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ORAL HISTORY TRANSCRIPT

EMIL R. SCHIESSER
INTERVIEWED BY JENNIFER ROSS-NAZZAL
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The questions in this transcript were asked during an oral history session with Emil R. Schiesser. Mr. Schiesser has amended the answers for clarification purposes. As a result, this transcript does not exactly match the audio recording.

ROSS-NAZZAL: Today is November 2nd, 2006. This oral history with Emil Schiesser is being conducted for the NASA Johnson Space Center Oral History Project in Houston, Texas. Jennifer Ross-Nazzal is the interviewer, and she is assisted by Rebecca Wright.

Can you tell us how you came to work for NASA?

SCHIESSER: I suppose it was a combination of inadvertent preparation and fate. I ended up taking on, as a challenge, I think, more than anything, the study of all the math and physics I could get in high school and college. So that's kind of the preparation side of things. But I graduated in the middle of the academic year and there weren't very many opportunities available at that time in the way of teaching or industry, at least that I was aware of.

David [A.] Beckman and I attended some of the same high school classes and I would occasionally go over to his house and watch him operate his ham radio. He had joined the Space Task Group. We had kept up a limited correspondence after high school and I told him that I was graduating and looking for a place to work.

He invited me to visit the Space Task Group at Langley [Research Center, Hampton, Virginia] to see if there was an opportunity for employment there. I flew to Virginia and was met by David. He took me to the Langley Field buildings where the Space Task Group was housed and I briefly talked with John [P.] Mayer and Carl [R.] Huss. They said they could probably use me and directed me to Burney Goodwin's office in the Human Resources area. Maybe it was a one-person Human Resources office at that time; I'm not sure. He was in a room all by himself, fairly good-sized, tall ceilings, single desk, one chair, hardly anything else in there. He opened his desk drawer and he gave me forms to fill out and mail later. I did that, and shortly after that I got an offer to go to work there at the Space Task Group. That's how I ended up at Langley. I finished the class work and requirements for graduation and on February the 6th, 1961, I started work.

ROSS-NAZZAL: What were your first few days like out at Langley?

SCHIESSER: The first day I was introduced to Paul [G.] Brumberg who was to give me my first assignment, and was given a desk in the room where I was to work. It was a large room with a tall ceiling in a dark-red brick building. The room had two rows of desks facing each other where the engineers were sitting, and they assigned one of those desks to me. I don't remember too much more than that.

There was a cafeteria nearby, and so for the first time I found out what crab cakes were, and grits. I had never heard of those. I was introduced to a different culture, in a way, because I was raised in the Midwest and I wasn't familiar with some of those types of food.

It was a little bit of a stark environment, I would say. Off to the side was a very small room, and that's where John Mayer and Carl Huss had their desks. Then down the road a block or so was a building with a computer in it, an IBM [International Business Machines] 704. The

Space Task Group buildings were on the opposite side of the airport from Langley Research Center main campus buildings.

I'm really happy that I had a chance to go there, and I owe quite bit to David for taking me to and from work, because I had no transportation. I was pretty much broke when I graduated. I put all my stuff in a foot locker and traveled from DeKalb, Illinois, to Langley by train—a cold train, no heat—where David met me. I had to walk everyplace else till I bought a bicycle. I eventually accumulated enough funds to buy a used car.

At work in the room were people like [Lynwood] Dunseith, Clay [Claiborne R.] Hicks [Jr.] and Hal [Harold D.] Beck in addition to Brumberg and others, but Dunseith was seldom there. He was usually working at Goddard Space Flight Center [GSFC, Greenbelt, Maryland], and they had a couple of buildings up there, one of which housed the computers that were utilized to do all the computations during the Mercury Program flights, and Dunseith was up there a lot.

The first assignment I got from Brumberg was to determine the accuracy with which a spacecraft could be launched into Earth orbit. Basically to assess booster launch guidance and navigation performance. There was a set of flight data from Atlas launches, position and velocity in a Cartesian coordinate frame; that is, in an *x*, *y*, *z*, Earth-centered coordinate frame. The data had to be transformed into speed, heading angle and flight path angle, radius, latitude and longitude in order to assess insertion performance, that is, to polar coordinates.

I ended up needing to learn FORTRAN to code the transformation equations so that the computer could perform the computations. This would be in 1961. I don't believe FORTRAN was invented until sometime in the middle or the late fifties, maybe '58 or so. So it hadn't been around very long, but it was a mature enough so-called high-order language that it was quite usable at that time. The high-order language goes through a conversion into assembly language, and then from there into machine language. So it's the third layer up from the basic cryptic language that's used to control the computer.

After a fairly short time I finished that project, and asked for another. They must have mentioned it to Bill [Howard W.] Tindall [Jr.], because he showed up, and Bill said he would like me to determine how well we are able to compute position velocity of the spacecraft in Earth orbit.

That was my second assignment. In those days we didn't have calculators, let alone any personal computers. There was a mechanical calculator that I used in college, a Frieden, but I didn't use one at work. What computations we did were done on IBM 704 mainframe or a little IBM 1620 computer that was about the size of a chest freezer. I think we even had to code up a series expansion to compute sine and cosine functions. I'm not sure those functions were available on one of those machines.

