrigley. In 1959, Golub left for Cambridge, England on a postdoctoral fellowship with Maurice Wilkes' EDSAC II group, where he shared an office with William Kahan and collaborated by mail with R.S. Varga on his first major paper. After returning to the USA, Golub worked briefly at UC Berkeley's Lawrence Radiation Laboratory and at Space Technology Laboratories, before accepting a visiting position in the Computer Science Division of Stanford University. After a one year appointment at the Courant Institute (on the invitation of Eugene Isaacson) during 1965-6 he returned to Stanford as Associate Professor and remains a faculty member there. Golub outlines the evolution of numerical analysis within the computer science department at Stanford, including its early history with George Forsythe and Jack Herriot and his later role with Joseph Keller and Joe Oliger in creating the Scientific Computing and Computational Mathematics program, which Golub directed from 1988 to 1998. He discusses his students, including Margaret Wright, Michael Overton and Eric Grosse. Golub reviews his extensive list of publications and collaborations, identifying those he considers most significant and explaining their origins and impact. Among these are his papers with Kahan, Christian Reinsch and Peter Businger on singular value decomposition, with Jim Wilkinson on the application of iterative refinement to least squares problems, Victor Pereyra on differentiation of pseudo-inverses and non-linear least squares problems, and Charles Van Loan on total least squares and the book "Matrix Computations." Golub explains in some detail the origins of his well known work on direct methods for Poisson's equation with Bill Buzbee and Clair Nielson, including discussion of his related work on the conjugate gradient method with Paul Concus, and of the roles of Roger Hockney and Oscar Buneman in triggering this research. Golub also summarizes his work with Carl De Boor and with DL Boley on inverse eigenvalue calculations, and with a range of collaborators on Gauss quadratrure rules. Golub has been very active within the academic community, and explains his long involvement with the Householder meeting series and participation in ACM SIGNUM, SIAM, and the national academies of sciences and of engineering. He has been most active within SIAM, where he was editor of its SIAM Classics series, the founding editor of its journals on Scientific and Statistical Computing and on Matrix Analysis and Applications, and a founder of the affiliated International Council for Industrial and Applied Mathematics (ICIAM).

HAIGH: Thank you very much for agreeing to take part in the interview. You could begin by talking in general terms about your family background and early upbringing.

GOLUB: I was born in Chicago in 1932. My parents were both immigrants, and they had arrived in the United States in 1923 independently of one another. My mother came from a very small town in Latvia, and my father came from Ukraine, a place called Zhitomir. I actually visited there this summer. I had always wanted to be there, and I had an opportunity to go. It's about 100 kilometers from Kiev. At any rate, they were not well educated. They were the classical immigrants of the nineteenth and early twentieth century. They came by ship, as everybody did at that time, and not a very high-class one. Then both of them went to Chicago. My father, because he had a brother in Chicago; my mother, because she had a sister in Chicago. If you look on the genealogy websites of Ellis Island, you can actually see the ship they came in and exactly where they went to. Interestingly enough, you could see that they lived within a very short distance of one another when they came to the United States. At any rate, they married in the United States. I don't know precisely how they met. They had two children, my older brother and myself. So I was born in 1932.

My father, through the years that I can remember, sold bread. Just as you have a milkman, he was the bread man. He would get up early in the morning and collect lots of bread from the bakery and then distribute it either to homes or to mom-and-pop shops. His day was somewhat different—he didn't work on Saturday, this was a Jewish bakery. My mother went to work when I was about five years old and going to kindergarten. In fact, I went to kindergarten just a half-year earlier than necessary because my mother wanted to go to work. She worked in making boys' caps. That was a big industry in Chicago then. I think that's all been outsourced by now. So I'm sort of amused when I see people wearing baseball caps. She had spent a good part of her life sewing the interiors of baseball caps.

At any rate, I grew up in Chicago. Chicago has lots of ethnic neighborhoods. As it turns out, I grew up in a Czech neighborhood, which was contiguous to a Jewish neighborhood, sort of borderline. So when I went to school, Haugan Elementary School, I had to walk through this Czech neighborhood we lived in, and then through a predominantly Jewish neighborhood. It wasn't always so easy. I was born in 1932. The war broke out in 1942, and Hitler had done his job, so to speak, there was anti-Semitism everywhere and there were some tense times. Many of the people in this neighborhood I lived in were of Eastern European origin and at heart were rather anti-Semitic. Nevertheless, I grew up there. But my friends were always in the other neighborhood, the Jewish neighborhood, which was half a mile away or so.

HAIGH: Did your parents speak English at home?

GOLUB: Not very well. Their tongue was really Yiddish. They were encouraged us to speak English by the schoolteacher because my brother spoke Yiddish when he went to school. So they spoke English, but it was a very low-grade English. When my mother conversed with her sister or with friends, it would be in Yiddish. So I heard a lot of Yiddish. Even today when I hear it, I'm not fluent in it, but I like the sound of it. It has a rather melancholy sound to it.

I went to, as I say, Haugan Elementary School. I don't think I was a particularly good student, but I managed okay. Then when I was 12 years old, I went to Roosevelt Junior High School in Chicago. That was much farther. It was probably a good mile to get there from where I lived. It encouraged me to do a lot of walking. Kids used bicycles very often. The way the streetcars were configured, you'd have to do a lot of walking to get to the streetcar, and then you'd take the streetcar to the stop, and then you have to walk some more to get to the high school. Typically, if you weren't too late, you walked. I guess there's no area whose streets I know better than walking through that area.

I had a group of friends in high school. At first, I didn't do so well because I didn't have eyeglasses. I didn't recognize the fact that my glasses need to be repaired. So I finally got glasses, and then my grades went up significantly. I took a classical set of courses, mainly four courses: one in math, one in French, one in English, and I can't remember the other. It wasn't too demanding, but we met every day.

HAIGH: Did you enjoy all the subjects, or were you drawn more toward the scientific ones?

GOLUB: I enjoyed math. I had facility with manipulations. No, I certainly was not considered an outstanding student. I think I was considered a nerd, but not an especially good student. In my graduation there, I may have been 50th out of 330, so it was not as if I was a stunning individual at the time. The teaching was varied. There were some good teachers and a lot of poor teachers, too. Maybe they faced discipline problems, but I don't think we were especially difficult kids. It was a nice school. It was boys and girls all together; it was coed. I had friends, but almost nobody was interested in science. As I look back, the people I know, have all become accountants or lawyers or businessmen. Nobody has really become an academic of my immediate friends. In fact, not so many people from the school became academics, although there have been some exceptions—there have been some very well known people who've gone to this high school. It's on the north side of Chicago, not west side of Chicago. At that time, the area around Roosevelt High School was a Jewish area. I don't know how well you know Chicago. Do you know it at all, or no?

HAIGH: I know the downtown area and a few of the outlying regions.

GOLUB: Well, this is in the city itself. Most of my friends, their parents were either born in Europe or one of their parents was born, or else they were second generation. Not many people had a long gringo history. That was what America was like at that time. I think the great wave of Jewish immigrant occurred at the beginning of the 20th century.

HAIGH: Did your parents put a great value on you getting an education?

GOLUB: Yes, that was always implicit. I like going to school. And I guess, implicitly, I had understood that that's the way to succeed. My parents didn't have much money, and I thought this would be a good way, somehow, to rise up the economic ladder. My first aspirations were to be something like a teacher. I wanted to be a grammar school teacher in grammar school, a high school teacher in high school—my aspirations grew as I went along. But I certainly had no idea of what was out there in terms of university life, and it was quite different.

I can't say I bonded with any of the teachers. There were some teachers I liked more than others. A few years ago back here at Stanford, I noticed at a supermarket a young girl who had the same last name as one of my teachers. Upon talking, we discovered that she was the niece of my teacher, and this was a wonderful teacher. She tried to expand the vision of young people, and she was just a well-educated, intelligent teacher. So that was the only connection. Most often, I don't look back at these teachers, when I can remember them, with particular fondness at all.

HAIGH: Did you have any particular interests outside school as a teenager?

GOLUB: No, I worked. I sold shoes, I worked in the pharmacy. I collected stamps. I began to get interested in music, although I never followed it up. One way to get to a concert or to the theater was to usher or check coats, so I would do that, actually, several times a year. That way, I saw lots of the plays, like "Death of a Salesman" and "Carousel" and so forth that had become well-known American theatrical productions. So that was the only entertainment. Of course, I loved radio like all kids did at that time, and movies. Movies were 12 cents, and Saturday afternoon we went off to the movies very often. I'm confusing various periods because there's a younger period, a preteen age period; then as you become a teenager, you hang out with your friends and do some things. I don't think anybody had a car, or maybe one or two people had cars. So it was a relatively simple kind of life.

HAIGH: Roosevelt was a junior high school?

GOLUB: Yes. Well, it ended up as a four-year high school. So I went for eight years to Haugan, the grammar school, or we called it grammar school. Then after that, I graduated from Haugan and I went on to Roosevelt and I went there for four years.

HAIGH: As you approached the end of your high school career, what kinds of options were you considering?

GOLUB: I wanted to go on to college. I mean, that was understood. But going very far from home was not an option.

HAIGH: For economic reasons?

GOLUB: Yes. It was rich kids who went away to school, stuff like that. The two alternatives at that time would have been Wright Junior College, which was a community college, or the University of Chicago at Navy Pier. But Wright was cheaper, and I went to Wright Junior College. I went there for two years. As I say, it's a community college. But the teaching was really very good. Excellent teaching. The other students were outstanding. I mean, they were pretty good. I still have some friends from that period of my life. Again, while I was in school, I was still working, often in the shoe store. My brother had become a shoe salesman, so I was a shoe salesman. Saturdays and Mondays I had work, and that provided me with some money. So I went to these places. Wright Junior College. Then after two years, I graduate and people scattered all again. I went to the University of Chicago, and that was tough on me because I really wasn't well enough prepared for going there. And secondly, it was a long trip.

HAIGH: Was that in Hyde Park?

GOLUB: Yes. We lived on the north side, so that was really a very long distance. It would take an hour and half to get there and an hour and a half to get back. By that time, my mother had to move. The housing situation was very bad at that time, so we moved into a smaller flat. I didn't have my own room. It wasn't easy to study at all.

HAIGH: So you did two years at Wright Junior College and then a semester at the University of Chicago.

GOLUB: One year, I think.

HAIGH: During this period, was your interest in science and mathematics developing?

GOLUB: Yes, it was at Wright Junior College. I thought of all sorts of things that I might have wanted to be, like a chemist, and then even a political scientist because I like politics a lot. But ultimately, the thing I enjoyed doing the most was mathematics or mathematical manipulations, so that's where I stuck. We didn't learn so much math. We learned, essentially, calculus at Wright Junior College in two years.

HAIGH: So after a year at the University of Chicago, you had implied that you were finding the environment there challenging and the commute long. How did you resolve that?

GOLUB: I thought, "Well, now I'll go to the University of Illinois. It's my final year of college; I might as well see what it is like to be away from home." One of my friends had gone to Illinois, so I decided to go down there. Of course, that made me and made my life, so to speak. Everything happened while I was at Illinois, my whole education. So my senior year at Illinois, I could a load of courses. I had to complete the requirements. For instance, I needed a general education requirement of biology, so somehow I ended up in an entomology course, studying insects. Can't remember a thing about what that course was about. Then I took some statistics courses. One of my professors encouraged me to go on in statistics. I didn't have any real knowledge of what was graduate school was all about.

HAIGH: So that year was the first time you really became aware of graduate school as an option.

GOLUB: Yes, but not so serious an option. Well, a mixed option, yes. I didn't know what I was getting into, I suppose! As I was graduating in the spring of 1953, I did make some applications. I had taken a programming course in the spring of 1953, so I thought there may have been places like Aberdeen Proving Grounds or certain laboratories at MIT and so forth. Also, they had a program in mathematical economics that looked very interesting at Carnegie Mellon, and I applied for that. I may have been on some sort of secondary list and got in under those circumstances. But the main thing is there was a man by the name of J.P. Nash, and he was teaching a programming course. This was in 1953. At the end of a semester, he called me aside and I said I'd like to be an assistant in the computing lab.

HAIGH: Would that have been the course Math 385, Digital Computer Programming?

GOLUB: Yes.

HAIGH: Yes, I found some information on the ILLIAC and the uses it was put to in Nash's papers at the Charles Babbage Institute archives. I was able to see some course descriptions and stuff.

GOLUB: Oh, that's very good.

HAIGH: Do you have a recollection of what the course was like? That must have been one of the first courses in computer programming offered anywhere.

GOLUB: We learned some programming, some numerical analysis already, some simple ideas, computer architecture, little things about that, round off error, etc. You know, just some of the things that were necessary to do computing at that time. So that was an important course for me to take. Of course, he played a terrific role in my life in that he offered me this assistantship. I didn't have a job at the time; I had applied for some. And I liked going to school. You know, computing seemed to be fun, so accepted that. I accepted it for as early as July of that year. During the summer, I already began as a research assistant. I forget what I was making, $90 or $100 a month.

HAIGH: So that started in July 1953.

GOLUB: Yes. That began my association with the computing lab at Illinois, really getting involved with it.

HAIGH: What made you decide to do the course in computer programming?

GOLUB: I was looking for something. I didn't like what I perceived as theoretical mathematics. I wanted something more applied. Also, I was working at one of the laboratories at the University of Illinois where I was doing some hand calculations. I thought it would be much better to be able to do it automatically if I possibly could.

HAIGH: When you registered for the course, did you have a clear sense of what a computer was?

GOLUB: Not really. I may have seen it, but I didn't know. All we knew was that it did operations faster than anybody had done before. So this man, Nash, he was head of the programming efforts at Illinois. He was a very nice man and a helpful man. He left after a couple years and he went to Lockheed and he worked there. He rose in the administration of Lockheed and eventually became a Vice President at Lockheed. He and his wife, lived here, in Woodside really. Then he went out to Cape Kennedy for a shoot, and he caught food down his throat, and he died. So he died, I guess, a relatively young age. His widow is still here. They have three children. One son actually works for Stanford, as a matter of fact.

In fact one of the things at Illinois was that professors really seemed to take an interest in their students. There was a man by the name of Franz Hohn, and he was a very nice man. He taught me matrix theory. He was so kind to people; the students just loved him. Unfortunately he's dead now, too. But when I had some spare money and I give some money for a student fellowship, I named it after Nash and Hohn. I thought they were the people that deserved that kind of recognition. They weren't great researchers, although Hohn had done some research, but they were just dedicated to students and they played a good role in my life. So I thought that kind of thing should be honored and recognized.

HAIGH: What was the experience like of programming the ILLIAC machine? Did you write directly in machine language?

GOLUB: Oh, yes. There were plenty of things to be done, and then I wrote a statistical package that was used. It took me a while to catch on to things because we weren't well educated, I would say, about how to do certain calculations. Now things are much easier, but it wasn't really specifically said that you should do this or that. The first program I wrote was for using a certain method that Nash had suggested to me. Well, it's not a very good method. It really has a flaw in this particular method.

HAIGH: Was that Milne's method?

GOLUB: Yes. How did you know that?

HAIGH: I did my research. That was a description of your career someone had written in the NA Digest online newsletter, honoring your 60th birthday in conjunction with a Midwest meeting of computer analysts.[1]

GOLUB: That was Milne's method. I guess we weren't knowledgeable enough at Illinois to realize people were beginning to write papers to show how bad Milne's method was.

HAIGH: That was supplanted by the Runge Kutta method?

GOLUB: Well, they had a Runge Kutta code already at Illinois. There was a big influence of Cambridge at Illinois. In particular, David Wheeler came to Illinois for a couple of years. Do you know David Wheeler?

HAIGH: Of Wilkes, Wheeler, and Gill?

GOLUB: Yes. And he was a very nice guy. He came to Illinois for a couple of years. He had written many of the basic programs for the ILLIAC. That's how I got a lot of my education about programming was to go over his codes. They were beautifully done. He was the inventor of the subroutine, so he was a really outstanding man.

HAIGH: Yes. One of the things that I found interesting looking at the archival materials on the ILLIAC was that, before the machine was even finished, they had a grant to produce software for it. I think from the Office of Naval Research, if I'm remembering right. Anyway, it's in the papers. Then quite soon, they realized the importance of having a library for useable mathematical routines. So I have copies of the listings of the routines there. It's possible that may be the first grant issued specifically to develop a software library.

GOLUB: Oh, I see. Many of those routines, of course, came over from England with David Wheeler. He spent two years at Illinois, and he had developed a whole bunch of good, solid routines.

HAIGH: I have a progress report, here we have Status Report #1, and that was issued in December 1950. [CBI 14.1.4] So I was surprised how early that was. Your routine would have been intended as an addition to the library?

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[1] http://www.netlib.org/na-digest-html/91/v91n27.html

GOLUB: Oh, yes, sure. I wrote a lot of library routines eventually, yes.

HAIGH: Can you talk me through what it would have been like, almost physically, to write a program. Would you start off by writing it down on a piece of paper?

GOLUB: Yes.

HAIGH: Did you have an assembler or would you manually go to a book and look up the numerical code for the operation?

GOLUB: No, there was an assembler. You memorized that L5 was an addition command—you know, you remembered all these different commands. They were very logically structured, so they were 8 bits. Depending on which bits were used, they added or multiplied or divided and so forth. There were as many as two to the eighth, I guess, possible commands. But we only used, I suspect, about 50 of them repeatedly to do different things. You know, doing the usual arithmetic operations plus a lot of housekeeping and testing and so forth.

HAIGH: If you wanted to use a subroutine, how would you do that? If I remember right, the machine used paper tape. So would you have to go and find the paper tape with the subroutine and splice it into your code?

GOLUB: Oh, yes. They had developed very good paper tape duplicators. There was a cabinet with little boxes, and you would pull out a little box and there was a copy of this routine. Then you would just sort of integrate it into your routine through all the devices. There was a device for just duplicating paper tape.

HAIGH: So it looks like each routine had a number, and some kind of code for what type it was. Would there also be documentation that you could pull up to see--

GOLUB: There's a book somewhere that would describe everything that you had. I may have it somewhere at the office or in some box.

HAIGH: Actually, I'm seeing you mentioned here. So this is Progress Report #9 covering January 1, 1954 to June 30, 1954. The report says that you were a half-time research assistant, and it says, "Mr. Golub has been studying the problems associated with factor analysis and is also working on other problems associated particularly with matrix operations. He has programmed Rao's Maximum Likelihood Factor Analysis Method, and has obtained new results, which he will publish. This summer, he presented a paper on tests of significance in factor analysis at the International Psychological Congress in Montreal, and in September will present another at the meeting of the American Psychological Association in New York."

GOLUB: That was very interesting. One of the groups that was very active at Illinois were the psychologists, or psychometricians, and they had a group of first-rate people there. I got to know a man by the name of Charles Wrigley quite well. He was a psychologist. I guess he was on a research appointment. He was from New Zealand originally. Wrigley got his Ph.D. in London under a man by the name of Cyril Burt; he was the originator of the Eleven Plus, to discriminate. He has been subsequently discredited because it appears that some of his data was made up.

HAIGH: The identical twins that no one could find. Was that him?

GOLUB: In this case it was a secretary that no one could find. Anyway, Wrigley worked for him, and he was a very powerful man and intellectual man, although rather sloppy in his presentations. Wrigley had a job in Montreal when he came to North America after he got his Ph.D.. Then he was being looked over at Illinois. When he saw the ILLIAC, he didn't want to do any further numerical computations. I guess he had used an old hand calculator, and he saw this machine that could do all these computations. The guys at Illinois, the psychologists who were there, they knew they needed somebody who had some experience around numerical computing, so they hired him. He was inspirational to me. He was so positive and helpful. He was just a very nice man and invited people to his house—just a terrifically good human being.

So I got in with that factor analysis group, and I learned a lot. It was really important for me subsequently. Nowadays, there's this whole tendency to look at larger and larger data sets. It's interesting. The people who first started this, I think, were the psychologists. They had large sets of data coming out of educational testing and so forth. So they were the ones that first began this whole interest in analyzing large data sets. Factor analysis is the term used for that, and they were very involved. Illinois was a hot center of this because of the people that they had, plus, they had a big Air Force contract. So then that continued. This area of research, of data analysis, has gone on. For instance, electrical engineers had been involved in that. And then finally, I was saying in the last ten years, computer scientists began to realize that they're getting masses of data, and then the question is how to analyze this data and what to do with it. It started me off in an area that has been of great interest to me. Simultaneously, when I was a graduate student, there was a name by the name of C.I. Rao. He's probably one of the world's great statisticians. He developed this technique for canonical factor analysis. He's still alive at Penn State University, and I'm going to actually visit him next summer.

HAIGH: After you punched your program onto the paper tape and found the appropriate libraries, would you sign up for a shift with the machine and wait for your turn to feed the program in?

GOLUB: Yes. Twice a day, there was a period where you would-- there was a blackboard, and they'd put your name on the blackboard for a few minutes. There was an operator there, and the operator would call you in turns. Then you would try your programs. Sometimes it would work immediately, and sometimes you made some little mistake and you recognized that you had to make a change in your program.

HAIGH: I have a colleague who is interested in this question, so I'll ask it: Was there anything that you could tell by listening to the machine about how it was functioning? Did it have any alarms or bells or a noise that would tell you if it was working or not?

GOLUB: Yes. Not alarms or bells. The ILLIAC had 40 cathode ray tubes, and it had a memory of 1,024 words. The first bit of every word was on one tube; the second bit on another tube. Just for the cathode team.

HAIGH: So that's the Williams tube then.

GOLUB: Williams tube, yes. I think they were maybe the first really successful users of the Williams tube memory. So they had this arrangement, but then they took the sound off of one of the tubes, maybe the zeroth bit of each word, and as it changed patterns you could hear things. Sometimes people use that to play music. They do things of that sort. So if you were alert, you could hear sometimes some funny things happen. It wasn't so commonly used. Maybe five percent of the people could use it. But there was some sound coming out of the ILLIAC. Some people began to understand how to use that, yes.

