RAYMOND HUGHEY
POLARIS MISSILE/POSEIDON MISSILE

Introduction:	<u>MUSIC</u>
Raymond Hughey:	I was born in a little town by the name of Winnsboro in South Carolina. We were there because my dad was an officer in the CCC [Civilian Conservation Corps], and he was on assignment in Winnsboro, and after he got transferred out of there, I never saw the town again. We lived in several towns in the Carolinas while he was doing that. The longest time [was] in Charleston for almost two years. When World War II started, my dad, who had an industrial engineering degree, got requested to go to Glenn L. Martin in Baltimore to help set up production lines special for guns and aircraft in World War II and also the test programs to test out the planes when the guns were installed. And so we moved to Baltimore when I was five. We moved into a brand new development there, what was essentially like the Boomtown houses here on the base that were here for so long. They were brand new houses, and we were among the first to move into the community. And I remember as a kid, the first two or three days we were there, I was out playing and got ready to go home, and went in, the door was locked. And I knocked on the door, knocked on the door. Then I happened to look up into the window of the house next door, and there was my mother. I was at the wrong house! [*Laughs*] They all looked just alike. And the other house was empty, as most of them were at that time. A year later, I started in the first grade there in a brand spanking new, huge school that was built primarily for workers at the Glenn L. Martin for the war effort. Then by the time I got to the second grade, there were so many people there that that the school had been overwhelmed, and I wound up being bussed to what had been the previous school in the area, which was then all second graders. The entire school was second graders. We stayed there until the war ended. Near I guess the last year of the war, my mother's two younger sisters came to live with us, and I remember celebrating when we heard the word on the radio that Germany had been defeated, the war in Europe was over. At my age, it took me a day to understand that the war wasn't over. We still had one with Japan, so a few months later we celebrated again when that war was over. We moved back after that to South Carolina, to Greenville, South Carolina, where we lived for a year while my dad built a house out on my granddad's farm. So I spent the next ten years living there and went to school at Easley High School. After graduation I left South Carolina and went to University of Alabama and graduated there in three years, then went to University of Kentucky for graduate school. Since that time I've taken quite a number of other graduate courses, but

	somehow I always looked for courses that applied to what I was doing and didn't ever pursue a masters' degree, even though I did about a hundred hours of graduate work. Well, while I was at the University of Kentucky, I got disillusioned with the way the department up there was working up there, as did most of the other first year graduate students, because it was clear that the only interest the faculty had in graduate student was to teach labs, lectures, and help them on their research and were not interested in you getting a degree. In fact the only one of us that stayed there of the 24 of us that entered was Bill Elsaesser, who you may remember. He stayed to get his graduate degree, but it took him four years to get it there. I think we made the right decision. They probably straightened that out a few years later, so I think it's a good school there now. But I vacillated back and forth about whether to leave. One of the days I decided I had had enough, I decided I was going to go see if anybody was interviewing that day for jobs. So I went over, and sure enough there was a recruiter there from Potomac River Naval Command, and he was interviewing for labs in the Washington area, including Dahlgren. Well I liked the idea of being out in the country rather than the city, so I started asking him questions about Dahlgren. It turned out he had never been to Dahlgren, so he couldn't tell me much about it. But he said that he would send me information about Dahlgren, which he eventually did. Then I got a little bit better satisfied, so I didn't ever interview for another job, and as the semester drew to a close there, it happened to me again. I decided I don't really need to waste another year here. And it turned out that that day I got an offer from Dahlgren. And in the meantime, I had applied for what was then a brand new Air Force program to get a direct commissioning as a technical officer in the Air Force and gone up and spent a few days up at Wright Patterson Air Force Base taking tests and so forth and had that offer there to go into the Air Force and my choice of two groups, the first of which was during the summer, the second was in November. Thinking I wasn't quite ready to go into that yet, I selected the November one, but I did not sign a contract because I noticed the contract said it was open until the end of the summer or something like that. But anyway, I decided to take the Dahlgren job, although I had told them that I may be leaving in November to go into the Air Force. They said "Come ahead anyway. We can use you." So that's where I first learned about Dahlgren.
Interviewer:	And that was in 1958?
Raymond Hughey:	That was in 1959
Interviewer:	1959.
Raymond Hughey:	When I came to work here, I was immediately taken over to talk to Dave Brown,