There was group of people there who were called math aides, and they would to take the data from the engineers and do the plots and work up tables and things of that nature. When it came time to writing a report, you would write it out in longhand and give it to a secretary, who would type it. I think we had perhaps electric typewriters for them then. IBM Selectrics or something like that came along somewhere in there. After it was typed and proofed the graphs that the math aides worked up would be folded in. It was a manpower-intensive process to create a report. Mary Shep Burton was lead of that math aid group. There were a number of math aides in the group that later on became engineers.

The organization and the titles given to people were quite fluid back then. I think the initial titles tended to be patterned after those used by the Langley Research Center.

Aeronautical research engineer, aerodynamic theory, was one of my early titles. That was later changed to other types of titles, some of them more appropriate than others.

The first U.S. human suborbital flight, first U.S. person in space, took place a few months after I arrived. Alan [B.] Shepard [Jr.] flew on May 5th, 1961. One day while we were there in the room, and somebody called us to come to the door. We all migrated to the door and Al Shepard came along and said hi to everybody. He said a few words and then went on to the next building. I don't ever remember that happening again, but given the small group that we had, he came around to say hi, which was really kind of special.

I think the Flight Operations Division at that time had about eighty engineers and managers in it. [Charles W. "Chuck"] Matthews was the chief, and Chris [Christopher C.] Kraft was an assistant chief. There were others at the management level, like [John D.] Hodge, [Sigurd A.] Sjoberg, and Chris [C.] Critzos who sort of a technical administrative assistant.

There were four branches: a Flight Control Facilities type branch by Tec [Tecwyn] Roberts, with Dennis [E.] Fielder and [Richard A.] Hoover and some others in it; a Flight Control Operations Branch, headed by a guy by the name of Fred Matthews, and in there was [Arnold D.] Aldrich and [Robert E.] Ernull and [Eugene F.] Kranz and others; a Spacecraft Operations Branch and a Recovery Branch. The Recovery Branch was by headed by Bob [Robert F.] Thompson.

A Mission Analysis Branch headed by John Mayer had three sections: a Trajectory Analysis Section headed by Carl Huss; an Operations Analysis Section with Morris [V.] Jenkins as the Acting Head; and a Math Analysis Section which Mayer himself was serving as the Acting Head of. I was assigned to the Math Analysis Section along with Paul Brumberg, Lynwood Dunseith, and some others. I think Elton Mayo was in there as well.

The math aid computing unit, besides Mary Shep Burton, included Shirley Hunt and Cathy [Catherine T.] Osgood, amongst others, and I think both of those later on became engineers. Back in those days, if you look at the photographs, you'll find many people in suits, or in a shirt with tie. That was our uniform and we interfaced with a lot of people who wore suits.

One day at work John Mayer came up to me and said, "Let me show you how to tie that tie." I had just swirled it around once or twice and stuck it in somehow, and it was not graceful, actually. He showed me how to tie a symmetrical Windsor knot, and so ever since I've tied a Windsor knot.

Shortly after that, on July 21st, 1961, the second suborbital flight with [Virgil I. "Gus"] Grissom took place, and that same month I bought a 1931 Model A Ford, the first car I ever owned. It was a wonderful car. It was all original. It even had the tar top and the original headliners. It worked well. It wasn't capable of its top speed anymore, and it had wires to the brakes, mechanical brakes, so you had to plan your stops in advance. But it was a wonderful relief. I didn't have to carry laundry on a bike for two miles to get it taken care of, for example.

There was quite a bit of business travel. I think I had only one flight under my belt before working at Langley, so it was kind of a novel thing for a young troop to be shipped around by plane to Cape Canaveral [Florida], to Goddard Space Flight Center, and later on to JPL [Jet Propulsion Laboratory, Pasadena] in California, and to visit contractors.

Some of those early flights were on a DC-3, which had quite a slant when it was parked, so you had to climb your way up into it from the back. The first ones came off the assembly line in December of 1935, so these planes were quite old, but they were flown beyond 1961, well into the sixties. I don't know how we came to fly in the DC-3s, whether it was a chartered flight or through Piedmont Airlines or some other airline. We rode the DC-3 for a while, and then eventually that phased out.

I traveled down to the Cape to attend a flight readiness review for the GE [General Electric] tracking system, which was used to guide the Atlas into orbit, I believe, and the travel orders were signed personally by Chris Kraft. That was around December of 1961.

One interesting thing again about that DC-3, since this whole thing is about transportation, is that up until the DC-3 came along, long-distance travel was predominantly by train. After the DC-3, there was a transition over to aircraft flight for long-distance travel.

Bill Tindall was a very intelligent guy, affable, I would say, and he, as I mentioned earlier, asked me to extract how well we knew the position velocity during Earth orbital flight, the first of which would have been John [H.] Glenn's [Jr.] flight. I think, looking back on it, that he and Eugene [L.] Davis were among those involved in the creation of the navigation capability for orbit coasting flight. The first Orbit Determination Program was implemented at Goddard Space Flight Center on IBM mainframes, and I have flow charts that date back to the 1958-1960 time frame in Tindall's hand, so at least Bill and some of the others were looking over the shoulders of those who were implementing the navigation capability if not directly influencing and participating in it along with IBM programmers and others from Goddard.

The Mercury Orbit Determination Program, designed to determine spacecraft position and velocity during orbital flight, was really quite interesting, I think, partly because it was fairly automatic. Tracking data from various sites around the world were immediately sent by teletype machine, a frame of data every six seconds, to the computing complex at Goddard. Each frame included the time at which the observation was made, the distance between the ground antenna and the spacecraft antenna, called the range measurement, and azimuth and elevation measurements, from the tracker.

