SOME MEMORIES OF MY CAREER
By
John R. Sellars

I graduated from New Mexico State University in June of 1945 with a BS in electrical engineering and immediately took a job with the Johns Hopkins Applied Physics Laboratory in Silver Springs, Maryland, where I found myself working on ramjets. I worked under Dr. Tom Davis who mentored me and we ended up being the resident experts on the internal flow of ramjets. My ambition, however, was to go to graduate school and, the war being over that summer, the next June I left for the University of Michigan where I enrolled in the Physics Department. Although I was in the Physics Department, I earned a living by working on various projects in the Aeronautical Engineering Department and I took a number of courses in this Department in advanced fluid mechanics. Because of my background from Johns Hopkins and since I was in the Physics Department, I was allowed to take the advanced courses without strictly meeting all the Department's prerequisites and I passed them all with no problem.

I received a Masters Degree in Physics in 1950 and I decided to shift to Aeronautical Engineering for my Doctorate as I felt that this was more my true interest. I began teaching in the Aero Department in the fall of 1951 with the title of Lecturer. In the spring of 1952 I received my Doctorate and was appointed an Assistant Professor. I taught aerodynamics, nuclear engineering and computing (using analogue computers!) I also worked on some research projects involving flame propagation with my office mate, Dr. James (Gene) Broadwell who had also received his Doctorate the same time as I did. This work was funded by the Air Force through Wright Field.

Early in 1955, I began thinking of getting some industrial experience, so I broached the idea of a year's leave of absence. I had taught for four years at that point, one year as a Lecturer and three years as an Assistant Professor. There were numerous recruiters coming through all the time. My wife, Ethel, and I were most interested in going to California, since the idea of a warm climate appealed to us after all those Michigan winters. There was a big supersecret project starting, and companies were avidly recruiting, but they weren't able to be very specific about the project because of its classification. There were several faculty members around who knew a little bit about it and they told me it was the intercontinental ballistic missile program. I talked to a Michigan graduate named Bill Dorrance about joining his group at General Dynamics in San Diego. I also talked to a recruiter from the Ramo-Wooldridge Company and indicated my interest. Ramo-Wooldridge then invited me to come out and visit them. I combined this trip with a trip to Boston, where I delivered a paper that I had written with two other faculty members, and I flew directly from Boston to Los Angeles.

When I interviewed at R-W, I talked to, among others, George Solomon and Louis Dunn, who was director of the ICBM program. Because of the tight security, it turned out that I already knew more about the program than they were willing to tell me in this interview. I left, feeling a little uncertain as to what the job really was. A little later, I received offers from both General Dynamics and from R-W. They were very comparable in pay, with the R-W offer a shade better. However, I had considerably more information about the General Dynamics job. General Dynamics was also working on the ballistic missile program, producing actual hardware. R-W was "Technical Director" of the program, and I envisioned them just pushing paper with no real power.

About this time, another one of those life-affecting coincidences occurred. One of the Professors in the Department, Myron Nichols, had taken a year's leave and had gone to the Ramo-Wooldridge Corporation. He came back for a short visit and the head of the Department gave a cocktail party for him. While I was there, I told him that I was considering taking the General Dynamics offer, as I knew more about what they were doing. Myron looked at me earnestly, and said, "Don't go to General Dynamics. Come to Ramo-Wooldridge. I know you, and you will do fine there." So I went home and talked to my wife, Ethel, and we agreed that we would go to Los Angeles and that I would work for Ramo-Wooldridge. Later, I learned that General Dynamics had not been one of the successful bidders on the work that I was being hired for, so I was glad I had chosen Ramo Wooldridge.

California

I accepted R-W's offer in the spring of 1955 and we made preparations to go to Los Angeles. Because I had agreed to teach part of one of the short summer courses, I told R-W that I couldn't report until September. In the meantime, we had to sell our house and arrange for a place in California. On my visit to California, I had found a rental house in the Riviera section of Torrance, which would be available in September so that part was taken care of. We sold our house for just a little more than we had paid for it—actually, to my barber.

I reported to R-W on September 6th ,1955 . We were housed in an old parochial school building over in the middle of Inglewood. Some offices were in the old chapel and the whole place was referred to, facetiously, as St. Ramo's. A few months later we moved into some new buildings on Arbor Vitae Ave. My secret clearance was already there and I could go to work at once. I was given a badge number of 823. This number was that high because the numbering system also included people in some other parts of R-W, notably the "commercial" parts, as compared to the military contract parts. Ramo used to talk about how much money could be made in the commercial parts as compared to the government contracts, which usually had limits on how much profit they would pay. As a matter of fact, most of the commercial parts died off, while the government part prospered over the years.

I soon found out that my worry about R-W being a paper-pushing organization, and nothing else, was unfounded. The ballistic missile program was a vast program that touched just about every aerospace company: It was our job to pull this all together. Much study had gone into picking the very best technology and there were backup contractors for every major subsystem. In fact, there were two complete missiles being planned: Atlas which was a stage-and-a-half configuration and Titan which was a two stage missile. Considerable work had been done at R-W in conjunction with various contractors and a preliminary concept of the missiles had emerged. Given the state-of-the art and reasonably anticipated improvements therein, the pieces of the missile were doled out. This started with the weight and yield of a hydrogen bomb to be constructed by Los Alamos National Laboratory. An allowance was then given for the weight of the reentry vehicle (R/V), or nose cone, as we called it then. Then the guidance system was given an accuracy goal, a weight limitation, a power limitation, and a volume limitation. The same was done with the structures and propulsion systems. Of course, these were iterated as the design developed, but, finally, firm specs were given for every component of the missile. It was up to R-W to tie these diverse components together so that they would all function properly. We could arbitrarily direct changes to a subsystem if we thought it was necessary.