HAIGH: You've talked a little bit about the kind of programming you were doing, but what were you personal reactions to the experience of programming? I know some people described that after they wrote their first program, they just have this feeling of discovering that this is what they want to do with their lives. Other people are good at it, but don't necessarily have that kind of feeling of being really wedded to it.

GOLUB: I enjoyed it a lot, as far as I remember. It would be frustrating, and then after a while, you learn to be careful and to do things. That's all I wanted to do the rest of my life. Although I was always looking for, especially at the earlier part of my career, to be involved in some really large project like an airline project or some other object that you could really get involved with. That never happened, but I always thought that it would be a lot of fun to be able to work on such a project.

HAIGH: Because of the idea that you would be changing something important?

GOLUB: Yes. I like that idea that there could be something really useful that you did for a change. Have you programmed at all?

HAIGH: Yes. Actually I did some programming on Wednesday. I was supposed to be grading papers. I just couldn't face it, so I did some programming for the first time in about a year. I find it immersive in a way that few other activities are. You can easily lose track of time.

GOLUB: That's true. Also, if you work hard enough at it, you can get real results out of it.

HAIGH: Yes. Compared with writing a paper, it's a very tangible thing. It either works or it doesn't. Although I guess mathematics is more like that than history.

So that's then the general experience of using the ILLIAC, your personal experience with programming….

GOLUB: I should say one other thing. It was a rather young staff that they had at Illinois. There may have been academics, half a dozen people. There maybe twenty students. It was a wonderful atmosphere there and a very supportive and friendly atmosphere. People like Nash and the younger people, especially. I was 21, 22, but the faculty were only 25 or 28. And they were really kind to the students who showed any sort of interest at all. So they had lots of parties, lots of heavy drinking. Martinis were the standard, and that sort of knocked the hell out of you. But it was not unusual for people to invite you to their house for dinner or drinks. It was really a nice atmosphere in the place. That's attributed to various people like David Muller, Ted Poppelbaum (he's dead now), Jim Robertson, he was an electrical engineer. And they were very good friends amongst themselves. There was just a great, friendly place. People entertained one another. I don't know what it's like in Milwaukee, it's a bigger city, but in Champagne/Urbana, there isn't so much to do. The social atmosphere was really very nice. There weren't so many people. Now the computing lab is enormous, so there are all kinds of people. It's hard to have one party for everybody.

[Tape 1, Side B]

HAIGH: Did you also have a sense of being part of a community of computer users that would include other installations, that stretched beyond just the ILLIAC group?

GOLUB: Well, there was some communication. The ILLIAC was actually duplicated elsewhere in Iowa, in Australia, to some extent; not always one for one, but close to it. And then there was some trading off of routines. There was a community of users, so to speak. I started to say also before, during this code check period it was really very interesting because you just sat around and talked. Often, you talked to people around one another's problems. That was very useful, too. It was fun to do that. So that had a good effect. It got you interested in other things, and what you might not have been doing. Then of course people were beginning to develop numerical algorithms, and you then you would translate that into programs for your own institution.

HAIGH: So you'd take a method that had been published somewhere.

GOLUB: Oh, yes. By the time the mid-'50s were coming along, lots of algorithms were beginning to be published.

HAIGH: Would you say that this was motivated by the availability of electronic computers?

GOLUB: Oh yes, I think people understood a different kind of paradigm was necessary in this form of computation.

HAIGH: We've talked in general terms about the lab. You had talked about other options that you considered for a career or graduate school, but I don't think you've talked yet about how you did actually come to enroll for the master's degree in mathematical statistics.

GOLUB: I had a professor there that called me aside and said, "Well, maybe you would enjoy working with statistics." You know, it's interesting. I have almost never done that to a student. I tried it on one student now, but have somebody call you aside and say, "Why don't you do this or that?" I always have the feeling Stanford students know what they want to do, but it's not true. [chuckles]

HAIGH: So a professor suggested that you should consider….

GOLUB: Statistics because there were a group of statisticians at Illinois that were quite good.

HAIGH: What made you say yes?

GOLUB: I knew I didn't want to do pure mathematics and I didn't know enough, really, about numerical mathematics. And I like the idea of working with data. I like numbers, so that was fun. You're so flattered that a professor should take this interest and make a suggestion.

HAIGH: Did you continue to work as an assistant in the digital computer lab?

GOLUB: Yes. That was supposed to be my specialty, somebody looking at statistical problems.

HAIGH: Was that degree offered within the mathematics department?

GOLUB: Yes. Statistics at that time was part of the math department.

HAIGH: What were your experiences during the degree?

GOLUB: Oh, I don't know. I really had most of my loyalty to the computing lab.

HAIGH: So you would turn up and do your courses, but your heart was more in the computer lab.

GOLUB: Yes.

HAIGH: What was the state of the art in terms of computerized statistics in those days?

GOLUB: I personally was working on least squares problems and trying to develop generalized routines that could be used for analyzing some kinds of data.

HAIGH: That would be the things like curve fitting and regression analysis, right?

GOLUB: Yes, regression analysis. I didn't do it very well, but I did it.

HAIGH: Now, I know those methods have been around for a while, but I would imagine that trying to do regression analysis with a desk calculator would have been absolutely horrible.

GOLUB: Yes, probably for five, six variables, whereas you could just do it so quickly on a computer.

HAIGH: Did that lead to a great upsurge of interest in those kinds of approaches?

GOLUB: In part. Illinois has a big animal science school where they do the analysis of variance, repeatedly. So those guys were actually interested in doing this nitty gritty sort of statistics. Each computation doesn't require so much work, but to put it all together might take some effort.

HAIGH: Did you have to do a thesis for the Master's degree?

GOLUB: No.

HAIGH: So it's just a year of courses?

GOLUB: Year of courses.

HAIGH: You received that degree in 1954. What were your thoughts on the future at that point?

GOLUB: Just to stay on! [laughs] Remember, the Korean War was on at that time—that was an incentive for staying on at school.

HAIGH: So you got a draft deferment for staying in school.

GOLUB: Yes.

HAIGH: As you progressed through grad school, did it become obvious to you at some point that you wanted to be an academic? Or was that still an open question in your mind?

GOLUB: Well yes, I think it was an open question. Things changed. There were hints that I might get an offer from the University of Illinois, and I just assumed that was going to happen. But I had worked several summers in industry in Los Angeles, and that was sort of fun. It was sort of semi-academic at that time. You had projects to do, but you have some time for research, too, or you could research yourself into the specific areas.

So I got my Ph.D. in 1959, and I applied for some post docs. One was to go to India and work with Professor Rao. Another one, I just applied for. NSF announced that they were going to give out post docs, so I applied for NSF. I just wrote a short paragraph, probably not a very coherent one. But I got it, and that's when I went to Cambridge.

HAIGH: Let's come back to that then after we've talked more about your Ph.D.. After you got your MA in mathematical statistics, you enrolled in a Ph.D. in the mathematics department. Presumably, you continue to be very much involved with the digital computer laboratory.

GOLUB: Yes.

HAIGH: Were there a number of Ph.D. students?

GOLUB: There were a few around. So the idea was that I was going to do continue in statistics, and I was going to write a thesis, possibly, under a man called Bill Madow. So the sequence of events is: the first year, I was a graduate student. Madow was on sabbatical, and C.I. Rao was at Illinois. The next year Rao left and Madow came back, but he came back without his wife. They had been in California on a sabbatical. He was not a well-tempered person. He was a very smart guy, but he was very difficult. Then after the year, he left to go back to California. So this guy, Madow, ended his life here in Northern California, worked for SRI. All right. Then Taub suddenly grabs a hold of me, so I could be his student. So he took me out as a student.

HAIGH: Was Taub head of the lab at that point?

GOLUB: Well, as soon as Nash left. I don't know what year Nash left. Taub became the head. So I was supposed to be Taub's student. He said, "Here, read this," and there was some paper he gave me by von Neuman that hadn't been published yet. So I looked at it. I didn't really see how to go from there, but eventually, that paper plate had an enormous role in what I did. So I started to work on that. He didn't know that field so well himself. He just put me into there. So I was no longer doing statistics, although I was getting my degree in statistics, I was doing numerical analysis. In the meantime, I was subject to a lot of abuse by Taub. He would just yell and scream at me. Did [Bill] Gear talk to you about this at all?

HAIGH: Yes.

GOLUB: I think he liked Gear better. Maybe Gear is a more secure and sophisticated person. I think the weaker you are as a person, the more he hammered are at you. He was really a nasty piece. At any rate, I finished the thesis.

The lucky break I had is I was working on something, and Taub invited a man by the name of R.S. Varga to come through. I told Varga what I had done, and Varga said, “Oh, I’ve done a similar thing. Let’s write a paper together.” So that was sort of mixed feelings. I like the idea of working with somebody like that. I suspect Taub deliberately called him in just to check me out. Then I finished up. Varga came in Feburary and I left at the beginning of April and I went to England.

HAIGH: All right, so that would be 1959.

GOLUB: Yes. It’s now 1959.

HAIGH: Okay. Let’s just cover a few more topics there there. Your dissertation was called, “The Use of Chebyshev Matrix Polynomials in the Iterative Solution of Linear Equations Compared to the Method of Successive Overrelaxation.”

GOLUB: That’s correct.

HAIGH: That had been inspired by a paper from von Neuman?

GOLUB: The Chebyshev part, yes.

HAIGH: So that was your first major topic in numerical analysis, was it?

GOLUB: Yes. I didn’t even realize what I had done. Have you advised many students?

HAIGH: No.

GOLUB: One of the things, at least in the American system, that somehow you’re supposed to guide a student well enough so that the student knows what he’s done. Taub probably didn’t know so much about, or have so much interest in, what I was doing. I had actually done a lot more than I had realized I had done. [laughs] And I realized that it was quite a lot that I had done. I really felt low at the end of my Ph.D., felt that it was just a weak Ph.D.. Well, it’s not one of the breakthrough theses, but it’s a solid piece of work. But he didn’t give me any sense of pleasure or pride in my own work. I thought, I don’t want to be around these mathematicians. That was one of the fundamental things.

HAIGH: I know that there were several other people floating around the place as students and visitors. You had just mentioned Bill Gear.

GOLUB: Yes. We were pretty good friends. I was friendly with him, with his wife (or ex-wife now). Just recently, I saw her. We all were sort of very friendly with one another. In other words, the student life was really very nice now.

HAIGH: I think William Kahan was also there at some point.

GOLUB: He came for a summer, the summer of 1957, because there was talk of Toronto and Illinois building a computer together. He was the fair-haired boy of Toronto. He's a brilliant fellow, so they sent him down to Illinois, and he was there for that summer.

HAIGH: What was he like in those days?

GOLUB: Well, full of himself. You've talked to him already. He doesn't seem to suffer from any sense of modesty. I don't know what to say. I was around him. He's the sort of person that's not bad to be around for a short time, but it's a little hard to take because he's so self-centered. Having a conversation with him is pretty one-way. Maybe you found it differently.

HAIGH: As an interviewer, it's my job to blend into the background anyway.

GOLUB: Yes, sure, but he tends to dominate in those conversations. Well, I'll tell you more about what happened between us in England. I don't know if you knew that.

HAIGH: All right. We're almost into England. Were there any other graduate students of visitors that you haven't mentioned who played an important role or who you would work with later in your career?

GOLUB: There was a man called R.T. Gregory. He was another one of Taub's students, and he died early. He was at Texas for a while, and eventually he went to the University of Tennessee. He was a very nice man. He worked under Taub. I think in some ways Taub killed him, in the sense that he had ulcers afterwards. He died on the operating table, perhaps; I can't remember. And he has a book that's out.

There's another person that not so many people know of but was Taub's brightest student, I always say, and that's Carl Farrington. Carl Farrington was a student, but he had a family and he left and he never got his Ph.D. If you look at the list of people that Taub directed, it was really relatively small. He eventually moved to Berkeley. Originally he came in as director of the computing center, and then when they decided to form a computer science department, they didn't ask him. So he got upset. He had nothing to do with computing after that. All his research was on relativity and whatever it is.

HAIGH: Was he the only person who was available as an adviser then? It sounds as if people would probably not seek him out, based on this description.

GOLUB: That's true. Nash would not really have been competent, because he was not himself. He was sort of like an academic bureaucrat. Taub was actually involved in research, and of course he was a buddy of von Neumann. Originally I was scheduled not to work with him, but to work with somebody in statistics. Farrington was going to work with him, but then Farrington left. I don't know if it was because of the ill feelings between them. He's a very nice guy. I actually went to his son's wedding just a few weeks ago in Berkeley.

HAIGH: I know kept you pulling back from it earlier, so perhaps now you'd like to talk about your post-doctoral experience at Cambridge.

GOLUB: Yes. I'll just reemphasize what a wonderful atmosphere it was at Illinois. Many of the people and I have remained friends. Or even if I don't see some of them, if I come across them, we'll still have involvement. There were a lot of visitors. We had a whole bunch of visitors from Australia because they duplicated a copy of the ILLIAC in Australia. So that was good.

HAIGH: Had work began on ILLIAC 2 while you were there?

GOLUB: Yes, there was talk about it in the background, but I didn't participate. There was another student amongst the people. His name is Roger Farrell, and we've remained very good friends. He's now a retired professor of statistics at Cornell University. Roger was really a statistician. He got his degree under Burkholder at Illinois. So it was just a good place and things were expanding. With respect to new ideas coming out of Illinois in terms of computation, Gregory had programmed the Jacoby method for computing eigenvalues. He wrote an article about that, which alerted a lot of people to the use of the Jacoby method. It was a method that von Naumann and Goldstein had advocated. Then Gregory wrote this little short-page paper showing what accurate it obtained, and that pricked up a lot of ears to the use of this Jacoby method.

But there wasn't a lot of innovation in numerical methods. That's what I'm trying to say. There wasn't enough leadership. I once got into an argument with Taub at Bill Gear's wedding party about the fact that there were no real numerical analysts at Illinois. [laughs] Maybe that's led to his being angry with me, I don't know. Our relationship went up and down, and there were times even after that it was up, but then I think at the end it was down again for various reasons. He died about four or five years ago.

Now what were we going to talk about next?

HAIGH: England. Was going to Cambridge connected with the relationship that you had had with Wheeler while you were in the lab?

GOLUB: A little. Let me just say that this big event in my life was when Varga came. Varga actually said to me that he had similar results to my own, and that we should write a paper together. That was one thing. The other thing is if I had a little more experience or a little more experienced adviser, I could have pulled out more results than I did. Well, maybe everybody has things like that happen.

HAIGH: Right. So you could've published more papers based on your dissertation.

GOLUB: Well, I'll talk about that in a moment, what the next step in the whole thing is. So I went to Cambridge, but I went much later than I anticipated, and I was put into an office with three others.

HAIGH: You mean you arrived later in the academic year than you had expected?

GOLUB: Yes, I arrived around the 1st of April of 1959. I shared an office with Velvel Kahan; Nick Capon, who ended up in Australia, as he was an Australian; and Colin Cryer, who was a South African, and he eventually became a professor in Germany. The four of us were in a little room not much larger than this alcove. And of course, Velvel was the dominating spirit. [laughs]

At first, we were pretty friendly and, because I had met him before, he invited me to his house for dinner and so forth. It was very nice on that basis. But I just couldn't take it after a while. I think the crowning blow came when I was writing a letter and I threw a draft of it into a wastebasket, and then he took it out and he said, "Oh, you can't write that. That's not right," or something like that. So I asked Wilkes, who was head of the laboratory, to be in a different office.

So I really didn't do so much at Cambridge. I had been so browbeaten by Taub, and maybe I'm a little lazy anyway, so I took the time really to enjoy life. I made a lot of friends there. I did a little work; I'll mention what that is in a moment.

HAIGH: Was that your first trip to England?

GOLUB: Yes.

HAIGH: What were your general impressions of Cambridge and of the computer laboratory there?

GOLUB: Oh, it was very nice. It was lovely. The computer laboratory didn't have the same spirit, at least for me, as we had at Illinois. You know, I was sort of like a senior assistant at Illinois—I had status, even though I was student. At Cambridge, I didn't have any status. Of course, there was always Velvel, who was a much smarter guy than I am, and people looked up to him. And I did know Wheeler and I knew a few other people there, but I hardly built on anything there at Cambridge. The one good thing is that Velvel and I went to talks, and we went down to, for instance, National Physical Laboratory. We heard Lanczos speak, and Lanczos gave a talk on the singular value composition. That stuck in my head. I remember that talk quite well, and it plays a role in what I do in the future.

We went over to Oxford and we saw Fox's operation. Velvel had a car; I didn't have a car. Most of the time, I was in Cambridge itself. I made a number of wonderful friends, whom I still am friendly with today. That is, these friends of mine are in London or in Los Alamos, and sometimes I see them often within the year or sometimes I don't. Also, I met a woman there that I was really enamored with. Nothing romantic, but I just took to her. She introduced me to her family, and she was very nice. I was seven years older than she was, she was a new student at Newnham. So then time went by, she married, she got a couple kids, and her husband died. Then I wrote her, but still nothing happened. But then I saw her a few years ago, maybe ten years ago, and we got together and we got married. But then we divorced. She didn't like me or California or my friends, I don't know. Everything was bad. So that didn't work out in the way it was supposed to. So my one and only wife has been somebody that came out of that meeting in Cambridge. But I still have many, many friends out of that period. It was a very nice time for me. I had time on my hands.

Now, in the meantime, Varga was working away on this joint paper of ours.

HAIGH: Where was he based?

GOLUB: At that time he was beginning to move to Cleveland to Case Western Reserve. He sent me a draft. But another paper had shown up by man called Sheldon. This paper had a result in it about the convergence of SOR [Simultaneous Over Relaxation] under certain circumstances. I took that expression, and actually it was even Velvel who urged me to try to simplify it, and I did. I simplified it, and that helped us in the paper that we were writing. Now, Varga is a much better mathematician than I am, but I knew this area we were working in so well because it was part of my thesis. I know all the ins and outs. So together, the paper turned out much better than had I written it, or he written it. I mean it was really a very good piece of work. Some people call it a minor classic. But it was my first real piece of research. It was done in the old days—no Internet or anything, mail went between the two of us, and we got through with a very nice result. That was my main achievement in England, that this paper, the Golub-Varga paper, got completed.

HAIGH: All right. So let me just find a citation for that one then in your voluminous resume, I imagine. Technical reports, books, conference papers. Ah, P-Series. So that would be that first paper there, really. [(with R. S. Varga), Chebyshev semi-iterative methods, successive over-relaxation iterative methods, and second-order Richardson iterative methods, Parts I and II, Numer. Math. 3, 147–168 (1961)].

GOLUB: That's right, and it has two parts to it. Do you have your computer going?

HAIGH: Yes.

GOLUB: Let's try scholar.google.com, now try "Golub Varga." It's probably not a great number of references, and I'll tell you why.

HAIGH: Yes, so it's giving me "results 1-10 of about 790."

GOLUB: Not 790 references.

HAIGH: It's what it says.

GOLUB: No, to this paper, "cited by 247." So that's a good number of citations, I think. This is the same paper. Often you find this, as a matter of fact. Oh no, this is a different paper. That's Fkeund. It's this one. This would be the one then. Seven citations, I see. Isn't that interesting? It should have a lot more, so what's the problem?

HAIGH: That's the same paper with a different name there, this citation there.

GOLUB: That's right. This often happens, of course. See, this has 44. This paper isn't nearly as important as the first one. So what happened here?

HAIGH: I imagine that Google Scholar may be biased towards more recent papers.

GOLUB: No, Varga was simultaneously writing his book. While he was writing the book, he incorporated the paper into his book. Now if you look at the book by Varga, you'll find that he has many references. It forms two chapters in his book. So because it forms two chapters in his book, people often refer to the book rather than to the paper.

Many people think of it as a minor classic. It was really one of the best collaborative efforts I've ever done because I knew that subject so much better than anybody else. You know, when you work on your thesis, there are a lot of byways and paths that you seek, and they don't necessarily come out in the thesis itself. I was able to sort of really have a strong influence on the paper. On the other hand, Varga knew a lot more than I did at the time about certain block methods, but everything worked out, and it came out as a good paper. Although I'm surprised there are only seven references to it.

HAIGH: We should run that on the Web of Science and we might get a better answer there.

GOLUB: Some selected works of my papers are going to be published, and I had to make a list and that was one of the first that I included, so I consider it a good paper.

HAIGH: That was your first published paper. Did that establish you in the field of numerical analysis?

GOLUB: In some ways, yes. I think people thought it was a good paper. It connected two methods that had been suggested and showed their interrelationship. You notice the word "SOR" is in the title there, also. So then I left, and I stayed at Cambridge a year and a quarter. Made a lot of friends. Worked on this paper. Also did Lanczos's lecture, which will turn out to be very important in my life.

HAIGH: Did you have any contact with Maurice Wilkes?

GOLUB: Just a little. He was very friendly. He invited me to have a drink or something of that sort. He's still alive, by the way.

HAIGH: Yes, I've met him.

GOLUB: Just to give you another story for maybe one of the other interviews: Wilkes was teaching a course on elementary numerical analysis, and he was being harassed by Velvel. So Velvel, he sent a polite little note. "Mr. Kahan, for the comments, why don't you come and see me privately?" Do you know this story?

HAIGH: I had heard Kahan's version, but I would be interested to hear yours.

GOLUB: So Kahan went down, and told him, of course, everything that was incorrect. Wilkes was not supposed to have taught that course; somebody else had died. So at the end, Wilkes said, "Well, it's only an elementary course," to which Velvel responded, "There's a difference between an elementary course and a superficial one." It takes fast thinking. [laughs] Is that the way he told it?