The Goddard computer would use a past estimate of the position velocity and predict forward in time to tracker measurement times to compute what the future tracking measurements should be if the position and velocity are correct, compare those with the actual measurements, determine the differences, and then adjust the initial position velocity until there was agreement between the predicted and actual measurements. The method for doing that is called least-squares, and it was all automated. The reason it’s called least squares is because the difference between the measured and actual range, for example, is squared, and the initial position and velocity is adjusted so that the sum of the squares of those differences is minimized. It's an old technique designed by, I guess, [Carl F.] Gauss.

My impression is that Paul Herget from the University of Cincinnati [Cincinnati, Ohio] was engaged in the development of the Mercury Orbit Determination capability. Paul had used a manual calculator to compute the orbits of comets using a least squares method. I thought he had to rework a sequence of computations organized for a manual calculator for implementation on the Mercury Program digital computer, but I've read that he had implemented techniques for locating celestial objects using a UNIVAC main frame machine in the mid 1950s.

Another feature of the Mercury Orbit Determination Program was that the most recent tracking data was given a greater influence on the result than older data. It was recognized that the forces acting on the spacecraft were not perfectly known, including the possibility that the trajectory might have been perturbed by the vehicle itself in unknown ways, such as from the attitude control system. The "down-weighting" of older data was accomplished by using all the good observations from the most recent batch of data with fewer and fewer measurements from prior batches of data, with the fewest used from the oldest batch. A batch consisted of the tracking data acquired from one pass of the spacecraft over a tracker.

Travel orders were signed for me to witness orbital navigation conducted during Mercury-Atlas 6 [MA-6], which flew on February the 20th, 1962, in the computer room at Goddard. The Mercury Orbit Determination Program results were displayed on an online printer. It was about four feet tall and two or three feet wide. That is were the performance of the navigation process was monitored. That was probably accomplished by James C. Stokes. He's a tall, lanky, quiet guy, and he was standing with a slight bend over that online printer, monitoring the results, so he was probably the first real-time on-orbit navigator for U.S. human spaceflight. I stood next to him, too shy to ask him any questions. My task was to gather all the data, understand what happened, and be aware how the process went so I could come back home and do the analysis to extract position velocity knowledge accuracy.

In the '62 time frame I met Jack Hartung. He had an interest in sailing, just like I did, but he had the enviable advantage that he had a sailboat. It was a Thistle, I believe. He took me out on I guess, the Chesapeake Bay, which was a lot of fun. Hal Beck was rumored to take his sailboat out into the current and sail all day just offshore and come back in. He didn't have to come back far since he was just out there sailing against the flow of the water. There were a lot of things to see and do in the Hampton area. I managed to get over to Williamsburg [Virginia], Jamestown [Virginia], and see some of those places for the first time.

ROSS-NAZZAL: What did you think when they made the announcement that they were going to build the space center here in Houston?

SCHIESSER: When we were notified that they were going to move us to Houston, I asked myself, "Where's that?" I'd never heard of Houston. I was vaguely familiar with Texas. I guess that's evidence of my naïveté. I had to find out where Houston was on the map. In those days things were happening so fast that I don't believe I ever reflected on it. I had been to Texas once before when I was a young man growing up. My folks and I traveled from Illinois to Albuquerque [New Mexico], and as I recall we crossed the panhandle, or part of Texas, anyway. It was ungodly hot. I saw my first horned toad; I think the only horned toad that I ever saw. So my impression of Texas was of a dusty, treeless, hot place. I was not too excited about that aspect of the move, but I really didn't give it much thought beyond that.

There were other Mercury flights in 1962 including MA-7 in May, and I would take trips up to Goddard for some of those flights to gather data. Meanwhile I was processing the tracking data from John Glenn's flight and managed to figure out what the accuracy was and publish it.

One day somebody came in the office and said that Kraft wanted to see me. Kraft was Mayer's boss. I went across the alley or the road to another building where Kraft, Jim [James J.] Donegan from Goddard and others were seated. Kraft asked me to identify the orbit determination accuracy on John Glenn's flight. I told him. He already knew. He turned to Donegan and said something to the effect, "See?" I suspect that Donegan was probably a bit more optimistic about the capability than what was actually achieved.

By the middle of 1962 Kraft was delegating the signing of some of the travel orders to his secretary. The organization was growing. I think around the middle of '62 there were probably about forty-seven engineers in Kraft's division which had about five branches by that time.

I moved to Houston in the middle of 1962. My travel orders were issued for travel from June 11th to June 15th, so I was given about five days to travel here. I didn't think my Model-A

Ford would make it, and I didn't know what else to do, so I sold it. One of the other employees had two cars and invited me to drive one of theirs down, so I put my stuff in the trunk and drove down in their car.

A good number of Mayer's group came down, probably twenty-five or thirty people, something like that. The titles were changed. Organizations continued to evolve. I was in what was called Math Analysis Section at that time. Tindall was deputy to John Mayer in the middle of 1962.

They located us near Gulfgate in buildings called the Houston Petroleum Complex on Highway 45. The mainframe computers that were used for engineering computations were located at the University of Houston [Houston, Texas], I think in the radio station building. There was a courier that took our card decks from Houston Petroleum Complex down the road to the University of Houston. I think they went twice a day.

