



Lockheed X-17
USAF Museum

My Career

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$$\frac{\phi}{c_{fe}^{1/2}} = \frac{1}{2^{1/2} \kappa} \left[\ln \frac{\kappa}{2D} + \ln \left\{ c_{fe} \tilde{Re}_{WT} \left(\frac{\rho_e}{\rho_w} \right)^{1-\beta/2} (1+\beta)^{1-\alpha(1-\beta/2)} \right\} \right]$$

where

$$\phi = \int_0^1 \left[\frac{\bar{p}}{\rho_e (1+\beta z)^\alpha} \right]^{1/2} dz$$

$$\tilde{Re}_{WT} = \frac{\int_0^x \psi_e^2 \rho_w u_e \rho_0^k dx}{\psi_e^2 \mu_w \rho_0^k}$$

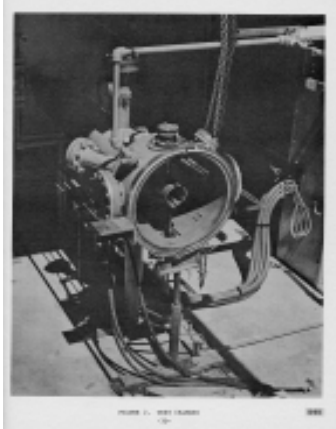


FIGURE 1. TEST ENGINE

Introduction

For many years I have been telling stories about things that happened in my career that I thought were amusing or instructive about how things happened in real life. Now at the age of 77 I want to make sure that the tale of my career is written down. I am doing this partly for my family and friends to have a coherent story about what I was doing at work and partly so that others can benefit from my experience. Some of my best accomplishments occurred by taking advantage of opportunities that showed up rather than elaborate planning. Most of what I have written here came solely from my memory. Although I believe it is accurate, others who went through the same experiences may remember it slightly differently. The story really illustrates the evolution in working conditions for engineers and the change in the value that employers and society have placed on technically educated people. I present the tale pretty much in chronological order.

Philco Corp – June 1951 to June 1952

After getting an MS in Engineering Science and Applied Physics from Harvard University I wanted to find work in Philadelphia my home city. I think Vicki had something to do with that. We were married on November 18, 1951.

I was in luck. During my last days at Harvard I met Mel Arsove who was working on his PhD thesis under Professor Howard Emmons. It turned out that he was also starting a new job as some sort of manager at Philco Corp. in Philadelphia. He hired me. The organization was headed by Carlo Boccerelli, a former Italian portrait painter. Philco built the laboratories with moveable walls to allow flexibility. Carlo soon became a master at moving his rivals into a tiny space by expanding his laboratory.

My colleagues included Irv Weiman and Dave Medved from Penn where I got my BS in Mechanical Engineering and Dr. Robert Goldman from Temple. Sometimes when I was commuting to Penn I used to give Irv a lift to school when my friend Bob Gross, who was riding with me, pointed out that Irv was hitchhiking. Dr. Robert A. Gross later became a Dean at Columbia. Both Irv and Dave turned up later when I worked at Electro Optical Systems in Pasadena, CA. Irv went on to become a venture capitalist in San Francisco. I think Dave's son Michael Medved, the movie critic, is better known than his dad. As I will discuss later I worked a little with Bob Goldman at Philco, but I lost track of him after I left Philco.

The environment in that laboratory would not be tolerated by today's workers. There was no air-conditioning. When we opened the factory type windows to get some air we also got the noise and soot from the railroad siding next to the building. At that time the switch engines that were used on the siding were steam powered coal burners. A white pad on the desk became gray with soot ten minutes after opening the window. Irv also contributed to the environment by running a frequency sweep for some reason at odd times. The whooop sound was kind of hard to take. When that sound was gone we could hear a radar antenna grinding back and forth. On most days those who were working on the electrostatic speaker project would play "Pop Goes the Weasel" at 105 db.

I noticed that each person had several projects to work on. I thought that it would be more productive if several people worked on one project. The only activity that fell in that category was the electrostatic speaker project. A conventional Philco stereo was fitted with the latest version of electrostatic speakers. After the workers with "Pop Goes

the Weasel” were satisfied, Carlo would call the company brass for a demonstration. He would tell them, “We can make these for fifty cents and sell them for fifty bucks”. Inevitably some vice president would turn up the gain and the electrostatic plates would stick together.