HAIGH: He also mentioned, if I recall correctly, that he and Wilkes both told the story to reflect on the character of the other one, so I imagine Wilkes drew a different moral from the same story.

GOLUB: Oh, I see. That's an interesting story. [laughs] So you interviewed Wilkes?

HAIGH: No. I met him, and then a few years later I got him to contribute a small sidebar to an obituary that I was writing.

GOLUB: Who were you writing the obituary on?

HAIGH: I. Bernard Cohen, a Harvard historian of science who was one of the first historians to look at the history of computing.

GOLUB: I ran a meeting on the history of computing. Are you aware of that?

HAIGH: Was that the one where Nash edited the book that was a kind of conference proceedings?

GOLUB: Yes, and Cohen was a speaker there. That's the only time I met him, I guess. So then I was at Cambridge and I came back.

HAIGH: Did they have the EDSAC II in place at that point?

GOLUB: Yes, and I programmed for the EDSAC II.

HAIGH: What was that experience like? Was it a big step up from the ILLIAC?

GOLUB: Yes, it had some nice features, like the subroutines were actually wired into the machine, and it had index registers. You know, it was very clean. Actually, Wheeler had wanted to further improve the ILLIAC. In Illinois they build two computers almost simultaneously: the ORDVAC and the ILLIAC. Because of the funding and so forth I guess, the ILLIAC was built after the ORDVAC. Between the completion of the ORDVAC and the completion of the ILLIAC, David Wheeler came in. For instance, he helped them improve it. Like instead of a 9-bit instruction set, he reduced it to an 8-bit instruction set. He was a very clever man, and he knew what he was doing at that time more than a lot of others. Let's see, what were we talking about?

HAIGH: I had asked what your impressions were of the EDSAC II.

GOLUB: Oh, yes. It was a good machine. Have you spent much time at Cambridge at all?

HAIGH: I've visited there a couple of times, for a few days each, so I've walked around and soaked up the ambience. Actually, I interviewed Roger Needham, who at that point was head of the computer lab. That's been my main contact with the place.

GOLUB: Well, there was a whole bunch of people there, and Roger Needham was one of them. Everyone spoke highly of him at that time, and I guess he eventually became the head. There were people that I knew, like there was a guy who had been at Illinois for a while, Neil Wiseman, and he was now back at the EDSAC. And a few other people were around then.

HAIGH: Was it a much larger operation there than the computer lab at Illinois?

GOLUB: I think so. It had its own way of operating, but it was very nice. Of course, I felt so much more comfortable at Illinois. Mike Powell was a student at that time. He was a diploma student, so I used to see him wandering around at the EDSAC laboratory.

HAIGH: Do you think that there were differences of culture between the two groups? That the Illinois group was more open, or less hierarchical?

GOLUB: Probably so, yes. Everybody was on a first-name basis, I think, with the exception of maybe Taub. But it was all first-name basis.

HAIGH: Did you get the impression that academic culture at Cambridge at that point was different in general from the American academic culture?

GOLUB: No. I probably wasn't aware of what I am aware of now, you know, the whole style. And of course, some people weren't even treated very well; their positions were not very regular. But they had also a lot of, of course, pride because a lot of the computing revolution started there. They had good ideas, so a lot of the software was wired into their computer. And there were a lot of clever people there who knew what they were doing. That was good. And then I met Wilkinson, of course. I met Wilkinson at Illinois briefly, but I saw him in London.

HAIGH: What were your impressions of Wilkinson?

GOLUB: Oh, that's a whole topic in itself. We can go into that later.

HAIGH: All right. So he pops up again later in the story, then.

GOLUB: Oh yes, sure. He's a great influence in my life.

[Tape 2, Side A]

HAIGH: All right, so do you have any other things to say about your time in Cambridge?

GOLUB: No, I'll just talk about this Lanczos lecture. At the end of the Lanczos lecture, I went to the reception and I started talked to Lanczos. I said [very slowly] "Professor Lanczos, I've worked on Chebyshev polynomials." And he had worked on Chebyshev polynomials in his book. So, I had just about said that sentence and maybe one more. A big pair of arms get to my shoulders and pushed me aside. It was Velvel Kahan; he wanted to talk to Lanczos. [laughs]

HAIGH: So that was as far as your conversation got, was it?

GOLUB: Yes, at that time. I met Lanczos subsequently. That was Velvel's way to be.

Now, while I was in Cambridge, I saw J. N. Snyder. Snyder was at Illinois. He was head of the laboratory at the time. He was a physicist, and he had done a lot of computing. He said to me, "Gene, there's a job at Berkeley. It's a nonacademic job, and it's working with a bunch of physicists." And I thought, "Gee, that's great. Get away from these damn mathematicians." So I took a job at Berkeley. I came September 1, 1960. Drove across the country, and I went to work there.

HAIGH: So that was the Lawrence Radiation Laboratory.

GOLUB: Yes. I don't know, they had me doing some work, which I never really took to. I realized I loved numerical analysis. I loved numerical computing. I didn't want to do ray tracing or anything else. Maybe it was a lack of imagination on my part.

HAIGH: So once you got away from the mathematicians, you found yourself missing them.

GOLUB: Yes, some kinds of mathematicians. Well, I used to enjoy the physicists at Illinois—they were nice people, they knew about computing. But at LBL, that's such a high-power place, and the guy that I dealt with was not so easy to…you know, it was a master-slave relationship and getting up at 2:00 in the morning and all that sort of stuff to use the computer.

HAIGH: All right. So this was the kind of place where you had physicists who thought that they were very important and hadn't much appreciation for computing people.

GOLUB: Yes. The one thing that was good there was I met a few people. I was there for five months, and one person I met there was Paul Concus, who is still affiliated with LBL. We talked a lot and we became very friendly. He, his wife, and I have remained good friends. Another person was the guy that was the head of the computing group itself was called Kent Curtis, and he was head, then he went off to NSF where he was head of the computer science division. But he died early; he died of a cancer. But with Paul, I maintained a relationship. Even now, we're very good friends and we've written several papers together, as a matter of fact. He found his niche there, so to speak. There's a disease called "Berkeleyitis"—once you get there, you want to live there. It's a very nice place to be. So he and his wife have remained there all these years with occasional travels.

So, I couldn't take it anymore there.

HAIGH: You arrived there in 1960, so you stayed about five months. So you must've started looking around for something new very soon after arriving.

GOLUB: After about three or four months, yes. I had two possibilities: One was IBM in New York, Yorktown Heights; and the other was with TRW. I had to spend one summer, the summer of 1957, working in Los Angeles for STL or TRW. They kept on interchanging the names. I had worked there, and it was sort of nice. And I knew a lot of people who worked at STL before. The head of that group was a man called Sam Conte, and I called him up, and within no time at all, an offer from STL was generated. I don't think the people at Yorktown Heights ever gave me an offer. I went down and I moved to Los Angeles, and that must've been in January of 1960. So I stayed in Los Angeles…

HAIGH: Wait, let me check that on your resume. The dates you have here are 1960 to '61 at the Lawrence Radiation Labs, and then 1961-'62, the Space Technology Laboratories.

GOLUB: Yes.

HAIGH: So do you think January of 1961 was when you moved jobs?

GOLUB: Yes, January 1961. I forgot that. I went to work there, and there was really a very nice group of people. There I felt, "Now I'm with my people." You know, there are numerical analysts, there are applied mathematicians. The guy who was our boss's boss, a man by the name of E. K. Blum, he was there, and he was very friendly towards me. So I had a lot of nice connections there.

HAIGH: I know STL was involved in building satellites. Was that its main business?

GOLUB: Yes. Tracking satellites, yes.

HAIGH: Were you part of a central computer center there?

GOLUB: I guess so, but they had a separate math and numerical group. They had a special group.

HAIGH: Was that doing research, or was it providing services to the other parts?

GOLUB: It was doing research. It was applied research. I enjoyed that job. It was a nice job. There were good people there. My friend, Carl Farrington, was there. That's one reason that I liked it. At that time, they had consultants from UCLA. You know, people like Peter Henrici were there very often. It was just a nice place to be.

HAIGH: What kind of work were you doing there?

GOLUB: Deriving methods for solving equations associated with satellites and rockets. You know, how the light formed onto a rocket. Plus, I was just maintaining my own sort of research problems, too.

HAIGH: In general, do you like having that kind of connection with actual problems?

GOLUB: Oh yes. Yes, that's been a great pleasure for me.

HAIGH: So would you say then that throughout your career, the topics you've worked on have been motivated by awareness of real-world problems?

GOLUB: Some, and sometimes I think I'm figuring out a technique that will be useful in some general setting without any specific application. And I've been very lucky. I've come to a lot of things of that nature.

HAIGH: How did things progress with that job then?

GOLUB: Oh, it was okay. I lived there for about a year and a half, but I didn't enjoy living in LA. I wanted to be back around a university. The funny thing is they sent me on a recruiting trip, and I went to Case, and Michigan, and a couple other places, and I got three or four job offers myself. Remember, this is 1961 where they're looking for people. Varga was at Case; he had given me an offer when I was there. Had other offers from a couple of other places. Then I wrote Forsythe at Stanford. I didn't hear anything at first, and then I got a call one day from Forsythe. He said, "Don't you read your mail?" What apparently had happened is he sent me a letter offering me a job. Well, when his offer came through, then I decided I'd try going to Stanford. It's a nice place. We're now up to 1962, I guess. I left Illinois in 1959, I had gone off to England, and then in '60 to Berkeley, and then in '61 to Los Angeles. I said, "Well, I'd better stick with this next job for a while." So August 1962 I came up here, and I've been here since then.

HAIGH: Now, this is getting into the territory covered in your 1979 oral history interview. That's code "OH20" from the Charles Babbage Institute with Pamela McCorduck. So you had written to Forsythe. How had you met him originally?

GOLUB: The summer of ’55, I worked at the RAND Corporation. The summer of ’56, I worked in Los Angeles at Thompson Ramo Wooldridge. Then the summer of ’57, I worked at Bell Labs.

HAIGH: That’s interesting. Did the digital computer laboratory not have funding to keep you busy during the summer?

GOLUB: No, I just thought I wanted to go elsewhere. The first couple of years I stayed around at Illinois, and then I thought, “You know, I should really get more experience.” Here I was, a young guy. It’s nice to travel around, and so I did that. I’ve always enjoyed going somewhere else and seeing stuff.

HAIGH: Did any of the experiences you had in those different places have any particular influence on your later career, or any relationships you made that were important?

GOLUB: Well, that’s a good thing. The RAND Corporation was very exciting, and I met a lot of people there. People were very distinguished. For instance, my former neighbor here was Ken Arrow, who’s a Nobel Prize laureate. I met George Dantzig, Richard Bellman. I mean, it was studded with people who are really eminent in applied mathematics. All those places in the 1950s just brought in people who later turned out to be at the very top notch. They probably don’t remember me, but as a young guy, I sort of remembered who they were.

HAIGH: Were you visiting as part of Paul Armer’s numerical analysis team?

GOLUB: Yes. He was my boss. Do you have any connection with him now? Do you know where he is now? North Carolina, or…?

HAIGH: I think he’s living in this area. I met him a couple of years ago at a reception at the Charles Babbage Foundation. I know he was rather influential within computing circles in the 1950s and ‘60s, although I’m not sure he did important technical work personally. But he was well known through his position as head of the RAND group.

GOLUB: Yes. That was 1955. He was there, and he had all kinds of people around him. I guess it was a booming area. All these places have subsequently sort of died out. STL got rid of their mathematical group. Mathematicians have not had a good time anywhere, including IBM. They still have a group, but it’s pretty low-key now. They used to have really outstanding people. At any rate, the people that I was fortunate to meet were just superb. When you went to the RAND Corporation, the quality of the people there was just great. I wasn’t even smart enough to know that some of the people were really important people. There was a guy by name of [Alexander M] Mood I saw at the Rand Corporation. We used a textbook by Mood in our course. I asked somebody to introduce me, but I didn’t have anything to say to him. I just wanted to meet him, I guess.

HAIGH: Did you say you had also been at Bell Labs?

GOLUB: Then I spent a summer at Bell Labs. I did simulations there. I, more or less, got to know what Bell Labs was like, and I had a good time. I lived in Chatham, New Jersey and I worked at Whippany, but the computers were at Murray Hill, so I had to go back and forth. I was working on a simulation problem. So I learned a bit about simulation, and about how the place works. I don't think I met many people who played much of a role in my life then. I may have met Hamming, but he was such an iconoclast. [Gene, do you mean inconoclast, or just "icon"] I don't think he ever remembered who I was again. There's a colleague here by the name of E. J. McClusky who just retired from our faculty. He's a friend of mine, Ed McClusky. I met him. Otherwise, I don't think I met so many people. And Phyllis Fox, of course, I must've met briefly there, too.

HAIGH: She wouldn't have been at Bell Labs in those days.

GOLUB: Okay. But later, then.

HAIGH: Joe Traub was there at some point.

GOLUB: No, but not then. This is 1957. Subsequently, they had a very active numerical analysis group made up of Stanford people. For instance, Margaret Wright was there. She was one of our students. Eric Grosse and Bill Korn, Peter Businger, they were all students here that I knew. But in 1957, nothing like that was true.

HAIGH: This experience of visiting these high-profile centers of computing research as a graduate student, did that push you further in the direction of wanting a career as a researcher when you saw what these places were like?

GOLUB: I think I wanted to be one! I think they pushed me in the direction of wanting to be an academic!

HAIGH: We've digressed somewhat. I had asked how you knew Forsythe.

GOLUB: So I met Forsythe during the summers that I was in Los Angeles. He was already a very well established figure. He came to Stanford in 1957. Next year, for the year 2007, I'm planning to organize a meeting called "50 Years of Numerical Analysis at Stanford" to celebrate his coming. Also it'll be my 75th birthday, so we'll merge those two events together. I want to just say a few other things, too. While I was working on my thesis—and this is what I talk to my students about sometimes—I worked out a little idea about solving tridiagonal systems of equations. That technique doesn't actually appear in the thesis, I just used that idea to sort of explore something. Later on, that idea reappears when I start doing work on cyclic reduction. So the idea of cyclic reduction really occurred to me while I was working on my thesis, and then it came back. A research student here at Stanford was asking for some help on a problem. His name was Roger Hockney. When Hockney was here, then we actually made it into a big thing for a while.

HAIGH: During this period, you had published that first important paper that you've discussed already. Now, had you been presenting work at numerical analysis conferences? Were you part of that community during this period of the early '60s?

GOLUB: There were meetings called the "Householder meetings" today, but they were called Gatlinberg meetings. I was invited to them at an early stage, I guess through the efforts of Varga. Even in 1962, the year I left for Stanford, I went to a Householder meeting. The Householder meeting is a meeting by invitation only, so I was invited at that early age. I guess Varga was one of the organizers, so he was very nice to bring me in.

HAIGH: Do you remember that first meeting as when you first got to know a number of the important people in the area?

GOLUB: Yes. I knew many of them anyway, I guess, to some extent. I was so excited that I could hardly sleep when I went to these meetings because all these heroes of yours were at the meeting. At that meeting…was it that one, or maybe the previous one. I think I went to the previous one. And I met a man by the name of Germund Dahlquist there. He was one of the great numerical analysts of our time. He died about a year ago. At any rate, I met him. And then as luck would have it, he was invited to UCLA by Henrici. So Dahlquist was at Los Angeles with his wife and children, but he didn't know how to drive. I was single, so on three or four occasions I took them out, and we became friendly with one another. He was a lifetime friend, and it was a privilege to know him.

HAIGH: That appears to bring us up to your arrival at Stanford, which would probably be a natural place to conclude the first session.

GOLUB: Yes.

HAIGH: Do you have anything else that's come to mind as we've talked that you want to say about your career prior to that arrival at Stanford in 1962?

GOLUB: No, I was just not really well developed as a person. I was trying to figure out what I wanted to do. I'm not a person who can easily figure out the future and everything. I knew I liked certain things. I knew I liked living in California. I wanted to be someplace, and I wanted to be somebody. I really had the ambition to be somebody, so maybe I gave up too much for that.

HAIGH: All right. Let's break there then and pick up after lunch.

<u>Beginning of Session 2, on the afternoon of the 22nd of October, 2005</u>

HAIGH: I think we had talked through, quite thoroughly, developments in your career to 1962, when you arrived at Stanford University, initially as a visiting assistant professor.

GOLUB: That's correct.

HAIGH: How did things proceed when you arrived?

GOLUB: I arrived, and actually, I didn't stay very long because I immediately went off to a couple other meetings. I really returned at the end of September from this. So the first month I was essentially traveling around. Then I was ready, so to speak, for activity after I had returned. Since I was on this sort of temporary assignment here, acting visiting professor, I didn't teach so much. The first year I taught an elementary programming course, then the second year I taught a course in advance numerical analysis.

HAIGH: So in the end, you were there from 1962 to 1964. Was this appointment for a fixed period of two years?

GOLUB: I believe so, but I can't really remember. Probably in some sense, I didn't even pay much attention to all the fine points about my appointment.

HAIGH: How did you find Stanford at this point compared with your earlier experiences at Illinois and Cambridge?

GOLUB: Well, it was really nice. I was happy to be in academic environment. I was a little ill at ease because I was on my own. The computer science division was in a building on one side of the campus. We had not yet been moved to the building that we lived in for so long, Polya Hall, so there was a couple months where we just sat in I think it Sequoia Hall, and I was part of a big three-person office, which had Seymour Parter and Ben Rosen. They're both numerical analysts and they both were there on a temporary assignment. Parter was at Stanford for a year at least, and then he went to the University of Wisconsin. He's now retired. The other person who was here was J. Ben Rosen, and he was here for a few years. Then he went from here to the University of Wisconsin also. Then from Wisconsin, he left to go to the University of Minnesota, where he founded the computer science department. He's now retired.

HAIGH: The computer science division, was that a division of the mathematics department?

GOLUB: Yes, it was part of the math department. But after a while, Forsythe agitated to have our own department because he wanted to make some appointments, which the math department would not go along with, like John McCarthy, who was quite a well-known person, but the math department was not interested in having him as a faculty member. So that's why Forsythe moved the department to its own department in Humanities and Science.

HAIGH: Obviously, that separation from applied mathematics was in general an important dynamic in the early days of computer science, and evolution has really continued over the decades since then. As someone on the mathematical side of things, how have you seen this?

GOLUB: Forsythe said that we were the queer people in mathematics, and now we're the queer people in computer science. When the department first began, there were six people, three of whom were numerical analysts. Now the department has well over 30 people, and only two people could be considered numerical analysts. So we really have been put aside in terms of the main thrust of the department.

HAIGH: Has numerical analysis retained a place in the graduate and undergraduate curriculum?

GOLUB: Well, we have undergraduate courses. A new program has been put together called CME, the Computational Math and Engineering Program. The courses that were formally in computer science are now cross-listed between CME and computer science.

HAIGH: I'll ask you about that program a little bit later then.

GOLUB: Yes, sure.

HAIGH: What kind of relationship, during this initial two years in Stanford, did you develop with your colleagues? Who were, other than Forsythe, the main people in place at that point?

GOLUB: Not much. I knew some of the people in statistics. They were friendly. In the computer science department, there was another numerical analyst, Professor Herriot. He's dead now, but he was also very enthusiastic about numerical mathematics. Although his research and mine had very little in common.

HAIGH: Were the courses that you taught at Stanford then the first courses that you had taught?

GOLUB: Really. I had taught a night course at UCLA when I lived in Los Angeles. But otherwise, I had not taught much. So that was really good. But the matrix course that I taught, the first time I taught it, was magnificent because many of those people have gone on to have good reputations. It was an outstanding class with many really excellent people in it, some of whom I've kept close contact with.

HAIGH: How did you find the experience of teaching in general? Was it something you found that you enjoyed?

GOLUB: Up to a point, yes. But it was a lot of work, and I learned a lot. That's the usual experience when you teach a course for the first time.

HAIGH: Would you say that you've developed a distinctive style as a teacher, in any way?

GOLUB: I have my own style, I guess. I think it's more or less common to lots of other people, but I have my way of teaching.

HAIGH: Which would be what?

GOLUB: Oh, I don't know. Just lecturing, you know, classically and enthusiastically.

HAIGH: During this period in 1962 to 1964, were you continuing with research along the same trajectory that….

GOLUB: More or less, but I hadn't really found my feet yet. I was just beginning and working on little problems, but I hadn't done anything very significant by then, no. Other than this paper with Varga, there are several other papers that get published. In 1965, I published a paper on solving least squares problems. That paper had a great influence on a lot of people, even though it in some sense was well-known before. For instance, Lawson and Hanson referred to that very often. It's the paper, "P6." [Numerical methods for solving linear least squares problems, Numer. Math. 7, 206–216 (1965)]. P7 is the program. [(with P. Businger), Linear least squares solutions by Householder transformations, Numer. Math. 7, 269–276 (1965)]. Those two programs, in a sense, are of great importance in solving a least squares problem. So I laid out the procedure for solving linear least squares problems.

HAIGH: So P7 is a publication on a program that you had done.

GOLUB: Yes. Peter Businger was my co-author. That's a graduate student here. He actually laid out the ALGOL program for doing that.

HAIGH: As the main focus of the project that I'm doing the interview for is on software, this would be an opportune moment to ask about your personal involvement with the production of mathematical software. Because I know some people have put a great deal of effort into making code that's robust and portable and documented and those other things. So have you done…?

GOLUB: No, I've never really done that. When I was a graduate student at Illinois, I did a lot of programming. But in the last 40 years, I've done almost none except to use email. No, I generally have a co-author who does the programming.

HAIGH: Why is that?

GOLUB: I used to love to program, but it takes up a lot of time, and it's easier if I do other things, I guess. Just a lack of time, I guess.

HAIGH: So the division has tended to be that you would focus on the mathematical methods and collaborated with the formal implementation?

