TOM CORBIN'S INTERVIEW OF DR. GEORGE H. KAPLAN
WEDNESDAY, JUNE 22, 2005

(START OF AUDIOTAPE)

DR. CORBIN: Okay, today is June 22, 2005. I am here with Dr. George Kaplan. I'm Tom Corbin.

George, we usually begin these interviews with some background.

DR. KAPLAN: Okay.

I was born in 1948 and lived in Hagerstown, Maryland, for a short time when I was quite young, so I don't remember any of it. My father worked for the Maryland State Department of Public Welfare, and his job had moved to Baltimore. So we moved to Baltimore before I started school.

My mother had taught school for a while, and my father, as I said, worked for the state welfare department. He started out in journalism actually, with a degree from Columbia. My father did sports writing and other stories for the Hagerstown newspaper, and he is still an avid sports fan and baseball critic. Both my parents are still alive and in good health. [My father died in 2008 at the age of 97.]

I have two brothers, an older brother David and a younger brother Douglas. I'm married to Carol Kaplan, We were married in 1972. We don't have any kids. So I guess that's —

DR. CORBIN: So you went to schools in the Baltimore area?

DR. KAPLAN: Right. I went to public schools. The elementary school I attended was near my childhood home in northeast Baltimore, where my parents still live, Garrett Heights Elementary, and then I went on to Woodbourne Junior High, another public school. And then I ended up in Baltimore City College, an old high school right in the middle of Baltimore, near where Memorial Stadium is, or was.

DR. CORBIN: Did you have an interest in astronomy during your public school years?

DR. KAPLAN: Yeah, I would get interested in different aspects of science, depending on how old I was. For a while I thought I wanted to be a doctor. And then I was very interested in biological science for a while, and then I guess somewhere around age 12 or so, I guess my interest in astronomy specifically sort of solidified.

And my parents got me a small three-inch reflecting telescope for Christmas one year and I guess that's when — you know, I learned how to use it, and learned my way around the sky and that sort of thing, and got really interested in it.

So by the time I was in high school I knew I wanted to be an astronomer, although I wasn't really clear about what all that was about.

So I made sure I took physics, and chemistry, and all the science courses and math that I could, and was accepted to the University of Maryland in College Park, which at that time had just begun an astronomy program as part of its physics department, which it was developing actively at the time too.

And Dr. Gart Westerhout, who would play a role later in my life here at the Observatory, had been appointed just a few years before as the director of the new Astronomy Program at the University of Maryland.

Within the last decade the Astronomy Program at Maryland has actually separated from physics and become its own department, but for all the while that I was at the University of Maryland it was a program within the physics department. The department was then officially called the Department of Physics and Astronomy.

DR. CORBIN: Well, when you first were attracted to astronomy and then as you went along in high school, was there someone who helped keep your interest going, for instance an astronomy club, or did you do this on your own more or less?

DR. KAPLAN: I did it on my own more or less. My physics teacher in high school, named Elmer Kreisel, who was actually a very young man at the time that I took physics from him. I think he was right out of college so basically he is only about five or six years older then I am.

I say that in the present tense because in the years since then I've had occasions to meet Elmer and talk to him. He's now teaching at Towson University, just north of Baltimore.

But anyway, Elmer was also an amateur astronomer. And at the time that I had started the physics course, the high school physics course, I guess it was around 1963 or so, there had been a total solar eclipse that had passed through Maine — passed through Canada but ended up in Maine — and I had convinced my parents to go to that eclipse as our vacation that summer.

It was in July. And we were clouded out at our observing site, although it was a beautiful day up until about five minutes before totality. The weather conditions sometimes change during an eclipse and unfortunately a cloudbank moved in and covered the Sun during totality.

But it turns out that Elmer had been on the Maine coast and had seen the eclipse. So almost from the first day that I took the physics course, starting that fall, we had lots of conversations, not only about that eclipse but about astronomy in general. So he was very instrumental in getting me interested and continuing my interest in astronomy and physics too.

There was a science club at City College and Elmer helped us a lot with some fairly dangerous experiments with a carbon arc lamp and an old spectroscope that we found in a back room.

DR. CORBIN: Yeah, I remember that eclipse well. I was up there too. I was in the Army at the time, and a friend and I drove up to Fort Devens, Mass. I remember our tearing around the countryside trying to catch clear spots.

DR. KAPLAN: Yeah, yeah. I mean it was the middle of the summer. It was a hot day for up there and a lot of cumulous clouds were forming and yeah, the people near the coast where Elmer was, I think it was on Cadillac Mountain, actually got to see the eclipse. Most of the people who were inland were dodging clouds.

But I also became a member of the Baltimore Astronomical Society at that time and that was a club that was part of the Maryland Academy of Sciences. It met in the old Enoch Pratt library building downtown, once a month. And it was a very nice organization, sort of a mixture of old guys and a couple of kids, you know.

And one of the older guys was named Paul Watson — always referred to as "Mr. Watson" — who had a small observatory, with a 12-inch telescope, on the bank of the Magothy River south of Baltimore, that he would invite the club to use.

DR. CORBIN: A 12-inch was pretty good in those days.

DR. KAPLAN: In those days it was, yeah. Anyway he was the man who used to produce what was called the "Graphic Timetable of the Heavens" for *Sky and Telescope* magazine. It was a black and white insert in every January issue.

And of course these were days before computers and he did this by hand, which of course required a lot of work — I mean it took him almost the entire year previous to the publication date to come up with this graphic. And it was a wonderful thing. I sort of miss the old black and white copies of the Graphic Timetable.

But he had a 12-inch observatory, and I don't know, there's something about the smell of old observatories. I don't know what it is. Maybe it's the smell of old astronomers! I don't know, but it's like each observatory seems to have its own distinctive smell, whether it's old equipment or mold in the woodwork somewhere, I don't know.

But those experiences with the astronomy club in Baltimore and with Elmer Kreisel as my physics teacher in high school I think really nurtured my interest in astronomy.

DR. CORBIN: Yes, those things are important.

DR. KAPLAN: Yeah. When you're young, I mean some small things can make big impressions.

DR. CORBIN: Right, they keep you going.

DR. KAPLAN: Yeah.

DR. CORBIN: How about college? Does anyone in particular stand out there?

DR. KAPLAN: Well I was very lucky in College. I started at the University of Maryland in the fall of 1965 and my freshman physics professor was named Sandy Wall -– Dr. N. Sanders Wall was his actual name, I believe.

The physics department at Maryland was ramping up. They were beginning to build a cyclotron at the College Park campus so the department was ramping up considerably and my recollection was that Dr. Wall had come down from MIT to be part of this.

He was a very good professor and taught me my introductory mechanics and heat. But I was starting to get 30s and 40s on physics tests, which was a little bit shocking to me because in high school I was always used to, you know, getting 80s and 90s.

It turns out that 30s and 40s were about as well as anybody was doing but I wasn't quite clued in on the whole concept of grading curves so I was sort of in a panic. **(TAPE INTERRUPTED)** Anyway, one day Dr. Wall walked into class and asked whether anybody wanted to do some extra credit work. Given my situation I thought maybe that would be a good thing for me to do, so I volunteered after class. He asked me whether I'd want to learn how to program a computer.

Now, my older brother David had worked for a while for the old National Bureau of Standards, working on computers. He had been a math major in college, and in the early 1960s computers were still a fairly new area. Our family had visited him once at his workplace at the old NBS campus on Van Ness Street and I really thought that computers were quite fascinating.

So I said told Dr. Wall, sure, learning to program a computer would be a cool thing to be able to do. So he gave me a bunch of self-teaching books on Fortran, and over the next couple of months I went through those and learned, at least in theory, how to program in Fortran. I was quite excited about it, actually.

And the physics department had just bought a PDP-8, a Digital Equipment Corporation PDP-8, and this was really the first minicomputer. This was late in 1965 or early in 1966.

The PDP-8 sat on a desk and I guess it was something like a foot and a half or two feet cubed, with all the lights and switches on the front, and you communicated with it through a teletype machine with paper tape. And it had connected to it an oscilloscope, an X-Y oscilloscope that could be used to draw pictures. The idea was that you could actually program the computer and show its output graphically on this display. Today we take this for granted but back then it was quite revolutionary. When I first saw it, the oscilloscope was displaying an animation of a bouncing ball; it was bouncing around a box representing the walls of a room. Of course it was bouncing according to all of the laws of physics and it was quite amazing to watch. These days we wouldn't think twice about it — in fact, it would be regarded as pretty simple-minded. But back then it was state of the art.

But anyway, this was the first computer that I learned how to use, this PDP-8, using Fortran to program it. Dr. Wall hired me over that summer, the summer of 1966, to do a project related to the cyclotron, which happened to deal with the focusing magnets for the beam.

And I didn't quite understand all the physics involved in this but he gave me all the equations and I programmed them up. Dr. Wall spent most of the summer at Brookhaven Lab, so I was pretty much on my own, although there were other summer students in the department to pal around with and learn from. And during the summer, not only did I become reasonably proficient in Fortran, but in order to make the oscilloscope work I had to also learn machine language and the assembly language for the PDP-8, and how to get a Fortran program to call an assembly language program and transmit data to it so that the assembly language program could create the proper display on the oscilloscope screen. And I managed to do this by the end of summer.

Actually during that summer, in 1966, there's no doubt that I learned more about computers then than I probably learned the rest of my life put together. I got very much into it and, as I said, just beginning to understand binary and octal notation, things like registers and addressing and the whole use of binary numbers within computers, you know, at a very basic level. I learned all that in that one summer.

So I was pretty lucky in getting that kind of skill. I was paid $1.25 an hour that summer but it was a good experience and really set me up for some things that I did later in life, including getting hired at the Naval Observatory.

DR. CORBIN: Well I think a lot of us who lived through that era were lucky in that respect. You know, in one form or another we had to have contact with some kind of machine language so we really came to understand what was going on in the machine.

I mean nowadays languages are such high level things that people have no idea whatsoever what's actually taking place in there.

DR. KAPLAN: Oh, right, right.

DR. CORBIN: And we did have that experience.

DR. KAPLAN: Yeah, I think it gives you a totally different perspective on computers than I think people have nowadays because you have to live and work much closer to the actual machine, the instruction codes and the electronics of the machine. Of course it's much harder to get anything done that way!

So my first couple of years at Maryland, I really didn't have that much contact with the Astronomy Program, since my course work was mostly physics and math, but starting at the end of my sophomore year I started to work for Dr. Frank Kerr in the Astronomy Program — despite the fact that I had yet to take a college course in astronomy! Now Dr. Kerr was an Australian radio astronomer, who had been a colleague of Gart Westerhout. They had collaborated on the first 21- centimeter neutral hydrogen map of the Milky Way.

Of course Gart was from Holland so he was working from the Northern Hemisphere and Frank Kerr had been working from the Southern Hemisphere, I think mostly with the Parkes Dish in Australia, and they were mapping the 21-centimeter neutral hydrogen line and basically creating a map of the hydrogen clouds in the spiral arms of the Milky Way. This was the first real map of the spiral structure of the whole galaxy.

So my understanding is that Gart sort of convinced Frank to come to the University of

Maryland as part of the relatively new Astronomy Program. So Dr. Kerr was new at the University of Maryland. In fact, I think he was officially still a "visiting professor".

Anyway, the long and the short of it is that my second and third summers at Maryland, and I guess some summers actually after that, I worked for Frank Kerr on his 21 centimeter projects. So not only did I go farther in computer programming, and that was for the IBM 7094 mainframe that the University of Maryland had, with everything on cards, but I also learned about radio astronomy.

And I didn't actually start taking astronomy courses until I guess it was my junior year of college. Most of it up to that time had been physics and math, but it was through Frank Kerr that I got an interest in and some knowledge of radio astronomy. There weren't many undergraduate astronomy majors in those days, so in the astronomy classes I ended up taking during my junior and senior year I got mixed up with a lot of graduate students. But I held my own.

Over the summers Frank would send me down to Green Bank, West Virginia, to the National Radio Astronomy Observatory, and I'd learn how to set up observations. It was there that I did my first precession calculation — by hand! We were working on the 140-foot telescope there.

And Gart Westerhout was there too during the summers. He practically lived at Green Bank over the summers because he was completing a massive survey on the ill-fated 300-foot transit radio telescope there.

So that was my first encounter with radio astronomy — actually, any kind of observational astronomy at the professional level — and I really enjoyed those summers and that time at Green Bank.

DR. CORBIN: So at this point radio astronomy hadn't become a big factor in the department itself as a course of study.

DR. KAPLAN: Well, it certainly seemed to be becoming that way because with both Westerhout and Kerr there, you know, there were a lot of graduate students who were doing studies in neutral hydrogen. Woody Sullivan was there. He was a grad student while I was an undergraduate. Jill Knapp, who's now at Princeton, also did that kind of work. So there were a number of people at the University of Maryland at that time who started off in neutral hydrogen, 21 centimeter line studies.

But I got caught up in all the late '60s social upheaval and sort of got disillusioned with the idea of science as a career. I worked very hard in the Eugene McCarthy presidential campaign in 1968. So by my senior year, I felt I wanted to do something for the uplift of humanity, or whatever. So during my senior year I took a lot of courses in psychology and sociology, and after I graduated, I didn't go on to graduate school in astronomy. I decided to try teaching school in the city.

So after I graduated, I taught school for a couple of years in Baltimore City. And of course this was in the time of Vietnam. I actually at one point got called up to take my draft physical but I have very flat feet, *extremely* flat feet, and they gave me a 4-F classification, which meant a permanent medical deferment. So getting drafted was not a major concern for me after that.

And during the summers of 1969 and 1970 I did continue to work for Frank Kerr at Maryland. One of Frank's other research assistants was a senior undergraduate astronomy major named Carol. The first time I saw her last name, which was Polish, was on an IBM card — the "job" card at the front of a deck — and I thought it was computer code of some type. Anyway, later we started dating and I ended up marrying her.