	and so I went into the Polaris program, which was very early in development at that time. There had been no test flights yet on the missile, although the contractor was working hard on building some test missiles. So I wound up working on Polaris right from the very beginning, the first day. I feel like I came to Dahlgren at one of the very best times, when the laboratory was changing from a proving ground to a laboratory. It was a naval proving ground when I came, and shortly after that they did change the name and make it a laboratory. The first thing I had to do was to learn about simulations, missile trajectory simulations, but within a month, I had been assigned a job of figuring out a way to correct for guidance system errors in flight and come up with a means to take measured risks, biases and accelerometers, and the rest of the gyros, linear risks, and acceleration sensitive risks, and take the numbers from the factory and field tests and use those to correct the pre-settings so the missile would still hit the target in spite of the problems with the guidance system. I spent probably a good part of my first year doing that. Now also, the lady who was to become my wife was working on the pre-setting computation itself, and so she computed the pre-settings for the very first test flight, first guided test flight from the pad at Cape Canaveral which was in the fall of '59. And then all the rest of us checked those numbers. They were really checked from beginning to end. Computing the pre-settings at that point consisted of flying a nominal trajectory on the computer and then iterating in to find the cutoff point that would give you the exact range that you wanted to hit the target and then look at the velocity you had there, and that was the correlated velocity you wanted to have cut off, and then you would integrate the guidance equations backwards to the launch point in order to determine the pre-settings. And we did that for the pad shot. So that was what was done for those pad shots.
Interviewer:	Would you mind telling us your wife's name?
Raymond Hughey:	My wife's name was Nancy Kipps, and she arrived at Dahlgren on the same day that I did. I saw her in the Industrial Relations Office when I arrived, and the next time I saw her was when she came also into the same branch there with Dave Brown, and Dave Brown introduced us to each other. My name changed at that point as well. I'd been "Raymond" all my life. Nobody had ever called me "Ray." Dave Brown introduced me that day to everybody, including my future wife, as "Ray," and it's been "Ray" ever since. We were married in July of 1960, July the 30th, so we're coming up on my 49th anniversary here. It took us a little while to get together. I remember—we were put in the same office, by the way, so that was an advantage. She was dating a jet pilot at the time [*laughs*], but I had the advantage of being with her every day

	[*laughs*], so I got an advantage out of that. She was a Mathematician, went to school at Westhampton in Richmond. She grew up on the Virginia Tech campus, so she was a college-associated girl all her life. Her father was a professor at Virginia Tech for forty years. Of course we spent so much time down there, I sort feel like Virginia Tech was part of my school too. We got married very shortly after the first two firings from the submarine. The scheduling of that program is amazing when you look back at it. They started the idea in 1956. In late 1959, we had the first pad launch test flight, and we had tactical-type missiles, of course with dummy warheads, launched from a submarine in mid-July of 1960. At the point we had people from other services who were working missiles, saying, "You can't possibly do this. This is not going to work. This program's going to be a disaster." And so there was the one shot that was scheduled, and they fired it, and it went off without a hitch. It was perfect. And just a few minutes later, they went ahead and fired a second one, and it was also a perfect shot. So we got the program off to a very, very good start. They built submarines, lined them with missiles. We did the software, all in that four years. The Navy has been very conservative, especially in the strategic area over the years, except during that phase. They were really pushing the technology at that time.
Interviewer:	First time? That was the first time for that?
Raymond Hughey:	That was the first real fire control computational program for the Navy. When I came to work here, we were in Building 218, over there with the big computers. The NORC [Naval Ordnance Research Calculator] was the one that was there when I came. I spent many hours down there with the NORC at times. People that look at computers today have a hard time relating to that one. It was one you walked around in. It was an enormous machine. [It was] designed by the Navy and built on a contract by IBM. I've always felt that's where IBM got their jump in large scale computers. They'd been building the small things for years, but that was really their first adventure into a large machine. And that machine was very special in another way, and that is that it probably holds the record for being the biggest and fastest machine in the world for the longest time. It held that position for four years, which is really a long time in computer history. Yes, I started in 1959. I worked on the FBM program for a little over twenty years, I guess it was, continuously, and had some connection with it for most of the rest of my career, attributed to some parts of it, although I was good in other things as well.