GOLUB: Yes.

HAIGH: Do you have anything else to say about those first two years at Stanford during your visiting appointment?

GOLUB: They were quite pleasant. When I moved from where we were before to Polya Hall, it was a modest building—the type of the building you might say is temporary, but has lasted at least 40 years. I had a nice office, and there was a nice spirit. There were people who I had a lot of common with. For instance, Roger Hockney was a student at that time. He was actually a research associate for Professor Buneman, and we developed techniques for solving Poisson equations. He later went on and he became a professor at Reading University. He died a couple years ago.

HAIGH: In 1965-'66, you were an adjunct assistant professor at the Courant Institute of Mathematical Sciences at NYU. How did that happen?

GOLUB: At a meeting in Czechoslovakia, Professor [Eugene] Isaacson at NYU, he said, "Oh, you want to come for a visit for a year?" So I did. It was a nice place. I taught a course, I met some people. I sort of proofread part of the book that Isaacson and Keller were writing [E. Isaacson, H.B. Keller, Analysis of Numerical Methods, Wiley, New York, 1966]. That was okay. And of course, it's very exciting to live in Manhattan.

HAIGH: What kind of activities were underway at the Courant Institute at that time?

GOLUB: I can hardly remember, afraid to say. Also, it was while I was there the following happened: I was interested in computing Gauss quadrature rules. I knew if you compute the eigenvalues of a certain tridiagonal matrix, that that would lead to the nodes, and then I wondered how to get the weights connected with the Gauss quadrature rules. Well, I was at meeting in Peter Lax's office, and it was very boring to me, so I happened to pick up a book on a side, and I saw some statement about Gauss quadrature rule weights. I opened it up, and then the penny dropped, and I was able to figure out how to compute the weights of a Gauss quadrature rule.

HAIGH: Did that lead to a publication?

GOLUB: Yes. That's the publication with Welsch in 1969, "Calculation of Gauss Quadrature Rules." [(with J. H. Welsch), Calculation of Gauss quadrature rules, Math. Comp. 23, 221–230 (1969)]. That's had a fair amount of impact, though. It's a very simple observation, but it seems to be very useful. That's what I love—taking some simple idea and showing how you can use it.

HAIGH: When you left Stanford to go to New York for the year, did you have the idea that you would be coming back again? Or did you not know?

GOLUB: Oh, yes, sure I did. Although while I was in New York, I was hoping they would invite me to stay there permanently, but they never did. The sort of thing I did was too pragmatic for the people there. I don't think they like that sort of work.

HAIGH: So then in 1966, you returned to Stanford. Was this now on a regular tenure-track appointment?

GOLUB: Yes.

HAIGH: And you've been here ever since.

GOLUB: That's right. And I was promoted, you know, associate professor, and then finally a full professor. But I'm not sure what the dates were on any of these.

HAIGH: That would be on the front page of the resume. Although what I will try and do, if you don't mind, is just paste the whole thing in as an appendix to the transcript, and then it'll be easy for people to check dates and citations as they read it. Yes, I'm seeing in 1966 as an associate professor in the computer science department; full professor from 1970 onwards. All right. Were you tenured then immediately on your return? Because it says "Associate Professor, 1966."

GOLUB: I can't remember. There was a time at Stanford that you could be an associate professor but not be tenured. It's common now, but there was a time when that was possible.

HAIGH: And then a full professor since 1970.

GOLUB: Yes. That was when I was 38 years old, I guess. Ten years after I had my Ph.D.

HAIGH: Was that seen as a relatively young age?

GOLUB: I don't think so. I think about standard.

HAIGH: Did your role within the department stay pretty much the same over the years?

GOLUB: In what way? How do you mean by "role"? You mean administratively?

HAIGH: It's a fairly open-ended question. I would think in terms of things like relations with colleagues, general kinds of participation, the kinds of areas that you were researching. It's really your own sense of your time there. Do you see it as very much a continuation, or do you see it mentally divided up into distinct chapters?

GOLUB: No, things happen on a continuum for me, I would say.

HAIGH: So what kinds of things did change over time, even if only gradually?

GOLUB: Well, I had some responsibilities. The department really formed in 1966, so therefore, as the department came into play, there were different obligations that one had. One thing that was difficult was the Vietnam War was coming into existence in the late ‘60s, or it was coming into people’s perception of it. Then our students were very concerned about it, and there were meetings with the faculty and a lot of discussion about what was happening. And then there was some radical action on the campus, too. Some professors led marches and so forth, so it created quite an unusual atmosphere.

HAIGH: Would you say that the character of the department changed a lot during the 1970s and ‘80s?

GOLUB: Possibly because they hired some new young people, sort of less traditional in their behavior that they didn’t conform to the usual academic style of the time, yes.

HAIGH: I would think of the growth of the computer industry around Silicon Valley over that period. Did that have any impact on your own career or on the character of the department as a whole?

GOLUB: I’m sorry, I actually faded out there for a minute.

HAIGH: My impression is back in the ‘60s, this area was not thought of particularly as a hub of the computer industry. Then with the rise of the semiconductor industry, software companies, and then internet businesses, Silicon Valley has come to be viewed as the center of computing matters. So I was wondering, did that have an influence on your own career or on the priorities of the department as a whole?

GOLUB: No, not on my career. I think on the priorities of the University. But the University came to the realization that there was something happening out there fairly late. When I was a chairman of the department, I don’t think the University was aware of the fact that Cisco was developed outside of Stanford. They hadn’t kept any record of it or anything. I would say as late as 1982, the University was just beginning to become aware of the things that were going on. Although Sun had come into being before that. Some of the people who played a role in Sun came out of our department.

Tape 2, Side B. Session 2 continues.

GOLUB: It is amazing how many companies are a direct outgrowth of Stanford and computer science in particular. I’m sure I only know a few of those companies. But in modern times, such companies as Google and Sun and a lot of other companies that you can name. But I think there a lot of the companies I can’t even name that had their startup with Stanford.

HAIGH: But you personally never were involved very much?

GOLUB: No. Well, except with respect to Google. There was a little involvement there.

HAIGH: That’s right. I saw some publications on the PageRank algorithm.

GOLUB: That's right. A student asked me for some help, and I wrote down something. They don't even use these results for Google, they had their own algorithms, which work quite well. But there may come some future date where they'll find it of use, or somebody else will. In the meantime, through various connections, I ended up with some Google stock.

HAIGH: Did you find that the development of computing in the region had an effect on where your students would be coming from or where they might go to work after graduation?

GOLUB: Well, there are my students and there are the department students. A lot of the department students came with the intention, at one point, of starting up companies, getting connections with startup connections. You know, being involved in this tremendous growth that was taking place here in Silicon Valley. That may have been around in the 1990s. Before that, I think things just happened, so to speak. But after around 1990, or possibly earlier, there was sort of a conscientious effort to emulate all the possibilities.

HAIGH: Was that a factor for your own graduate students? Or were those companies not just very interested in mathematicians?

GOLUB: Yes, they're not. Although these people had degrees in computer science, they could have done a lot of that. Around 1980, we had a splendid group of students. They all ended up with strong academic careers.

HAIGH: According to the online tree, your first graduate student was Richard Bartels, graduating in 1968.

GOLUB: That's correct.

HAIGH: Do you have anything to say about your own approach to advising graduate students?

GOLUB: No, not really. I don't feel I've been overly successful as an adviser. Richard was here, and I was looking at some things to do with linear program. I asked him to look at the numerical properties of the algorithm, and he did a very nice job. He wrote a thesis that I think is well acknowledged. So he got his degree here, and then he went on. He was from Stanford, he was at Texas for a while, he was at Johns Hopkins for a while. He ended up his career mainly at the University of Waterloo. Now for family reasons, he's retired in British Columbia near Vancouver.

HAIGH: Why do you say that you weren't particularly successful?

GOLUB: I don't know. I just felt other advisers, somehow they give students very specific problems to work on. I've been, more or less, sort of not very direct with the students. With Richard and a couple other students I've given them specific topics to work on, but other students have had to find the topics that they want to work on.

HAIGH: So you hadn't been one of those advisers with big ongoing projects where you've had a little piece to get to each?

GOLUB: No, no. I haven't had a lot of big ongoing projects for myself.

HAIGH: All right. We have the list here of your students over the years. If there are any…

GOLUB: Just a wonderful list of students, and they've accomplished a great deal. I'm all pleased to see how well they've done, but I think they did it on their own, more or less. Sometimes they got some more advice from other people. I guess I provided an atmosphere where they could work and be around. Richard Bartels, I gave him a specific topic. Richard Brent, he's probably the most brilliant of all these students, and now he's found his own topic. He's now a professor in Australia. He has a very specialized chair. Michael Saunders is here at Stanford. If you want to know a list of what's happened to any of these students, I don't even know myself, though. Michael's a professor now, MSE program. John Palmer was an entrepreneur. I don't know what's really happened to him. Richard Underwood, he's dead. Dianne O'Leary, she was a student here and now she's a professor at the University of Maryland, and this weekend, she's getting an honorary degree from the University of Waterloo. John Lewis, he works for, I think, Cray in Seattle. Margaret Wright is head of Computer Science at NYU. Michael Heath, he's at the University of Illinois; he has a chair. Franklin Luk, he's at RPI. He was chair of the department, but he's no longer doing that. I don't think it's quite clear what he's going to be doing. Michael Overton is doing extremely well. He's at the Courant Institute. Petter Bjorstad he's back in Norway, as a professor. He had some companies, which he sold. Dan Boley was a student here and then went on to the University of Minnesota. He's a professor there, and he's done nice work. Eric Grosse was a student here. Now he's head of computer science at Bell Labs. Stephen Nash, I think he's an associate dean at George Mason. Mark Kent, he lives in this area. I think he has a little company. Ray Tuminaro got his degree and now works at Livermore Sandia and has done extremely well. He's a very well known figure. Hongyuan Zha is a professor at Penn State. Oliver Ernst; he's in Germany. He was a very smart guy, but somehow he hasn't gotten a chair or a professorship. Xiaowei Zhan. I can't remember what happened to him. Tong Zhang, an extremely able student. Went to work for IBM and has just gone to work for Yahoo. Nhat Nguyen retired into private industry. Yong Sun lives in Beijing and has started a company. Urmi Holz worked for, I believe, the NSA. And James Lambers, he's still at Stanford, but he's a research associate for another group. So it's a very nice group of students. So a bunch of these people have done very well and are professors. Some of them have gone into private industry or done other things. There is a list on the Web of all these students and then how many students they've had.

HAIGH: Yes, I have that on my screen here.

GOLUB: Oh, okay. So you can see there are a few people like Richard Brent and Dianne O'Leary who've had really a lot of students. The others haven't had so many students at all.

HAIGH: Unless you have other thoughts in general about your work as an adviser or teacher, then we can follow through your own research career.

GOLUB: OK, fine.

HAIGH: When we reach the point where you start producing textbooks and edited volumes we can talk about those as well.

So I think we talked through the papers that you published through about 1965. How did things progress in terms of your research through the second half of the 1960s and into the '70s?

GOLUB: Let me repeat one comment. I was at a meeting where Ben Rosen gave a talk, so that must've been '63, say. At the end of the talk, Forsythe got up and he said, "Would somebody please figure out how to compute the pseudo inverse of the matrix?" And then I remembered that lecture of Lanczos, you know, remembered that could be done. So then I started to work on the singular value decomposition in some sort of simple ways. I asked Peter Businger to do some experiments for me, and he did. They looked a little funny, but they inspired me to think about what kind of form you can reduce a general rectangular matrix to. Then I went away and I kept on playing around with this. In the summer, I went to work at Boeing as a consultant, and I stayed with Peter Henrici. We just talked, and we just stayed together, and I went to Boeing. Suddenly it hit me how to do this computation and how to decompose a matrix in order to compute its singular values. I was really excited; I could hardly contain myself.

That fall, I went to a meeting in Madison, Wisconsin and I kept my mouth shut most of the time. But then I sat next to Velvel Kahan and I said, "Look, I have this terrific way of doing such and such." He said, "Oh, yes, I know about it. Let's write a paper together." Well already Velvel had established a reputation of not having written many papers, so I was sort of nervous that we'd ever get anything done together. But we had enough money to invite him to come to Stanford, and he came to my apartment. Not here, but I lived elsewhere at the time. Then we worked on it together, and in no time at all, we had this paper written. That was fairly well received. But later on, in collaboration with Reinsch, we published a program to do the computation. The two of the those things really put us on the map, so to speak.

HAIGH: All right. So then the first of those would be the paper that's coded as "P8" on your résumé, "Calculating the Singular Values and Pseudo Inverse of a Matrix." [[P8] (with W. Kahan), Calculating the singular values and pseudo-inverse of a matrix, SIAM J. Numer. Anal. 2, Ser. B., 205–224 (1965)].

GOLUB: Yes, that was only a part of the real research. That is, we hadn't computed it by that time.

HAIGH: So that was SIAM Journal of Numerical Analysis in 1965. You had mentioned that you were working on Businger on that, so did that give rise to this paper, "P19"? [(with P. A. Businger), Singular value decomposition of a complex matrix, Algorithm 358, Comm. ACM 12, 564–565 (1969)].

GOLUB: Yes, in a way; but if you notice, it's a complex matrix. So that's a program, actually. And then we figured out to simplify the calculation and not work with complex arithmetic. So you have a complex matrix, and you reduce the problem to a simpler problem.

HAIGH: Is there another paper then with Kahan that had the final version of that research?

GOLUB: No, that's the only paper with Kahan.

HAIGH: You said that that put you on the map.

GOLUB: Well, people thought it was a good piece of work, I guess.

HAIGH: What did that mean in practical terms for your career? Were you being invited to give more talks at better places, giving keynotes? Serve on committees? What did it mean?

GOLUB: Probably so. Oh, I don't know. Just maybe more visible. [laughs] I can't be very specific.

I feel that was one of the best pieces of work that I did at that time. You see, the whole idea is you take a rectangular matrix, and by doing some transformations, you make it into a bidiagonal matrix. Then the question is: how do you compute the singular values of a biadiagonal matrix. I had figured out how to do that, and I described it in a paper. The next thing that happened is Christian Reinsch sent me a pre-print of a joint paper where he was using these ideas that I mentioned. So there was that paper. He also discussed some work that he had done on splines where he needed to get a certain parameter. That required a certain calculation, and I knew how to do that calculation. I simplified what he wrote and could show that it's really the solution of a singular value problem. So that paragraph that we added in was important in itself. Let me say it's a paper on the computation of the SVD with Reinsch.

HAIGH: Would this be P20 there? [(with C. Reinsch), Singular value decomposition and least squares solutions, Numer. Math. 14, 403–420 (1970)].

GOLUB: Yes. The mathematics for that had more or less been developed. He introduced a paragraph on some least squares problem, and I simplified that calculation. Later on, in writing the book with Charlie Van Loan, we put in this new calculation, and we call it "TLS", total least squares. So it was Charlie who thought of the idea of total least squares. That paper is the first one of my papers which describes total least squares.

HAIGH: But I'm seeing another paper here published in 1966 with Jim Wilkinson. I had asked you about Wilkinson earlier, and you said that that story belonged a bit later. So that was "Note on the Iterative Refinement of Least Square Solution" published in Numerical Mathematics in 1966. [(with J. H. Wilkinson), Note on the iterative refinement of least squares solution, Numer. Math. 9, 139–148 (1966)].

GOLUB: Yes. There was something called "the handbook" that appeared in the journal, <u>Numerische Mathematik</u>. The handbook had some very good routines. It's all of the handbook that EISPACK and LINPACK all developed. So I talked about iterative refine for least squares problems, and I had Peter Businger do some calculations, and the calculations were weird and it didn't converge in the way that I anticipated. So then Wilkinson came in, and he looked at it, and he realized quickly that there had been a problem in the way that we had formulated the problem. So he changed the problem somewhat, and then he was able to show under what conditions you might be able to do iterative refinement for least squares problems. But for a while, it had sort of a bad connotation to do iterative refinement for least squares problems. It wasn't until Åke Björck did his thesis that it was really figured out how to iterative refinement for least squares problems. In that paper with Wilkinson we showed that you just don't get very good results if you do iterative refinement on normal equations.

HAIGH: What were your general impressions of Wilkinson?

GOLUB: He was a wonderful man. He was brilliant. He was articulate, intelligent. He had a great love for life. He enjoyed music and drinking and women. He just was full of good conversation, good will. He tried to be helpful to people. He just was a very, very nice human being. He had a lot of wit. In my case, at least, he helped me tremendously, you know, being supportive. People enjoyed his company. He liked to drink. He was just full of life, I would say. A very special person.

HAIGH: I think you had mentioned earlier that in the early 1960s when you first arrived at Stanford, you were very ambitious, but that you still hadn't really settled on the way in which you wanted to apply this ambition. Would you say that during the late '60s, with the series of papers, that you really gained more of a focus or a direction?

GOLUB: Yes. Well, I began to see what I could do. I don't know, but somehow the papers just flowed. I just would write a paper, and then maybe somebody would talk to me about a secondary idea, and in collaboration, we would do something new. If you notice, almost all of my papers are joint efforts. So it's not as if I had the drive to work on one specific problem over and over again. I like working on a specific problem, but then people often would approach me and say, "Gene, have you thought about this?" and then we'll work together and get somewhere else, get somewhere further.

HAIGH: It also seems as if your approach to your career has been quite socially oriented in the sense that you moved around a lot, and you cultivated a large network of people. Even in this interview, whenever I mention anyone from 40 years ago, you know what they're doing now or saw them last week. So would you say that that kind of sociable style has translated into doing all these jointly authored papers?

GOLUB: Oh the networking, yes. And a lot of people sort of think of it as some kind of bonus to be connected with me and have my name on their paper. Sometimes I feel guilty and think, "Well, have I done enough?" I'll say, "Do you really want to put my name on the paper?" Inevitably, they say, "Yes, sure, you belong." You know, "You give us the idea," or "You did this or that or something other." There are different levels of involvement that I've had on different papers.

HAIGH: I'm also seeing here a series of four papers with Bartels. Would that be material that was from his thesis?

GOLUB: No. These are problems similar to what he did in his thesis, and the same technology can be used. There are sort of extensions of his thesis to other problems, and they have some slight variation of that, yes.

HAIGH: How was your general participation in the numerical analysis community developing over the same period of the late 1960s? You mentioned that you went to your first Gatlinburg conference in 1962, and I know that from 1972 onwards that you were a member of the organizing committee. So did you become a fixture at these conferences?

GOLUB: After a while, yes, and then I ran a couple of the meetings, too. Then I became involved with SIAM and I started going to SIAM meetings.

HAIGH: Do you recall when you first would have gone to a SIAM meeting?

GOLUB: No, not at all.

HAIGH: All right. I'll ask you about a few other things first then. How did those conferences develop? 1962 must have been pretty early in the sequence. I know that the Householder meetings have been kept quite small and exclusive.

GOLUB: Oh, yes. There's been a constant struggle about that, you know. People like F. L. Bauer wanted to keep them relatively small. The times that I've run them, I've always sort of expanded it to 100, 120, and some people have been furious. G. W. Stewart, if you get him in the subject, he's never wanted to have a very big Householder meeting at all.

HAIGH: So they got bigger or smaller depending on who's been doing them then.

GOLUB: That's right. Last time I think there were 120 people at the Householder meeting in Pennsylvania.

HAIGH: As that series of meetings has progressed, would you say that they've stayed pretty much the same in format and character, or have there been noticeable developments over time?

GOLUB: Well, there's much more emphasis on young people coming in than there was in the past. Or else, there aren't so many old people left around.

HAIGH: Why do you think this series of meetings has been so significant and so long running?

GOLUB: I think people assume that they're important. The young people all want to go to these meetings, and then they sort of try to get up there. They find the right audience, they think, for their research. I don't know. It's just very standard. It's interesting what you're saying. It's become more and more important because all the people in linear algebra try to go there, you know.

HAIGH: Right. It's the same reason that if anything is desirable, then it has a mystique, it has a history, that attraction would feed upon itself.

GOLUB: And some of the best-known people have never gone to the meeting. It's just by accident. There's a professor at Yale by the name of Martin Schultz, and he had a lot of influence in the '70s and '80s. But for some reason he never came to a Householder meeting. Now he's not even being invited because he doesn't do so much work. But in the old days, he was on the list all the time.

HAIGH: So you were on the organizing committee from 1972 to 1990. It's right here. You mentioned that your personal feeling was that they should be larger rather than smaller.

GOLUB: Yes, sure.

HAIGH: Do you remember any other controversies or important matters that the organizing committee had to deal with over the period?

GOLUB: No, the size has been the main one. Pete Stewart, he's always for having it small and elitist.

HAIGH: Were there ever any questions about breadth of coverage? About whether some area should…

GOLUB: No. Because once you invite people, they can talk about whatever they want. Maybe it's self-fulfilling: You tend to invite the people whom you know what they're going to talk about. The whole original idea was to make it very informal, but over the years it's got more and more formal. You know, who's going to speak when, and who stays with what room and so forth.

HAIGH: Now, several of the people involved with mathematical software have discussed their participation in these conferences. So I was wondering, did the production of software packages ever surface as something that people would be presenting on or discussing there? Or did the focus stay very much on the mathematical methods themselves?

GOLUB: I would say 85% has been on the mathematical methods. Then of course, there will be something like the BLAS. So they'll introduce the idea of the BLAS, and there will be a subgroup of people who come out and start talking about the BLAS. There have been some innovations in software. Like Cleve Moler, he just gave a talk about parallel MATLAB at the last meeting.

HAIGH: So it sounds then that one of the distinctive things would be that rather than selecting what would be presenting there according to an official theme or an abstract or a paper, that you're just picking people, and then once the people are picked, that they can present on matters that they find interesting.

GOLUB: Sure. Once a person is selected, and even if they said they were going to talk about X, they could talk on Y.