After two years of teaching science in junior high school in Baltimore City — this was at Pimlico Junior High — I guess the second summer I got a call out of the blue from the Naval Observatory. I think it was from Ralph Haupt who was at that point assistant director of the old Nautical Almanac Office, under Ray Duncombe, who was the director.

And the way this call came about, as I understand it, was that at some point I had put my application into the Civil Service System because I had been looking at jobs both at Goddard Space Flight Center, which of course is quite near the University of Maryland, and also at the Applied Physics Lab.

So sometime, and I can't remember exactly when, I had filled out the old SF-171 form and got my name in the Civil Service System. And at that point everything was very centralized. The Civil Service Commission, as it was called then, maintained all of these "registers" of applicants, and when an office in the government wanted to hire somebody, usually the first thing they had to do was go into the register and pull out all of these SF-171s.

So apparently that's what had happened. The Nautical Almanac Office had found my SF-171 and I seemed to have the qualifications they were looking for. And by that time I had decided that I wasn't the one to save the world and was interested in getting back into science as a career.

In those days it wasn't all that common for people to have prior computer experience, and the other thing that I didn't know at the time but became clear after I was hired here, was the fact that I had some computer graphics experience. At least that's what it would be called today. There wasn't really a name for it then.

I had not only the experience with the PDP-8 oscilloscope in the physics department but also with a CalComp pen plotter at the University of Maryland Computer Center. And as part of my hobby as an amateur astronomer I also knew how to develop black and white photographic film.

Anyway all of these particular skills seemed to be what they were looking for. The position was just at the GS-7 level.

So anyway Haupt called me up and I said, sure, I was interested and I guess I came in for an interview. I don't remember whether I interviewed with Haupt or with Ken Seidelmann, but in the end they decided to hire me.

And I was pretty enthusiastic about coming to Washington. I would actually get the job title of astronomer, which was pretty neat, and I'd get to work at this world famous observatory every day, on this beautiful campus. And, be an employee of the U.S. government! So I couldn't turn it down.

There were some glitches in the hiring processes as is frequently the case. I had hoped to start over the summer so that I wouldn't have to start a new school year in Baltimore but things got delayed and it turned out that I had to actually teach for about three weeks before having to resign and come down here to Washington to start at the Naval Observatory.

DR. CORBIN: What was your starting date?

DR. KAPLAN: I started in September of 1971.

I left my classes in very good hands though. I felt bad about leaving at the beginning of the school year but there was a teacher who had been substituting, who had been doing long-term substituting, and knew all the kids in my classes and he actually took over from me as a full-time teacher. He was quite good and so my junior high school classes were left in very good hands.

So I started here on the same day that Marie Lukac, who was then Marie DeLuccia, started. We were both hired in the Nautical Almanac Office on that exact same day and we both trooped down to the Navy Yard together and did our new-hire thing, fingerprints and all that.

The first project that I was assigned to, and the reason they were so keen on hiring me because I had this experience with the CalComp pen plotter, was the Air Almanac sky diagrams. As you know, this was a major piece of work done by hand that required I guess really one whole person just to do it all the time, pasting little symbols on preprinted forms. And I think it was Ken Seidelmann's idea that this could be computerized and put on the CalComp plotter at the Observatory.

And there was something he mentioned to me sometime later. He may have had a bet with Duncombe about whether this could be done or not. So that was my first project.

They actually had a summer student do a little bit of preliminary work on it so I was picking up with some code that had been written previously, but basically it became my project over the next year and so that was really how I got my start.

Now the ironic thing is that pretty much the same program has been running for over 30 years for those sky diagrams that are still in the Air Almanac, and Marie, who after she married Carl Lukac, became Marie Lukac, took over the production of those sky diagrams.

And there were still a lot of special instructions you have to give to the programs to make sure the diagrams came out in an acceptable form and Marie was always quite willing to do that, and put a lot of time into making sure things were just right.

But now as we're transitioning on and the Air Almanac is less important than it used to be, the sky diagrams themselves are much less important then they used to be.

We're now in the stage of trying to simplify the whole process and so I'm now helping Bob Miller with the same code that I wrote, what 33, 34 years ago, and trying to make it a little more easy to use so that the whole process is pretty much just turn the crank and the diagrams come out.

And of course they're now all done on Macs. We just get PostScript plots that come right out of the printer, which was a far cry from the days and weeks of production, even with the CalComp plotter, that they required in the old days.

DR. CORBIN: Well I guess when you became involved in this, Almanac had moved down to building 52, hadn't it?

DR. KAPLAN: Well actually my first three weeks at USNO, maybe it was only two weeks in the fall of '71, we were in the main building so my first office was a shared office. I guess it was QC that I shared with Paul Janiczek, Alan Fiala, and Dan Pascu. We all had desks in there.

DR. CORBIN: That was the one with the raised floor.

DR. KAPLAN: That still had a raised floor at that time, that's right.

DR. CORBIN: The 1410 had been in there.

DR. KAPLAN: Right, right. And at that time the IBM 360 model 40, which was the main computer for the Observatory, was down in building 16.

So very shortly after I came was the move of the Nautical Almanac Office you mentioned down to the second floor, building 52, and the computer was moved to an interior specially prepared computer room in 52 at that time too. So, yeah, shortly after I began, everything was sort of consolidated there in building 52.

My understanding of the way the Observatory worked, somebody explained to me, was that each department of the Observatory, I think they were called divisions at the time, had some instrument that they were responsible for.

There was the 7-inch Division and the 6-inch Division for the two transit circles, and Time Service had the clock system of course.

DR. CORBIN: And the Photographic Zenith Tubes.

DR. KAPLAN: Yes, the PZTs. And there was what they called the Astrometry and Astrophysics Division, which had the 26-inch. And the Nautical Almanac Office was responsible for the computer, so that was our instrument that we were responsible for.

So in the new arrangement in building 52, all of our offices were along the outsides of the building, with windows, and the computer room was in the center, centrally located.

DR. CORBIN: Well, the other divisions, I don't know if you're aware of this, but they also viewed Almanac as kind of the primary source for computer expertise at the Observatory, and this went all the way back, I think, to when Herget was coming here working and, you know —

DR. KAPLAN: And Eckert's work here too probably.

DR. CORBIN: Yes, celestial mechanics had always been the driver for bigger and better computers and a lot of the cutting edge programming, so we usually would come see you guys whenever we would get a new computer, or if a new language came in, or something like that.

DR. KAPLAN: Yeah, that's right. NAO was responsible for providing the computer services for everybody else and later on in my career I got heavily involved in that kind of support.

DR. CORBIN: In fact, I don't know if you remember it but you helped me some with the CalComp because I started plotting things for the proper motion work.

DR. KAPLAN: Yeah, that's right.

DR. CORBIN: But that was after it came down to building 52.

DR. KAPLAN: Yeah, yeah. The 360 model 40, we were using — I guess when it was initially purchased, which was a few years before I came, I think it was one of the newer 360s. I think the IBM had just transitioned away from the 7094 series and so it was a brand new architecture for IBM.

But that 360 model 40 was slow, but because we weren't well funded in those days, we held on to that computer for far longer then we should have and were somewhat hamstrung in the number and quality of computations we could do simply because the machine was too slow.

DR. CORBIN: But I know it had a fairly early version of disk drives when it first came in. It seemed to be reasonably strong in its I/O handling capabilities as opposed to pure computational functions.

DR. KAPLAN: Yes, that's correct.

DR. CORBIN: And of course for a lot of us, dealing with observations and so on, the I/O is pretty important, so most of the divisions were not all that unhappy with it because we didn't do the heavy duty computing that you folks did.

DR. KAPLAN: Right, yeah. But projects like — Tom Van Flandern did lunar occultation predictions and of course that's very compute intensive and that was something that took literally the better part of a year. It would run every night overnight for months and months to do all of those predications for the various stations.

And when I got a little more into doing numerical integrations of planetary orbits, even doing a one-body integration for, say, 200 years, would take, you know, 20 hours of computer time or something like that to get through the whole thing.

These days, Marc Murison can do that in real time and just show me the output as it's coming off, and be done in two seconds.

DR. CORBIN: But to put things in perspective, I was going down to the University of Virginia a lot there in the mid '70s and working with Nelson Limber on a research project. It was required to do a project outside the area of specialization for the dissertation. He and I worked on Wolf-Rayet stars, and USNO our machine was so much better then the one they had at UVA. I would bring the runs here and run them over the weekend. We were doing Monte Carlo photon firing but he thought it was just wonderful what we had, so they were worse off then we were.

DR. KAPLAN: I guess so. In the late '70s we got the opportunity to use a computer at the Patuxent River Naval Air Station. I think it was 360 model 195, which in those days was like a super computer. It was amazingly fast compared to what we were used to.

So several times I took some of these numerical integrations that I had been doing down there to run on that machine and these were, as I said, computations that would take days, and you'd put them on that machine and they were done in a minute.

And in fact the operators down there had never seen a computer program that was entirely computational. There was a little light on these machines, which was called the "wait" light, and it basically lit up whenever the CPU was waiting for some I/O operation to take place and on most computers it was on 50 percent of the time.

And for a machine as fast as a 360/195, it was on all the time. The operators were just used to seeing it on all the time and if it went out, that usually meant a program had fallen into a loop, an infinite loop.

If the "wait" light went out and the computer was still running they would just cancel the job that was running. It took us a couple of times to convince them that no, this program actually is computing real stuff, and yes, the "wait" light is going to go out for a minute and a half and that's okay because it's doing what it's supposed to do.

DR. CORBIN: Well, as I recall it was fairly early on in your time here when the people running the Almanac, the people in charge of things, recognized you as somebody who was pretty capable and had a lot of potential. And my impression was you were given a lot of responsibility fairly quickly after coming here, I mean within just two or three years. Is that right?

DR. KAPLAN: Yeah, I think that's right. One of the things that Paul Janiczek and I did fairly early on in my career was develop a system for typesetting in Fortran. We worked on this as a back-burner project. Nobody asked us to do this.

But we had been given — well let me back up here. The publications at the time, the almanacs, were produced at GPO. Some of it was hand typeset but a lot of it, especially the Nautical and the Air Almanacs, and increasingly a number of pages in the Astronomical Almanac, were done on what was at that time a new electronic typesetting system at GPO called the Linotron.

Like everything in those days, it was very crude compared to what we have now for typesetting even on just personal computers, but it was a rather large machine and it took a magnetic tape of commands that you had to send down.

And to generate the magnetic tape of commands, a programming system had been developed here by David Scott that required everybody to use assembler language. Needless to say this was a slow, laborious, and error prone process because it could take several weeks before you got any output back and then you'd find that your pages were totally different than what you had expected. And then it was another two-week cycle before you had any idea whatsoever what was going to come out of the next run.

DR. CORBIN: David Scott would do things like strip zones off of characters in the heading. If you change the heading then your computations changed.

DR. KAPLAN: Yeah, it was all very strange, and his circular, which described his system, was not quite as clear as one might hope.

So Janiczek and I had been handed assignments of doing some pages for the Linotron, some pages for the Astronomical Almanac. And Paul and I have always gotten along. I still communicate with Paul. And somehow we got together and we said, there has to be a better way of doing this.

So what we did was — Paul took some parts of this and I took others — we devised a subroutine system that was callable from Fortran, that would do all of the typesetting. Some of this had calls to assembler language routines that I wrote, but it would make up all of the codes that would go on the tape that would then be sent down to GPO for the Linotron.

So the idea was to make this a lot more user friendly and less error prone, and also we did it entirely for selfish reasons in that we didn't want to use Dave Scott's system for our own work.

So basically we came up with this system, we called it FATS, the Fortran Automatic Typesetting System, and we used it for generating Linotron tapes. It made people in our department a lot more flexible and eased the burden on them, and we got a lot more pages done that way because you could just go directly from Fortran and do a lot of the computations if you needed to, and then typeset it right there from the program that did the computations.

So I think that was one of the things that got me recognized.

And as part of that, because the system had to be able to write the magnetic tapes that went to the Linotron, I had to understand the coding for the Linotron. So somewhere about that time I got the idea that I could work the process in reverse, read those tapes back off, read the codes off the tape, and generate CalComp calls to the CalComp plotter subroutines that would make a facsimile of the printed page that the Linotron would produce.

And I remember there was one day — and obviously I have to test this out, not only with Linotron tapes that our system had made but also with Linotron tapes that Scott's system had made. You know, if I did it right, it shouldn't matter what had generated the Linotron tape.

So I was able to actually create facsimiles of almanac pages from a Linotron tape, regardless of what its origin was. Okay, well if the program could do that, that shortened up the debug time because now you could use my program to look at what your output was going to do, and see the page setup, and you didn't have to wait two weeks to send this thing to GPO.

DR. CORBIN: So previously you had to wait for them to send —

DR. KAPLAN: Oh, yeah, you had no idea — you'd run the thing, the tape would go off, it would disappear into the black hole of government for two weeks and then suddenly pages would appear, which would either look like what you thought they were or not, and it usually was not.

Well now you could make one run on the machine and at least get a pretty good idea of whether you had your pages laid out right. It wouldn't reproduce all of the exact characters of the type font but it would show you where each of those letters was going to be.

(END OF SIDE A, START OF SIDE B)

DR. KAPLAN: I remember one morning just — it needs to be said again that Paul and I worked on this project fairly surreptitiously. We never told anybody what we were doing ahead of time.

DR. CORBIN: What year was this?

DR. KAPLAN: Oh, this must have been '72, '73.

DR. CORBIN: Oh, that early.