At that time the big unknown was the reentry system. It was agreed that a propulsion system could be developed, having roughly the performance demanded. If it didn't quite make this performance, then the range of the missile might be a little less than 5500 nautical miles, but this would still be acceptable, however undesirable. If the guidance system wasn't as accurate as hoped (one nautical mile in those days), a missile with a hydrogen bomb aboard was still a threat to a city even at two nautical miles, and so on for the other system components. But if the reentry system, which nobody had ever tested under the actual environment, burned up, then there was no ICBM at all.

My job was to work on the reentry vehicle. The heat shield that had been chosen before I got to California was a heavy solid copper cone with a rounded nose. (Sphere, cone.) It was thought that a material like fiberglass, or carbon, would be lighter but the understanding of the aerodynamic heat transfer to such a surface was not well understood, and, of course, there were no test facilities that came anywhere near Mach 22 speeds. We were under pressure to get a missile that would work, not necessarily the best, lest the Russians beat us to it and blackmail us with threats. Such was the mentality of the cold war—on both sides, from what one can read now. Materials like special forms of fiberglass had very complicated interactions with the aerodynamic flow. Some parts of them melted and ran, other parts, especially the epoxy fillers, evaporated. In tests, putting them in the exhaust of a rocket engine, sometimes they just came apart for reasons not well understood. This evaporation affected the flow by a "blowing" effect at the surface, which generally decreased the heat transfer to the surface, but also caused turbulence that <u>increased</u> the heat transfer. Copper could absorb about 200 BTU/lb. before it reached its melting point at around 2000^{o} F. Some of the ablating materials (materials which were removed by erosion or evaporation) would absorb perhaps as much as 5000 BTU/lb., effectively, including the "blowing" action, so it was evident that the potential weight savings of these materials was significant (if they didn't come apart.) Bob Bromberg, who ultimately became my boss, remarked that ice would be almost an ideal heat shield material, since almost all of it would evaporate rather than running off at the high heat rates involved. Nylon was actually a pretty good substitute for ice, especially if it were mixed with a binder such as quartz fibers.

Our two reentry contractors were G.E. and Avco, each team acting as a backup to the other. G.E. had started earlier and was further along than Avco. G.E. turned out to be the best engineering organization, but Avco was the most scientific and had the more basic understanding of such things as heat transfer. (I once invented a scheme for improving the accuracy of the airburst fuze by correcting for variations in the missile trajectory: G.E. adopted this scheme; Avco proved that it didn't quite meet the specifications. It was pretty good though.) We had many technical direction meetings with both of them, usually one a month, alternating between the East and West Coasts. Since I also had to visit other facilities, I spent a lot of time on airplanes in those days.

The whole R-W Aerophysics Department, as it was called, reported to Milton Clauser. There were only about 15 to 20 people in it at its peak. George Solomon was nominally in charge, under Milt, in the somewhat loose organization that we had. Bob Bromberg seemed to be the Chief Scientist in many ways. Bob was so persistent in his questioning of the contractors, usually asking about something they hadn't thought of, that we found out later they used to designate a devil's advocate to be Bob Bromberg, and he would question everything when they were preparing for a presentation. That usually didn't work as Bob was pretty sharp and they never quite anticipated his questions. A little later, we adopted more of an organizational structure, with George Solomon formally in charge, Bromberg was George's assistant and in charge of one section, devoted mainly to aerodynamics and heat transfer and I was in charge of testing of all sorts, but we never had rigid walls around any part of the group.

I was put in charge of monitoring the test program of the R/V a few months after I got to R-W. This included overseeing a number of tests being carried out around the country as we desperately sought out any data that would help the program. The first time I went to a Technical Direction back East, we went to G.E. in Schenectady. (They later moved to Philadelphia.) When we were met at the door someone said, "Is Dr. Sellars here?" I identified myself and they told me that my meeting was down the hall. I anticipated that three or four of us would be sitting around a table discussing some flight test plans. When I walked in this room, I saw at least fifty people. The room was like a large university classroom, with raised seats going to the back of the room. I found out that <u>everybody</u> was interested in flight tests and the reentry vehicle. There were military officers, not just the ones we worked side-by-side with in the Ballistic Missile Division, but officers from the Pentagon and from Kirtland AFB in Albuquerque. There were people from Los Alamos and from Sandia Labs, and, of course, a flock of G.E. people. In front of the array of seats was a lone chair—it was for me. "We want to give you a briefing," a man said. His name turned out to be Bob Havilland and he started flipping big 20"x30" charts that we used before we went to viewgraphs. This was pretty intimidating for a young excollege professor. Now it was known that, during reentry, there would be a time when the telemetering couldn't be heard because of the ionized air around the blunt vehicle. So Bob considered the case where the vehicle burned up during the telemetering blackout and the case where it survived for long enough to play back the tape recording made during the blackout, and variations on all these cases and options in each case. At the end, he turned to me and said, "What do you think?" The whole room leaned foreword to see what this hotshot from R-W was going to opine. I thought a minute and said, "Give me a copy of your charts. I'd like to study it a bit." This was my introduction to technical direction. As it turned out, we never did decide in advance what we would do under certain kinds of situations. In effect, we decided to see what happened first and then make a decision as to the next step.