I spent three years in F Department from 1979 until 1982 and went back to K. So I was in K Department for 32 of my 35 years.

There was a considerable amount of competition for the job, especially by General Electric. They were the ones with the inside track since they were the ones who were going to build the fire control hardware. There were some other companies that attempted to get into it, but they never really were able to make any headway because they didn't have the qualifications. We had two big advantages. One was the computer; we were the only one with a computer that could handle the problem of doing a worldwide grid of trajectories that would be required to generate the pre-settings.

There was considerable competition for who was going to do the fire control software for the Polaris program from the beginning. The primary competition was between General Electric and us here at Dahlgren. There were some other companies that tried to get into the pact, but none of them could show enough qualifications to convince the sponsor they were a serious candidate. GE had the big advantage in that they were building the hardware for fire control. They felt that they should also handle the software. We had two big advantages here at Dahlgren. One of was the big computer that we had, the biggest in the world, which was easily capable of handling the worldwide grid of trajectories that had to be run and generate pre-setting information to produce the equations. The other advantage we had was in having Dr. Charles Cohen here at Dahlgren, with his reputation of accomplishments along the way.

Actually Dr. Cohen was the first one to find a gravity computation that would have enough accuracy to able to be used to give precise trajectory computations for a long-distance missile to a target. Before that, any trajectory done was just a rough approximation as to where the missile would actually go. That was a very key one that he made. Then he made many other computations along the way to improve that gravity model, and we had generated here, as a result of that work, the standard gravity model that's used worldwide virtually by everyone that uses a gravity model for the first several accepted worldwide to-date representations. There were some other labs that got into those kind of computations later, and one or two of those along the way did get computed by Johns Hopkins's lab up there. But Dahlgren stayed the leader in that all way the through.

Let's see. Who were some of my coworkers? We had… Well, when I first came into work, there were a lot of new hires that came to work in the same branch. Carlton Duke was one of them. Ron Crutchfield, who later when on to Lockheed. I continued to work with him through Lockheed years later, but Ron made some good contributions at Dahlgren before he left. Barry Bressler was here. There was Jean Calvert, who was one of the first in Dave Brown's group, and she made a lot of accomplishments along the way. Let's see… Right at the beginning, of course

	Dave Brown was key to that effort once it got going with the help Dr. Cohen. Once established, Dave Brown was the key to Dahlgren's role, early role, in both the analysis and the fire control program development. Without him, I'm not sure what would've happened to that program. He coined the word "geoballistics," and that was really the work that we did here in those days. There was Dr. Cohen, Dr. Russ Lyddane, who was Technical Director, who we worked with some. He was still contributing technically to some of the problems, and I worked personally with him some. Doreen Daniels and Dee Reinsdorf were here. Doreen had worked on ASROC [anti-submarine rocket] prior to the Polaris effort coming along. And she was still involved in that for several years after I got here, and the ASROC work.
Interviewer:	Were the roles pretty much established by the time you got here?
Raymond Hughey:	There were still some… GE was still making attempts to get that, but very shortly after I came, it was established. SP [Special Projects Office] made the decision that we were going to be the ones to do that work. And actually the earlier decision was the handling of the targeting data would be done here, and that decision was made first. And SP made the decision that there should be no contractor involvement in that targeting effort, that and that together the fact that we had the computer facilities to do that job. So that was an added edge that came along there.
Interviewer:	Fire control came first? Or was fire control and targeting kind of together?
Raymond Hughey:	Targeting actually—. The assignment of targeting work came to Dahlgren before the fire control. Not by much. But the fact that it was here helped contribute to our selection for the software development. And then SP, at least as long as I was associated with them, never gave into the idea of contractors being involved in the actual targeting, using the specific top secret target data. When the Poseidon idea came along as missile with multiple independently targeted reentry vehicles, I first heard of it when I went to a meeting out at Lockheed, still associated with the A2 work. And one of the executives out there got together with me and showed me the kind of missile that they were going to present to SP as the next step in development, which was in large part what the Poseidon turned out to be. And he says, "We have a problem with this missile, and that is nobody knows how to guide it, especially for a launch from a submarine." If you were going to launch it from land, you had huge computing power. You could compute pre-settings for it and again use the guidance systems that could use those and still be very simple. Now, some of the problems with this, of course, was the computing technology was not very advanced in the '60s. An example of that is the thing that NASA