DR. KAPLAN: Yeah, it was pretty early on. And this project of mine, which I called LINSIM, the Linotron Simulator, was something that I had kept pretty close to my vest too because I didn't want to show it to anybody until I had finished it and made sure the darn thing would work, because I wasn't ever sure that I was going to be able to do it exactly.

I had shown it to Ken Seidelmann, who was my boss at the time, and he was pretty amazed and he called in Ray Duncombe one morning to just watch this thing plot out a page simulation on the CalComp plotter. The plotter was generating an almanac page, a simulation of an almanac page.

And Duncombe was pretty amazed at that and I think that may have been the thing that led Duncombe and Seidelmann to encourage me to further my career, and they started pushing for me to go back to graduate school very soon thereafter.

And for a while, since I'd been out of school for a couple of years, I took some courses at the University of Maryland, not as part of the degree program but some of the physics courses that I thought I should have as preliminaries if I was going back into the astronomy program, which was then still part of the physics department. Remember that I had taken all these psych and soc courses as a senior so there was some upper level physics I should have taken but didn't.

And then I guess around '73 I applied for graduate school and I applied at the University of Maryland mainly because — for one thing I was still interested in radio astronomy and at that time there was starting to be some experimentation with very long baseline interferometry, which would make radio astronomy relevant to astrometry, which was what USNO's mission was.

And before that of course radio astronomy was almost completely useless to what USNO did because you couldn't measure the positions of anything very precisely. But VLBI was a new technology that was coming on that would actually make radio observations possibly relevant to what the Observatory did.

And the other thing was a very practical matter involving the Navy program that supported long-term training, including graduate work. Although a number of people, including you, Tom, had been able to go away for a couple of years to colleges away from the D.C. area, at this time that program had been whittled down pretty much and I was made to understand that I would get at most one year, two semesters full-time of out it.

DR. CORBIN: Well, this was post-Vietnam, and I know the budget was tight, although I always thought Strand did a good job at keeping us going. Especially since at other agencies you saw whole divisions and so on being axed. The only people we lost were Vicky Meiller's husband, who was the official photographer, a somewhat supernumerary position, and one clerk-typist in Admin. So he did well I thought.

DR. KAPLAN: Yeah. I never had much contact with Dr. Strand — at that time I was too low on the totem pole.

DR. CORBIN: I mean, his other problems aside, he did his job as Scientific Director, keeping — I didn't mean to get off on a tangent there but it was relevant to your schooling.

DR. KAPLAN: Oh, yeah.

DR. CORBIN: But that you were able to go to school was the important thing.

DR. KAPLAN: Yes, but with the restrictions on the program, I thought the school probably needed to be local to be practical, so I could do some work part-time. So I was accepted to graduate school, in Maryland's Astronomy Program. I think it was in '74.

It was understood I was going to be mostly part time, and I think I may have taken a semester or two part time and then got my two semesters of full time work there. And so it was two semesters, and at the time the Astronomy Program was giving its Ph.D. qualifying exam at the end of the first year of graduate school so that fit well.

I could go for basically two intensive semesters and then take the Ph.D. qualifier and that's what I did. And I did pretty well on the qualifier, but then I was back down to part time work. You know, that was my two semesters — I had expended them in course work and I was still taking courses part time, but I needed a Ph.D. dissertation topic that I could do part time.

Well, I had gotten involved with the VLBI group at Goddard Space Flight Center and this was under Tom Clark, and Chopo Ma was there, and of course he's still there. And a number of other folks from the University of Maryland were part of that group too so I learned a lot about VLBI. Ken Johnston, who was at NRL at the time and was doing his own VLBI experiments, also knew Tom Clark so there was a pretty good local VLBI community.

I understood there to be a meeting or conversation between Ken Johnston and Tom Clark about what I could do as a Ph.D. topic, although apparently I'm the only one who remembers this and I only know it secondhand. But the way Tom introduced the subject to me was that he and Ken had this conversation one day and were trying to think about what a good dissertation topic would be for me and they decided that trying to measure nutation, which was then a hot topic, through radio means would be a good project.

For one thing it would take a number of years to do because the nutation periodicities are fairly long, so you'd have to collect data for a fairly long amount of time, which would suit a part time dissertation quite well.

So that was the idea I was presented with and I was pretty enthusiastic about it because it was something that hadn't been done before.

It was measuring a natural phenomenon and it was a phenomenon about which there a lot of interest at the time. In the late '70's there was quite a number of symposia and there was active work on the theoretical side toward generating new nutation theories that could be tested if some very good observational material became available. It looked like, using the radio techniques, that we had a very good chance of doing much better than had ever been done optically in measuring the nutation components. So I was really enthused about this idea.

Now about the same time, Dr. Strand retired as Scientific Director of the Naval Observatory and Gart Westerhout was hired as the new Scientific Director. I guess this was 1977. Gart was coming from the University of Maryland as a radio astronomer, and it was just at the time when a number of new technologies were being developed that were applicable to what USNO did.

And not only was there radio astrometry, but lunar laser ranging and radar ranging to the planets, and also spacecraft ranging and Doppler velocity data was starting to come in. And all of this had the potential to revolutionize the kind of work that USNO did.

Now USNO in fact did not get involved in a lot of those technologies, which I think is unfortunate, but the one that we did get fairly heavily involved in was radio astrometry.

And at the time — late '70's — the National Radio Astronomy Observatory was building the VLA out in New Mexico and they had operated a four-element radio interferometer at Green Bank, which was a test bed for the VLA. The Green Bank interferometer was a wonderful astronomical instrument in itself, and produced a lot of good results on its own.

NRAO was seeking to basically put it to bed or see if another institution was willing to take it up. In any event they were building the VLA and the VLA was almost finished at that time, and they wanted to shut down the radio interferometer at Green Bank.

So with Gart now on as Scientific Director, and Ken Johnston at NRL having done a lot of work at Green Bank and published some papers on radio astrometry using the Green Bank interferometer, what happened was, was that the Naval Observatory in a collaboration with the Naval Research Lab picked up the costs for the operation of the Green Bank interferometer. This was the late '70s. I don't know whether it was '77 or '78.

And we started using that for measuring Earth rotation, UT1, and polar motion components. We could "see" all these variations with that instrument. And I started using it, the same data, to measure nutation.

I got fairly heavily involved with the group at USNO working with the interferometer data, even though it was mostly Time Service people. Besides Ken Johnston from NRL, and of course Gart, there was Bill Klepczynski and Dennis McCarthy, and a couple other folks from Time Service, and a couple other folks from NRL, in operating the instrument and coming up with the data analysis procedures for getting the data that we needed out of the instrument. The instrument and the software we inherited from NRAO were not really designed for precision astrometry, so there was a lot of work to be done.

For a while it looked like I might even transfer to Time Service because that's where most of my work was being done at the time but I decided to stay in NAO and work from there.

And part of that was because of my continuing interest in the computer system, and managing the computer system and modernizing it, which at that point was sort of a hobby of mine.

DR. CORBIN: Well I wonder if you weren't also influenced by the fact that, especially in those days, Time Service had a little bit of a reputation as being something of a crank-turning operation and once —

DR. KAPLAN: Yeah.

DR. CORBIN: I mean you certainly wanted to be in on the development of this but once it became an operating system, did you feel that there were more opportunities to go on with other things in the Almanac Office?

DR. KAPLAN: Yeah, the Almanac Office — I mean Ray Duncombe, and Ken Seidelmann, and Paul Janiczek, and Tom Van Flandern had always been very good to me in encouraging me to explore new things and not sink into a rut.

And so generally speaking I think the climate in the Nautical Almanac Office was very positive toward research oriented projects so that was a main consideration in my wanting to stay in NAO rather then going over to Time Service. Ken Seidelmann had become director of NAO after Ray Duncombe retired. What happened was, Dennis' group, you know, became an actual branch devoted to analyzing this data and eventually it became what is now the EO Department.

DR CORBIN: Yeah, and I think there was certainly more going on in that group by way of research and development.

DR. KAPLAN: Yes.

DR. CORBIN: But that was still on the horizon I think when you were —

DR. KAPLAN: Right, it wasn't really clear where things were going in the late 1970s. The Green Bank interferometer was experimental as far as our use of it. We weren't sure whether to stay with connected element interferometry or to go to VLBI.

And we had experiments and I was involved in some of the VLBI experiments at the time too. Time transfer using VLBI was another USNO-NRL project. I mean there were some really crazy experiments where we were running trucks around with atomic clocks, between USNO and Green Bank, and USNO and Haystack Observatory, and flying clocks from one observatory to another, and doing a lot of time transfer experiments with VLBI in those days. This is before GPS of course.

So yeah, there was a lot of experimentation of the uses of radio astrometry for the kind of work that USNO was interested in and it wasn't clear where we were going, whether the Green Bank interferometer was going to be a permanent part of us or whether we were going to VLBI or what.

But I was pretty heavily involved in the data analysis and just reprogramming — I mean we had inherited from NRAO the entire data analysis system for the Green Bank interferometer but it was very much oriented toward making maps of celestial sources.

It wasn't geared to astrometry and I recoded a lot of the stuff from scratch and worked with the other people in the group — Jerry Josties got pulled into the group at the time — in devising the whole system of data analysis to get the best data out of the instrument.

And in fact the data from that instrument over the four or five years that we operated it was the data that I used for my dissertation on nutation. I actually did not end up using VLBI data for that, even though I had started out sort of connected to the Goddard VLBI group.

Although the VLBI groups were getting interested in nutation themselves by that time, mostly the VLBI experiments were like one day, every couple of months. You can't do much with nutation with that kind of an observing schedule.

So certainly for a while around 1980, 1981, 1982, I had I'm pretty sure the only radio data that had been acquired that related to nutation at all.

And in the 1980s, there was this project called MERIT that I think was sponsored by the IAU and the IUGG, which involved comparing these techniques, the new techniques that had come on line, laser ranging, VLBI, connected element interferometry, and satellite ranging, all of which were being applied to the determination of Earth rotation.

The idea was to try all these out over a couple of years and see which one shakes out as being the best one for the future. So we were very involved in that project at the time.

And what sort of came out was that VLBI was more precise then the Green Bank interferometer data, and as part of project MERIT the VLBI folks did some intensive campaigns where they would observe for like a week or ten days straight. They could get observing time to do that.

So they started getting into the nutation business too as I was finishing up my dissertation. In fact by the time I really finished and got my Ph.D. in 1985, it was clear that the VLBI data was much better — possibly by an order of magnitude — than the Green Bank interferometer data for this and that VLBI was going to be the way to go for the future.

So my dissertation never produced any scientific revolution because it was — the technique itself was being sort of overwhelmed or superceded as I was finishing, by VLBI. But as I said, it was the first measurement of nutation by radio means.

DR. CORBIN: Well it pointed the way. I mean —

DR. KAPLAN: Yeah, and it showed — I mean I still got some interesting results, they just weren't quite on the level of precision of the VLBI results that were coming in at about the same time. I remember one meeting at Goddard where Tom Herring put up a slide of the VLBI nutation data, and it was so much better than what I had, I was depressed for weeks, and wasn't sure I wanted to finish my dissertation. But, yeah, it did show what could be done. Also the one thing of lasting value that came out of it — although it was only a small intermediate result that I didn't think much about at the time — was that my computation of the right ascension of 3C 273B at epoch J2000 became the basis for the radio system of right ascension for everybody. I think it has even been indirectly propagated, through a number of catalogs, into the ICRS.

And eventually USNO decided to basically put all its eggs in the VLBI basket and so I guess sometime in the 1980s, we discontinued the sponsorship of the Green Bank interferometer and started putting our resources into VLBI and that's how we ended up with the VLBI correlator being here.

DR. CORBIN: Now through this period, was this pretty much occupying 100 percent of your time?

DR. KAPLAN: No.

DR. CORBIN: Because I thought you were involved in other reference-framed questions.

DR. KAPLAN: Yeah. Also during that time, the early ‘80s, I was getting increasingly responsible for the computer system here. So if not in title but in fact, by the early ‘80s I pretty much ran the computer system, and in 1980 we got the IBM 4341, which freed us from the old 360 model 40.

DR. CORBIN: Was it that early? — I guess that’s right. I was always confused about that, whether we had gotten an upgraded 360 or whether the 4341 came in at that time, you know, as essentially a new system. I’m sure you know.

DR. KAPLAN: Yes. Now we had made several efforts to get a better IBM 360 system. We were very hamstrung by budget in those days. Our claimancy was an activity called Field Support Activity and they never understood what we did.

They weren’t really prepared to fund big science in any way. It was sort of a claimancy that had charge of a couple of random Navy facilities, I think like the Naval Academy, and there was us, and a couple of other random Navy facilities that nobody knew exactly how to classify.

DR. CORBIN: Well we weren’t doing much with the requirements. I mean we weren’t selling ourselves to the Department of Defense.

DR. KAPLAN: That’s right. No, we weren’t. And so the budget we had for upgrading computer systems through the ‘70s was always inadequate for what we needed. The 4341 was really the first wave of new computer systems built on micro chip technology, so the price was way lower then any comparable machine before it.

It was a brand new machine for IBM. It was built on the 370 architecture but in physical size, it was much smaller than any of their 360s. It was all air cooled because it didn’t have all of this heavy-duty electronics.

DR. CORBIN: I remember a discussion about putting plumbing in at one point.

DR. KAPLAN: Yeah. Oh, yeah, yeah, if we had gotten a bigger system — you know, there were water-cooled machines in those days. But the 4341 was sort of the breakthrough. As we were shopping around in the late ‘70s and we were looking for excess equipment too, that’s when the 4341 just came on the market.

I mean we got one of the first ones. I think we got the first one in the D.C. area, and its price was such that we could actually afford it. And its speed was much more than we had anticipated ever being able to get.

And the Saturday that it was put in, it was pretty much just a brain transplant — we kept all of our old peripherals. We had just hired Marta Goldblatt to be the systems programmer, a new position for us. The IBM guy took the old 360 CPU out and put the 4341 in, hooked up all the peripherals in exactly the same way, and we used our old operating system.