We'd take big sheets of paper that were marked off and write our code on them, and then keypunch the instructions on the cards to create a deck of cards and either take the deck to the computer ourselves or give it to the courier, which often meant that you got your run back the next day. If you made a little bit of an error, then you had to fix it and submit the run again. There was a lot of effort checking what you'd done in order to not delay too long getting a decent run, whereas today they get on the personal machine and resolve the errors in minutes. It's just a totally different environment.

The card's size was larger than today's dollar bill. When it came time to deciding what the size of the cards were, it was my understanding that there was a big discussion amongst managers at IBM and the manager in charge took out a dollar bill and said, "Make it this size." Apparently a bill was larger than the ones we have today or else the rumor is not true.

Mercury-Atlas 8 flew on October of 1962. As an aside, there was recently a commercial suborbital flight. I think they flew to just barely above sixty miles; probably the first commercial space vehicle to enter space. Though reusable, it was not capable of returning from Earth orbit, whereas Al Shepard's vehicle was, and Al Shepard's flight went much higher. What was accomplished on the first Mercury flight has yet to be accomplished through private funding.

The Mathematical Physics Branch was formed in 1962, and I ended up, I guess sometime in '62 or '63, being assigned as the group lead for Apollo navigation. I was asked to define the ground navigation capability for the Apollo missions and had a few civil service people to work with to do that. We had support from—or maybe we were supporting—IBM and others in the development of the ground navigation capability. Orbital navigation was performed at Goddard up through Gemini III. The first time that the control center in Houston was used to do orbit determination, navigation for the conduct of a flight, was on Gemini IV.

The Mercury Orbit Determination Program wasn't that well suited for ongoing work. It was kind of tailored to the discrete Mercury flights. For example, take what I mentioned earlier about how the older data was given less impact on the solution for position and velocity as a function of time/age. There was no way to account for a prior tracking pass that preceded a long time gap vs. one a short time back, yet the one prior to the long gap should have less effect on the solution than one more recent. It was necessary to come up with a little more sophisticated means for down-weighting old data that took into account how far back in time that old data was.

The automatic character of the Mercury Orbit Determination Program was maintained but the processing of all the tracking data together each time data from a new tracking pass was received was replaced with a sequential batch process. In order to do that the accuracy to which position velocity was known was estimated based on prior tracking data processed and used when incorporating the most recent batch of tracking data. If the position and velocity are thought to be well known and if there was not much confidence in the quality of the latest tracking data, the position and velocity estimate would not be changed very much. If, on the other hand the current position and velocity estimate was thought to be poor, such as the result of having to predict forward over a long time interval, and the data appears to be good, then a lot of weight is given to the current data to allow it to influence the updated estimate of position and velocity which defines the orbital characteristics.

Tindall asked me to participate with a small group of people in the development of the Gemini Orbit Determination Program while we were also in the early stages of the Apollo navigation capability definition. The first meeting I attended on Gemini Orbit Determination capability development and maintenance was on September the 25th, 1963. IBM had been heavily working on the sequential batch type processing capability, and it was compared with the Mercury approach results using actual flight data to examine performance differences and operational characteristics. The sequential batch approach was adopted for Gemini ground based orbit determination.

The Tindall formed group included civil service employees Jim Stokes, Larry [J.] Dungan, Eugene [L.] Davis [Jr.] and from IBM, Frank Ditto, an excellent programmer/analyst and Herb Norman, who was more of a technical manager, and others. John Mayer formed and named the Mission Planning and Analysis Division sometime in 1963. The Mathematical Physics Branch was among the branches formed within the Mission Planning and Analysis Division. Somewhere in this time frame Bob [Robert T.] Savely joined our group, maybe a little earlier than that even, but he wasn’t at Langley, so he joined us after when we were at the Houston Petroleum Complex buildings. We began to work more closely with Larry Dungan and

Mike Conway on the orbit determination capability. Both of those people were in an area that was focused on the Mission Control Center computers and software and the implementation of the navigation scheme.

There was a group at Goddard under Fritz Von Bun. Von Bun had flown a fighter or some kind of airplane during World War II on the other side. He was interested in mission planning and was beginning to publish Apollo mission planning information. Mayer was concerned that the two sources might create confusion so he got with Von Bun to forge a cooperative effort and together with the blessing/sanction of the program office formed the Apollo Navigation Working Group [ANWG] to which I and others were assigned. I thought that was great, because we were a little bit isolated from the tracking systems which were under the control and responsibility of Goddard. We needed a detailed understanding of the ground based tracking system characteristics and operation since it had to be coordinated with the development and operation of Apollo ground based navigation in Houston. Von Bun in turn, I suspect, wanted to know what the tracking was to support, mission profiles for example, and there needed to be agreement on coordinate frames and force models, tracking systems and their error characteristics in order that GSFC and JSC navigation analysis results could be compared and later in support of flight operations. The basic information was jointly published formal ANWG reports.

The ANWG was supported by the Jet Propulsion Laboratory. JPL had been flying probes to the Moon using the Deep Space Network [DSN] which had trackers located at three places: Goldstone, California, Madrid, Spain, and Canberra, Australia. The Manned Space Flight Network included trackers at two sites in Australia: Canberra and Carnarvon. [NASA] Headquarters [Washington, D.C.] requested that a Manned Space Flight Network to support

Apollo be developed that included trackers co-located with the Deep Space Network trackers and also other trackers scattered around the world: Hawaii and Ascension Island, Bermuda etc. About a dozen trackers were developed and used for Apollo navigation. All are gone but three, and they are scheduled for retirement.