There was quite a lot of testing going on. As there was no way of getting the high pressures and fantastically high temperatures on the ground that are encountered in an actual reentry, we resorted to using small models and getting as high a velocity as we could. The Aerophysics Development Corporation in Santa Barbara used a cluster of small rockets to boost a 6" diameter model to about Mach six. These were fired at the White Sands Proving Ground. What we were particularly interested in was the point of transition from laminar flow to turbulent flow. When this happened the heat transfer doubled or tripled. So if transition could be delayed as long as possible to a higher Reynolds number, the heat shield could be made lighter, or at least have a greater factor of safety. It was known from subsonic testing, that surface roughness caused earlier transition. Since the boundary layers on the small models were thinner than on the full-size nose cones, we had to polish the models to a very high gloss. Thermocouples under the skin told us what the heat transfer was, and thus when transition took place. This program was only partially successful, due to a number of rocket failures, but it gave us a few clues and we were desperate for any data at all.

The big program that I monitored was the X17 missile at Lockheed. This was a three-stage missile. One stage lofted the upper stages on a high trajectory that took them up above 100,000 ft. Then as the upper stages dropped back into the atmosphere, the upper stages were fired, one after another, to drive the model back down into the dense atmosphere, reaching about Mach 15 at 40,000 ft. altitude. The test model was 6 inches in diameter and was well instrumented with thermocouples. The heat transfer could be calculated based on the thermocouple readings and, in general, it showed that transition was delayed until a very high Reynolds number was reached, and then the turbulence spread quickly foreword. If the full-scale reentry vehicle behaved the same way, it would give us a fair factor of safety as most of the heating would be over before transition took place.. G.E., in their design, assumed something slightly more conservative than this. Avco, on the other hand, succeeded in designing for complete turbulence all the way. The Avco Shape 52 was almost flat, with a very slight curve across the front, like the Apollo capsule, with a very low W/C_DA. When it was later decided to go to an ablating material design for the Titan missile, the Avco copper heat shield design was cancelled. Abe Kahane, the chief aerodynamicist at Avco, claimed that Shape 52 lived on in the Apollo design. It was obvious that NASA had made good use of the ballistic missile data in coming up with their design. The use of ablating materials for the new Titan R/V meant that it could be considerably lighter than the copper one. It would also be more accurate since it had a higher ballistic coefficient (that is, it was more streamlined) and thus less susceptible to wind drift because of the shorter flight time at low altitudes.

As a side note: When the Request For Proposals for the Titan R/V was issued, it specified that a certain number of balloon decoys were to be launched with it, each to weigh no more than 2 lbs. There was a typo in the RFP and it specified that the decoys were to weigh no more than <u>0.2 lbs.</u> None of the contractors had any trouble meeting this spec, which shows how easy it is to add simple penetration aids to a missile.

To return to the X-17 program: Lockheed had hired a bunch of high-powered theorists to reduce all of this data and interpret it for use on the ICBM program. The aerospace industry was awash in research money at that time, so, seizing the moment, a group of these people, (including Joe Charyk, who later was an Assistant Secretary of Defense for Research, and who went on to head Comsat Corporation,) left to form their own company, Aeroneutronics, down in Newport Beach. We were upset, thinking that we would then have trouble getting our data reduced and interpreted. We needn't have worried: two young engineers, Dick Denison and Dan Tellep, were doing all the real work there. Dick some years later came to work at TRW and Tellep went on to become Chairman of the Board of Lockheed. They turned out "quick look" reports one week after each flight that were so good that we hardly looked at the final report on each flight which came out a month later. We had about 15 or 20 successful flights and they were going off about every two weeks. They, and Lockheed, did a remarkable job.

Another interesting set of tests was our drop tests. As it happens, it is easy to calculate the stability of the R/V at high supersonic speeds, but there were questions as to whether these blunt bodies would tumble near Mach 1 or at subsonic speeds. Because they were so blunt, they ended up reaching Mach 1 near 40,000 ft. altitude and they were only going a few hundred feet per second when they hit the ground. The fuze to detonate the bomb was a barometric fuze with an option of a contact fuze when it hit the ground. To simulate this part of the trajectory, the R/V was attached to a solid rocket and dropped from a B-52, the only airplane big enough to carry the R/V to a high enough altitude. The rocket then boosted the R/V to about Mach 1.4 and the R/V was separated and allowed to fall by itself. Thus we determined that it was aerodynamically stable and we also got to check the pressures at the base of the R/V so that we would know how to design the barometric fuze. These drops were done for us by the Special Weapons Test Center at Kirkland Air Force Base in Albuquerque. Once, when I was over there, I had a chance to crawl through the B-52. It was mighty impressive.

As a matter of fact, the reentry vehicle survived just fine when we fired it from Cape Canaveral to Ascension Island, out in the middle of the Atlantic. Only one failed in the sense that some temperatures went above the melting point of copper. There was a meteor storm at that time and we suspect that the vehicle was pitted during midcourse, apparently badly enough to cause early transition to turbulence. These meteors were the size of dust particles, but hitting at a relative velocity of 70,000 ft/sec, or so, they would make a small crater on the surface much bigger than the particle themselves.

In the summer of 1956, Simon Ramo convened a meeting a group of prominent scientists to assess how the program was proceeding and to ask them if they could see any fundamental flaws in what we were doing—that is, could any of these men, one of them a Nobel Prize winner, discern that we were violating some fundamental law that we had overlooked. Ramo was particularly concerned with the reentry system, since nothing like that had ever been accomplished, whereas there had been rockets fired, and guidance systems built, containing, at least, most of the elements of the guidance system we would need.

As it happened, we spent most of our time educating the members of the committee, rather than the other way around. These were all very smart people, but they were not familiar with the particular problems we faced. This suited Ramo's purpose in any case. We were proceeding to spend millions of dollars on these missiles, and we would spend billions before we would ever find out if it worked. Ramo was anticipating the furor that would ensue in case it **didn't** work, say it turned out to be virtually impossible—there would be congressional hearings, fingers would be pointed, and there could even be serious damage to R-Ws reputation for being associated with—and even abetting—such a fiasco. Ramo wanted to stand up and say, "We had the brightest minds in the country review this program, not once, but repeatedly. And they thought we were proceeding correctly. If we missed something, it wasn't because we didn't consult the right people." Hence the committee.