HAIGH: Is that unusual? Are there other meetings that follow that approach?

GOLUB: I don't know. Some places, you really contracted to speak on a specific subject. But I think many people feel they can just change, talk about whatever they want at the Householder meeting.

HAIGH: All right. Looking as well at your general involvement, there are just some clues from your resume where you list your various editorial roles. So I'm seeing that from 1967 to '69 you were an associate editor of <u>Mathematics of Computation</u>; from '68 onwards, of <u>Numerische Mathematik</u>; and from 1969 to 1980 of the <u>Journal of Computer and System Sciences</u>. Did appearing in these editorial boards kind of represent your acceptance as a major figure in these areas?

GOLUB: Oh, I think so. Yes, sure.

HAIGH: Would those particular journals, did being associate editor involve you in setting policy for them?

GOLUB: It should've, but it really didn't. The editor-in-chief and Board of Directors decide what to do.

HAIGH: So the associate editors would just be reviewing papers, mostly?

GOLUB: Mm-hmm [yes].

HAIGH: Are there any of them where you remember having an involvement with the publication that would go beyond just reviewing papers?

GOLUB: No. At Numerische Mathematik, they seldom call us together. Once or twice, we all met together, but really, they don't do that. It makes me skeptical of their organization.

End of Session 1.

Beginning of Session 2, early afternoon of Sunday, October 23, 2005.

HAIGH: Continuing where we reached yesterday, you were discussing your involvement as an editor, associate editor, or editorial board member of various publications beginning in the late 1960s.

GOLUB: So I've acted as editor for a number of journals, and for most of those journal I had behaved in a similar fashion, and that was to get a paper when it was sent to me by the editor-in-chief, and then look over the paper and then send it to a couple of referees—two or sometimes three—and then await their reviews. If they had a unanimous agreement whether to publish or not publish, then I would generally go along with that. But sometimes you would have one referee who would say "publish," the other "don't publish," and then I'd have to either get another referees opinion or decide what to do myself.

I never did something that I should've done, which is actually even consult with the two referees. A colleague of mine, Peter Henrici, once said when he gets such a situation, then he sends Referee A reports of Referee B, and Referee B reports of Referee A and he says, "Listen, you two guys disagree. What's the difference? Why can't you agree on this paper?" Occasionally, I didn't agree with either editor and I did make an independent decision.

Another role I liked to play is if I went to a conference and I heard some person give a talk which I thought was very good, then I would approach this person after the lecture and say, "Why don't you publish it in this or that journal?" So that seemed to work out okay. And it's amazing how often, especially a young person, they're flattered when you come to them and say that you'd like to see their paper in print, and then they'll come and submit their paper to you without any problems. Older people generally have more fixed ideas or they've committed themselves already. I guess it's good to be an editor of a journal, but I must say, in recent years, I've slacked off and not been as conscientious as I should've been.

HAIGH: While we're on the topic of your involvement in professional societies and various roles into the 1970s, I know that you were at some point a member of the ACM SIGNUM, the numerical analysis special interest group, and served on its Board of Directors from 1976 to

1978. So I was wondering what your recollections were of that group and the role that it played within the community.

GOLUB: SIGNUM was an important organization. Although I was associated with it and the Board of Directors, I don't seem to feel that I had much impact on SIGNUM at all. And I don't know why it's not a more visible organization now than it was before. I think SIAM has really been the leading organization in this whole area. The ACM, although initially it was important for numerical computation, maybe the handwriting was on the wall and over the years they've had less and less interest in numerical mathematics.

HAIGH: In the days when it was more active in the late '60s and early '70s, do you have any particular recollections of SIGNUM having done anything which you think had a significant effect on the development of the field?

GOLUB: No. The only thing I can think of is, of course, the TOMS, which was organized originally by John Rice. They were maybe the first serious journal to look at refereeing of algorithms and really trying to get good algorithms into the literature and available to all.

HAIGH: Why do you think it was that numerical analysis became so marginal within the ACM?

GOLUB: It's just that other areas of computing and computer science just played a more important role or were becoming stronger, and numerical analysis is fairly detail-oriented. You know, you don't have grand schemes for things, whereas certain areas promised a great deal. They promised more than actually gave. One of the good things about numerical analysis is these problems have been around for a long time, and they continually evolve. You know that there's always going to be some problem in numerical analysis that's relevant. There are several reasons for this. One is technology is changing constantly, so an algorithm you design in 1960 and then as time goes by it's discarded then may arise again because of a new computer architecture. This certainly happened with respect to parallel computation, so methods like the Jacobi methods, which was one of the early methods for computing eigenvalues then became of less interest after other methods were devised. And then when vector computers came along, then again, it came into being. Then of course, technology throws up new problems, so we have new sources of problems all the time that are continually coming up. So it's a dynamic subject, numerical analysis: constantly changing, growing, developing, but always perhaps with one foot in the past.

HAIGH: You had alluded a couple of minutes ago to TOMS. The 1970s appeared to have been an important time in the development of mathematical software with events such as the founding of TOMS, the series of mathematical software conferences that took place, and the LINPACK and EISPACK projects. I was wondering, did you have any personal involvement or influence on any of those?

GOLUB: Well, I was on one of the original LINPACK committees, just on a committee that wasn't enthusiastic about it. But personally never did anything. I don't know exactly why. I enjoyed developing algorithms, but the software I generally regarded as a black hole—work can go on endlessly in developing the optimal software. Although, I've contributed to software in certain senses. I've been collaborator on a number of pieces of software that have been extensively used.

HAIGH: Was this committee to oversee the grant received for LINPACK? [Gene – can you verify whether this was LINPACK or EISPACK – you say LINPACK twice, but then in the next paragraph you are talking about EISPACK]

GOLUB: Yes. I recall attending a meeting at Argon, and people were discussing it at that point. EISPACK really became as the initiative of one person, Virginia Klema. She had seen these programs written in Algol in the handbook from Numerische Mathematik, and then she started translated them into Fortran, trying to make them available, and somehow that evolved into EISPACK. So she really started things off in a way.

HAIGH: While we're on the subject of software, have you been aware that any codes produced by you and your collaborators, as you've been working on these methods, have made their way directly into widely-used libraries such as the NAG or IMSL collections?

GOLUB: Oh, I think so. Like the program for computing the SVD was originally based on the work I had done. Maybe I should say a word or two about that. The original paper that Kahan and I had published in SIAM Journal of Numerical Analysis discussed the SVD and the first set of transformations. So what we did is we took a general rectangular matrix and reduced it to bidiagonal form, but we didn't do anything further with that. That is, we never really discussed at length how one goes from the bidiagonal form to the diagonal form. Then later, it occurred to me that one could generalize the QR algorithm in a nice way for computing the singular values from the bidiagonal matrix via the QR method. I presented a little paper to that effect in Czechoslovakia. It was then published in a Czech journal. The paper is seldom referred to because much of the material in that paper eventually ended up in my book.

Tape 3, Side A. Session 3 continues.

GOLUB: The paper is listed as P13, and it's called "Least Squares Singular Values and Matrix Approximations." [Least squares, singular values and matrix approximations, Appl. Mat. 13, 44–51 (1968)].

In that paper, I describe a number of applications of the singular value decomposition, and then I also showed how the QR algorithm was generalized for computing the singular values of a bidiagonal matrix. Then the next thing that happened was that I got a draft of a paper from Reinsch where he had take the algorithm I had outlined, plus, he had embellished it and improved upon it. That ended up as our joint paper, which appeared in Numerische Mathematik, and then there's a program there. Subsequently people had improved upon that algorithm, especially for computing the small singular values to high accuracy, and that was done by some work in Demmel and Kahan.

HAIGH: Is that paper that you just alluded to P20? (with C. Reinsch), Singular value decomposition and least squares solutions, Numer. Math. 14, 403–420 (1970).

GOLUB: Yes. The last one with Reinsch was P20. That was sort of interesting. I don't know, am I digressing too far?

HAIGH: Well, I think you had brought this up in the context of an example of a time when your software had made its way into wide-user and to a library.

GOLUB: So Reinsch improved a version of the singular value decomposition. Then as an example of the SVD, he talked about a least squares problem where the data and matrix and the matrix of observation both have error. He had some way of computing a certain parameter, and then I played around with it a little further. In his calculation, you need to compute two singular value decompositions, and then I showed you could do it with one singular value decomposition. Then later on, Charlie Van Loan and I gave him a simpler derivation of how to compute this parameter. That was the start of total least squares. We didn't call it that, but in a paper with Charles Van Loan, Charlie invited the name "TLS," and that's caught on very well.

HAIGH: So that would be P47 here, would it? [(with C. Van Loan), An analysis of the total least squares problem, SIAM J. Numer. Anal. 17, 6, 883–893 (1980)].

GOLUB: Yes. Shall I talk about my book a bit? [Matrix Computations, Gene H. Golub and Charles Van Loan, Johns Hopkins University Press (1983)]. Or is that premature at this point?

HAIGH: I don't think that's premature if it's following on from these papers that you've already discussed. I do just want to ask you a couple of other questions to give some context.

GOLUB: Okay. But the book plays an important role in my life and career and activity.

HAIGH: Yes, definitely. Now was the least squares topic coming out of your earlier interest in statistical matters?

GOLUB: Yes. That's when I got into the whole least squares problem business. I had seen a paper by Householder on using what we call Householder transformations for solving least squares problems. Actually, the method goes back to the 1930s, I think, to a book by Turnbull and Aiken. H. W. Turnbull and A. C. Aitken, <u>An introduction to the theory of canonical matrices</u>, Blackie, London and Glasgow, 1932]. But it was Householder's paper that stirred up a lot of interest. Householder was a very good scholar, and he knew of this work by Turnbull and Aitken and actually refers to it in his paper. But it's a question of the right paper at the right time, whereas Turnbull and Aitken may have discussed what we call these transformations in the '30s, it wasn't the right time for the computation to be done, whereas Householder brought it to everybody's attention just at the right time. Then I wrote a paper, which further elucidated this technique and showed how it could be used in a statistical calculation, and the benefits of using this method in solving the least squares problems. Least squares problems were just beginning to rear their heads, let's say in the 1960s, and earlier, too, especially in connection with satellite computation. Data fitting became really a more important activity in general, and large data sets.

HAIGH: I understand that applications for the techniques emerged in control systems.

GOLUB: Yes. Also was useful there too, yes.

HAIGH: So would just the idea be that as it's possible to crunch the numbers faster, you can begin to use it in real-time applications?

GOLUB: That's right. Using these methods advocated originally by Householder, let's say, the problem of updating data, you know, introducing more observations or introducing more variables or dropping variables. It became relatively simple to do, whereas if you had normal equations, then the calculations were a little more complicated. Although there were formulas, there were well-known methods, for doing that.

HAIGH: It seems you have three other important papers in this area. So I will leave it to your judgment if you want to discuss those first and then talk about the book, or if you think it makes sense to talk about the book and then return to that.

GOLUB: Well, some of this will involve scholarship and some of it personal details.

HAIGH: I think both would be appropriate.

GOLUB: I don't know how to say this. I love these papers because I was intimately connected with them. So there's a paper with Victor Pereyra [(with V. Pereyra), The differentiation of pseudo-inverses and non-linear least squares problems whose variables separate, SIAM J. Numer. Anal. 10, 413–432 (1973)]. It was variable-separate. Victor's an old friend of mine. He was at Stanford in 1962, and that time we became very friendly. We've continued our friendship and we're currently working on a book together. So that's well over 40 years.

At any rate, we were both at a meeting in Argentina, and we start talking a little. But that meeting stimulated in us, and I think it was after the meeting that we were in correspondence again about a problem that he had already begun to investigate. But what we were able to do is put this onto a rigorous basis. It required the differentiation of the pseudoinverse, which is deeply involved in computing least squares problems, and finding the derivative of a projection matrix. So Victor and I had some nice formulas for doing that. Then I was talking about this when I gave a lecture at Texas. During my lecture, Pete Stewart whipped out a technique for getting the derivative of a projection matrix more easily than what we had done. It wasn't really necessary to differentiate the pseudoinverse.

Now, the derivative of the pseudoinverse was interesting in its own right, and it plays a role in some perturbation theory. We had the form for differentiating the projection matrix, but Pete gave a very simple and elegant proof of how to do this. That became apart of this. And then Victor produced a program for solving this problem.

The idea is you have two sets of variables and you separate your variables out, and you eliminate roughly half the variables and iterate on the non-linear variables. It's the linear variables that you eliminate. It's a simple idea and it's an idea that's occurred very often in the literature. Victor and I sort of set up rigorously, and then a program was produced. Then Linda Kaufman noted later that there was even a simplification of this and showed how to eliminate one of the matrix computations. Nevertheless, the program is based on work that Victor and I and started out with. It has really become very widely used. This will also tell you something. Who uses it? Well, it's people who are in the applications area. But people working in the optimization area pretty well ignored this program and this paper. Sometimes I've done work—and maybe I should feel pleased by this—I've done work which is of more importance in the applications area than it is to the other numerical analysts, or people working in some specialized area. So it's an old story. You can sometimes reach out to other communities and have more effect than when you have connections with your own communities.

HAIGH: Would it be true to say then that as you've been working on these different areas that you've had an active interesting in seeing how the results and the software that is produced have been picked up in application areas?

GOLUB: Oh, I love that. For instance, a young Danish guy wrote me recently from Santa Cruz. He was visiting there for six months. He was interested in a certain problem, and I told him to come by and we would talk about it. He's an economist. And I loved that. You know, numerical analysis is a subject that people use. Sometimes, like every other discipline, we have our own language and our own techniques. There's really a necessity to talk to people in other disciplines. So the more we reach out, I think there's a tremendous reward in doing that to us personally. At least, that's the way I feel about it.

HAIGH: You also selected P25 and P43 for your list of most interesting papers.

GOLUB: Yes, and I'll mention some of those, too. Then there's another paper that I'm very pleased with, and that's with Mike Heath and Grace Wahba. [(with M. Heath and G. Wahba), Generalized cross-validation as a method for choosing a good ridge parameter, Technometrics 21,2 215–223 (1979)]. Grace Wahba's a well-known statistician, she's a member of the National Academy of Sciences, and she was a graduate student here at Stanford. She's a very lively, dynamic person. She came to Zurich to give a talk while I was on sabbatical there. While there, she presented this problem associated with cross-validation. Cross-validation is a way of determining a certain parameter that regularizes the solution of least squares problems, and there's been a big industry associated with that. At any rate, Grace talked to me about this. I have the feeling we were on a train in Switzerland where much of this conversation went forward. Then I later developed things for this technique. Grace and I had a lot of correspondence. Then Michael Heath, who was a graduate student here and is now a chaired professor at the University of Illinois, he did the programming and some further work on this method. We did a simplification of a computation of this parameter, which put it within range of numerical computing.

Now, I continued to work on this problem in some ways. For some reason, aspects of my work often feeds into one another; that is, I work on one topic and another topic, and then I'll figure out Topic B and Topic A have a connection with one another. For many years I continued to think about this and be interested in this. Finally, when I did some work on moments, I realized that a lot of this work with Grace could be connected to these moment ideas. Then I worked out an algorithm, which was a great improvement over what we had done in the past and allows for computing this cross-validation parameter for very large data sets.

Now, the original paper was published in Technometrics. Then later on, the last paper with Urs von Math, where we show how to really simplify the computation, was published in a journal on statistical computation. [(with Urs Von Matt), Quadratically constrained least squares and quadratic problems, Numer. Math. 59, 561–580 (1991)].

In either case, you sort of miss certain audiences. The numerical analysis community, they are interested in methods for computing the ridge parameter, but they seldom look at the stuff that I did because it has a strong statistical flavor to it. It's often very difficult to cross boundary lines. You may develop a technique that you think is very useful in another discipline, and you'll be completely ignored. I found this in many instances. I'm not so arrogant to believe that I hold the unique solution to these problems, but I do think in many instances it could be very useful to look at some of these techniques that I've worked on. Some of them are referred to. In fact, this paper with Grace is referred to 323 times according to this Google citation. Maybe I should check it out again. So, it's not as if the paper is complete ignored, but somehow, like the more recent work with Von Matt, I don't think that's completely used. You know, sometimes you can get too far of your community and you have ideas that they're not familiar to it, and then they don't respond to those.

HAIGH: Let me just follow up on one of the things you mentioned there, your sabbatical in Zurich. I see from the resume that in 1974-75, you were at ETH in Zurich and at the Courant Institute. What made you go to ETH? Was that motivated by its general reputation in numerical analysis?

GOLUB: Yes, and there was a wonderful colleague there by the name of Peter Henrici. Although half my salary came from Stanford, he was able to cover the other half of salary and just made me feel at home there. I think nowadays, I could go somewhere and just say I'd like to come and probably get some response. But in this particular instance, when I was younger, if somebody say, "Oh, Gene, you ought to come on your sabbatical here," I would do so. Henrici was one of the giants of modern numerical analysis. Not that he was so original in his work, but he was a great expositor, and he understood so much. He wasn't as original as, say, Wilkinson, was, but he did terrific work, and he was such an interesting human being. He was really a cultivated European person—loved music, and the fact that when he was a younger man, he had a conflict with it to be a mathematician or musician. He ended up doing up mathematics and being an amateur musician. Henrici's been dead many years. He has a son who actually got a degree from the conservatoire, but is now doing advanced mathematics for his degree. So this love of music and mathematics is not an uncommon phenomenon at all.

There's another paper here in this list of least squares papers with Charles Van Loan, and that's called "Analysis of Total Least Squares Problems," which I've already referred to. As our book was being written—mainly by Charlie, who did most of the writing—he would take topics that I worked and then expand them and improve them. So he began a paper on total least squares, and then I had lots of little tricks and we introduced those, and it simplified the analysis of the problem.

HAIGH: So this was the book, Matrix Computations, which was published in 1983. [Matrix Computations, Gene H. Golub and Charles Van Loan, Johns Hopkins University Press (1983)].

GOLUB: Yes. That book is based on a series of lectures at Johns Hopkins University. Johns Hopkins set up a mathematical sciences department, and the chair of that department was Roger Horn. Roger had been a student at Stanford; in fact, he was in this first class that I taught. He's a very well-organized person. He ended up at Johns Hopkins as creator and director of this new department. As part of the agreement with Johns Hopkins Press, I guess, is that he would organize each summer a summer school, and he had several people in line. Part of the honorarium was to produce a manuscript, a little monograph. In the audience, Charles Van Loan, who had been a student with Cleve Moler, he was there. I'm not very organized in writing up things, and I later asked Charlie if he would be a co-author. I asked Richard Bartels because I wanted to include some things on optimization, but Richard bowed out after a while. Charlie was a younger guy and he's a very energetic person, and he started to work on the book. He took several topics that were in my notes. The book was based on the lectures that I had delivered and on my papers, and then he would sort of start elucidating on these topics and writing them up and putting them into paper form. So during the writing of the book, there were five or six papers that came out. They were really good papers, with the total least squares being one of them. Then, of course, it was part of the book. I think most often now people refer to the book rather than to the paper itself. At any rate, he was a great co-author to have. He's very clever and has a very fast mind. I think the writing of the book and the papers that came out were really the heyday of both of our careers. Lots of good stuff came out of that.

HAIGH: So the book had been intended as something that would be used primarily as a textbook?

GOLUB: Yes. I just based it on notes and lectures I had given. We took several years to produce. I told Johns Hopkins that this was going to be a good book. I didn't know I was confident that bringing this material to the general public would be useful. Finally, we delivered the manuscript; it took several years. In total, the three editions have sold over 50,000 copies. It's been wonderful for me to be connected with this book.

HAIGH: Did you have a sense of the sales were coming from? Would it be an assigned book for a graduate course?

GOLUB: Sometimes, and I think lots of engineers have bought the book and used it just as a reference. There are other books that are simpler to read or better in a literary sense perhaps, but the book contains a lot of material and originally, it was quite cheap. In paperback, it was, like, $25; now I think it's $45 or so. Even so, compared to lots of modern textbooks, it's relatively inexpensive.

HAIGH: And it's had three editions?

GOLUB: Yes.

HAIGH: Where did the motivation come from for the updates? The second edition appeared only six years after the first one.

GOLUB: Well, because during that period, a lot of work had been done on parallel computing, so parallel computing is heavily emphasized in the newer edition.

HAIGH: You said that the publication of the first edition actually gave rise to new papers and new research. Did the later editions involve any new research on your own part?

GOLUB: I think so, but I can't be very explicit right now; I have to think about that. I think that's the benefit of a book sometimes—it promotes ideas in a field, and then people can improve upon ideas. One way of getting a high citation is that people would say, "Well, this work by Golub and Van Loan is not good, and this is how we improve upon it." Sometimes you get high citations in that manner. One of my colleagues here in the computer science department wrote a great book, and he wants to extend it. He's always complaining that there are too many new pieces of work being done. Well, in part it's because of him—he led the field, and that was a great impetus for people to do new things. [Gene – do you have Knuth in mind? If so, can we add his name?]

HAIGH: Is there anything else that you want to say about your work over the years on least squares?

GOLUB: No, it's very satisfying. It's interesting how some ideas, they may take a while to get going. For instance, here's a paper by Björck and myself on angles between subspaces. It was published and it showed how to compute these angles, which is closely connected to what's called canonical correlation in statistics. It takes a life of its own, and then every few years somebody rediscovers it and decides that, "Oh yes, this is a good idea," and they find it to be useful.

HAIGH: So that's P25 on the list. [(with A. Björck), Numerical methods for computing angles between linear subspaces, Math. Comp. 27, 579–594 (1973)].

GOLUB: Yes.

HAIGH: Then perhaps we should turn to the other stream of research on iterative methods in fast solvers. We discussed your very first paper, which you've grouped in that category. Let me see, does Google give us insight into this? 149 citations. So certainly, one of your most widely known papers is the paper on direct methods for Poisson's equation with Buzbee and Nielson. [(with B. L. Buzbee and C. W. Nielson), On direct methods for solving Poisson's equation, SIAM J. Numer. Anal. 7, 627–656 (1970)].