In the 1964 to 1966 time frame we were engaged in refining the Gemini capability and using it for flights, although I personally was not involved in the Gemini in-flight activity, and on the development of the Apollo ground navigation requirements, the equations and logic for the ground navigation capability. The work included predicting what the ground based navigation performance might be given the trackers that were being developed, and some support to the other division branches engaged in trajectory design work: figuring out how chart a course to the Moon and back, how you perform Command Module Earth entry, and lunar landings with the LM [Lunar Module] and so on.

At some point we were asked to work with MIT [Massachusetts Institute of Technology, Cambridge, Massachusetts], who was charged with developing the onboard software for the command module and the lunar module. That included the equations and logic and processes for using star horizon measurements on the way to and from the Moon to independently determine position and velocity onboard, and the use of landmark tracking for lunar orbit navigation. The effort also involved means for maintaining vehicle attitude knowledge through the use of an inertial measurement unit (IMU), a sextant and other devices, and for measuring velocity change using the IMU during maneuvers in order to calculate position and velocity during maneuvers.

We were asked to follow and help support the development of five Apollo tracking ships. It was a very complex task to acquire tracking data from those ships. We also met with Marshall [Space Flight Center, Huntsville, Alabama] to agree on the models for Earth gravitational, atmospheric drag, and rocket venting forces for the Saturn V to ensure consistency between the Saturn V and the Houston control center navigation capability.

The Saturn V instrumentation unit, a ring three feet high and twenty-two feet in diameter that sat at the top of the Saturn V, contained the rocket's navigation capability. The equipment was fastened on the inside of the ring. The Saturn V navigation capability was autonomous from liftoff all the way through translunar injection. There was one flight during which the Saturn V's knowledge of position and velocity was replaced with the ground estimate and that, I think, was during Apollo 15. Nothing comparable to the Saturn V has been developed since; maybe never will.

The Saturn V had to allow gasses to vent and that created a continuous force on the vehicle in Earth orbit. It was too large to be ignored. Marshall provided us with the equations to account for it when predicting or propagating position and velocity forward in the Mission Control Center navigation program, and Marshall used them in the Saturn navigation system, but the vent wasn't modeled in the Command Module navigation software. The ability of the control center to use Saturn V knowledge of position operationally and to provide such knowledge when required to the Saturn V was promoted by using the same force models in the Saturn V navigation capability and that of the control center.

Goddard and JPL provided the ground tracking information for the ANWG report including tracker locations, antenna orientation, terrain masking (the extent to which the field of view near the horizon was obscured), the accuracy of the measurements, the rates at which measurements could be obtained and the reach or how far out they could track.

A set of coordinate systems, frames against which to describe the location of the vehicle, were defined. These included vehicle, Earth and Moon fixed coordinate frames and an inertial frame, one that's fixed with respect to the stars. The process by which position and velocity expressed on one frame is determined relative to another frame through transformations was also defined and agreed to across the program. That was done more outside the context of the ANWG, however. We had to spend a lot of time identifying navigation software formulation, equations and logic, that would not tax the limited control center computational capability.

The coordinate systems and the units used internally were constrained by the memory and word length and the speed at which computations could be done. For instance, the Mercury Program had a quasi-inertial coordinate frame, which means that the inertial frame was redefined each increment in time. We moved away from that to a Mean Nearest Besselian Year inertial frame for Apollo. These systems made it possible to simplify the equations used for the transformation between Earth-fixed and inertial coordinates, which meant it took less time and less memory, both onboard the vehicle and in the control center. I would have preferred an inertial frame whose orientation didn't change each year.

Canonical units of some sort were used on Mercury. Earth radii and Earth radii-per-hour units were used internal to the computer on, I think, Gemini, if not Apollo, rather than kilometers or nautical miles. The length of a computer word needed to be considered. For instance, the distance from the center of the Earth to the surface is over twenty million feet. It takes eight decimal places to express the distance to the nearest foot. If a computer carries only seven places the number can be stored to the nearest ten feet. The sequence of computations received attention since it can affect the numerical accuracy of the result. The effect of computer computational round-off and truncation error was considered.

The role of the ground versus the onboard navigation was discussed in the '63 and '64 time frame. Most programs start off with the desire to create an autonomous onboard capability but as time goes on and resource constraints become more apparent some of the functions migrate to the ground. It was necessary to implement an onboard capability for the crew to get back without ground support should communication with the control center be lost. The ground had the ability to independently locate the vehicles throughout most of the flight.

The ground had a difficult time competing with the onboard navigation performance during Earth entry and portions of rendezvous. Except for these phases, position and velocity determined in the control center was sent to the Command Module and Lunar Module for flight operations. Paul Brumberg came down to Houston and was with us for a short time but then went to Goddard Space Flight Center.

In the 1964 time frame the Analytic Mechanics Associates contractor was hired to support us young kids. Sam [Samuel] Pines was more or less assigned to help the Mathematical Physics Branch. There were four people in the group, Sam Pines, Henry Wolf, Stan [Stanley F.] Schmidt, and Hank [Henry] Kelly. I ended up working with Stan Schmidt and Sam Pines. Hank Kelly was an expert with the use of optimization techniques and worked with Bobby [R.] Uzzell and others on trajectory optimization work, that is, how to get from one place to another, meeting all the constraints with minimum fuel use. Pines would come by from time to time and also interfaced with other agencies, sharing the knowledge and contributing to it himself.