I had several projects. One was a Slumbum fan. I think that was his name. Slumbum thought everything could be done with filter papers. He used them for making coffee. The fan was a set of filter papers on a shaft that would ingest air by means of friction. Analysis of a rotating disk is one of the few exact solutions to the Navier Stokes equations. The flow through a porous disk is another story. Mel Arsove thought we could reduce the fan noise in a room air-conditioner by means of a Slumbum fan. Instead of filter papers 4 or 5 perforated aluminum disks were placed on a shaft where normally there would be a propeller type fan. In my experiments the disks tended to fly off the shaft. It is a wonder that they missed me. I found that in order to get the same flow with the Slumbum fan as with a simple conventional fan the rotor would have to turn at several times the rpm. This resulted in quite a bit more noise. At a visit to York air-conditioning, owned by Philco, the engineers were wisely skeptical.

Philco had a Navy contract to develop a sonobuoy to be deployed in the ocean to detect underwater sounds and broadcast the data. The hydrophones were in a lower unit connected by a spring loaded cable to the antenna section which floated at the top. I worked on a couple of aspects of this project. Mel took me out to a quarry that was filled with water. He set up a dummy system with a crank to simulate various sea states. I was out at the end of a diving board taking readings on the dynamics. Mel told me about a problem that the project was having. The sonobuoys were deployed by dropping them from an aircraft. The dummies behaved fine when dropped, but the real models went into a spin. After they hit the water all that could be seen was a dye marker. Mel thought we should get aerodynamic data. We took a trip to the David Taylor Model Basin where wind tunnel tests were conducted. I thought the exterior of the dummies and the live models were identical which made aerodynamics an unlikely cause of the failures. I talked to some Electrical Engineers who assured me that the weight and center of gravity were the same. I asked them if the moments of inertia were the same. I got a blank stare. Later we did some tests and found that the moments of inertia were very different. I do not know if that helped. I was on to other things.

I did do some work on the electrostatic speakers with Bob Goldman. He was working on acoustic and dynamic response of the speakers. I did some analysis of the air loading on the electrostatic plates as they responded to the recorded signal.

After working at Philco for about a year I got worried about where my career was going. Most of the people who worked there were Electrical Engineers or Physicists. It was kind of fun working there but the work did not utilize much of my training.

ITE Circuit Breaker Company - June 1952 to June 1953

I interviewed with Max Berthold, a Swiss engineer, who had a Navy project at ITE to develop a new kind of propulsion system for aircraft. In the interview Max asked me which way constant pressure lines go on a temperature entropy diagram. Of course I knew. I had taken a Turbo Machine Design course at Penn with Professor Adolf Egli, who went to the same school as Max in Switzerland. Both relied heavily on the work of

a Swiss engineer named Aurel Stodola. There were very few other Americans that could satisfy Max so I got the job.

Between jobs Vicki and I took a vacation on Cape Cod. I was not in a good mood. I thought I might have made a mistake choosing my new job. Also we both got the worst sunburn we ever had. Some fun. I came back and started at ITE.

The Navy contract was for development of a Compex, also known as a wave rotor, to compete with a turbo-jet engine. The Compex is a set of shock tubes side by side on a rotor surrounded by a stationary casing. The casing endwalls are penetrated by inlet and outlet ducts that port gases at different pressures and temperatures to and from the rotor flow-annuli. One of the selling points had to do with a lower material temperature than in turbojets because of the cycling of hot and cold gas in the same tube. Gasdynamic shock and expansion waves are initiated as the rotor passages open and close to the ported flows. The rotative speed is set by aerodynamic design trades. The gas compressed by a shock wave feeds a combustor on the outside which loops around the rotor to the expansion side. Two other ports are for inlet and exhaust. The Pulse Jet Engine used in the German V1 Buzz Bomb that attacked London is a distant cousin to this device. The small scale device performed OK, but the scaled up version ran into serious warping problems in the brazed channels.