	mentions from time to time now about the Apollo guidance and the moon shot. The computer onboard that mooncraft was somewhat less powerful than that that was found in a calculator wristwatch of two decades ago now [*laughs*]. Well, what we could put in our missile was a lot less than that even. It had to be a very simple computer, and it used a plated wire memory with very limited memory. And then furthermore, the computer we could have onboard had very little power compared to what we have today. In fact, it had 32K of memory in it. That was it. It was a pretty complex problem to solve any way you looked at it, but we knew that's what we were going to be stuck with when we deployed this missile, so right from the beginning, we realized we had a challenge. But anyway, right when I came back from that meeting at Lockheed and talked to Dave Brown about it. I think that was on Friday, and on Monday was the meeting at SP that Dave Brown attended where Lockheed made their pitch about the new system—the new Poseidon system. It didn't have that name yet. It was called by a variety of names, Hydra being one of them. I can't remember the others now. Dave came back and said that they had discussed up there the problem of how do you guide this thing, and the people from Draper Lab—what was at that time called MIT Instrumentation Lab, it was before it broke off from MIT—got together with Dave and some people from a couple of the other contractors and talked about what they could do about the guidance, and MIT Instrumentation Lab said they were going to look at expansion of the Q guidance that was used in Polaris A1 and A2 which used an initial velocity to-be-gained vector, and the matrix of partial derivatives, with respect to position and velocity in order to guide the missile to the target, and it was those touchstone equations that were running backwards in the computer to generate the pre-settings. GE was going to work on Delta guidance, the variation of the Delta guidance that the Air Force was using. Dave Brown told them we would see what we could find and contribute to it. And so he came back on a Tuesday or Wednesday, and we talked about it at the office for a day or two. The next day was Thanksgiving, and we took off that day, and then on Friday, Jim Brown and I came back into work and started trying to figure out what to do about this problem. An explicit guidance system had been brought up, and the problems with it had been brought out, and it had been essentially discarded at the previous mission meeting. But I kept thinking about the explicit guidance. And one of the problems they saw with it was you could compute in fire control with the oblateness term in there, but then you couldn't compute the reentry portion of that. And so you were going to have a mismatch, and it just wasn't going to work. So I kept thinking about, well, if you use the point mass, the Stripp Kepler equations, and do a computation using that for the target, and then compute target offsets to provide a correction for the reentry portion that if you were using basically the same trajectory, you could do that, and you could be able to functionalize those target offsets and let the guidance computer compute the information to a false target,