And people had widely believed before this installation that we would be without computing facilities for weeks as we would be debugging problems and getting the whole thing working. Anyway, up until this Saturday, which I think was March 29, 1980, people were frantically trying to get the last of their jobs done on the old 360 model 40 because they figured they were going to be out of business for weeks.

DR. CORBIN: Well, see, the previous CPU change had been 1410 to 360, and that really was a tooth pull.

DR. KAPLAN: Right.

DR. CORBIN: I mean that was a whole new system we went into.

DR. KAPLAN: Yeah, yeah.

DR. CORBIN: You guys made this pretty painless as I recall.

DR. KAPLAN: It was. The guy who installed it didn't have the typical IBM look and demeanor. Of course in those days IBM people just wore dark suits and white shirts with ties and they were always very clean cut people. You know, it was always strict business attire. Now the guy who came in to install the 4381 — you could always spot people in IBM who were indispensable to the company, who really knew what they were talking about, because they were able to disregard all of the dress codes and all of that.

The guy who installed the 4341 was like that. He smoked the entire time, was dropping cigarette ashes down onto the floor and everything, but he sure as hell knew what he was doing and he had that machine basically working by early afternoon.

We actually ran a few jobs on the 4341 that afternoon. I think Clay Smith may have wandered in and said something like, well, how's it going, and he had a bunch of cards in his hand. I said, well, give it a try. So we had the thing working, but of course it did everything almost instantaneously. You know, it was all totally determined by how fast the printer could print things out.

DR. CORBIN: Right.

DR. KAPLAN: So at the end of the day we said there's nothing else to do, we'll put Van Flandern's job on here and see what happens. And I had mentioned previously in the interview about Tom Van Flandern's lunar occultation predictions taking weeks and weeks of computer time. Well, by the next morning his weeks and weeks of computations were totally done. It was done! Done for the year. And so it was a very smooth transition and that really gave us a lot of boost in our computing power.

And we went on from there to — and of course Marta was the one who really bore the brunt of doing the dirty work on this — upgrading our operating system, putting in interactive terminals, and going to VM, which allowed us to run our old operating system at the same time we were running an interactive system called CMS. And it was a fun time to be managing the computer system because we were doing some innovative things.

Time Service was just getting into using fiber optics for intercommunication among the clock systems and so I worked with what I guess is now Paul Wheeler's group, with a technician named Dave Chalmers, and he and I worked together very well in hooking things up over the fiber optic network. At first, this was mostly using RS-232 connections, which were painful to get right.

But we started working with a company called Fibronics and long before anybody else was into this stuff we had a fiber optic terminal network all over the grounds that connected interactive terminals from building 1 and building 78, to building 52. And so yeah, it was a fun time, and I liked putting more and more computing power into people's hands, especially using this whiz-bang technology. I received the first Gillis Award in 1983 for my computer work.

And we were learning how to connect other devices. We had dedicated word processors in those days. I don't know whether you remember those things, called NBIs.

DR. CORBIN: Yes.

DR. KAPLAN: But we learned how to connect them up to the mainframe too so that we could get documents transferred back and forth. And this was when the Observatory was getting involved with the Hubble Space Telescope, at least the planning for Hubble, and there was some image processing work that was being done in NAO and AD. And we had a thing called the Ramtek image display device that we hooked up to the 4341, so it was fun because we were actually at the forefront of what people were doing with computers in those days and it was cool being on leading edge of some of this stuff.

DR. CORBIN: Well the great thing for me was when you got the dial-in capability going and Marta provided me with a fair amount of disk space so I could make those big catalogue compilation and proper motion runs from home.

DR. KAPLAN: Right.

DR. CORBIN: I'm not sure I would have gotten that ACRS project finished if it hadn't been for that.

DR. KAPLAN: And really that's how I finished a lot of my thesis work, was being able to queue up some of those analysis jobs from home, yeah.

DR. CORBIN: I don't know if you ever knew this. I was pretty friendly with Marta. Demetrios was always running stuff in the evening, and I would get on and try to run and things would just bog down. So, Marta gave me some — it was either a code or something that let me —

DR. KAPLAN: Get ahead.

DR. CORBIN: Set my job in a higher priority then Demetrios and I could go ahead. My stuff didn't hold him up much, but he was killing me because I had a lot of I/O and so on.

DR. KAPLAN: Yeah, I mean there were some funny parts of that whole transition to an interactive, multitasking computer system that people weren't used to.

For example, Time Service always had a dedicated part of the day that they used for their computations and soon after we installed the VM system we established some job classes so that different jobs could run at the same time without them interfering with each other.

Time Service just ignored all of this. They thought it would be cool to run all of their jobs as class C jobs, which meant that three or four of them could run at the same time. Even though we had told them at one time those are class A jobs, that you only want to run them sequentially.

Well they made them all class C jobs and ignored our advice. And I walked into the computer room one day. I think Carl Lukac was in there and he was going nuts because every ten seconds the system would tell him to swap out a disk pack and put another one on. And he had been doing this for ten or 15 minutes and it was literally a continuous shell game with these 20 pound removable disk packs and he was getting physically tired.

He says, I can't understand what's happening. So I looked at his job stream and I said well no wonder, you've got four jobs running. They all want different disk packs and you've got them all running simultaneously. I said listen to what we tell you.

So I finally convinced him to run these things as class A jobs, and when they did that, you know, it just flowed through exactly the way we had anticipated. But sometimes it was hard to get people to, you know, break out of their old habits in the way they did things.

Anyway, that's what I did in the early '80s; half was my dissertation and half was managing the computer system at the time and both of them were fun projects. I really enjoyed that time at the Observatory.

And as you mentioned I also got involved in some reference systems stuff. Of course, back in 1976, the IAU had passed a lot of resolutions regarding reference systems, and precession, and nutation, and the whole way that astrometric computations were to be done. I developed what later became the NOVAS subroutine system for doing those computations. This system was used in the software I developed for the Green Bank interferometer data analysis, and also for some of the new computations in the Astronomical Almanac, which had just been redesigned.

And USNO Circular 163, which I edited, described those IAU resolutions, and how they could be implemented, and what formulas you could use, and it became a very popular resource at the time. So, yeah, I was able to get into some other areas at the same time too.

DR. CORBIN: And by the time of the Virginia Beach meeting — I guess that was an IAU Colloquium wasn't it?

DR. KAPLAN: Yes, colloquium 127.

DR. CORBIN: You were a fairly major player in these areas as I recall.

DR, KAPLAN: I guess. Maybe at USNO, but not beyond, although I knew a lot of people. Gart Westerhout and I co-chaired the Virginia Beach meeting local organizing committee. That was in the fall of 1990, which is a little ahead of the story.

And what happened in the late ‘80s, was after I got my Ph.D., I was promoted to being a division chief of what was called the Astronomical Data Division in NAO. And that had cognizance over the computer system itself, and also in the way that we were distributing our data digitally.

And Tim Carroll, LeRoy Doggett, Ken Seidelmann, Susana Deustua (who was a summer student then), and I created the Floppy Almanac, which was a self-contained piece of software for the first series of IBM PCs. Each year was on a three-inch floppy disk, which I think in those days might have had a capacity of something like 256k or something like that. I think the first Floppy Almanac was produced for the year 1986.

So that was another innovative project where we were actually on the cutting edge. It was not the first astronomical program for PCs but it was the first precise almanac program that would actually give you really good answers comparable to what was in the astronomical almanac. That is, it was for professional use. USNO actually put out a press release on it, which was picked up by some newspapers. I guess news was slow then!

DR. CORBIN: And as I recall that became popular very fast.

DR. KAPLAN: It did, and unbeknownst to us, because as you mentioned at that time we had not really developed a good sense of what the Navy requirements were, the Floppy Almanac became very popular on Navy ships for reducing sextant observations.

And we had very little idea of how far it had gotten into the fleet until I went to what they called a “dockside” meeting in Norfolk, I guess it was in the early ‘90s, on nuts-and-bolts navigation issues.

DR. CORBIN: I think this was in the era when we were trying to firm up our requirements situation. Bob Harrington I know was involved in that.

DR KAPLAN: Yeah, it was clear we were having to do that, that we couldn’t be a sleepy little observatory and expect to get the support that we needed. It was also about the same time I guess Captain Roberts was here.

DR. CORBIN: ‘83 or ‘84.

DR. KAPLAN: Yeah, he was here I think four years.

DR. CORBIN: Yes, he got himself extended because he liked the place so much, but he was here before Anawalt, Hagen, and those guys. Hagen was — I remember the time of the Baltimore meeting, Hagen was a fairly new superintendent.

DR. KAPLAN: That’s right. And you might know this better than I, but I thought it was Roberts whose real project was to bring us under the Oceanographer of the Navy as our major claimant.

DR. CORBIN: I think that's right because the Ooeanographer had moved out here in the very early '80s and he saw some benefit in being attached to that group in order to facilitate the funding, because before that we were a separate line item under the CNO — I remember Jim Hughes going to a budget meeting, and they had these bar graphs showing the various line items and the USNO was a pencil line.

DR. KAPLAN: Yeah, right.

DR. CORBIN: Had no width on the graph, with these billion dollar projects next to it.

DR. KAPLAN: And I know this was a development that not everybody at the Observatory thought was a good idea but I think in hindsight looking at the way the budget situation did develop over the next couple of years, I think that was what we needed to do.

The Oceanographer at least understood what we did or was in a position to understand what we did much more then Field Support Activity or whoever our previous major claimant was, and he had literally a multi-100 million dollar budget. He had this whole fleet of ships that he had to support so we were actually a very small part of his empire and plus-ing us up a couple of million dollars was like nothing, it was like pocket change.

So I really think in the long run that the transition to being under the Oceanographer really benefited us, at least over the next ten or 15 years. Certainly some of the projects we got involved in we could not have done without that kind of support. We just would not have had the money.

DR. CORBIN: Yes, those of us left in building 1 were always irritated because they kept taking our office space, but I think in terms of what you're saying, you're right. I mean it really did —

DR. KAPLAN: Yeah, I mean that's always been — still is — an irritant. The game never changes.

But what was happening I guess in my situation at the time, talking about '85, '86, after I finished by Ph.D. and became this division chief, was that it was about the time that the Navy and the government in general starting looking at the whole business of computer management in a different way.

And this was in response to the PC revolution of the early '80s where everybody was buying PCs left and right and there weren't any controls, or guidance, or standards, or anything.

So the government as a whole, and of course that means Congress and the Navy in particular, were getting a lot more nervous about how computer systems were managed in general, and we started to see a lot more paperwork requirements, inventory control, five year plans, life cycle management, security, accreditations.

All this stuff started coming on top of one another and it became obvious to me that my job, if I stayed where I was, was going to be mostly doing paperwork, that basically the fun stuff was over and now it was time, you know, to put on the green eye shades.

And I thought pretty hard about this. And I went out to an AAS meeting. I think it was the one in Ames, Iowa, where I did a demo of the Floppy Almanac. I took a portable computer out, a very crude portable that Time Service lent me. You know, it was a like a three megahertz machine or something like that.

Anyway, on the plane ride back I sat next to Gart and was just sort of talking about my general situation, and he started talking to me about the optical interferometer project in AD. Mostly at that time I think Jim Hughes — and maybe to a lesser extent Steve Gauss — might have been involved with it, but the project was mainly that of Mike Shao from CfA. The interferometer itself was on Mount Wilson.

And the long and the short of it was that since I had had a lot of experience in radio interferometry and that basically, both conceptually and algorithmically, the problem was pretty much the same in optical astrometry, I did some computations for that group and eventually I actually transferred to AD for a couple of years to work on that project. That was from I think late 1986 to about '88 or '89.

DR. CORBIN: Was that when you essentially had a position that was Gart's assistant?

DR. KAPLAN: That was later.

DR. CORBIN: Oh, that was later, okay.

DR. KAPLAN: Right, right. I was a regular member of the AD Department. My office was down in building 52. I think it was Geoff Douglass' old office I took over then. And I was sort of not well integrated in the department because the project was so — there wasn't anybody else really other then me and Jim that got involved in that project in any depth.

So it was mostly me flying out to Mount Wilson for about ten days every three weeks or so for a couple of summers when we were doing observations and doing data analysis and programming for it, you know, when I was here. It was hard since everybody on the project was a workaholic.

Despite the pain, I learned a lot on that project. I had never been involved in an instrumentation project before, and the people involved, especially Mike Shao and Mark Colavita from CfA, who were the brains of the outfit, those guys are really smart. Here at USNO, John Pohlman was involved too because the shop was doing a lot of the instrument work — they were the only ones with the expertise to do the kinds of precise machining that the project needed. And John Hershey, who was a member of the AD Department, got involved in it, too.

DR. CORBIN: Well, it seems like Johnston used to come in and talk to Jim about it too.

DR. KAPLAN: Yes, Johnston was involved — of course he was still at NRL at that time but that's right, he had some people. He had Dave Mozurkewich and a few others from his group. Mozurkewich was heavily involved, and he's another really smart guy. He spent a lot of time on Mount Wilson.

So I mean we all spent a lot of time out at Mount Wilson for the couple of years that the project went along, and of course by the end of that period of time we were starting to think about a more permanent Navy facility for optical interferometry.

And an engineer Penny Lemire was hired. I think she was actually a USRA employee or something like that. She was not a government employee but Jim hired her and I remember Penny and I tromping around some of the mountains on the west coast near Big Sur looking for an adequate facility to put a Navy Interferometer.