There are several ways in which to extract position velocity knowledge from tracking measurements: the single-batch and sequential batch approaches mentioned earlier, and a point-to-point or measurement-to-measurement method. A computationally efficient approach for the later was introduced by [Rudolf E.] Kalman around 1961. Stan Schmidt adopted the Kalman filter to navigation and for a while some referred to it as the Kalman-Schmidt filter. Pines developed selected navigation formulation and hired a person to help us full time. Pine's name is still associated with some of the control center navigation software functions he helped create.

The processing of tracking data a point at a time as it arrived, such as during ascent from the Earth, was performed using a sliding window polynomial approach until the Kalman filter came along. A sequential batch approach for point to point processing could be used, but such an approach is computationally inefficient because it is necessary to invert a matrix the size of the number of state elements every measurement frame, whereas with a Kalman filter the computationally costly matrix inversion is not required. The processing of each measurement as it arrives or becomes available is typically used for ground based real time position and velocity determination during atmospheric and powered flight, and in onboard navigation applications.

We started examining the use of Kalman filters for navigation around 1962 or 1963. Some of those investigating the use of Kalman filters would not tell the filter that it had imperfect knowledge of the forces acting on the vehicle when propagating the state vector forward in time, such as from one observation time to another. Atmospheric drag and other forces are not perfectly known in practice and if the filter is not so informed, it will estimate that it is doing a better job than it actually is, and eventually refuses to incorporate new observations because it has erroneously determined that it has near perfect knowledge of position and velocity. At that point the solution will diverge from the actual.

There are two ways to mechanize the filter’s estimate of the uncertainty in position and velocity, in a squared form, the standard deviation squared, or as the square root of such a variance-covariance matrix. Back then the square root filter would slowly diverge from the correct answer when it thought the state was perfectly known while the squared form tended to lose all knowledge of the answer. Starting with the Shuttle Orbiter all onboard and ground

Kalman filters have been the squared form. The square-root form was used for Apollo onboard navigation while the squared form was used for ground based navigation during Lunar Module descent and ascent.

Stan Schmidt was working with us and implemented equations to account for imperfect forward propagation of position and velocity due to imperfect knowledge of the forces acting on the vehicle to create a viable Kalman filter for navigation purposes. Around 1964 Bill [William M.] Lear from TRW started to help us. He worked, I think, in Redondo Beach, California. I asked him to work on the development of Kalman filters for the various Apollo navigation tasks. He was a really smart guy and easy-going; smoked a pipe, professor type, Dr. William Murphy Lear. From then on and throughout all of the other programs he was the one we relied on for all our Kalman filter formulation and design. He could do more work in two months than a team of five people could do in six, and it would be better. This might be a bit of an exaggeration. But then he tended to work day and night. Bill contributed several advancements to our Kalman filter design, including: "measurement underweighting" to account for the use of linear assumptions for a non-linear relationship between deviations in a measurement to deviations in the local position and velocity; forcing the estimated uncertainty matrix to be symmetric; and the use of exponentially correlated random variables for modeling state parameters related to systematic error. The Kalman filters developed by Bill are sometimes referred to as Lear Filters or sometimes the Lear filter, though there were more than one.

Sometime along in here the branch management was phased over to Jim [James C.] McPherson and I was a section head and in charge of ground based navigation. I was also involved with some of onboard navigation activity relative to the MIT effort and assigned to a group engaged with the Gemini ground based navigation. Jim McPherson was a tall gentleman, and he did an excellent job orchestrating the activities in the branch and was technically involved, including with the preparation of reports for higher level management. The reporting process was professionally disciplined with acknowledged procedures for transferring information or making requests up and down and at various levels, branch to branch on up to Center to Center and with program offices and Headquarters. In my view, there was an understanding as to what level you could work informally, and in cases where organization boundaries and responsibilities were crossed on a repetitive basis on significant activities, some structure or agreement was generally in place, such as a chartered working group with a sanctioned responsibility. Or there would be agreements between managers and permissions granted or perhaps acknowledgements.

We had a group, a team, an extension of what we had already going on Gemini, to work the Apollo ground navigation. Frank Ditto and other IBM people and ourselves tested the Kalman filter and the sequential batch filter, which they called a Bayes filter. Actual Mercury orbital flight tracking data were processed using the Kalman and sequential batch filters and the error in the resulting solutions computed in support of a decision to be made on the selection of coasting flight tracking data processing methods for the Apollo lunar missions.

It turned out that we could make either one work, but for efficiency reasons, both computationally and operationally, I ended up deciding to go with a sequential batch process, pretty much what had come out of the Gemini effort, combined with an ability to process selected multiple batch data in a single batch, for ground based coasting flight phase navigation. Use of a Kalman filter for coasting flight phase was not chosen because it’s numerically less computationally efficient when reprocessing data, an factor given computer capability, and at this time I think we were actually a little more comfortable with something similar to what was in use for real-time ground based navigation. This took place around 1964 or so. I think we proposed using a Kalman filter for ground based determination of the Lunar Module position and velocity, fully independent of onboard data, during LM descent and ascent to and from the lunar surface. In any event, if so, the need for such a capability was not embraced until much later.