GOLUB: Oh, yes. That has a nice, I consider, a nice history, but perhaps a long one. I came here in '62 and shortly afterwards a researcher came from England by the name of Roger Hockney. He worked between Professor Forsythe and Oscar Buneman. Buneman was a professor in applied physics, and he was a wonderful man. He seemed elderly at the time, but he was full of energy and charm. He's just a lovely person. Hockney worked for him, and one of the problems that he needed to solve was to solve Poisson's equation over a rectangle.

Hockey was assigned the task of developing a numerical algorithm for doing this based on some ideas that Buneman had from his years in the UK during the war. Roger had actually studied towards a Ph.D. in the United States some years earlier, dropped out, and then went back to Britain (he was English). I guess he was looking for a new opportunity, so he took this job with Buneman and decided also to work on a thesis. Hockney happened to say to me one day that he had this tridiagonal matrices that he had to solve very fast. I had an idea that I worked out when I was a graduate student but it never appears in thesis, of what's now called cyclic reduction. So I told him about cyclic reduction, and he understood it, and he modified it and improved upon it. There are certain problems that overflow. Then I told him about other ideas, too. Together, we sort of figured out not only you could do cyclic reduction on a tridiagonal matrix, but on a block tridiagonal matrix. We talked to Buneman about this, too. Buneman wasn't a numerical analyst, but he had a wonderful feel for numerical computation and he loved to do computing. So, we discovered this idea that we had, cyclic reduction on block matrices was not a good numerical algorithm, but if you did one step, it was okay. Roger incorporated that into his codes.

Then this was sort of dropped. Hockney had developed a lot of very interesting programs for solving this problem, and he was really interested in the physical problems, too. Hockney and I had never really been able to figure out how to implement cyclic reduction for block matrices, but Buneman had a very nice idea of how you modify the right-hand side. At any rate, I wasn't even aware of this, but there was a conference at Los Alamos, and Buneman presented his results and he showed the code. He was rather a pixie-ish guy, he a little white beard and ran around a lot. Everybody was really interested and said, "I have this page-and-a-half code that'll solve Poisson's equation on a rectangle," and people were excited. At that same time, although I wasn't at that conference, I was a consultant at Los Alamos, and I came a week after Buneman had been there, and Billy Buzbee said to me people were very excited about Buneman's lecture. Then while I was there, there was a guy called Clair Nielson who came in. He said he would like to use this idea, only he has different boundary conditions. So Buzbee and I started to work on these ideas of Buneman, and tried to write it down and show the mathematical ideas behind it and how it could be extended. Nielson, more or less…there was one difference equation that needed to be solved; he showed us how to do that. Actually, a very nice way of looking at this was shown to me by Alan George. He was a student here, and I showed him the stuff. He worked a nice little way of looking at things. This all appeared in this paper.

At one time, the paper by Buzbee, Nielson, and myself was the most frequently cited paper in the SIAM Journal of Numerical Analysis. It's probably been overshadowed by other papers, but that was really a hot seller, as they say, for a while. Later on, a package called FISHPACK was developed, and that used a lot of these ideas. So it led in several different directions. The fast Poisson solvers were developed in various places, but basically here, a lot of work was done here at Stanford. They were based on ideas that had been around. It was for solving problems on rectangular domains. The immediate question is: suppose your domain isn't rectangular, but it's the union of two rectangles. So we started to look into that. That, and how can you break up the problem into union of rectangles and then solve the problem on each rectangle, and then combine the solution in some way. So we wrote about the T-shaped domain, I'll call it, the simplest version. I think I may have called it domain decomposition, and I did it in a very primitive way. But since then, this whole idea of domain decomposition has taken off. There's a conference every 18 months or so on domain decomposition. I'm no longer a player in that business, but other people like Olof Widlund at the Courant Institute, he's very busily involved in domain decomposition. He's milked it considerably. So Oluf has really generalized it in many different directions, and he has stimulated that whole community.

HAIGH: Did you publish this idea of domain decomposition at the time?

GOLUB: Yes, but it's sort of like in a conference paper, and it's just a little side comment, I think.

HAIGH: Well, for the sake of history, let's just see if you can find that.

GOLUB: It's C38 with David Mayers of Oxford, "The Use of Preconditioning Irregular Regions." There was one generalization that was very important. [(with D. Mayers), The use of pre-conditioning over irregular regions, presented at the INRIA Sixth International Conference on Computing Methods in Applied Sciences and Engineering, Versailles, France, 1983].

Now, the other thing is, suppose that you don't have Poisson's equation, but you have some equation near to it. You split your problem into Poisson's equation plus something else, and then you want to iterate; then the question is how can you iterate and do work as fast as possible. That led to some acceleration techniques, and in particular, I started to work with the conjugate gradient method. So then with Paul Concus at Berkeley, and Dianne O'Leary, who was a student here, we developed ways of solving these problems.

HAIGH: And this would be papers C11 and C14? [(with Paul Concus), A generalized conjugate gradient method for non-symmetric systems of linear equations, presented at the Second International Symposium on Computing Methods in Applied Sciences and Engineering, Versailles, France, December 15–19, 10 pp., 1975] and [(with Paul Concus and Dianne O'Leary), A generalized conjugate gradient method for the numerical solution of elliptic partial differential equations, in Proceedings of Sparse Matrix Conference, Sparse Matrix Computation, Academic Press, Inc., 309–332, 1976].

GOLUB: Yes. C11 is just a little different, but C14 (with Concus and O'Leary) is the one I would cite. Let me back up a bit. The conjugate gradient method was described in 1952 in a paper by Hestenes and Stiefel [MR Hestenes and E. Stiefel, Methods of conjugate gradients for solving linear systems, J. Res. Nat. Bur. Stand., 49 (1952), pp. 409—436]. This method was very exciting for people because it said if you have n equations and n unknowns, then you'll get the solution to the system of equations in at most n steps, if the matrix is symmetric and positive definite. You don't actually need the matrix; you just need to multiply the matrix times a vector. It seemed ideal for sparse matrices. So the method came along in 1952, and it was quickly discovered it didn't really work the way mathematically it was supposed to. Sometimes it fell apart. Sometimes if you had a 100 by 100 equations you would get the solution in 80 iterations; sometimes you took 110 iterations to get the good solution. So people began to be wary of it. It showed you the difference between a numerical method and a mathematical method. So it was discarded. Then as computers got larger, then it became necessary to solve large systems of equations of a certain kind. Some engineers started working with the method again, and they found that it worked. Then a second look was taken at the method.

About the time that we wrote our paper, there were other paper that had similar ideas and using the conjugate gradient method. There's maybe three or four different papers all with the basic idea saying, "Yes, it's okay to use the conjugate gradient method, but you have to use it in this way," and it took off from there. What I like to say to my students, at any rate, is that here we developed fast Poisson solvers, and then we moved into two different directions from there. Two other fields spun off from this idea, that you would use a fast Poisson solver. One would be the use of the conjugate gradient method in a variety of applications. Not precisely the way we stated in this paper, but it was the incentive for working in this style. Secondly, this idea of domain decomposition. That was also connected with the fast Poisson solvers. So you never know where your research is going to lead you. Sometimes out of one paper lots of other papers are generated, and even sub-disciplines are actually generated.

Does my description agree with Buzbee's description?

HAIGH: I think it does, to the best of my recollection. Are you aware of any discrepancy between the way the two of you viewed the matter?

GOLUB: Slightly, but I won't go on. [laughs]

HAIGH: All right. Well, I will leave people to read the two versions side by side and draw their own conclusions.

GOLUB: It's one of many papers I have. It was maybe Buzbee at his peak, and his whole research direction has gone differently than my own. He's been much more interested in hardware or software issues, which I haven't been so much involved with.

HAIGH: What were your impressions of Buzbee at the time that you were working on the paper?

GOLUB: He was a very nice man and a little reserved, but easy to talk to. Just off the record, we meet every few years, but actually it's not that we have bad relations, but we just don't have good relations. We were really close, but then somehow, maybe we just don't have anything to say to one another any longer. But he used to entertain me at his home. I gather that he's no longer married to the same wife and things of that sort. He's somewhat distant, and I don't see him so often. You know, if you see somebody every five years or so. But he's a thoughtful person.

[Tape 3, Side B].

HAIGH: You've discussed P21 and C14 on your greatest hits list. You had mentioned that C11 was something slightly different. [(with Paul Concus), A generalized conjugate gradient method for non-symmetric systems of linear equations, presented at the Second International Symposium on Computing Methods in Applied Sciences and Engineering, Versailles, France, December 15–19, 10 pp., 1975]. Is there anything you want to say about that?

GOLUB: It's a generalization of conjugate gradient method. Two matrices that are symmetric but are positive definite. That work doesn't seem to be so well known, but I like it. So sometimes you have papers that you're fond and you think they contain a nice idea. Later on, some other work was done with the same splitting ideas, so it's affected some people.

HAIGH: In the same section, you have the series of three papers from much later. Would it make sense to talk about those later in the interview, or are they a natural continuation of the things that you've already been talking about?

GOLUB: Well, these are all connected with nonsymmetric systems of equations. The paper with Andy Wathen came about, he was visiting here and he just showed me some preliminary results, and then I observed that you could use these results in a much greater situation. So that paper then became well referenced. [(with Andrew J. Wathen), An Iteration for Indefinite Systems and Its Application to the Navier–Stokes Equations, SIAM Journal on Scientific Computing, 19:2, 530–539, (1998)]. In fact, it's surprising. So you see, it has an impact factor at 1.231, which is about as high an impact factor as any of my papers.

Then these other papers are connected with methods that I've worked on where you have what's called a preconditioner and you don't solve a problem exactly, but only approximately. I know I just feel that they contain some useful ideas that can be applicable for solving nonsymmetric systems of indefinite systems, and these three methods are given here. It's interesting; this last is just called "P." I don't know what happened there. I don't know why it's not having a number after it. [(with Z.-Z. Bai and M.K. Ng), Hermitian and skew-Hermitian splitting methods for non-Hermitian positive de nite linear systems, SIAM J. Matrix Anal. Appl., 2001 [18] 56:162(2004)].

HAIGH: Perhaps you haven't yet assigned that paper a number on your resume when you made this.

GOLUB: That's interesting. It's actually not in this list, so I'll have to check on that one. I guess the best place for looking at a bibliography is-- because you found a list in the papers, yes.

HAIGH: We've talked about the sequence of papers. So that's two areas that you were very active in through the '70s. Would you say that between them, the fast solvers and the work on least squares accounted for the two main thrusts in your research of the '70s?

GOLUB: Yes, I think so. The last paper, the one with Bai and Ng, that was done already in the 21st century.

HAIGH: Yes, so those three papers, and then the new one.

GOLUB: One thing that I think was a reflection on the years. I continued to work on the same problems in one way or another. I know some people like to change every ten years, fields and directions. But I love to return to problems I've worked on in the past and see if there's a new twist to them, so around and around I go, trying to fix them up. In summarizing about my work, though you haven't asked me, I'd like to say that the work of the '60s and '70s, that established the kinds of problems I like to work on more than anything. Then I continually revisit those problems, often in terms of applications, and just continue to do the same thing. I don't make dramatic changes, and I guess there are some people who would criticize me for that. But I think some of these problems are interesting. There are new kinds of problems, and some of these methods that I've worked on in the past are applicable to those, too.

HAIGH: I think you had implied earlier that the architectural changes, such as the development of parallel computers, had brought back some older methods into use. Have you heard any cases of that with your own work? Examples where something you had done in the '60s or early '70s was picked up on later with the interest in parallel computing?

GOLUB: Well, the only things I could think of are the fast Poisson solvers. You might actually implement those in a way different way today than you would in the past.

HAIGH: It seems that this stream of papers on eigenvalue calculations has been another significant area of your activity during the '70s.

GOLUB: Yes. It's interesting, this paper so modified matrix eigenvalue problems. That's a well-cited paper. [Some modified matrix eigenvalue problems, SIAM Rev., 15, 318–335 (1973)]. I don't understand, it seems to have an impact factor of over six. Well, it has 112 references. That's P26. Now, somehow I had a small sabbatical, and I needed to show I had done something on this sabbatical, and at the same time, someone asked me if I had a SIAM Review paper at hand. So I put together a lot of these little results that I had, and I called it some matrix eigenvalue problems. It all began initially when Peter Henrici was visiting one summer. He said had a certain eigenvalue problem. He wanted to minimize a quadratic problem subject to homogeneous linear constraints. So I looked at what he had written, and somehow I figured out that instead of doing two eigenvalue calculations, you could reduce it and only need to solve the problem for one eigenvalue calculation. I was stimulated by him and I worked on this problem, and then I wrote up a little paper with Richard Underwood, who had been a graduate student of mine, and Underwood did the programming for that. So I took those results and whole bunch of other little results and I put it together, and then somehow this drew the attention of a good audience. Later, of course, many of these results were incorporated in the book. It's a very interesting point, of course, that sometimes you have a little idea, and you don't know how useful it's going to be. Then somehow you continue working at it, and it finds its audience.

HAIGH: Do you know if that technique that you came up with, to do it with one calculation, was incorporated into the packages that people were using?

GOLUB: No, I suspect they haven't been. But it's easy enough to use these ideas, yes. I hadn't really kept up with all the packages, so I'm not really sure whether my work has actually gone into packages.

Then there's another paper with [Jim] Wilkinson, a SIAM Review paper that appeared in 1976. I think that was another case in point where the editors of SIAM Review wanted some papers. Wilkinson knew of some of the work. I had an idea about computing, what's called a Jordan canonical form, and the generalized eigenvectors. I just wrote down the first few steps, but Jim showed how to analyze it completely. We had a long paper that's frequently referred to. It's on computing the Jordan canonical form, which is a very difficult matrix decomposition to compute. Richard Underwood was a student here, and he was here for many years. It occurred to me that one can work with several vectors simultaneous rather than one vector, and I suggested that to him as a thesis topic. Actually, other people had worked on that too, at IBM. Cullum and Willoughby both worked on that method, too. But Richard wrote a program which was in the public domain, and he analyzed this method, too, where you're working with several vectors rather than one vector.

HAIGH: So those two papers are P37 and C17, respectively. [(with J. H. Wilkinson), Ill-conditioned eigensystems and the computation of the Jordan canonical form, SIAM Rev. 18, 578–619 (1976)] and [(with R. Underwood), The block Lanczos method for computing eigenvalues, in Proceedings of the Symposium on Mathematical Software, University of Wisconsin, Madison, March 1977].

GOLUB: That's right. I'm sorry to say Richard is dead now. He was a nice man. He worked at various places like Ohio State University and for, I think, McDonnell-Douglas in St. Louis. But unfortunately, he had cancer and died. His wife was a secretary for us for a while, and that's how he met her. She now lives in the Midwest.

Finally, there's this work with Carl De Boor, and I should've almost mentioned a paper by [DL] Boley and myself. There are several in fact. I was in Chicago—my mother was ill—and while I was there, I went to visit a scientist, Victor Barcilon, at the University of Chicago. He talked to me and he said that he was interested in the inverse eigenvalue problem for matrices that are five diagonal. I said, "Do you mind if I work on it?" He said, "No, no, go ahead."

Then after I was in Chicago a couple of weeks, I went to Madison, Wisconsin. I was scheduled to spend some time there, and I started talking to Carl De Boor about inverse eigenvalue problems. Together, we figured out a way for solving the tridiagonal case. Remember, Barcilon wanted the five-diagonal case, but Carl and I started looking at the tridiagonal case. A few years earlier, Ole Hald, who was a student of Widlund, had worked on this problem. He had a complicated numerical procedure for solving the inverse eigenvalue problem for tridiagonal matrices. Carl and I sort of worked on another method, which only required linear algebra. We essentially gave a nice algorithm for solving the problem for which Ole Hald got his degree. He actually even got a Householder prize for doing it. But our procedure is somewhat simpler. Subsequently other people worked on the problem, and they had other ways of doing it. In fact, the method we worked out was really known in the literature already. But I think we gave publicity-- again, it's solving things at the right time and publicizing it properly. Of course, sometimes when you publicize it properly, you get a message that says, "Didn't you know X or Y had done it before?" But I think De Boor and I re-solved it in our own language at a time that seemed more appropriate. [(with C. de Boor), The numerically stable reconstruction of a Jacobi matrix from spectral data, Linear Alg. and its Appl. 21, 245–260 (1978)].

Then the groundwork had been done for how to handle the general case. With Boley, I figured out how the five-diagonal case and the general case in general. [(with D. Boley), Inverse eigenvalue problems for band matrices, in Proceedings of Conference on Numerical Analysis, Dundee, Scotland, June 1977]. So that paper, I don't know how frequently it's cited, but it's a well-known paper. The trouble was in a tridiagonal case, you more or less get a unique solution to the problem. But when you get have the five-diagonal case, you don't get a unique solution. I told this to Barcilon afterwards, and he dismissed us. I mean, it was sort of upsetting. This often happens, I think, in mathematics: someone poses a problem, and then you solve it, but you don't get the kind of solution he wants and then he sort of puts you down for it afterwards.

Beginning of Session 4, conducted in the afternoon of Sunday, October 23, 2005.

HAIGH: I think we've been discussing your academic work in various areas from the late 1960s onward. I believe we had concluded the discussion of eigenvalue calculations.

GOLUB: Yes. Well, in part, yes.

HAIGH: So, if you would like to discuss another one of your research areas…

GOLUB: Okay. For many years, I've been interested in calculating quadrature rules. In particular, I became interested in calculated the Gauss quadrature rules. The Gauss quadrature are associated with the zeroes of orthogonal polynomials, and orthogonal polynomials satisfy three-term recurrence relationships. One can easily see that the zeroes of the orthogonal polynomials correspond to the Gauss quadrature rule, and therefore it seems appropriate to compute the eigenvalues of a tridiagonal matrix associated with the orthogonal polynomials recurrence relationship to get the nodes.

But then the question arose how to compute the weights connected with Gauss quadrature, and that turned out to be relatively simple, too. By computing the first component of each normalized eigenvector, you can actually get the weights associated with any particular node. It turns out that a very effective way of doing that is to use the QR method and use it in a specialized way, and that will yield the first element of each eigenvector. So instead of having long tables for different wave functions, it's just as easy to compute the quadrature rule directly. A piece of software for this was produced by John Welsch, and I think it's found many uses, this particular algorithm. You can generalize this in various ways. For instance, there's what's called a Gauss-Radau rule or Gauss-Lobatto rule. This corresponds to fixing one of the nodes of the quadrature rule. For me, this really represented the beginning of work on inverse eigenvalue problems, because in a certain sense we're prescribing the eigenvalue or values or a matrix and then having them. So we construct the tridiagonal matrix and compute its eigenvalues. This technique is now used quite often. There's even further extension in generating the Kronrod rules. I think, together with Reichel and Calvetti, we have produced a very good routine for computing a computation of a Gauss-Kronrod quadrature rules. [(with D. Calvetti, W.B. Gragg and L. Reichel), Computation of Gauss-Kronrod Quadrature Rules, J. Math. Comp. 69 (2000), no. 231, 1035–1052].

HAIGH: You have a series of paper there. So P18 clearly fits what you've been talking about. [(with J. H. Welsch), Calculation of Gauss quadrature rules, Math. Comp. 23, 221–230 (1969)]. Does that description relate to this whole series of papers?

GOLUB: No, the others are related, but different. For instance, with Jerry Kautsky and Sylvan Elhay, together we wrote a paper on how to update the coefficients of the orthogonal polynomials when more data is added. [(with S. Elhay and J. Kautsky), Updating and downdating of orthogonal polynomials with data fitting applications, SIAM J. Matrix Anal. Appl. 12, No. 2, 327–353 (1991)]. So that's an interesting problem in itself, and it seems to have some wide applications.

HAIGH: So in that 1991 paper then, you're coming back to the work that you had done in the 1969 paper and building on it.

GOLUB: In some ways, yes. In part.

Finally, a topic that's been of great interest to me is using moments associated with a matrix. From those moments, you can determine upper and lower bounds on various quadratic forms. Again, this involves the use of quadrature, and there's an elegant theory behind it. The method is used in some circles, but the understanding of it can be complicated, and it's not always followed as much as it should be. In my opinion, I should say.

HAIGH: Is that the paper C44 on the list? "Matrices, Moments, and Quadrature." [(with G´erard Meurant), Matrices, moments and quadrature, in Proceedings of the 15th Dundee Conference, June-July 1993, D. F. Griffiths and G. A.Watson (eds.), Longman Scientific & Technical, 1994].

GOLUB: Yes, yes. That was done together with Gerard Meurant.

HAIGH: All right.

GOLUB: There are two other problems. There's a problem connected with inverting shape from moments. This began as a result of hearing a lecture by Peyman Milanfar, who had done some work on this problem in his thesis. What I had done with Varah and Milanfar is to show how to construct a certain matrix of moments, and from that one could get the nodes with respect to the shape of the domain. [(with Peyman Milanfar and James Varah), A Stable Numerical Method for

Inverting Shape from Moments, SIAM J. Sci. Comput. 21 (1999/00), no. 4, 1222–1243 (electronic)].

HAIGH: How would you say that the stream of research in that area fits into your broader career?

GOLUB: I don't know. These had just been problems of great interest to me. Furthermore, I think they have numerous applications. Not everybody knows about this, so that takes a while to get the word out. It requires a fair amount of mathematical and numerical background to implement these methods.

HAIGH: So that leaves one heading that you created for these collected papers, and that only has one paper in it. [(with P. E. Gill, W. Murray and M. A. Saunders), Methods for modifying matrix factorizations, Math. Comp. 28, 505–535 (1974)].