DR. COBBIN: Well Anderson Peak I remember —

(END OF TAPE 1, START OF TAPE 2, SIDE A)

DR. KAPLAN: Yes, that's where we did the tromping. So after a couple of years of working in that project, the travel schedule really got to me. And, I thought, well if I'm going to continue with the project, I probably ought to move to California. But it really wasn't clear that it was going to stay in the Los Angeles area anyway. The whole future of what we were going to do was very much up in the air.

DR. CORBIN: Jim Hughes and Gart had endless arguments about that.

DR. KAPLAN: Oh, yes, yes. Well, Jim and Gart had endless arguments about a lot of things.

DR. CORBIN: Well that's true.

DR. KAPLAN: But I was looking to get out of the project at that point and Gart had said that he was looking for someone to serve as his assistant. So I was a little bit nervous about that because it seemed like a lot of administrative stuff. But on the other hand it was something new and I thought I could learn about what the Observatory was supposed to be doing for the Navy.

So I signed on with Gart for this job. So I was his assistant for something like a year and a half or so. This was about 1989, 1990. My title was Assistant to the Scientific Director, which Gart made sure that everybody understood was not Assistant Scientific Director or Deputy Scientific Director.

DR. CORBIN: Or the next Scientific Director.

DR. KAPLAN: Right. No, I was the go-fer for the Scientific Director but I did get to see some different aspects of the Observatory. He had me pretty heavily involved in the write-ups, and the reports, and everything for the 6.2 money that we got from ONR.

DR. CORBIN: Research funds.

DR. KAPLAN: Right, which at that point was like I think about 1.3 million dollars a year and that went to different projects in a lot of departments. Actually it was distributed fairly widely.

And so that got me into requirements — because ONR was increasingly demanding more justification for where they spent their money and how this related to what the Navy's mission was and everything, this was sort of my first good look at what the Navy really is looking for in long term, in terms of their requirements and how we should be supporting them and all of that.

So there was certainly a lot of stupid paperwork exercises involved, but I did in that job get a wider perspective of what we should be about, and how we should interact with the Navy, and what people expected of us in a wider context.

DR. CORBIN: Now, did you work with Harrington on requirements?

DR. KAPLAN: A little bit. Yes, that's right. At that time Bob was I think the requirements guy, at least on the civilian side. I worked with him a little bit on that. Gart had hired — I think he regretted doing this — but he had hired two guys, I forget their names, recently retired from the Navy, both were Navy officers, to do a requirements study. Supposedly these guys had a lot of connections in the Navy, and could ferret out what we needed to be doing.

Now this study cost like a couple hundred thousand dollars and basically it didn't tell us anything that we didn't already know. They got most of their information from talking to us so it had just parroted us, what we already knew.

And I remember being in Gart's office one day when he showed me the document that had come out of this couple hundred thousand dollars. I said, Gart, I could have written this up myself for half the amount. He says, I know.

So we were very much trying to find our way in the requirements area at the time and I guess that was about the time that we first started having a naval officer assigned to us with the job of being the requirements officer.

DR. CORBIN: And I think this is when I started to get involved, when Bob died.

DR. KAPLAN: Probably so, yeah. That's correct, that's right. That's what happened.

And the other thing that I was working on at the time — since I had all of this experience with the computer system — was the LAN working group. This was a committee that had been formed to look into local area network for connecting all the PCs and many computers and everything that had sprouted up around the Observatory.

And I think Neville Withington was the chair, but the group was Neville, Steve Gauss, John Bangert, maybe Rich Schmidt, and me. Marta Goldblatt was still here at the time but not on the committee. Also Dave Nutile was involved in some way, possibly in the working group. Anyway, the task was to draft up a document, the statement of work, to establish a fiber optic local area network around the Observatory grounds, connecting all the computers, big and small, and that's what really became the network that we currently use.

DR. CORBIN: Are you going to go on to something else now? I was going to ask something of a backpedaling question.

DR. KAPLAN: Okay.

DR. CORBIN: Back in the ‘80s, and I think it was mid to late ‘80s mainly, when you were in charge of the division, handling the computer and data operations and so on, as I recall you were the one who had to more or less orchestrate the big conversion away from punch card operations and storage.

DR. KAPLAN: Yeah, that was in the early ‘80s, actually.

DR. CORBIN: And I mean that was a pretty big deal here. We put a lot of man-hours into getting all that done. Of course it had to be done, but I didn’t know if you had any comments on that.

DR. KAPLAN: There were a lot of bumps in the road. I mean, the first time we hired a computer operator, you know, that didn’t go over well with everybody because people were used to running their own jobs.

But once people started migrating to interactive terminals you almost had to have an operator because if you’re sitting at a terminal in building 1 and needed a tape mounted, you need somebody to do that, and some standard way of doing making the request.

And that meant that you had to have some standard tape cataloguing system. And everybody had their own names for their tapes and knew where they were, and now we were sort of trying to impose a numeric system on people so that the operator could find the tapes.

So there were a lot of that kind of bumps in the road in transitioning from a sort of mom and pop operation, which is the way we ran the 360 model 40, to a more modern operation where people were actually more productive. But in some sense I think a lot of the folks that had been around for a long time missed some of the old ways of doing things, being able to be your own computer operator.

And the other thing — Ed Jackson has commented and I think you have commented on this a number of times — that in the old days the 360 model 40 was sort of the village well.

DR. CORBIN: Right, that’s what we called it.

DR. KAPLAN: That’s right. We had what we called batch sessions where anybody with a program that didn’t take longer than five minutes could come in and run their job.

And this was a great gathering point in the morning and the afternoon for people and, you know, a lot of gossip, and that’s how you found out what was going on in other parts of the Observatory.

And that’s really how I met people like you, and Clay Smith, and Dennis McCarthy, and got interested in what was happening in some of the other divisions in the Observatory. And of course once we started this interactive terminal network we didn’t have that anymore.

DR. CORBIN: And I think we have become a lot more fragmented as a result.

DR. KAPLAN: Yeah. It's always been a problem at USNO, the fact that we tend to have fairly high and thick walls around what each department does and there isn't enough inter-communication among departments.

And one of the things in my career, I've always been interested in what was happening in other parts of the Observatory and I've tried to get involved in projects that were a little bit outside of what I'd been doing before, rather then to fall into a given specific line of expertise and just keep doing the same thing.

I guess I could be regarded as somewhat of a dilettante in this area but I've had a lot of fun, and if you look at my publication record you see some titles of some pretty different things, from radio observations, to optical interferometry, to planetary orbit calculations, to Earth rotation, and you know, computer applications.

There's a whole lot of stuff there and overall in my career I guess I've been fairly happy that I've had continual intellectual stimulation from trying to navigate around different projects in the Observatory and learn new stuff.

DR. CORBIN: Well, back in the days before we came, people, especially the newer people, were transferred around for a while to the different divisions.

DR. KAPLAN: Yeah, and I always thought that was a great idea.

DR. CORBIN: Well, I'm sorry, I didn't mean to interrupt.

DR. KAPLAN: I think we're probably — we're up to the early '90s, 1990, at the point at which the next step would be when the AA Department was formed and probably this is a good point to stop.

DR. CORBIN: Want to break? OK.

THURSDAY, JULY 7, 2005

Dr. Corbin: Well, today is July 7, 2005, and we're continuing with the interview of Dr. George Kaplan. I'm Tom Corbin.

George, do you want to just pick up where we left off?

DR. KAPLAN: Sure. We left off — I guess it was when I was working for the Scientific Director, Gart Westerhout. That job was really only about a year and half or two years, I guess 1989 into 1990, when I worked as the assistant to the Scientific Director.

One of the things that happened at that time, the captain at that point was named Hagen, James Hagen, and I think he was the last of the captains that we had who was a regular line officer. He was a submariner, and I think all the captains since then have been from the oceanographic community.

But I know some people regarded Hagen as sort of a blowhard and he could be — you know, he certainly had his own ego about him and he wanted things done his way, but he was a pretty smart guy.

And there were lots of times during the time when I was assistant to Dr. Westerhout, when Westerhout had several surgeries. I think he had a hip replacement surgery at the time, and I think he had bypass surgery, heart bypass surgery.

DR. CORBIN: It was extensive wasn't it? Quadruple or something like that?

DR. KAPLAN: Yeah, he was out for quite a while. So during that time I had the opportunity to talk to Hagen.

Now Captain Hagen's big thing, big push and big emphasis was that the Naval Observatory was not being responsive to Navy requirements, that he wanted us supporting the war fighter was the way he put it.

And for most of us who had been at the Observatory for a long time, that was a fairly strange phrase and people who were in one scientific specialty and had sort of stayed in one scientific specialty for a good long period of time, there's a certain resentment that this guy was trying to shake things up and make us move in different directions.

But I think he regarded it as the way that the institution was going to survive long term, that we really had to be more, as I think he would put it, more customer based, and that we had basically turned too much inward, that we were too concerned with really the science of what we were doing, which we were good at, but we were not really reaching out to the community that was supporting us.

DR. CORBIN: And if I can just inject something here: I had a lot of talks with Jim Hughes about this at the time. There was a feeling that we were in many ways already producing what the Navy needed, but we were not letting them know about it. So, a lot of our product was going through contractors whereas the Navy could have gotten it directly, DoD could have gotten it, directly from us.

DR. KAPLAN: Yes, that kind of thing was pervasive — the whole business of having contractors deliver to the Navy what we were originally producing. It was not just star catalogs, it also was Earth Orientation. A lot of stuff was coming into the Navy from the back door that they were paying for, that they could get directly if they had just walked in our front door. We would have just handed it to them and provided some expertise in how to use it properly.

DR. CORBIN: And it made us invisible because of course the contractors often wouldn't make it clear where they had gotten the information and data.

DR. KAPLAN: Of course not, right. That's right. So I think since then, since Hagen's time, there has been much more of an emphasis — I mean each captain since then has been a little bit either more or less oriented in this direction, but there has been more of an emphasis on trying to determine what the Navy requirements are and how we can feed into that and become a lot more visible than we had been with the Navy.

DR. CORBIN: Did the astrometry forums start under him? I forget.

DR. KAPLAN: They very well might have. One of the things that he did, and this takes it to sort of the next point in my career, was that he looked in particular at the old Nautical Almanac Office and thought it was not sufficiently productive and customer oriented.

At that time NAO had gotten wrapped up in some pretty odd stuff like the Hubble Space Telescope astrometry, which was out of the main line of what NAO did and really didn't have good expertise there, and there were some other activities that NAO had gotten into that I think Hagen sort of regarded as wheel spinning.

So his decision was to actually form a new department, to split NAO up into two departments, one of them called Orbital Mechanics that would pursue the dynamics part, the celestial mechanics aspects of the old NAO, and another new department called Astronomical Applications, which would essentially sort of be the front end of a lot of USNO products. That's where the almanacs, the Astronomical Almanac, Nautical Almanac, and Air Almanac would be, would reside.

And software products would have just gotten started with the Floppy Almanac and there was a new product, I guess at the time it was nearly completed, called MICA, to replace the Floppy Almanac. Bt the idea for the new department was generally to try to make more contact with the fleet and figure out what sort of products they needed to do their job, that USNO could supply.

So Dr. Ken Seidelmann, who had been the previous director of NAO, the old Nautical Almanac Office, became at that point the new director of the Orbital Mechanics Department. And it was a fairly small department, had about five people in it. I guess it was Alan Fiala —

DR. CORBIN:Hilton was in it?

DR KAPLAN: James Hilton, Dan Pascu.

DR. CORBIN: Who was the fellow that went out to Arizona who —

DR. KAPLAN: Well there was Hal Levison. They hired Hal Levison. Oh, and Peter Kammeyer, who had been recently hired on. So those guys were given some offices in building 56 — oh, and Jim Rohde.

And the new department, the Astronomical Applications Department, was put under Paul Janiczek as the director and they asked me whether I would like to become deputy director, and I said yes.

So I moved physically back down from the main building to building 52 again, in fact, to one of the offices that I had first occupied when I came to USNO shortly after the old NAO moved to building 52. I was back in that same office — room 305 — again.

DR. CORBIN: What year was this?

DR. KAPLAN: This was the end of 1990. So September of 1990 was when the new department was established. So of course there was a lot of — you know, the whole first year there was a lot of disruption, and uncertainty, and not knowing who was responsible for what among the two departments.

But gradually it got straightened out and the new department, AA, actually made some decisions to drop some things that we had been doing, like lunar occultation predictions and detailed solar eclipse circulars.

So there was a gradual sorting out and a sort of reprioritization of what was going to take place in the two new departments.

Orbital Mechanics started getting more interested in artificial satellite orbits, GPS and other satellites, and Ken Seidelmann in particular I think got involved with the Air Force in developing some standards for orbital calculations for artificial Earth satellites. So I guess in a sense both departments got more customer oriented in Hagen's way of thinking.

So I was deputy director of AA then until 1994, when under another captain — Blumberg I guess it was — there was another reshuffling. Orbital Mechanics was abolished as a department.

Ken Seidelmann became the Director of Astrometry. There were two directorates set up, the Directorate of Time and the Directorate of Astrometry and I guess Winkler became the Director of Time, and Seidelmann became Director of Astrometry. The individual departments in the Observatory all got put under one of these two Directorates. I guess by this time Gart Westerhout had retired and Ken Johnston, formerly of NRL, had come in as the new Scientific Director. So we established another layer of bureaucracy, which is now gone. And those who had previously been called department directors then became "heads".

DR. CORBIN: The thing about Time was they basically split the old Time Service, which had kind of de facto split itself by then.

DR. KAPLAN: Yeah, that's right. I guess that was when Earth Orientation formally split off from Time Service. Although as you say that group was already pretty independent, and was in building 52.

DR. CORBIN: But in the case of Director of Astrometry, Ken got both the Astrometry Department and Flagstaff and there was supposed to be some improvement in the relationship and coordination between the two departments.

DR. KAPLAN: Yeah, which has never occurred actually.

DR. CORBIN: Well Astrometry and Almanac have always worked pretty well together.