I don't remember the names of all the committee members, but some of them stand out in my mind. There was Robert Bacher, who was the head of the Physics Department at Caltech; Lester Lees, also from Caltech and a brilliant aerodynamicist who had been a consultant on the program for some time; another was Edward McMillan, who had shared the Nobel Prize for the discovery of Neptunium, the first of the trans-Uranium elements; and there was George Kistiakovsky, a world-renowned chemist, then at Harvard. Kistiakovsky had worked on the atom bomb and was responsible for the design of the shaped explosives that set off the bomb.

I gave one of the briefings to this group and I sat in on some of the others. Since the group ranged from Lester Lees, who knew far more aerodynamics than I did, to people completely ignorant of the subject, I thought I would try and explain why we were trying to design an R/V with a low ballistic coefficient (W/C_DA). (Basically, a high drag compared to its weight.) Everybody in R-W seemed to accept this as gospel when I arrived, so I had accepted it too. When I was asked to give this lecture, I thought that this would be a good point to clarify for the committee, and for myself, so I dug into it and came up with a simple way to show it mathematically. After I finished, George Kistiakovsky, who had been associated with the program since its inception, came up to me and said, "You know, I have always heard that we must have a low W/C_DA, and I never really understood it until now." I took that as a great compliment. After another briefing, Si Ramo also complemented me saying something to the effect that I had a good eye for picking out the most important points and arranging them logically. Since Si was a master briefer himself, I appreciated this compliment also.

During one of the meeting sessions, one of the members said, "Is this the committee that's supposed to take the fall if this thing doesn't work?" Kistiakovsky answered, "No, that's the Scientific Advisory Committee, another committee I'm on."

The Space Age Begins

Late in the summer of 1957, I was temporarily made R-Ws Program Manager for G.E., that is to say, I was responsible for contacts with G.E. and for relaying any technical directives we needed to send them. It was understood that I would give up this job as soon as they hired a suitable replacement, someone who was more the program manager type, as compared to the more technical type.

On October 4th the Russians surprised the world by launching Sputnik. The U.S. had had a program called Vanguard, designed to launch a satellite. This program, by design, was kept completely separate from the military programs to keep it "pure." It was underfunded and mismanaged and had several launch failures, but nobody in the administration was particularly concerned about this: after all, the Russians could never accomplish such a feat, and even if they did, so what! The world reaction when they succeeded, I have read recently, surprised even the Russians. When they put Sputnik up, they didn't make a big deal of it—just a short article in Pravda. When they saw the world's reaction, they made a really big deal of it—front page—medals awarded—the works. The United States administration finally realized that the Russians had scored a propaganda coup. The Army was authorized to cobble up an upper stage for a Redstone rocket (a short range missile) and launch a small 20 lb. satellite, which, however, actually had more scientific instrumentation in it than the Russian 200 lb. satellite. In fact, it discovered the Van Allen belt of radiation. But, to the world, we were definitely second and with one-tenth the payload. Werner von Braun's group in Huntsville, Alabama, was responsible for the successful launch of our satellite.

Project Score

To counteract the Russian coup, someone then thought of a stunt, designed to impress the world and recover some of our prestige. If one removed the reentry vehicle from an Atlas missile and replaced it with a light, streamlined, fairing, it could just make it into orbit. So one day, Dr. Paul Dergerabedian and I were called into Dr. Ed Doll's office for a very hush-hush project. Doll was the director of the Atlas program at that time. The plan was, interestingly enough, to put an Atlas into orbit. The only payload was to be a tape recording of President Eisenhower broadcasting a Christmas greeting to the World. Paul had done most of the performance analysis of all of the missiles up to then, and he could run computer studies to see if it was feasible to put an Atlas in orbit without modifying it substantially, and what the best trajectory should be. We could also advertise that we had a <u>multi-ton</u> satellite in orbit, never mind that it was just a burned out Atlas hulk.

Now comes the tricky part: This burned out stage would probably be tumbling slowly in orbit. Thus it would present a large area to create drag, even in the tenuous atmosphere a hundred plus miles up. At this time there was about a factor of three uncertainty in the density of the atmosphere at these altitudes. If we launched it, and it fell out of orbit in a few hours, it would look ridiculous. It should stay up at least a week to get the full value as a "great accomplishment." So Paul took on the task of getting the missile into various orbits and I took on the task of getting someone to run a series of life-time calculations for these same orbits. Since it cost energy to lift the fuel up to the orbit, I suggested that he look at a series of low perigees (the lowest point of the orbit) and use the remaining fuel to overspeed the missile, that is, go over the speed that would give a circular orbit at that altitude. This would give a high apogee (about an extra mile for each ft/sec of overspeeding.) Even though there would be some drag at the low perigee, the effect of this is mainly to bring the apogee down little by little. I speculated that this might give longer lifetimes than if we went for the highest circular orbit. Paul agreed and he produced a series of perigees with the maximum apogee possible for each. I, in turn, got Fritz Hartwig to run the necessary orbital aerodynamic calculations. I couldn't tell Fritz what we were really doing, so I told him another manager had asked us to do this, even though it was going to cost a lot of money for computing. Paul and I then picked what we estimated was the combination that gave the longest life and that's what was flown. .