GOLUB: Oh yes, matrix factorizations. That was done with Phil Gill, Walter Murray, and Michael Saunders. I recognized at some points that a lot of updating methods and mathematical programming had to do with updating matrix factorizations. Phil Gill and Walter Murray had also worked on those problems too, so together with Mike Saunders we produced a paper, which describes various ways of modifying matrix factorizations. I believe these methods will be of great use in certain problems, but the details have to be worked out there.

HAIGH: So in that sense, that's an area that it's still very much in play…

GOLUB: Oh, yes. There's always something new to be done, a new matrix factorization that comes along, yes.

HAIGH: Do you think that this practice of returning later to areas that you had worked on before and extending things, building upon what you had done before, developing new directions, coming back to unsolved problems, is something that many researchers do? Or do you think that this is something that you do more than most?

GOLUB: Well, I think so do it, especially those of us who are working consistently in one field and mining and re-mining it. Then it occurs to us to reexamine some of these problems that we've worked on before. I think it's good. Of course, it's good that other people just try their hand at new problems every few years so that they can possibly make some contribution then.

HAIGH: All right. While we're on the subject of the academic work that you've done over the decades, let's shift slightly then to consider Stanford itself as an organizational and institutional element in this. And obviously, a number of these collaborations have been with students or with people who had been visiting. Would you like to talk about numerical analysis research at Stanford and how the group responsible for it has developed over the years?

GOLUB: I've been here over 40 years. When I came here, George Forsythe was here, and so was Jack Herriot. George had a number of really topnotch students like Cleve Moler, Beresford Parlett, Jim Varah, and others. People looked up to George because he knew so much and he had worked in a broad range of problems. So through him, this really attracted students to Stanford. In the beginning, we'd have two or three students coming in each year. So that was really exciting. First, they came in through the math department, and after that, they were in the computer science department. This continued until George's death in 1972.

In the meantime, of course, I was developing a reputation and students were coming in who worked with me for a while. So we had a nice group of students, a wonderful group of students. They worked together. Sometimes they worked for George; sometimes they worked for me. It was just a great period in the early '70s. Sadly, Forsythe died in 1972. Herriot was never very active, so it fell upon me to try to move things as much as possible.

After Forsythe died, we tried to make appointments. Eventually we hired Joe Oliger, and Joe was an expert on solving partial differential equations. He had completed his degree under Heinz Otto Kreiss. He had had a lot of practical experience at NCAR. At any rate, in the mid-1970s, some wonderful students came to Stanford. They really preferred to work on PDEs rather than linear algebra, so they worked with Joe. I would say some of our very best students were attracted to the kinds of interests he had. Some of these students later actually worked for me. That is, in our vision of things, our students were both interested in linear algebra and optimization and PDEs, but maybe only the best students had that breadth of interest. At any rate, I'm pleased to say that through Joe, we had a splendid group of students who have now been widely acknowledged as being superb. People like Randy LeVeque, who's a professor at the University of Washington at Seattle; Nick Trefethen, who's a professor of numerical analysis at Oxford and a member of the Royal Society; and Tony Chan, who is now a Dean at UCLA. So, there was a fantastic group of students who were here. They were lively and interested, and they interacted with one another. So it was a very special period.

I actually assumed that it was going to be like this forever and forever, but instead of being the beginning of a great era, it was the end of an era. The students who followed weren't of the same quality. So upon reflection, I came to a conclusion that it would be better to somehow organize a numerical analysis activity in line with applied mathematics and engineering.

HAIGH: What time period are we talking when you would've had this realization?

GOLUB: This would've been around 1980 that this actually took place.

HAIGH: Prior to that, had there been much interest in the mathematics departments in these matters? Or had that finished with the creation of the computer science department?

GOLUB: Well, some of our students still were interested in mathematics and they took many courses in mathematics.

HAIGH: And among the graduate students and the faculty who were primarily in the mathematics department?

GOLUB: There wasn't so much interest. But in the early 1980s, or even earlier, Professor [Joseph B.] Keller came to the math department. He's an applied mathematician. Some of our best students worked with Keller and our numerical analysis group. Keller is a well-known and famous applied mathematician in the United States. So it was really going well, I would say.

Then after a while, as I said, it was not the beginning of an era but the end of an era, and some of the students were not of the same quality. So it seemed to be a period to regroup, connect numerical analysis with engineering and applied mathematics, and sort of try to run out our students and have them interested in as many areas as possible. That was then the beginning of the SCCM program.

HAIGH: That's the "Scientific Computing and Computational Mathematics" program?

GOLUB: That's correct, yes. That program, after some effort, was finally approved in 1987, and then we opened our doors to a new program.

HAIGH: Was that a Master's program?

GOLUB: Master's and Ph.D.. So the intent was that a Ph.D. would also be granted. The founding people, you might say, were Joe Keller, Joe Oliger, and myself. There were other people from chemistry and chemical engineering, like [George] Bud Homsy, who also participated in the founding of the program. At first we just had a few students, but then a few years of the program grew. Again, we had some excellent students who came through that program. They're a little less well-known than the ones that preceded them because there's been less time for them to be known.

HAIGH: You were director of the program from 1988 to 1998. Was 1988 the first year it was operational?

GOLUB: 1987, I believe. But I think it was away in '87. '88 was really the beginning of the program for me, at least with respect to myself.

HAIGH: So the idea would be stimulating interest in numerical analysis and connecting it with applications in the engineering disciplines?

GOLUB: That's right. We were very fortunate in that we could admit our students, and then also even give a degree. We had a charter for five years, at least, to do that. That meant that people came into the program. We admitted, we looked them over. They may have been rejected by other departments. There was one young person who was rejected by Math as a student, but we admitted him to the SCCM program. He wrote a thesis under George Papanicolaou. Then after he got his degree, he actually became a lecturer in the math department at Stanford. So even though they had rejected him at one time, he was good enough to have a temporary position, and now he's elsewhere.

HAIGH: So would say that in general, the program succeed in the objectives and attracted the number and caliber of students that you had hoped?

GOLUB: Like most things, it went up and down. Unfortunately there was a turnover in personnel. Joe Oliger retired after a while. Then we hired Andrew Stuart, who was a wonderful colleague. He was very dedicated to the program, but he wanted to go back to Britain, and he's a professor in Warwick now. So that was really a blow because Andrew brought a lot of enthusiasm and energy to the program. You can see we started out with a number of people, and then the attrition was great after a while with Stuart leaving and Oliger disappearing and so forth.

HAIGH: It seemed from the website as if it's been replaced by something called "Computational Mathematics and Engineering."

GOLUB: Yes. After Andrew left, the dean called for some study as to what to be done in the future. One proposal was to start a new department called "Computational Math and Engineering," but that didn't fly. Instead, we became an institute. So there's an institute that has many features of the old scientific computing program. Maybe it's more engineering-oriented, but the faculty has worked very hard to figure out exactly what the program should be like. At any rate, there are some very good students. Of course, one of the things that happened when they reorganized the program into the CME is that there was more funding forthcoming from the dean's office, and that helped in bringing new students.

HAIGH: Was the funding forthcoming just because it was now a new, exciting thing?

GOLUB: We took over some courses to teach, and that's how more assistants were supported.

HAIGH: Okay. You've talked about the people who were involved in the program. Do you have anything to say about the kinds of institutional support that was forthcoming? Things like fellowships or office space or many of the conferences or any kind of symposia series of visiting programs that you might have established?

GOLUB: We didn't really establish the visiting program, but there were many fellowships. It's an ongoing project to actually extend the program and to figure out exactly where it should go. At this time, there's no head of the program. They're seeking to appoint somebody as head.

HAIGH: How about the support from the 1970s onwards for numerical analysis in general?

GOLUB: From the University it wasn't very large, but most of us had grants. Joe Oliger had grants; I had grants.

HAIGH: Where were those coming from?

GOLUB: NSF, DOE—government agencies and so forth. We've never had a lot of money, and I haven't been successful in framing proposals, but we've had some very good students, nonetheless.

HAIGH: I think you had mentioned earlier the importance of people who had been coming on sabbaticals.

GOLUB: Yes, every few years, people visited us, and that provided a lot of stimulation both for the students and the faculty. Sometimes the students ended up working for some of those people, some of the visitors, and there were a lot of interesting exchanges.

HAIGH: In terms of your own career, I saw that since 1992 you've been the Fletcher Jones professor. Did that chair exist before you assumed it?

GOLUB: Yes. Fletcher Jones died in an airplane crash, I believe, so the question was what to do with his estate. I believe it was Computer Usage Corporation that he founded. In his name, they established chairs at USC and Caltech and at Stanford. Not only did one have the recognition of a chair, but there was also financial support for this chair. The first person who held a chair here at Stanford was Don Knuth, and then he retired so I got the chair and, and am the current Fletcher Jones professor.

HAIGH: Is there anything else you think we should say about Stanford as an institution, your career there, the development of the department, numerical analysis…?

GOLUB: I've had a wonderful career at Stanford. Lots of people like to come to Stanford. Students are attracted to Stanford. As time has gone by, of course, the reputation of the University has grown and gotten better. We get better students all the time. It's really a point of attraction for many students. I feel my career was enhanced because we had so many very good students here that I could work with.

HAIGH: Now, you already mentioned your sabbatical at ETH in Zurich. I see that you've had a number of other sabbaticals. If there's anything in particular that jumps out in terms of the significance to your career, then it might be appropriate.

GOLUB: No, they've all been very pleasant. I like to see other places, and the Courant Institute was very nice because I lived in New York. I liked being at Imperial College because it was in London. St. Catherine's was very stimulating. I've been there twice: once in the 1980s and once in the '90s. I got to know Leslie Fox, who was one of the icons of modern numerical analysis. So all these places have been wonderful to be at. I also spent some time at the IMA on leave, and that was a very interesting place to be, too, because they had a year of linear algebra, so that was my specialty, and I was on the board when it was decided upon, so we invited a number of distinguished people there. It was really good.

[Tape 4, Side A].

GOLUB: I've been a co-author of a number of books, but the one that stands out most in my mind is the joint effort with Charles Van Loan. That was published through Johns Hopkins University Press.

HAIGH: You've already talked about that book in some depth.

GOLUB: There have been three editions of the book, and we hope this next year we'll work on a fourth edition of the book.

HAIGH: Your first book was Recent Advances in Numerical Analysis, which you edited with Carl De Boor in 1978. [Recent Advances In Numerical Analysis, edited by Carl de Boor and Gene H. Golub, Academic Press (1978)]. That was your first experience of editing. Did that book in any way kind of set the template for your edited volumes to come? Interest you doing this kind of activity?

GOLUB: Not particularly. In truth, I was the co-organizer with De Boor for a conference at Madison. After the talks were given, we collected the papers, and Carl did most of the physical editing of the papers.

HAIGH: And that would be a similar kind of pattern with the other edited volumes?

GOLUB: Yes, I would say so. I haven't been so good at doing detailed work.

HAIGH: I'm seeing one here that you edited on your own, or at least no one else is credited. Studies in Numerical Analysis, 1994. [Studies In Numerical Analysis, edited by Gene H. Golub, The Mathematical Association of America (1984)].

GOLUB: That was a collection of papers for the Math Association of America. They wanted to have a broad scope of numerical analysis, and what I did is approach various people for their papers to be in this edition. To be honest, I can't remember what papers are in this book right now. I know one of my own papers is in it, but that's about all I recall at this point. To be honest, like the book with Van Dooren and the one with Bart De Moor and Luskin, they really were books where I was involved in the invitations with the speakers, but I didn't actually do much of the scientific work myself.

HAIGH: So in that sense, it was a natural extension of your existing patterns of collaboration and your professional network.

GOLUB: Yes.

HAIGH: So that's made short work of a large chunk of resume. Now, let's talk then about some of your other professional activities. We had already discussed your work on various editorial boards and involvement with ACM SIGNUM. We haven't really said anything yet about SIAM. Do you remember what your earliest involvement with or awareness of SIAM would have been?

GOLUB: No, I can't. It must've been after I came to Stanford in the '60s, and I started going to SIAM meetings. They were really quite small, the SIAM meetings at that time; maybe a couple hundred people at most. I just sort of enjoyed going there. I enjoyed the friendliness of the groups. I knew some of the people who were in the organization, or in the management of the organization. The elected officials, I would say. I'm not sure when I met Ed Block, for instance, but somewhere along I met him. But it's played an important role in my professional development, and I hope I've helped SIAM, too.

HAIGH: This seems to be the organization that you became most heavily involved with.

GOLUB: Yes.

HAIGH: What was it about SIAM that made you want to get involved with it to a level that you hadn't with other groups?

GOLUB: Of all the organizations, its interests and my own seemed to be the closest of the time.

HAIGH: From your resume, the earliest office within SIAM that I saw mentioned there was associate editor of SIAM Review from 1974 onwards. Did that involve any particular hands-on work?

GOLUB: When I became an editor, then I sort of wrote a lot of people and I said, "Would you like to submit something to SIAM Review?" I think it almost had no effect. Everybody agreed, yes, they would like to submit a paper to SIAM Review, but then nothing showed up in the mailbox. But the Review, it varies in quality. Sometimes it has really some terrific articles. And sometimes, well, they may be terrific, but they're out of my own range of interest.

HAIGH: Actually, I think Ed Block had discussed the original motivations for the Review as being that it would containt, if I recall correctly, problems and things that teachers and people would be able to read and get the sense for the work in an area.

GOLUB: Yes, I guess so. I don't think it was lively enough in the old days. Now it's been changed and it has some other perspective. But I thought there should have been some broader view—less formal and more casual. Something more of the order of Statistical Science, which is a very nice journal.

HAIGH: From 1975 to 1977, you were a member of the SIAM Council.

GOLUB: Yes.

HAIGH: Was that a body to which one ran for election?

GOLUB: Yes. Long ago, I guess, it was decided that there would be real elections for all offices in SIAM. You know, there's a president and more than one candidate for the president, and that's so that nothing gets very fixed in advance.

HAIGH: Was it your idea to seek election to the council?

GOLUB: No, somebody just proposed me, and I didn't even think I was worthy of it at the time. But then I ran, and I was elected.

HAIGH: Do you remember anything about what kinds of…basically, what things the council did? Decisions that you had to make? Any controversies?

GOLUB: That's interesting, I can't remember a thing. I know I was sort of outspoken about certain aspects, but I can't remember anything that went on. I'd have to look at the minutes, I guess, to determine.

HAIGH: You can't remember the specifics?

GOLUB: No.

HAIGH: Do you have a sense that it was a lively body in which there would be discussions and disagreements? Or was it more of a rubber-stamping kind of…?

GOLUB: No. Of course Ed Block would have liked it as a rubber stamp, but I think people discussed things. Some people were always contrarians, and they would always have suggestions that seemed unusual.

HAIGH: Do you want to talk a bit about Block, now? I know he was a kind of outsized presence for a very long time within SIAM.

GOLUB: He was important for the organization, and he was single-minded and wanting to see it grow and advance. I don't know how scientifically oriented he was. He was sort of a businessman by then, although he had a Ph.D. in mathematics. Maybe that was important, so some aspects of SIAM have gone very well; you know, the publications and so forth. But he didn't have a real academic outlook. For instance, I had proposed that we try to have special rates for students. Well, he tried to squelch that. It sounded like he wanted us to come up with a complete program, which maybe included people in K through 12 also. But eventually, within the help of Cleve Moler, a program for students was included. You see, what I recognized and others did, that if you have students as members of your society, then when they graduate and have real incomes they'll be willing to continue their subscriptions and membership. Nowadays, any student who wants to can become a member of SIAM. So, those sorts of things, he wasn't very positive about. I also once proposed that there be exchange subscriptions of ideas, you know, that if people belonged to SIAM, then maybe you would get the AMS journals cheaply, and likewise, people in AMS would get the SIAM generals more cheaply. So, well, "What's in it for us?" and so forth. He was complete dedicated, and there was actually a very nice man there, but he's also rather autocratic and he ran things the way he wanted to. Because of his dedication, SIAM is where it is today, as a matter of fact.

HAIGH: You may speak some more about these issues a little bit later when we talk about your term as president. But chronologically, the founding of the <u>SIAM Journal on Scientific and Statistical Computation</u> comes first. So you were the editor of that journal from 1980 to '84. Were you the first editor?

GOLUB: Yes. Actually, the proposal to have such a journal was made by Jim Ortega and myself. Jim had been a student of George Forsythe here at Stanford. At that time, I don't know, he may have been at the University of Virginia. He had several positions in the Southeast. So we thought that the time for pure numerical analysis was coming to an end, and the <u>SIAM Journal on Numerical Analysis</u> wasn't really reflecting what people were doing at that time. So we got together and we suggested a "SIAM Journal of Scientific Computing", but I recall one meeting, Virginia Klema, who was a friend of mine, and she said, "Gene, maybe we should include statistical computing." So then I said SIAM should be the "Journal of Scientific and Statistical Computing." Then I proposed that and that went through, but as Ed says in his notes, the statistical part never really took off. We had statisticians whom I thought, as editor, would actually help out, but somehow, they didn't draw enough papers.

HAIGH: So the idea was to have papers that included more discussion of applications of numerical analysis in different disciplines.

GOLUB: That's right.

HAIGH: So were you anticipating that engineers and physicists and people like that would be submitting papers?

GOLUB: Yes, I was hoping so. There was another journal, the <u>Journal of Computational Physics</u>, JCP, which somehow wasn't of a very high quality at the time. I thought we should have something more than just say we did this and that the other thing, but also have some substantial theoretical background. In the meantime, over the years, JCP has changed dramatically and is a much better journal than it was initially, in my point of view.

HAIGH: Once the proposal was accepted and the journal was established, did it develop along the lines that you had anticipated?

GOLUB: Yes, but it's drifted towards a theoretical journal, and now they're thinking of starting another journal that will be again more practical.

HAIGH: Do you think there's some kind if inexorable trend where journals evolve towards the theoretical?

GOLUB: I guess so, yes. That seems to be the case. Often it's maybe the signals that people get from the people who manage the journal—what kind of papers they want.

HAIGH: That theoretical things seem more respectable?

GOLUB: Yes.

HAIGH: How much time did it take up being an editor? Was this a large chunk of your weekly activity?

GOLUB: Yes, a few hours a week because you had to look at a paper and decide who should the editor and make some other decisions. I think the journal is quite successful right now. You don't happen to know the circulation of the various journals, do you?

HAIGH: No. I talked to Ed Block, but I don't think he had the most recent information. But I was struck, actually, when I originally started to look at SIAM, by how many publications it had relative to the size of the membership.

GOLUB: Well, it's grown a lot. Originally, there was the journal of applied mathematics, and numerical analysis, the SIAM Review, and it seems to have grown. But I think the CISC was the first for a very long time that it had come into play. But yes, there are quite a few journals that are public now.

HAIGH: I know one of the frustrations involved in having a journal sometimes can be dealing with the professional staff members of the association in terms of copy editing, logistics, printing, and so on. Would you say that SIAM functioned smoothly in giving support in those kinds of areas?

GOLUB: Oh, they've always had a very good staff, people who really helped out a lot. In fact, starting a new journal was relatively easy with the SIAM staff. They made it easy for someone to do.

HAIGH: Would you like to talk now about the Journal on Matrix Analysis and Application, which comes a little later chronically? Or do you think it makes sense to talk first about your time as SIAM president and on the Board of Trustees?

GOLUB: No, I'll talk about SIMAX a bit because I don't really remember so much about the others, but this other is maybe painful. Ed Block used to run around and say, "Oh, what happened to all those papers on pseudoinverses? We don't publish them anymore." I don't know why, but he felt that some journal on linear algebra was really necessary.

HAIGH: So this time, the original impetus was coming more from Block himself?

GOLUB: Well, yes, and he would say this. However, I was president of SIAM, and we were meeting for the last time in my administration, so to speak, and it was in Chicago in a motel. We were talking, and suddenly Ed Block says, "Well, we're going to introduce a new journal on linear algebra." It's the first I heard of it. And here, I'm one of the people who's extremely well-known in this area, that a lot of people know about me. I'm just sitting here. I don't know if I was dazed or what. He said, well, yes, he's discussed it with Bill Gear, who was the president-elect; and with Bob O'Malley, who was in charge of SIAM publications. They talked about the journal, and they had even offered the editor-in-chief to one of our former students here from Stanford. It was sort of on the table without ever having discussed it with me. In retrospect, it was one of the worst things that's ever happened to me. I really am offended by that, and especially since both Bill Gear and Bob O'Malley are friends of mine. They must've known that I didn't know, and it must've been deliberately kept from me. And these are two people I admire, I liked, and indeed, I was even connected with their success within SIAM. So, it was really very painful when that happened. I should've walked out of the room, I think, but I'm not very quick.

HAIGH: Do you have any idea why they might have not included you in the earlier discussion?

GOLUB: No. Because to be honest, I wasn't keen on such a journal because I felt if there was such a journal, it would detract from CISC. You know, CISC was my baby. I founded it, so then I thought SIMAX should follow. Well, no, skip that. I felt that I didn't want competition from another journal within SIAM.

Nevertheless, they were very persistent, and this other person who was offered the editorship, he called me up and he said, "Gene, what's going on? Is there anything? Do you have any reluctance about this or anything?" At that point, I felt it was a done deal. If Ed Block was going to suggest such a journal, it would take place. So then this guy actually decided not to be the editor-in-chief. So then Ed Block called me up and said, "Gene, we need to find somebody to be in the editor-in-chief of this journal," and so forth. I guess I should've hung up, being pissed off at him, but I reckon there was going to be such a journal—there was nothing that I could do about it, it was in the works. I said, "Okay, well, I'll do it." You see, this other person was a very good numerical analyst. His interests were not as broad in matrices as mine were. I said, "Okay, I'll do it. But two things. One is I don't want to mess around with the paperwork. You can do it. Secondly, I don't want to (I don't know if I said it so explicitly), I don't want it called Linear Algebra, but I want it to be called Matrix Analysis." There's a book by Bellman on matrix analysis and applications. So I said, "Okay, okay." And then of course, everything happened and in no time at all it was accepted. So, I was in the editor-in-chief for the first six years, I guess.