DR. KAPLAN: Yeah, we were grouped there under Ken too. The other thing that happened as part of the 1994 reorganization was that the IT Department, Information Technology Department was formed. Previously NAO and then AA had been sort of the gurus of computing at USNO.

Well, computing in the early '90s was taking a totally different turn. AA had assumed from NAO the responsibility for the mainframe, which at that time was an IBM 4381, but early in the '90s we got some sort of a directive that basically the Navy was consolidating the mainframe computing locally at the Navy Yard and we were told that we were not going to be able to continue to maintain our own mainframe system.

By that time a lot of the computing at USNO had naturally migrated onto either PCs or Unix workstations anyway so one of the jobs, and this is one of the things that I helped oversee at the beginning of the '90s, was trying to make sure that all of those old IBM 4381 applications got migrated onto some other machine, so that we could retain our computing capabilities even though the mainframe was going away.

So that was a very busy time for a lot of people, moving applications off the 4381 and learning Unix, and making sure, you know, that everything worked. It was not just a matter of recompiling a few programs, but there were entire libraries, there was entire datasets that had been used for years that were specifically geared toward the IBM 360/370 system environment that had to be migrated over.

So is was a very busy time in the computer area and also about that time we were beginning to get the first of our local area network installed to connect all our PCs together and also the workstations, and initially that was when we became involved with e-mail and shortly thereafter early versions of the Internet.

So Neville Withington, who had actually come to the AA Department from the Earth Orientation group to supervise our computer division, in '94 when the IT Department was formed, she became the head of the IT Department then.

DR. CORBIN: Just a small point. I thought she never was allowed to transfer to Earth Orientation. She was not very happy about it, and that was one of the reasons she was eager to leave.

DR. KAPLAN: That's a good question. I don't know what the sequence of events was. I guess you're correct. Earth Orientation was formed as part of that '94 reorganization. Yeah, it must have been, yes.

DR. CORBIN: But as I recall she did not want to stay in Time Service, she wanted to go into Earth Orientation.

DR. KAPLAN: But I thought she had basically moved over to — I mean Dennis McCarthy had the Earth Orientation group that was in building 52, even before it was a department. And Neville was physically there.

DR. CORBIN: Well perhaps it was prior to the official establishment of EO.

DR. KAPLAN: It could have been, but I thought she had moved to building 52 and was in Dennis' group before it became a department and — I mean we had to do some cajoling to get her to move to AA in 1990 as I remember.

But the other thing that I became involved in at this time, and this started when I was working under Gart, was I became chair of the Executive Committee of the Washington Area Astronomers Meetings.

And this was a group, a committee of people from local astronomical institutions, NRL, University of Maryland, Goddard Space Flight Center, Space Telescope, Johns Hopkins.

And I think later on we actually had representatives from George Mason, plus some representatives from some of the independent organizations around here. We organized twice-a-year astronomy meetings, each a one-day meeting, mainly for local astronomers, with just mixed papers, just totally random subjects. This had been going on for quite a while before I got involved with the Executive Committee.

You know, at one meeting typically 80 to 100 people would show up and the idea was sort of just to get everybody to know everybody else, and also the mix of papers took you out of your field and gave you a little bit of a feel for what else was going on beyond your particular specialty.

So those were sort of fun but the meetings had sort of become an anachronism, and eventually I guess it was either in 2000 or 2001 when we discontinued them and abolished the Executive Committee.

But they had started at a much earlier time when they were called neighborhood meetings, at a time before the Internet and before the time that air travel was very easy, and establishing local connections among your colleagues was much more important then.

Nowadays with e-mail, and the Internet, and jet travel, and more conferences then we have time to attend, international collaboration is extremely common and the physical location of people has become rather unimportant. But in the old days that was not the case and that's how these meeting grew up.

But they were still sort of fun and they had a devoted following. But one of the reasons they didn't continue beyond 2000 was that we were never able to develop a good contingent of younger astronomers who enjoyed the meetings. The postdocs and the graduate students were all very much burrowed down into their own field and didn't really want to take a day to just meet other local people. That was just not an important thing to them.

So we sort of had an aging group of people who were interested in the meetings and attendance gradually declined and so we ended them in 2000. But they had a good run and it was a fun thing to organize, and I always enjoyed those meetings and learned an awful lot too.

But back to USNO, I was involved in the computer transition. So there was a day when they actually wheeled the 4381 out of the building.

There was one specific date — I think it was '92. It was the end of one specific fiscal year where, on September 30th, we turned it off and it was never turned on again. And shortly thereafter it was actually wheeled out of the building and the computer room then became mostly empty, although it became filled with network equipment shortly thereafter.

DR. CORBIN: As I recall there was another episode in this period of the early '90s that you were involved in, and that was when we lost Jim Hughes and Clay Smith.

DR. KAPLAN: Yes, right. Yeah, good point. In the Astrometry Department at that time there were several deaths in rapid succession, all due to cancer. I guess Clay Smith who had been battling prostate cancer for a long period of time and apparently successfully for most of the '80s, but finally —

DR. CORBIN: But Jim died first.

DR. KAPLAN: Jim died before Clay?

DR. CORBIN: Yes, Jim died right after the '91 IAU in Buenos Aires. He died about January 2nd or 3rd of '92.

DR. KAPLAN: Yes, it was very sudden. I think he was at work that December.

DR. CORBIN: In and out, and he felt poorly all fall.

DR. KAPLAN: In and out. Right — I think he was diagnosed with cancer not until mid December, and he was gone within a month.

DR. CORBIN: It was esophageal cancer and they didn't really identify it until, I think, after Christmas. And they were just putting together a treatment regimen when he died. They never even treated him. But it was misdiagnosed as some kind of bronchial infection all fall.

DR. KAPLAN: Yes, that's right.

DR. CORBIN: Then he was taking antibiotics, and he was having trouble swallowing and clearing his throat all the time. Yeah, that was handled poorly by his doctor.

DR. KAPLAN: And I guess what happened after that was that Clay Smith became the department director.

DR. CORBIN: Right.

DR. KAPLAN: But then shortly thereafter then his prostate cancer flared up again.

DR. CORBIN: Yes, it had metastasized.

DR. KAPLAN: Yeah. So he was only actually department director for a month or something — two months.

DR. CORBIN: Well I think what you're probably thinking of is that he was in here only a couple of months after becoming director, but he lived until May of '93. And so he was director for almost a year but for a lot of it he was bedridden.

DR. KAPLAN: That's right, but they did — at some point along the way they did shift him out of the director position into a non-supervisory position so that they could go out and re-hire a director. And I mean I think during that time, you and —

DR. CORBIN: Steve Gauss.

DR. KAPLAN: And Steve Gauss and Bob Harrington were serving as acting department head.

DR. CORBIN: No, Bob had already died.

DR. KAPLAN: When did Bob die?

DR. CORBIN: Bob died at the beginning of '93. He died before Clay.

DR. KAPLAN: That's correct. By that time I think you and Steve had cycled through being acting director like two or three times, and they'd actually have to pay you more money if they did it again.

DR. CORBIN: Can't have that.

DR. KAPLAN: So we can't have that. So they actually pulled me up from AA on a temporary basis to be acting director from late January of '93 — I remember just being told a few days before Bob's funeral that that was going to happen, and that lasted I guess until first couple of days in May of '93.

DR. CORBIN: I thought it took a little while after Clay died to fill the job. I don't remember when Steve became director.

DR. KAPLAN: He succeeded me directly. That was during —

DR. CORBIN: Right, but I don't remember what month it was.

DR. KAPLAN: I'm pretty sure it was early May '93. Yeah, so actually I was — that's right, I was acting head of AD at that time for three months.

I think one of my proudest moments at USNO was during that time. You probably remember this. Larry Taff, who was an astronomer at Space Telescope and had done some work in astrometry — and I think basically he thought that everybody else who worked in the field was a hack and that only he really knew anything about it — called Gart Westerhout during that time and said that in one of our catalog products, and I forget which one it was, there were all of these systematic errors that he had found and he wanted to come down and show us all of the — you know, essentially he wanted to show us all the mistakes that we had made and he wanted to do it that afternoon.

And so Gart was all in a tizzy about this. But a bunch of us got together and started looking at the plots that Larry had sent down to Gart and it looked a lot to us like an error in precession.

And I remember scrambling around that morning doing some simulations myself on the computer, and by the time that he arrived in early afternoon, we were almost certain that he had made an error in precession.

And of course he denied it at first. He had used the new precession but what he had forgotten was to make the appropriate compensation in the proper motion system, so basically his proper motion system was carrying the errors in precession.

And so we were pretty nice to Larry considering the fact that he was seldom very nice to anybody else that he tangled with. But he definitely left with his tail between his legs that day.

DR. CORBIN: That became so famous on the grounds that even people who weren't at that session all knew about it.

DR. KAPLAN: Oh, yeah, yeah. I remember it well and I think everybody was happy that we were able to send Larry away empty handed so to speak.

DR. CORBIN: Well you mentioned earlier, in the previous session of our interview, having been involved in a lot of different areas of the work here at the Observatory. This was the kind of thing that people like you were eminently well suited to deal with not only because you had worked with dynamically based quantities such as nutation, but you also had a lot of familiarity with star catalogs, time systems, etc., and so you could put a problem like this together and see the correlations. I think it was a good example of what that background produced.

DR. KAPLAN: Yeah, Gart seemed pretty impressed that we were able to put that whole thing together in really just a few hours time because he thought we were going to get walloped on the head here, but as it turned out we actually did know what we were doing, much to Gart's surprise I think.

So then it was in the first couple days of May of '93 that Steve took over as head of the Astrometry Department, so I went back down to being the deputy head of AA, and moved back down to building 52.

And at that time AA was starting to get into doing an upgrade to the old Floppy Almanac, which had some celestial navigation features on it but would not actually do a fix for you. It would do all the astronomical calculations and tell you where the stars were supposed to be but you still had to plot a fix on chart paper.

(END OF SIDE A, START OF SIDE B)

DR. KAPLAN: As a result of one of those dockside meetings in Norfolk, we had gotten a requirement for a piece of software that would actually do the entire navigation solution for the Navy and this became what's now called STELLA. Actually, I was the one who dreamed up the acronym for that, which is — I think it's System To Estimate Latitude and Longitude Astronomically.

But when I came back from being the acting head of AD, development of that product was starting to take place, the initial design.

John Bangert was head of the software development group and the question was how to deal with what are called moving fixes, which are pretty much any fix you take — it's very seldom the case that you're standing still when you're doing a star fix. The ship is always moving and there's always the question of how to deal with that movement. And there are some clever graphical techniques that had been applied to the problem for what's called a running fix and it basically involved shifting lines of position on the graph.

And either Paul or John asked me to sort of look into how we should deal with this particular aspect of the problem. What it came down to was that I pretty much looked at the problem from the beginning and saw that a lot of the techniques that were used for running fixes were actually approximations.

Some of them are very clever and very good approximations but it seemed to me that if we were developing new software for the Navy that we ought to look at the problem ab initio and do it right. We had the computing power to do it right so why not.

Paul Janiczek himself had done a very clever matrix method for reducing a fix, but there again it assumed you were in the same position and observing all three stars. It was very elegant — I mean the whole thing fit on one page. He published it in *Navigation* and it's a very elegant solution to the mathematical problem.

So we wanted to incorporate that but we also wanted to incorporate the running fix, some sort of running fix solution.

So in looking back over the literature, I developed some algorithms that would first off accurately plot the course of a ship across the oblate Earth. I was very surprised that there weren't good formulas for this that had been developed previously. It's very closely related to the Mercator map projection problem.

So the first thing I did was develop formulas for latitude as function of time and longitude as a function of time, assuming that a ship was sailing on a loxodrome, a rumb line course, a fixed heading course at a constant speed, and it took the oblateness of the Earth into account.

Well, once you have that then you can sort of do the running fix problem properly, because you can develop the partial derivatives for star position in the sky as a function of time, due to the ship's motion.

So I've worked on this and developed the equations for a least squares fit that would allow you, if you had enough observations, to solve not only for your latitude and longitude at some specific instant of time but also for your course and speed, because the ship's motion had became part of the problem. It was integrated into the problem from the beginning.

It wasn't an add-on correction or something that you slapped on afterwards to correct the observations, or to correct the lines of position on the chart. It was something that was built into the problem from the beginning.

And then later on I generalized the algorithm to also accommodate changes in course and speed so that you could actually do observations over several legs of a voyage and combine them into a single solution.

So these were published as a series of three papers in *Navigation* and the algorithm was incorporated into STELLA, which is where it is today.

And I mean for the simple situation where you've got just three or four star sights that you do in twilight, the whole thing reduces to a traditional fix but the mathematics are there to accommodate many more observations, if you can collect them, and to do it at a much higher level of precision.

The old algorithms were generally geared to sextant observations that were good to about an arcminute, which is about a nautical mile on the surface of the Earth. But everything in STELLA is down at least to the arcsecond level so it could actually — the same set of algorithms could be used for some sort of an automated device that could take observations of stars automatically, large numbers of these, and combine them into a single solution.

So that's some of the work that I did in the mid '90s. Those papers were published around '95 I guess, '96 in *Navigation*, the journal of the U.S. Institute of Navigation.

And STELLA was sent out to the Navy, I guess it was about '95 or something like that, when the first version of STELLA went out, and it was extremely successful and so every ship in the Navy now has a copy of STELLA and they are teaching it in the navigational schools. It has become well integrated into the fleet.

And about this time as a natural adjunct to this work, we began advocating for the development of a navigation sensor device, a device that actually could see stars and measure their positions, daytime and nighttime, so that the algorithms that we had developed would have sufficient numbers of data to work with, and give a much better fix.