Now down at the Cape, they published a document called the Detailed Test Objectives, or DTO. This specified the trajectory but it faked the reentry part of it, so as not to give away too much about the real mission. The DTO was used by the instrumentation people and the range safety people, among others. The DTO showed that the reentry vehicle would come in at such a shallow angle that it would probably burn up. As soon as G.E. saw the DTO, I got a phone call from an engineer there named Bob, (whose last name I no longer remember). "We've just seen the DTO for the next flight. There's something wrong. The R/V will burn up." I told him, "Bob, don't worry about it. We won't burn up a nose cone." Bob had a kind of whinny, drawly voice. "Jaahn," he said, "I can't go back to my boss and just tell him 'John Sellars said not to worry about it.'" "Don't worry about it." I repeated. There was a long pause and he said, "Is there going to be a nose cone on that missile?" I told him again not to worry and he said "O.K." I guess he got the idea.

The flight went off as scheduled, and it stayed in orbit for a whole month, endlessly repeating Eisenhower's Christmas greeting until the batteries ran down. Let nobody call this just a cheap stunt: it must have cost quite a few million dollars.

The Man In Space Program

In the spring of 1958, the Air Force decided that they had better get on the ball or they would miss out on Space. They had programs underway for developing reconnaissance satellites but nothing involving pilots. Words were thrown around about space being the "strategic high ground" which one must occupy before the enemy took possession of it. Nobody had quite figured out what the man was supposed to do there that couldn't be done better and cheaper by unmanned spacecraft, but the Air Force was convinced that the proper mission would develop as we gained experience in space. Accordingly, the Air Force decided to do a study, with R-W coordinating it, which would involve a number of parts of the Air Force, including Wright Field, the Systems Command, medical human factors people, and others. I was to coordinate the reentry capsule.

We had in our possession a thick document that Von Braun's people had put together. It outlined just about every mission that they could think of in space. It included mail rockets to be fired from, say, New York to Chicago. It included manned missions to the moon and to Mars and Venus. In short, it was just a compendium of everybody's wild ideas. The Air Force was hard put to come up with missions that applied uniquely to themselves. Finally Dr. Rube Mettler, who later became President of the Company, saved the day. He told the Air Force, "Since a clear mission will not arise until we have some experience in space, I suggest that you undertake a mission, sufficiently difficult, that when you have finished developing the hardware, you will have the means to accomplish whatever mission you choose. I suggest that putting a man on the moon and returning him would be such a mission." This was like a revelation: the whole Air Force jumped on the band wagon and we were off.

We laid out a capsule just big enough for one man, unlike the three-man Apollo capsule that was finally built. Our booster had only (!) about one million pounds of thrust and the capsule did not orbit the moon but landed directly. I laid out a test program consisting of orbiting the earth for two weeks or so (a time comparable to going to the moon and returning), then very high up-and-back orbits to test reentry at speeds comparable to returning from the moon (about 35,000 ft/sec) and, finally, passing around the moon. Then we would be ready to be landed on the moon.

In the Aerodynamics Department, we had done some calculations that showed that you had to hit the edge of the atmosphere rather precisely as you returned. At an altitude of about 400,000 feet, where the reentry vehicle begins to feel the atmosphere, if it hits at too shallow an angle, it simply pierces the atmosphere and goes on through and out the other side, to return, in several days, God only knows where. On the other hand, if you passed through 400,000 ft at too steep an angle, the capsule later experienced enough G forces to seriously injure the man. Below 4 degrees was too low while 5 degrees already gave 12 Gs at the peak, and the medics advised us not to exceed that, especially as the man had been in zero Gs for several days on the way home. This narrow angular range translated to about a nine nautical mile corridor that the guidance was required to hit.

Dr. Jim Fletcher was in charge of the guidance study. (Jim later left to form a company called Space General and he later twice headed NASA.) While I was sitting in my office one day, Jim came to see me from down the hall. "The guidance can't make the accuracy required that you people have specified," he told me. I said, "Well, this is all some ways out in the future. Surely you will have made improvements by then." Jim said, "We have postulated every improvement that we can reasonably conceive of and some that we can barely imagine and we still can't get there. Is there anything you can do?"

Now, as it happened, one of the young engineers in the Aerodynamics Department, named Dick Phillips, had run a number of calculations of reentry from near earth orbits. We tried different schemes to lower the maximum G forces. We tried varying the drag by changing the shape during the reentry, but the most effective way seemed to be to use a little lift. The idea was to come in a little steeply, but then, instead of diving on down into the dense air while still at high velocity, one pulled up gently and stayed in the thinner air while losing speed. A small amount of lift was surprisingly effective. A lift to drag ratio (L/D) of only 0.5 made quite a difference.

Remembering all of this, I said to Jim, "Maybe we could use lift." He said, "Would that work?" I said, "I don't know, but we could run some calculations." Shortly after, I got Dr. Budd Cohen, who was my Associate Manager in the Aerodynamics Department, and Dick Phillips on the phone and explained the problem to them. They started doing the calculations and a few days later we had the results. We could now go between 4^{o} and 10^{o} at 400,000 ft. without exceeding the G limit. This required an L/D of only 0.5, easily obtained on a blunt body by just tilting it a bit. This opened up the corridor from 9 miles to almost 90 miles and made Jim Fletcher happy.

We briefed this study all over the country. Sitting in the back of the room on a couple of the briefings were three people who would later become important wheels in NASA when it was formed from the old NACA: Hugh Dryden, the head of NACA, Bob Gilruth, and Max Faget. Dryden retired shortly after NASA was formed but Gilruth and Faget went on to hold high positions in NASA. I found out a few years ago, after reading an article in the New Yorker, that I had known Max when we were children. We both lived at Fort Stanton, New Mexico at the same time when we were very young but I had never connected him with that child.