The Gemini III flight was March 23, 1965. John [W.] Young flew on that one, along with Grissom. We were involved in coming up with the constants that the software needs during a flight, jointly with IBM and the Ground Data Systems Division, I think is what it was called. We had support from a person from the Air Force. His name was Wilson, and was a tremendous help because we had very limited staff. He helped with the navigation on the Apollo tracking ships and many other areas.

I probably could mention one other technical thing, and then we can maybe move on. The navigation methods include the ability to relate deviations in position and velocity at one time to changes in position and velocity in another time. That's done through what is called partials. We spent a considerable amount of time looking at different ways to compute partials. I asked Sam Pines to come up with the equations to use the position and velocity at each end to define a mean conic (a path that a vehicle would follow if the only force were that from a spherical Earth), instead of using just a plain conic based on one end on which to derive a partial. Those equations along with mean conic equations published by Bill Goodyear led to the computationally efficient use of mean conic partials in the Apollo Orbit Determination Program

I asked Sam Pines if we needed to model any relativistic effects when processing Doppler measurements. The tracking network Doppler measurement is a very accurate, precise measurement. He assigned a Ph.D. physicist to make the determination, and after a long time the question was not resolved, so he assigned another Ph.D. physicist also without resolution. I mentioned to Sam that we had to make a decision. I wanted to model the measurement as a simple range change for the Doppler sampling interval, taking into account the speed of light delays. After a couple weeks Sam came back and said, for the two-way Doppler we intended to use (the signal travels from the ground tracker to the spacecraft and back to the same ground tracker) that it was not necessary to model relativistic effects. I asked him how he came to that. He had asked research people if the Doppler data could be used to validate Einstein's work and they said no.

Sam Pines went with us to meetings at Goddard and after work once suggested an Italian restaurant. He knew about restaurants and other areas with which I wasn't familiar. I'd never been to an Italian restaurant. I had no idea what lasagna was. He said I'd like it and he was right. Once on a trip to California, probably to Rockwell, we also went to a restaurant after work and he ordered an after-dinner drink. I didn't know anything about after dinner drinks. He must have read my puzzled face; Sam suggested Kahlua.

ROSS-NAZZAL: I think this might be a good time for us to break and change out our tape real quick.

SCHIESSER: Oh, I went over the hour and a half already?

ROSS-NAZZAL: Oh, no you didn't.

[Tape change]

SCHIESSER: Let's see. Where did we leave off?

WRIGHT: You were talking about going and having Italian food for the first time.

SCHIESSER: Oh yes.

ROSS-NAZZAL: Yes, and Kahlua.

SCHIESSER: Yes. Earlier I mentioned the process of relating deviations in the position and velocity at one time to that of another time for coasting flight. It is also necessary to express vehicle position and velocity relative to the Earth and Moon in a variety of coordinate systems and that requires transformations such as between an inertial star fixed coordinate frame and lunar and Earth fixed coordinate frames. A TRW task to examine such was given to Pete [Pedro Ramon] Escobal. The technical material required for the transformations had its origins with JPL work, astronomers, geologists and others. Pete published some of the transformations in his book (*Methods of Astrodynamics*, 1968). He gave me a signed copy of the book with: "Thanks for the interesting projects."

We were interested in the accuracy to which one could determine the orbit of a vehicle circling the Moon, in practice, using actual flight data, through the use of the Deep Space Network in this case. Langley had placed the Langley Lunar Orbiter in a high elliptical lunar orbit at first for the lunar mapping mission, but they dropped it down to a circular orbit for us. We got with JPL and Langley and jointly used JPL's navigation software to process the DSN

Langley Lunar Orbiter tracking data. Later we used that body of knowledge to help verify the independent control center navigation capability in Houston.

The trajectory/orbit that we were fitting to the Doppler measurements didn't reflect the actual satellite motion when the Langley Lunar Orbiter at its closest approach to the Moon in the high elliptical orbit. That resulted in unexpected variations, wiggles, in the difference between the actual measurements and those expected. The variations showed up across the entire face of the Moon when the satellite was placed in the circular orbit and were due to our imperfect knowledge of the gravitational field of the Moon. That was the very first signs of poor knowledge of the Moon's gravitational field. That occurred in probably the 1965 time frame and though some improvement was made to the model, its imperfections plagued us throughout the entire Apollo Program.

Toward the end of 1965 our civil service orbit determination group included Bob Savely, Laura [Laurel A.] Phillips, she went by "Sissy" Phillips, Jerry [Jerome N.] Engel, an older member, and Linda [K.] Schultheiss. One of Sam Pines' people, Lamar Bannister, was helping us at that time along with TRW and IBM people.

Bill [Wilbur R.] Wollenhaupt from JPL joined my group. He and I and Bill [William] Boyce and some others traveled to Langley, and met with the Langley people over the weekend, we spent the whole time reprocessing Langley Lunar Orbiter data day and night. Wollenhaupt was a man of iron. I was getting tired. One of the guys laid over onto the online printer and fell asleep. It was rugged. But Bill kept on trucking.

That gets us up through 1965 more or less.

ROSS-NAZZAL: I thought of a couple of questions as you were talking. Can you give us a sense of what your daily workload was like? When would you go to work? When would you come home? What would you do?