And because this was shortly after the GPS constellation had been completed, we could hardly get a hearing on anything that wasn't GPS. I mean everything was GPS this, GPS that. You'd go to a navigation conference, and GPS was all anybody was talking about.

So despite the fact that the Navy often tries or at least says they want to encourage people to think out of the box, if you think too far out of the box nobody wants to fund it or hear about it.

So we did a lot of briefings on this idea but were never able to get it off the ground because it was basically just an idea. This was mainly John Bangert and I, encouraged on by Commander Chris Gregerson, our requirements officer. We knew the technology was available to do it and that there had been some work done in the late '80s on this kind of thing but we didn't have any real engineering tests.

And that's what everybody would say. Come back when you have some engineering data. Well, you can't get engineering tests done if nobody gives you any money. So it was a chicken and egg problem and we never got very far until just a few years ago actually, when we linked up with SPAWAR in San Diego. SPAWAR had been tasked with implementing STELLA on a Navy navigational computer called NAVSSI, and one thing led to another.

And of course what's happening in the meantime is that people have gotten a lot more concerned about GPS deniability and jamming and it's been found it's pretty easy to jam GPS. And so there is a major industry now involving countermeasures to GPS denial, and upgrading equipment so that it's harder to jam GPS, and alternative navigation schemes to carry you forward if GPS is denied.

So it's within this newer context that we were able to actually get some funding to get some engineering studies done, and feasibility studies done. And so that has worked up to right now where we have two contractors on the west coast, one's developing a day/night star sensor in the visible, and another is developing a day/night star sensor in the infrared.

And they are at the point of actually building these devices and the end product is supposed to be a prototype device or two prototype devices that will give you your latitude and longitude automatically, just from stellar observations.

So the first of those tests should take place within a couple of months. So the whole navigation thing, an automatic stellar navigation system, has been something that I've been involved in now for ten years.

Now it's taken on an even more important role because GPS of course doesn't really give you a good absolute azimuth reference. It can tell you what direction you're going but it can't tell you which way your ship is pointed. Sometimes that is important — in particular, the Navy Aegis system, which is a high powered very precise radar system that's being used for one aspect of the ballistic missile defense system, needs an absolute calibration of its radar direction. That's because some of these systems involve handing off coordinates from one ship to another that might be hundreds of miles away, so they work in what they call an Earth-centered Earth-fixed coordinate system.

So they have to know in absolute terms where their radar is pointed and it turns out a stellar reference is one of the few ways that they can get a decent azimuth determination.

So now our automated navigation system that was really designed for GPS denial situations has taken on another, possibly even more important aspect, in terms of being a good calibration system for azimuth, absolute azimuth.

And of course the Aegis ballistic missile people have got just tons of money to throw at this problem — it's a multi-billion dollar program so when we ask for a couple hundred thousand dollars to keep this thing going it's pretty much pocket change for them.

So in fact John Bangert, Sean Urban, and I are going out to another meeting on this very topic in San Diego next week.

DR. CORBIN: How well do you envision it performing these functions?

DR. KAPLAN: Well the specifications for the contractors that are working on it now are an arcsecond. The contractor who is working in the infrared is several months, probably almost six months ahead of the one working in the visible, and they are now routinely taking star measurements day and night and they're definitely able to centroid stars and measure their relative distances, arcs on the celestial sphere, to sub-arcsecond accuracy.

Now when you have to combine that with a tiltmeter that gives you your vertical reference for latitude and longitude, that's where the rubber meets the road. And we don't have an answer on that but in terms of just measuring star positions within the sensor's field of view, they're doing that at sub-arcsecond accuracy.

DR. CORBIN: Because there must be problems like the mechanical mounting of the instrument on the ship and so on.

DR. KAPLAN: Absolutely, and the whole transformation of the coordinate system from the focal plane array that they're using to something absolute on the ground where their tiltmeter is, is obviously the next step in the process and doing all those coordinate transformations and getting all the systematics out is going to be a challenge, yeah.

But that's a project that I've been involved in, in various ways over ten years and it's very gratifying to see something that started off as just a purely theoretical exercise, develop into not just hardware but hardware that actually might be used for something quite useful and needed in the national security area.

DR. CORBIN: Now is this something that was mostly developed after John took over AA?

DR. KAPLAN: Yeah, although we started doing the briefings and providing some of the technical briefs on the idea back in Janiczek's time.

DR. CORBIN: Okay, so Paul supported it too.

DR. KAPLAN: Yeah, yeah. He even wrote an article about it in a DoD magazine. And the thing was, and we found this consistently, is that if you go down to Norfolk and brief this idea at these dockside meetings where you have people from the fleet come in, they say yeah, yeah, yeah, you know, that's exactly what we need, but when you brief it to the Navy research community, the people at ONR, that's where you get sort of quietly shuffled out of the room.

There really does seem to be, at least in this area, a disconnect between the fleet and what the research community is responding to.

Now, after the Orbital Mechanics Department was abolished in '94, those people were reshuffled in the departments. Jim Rohde and Peter Kammeyer went to Earth Orientation, and Alan Fiala came to AA, as did James Hilton. I guess Dan Pascu went to Astrometry.

Shortly thereafter, in ‘96, I guess it was — LeRoy Doggett died. LeRoy was the chief of the Nautical Almanac Office. That office had became a division within the Astronomical Applications Department and LeRoy was its chief.

And unfortunately it was a situation sort of like Jim Hughes where he thought he had a sinus infection and that’s the way it was being treated for apparently too long, and it was actually a tumor. LeRoy died in the spring of I guess ‘96. It was during a DDA meeting that was being held here. It was quite sad. So that left us without a chief of the Nautical Almanac Office and Alan Fiala took over those responsibilities then until his retirement.

And during this time, there were several times when I was either acting department head or acting chief of the Nautical Almanac Office. Paul Janiczek I think retired in ’97, and Alan Fiala I guess retired maybe two or three years after that.

And so there were some gaps in coverage both in terms of the department head and in terms of the Nautical Almanac Office itself, and I served in those capacities a number of times around then.

DR. CORBIN: Now as I recall, you and I were of a like mind on these director jobs. We really weren’t too interested in them.

DR. KAPLAN: Yeah, I never — when Janiczek retired, I thought about it a long time and I just was not that interested in becoming department head because unfortunately I guess I knew what it involved from these stints that I had done both as AD head and the short stints as AA head. I knew what all went into those jobs. And I guess you had similar experiences.

And the other thing was that John Bangert was interested in the job and I just thought John would do a better job then I would. He had much more patience with the administrative aspects of the job and was just more thorough in his approach to all parts of the job.

The model that seemed to be followed in several departments that was very successful, and I think both in AD it happened and in AA, was that you would have a department head and a deputy, who might have been just one of the branch chiefs, and one of them would handle the administrative stuff and one of them would handle the science, and you almost needed two people to do that.

And that’s how things have evolved recently in AA. After Paul and Alan retired, then I’ve sort of become the chief scientist in AA and I try to look out for the scientific content of our products.

And really right now my position is chief of what’s called the Science Support Division and that’s exactly what the mission of the division is, is to keep current the scientific content of both our software and almanacs, and to make sure it keeps moving forward and upgrading it.

Now I haven't talked about my diversion into FAME for, what was that, two years or something like that. FAME was — gee, I don't even remember what the acronym is — oh, the Full Sky Astrometric Mapping Explorer, a satellite project, an astrometric satellite project that was submitted as a proposal to NASA for what's called a mid-Ex mission, middle range or middle of the road Explorer mission, whatever.

DR. CORBIN: Yes, I think there were upper and lower funding limits on the projects in that group.

DR. KAPLAN: Right, yes. Yes, they were very firm — I think the initial funding cap was like 140 million dollars and of course all the proposals are for 39 million, you know, 999 thousand and 99 cents, as was ours of course.

And this was actually accepted as a mid-Ex mission. I guess it was in like '99 or something like that. So we had zero experience with space missions at all. I mean other than the support that you guys did for Hipparcos, I think that was our total involvement in space missions and now all of a sudden Ken Johnston was the PI and there had to be a massive gearing up to take on this mission.

So the long and the short of it was, three or four people were hired and Marc Murison and I were sort of drafted from the AA Department to go into FAME to work in the data analysis area. So that happened in the fall of 2000 until FAME was cancelled I guess in the early days of 2002.

DR. CORBIN: I know they didn't make the cut at some stage in the proposal process — they didn't go from one level of study to another.

DR. KAPLAN: Yeah, at the end of what NASA called phase B, yeah, this was I guess in the fall of 2001 — yeah, it was shortly after September 11th so we had to put on what was called the Preliminary Design Review, PDR, and this was a big thing. This was a major milepost that you have to get by.

I think the analogy probably should be as a gate because you've got a major NASA review of what you were presenting at that point and a go or no-go decision from NASA.

Of course by this time several million dollars had already been spent on the project. I think the figure was 20 million, something like that. A lot of this went to Lockheed who was the prime contractor of course.

DR. CORBIN: And I understood — of course I had no direct involvement by then, but NASA had become very sensitized to any project where it thought there was internal dissention within the project, and I know people like Reasenberg were going off in directions that some of the people here thought were not advisable, and this created, as I understand it, a fair amount of dissention within the project.

DR. KAPLAN: Yes, yes, there was, although Bob Reasenberg left fairly early on. There was, and the basic problem was that it was a very difficult problem that we were trying to solve, that the precision levels that we were aiming at, which were in the 50 microarcsecond range, had never been achieved before.

And as you got to starting to look at the problem itself, you began to realize that the spacecraft as it had been proposed didn't have the calibration capabilities that you needed and therefore to get to that level of precision you were going to have to essentially calibrate on the fly, use the observations that you collected to wring out the systematic effects.

And some smart people like Norbert looked at the problem, as did Dave Monet. And Valari Makarov also looked at it. And sort of the conclusion was that we weren't going to take enough data to calibrate out all of the effects.

It was highly complicated by the fact that it was a scanning mission, that we were using time delay integration on the CCDs so we we're not really getting images. We had to infer what the images were from what we pealed off the end of the chip and it just looked like we didn't have the information needed to do the job.

And although Norbert, and Valeri, and Dave came at the problem from different directions, they mathematically looked at it differently, they pretty much all came to the same conclusion that this was not, it just was not going to be doable.

But the thing that really killed it — I actually think we were successful in sweeping that issue under the rug at PDR, convincing people that although it was a major problem that we were on track to develop schemes to solve it. The thing that killed it was just cost overruns.

I'd like to take a break here and get water. I'm starting to dry out.

DR. CORBIN: Oh, sure.

DR. KAPLAN: Anyway the thing that killed FAME was the cost overruns. Ken Johnston would go down to the meetings at NASA and they'd tell him, look, you can't exceed this particular cap and he would come back and he'd say things like, well, this is what they told us but I've got to find out what the real cap is.

You know, he always thought that they weren't really leveling with him, that they were giving him a target that they understood that he would exceed and it would be okay if he just didn't exceed it by very much.

Well, when it came to PDR it turned out that what they were telling him was in fact the limit and that the cost that we estimated at PDR was over the limit, and apparently he expected that they would give him the slack, but the fact is that the limit was the limit and they cancelled the project.

So at the beginning of I guess it was 2002, the project was cancelled. It was cancelled just a few days before an AAS meeting, and we had a FAME poster there. That was I guess the Washington AAS meeting in January of 2002. By that time we had been given notice that the project was cancelled and Marc and I returned to the AA Department later that year.

Of course the astrometric satellite idea has never died and there are still people who are pursuing it.

DR. CORBIN: I saw the front end of FAME when it was first being put together and one of the things that bothered me about it was that I had a little bit of a sense that they looked at what would get support from the astrophysicists, for instance, or the cosmologists. Of course one of the biggest considerations was getting accurate parallaxes for a number of the Cepheids, and that's where the 50 microarcseconds came from.

So a number was picked, and then they said, "Okay, that's what we can do" before they had really developed anything or had any idea whether they could do it or not.

DR. KAPLAN: Absolutely, that's exactly right. And in retrospect there were many times during the development that we would say things like, if we could just get a stare mode frame for calibration, but the design was too far along.

It had been decided — and the way the thing was going to be orbited and boosted into orbit, the spacecraft was going to be spun up before the apertures were opened so there was no chance for even getting a stare mode calibration.

I mean you could do a certain amount of calibration on the ground but everybody recognized that when you put an instrument like that on top of a rocket, accelerate it at a couple of g's at a very high vibration level, what you calibrate on the ground isn't what you're going to have in orbit.

And the whole idea of doing this calibration through the normal data analysis, we just realized there probably wasn't enough information there. It was just too difficult to extract, to do it using the CCD data from scanning mode. A very difficult problem.

Since I returned to AA after FAME I've been very much involved in implementing the new IAU resolutions that went into effect in 1997 and 2000 regarding the astronomical reference frame, the ICRS and the resolutions regarding precession-nutation and how we measure Earth rotation.

So that's actually been a big part of my life over the last several years, both developing the algorithms to implement those resolutions in a way that's different than the way the Earth rotation people are approaching them, but equivalent. We've shown them to be equivalent.

Right now I'm near the end of a process of writing a circular documenting the resolutions, not only just giving the formulas but also explaining what those resolutions mean. And this is sort of the successor to Circular 163, which did that for the resolutions that were adopted in 1976 and 1979, which was very much sort of the equivalent thing.

But those were much simpler times and that Circular 163 was maybe 30 pages. This one's going to be well over 100. And so that's what I've been working on up to this very day actually.

One other aspect of my career that is probably worth commenting on is in the later years especially, since the early '90s I started doing science fair judging and I went to a lot of the D.C. science fairs, the city-wide science fairs.

And also I have mentored a couple of summer students, and in fact there were two kids that I found at the D.C. science fairs, one of them was Sabrina Snell, I guess about five years ago, and the other one is Andre Munteanu, about two or three years after Sabrina, who I thought were really talented kids and very bright, quick learners, in very different ways.