I remember briefing the program to a group of officers who were passing through on an orientation tour in the fall of 1958. I was supposed to expose them to some of our advanced ideas. When I finished, one of the Colonels spoke up, "Are you saying that, if you got the go-ahead, within ten years you could put a man on the moon and return him?" "That's right," I said. He slapped his thigh and let out a guffaw, "Boy, they sure think big around here!" Most of the officers, I am sure, thought we were out of our minds. Actually, President Kennedy started the program in 1961 and by 1969 we had men on the moon, so it took less than ten years. The NASA program was much more elaborate than our program. But considering that we generated the whole thing in about two months, I think we did pretty well. The flight test part of Apollo, in particular, was pretty much as I had laid it out—near earth orbits, circling the moon, etc. As for the Air Force, they never did figure out a military reason to have men in space.

The Thor-Jupiter Controversy

It was decided that the country needed an Intermediate Range Ballistic Missile, an IRBM, which would have a range of about 1500 nautical miles, to be placed in friendly countries like Turkey and England, from where they could threaten Russia from another direction. Accordingly, Douglas Aircraft won a contract to build the Thor. It was to use the G.E. R/V with some of the copper left off, since the reentry conditions were much milder. Now the Navy had been planning a ship-launched ballistic missile of the same range called the Jupiter, which was made by the Army's Redstone Arsenal, that is, Von Braun's crew. When the idea of using a nuclear submarine loaded with solid propellant rockets came up, the Navy "walked out on them," as I heard Von Braun express it at a meeting later. Now the country had two IRBMs and they clearly needed only one. Neither side was willing to withdraw so a competition ensued since the administration wasn't willing to make a firm decision.

The Department of Defense's Scientific Advisory Committee had both sides come back to the Pentagon and brief them, hoping that the resulting give-and-take would help them clarify what should be done. This committee included some very distinguished members, notably, Clark Millikan from Cal Tech, and Charles Lindbergh, and others of that prestige and stature. They hoped to have a real cat fight, out of which some facts would emerge that would favor one missile or the other. I was to present the reentry part for the Air Force team. We made our presentation without a single reference to Jupiter, which was a strategy set by Simon Ramo. Von Braun, who made the whole presentation for Jupiter, continuously compared the missiles, subsystem by subsystem.

We had estimated that the accuracy of our R/V alone was about 1500 ft, plenty good enough, considering the guidance errors. Bob Bromberg had talked about this at a meeting a month or two before, and Von Braun and another German from Huntsville had questioned this, claiming that they thought it was too low. (Their R/V had a higher ballistic coefficient that gave a greater accuracy due to less wind drift and less sensitivity to variations in the atmosphere.) When Bob came back, the questions by Von Braun worried him, so he decided that they should try and evaluate this more carefully. His conclusion was that it should be more like 4300 ft. When I gave that number in my presentation, I saw Von Braun's head snap up. He was ready to jump on it during his presentation. In fact, he did make a remark that our accuracy was about 4300 ft, "Which I see they now agree with."

When Von Braun finished, Si Ramo got up and did a masterful rebuttal of some of Von Braun's points, without notes, as though he had just thought of them. I knew, however, that he had spent many hours studying both systems. Von Braun started to answer, but General Madaris, his boss, cut him off as he was clearly up against a master debater. "We'll let the flight tests speak for themselves," he huffed. Actually he had a good point there: they had had a couple of successful flights, while the Thor had two failures as well as having one missile blown up by range safety. In the latter case, it turned out that the plotting board showing the missile's trajectory was reversed and the range safety officer thought it was going inland instead of out to sea. As one of the Air Force Officers with us muttered to me in the back of the room, "Our problem is to prove that a missile that hasn't worked is better than one that has." Finally, this problem was referred upstairs to Vice President Nixon, who was asked by President Eisenhower to evaluate what we should do. He finally recommended that each missile should continue, a typical political decision.

There is another interesting sidelight to this controversy in which I had a small part. In order to test their reentry material under pretty realistic conditions, the Army put an upper stage on the Redstone with a half-scale R/V on it. Since this would reenter at about the right speed, this would make a very good test. They used the guidance system of the Redstone to steer it and put the upper stage on a spinning platform so that it would continue on without veering off or tumbling when it was separated from the Redstone.

Someone in the Air Force happened to show me a movie of the launch of this hybrid, which they called Jupiter-C for Jupiter-Composite. (The Air Force thought that they called it Jupiter-C so that people would think they had an actual Jupiter already in flight test. They denied this, of course.) In any case, sometime later I remembered how this had looked just as it took off. The film was in slow motion, yet the upper stage seemed to be spinning at a great rate even in slow motion. I remembered that Dr. Herman Leon, one of our shrewder analysts had done some work showing the effect of spin on the reentry vehicle as it reenters. If it enters at an angle of attack, rather than straightening out with a few oscillations, it precesses like a top and resists lining up with the flow. Since the half-scale R/V entered almost broadside to the flow due to the way they launched it, it had, in effect, a low ballistic coefficient, i.e., a high drag, and, since it was spinning so fast, it never straightened out, which translated to a much lower heat input. Thus their test was not as definitive as they thought. Herman and I spent a half day using an analogue computer to verify this. It turned out that Von Braun had heard Herman talk about this effect at a classified technical meeting a few weeks before. As a consequence, they had <u>halved</u> the rotation rate just in case Leon's analysis was right. However it was still spinning at about 1 rev. per sec., still much too fast. I made an estimate that it had gotten about one half the heat input that they expected. This was reported to the local Air Force who passed it on to General Schriever, who together with General Madaris, was meeting with Nixon. Madaris couldn't refute it on the spot, so he just said that they would have full-scale Jupiter flights soon and that would settle the question.