HAIGH: 1988 to 1996, according to your CV.

GOLUB: Yes. I think it's done all right. It's a nice journal. I was a little nervous also. Maybe that just shows you what can happen. I was an editor for Linear Algebra and Its Applications, which is another journal. It was run by friends of mine, and I didn't want to be in competition with them.

HAIGH: Who published that journal?

GOLUB: Well, it's published now by Elsevier. The leading figures behind it are Hans Schneider and Dick Brualdi. So one of the things I did is I appointed Brualdi as one of our editors. I mean, I wanted to have a smooth relationship with the other journal.

HAIGH: So basically, then, your worry had just been that there really wasn't an unserved niche that this journal would serve. It would just cannibalize these existing, successful, publications.

GOLUB: Mm-hmm [yes]. I wanted it to have its own vision of which way to go. I didn't want it to be a journal of numerical linear algebra. There actually now is a <u>Journal of Numerical Linear Algebra</u>, but I didn't feel that was the appropriate thing because we had the other SIAM journals on numerical analysis. Now, what's happened over the years, though…

HAIGH: Yes. As things developed, did it find its own niche that turned out to be distinct from the existing publications?

GOLUB: I suppose so. And I think it's got a lot of prestige. At least, the people I work with, that is, my fellow co-authors, they're always interested to publish it in SIMAX. SIAM journals really enjoyed a good reputation abroad, in countries like China and Hong Kong and Singapore. When a faculty member publishes a paper, they're very interested in the quality of that journal as well as the paper. So if you publish in a SIAM journal, that's generally considered very good.

HAIGH: And you think that perception is even stronger outside the United States than inside?

GOLUB: Yes. It's really considered a good organization.

HAIGH: Any idea why? Where there any kind of deliberate international outreach?

GOLUB: No, I think the quality speaks for itself, that people think well of SIAM journals. They're well-edited, and the services are good and so forth, yes.

HAIGH: Do you think there's anything distinctive about your style running a journal?

GOLUB: No. I probably offend some people, but it's okay, or it was okay. I actually proposed a third journal with Lothar Reichel to SIAM, and after years of delay, it was rejected. I'm quite offended by it, to be honest. I founded the two journals, which have been very successful, and I thought this other journal had a very good idea behind it. It wasn't actually even mine originally, but Lothar Reichel and Daniela Calvetti, but somehow, it was turned down. I get very mixed signals on the whole did.

HAIGH: What would the journal have covered?

GOLUB: Inverse problems, which I think is really an important topic to cover. There is a journal on this now, but I think SIAM, again, would bring a distinctive flavor to it. Now, with respect to the SIMAX, it sort of drifted off into a numerical linear algebra journal. After I was editor-in-chief, Paul Van Dooren was editor-in-chief. Again, he has pretty broad interests in linear algebra. Then after Paul stepped down, there was another editor appointed. I had recommended one person, but they selected somebody else. Paul and I both recommended somebody, and they chose someone else. I don't know. I take these things personally, and I feel that my suggestion would have been a much better one, but we didn't succeed.

HAIGH: In staying with the topic of new SIAM ventures, I understand that you were also very much involved in the creation of ICIAM.

GOLUB: Yes. Well, I reckon that there wasn't so much interest in numerical analysis and applied mathematics at the International Congress of Mathematics, and that we should have our say, too. I didn't think of this at first. Sometimes I have good ideas after the event. But by focusing on applied mathematics, it might bring a lot of organizations together. I sent a message out that I was going to be in Paris on such and such a date, and I knew of three other organizations besides SIAM. There was the IMA in England, and there was a German organization called GAMM, and then there was the French organization, SMAI. We all met in Paris and we had a big discussion about a future organization.

HAIGH: What year would that have been?

GOLUB: It must've been about 1985. I'm not quite sure. Because it took a couple of years to…

HAIGH: But were you SIAM president at the time?

GOLUB: It may have been just before I was assigned. What year was I assigned?

HAIGH: You were president 1985 to '87.

GOLUB: Yes. So I think my predecessor was Hirsh Cohen, and he said to me, "Gene, I want you to be in charge of international affairs." So that's when I think this occurred to me.

HAIGH: Was that because of your wide network of contacts?

GOLUB: Possibly. I guess so, yes. So then I brought these people together, and somehow it all came out okay. By the time the meeting was held, I was no longer president of SIAM. I think there were some glitches at the meeting. One of the reasons there were glitches, it was much more successful and more people came than anticipated. We used to guess 800 people, but close to 2,000 people actually ended coming to the meeting.

HAIGH: Over time, would you say it's lived up to your vision?

GOLUB: Oh, I think so. It's had one good effect, as I mentioned, that in other countries, they had formed applied mathematics organizations, and that's worked out very well. So now the the steering committee, or the organization that runs ICIAM, is made up of people from many different countries. I considered ICIUM to be my baby, and yet when the second meeting was organized they didn't ask for any… I was kicked off. I mean, I was just ignored. I always assumed that had to do with Ed Block. I don't know.

HAIGH: Is there a completely new steering committee created for each meeting?

GOLUB: Yes. In part, there were representatives from SIAM. I don't recall now how it was exactly organized. I spoke at the second one. I was an invited speaker. But other than that, I was just ignored as a person. It was sort of painful. I thought I should've been included. And Ed Block, if you'll notice in his notes, he thought of me as a person who had ideas. I'm not a very original person necessarily, but things occur to me and I'm willing to speak up, I guess. I don't know. Maybe it was their loss, but they ignored me.

The meetings have been pretty successful. I think they've reached a plateau now. The last one was in Australia. The next one is scheduled for Zurich, and that'll probably be my last one. I have to go there because friends of mine are in Zurich are behind it. I have close connections with people in Zurich, so I like to go. But otherwise, long ago maybe I should've given up. I feel very upset when I go to the ICIAM meetings and don't feel I've been accorded my proper place in the organization.

I tried to do other things. I tried for SIAM to have a subsidiary, so to speak. That is, I thought it would be nice to have a SIAM branch in Scandinavia and in other countries, because there was no applied mathematics organization. They tried to begin a series of meetings, but the Scandinavians really didn't have their heart in it. They had one or two meetings, and then it fell by the wayside. The difficulty was that in Nordic countries, to get between, say, Helsinki and Bergen, was maybe the same cost as going from Helsinki to the United States. You know, flying within the Scandinavian countries is very expensive. The other thing is if they were part of SIAM, they didn't get a seat, so to speak, at the ICIAM council. They excluded them from being part of the international scene. So I think somehow, that's all been worked out now.

HAIGH: Okay. Then sticking with SIAM, we have your other institutional roles in it. From 1982 to '84, you were on the Board of Trustees, and then following that, from '85 to '87, you were president. Do you remember anything about your service on the Board of Trustees?

GOLUB: Not a lot. Well, there were financials matters discussed. You know, how much do we pay Ed Block, things of that sort. The person who really played an important role on that was Eugene Isaacson, who seemed to look over the financial statements with great care. But I don't really like that so much.

HAIGH: So you impression is that the trustees were mostly involved in financial oversight?

GOLUB: That's what it's supposed to be, yes. Financial matters are supposed to be in the hands of the trustees.

HAIGH: How did you become SIAM president? Did they have a committee who would nominate people for president?

GOLUB: I guess so. I don't know. I mean, Hirsh Cohen just called me up and said, "We would like to nominate you." I did run for another office in SIAM, and that's vice president. I wasn't elected. Joe Keller, he's a mathematician here at Stanford, he was elected. He's a very famous mathematician.

HAIGH: Was that earlier in the '80s?

GOLUB: Yes, I think so. Maybe four years before I was president.

HAIGH: And the election of president is contested. So did you have a platform? Any particular proposals for your term?

GOLUB: No, no. At that time, I don't think anybody said very much. It was Personality A against Personality B.

HAIGH: Okay. Then when you were elected, do you remember anything other than what you've already said about ICIAM in terms of initiatives that were ongoing during that period? Any controversies? Anything of that nature?

GOLUB: I'm sorry, I can't remember. It's over 20 years ago now. I don't remember much at all of that time.

HAIGH: Do you remember anything other than what you've said already about interactions with Ed Block while you were SIAM president?

GOLUB: No. I tried to get him to move on things. Oh, this is interesting. I had a friend who was very interested in history and he was a mathematician, and I wanted him to interview Householder. The statistical science journal has very good interviews, and I asked Ed if he would support this, and he said yes. But then this friend of mine never actually was able to do it. But it would've been good to have a nice interview with Householder of this nature.

HAIGH: I know you've mentioned Householder several times during the interview. I'm not sure if you've described your general impression of his personality and way of working, so that might be interesting.

GOLUB: He was a lovely man. Rather quiet. I mean, he did not have a big personality or anything. From the Midwest. He just wrote away, in a very terse style. It was almost impossible to read his papers. And he wrote several books. But there was something very much of a gentle soul. He smoked, he drank. He didn't have a very happy marriage, so that was a downcast for him. But otherwise he was just a nice man to be with and unpretentious and rather a kindly person. I think one of those good souls that you like to see in an organization. There are not enough of them. That's one of the good things that we've had in this group, so many really nice people like Householder and Wilkinson and Henrici and Forsythe, who were all good people, I would say. Householder outlived all the others. That is, Forsythe and Henrici. I used to joke he had the longest life, and yet he smoked and drank. He was okay.

HAIGH: I know you were also managing editor of SIAM Classics series from 1993 to '97. Was that something you originated?

GOLUB: No, somebody else originated it, but they asked me to take it over. I forgot how that happened. Then immediately, I decided to have a board, so I asked three or four people to be on the board. Out of that board, I got ideas from other people, and I solicited their ideas and several good books were published as a result of that. I've always felt that the membership should be asked to participate as much as possible. So when I was head of that series, I tried to get other people's opinions about what to publish.

HAIGH: Did that reflect a general interest in preserving the history of the discipline?

GOLUB: No, but already now as an older person, I started to feel…. Typically you always think that the books you grow up with when you're a student are the last word in the topic, so I tried to republish some of the books that I thought were really good. I mentioned already Bellman. Bellman had this book, Matrix Analysis and Applications. We republished that, except the people at SIAM were so clever, they republished the wrong edition—instead of the second edition they published the first edition.

HAIGH: I guess that seemed more historic, more classic.

GOLUB: Yes. Well, you want the best. And then a book by Lanczos on linear differential operators. The SIAM people, they're very professional. I've often made suggestions to Dover Publications. Their books are cheaper than SIAM, I believe, but they don't worry about copyrights, or they don't seem to worry about copyrights.

HAIGH: I know you were also involved with the conference that was held under the auspices of the ACM on the history of scientific computing.

GOLUB: Yes. I forgot, I guess some other people were asked to do that, but it came down to me.

HAIGH: Can you remember who asked you?

GOLUB: No. I wonder if John Rice had a role in that, but I'm not sure, exactly. So then I put together a committee and started inviting people. I tried to get it as broad as possible. Some people didn't like my ideas. They thought I was skipping certain people, and I was denounced because I was not a member of the ACM, and so forth. But it was okay.

[Tape 4, Side B].

<u>Beginning of Session 5, held on the afternoon of October 23, 2005.</u>

HAIGH: I think the last thing we talked about was your involvement with the SIAM Classics series, and then you just said a few words about your involvement with the ACM History of Scientific Computing meeting.

GOLUB: We ran that meeting in Princeton. Oddly enough, one of the people we wanted to speak was John Tukey, who lives in Princeton. I think he forgot about it. He didn't appear, unfortunately.

HAIGH: Do you have any memories about the people who did speak?

GOLUB: They were okay. It was actually taped, but I've never seen the tapes.

HAIGH: Are you aware of any initiatives, with the exception of the current projects, since then of a historical nature?

GOLUB: No, not really. Because I like these interviews in <u>Statistical Science</u>, I thought we should emulate that in some way for SIAM. You might look at that journal sometime, and you can see they've done very good interviews, in-depth interviews with scientists. I know you've read my CV, but the people who did the interviews were technically aware of what the other persons had done, which adds another dimension.

HAIGH: Certainly. The main topic that seems to be left to us would be your other forms of involvement in professional and scientific societies. One of the other things that I know is that you were made a fellow of the AAAS in 1981. Does that carry with it any kind of active involvement?

GOLUB: None whatsoever. In fact, I'm a member of NAE and the NAS, and I've done almost nothing except attend and voted in those.

HAIGH: You were also, as I understand, the chair of the mathematics section.

GOLUB: Yes, and I didn't do anything.

HAIGH: What does the mathematics section do?

GOLUB: They're supposed to organize meetings and such, and I didn't do anything there. I was not interested in that organization.

HAIGH: Why not?

GOLUB: I don't know. Well, I went to one of their meetings, and it's really dominated by biologists. If you look at their science, you see very few mathematical articles there.

HAIGH: Anything else to say about the National Academy of Engineering and the National Academy of Science?

GOLUB: No. Well, except the following: I was probably the first active numerical analyst in the National Academy of Engineering. Joe Traub was a member, but I was the first classical numerical analyst elected.

HAIGH: I believe that group has produced a number of blue ribbon reports and studies over the years.

GOLUB: I have never been asked to participate whatsoever.

HAIGH: Do you have an impression that those reports have had any kind of effect on anything?

GOLUB: I don't know.

All I wanted to say is I managed to get more people elected. There were quite a few numerical analysts. Both Bill Gear and Carl De Boor and Michael Powell and Margaret Wright, I nominated them, and now, of course, it takes on a life of its own.

HAIGH: On your resume is a very long list of awards and distinctions. I won't read them out one by one, but I'll just ask you to take a look through. If there are any that you're particularly proud of that come with an interesting story or you feel illustrates something broader, then please…

GOLUB: No. This is all just beyond my own belief that such a thing would happen to me. I have no idea why it did. But I think, in part, just being here at Stanford, it's made me visible. But there are plenty of other well-qualified people to do this. I've tried to be an activist in some cases, in the sense that I tried to bring in other people so they can have these same honors, too. It gives you a five-dollar-a-year increase in salary or something like that. [laughs] Yet it makes you feel good when you get these things. It's sort of unbelievable. I guess it just shows you that you're around for a long time.

I've written a couple articles for Acta Numerica, which is a very nice-- I don't know if you want to call it a journal. It's a book that's issued every year. Arieh Iserles of Cambridge University is the one who edits it. So I've had a couple articles which have been really well-received, so that's the great feeling. I organized a couple SIAM meetings at Stanford, and they've been very successful, I think, on the whole.

HAIGH: In my research I came across one quote as a description of one of the roles you had played, which was "as an interface between analysis and linear algebra."

GOLUB: Oh, that was Walter Gautschi. That was a very nice thing for him to say.

HAIGH: What do you think he meant by that?

GOLUB: I'm not quite sure! He's well-known for his work in orthogonal polynomials and quadrature, so that's the analysis part of his life. I think what he has liked is the fact that I've introduced these ideas of numerical linear algebra into quadrature and orthogonal polynomials. So that's what I heard, yes. But I was rather surprised that he wrote that, too.

HAIGH: So then in your mind, was that a particularly important contribution? Or is it just one example of many ways you've just been serving as an interface between different communities?

GOLUB: Maybe the latter. I like that, though. That's very pleasing to me. You can figure out something from Area A and put it into Column B, and so forth.

HAIGH: Yes. I think that remark was made in the context of a 70th birthday conference that was held.

GOLUB: Yes.

HAIGH: How do you feel as the subject of one of these things?

GOLUB: In a way, almost existentialist. I go to it and I hear people praise me or say things about me, and then, you know, you have to hold yourself back. Otherwise, I guess you would start laughing or something. [laughs] It's very nice. I'm very touched when people say these things, but it's just sort of unbelievable.

HAIGH: Do you have any current and ongoing projects that you haven't discussed yet?

GOLUB: No. There are a couple books that I'm in the process of working with others on. One on least squares with Victor Pereyra and Godela Scherer, and another book possibly with Gerard Meurant associated with matrices, moments, and quadrature. It's an area that I've enjoyed working on.

HAIGH: You had mentioned to me, but I'm not sure if the tape was running, a meeting that you're planning for 2007.

GOLUB: Yes. George Forsythe came here to Stanford in 1957, so it'll be 50 years. For instance, our statistics department had a 50^{th} anniversary. I thought, well, might as well have a 50^{th} anniversary numerical analysis at Stanford.

HAIGH: As things that were purely honors and awards, I noticed that you had also been involved with a number of boards and advisory committees and so on. Any of those where you feel your work had any practical significance?

GOLUB: No, I just stayed there and listened to people, and maybe commented occasional. Actually, this is one that I enjoyed the most—reading about future graduate students, people who have done very well.

HAIGH: All right. So that was your service as a member of the NSF panel for awarding graduate fellowships in mathematical sciences?

GOLUB: Yes. That was a long time ago. What year was that now?

HAIGH: 1977 to '80. Would it be fair to say that you don't particularly enjoy being on that kind of body?

GOLUB: I don't think I work very well. Sometimes I get very bored by it and sometimes I fall asleep. [chuckles] The race goes to the most articulate, those people who speak well, and I don't speak particularly well.

HAIGH: Are there any important topics that you think we haven't covered?

GOLUB: No. I wish I could think about it. What I've enjoyed most is making Stanford a home for all the other numerical analysts. You know, making people feel comfortable here. So, I like that that. I think this is maybe the subject of another question of yours, but a lot of people have come here and they've gotten involved with other co-authors they wouldn't necessarily have known, but by coming to Stanford, sometimes somebody from Australia and somebody from England, they might actually meet here and work together. For instance, recently a book was issued, two authors were from England and one was from Maryland, but they all met here at one time or another and that's how their project got going. So I like to facilitate people's meeting and getting together.

HAIGH: All right. Well, that would lead me on to the two questions that I've asked at the end of the interviews in the series then. The first one would be that as you look back on your career as a whole, what do you think your biggest regret would be over your professional life? Either in terms of a decision that you made, or perhaps just in terms of the way that something turned out.

GOLUB: I'm sorry that numerical analysis doesn't have a stronger foothold here at Stanford; that when I leave in a few years, that'll probably be the end of classical numerical analysis. It doesn't have a home here. The computer science department is really not about to buy another numerical analysis. They view their mission much differently than the way I see things. It may well be that the time has come and gone, but other institutions, like the University of Maryland has three excellent first-rate people in their computer science department. So I think here at Stanford the end is in sight, so to speak. Maybe had I been better at political matters, I would have been able to sort of arrange things better. It's not as if we didn't have opportunities. Some people were here, like Joe Oliger, he was here for several years and he had good students. Then he drifted into some kind of alcoholic dream or world, and he really failed at the end, I think. But when he first came, there were wonderful students who worked with him. I don't know if one should say this for your paper now.

HAIGH: Oh, you'll have the opportunity to edit the transcript before it's made public.

GOLUB: But other people have come and gone, like Andrew Stuart, unfortunately. He was here and he was full of energy, and then he left. Then there was another person who succeeded him, but had no interest in really contributing to the numerical analysis activity. So it's not really taken off. I know a lot of people are fond of Stanford, have good memories of it, but I just don't see how it can continue now. Didn't find the right mechanism for maintaining it, as a place. Maybe everybody wants to see a successor to themselves, but I don't see it offhand.

HAIGH: To ask the more positive reserve of the question, again looking at your career as a whole, what do you think the thing that you would take the most pride in would be?

GOLUB: Well, these lists that I just showed you. The fact that we've had students who are members of various academies.

HAIGH: So I should clarify for listeners that this is in terms of a list of the numerical analysts who have been associated at one time or another with Stanford as faculty members, students, post-docs, or people visiting on sabbaticals.

GOLUB: What I was referring to was, well, for instance, the fact that several of our formal students, at least four, are members of academies. Marsha Berger, NAS; Cleve Moler, NAE; Margaret [Wright], NAE and NAS. These guys are member of their national academies. Then, of course, you can see that we have several students who have become wealthy. But more than that, what I'm really proud of is just the good people we've had here. We've really had some terrific people just in human qualities. You know, they've just been great. So I'm really happy about that. Stanford was a place that many people wanted to come here, and I try to accommodate them as much as possible.

HAIGH: It's interesting that the numerical analysis community at Stanford is both the item of pride that you've chosen, and also the source of the regret, rather than anything from your personal body of work or a paper.

GOLUB: Well, you know, I'm pleased. Whenever somebody mentions the singular value decomposition, I want to get up and take a bow.

HAIGH: I did notice your license plate, "PROF SVD."

GOLUB: Yes. [Laughs] The SVD existed before I did any work on it, and what I did is give an algorithm and told people about the algorithm. Somehow, that combination got out. I don't know where people learn about SVD, but they've been using it in control theory and all kinds of different areas. That's really pleased me to no end that that's been there. So that technical aspect. Just as I listed five topics in this list, I would say if you clustered each one of those: least squares, that would be sort of a topic I'm proud that I contributed to; and fast methods and iterative methods, I'm very proud of that. I like all that stuff. I'm really happy because it's useful for people to be able to hear this. One of the satisfactions of numerical analysis, at least the way I do it, is that there are people out there who can use it. They find it of use.

HAIGH: All right. That exhausts all the topics I had prepared for discussion. If you would like to say anything, then this would be the moment.

GOLUB: No, no. I think it's best just to shut up at this point. I've said a lot, and maybe I haven't formulated everything as well as I would have liked to. You asked me this final question, and I just answered you in all sincerity. Maybe there'd be a more clever way of responding. I'd like to see other people's answers, to see what they think they've done.

HAIGH: Thank you very much for agreeing to take part and giving your time over the last two days.

End of Session 5 and of Tape 4, Side B, and conclusion of interview.