But I managed to bring them into our summer student program, both of them, and Sabrina worked with me, and Andre is still in the summer program. He's downstairs right now working with Marc Murison.

And I think we're lucky. The personalities matched pretty well between mentors and students, and both Sabrina and Andre, based on their projects here, have gone on to become finalists in the National Science Talent Search, and Andre in fact won ninth place.

So these kids have done very well working at USNO and have given us a little bit of publicity too.

And in the last couple of years, last two years, Merri Sue Carter and I have been running the summer student program and now that I'm not working physically at USNO, Brian Mason has taken over that program.

But even though I'm living out in the sticks now I volunteer to do science fair judging. And I could have done three. I was away on travel, which prevented me from judging two other Cecil County fairs that they asked me to do, but I did get to judge the local middle school fair in Rising Sun this year so I'm sure I'll get to do more of that.

And that's something that I like to keep my hand in. I think science education is really important, and there are times when I think that I'd like to go back into the classroom, which was where I started my career.

I think I'm probably too old to have the energy to handle five classes a day in science but I do intend to keep active in that area in various ways, and one of the things I'm going to do in retirement is try to be active in science education.

DR. CORBIN: I think it's important to keep real scientists in contact with students because given the rather disturbing rise — I don't know if it's an anti-intellectual one or what — that we've been seeing in recent years in the country where religion is creeping back into what should be the teaching of science. I think it's good that people like you will —

DR. KAPLAN: Yeah, that is a concern of mine. It's important that science education in the public schools not be co-opted by any particular religious viewpoint and there's certainly a danger — well there's more than danger. I mean in many cases it's happened. Textbook publishers are now extremely wary of what they say about evolution and it's just had ripple effects through the educational system unfortunately. So yes, I agree with that and I think it's an unfortunate turn of events.

From a religious point of view it's also an unfortunate turn of events because a lot of people think that if you are a religious person you basically have to take the creation story in the Bible literally and that's just not the case.

There are many people of deep faith who don't regard that particular story as the literal nuts-and-bolts way in which the world came into being and unfortunately it's the conservative biblical literalist view of Christianity that is being viewed by a large segment of the population as representing the religious community at large, although in fact there is enormous diversity of opinions and viewpoints within the religious community that aren't being well represented by the current controversy.

So yeah I think it's unfortunate both from a scientific standpoint and a religious standpoint that evolution is being sidestepped.

I mean if you talk to anybody in the biological sciences, evolution and natural selection are among the unifying themes of biology. Basically we can't understand nature as it exists on Earth without having natural selection as being the driving force, and it is to biology pretty much as the Big Bang is to astronomy.

And that is one of my motivating reasons for getting into or trying to be active in science education in some way or another, is the fact that I think it's being shortchanged lately.

The other thing that has been an unfortunate influence is that the emphasis on basic skills and the tests, the achievement tests that —

DR. CORBIN: One size fits all.

DR. KAPLAN: One size fits all. Not only that but now the teachers have to teach to the tests so certain things in the curriculum have to go.

High schools teachers in Virginia have told me, basically there is no astronomy being taught. It sometimes gets wedged into an Earth science unit in seventh grade or something but that's it because it doesn't appear on the tests. If it doesn't appear on the tests there's no point in teaching it. In fact you shouldn't teach it because that's not what you're expected to do anymore.

DR. CORBIN: When I got my new telescope I offered to do things for the local school including setting up in the daytime. I told them I was pretty sure this would be possible if I could mark tripod foot positions on a piece of asphalt or something like that. I could come over in the daytime, set up, and pick up Mercury, Venus, and so on.

The woman I talked to, who was head of the science program in the school, was really enthusiastic, and then all of a sudden — nada. It was obvious she had gone and talked to some bureaucrat in the system who told her not to waste time on such activities, just what you've said.

DR. KAPLAN: Yeah, yeah. I have some general observations on 34 plus years I've had at the Observatory that I'd like to mention here. I jotted down some notes last night on this.

First off I really have to say that for me, working at the Naval Observatory has been a wonderful career. The environment has been very supportive. I've been given a lot of freedom to do things that I wanted to do and I've had good bosses.

I really have to thank Ray Duncombe, Ken Seidelmann, Jim Hughes, Gart Westerhout, Paul Janiczek and John Bangert, who have been very supportive of some of the things that I've gotten into and have given me interesting projects to work on for the most part. And the people around here, the career scientists are generally of extremely high quality. They're not ego driven, they're driven by their own scientific interests and really wanting to get the answer right. And generally people around here are good at what they're doing so it's been a very — for me a very intellectually stimulating place to work and a very supportive one.

(END TAPE 2 START OF TAPE 3, SIDE A)

DR. KAPLAN: Secondly, of course, and everybody when they retire mentions the fact that the technological change over the course of their career has been enormous and that certainly has been the case for me.

In terms of computer power there were things that I remember working on in the '70s on the old 360-40, doing orbits of asteroids, that would take us hours and days of computing at that time, that are literally done in seconds now or even less.

Marc Murison does asteroid calculations by the hundreds and thousands whereas we were doing them individually back in the '70s, and of course that kind of capability gives you just a much — the science that can be done with those sorts of capabilities is much broader then we had when I first started here.

And of course that sort of computing capability is not just in raw computations but it's made technology like the VLBI correlator possible and the FTS and the optical interferometer. Without microprocessors controlling every aspect of those kinds of instruments they just wouldn't work, period.

And of course the transitions from photographic emulsions to CCDs, although that hasn't been an area that I've worked, obviously it's influenced all areas of astronomy and astrometry in particular.

I did get involved at one time in a couple of clock trip projects. We used to have to carry atomic clocks around. Well, we just don't do that anymore because GPS is really the way in which we synchronize clocks worldwide.

So the technological change obviously has been enormous.

And I've mentioned this before in regards to Captain Hagen, but it seems to me that USNO, if it's going to survive as an institution, we do have to be product oriented.

I don't think the Navy is just going to pay us to do pure astronomy for a very long period of time if we aren't producing something that they can use and that they need. That's why we're mostly paid on appropriated funds.

Some departments are more into this than others. I think the AA Department has always been fairly product oriented and Time Service too. Although Time Service has a product that is continually distributed and they don't really have to advertise it very much, although there is a danger that most people in the Department of Defense think the time comes from GPS magically, and they don't actually understand that we're the source of the time, but it comes from GPS.

So there's still a need to go out and sort of proselytize, advertise what we do and make sure that we still are on the cutting edge of what the military needs.

On the other hand, and I think the department heads probably understand this better then our Scientific Director does, there are some projects like NPOI and FAME that don't fall into this military-need category — although FAME for a while was funded by NASA — and they take a lot of in-house resources.

And NPOI unfortunately has never turned out to produce the kind of astrometry, wide-angle astrometry that it was designed to do.

So there are some projects that actually are drains on our resources and have become sort of sacred cows that just go on year, after year, after year, without really producing anything of value. And in down-budget times, unfortunately you don't have the luxury of carrying these kinds of projects anymore, but they're there and they're the boat anchors that I think prevent us from doing the job that we need to do for the Navy better.

DR. CORBIN: I thought Dave Monet had an interesting take on this the other week when he was here.

DR. KAPLAN: I wasn't here for that. What did —

DR. CORBIN: Well, his main point was that the Observatory really no longer has the resources to be producing or developing large projects on its own, and it should be plugging into some of these big survey-type programs that have huge CCD arrays and that need astrometry to do their own reductions. We can offer them this with products like Norbert's catalog. The programs then provide us with data to do astrometry at fainter magnitudes, and we would get this without these big outlays of money to produce big expensive instruments.

DR. KAPLAN: Right. Yeah, that we're not really suited to do. And I agree. I mean that was our role really in the Sloan Sky Survey, which I think we were pretty effective in.

DR. CORBIN: Yes, that worked well.

DR. KAPLAN: Yeah, I agree. In terms of sort of oversight to make sure that we're on track in being a useful institution, I think the Navy takes its share of the blame too. This constant reshuffling of commanding officers, which used to be every three years, has recently shrunk to now every two years, and our present captain will be here like one and a half years.

This is nuts in my opinion. It might work for ships but for a highly technology-based shore institution it basically guarantees that the captain is not going to be able to have effective oversight or management of the institution at all. Because it happens that they get to understand what we do at the end of their tour, and by that time they're looking on to their next assignment or to retirement, and so there is no proactive Navy oversight of our mission for that reason.

So I think in terms of our long-term vision, we don't have a good long-term vision. That's what it amounts to. Unfortunately, our last two Scientific Directors have not been able to articulate a very good long-term mission, so defining what we as an institution ought to be about really has not been done.

And occasionally there are pushes to look to the future, to formulate a future plan. There was the session that was done I guess in early September of 2001 involving all of the department heads and now there's also talk of doing another thing like that.

But we have to keep ourselves relevant to the Department of Defense, I think, if we're going to survive and I'm not sure that we've been very good at that.

DR. CORBIN: Well I don't know how you feel about it, but I personally have felt that the change in the chain of command that took place in the mid-'90s when the Scientific Director was put directly in the chain of command over the department heads was not a good step.

DR. KAPLAN: Right.

DR. CORBIN: Because this lost the diversity that the Astronomical Council used to have and the need to come to a consensus on projects and other issues.

DR. KAPLAN: Yes, yes. So right now the Scientific Council doesn't really function as a deliberative body.

DR. CORBIN: Right.

DR. KAPLAN: I mean your boss is there, the Scientific Director is there, and he gets to call the shots.

DR. CORBIN: And given the fact that, as you say, by the time a captain figures out what's going on here, he's either on to his next assignment or retirement. That leaves only the Scientific Director setting the course for the future.

DR. KAPLAN: Or not. That's right.

DR. CORBIN: And if you have only one person doing it, he had better be right.

DR. KAPLAN: Yep, yep. That's exactly right. And you mentioned before of course and I agree, and this is something that hasn't really changed much since I've been here, is that the walls between what used to be called divisions and are now called departments have always been too high and there's been not enough exchange of personnel, so that people don't really understand what goes on in the other departments.

And I don't think that's healthy for the institution because if you're going to have — if for example you wanted a Scientific Council that was deliberative and was able to make decisions on the future, you need people who understand what is happening in the other departments and can relate what their department is doing to what other departments are doing. And without that everybody is just covering their own turf and trying to protect their staff and in many cases —

I've been to Scientific Council meetings as acting department head at various times. I've been there and I don't think it functions effectively as a deliberative body. Everybody is too afraid of -– they're into protecting their own people and not stirring the pot.

There's another theme that I've noticed, and this goes to what you just said concerning our lack of expertise and our lack of resources to get into these very large projects, and that is, I've noticed a consistent tendency, going back to the '70s, really to underestimate the difficulty of projects and to overestimate the capability of our in-house staff to do everything.

And in many cases we've been reluctant to hire the expertise that we need from the outside and to recognize our own deficiencies and this certainly hurt us in FAME. I mean we needed experienced spacecraft people, people who had done space projects before and knew the pitfalls, and knew how you had to deal with contractors, and could keep the thing on track, and we didn't have that.

Ken Johnston as PI tried to do too much. He tried to run the whole project by himself and he didn't have the experience in doing that. It was a constant battle with Lockheed.

And look at a project like Deep Impact, which was just successful. Mike A'Hearn is the PI for that. Well, I know Mike. Mike is not the person I think to get into the nuts and bolts of the administration and engineering of this thing and I'm sure he wasn't.

I mean if you look at the people who are involved, the engineers at JPL and other people, I think Mike sort of took a back seat in terms of making this mission work. He will be in the forefront of interpreting the science and that's what the PI is for, to keep the science on track, but in terms of managing the details of the project, you need people who have been there before and know what they're doing.

And this has affected us on smaller projects too, that I think we've been too late in hiring people on the outside with the expertise that we needed because we assume that we're smart enough, we can do it all ourselves.

DR. CORBIN: And of course one disadvantage we've had in that area is that the Naval Observatory traditionally has had a relatively low grade structure and some of these people that we would need to hire are not going to come here for what we would pay.

DR. KAPLAN: That's exactly right. That's exactly right.

DR. CORBIN: Someone pointed out that a study has shown that the grade structure, especially in scientific activities, is a function of how old the activity is. That is, the older ones like the Naval Observatory tend to have lower grades on average.

DR. KAPLAN: Yeah. There has been a positive development within the last decade, and I do have to credit this to Ken Johnston as Scientific Director, in that he has been willing to hire either postdocs or what are equivalent to postdocs, and to keep them on if they work out well, and we've had some really smart people hired within the last decade that I think have benefited the Observatory.

They are people like Marc Murison and Arsen Hajian, and more recently Michael Efroimsky, and folks like that who — and really the whole fountain clock development group. I mean those are a bunch of really smart guys and they have put another level of intellectual stimulation within the place that I think we had lacked before.

DR. CORBIN: On the other hand my own experience and observation with people like that is, that if anything, better supervision is required because these people, if just left to their own devices, will tend to drift off into whatever it was they were doing before them came here and not necessarily what we need.

DR. KAPLAN: Absolutely, yeah. Dealing with very creative people like that is a real challenge, keeping them on track. I mean I've got a couple of them in my own division and believe me it is a constant battle to keep them from going off on tangents and doing stuff that isn't really all that relevant. And it's hard to do. You're absolutely right.

The thing they do for us, even if they do go off on tangents sometimes, is they do publish papers with the U.S. Naval Observatory as the institutional byline and since we are scientifically up against a lot of competition in Europe, you know, just keeping good scientific quality research papers being published is a contribution in itself, in my opinion.

DR. CORBIN: Sure, yeah.

DR. KAPLAN: So that's about all I've got to say.

DR. CORBIN: Okay, well thank you, George.

(END OF AUDIOTAPE)

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