Some months later, Col. Bill Ebelke and I went down to Huntsville to hear their presentation of the data of this flight. Von Braun said that, due to the spinning, the vehicle had gotten about one half the total heat input that they had planned on. I gave Ebelke a big nudge in the ribs when he said that.

The Thor-Jupiter controversy dragged on until Von Braun's people were handed the booster role in the Apollo program. Then the Jupiter was quietly dropped.

Back in the Technical Direction Business

The government became worried that TRW, a private concern, was doing all the planning for their advanced missiles, in particular, the reentry vehicles. To counter this, the Aerospace Corporation was formed and they took over the technical direction of future nose cones. They supervised the design of the Mark 12 R/V that went on the Minuteman Missile. This was meant to be used as a multiple R/V on a single missile. They also did the preliminary design of a larger single R/V also to go on Minuteman. G.E. was the contractor for the Mark 12 and Avco was the contractor for the heavier R/V. At some point the Air Force decided to have Aerospace concentrate on Space Missions and they asked TRW to take back the technical direction of the reentry vehicles. The Mark 12 was essentially developed, but Avco was just getting started on their contract. Some people were transferred from Aerospace to TRW and they gave us a briefing on the program to prepare us for a visit to Avco, where we got essentially the same briefing.

I only made a small contribution to the new technical direction activities: When the ex Aerospace people gave us their briefing, I thought I had spotted an error, but I dismissed it, thinking I had misunderstood what the briefer had said. When Avco repeated the same error when they were briefing us, this time I was listening carefully. When the Minuteman gets out of the atmosphere, the streamlined fairing over the nose is jettisoned. Avco did this by taking pains to fire the fairing straight ahead with its own little rocket. It went far enough, according to Avco, that by the time the still-accelerating missile caught up with it, it had had time to fall out of the way due to gravity. Unfortunately, they had overlooked the fact that the missile was also being pulled sideways by gravity (that's what gradually curves the missile path over) and that the missile and the nose fairing would collide in a couple of seconds. They were misled by the fact that the guidance people referred to this part of the trajectory as "the straight line part." It **is** almost a straight line if you plot it. We had a long argument, and finally we retired to another room to continue it. Afterwards, Avco had a meeting of their own and then they came back into the meeting and said that they agreed with me. Actually, I was a little embarrassed to point this out; I felt bad for the young man who had done the calculations, not so much for all the bosses who had approved them.

When I was first at the University of Michigan, I had a friend named Frank Faulkner who was working on some space interception problems, and he showed me a way of looking at these problems that simplified them greatly. That's why I was able to spot this problem, not because I was especially brilliant. A few months later, the Air Force cancelled the Avco reentry vehicle, so whether or not this problem was identified was immaterial.

Project Able Zero

At some point, R-W, by then known as Space Technology Laboratories (STL), persuaded the Air Force to let them mount an upper stage on the Thor. This used an Aerojet engine and tankage. This stage was called Able. STL used it to make an abortive attempt to take some photographs of the moon. We missed the moon by a lot, but the spin was that we had set a new altitude record! It was decided to use this vehicle to send a scale model R/V down range to Ascension Island and thus simulate a true ICBM reentry. This particular flight was called Able Zero, for reasons that escape me now. It would use an ablating material as a heat shield, and thus move our technology along, as well as stop some of the carping we were getting for sticking with the copper heat shield.. We wanted a very conservative design, so I sketched up a shape and took it to G.E. The heat shield would be a type of a fiberglass called refrasil, which stood for refined silicon. The glass fibers were almost pure quartz that had a very high melting point and was very viscous when it melted so it stuck around on the surface longer. G.E. fabricated this shape and made several of them. Laurel Van der Wal, one of our engineers, had a little life support system built and we put a mouse in the R/V. Since nobody had been in space at that point, we wanted to see how the mouse would fare. All we got was his heart rate as we were never able to recover the R/V, although it had a recovery system built into it. The heat shield worked fine, but the missile was somewhat off target, just as in the moon case.

A few days later, General Schriever was testifying before Congress. Someone asked him if there was an animal in that R/V. Since Generals aren't allowed to say, "I don't know," he took a guess and said no. By custom, the testimony is then sent to the witness who can correct it if necessary. He, having learned the truth in the meantime, changed his no to yes. This occasioned a little laughter at his expense, another thing Generals don't allow if they can prevent it. He then ordered that no more animals be flown, and that was the end of our little side experiment, much to Laurel's dismay.

Ordnance Adventures

In the 1960s, TRW in Cleveland tried to get in the gun business. They bid on manufacturing a number of M14 rifles. This was to be the last buy of M14s as the Army was developing a new rifle called The Special Purpose Infantry Weapon (Spew). This weapon fired tiny flechettes (little arrows), and it could fire three in quick succession, one at a time, or go full automatic. Now TRW had deliberately underbid the M14 contract, so that they lost money on almost every gun. But not to worry. At the end of the contract they were making money on each gun, even though losing money on the whole contract, and they expected to get rich on the follow-on production. Since there was to be no follow-on production, how did this make sense? The answer is simple: the Spew was known by our people to be in big trouble and couldn't possibly go into production. So the Army would <u>have</u> to buy more M14s. But a little knowledge is a dangerous thing. Even as we were producing the M14s, a man named Gene Stoner was persuading the Air Force to adopt a new gun he had developed, called the M16. The Air Force in turn persuaded the Army to test it and finally adopt it. Gene Stoner got rich and TRW had to eat the overrun on the M14 contract with no follow-on. Gene got something over $1 per gun for the first several million guns.