	which was offset by the pre-computed target offsets and reach the target. So Jim and I started working on that and doing simulations with it, putting in perturbations of the flight, and one of the problems with it was that you had a bus that dropped off the individual reentry vehicles, and you didn't know what that was going to look like until you were at your launch position ready to fire. And so if you could produce the target offset for a target, the way you got to that target might be very different depending on what other jobs the bus had to do in delivering the other reentry bodies, and you had no power to compute those trajectories on board. So the question was could you come up with a target offset that would work with the huge perturbation trajectory where there might be hundreds of miles between the first drop and the next one. We had first discovered that if you had the data with a normal missile perturbation, the target offsets were costing enough that you could use them and correct for those perturbations and work just fine, easily within the accuracy requirement of the system. But we worked with the idea of how can we optimize these target offsets in a way so that even with these huge perturbations which you get from trajectories essentially designed for onboard a submarine, without the help of simulation, and we came up with a method of doing that, and it worked pretty well. We worked on that every day from the day after Thanksgiving, we took off Christmas, and we worked every other day up until January the 15th, and by that time, we thought we had the problem solved, and we put together a presentation, which Dave Brown presented at a meeting up in Boston, where GE and MIT were presenting as well. GE said they were unable to find something that worked for the Delta guidance that was usable. MIT said they could marginally meet the requirement with a variation in the Q guidance, but they didn't know how you would ever solve the pre-setting problem onboard the submarine, it would be so complex. Dave presented our method, and it was agreed that that was the only solution that was satisfactory to the Navy. A while after that, MIT Instrumentation Lab, generated a letter to us here at Dahlgren, essentially announcing the fact that that method had been selected, the one we used, giving us credit for it. They gave a description of it in detail. For that reason, the letter was classified. That letter was kept in a safe in the K Department files for some years, a good number of years. But we went through periods here during those years where there were actual ceilings on buying security containers. You could not get any more security containers. So periodically there would be a command from above that said get rid of everything over five years old unless you could give us written justification of why you're keeping it; we've got to make some space for our new classified material. And one of those came in while I was away at school, and that letter got swept up in that clean out there. There wasn't anybody here that looked at it, or many people that knew where it was or what it was, so that letter got sent to burn, so we lost that letter that gave us credit for coming up with the guidance scheme for the

	Poseidon. The U.K. made the decision to build submarines and take the Polaris missile. The agreement was that they would use the boosters and the guidance, but they would do their own reentry systems and reentry bodies, which meant that they could not use the pre-settings that we used for ours. And so the U.K. came to us and looked at what was involved in the pre-setting problem and decided it would be much better if we did it for them rather than their doing it themselves and trying to reproduce the facility we have here at Dahlgren, so we had another program on our hands at that point because the pre-setting computation had to be done from scratch for them because of different characteristics. We also were breaking the U.K. people in on the program, and there were a lot of meetings between the two organizations, one in the U.K., one at Dahlgren. They sent a number of representatives here that stayed for as long as two years at the time to learn what was going on, and we had a team put together to specifically produce the U.K. program. Doreen Daniels was very involved in that. I went to the U.K. myself three times, I believe it was, on that program. One thing that I found especially interesting was I was on a lot of different SLBM submarines for the U.S., just about every class, and probably nearly half the boats at one time or the other. And I got tours of the ships. I never got a tour of the nuclear section. I never saw the reactor or any of that equipment there.
Interviewer:	On the U.K. boat?
Raymond Hughey:	On the U.S. boat. I never saw it on the U.S. boat. But when I went over to the U.K., and they gave me a tour of the first U.K. boat, they showed me everything. They showed me the reactor and how it worked and the whole works, and I thought that that was very interesting that I was able to see it for another country but I couldn't see it for my own. That's one of the things I remember about my trips over there. I made some good friends among the Brits, still communicate with them today. We visited one of them, as a matter of fact, on 9/11. Nancy and I were vacationing in—we started down in Paris, then went to U.K. and Scotland, but on 9/11 we were out driving in England there in a rental car and decided to turn on the radio, and when we did, the first thing we heard was they were talking about the second tower that was coming down. At that moment, it was falling. That's where I learned about what was happening. Well I had an agreement to meet Tudor Parry that evening in Bath. And so we did that. But that was—the connection the U.K. there got reinforced again because we sat there and talked about what was going on in the U.S. that day. And I was impressed with the fact that we got sympathies from so many people that we ran across in England who recognized us as Americans. People were really on our side at that point.