SCHIESSER: Well, we would get to work at, I guess, around eight; not any later than eight. Then we would work anywhere between eight and fourteen hours depending on the circumstances, sometimes including weekends. I seem to think that on trips the technical sessions were long. It would be late getting to a restaurant. Some sessions would go for a couple weeks and there might be a chance to visit Knotts Berry Farm [Buena Park, California] on one of the weekend days, but that would be exceptional, at least in my experience.

I've heard that the long hours put a strain on some marriages, but I was single prior to the first lunar mission. The atmosphere, though, was maybe not so grim as you might think. For instance, one of the people that joined us was Bedford F. Cockrell. He went by "Butch," Butch Cockerell. I recently asked Butch what it was like for him. We were at the Houston Petroleum Complex; this would have been in about '63 before we moved out to the current JSC site. Butch said that on Saturday they would go to the computers located at the university. They would submit their deck of cards with computer instructions and then play baseball. Later they'd go in and get their deck after it had been run on the computer and submit another run and go out and finish the game. He said, "You know, it was fun. It wasn't work." That was his view.

ROSS-NAZZAL: Did you agree with that?

SCHIESSER: For me, personally, I was a little bit too serious for that. It's only recent that I've tried to achieve a better balance. It's time for me to learn how to relax more, be more peaceful, and enjoy the simple things of life on a daily basis, instead of being in the head all the time, and thinking constantly day and night about the intricacies of manned spaceflight.

ROSS-NAZZAL: Did you get a chance to go out to the remote sites or the navigation ships, the tracking ships?

SCHIESSER: In the sixties I drove down to view the Manned Space Flight Network tracking site at Corpus Christi [Texas]. It was a thirty-foot diameter dish S-band tracker. S-band is a frequency band that the trackers used. They transmitted a radio signal at about 2200 megahertz. The C-band trackers used a lower frequency.

The Apollo S-band trackers, including that at Corpus Christi, used an X-Y mount instead of an azimuth-elevation mount. It had the advantage that the antenna didn't have to rotate 180 degrees for an overhead pass of the spacecraft. I was out to Goldstone once, saw the 85-foot diameter dish tracker, and I think I saw the 210-foot dish as well.

We used the S-Band trackers to acquire two-way range and two and three-way Doppler measurements. The two-way measurement travels from the ground antenna to the spacecraft and back to the same ground antenna. The three-way Doppler measurement is obtained when the signal leaves one ground antenna and returns to a different ground antenna.

There's also the possibility of a one-way measurement for which a signal sent by the vehicle is received by the ground antenna, but that type of measurement was not used during Apollo since it is subject to frequency differences between the onboard and ground oscillators.

The GPS [Global Positioning System] satellite signals are one-way. They send the equivalent of a one-way range and Doppler measurement. It's called pseudo-range, because it's got a big bias on it due to the local clock bias. The Doppler measurement also has a big bias on it due to the local clock frequency error. The biases are determined along with position and velocity through the use of measurements from four satellites concurrently.

ROSS-NAZZAL: When you moved on-site, where was your office located?

SCHIESSER: I had an office in the office wing of the control center. The control center has two halves. One is a three story office wing, and the other has no windows and houses all the computers used during a flight and the Mission Operation Control Rooms, two Mission Operation Control Rooms, back then. They were on the second and third floor. The main frame computers were on the first. They were in a huge room, and there was a glass window partition between them and a long, narrow room adjacent to the computer room. In the long narrow room where the consoles were for controlling the computers and for doing the ground navigation function. The ground navigation function was done there on a two-bay (two TV screens) console, in concert with the operators of the computers. From what I remember, the first floor computer operators were the only ones that could send a command to the computers used to fly the Apollo mission.

I don't think any of the Flight Controllers, nor those of us at the navigation console, were able to enter a command to the computer, other than to call up displays and push a button to make a hard copy, should that be considered a command. The hard copy would be sent through a p-tube [pneumatic tube] to us, sort of like those at outside drive-in banks.

In order to remove an erroneous observation from a batch to be processed we had to ask the computer controller to edit that point between two coordinates on a display that we both had in view on our separate consoles, and they would enter the edit command for us. They eventually got tired of that and during the Shuttle Program, there were two people that could send commands to the computer, the computer supervisor and the ground navigation person at the two-bay console for data editing.

ROSS-NAZZAL: Great.

SCHIESSER: Yes. I worked in the third floor of the office wing for twenty-eight years.

ROSS-NAZZAL: Wow.

SCHIESSER: Roughly; well, maybe not quite that long. I think we moved to the building in '63 or so, and I left at the end of 1989. That would be around 26 years.

During Apollo Chris Kraft, Bill Tindall and John Mayer had separate offices just down the hall. Kraft had a pretty good sized corner office next to a big conference room.

We were heavily involved in the onboard navigation for Apollo. Bob Savely, Jim Blucker, Rick Eckelkamp, and others staffed an onboard navigation console on the second or third floor of the control center. Jon [C.] Harpold, Claude [A.] Graves [Jr.], Floyd [V.] Bennett, and others also staffed consoles in support of trajectory control, guidance and targeting.

ROSS-NAZZAL: I think that's all the questions I have, unless you have anything, Rebecca.

WRIGHT: No.

ROSS-NAZZAL: All right. Is there anything you want to add?

SCHIESSER: I think this is a good place to pick up on later.

ROSS-NAZZAL: Okay. That sounds great.

[End of interview]