Dave Dardick was the inventor of the, so called, open chamber gun system, which potentially could fire at very high rates. He had been under contract to TRW in Cleveland, but they decided they had too much on their plate and they asked if we would like to take over this invention, and Dardick as well, and see if we could sell it, at least as a development contract to the government. This we did and I was exposed to the gun world, which was vastly different from the missile/space world. The gun world was a closed society that didn't welcome outsiders, especially smart alecks from aerospace. There was much lore that only they knew, and much jealousy. The Nobel Prize in this business was to be associated with a numbered gun—Gerrand with the M14, Stoner with the M16, and they weren't going to let someone like Dardick get his name on one before they got their name on something.

I remember briefing a Brigadier General down at Eglan Air Force Base in Florida. He was a short little man who tried to make up for it by acting tough and decisive. We referred to him as Boom-Boom. I told him all about uranium flechettes and showed him what they did to a piece of heavy steel. He sat in a high-backed executive chair. At certain points of the briefing he would hold up his hand, stopping us. Then he would rotate his chair until all we could see was the back of it. Not even the top of his head showed. After a suitable time of dead silence, he would rotate back into view and indicate that we should continue. His Colonels in attendance seemed to be used to this as they tolerated it without showing any surprise, or maybe they didn't dare to. He was only one of the colorful characters we met. We built a prototype of Dardick's gun that fired at the rate of 30,000 rounds/minute, probably a world record, but we weren't able to sell a development contract.

Some months later a man named Sam Wong, who had previously worked for TRW and now worked for the Defense Advanced Research Agency, showed up at TRW and asked us to look at a "low maintenance rifle," which should also be cheap. "Cheap" meant that it shouldn't cost over $130. I asked why they were so worried about the initial cost of the rifle. A rifle was expected to fire at least 10,000 rounds over its lifetime, which would cost over $1,000. Also the cost of the soldier was thousands of dollars each year. Why then, stint on the gun, which, for a rifleman, was his sole reason for being there. Sam looked completely baffled: I guess he had never thought about it before. Finally he said that, since so many were bought, that Congress was concerned about the total cost.

We did develop the rifle for the Small Arms System Agency of the Army, where the contract from DARPA was transferred. The idea was to try and design a rifle that would be less susceptible to rust and dirt under combat conditions. Don Stoehr designed a rifle that had a number of unique features. For one thing, it fired at only about one third the rate of the M16, which fires, in full automatic, at over 600 rounds per minute; it can empty a clip of 30 rounds in three seconds, which is much too fast. Our gun was aligned in such a way that its recoil didn't cause the aim point to climb while firing a burst. I fired both this gun and an M16 down at our Capistrano Test Site. I could hold this gun right on the target as I fired out a whole clip; with the M16 the recoil pushed the barrel up and it was brought back down on the target with difficulty. Even so, the M16 was much better than the M14, which, someone said, only an ape could hold on the target when firing a burst.. Our gun is languishing somewhere in the archives of the Small Arms System Agency. It would have taken many millions of dollars of further development to complete the engineering and the testing needed to make this into a standard issue weapon, but the Stoehr design was a brilliant start. It fired the same round as the M16 (5.67 mm). Later, in fact, I had a call from someone, whom I believe was in a National Guard unit, who had fired the gun and wanted to know how he could get them for his outfit. He couldn't, I told him.

We had another contract from ARPA originated by a man named Dick Caesaro. This was at the height of the Vietnam War and Dick wanted a way to shoot up trucks on the Ho Chi Min Trail. The idea was to have an unmanned helicopter with a 50 caliber machine gun. The gun was to fire single shots of a flechette made of uranium which could penetrate the engine block of a diesel truck and make it impossible to repair in the field. The ammunition had to be very accurate. We had to put most of the flechettes in a six inch circle at 1000 feet. We finally developed a round that would do this, and we produced 20,000 rounds which we delivered. I have no idea where they are now.

These flechettes had a little four-fin aluminum tail, each fin of which was canted one degree to give the flechettes a spin, which would keep them going straight. To our surprise, the first few we fired fell well short of the target, indicating that they had a very high drag coefficient. In thinking about this, I started wondering whether or not they were perhaps spinning <u>too</u> fast. I made some rough calculations, which indicated that they might be spinning above the shaft critical speed and literally bending themselves. This would account for the high drag. We lowered the cant on the fins to half a degree and suddenly they were flying straight and true. I had to make a lot of approximations in this calculation, so the results were doubly gratifying.

The Energy Division

In 1980, there was a reorganization, and I joined the part of the company which was exploring the energy business. I took along a number of people from the Advanced Technology Division, namely, all of the chemistry people, and the people working on isotope separation. I was made Vice President and General Manager of the Energy Technology Division. This position I held until just before I retired August 1, 1986.

The chemistry group, under Bernie Dubrow, came up with a number of interesting ideas. One of the most prolific inventors was Jack Blumenthal, whose work led to some new forms of carbon fibers. Unfortunately, TRW, not being in the materials business, was not in a position to exploit these breakthroughs, and after spending a considerable amount of money on them, they were allowed to die.

The biggest project we had was an isotope separation scheme, called the Plasma Separation System, to be used to separate U_{235} from U_{238}. This was a scheme invented by Professor John Dawson of UCLA, one of our consultants. We succeeded in entering a competition to develop this system against two National Labs: Livermore and Los Alamos. We showed that our scheme was better than Los Alamo's, which was based on using lasers, but the government ruled in favor of Livermore's scheme, also a laser scheme, although we felt that their system had serious problems. After a while their system was abandoned, partly due to technical problems, and partly due to lack of demand for enriched nuclear fuel. So the whole program came to a halt.

I retired at the end of July, 1986, after thirty-one years with TRW. It was a great experience: I was connected with some important and interesting projects and I got to work with an amazing group of people.

John R. Sellars
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