	Doreen made an awful lot of trips—Doreen Daniels—over to the U.K. and hosted a lot of the U.K. people. I was involved in it, but my part was not as active in the U.K. program as it was in the others. The U.K. decided to build its own reentry vehicles and warheads, partly because they wanted to have something of their own but also because they were not able to get as much information as they wanted from this country about what was in the warheads and the reentry vehicle itself released to them, we'd known, and so they decided to go their own way on that. It was a fairly expensive decision for them, but they did that. And I don't think they ever regretted it. They always indicated they thought that was the right thing to do. To me it appeared right when I came in in the early days of '59 that there was quite a bit of diversity among the workforce, especially in K Department. There were a lot of women involved in key scientific roles here, and there were a lot of blacks involved in key roles, and we very soon had other races as branch heads and division heads, [and] a little later as department heads. I saw the center really as a leader in that area. In our own division, SLBM was a pioneer in certain aspects of that, especially with the women. Doreen Daniels was the first branch head on the base, first GS-14 female on the base. And also Dee Reinsdorf was the first technical [GS-]14 female on the base. And we also had a number of minority employees that reached those kinds of positions in our department. So I think we were a leader in the area.
Interviewer:	Ralph Niemann had lots to do with that.
Raymond Hughey:	Ralph Niemann had a lot to do with that. He was very insistent that we looked at the candidates hard when they came in and didn't make any pre-judgments based on what they looked like. That was very much on his mind. Iwas always very frustrated by not taking the extra step in research before committing to development. And I thought especially as time went on in the program that versions of trying new things got worse in the Navy. One of my regrets is I wasn't able to be more persuasive in convincing them to try some threats to technology a bit. I think we would've had some—we could've had some systems that maybe could've gone a little bit longer without losing their effectiveness if we'd done that. There are probably a lot of things I know I could've done a better job if I'd had the right foresight or been willing to challenge someone a little harder and try to get through it. I always worked on the idea that you could get a lot more done if you didn't care who got the credit. And you could use that to some advantage with the sponsors where I would plant an idea somewhere and call it the guy up there's idea and before long he would be out selling it himself. And that worked pretty well. Dave Gold carried the

	weight on a number of times in that kind of situation, where I probably would've had no chance of selling it myself.
Interviewer:	You might talk about who Dave Gold is.
Raymond Hughey:	Dave Gold was Chief Engineer in SP-23, which is Fire Control and Guidance Branch in the Sponsor's Office. He was probably the closest thing they had to a systems engineer in the early decades of the program. He understood the whole system, made contributions to the whole system, although he was in that one branch, and perfected it in many ways. I had a lot of respect for him. He was a… He had his own peculiarities, like a lot of those people do, but he was quite a man to work for. Well I guess the one that I appreciated the most was the John Adolphus Dahlgren Award, which was given in 1977, I believe. And in that citation they did mention the Poseidon guidance contribution on there, so that's one place that that's written down. I appreciated that award. There were a number of others: two Meritorious Service Awards, a number of Outstanding Performance Awards, but I frankly never paid a lot of attention to awards, but the one John Adolphus Dahlgren Award got my attention. Back then we had a little bit of a museum there in the K Department lobby. We had in that a log from the old Aiken Mark II computer that was one of the first computers built for Dahlgren, one of the first major computers built for Dahlgren. It had been put together and checked out up at Harvard [University]. It was to be transferred to Dahlgren, and there was a crew that maintained the machine, and several of the people that worked on that continued to work at Dahlgren for years, including Ralph Niemann and Bill Burke and others. It was a relay calculator, and one day in the afternoon there [at Harvard], the machine stopped, and Bill Burke went in and found there was a moth caught in one of the relays that'd stopped the computer. He removed the moth and restarted the machine and logged that he'd found the bug in the machine, and debugged it, and it was running again. That incident is the origin of the term that's now used throughout the computer world about debugging the computer, debugging the software, or anything like that. That's where it came from. Bill Burke came down here and worked on computers for many years here after that. I knew him well. That log was kept, and it was at various times in the first museum here on the base, which got taken away because they needed the space for something else, and then we had it in the lobby, and that was taken down. When they did, I managed to get the thing and put it into the safe upstairs, and that's where it stayed for some years. Well, I was getting concerned about it because it was

	1940s acid paper the log was on. It was just a moth, and it was held in place by a piece of Scotch tape. I would get asked to get it out and show it to visitors when they came, and as time went along, I finally looked at that thing and realized one sneeze and that bug would be gone [*laughs*]. That would be the end of it. Somewhere along that time, I got a call from the Smithsonian, and they said they understood I might be able to tell them something about where the original bug is that started that phrase of debugging a computer. And I said, "Yeah, I have it in my safe." And so then they started to pitch to say the Smithsonian should have that. It's a big piece of American history. Also they said it needs to be preserved; it needs to be properly preserved. And they said, "How are you preserving it now?" [*Laughs*] Anyway, I talked to a few people around the base about it, about what we wanted to do about that and didn't get much of an answer. "Can we preserve this thing? What can we do with it?" Anyway, at some point after thinking about that a while, I called them up and say, "You can have the bug. We'll bring it to you. We expect to be able to get it and display it if we need to from time to time. But you can have the bug and preserve it because I'm afraid any day now it could just disappear." So we wrote a letter and gave the bug to the Smithsonian. I took it up on July the 3rd and presented it to the Smithsonian, and that was in 1992, I believe. It could've been '93. I take that back. I believe it was 1993 I took that up there. And after that, I continued to get calls from various people: "Do you know where the bug is?" I'd tell them, "Yes, it's down at the Smithsonian, but it's not on display. They're supposed to be getting it stabilized so they can display it." One time I got a call from what effectively is Japanese Public TV, and they wanted to film the bug, so I put them in contact with the curator up there at the museum. And they did go up and make videos of it and put it out on their public TV. They told me they'd send me a copy of that, although it was going to be in Japanese, but I never got a copy of it. They thought they owed me something, I guess, so they sent me a clock [*laughs*] because I'd helped them in that [*laughs*]. One of the people that was at Harvard during the days that the Mark II was being checked out was a young lieutenant, Grace Hopper, and she was working with the people up there. She later became a real spokesman for computers in the Navy and other things, gave many presentations all over the country and even overseas. But almost every presentation that she gave included that story of the moth. And it was very interesting to watch her progression over the years at the different times I heard her give that thing. The first time I heard her tell it, she told it, she told it pretty much the way I'd heard it: Bill had found it, and they looked at the log, and they started discussing it up there at Harvard. I thought that was very interesting. As time went on, first she became a party to it, she was there when the moth was discovered. And then a little later she was taking credit for it that she had found the bug, so as time went on and she got older, she was moved

	up in rank, I guess she was an admiral when she retired, but she became the action person in that story, that tale she told. And you see that in textbooks now, in the history of computing, they say that it was discovered by Grace Hopper and then labeled [*laughs*], but it had Bill Burke's signature on it in the log.
Interviewer:	F Department. You were there for two years?
Raymond Hughey:	Three years, headed the Electronic Warfare Division. I headed that for three years, had a very wide variety of projects in there. Programs—a lot of them were unstable. They were in the budget one year after a next. A lot of them were very highly classified. Some of them were obviously black programs, not because we were afraid the enemy would find out what we were doing but because our own people would find out what we were doing [*laughs*]. But it was some very interesting work there. We sent people overseas sometimes with no Navy identification and suitcases full of cash to get various things done. There was a case with a group developed a device for detecting nuclear weapons, and it was on ships, and so we were able to tell when you passed a ship whether it had nuclear weapons, one ship passed another one. But the head office up there in Washington was convinced that nobody had those but us. So we went to places like Israel and airports with nuclear weapons, which was against the treaty, and also Japan. And we received notification from those countries, "Just want to let you know that we know you had nuclear weapons on that ship when you came into [*laughs*] our port." So we were overconfident in some respects in this country about what our capability compared to others.
Conclusion:	MUSIC

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