My job was to analyze the waves in one tube as they reflected from the boundaries during the progression of the tube around the rotor. This was done by a graphical method of characteristics. At the top of the page an assumption was made as to the conditions. After graphically determining all the subsequent waves and their reflections the conditions at the bottom of the page had to agree with the assumed conditions at the top. If not new assumptions were made for starting until agreement was obtained by iteration. Today this could be done in a few seconds on a computer. The graphical method took several days to converge.

There were four of us in one room. The other three were Swiss. They spoke Swiss German all day long. Every once and a while Max would come over to my drafting board and say, "Let me see once. What are you doing? "

The good thing about the job was that it was so bad that it forced me to look outside Philadelphia for a new job. That's how we ended up in California.

North American Aviation – June 1953 to December 1955

I became interested in North American when I saw a paper by Ed Van Driest¹ who worked there. Later that paper would be the basis of several papers that I wrote. Van Driest was very meticulous. When deriving the turbulent conservation equations he included all the correlations of fluctuating quantities. This resulted in some equations that took virtually an entire page in the paper. This kind of careful attention to detail served him well. Many of his papers were the cornerstone of particular aspects of fluid dynamics. However, Ed's sometimes nervous presentations to his peers made him believe that he needed something to build up his confidence. His solution was to get a second PhD in Switzerland after Caltech.

After reading Van Driest's paper I noticed an ad in the Philadelphia Inquirer for employment at North American. I called the number in the paper and spoke to Gordon

¹ Van Driest, E. R., *Turbulent Boundary Layer in Compressible Fluids*, Journal of the Aeronautical Sciences, Vol. 18, No. 3, pp. 145-161, March 1951

Olson who would become my supervisor. North American did everything based on a written application and the phone call. There was no visit to California. We left so quickly that in order to comply with University policy Vicki had to pay someone to substitute for her at her graduation ceremony at Penn. Vicki and I left for California singing, "Paint Your Wagon – where we are going I don't know" and "Que sera sera". We ended up on the famous Route 66. It was a memorable trip. We had never seen the West. We went by way of the Grand Canyon, Hoover Dam, and Las Vegas.

When we entered Los Angeles I was struck by the clean white and bright look of Wilshire Boulevard. Philadelphia usually had an overcast gray look. While exploring the surrounding area we entered an elevator in Hollywood. A young girl looked at me and asked, "Are you anybody?" We were not in Philadelphia anymore.

We looked at the area surrounding North American in Downey and decided that we liked the Westside better. We rented an apartment at 201 South Sepulveda at the edge of what is now called Brentwood Glen. We knew it as the Rattery Tract. The 405 was not yet built. There was a lot of traffic on Sepulveda. Every time a truck went by the whole apartment shook. One day Vicki and I drove north on Sepulveda to see the sights. We got to the middle of the pass, near Mulholland Drive, and turned around. Obviously there was nothing ahead to see. I still have the Auto Club map of the San Fernando Valley from 1953, and it is almost blank.

I joined a carpool so Vicki could have the car sometimes. The route we took from Downey to the Westside varied but we usually went over Imperial. There were lots of cows along the way. The 101 and the 5 were not completed.

North American was a pretty regimented company. You could tell the status of everyone by the badge he wore. A yellow badge with numbers and no name meant an hourly employee who had to punch a time clock upon entering and leaving. He had to get permission from his supervisor to go to the men's room. He could not leave before quitting time when a whistle or a bell, I don't remember which, would go off. I had a yellow badge with my name on it. I could go to the men's room if I wanted and I could leave before quitting time, but I needed a note from my supervisor to give to the guard upon leaving. Also the parking spaces near the building were reserved for people with gray or white badges. Gray badges were for senior specialists like Van Driest while white badges were for supervisors. The guards inspected everything as we entered or left. This included even brown bag lunches. My friend Don Dooley was in Operations Research. He tried to enter with a book called "The Theory of Games" which he needed for his work. The book was confiscated by the guard. The guards got a bit scary during a machinists strike. They showed up in complete battle garb including steel helmets and rifles with bayonets attached.

I was in an organization called Preliminary Design. We occupied a region on the second floor which had skylights to give the draftsmen good light. We were housed in what was known as a bull pen under the skylights with no separation from our neighbors. Those with the loudest voices talked about high-fi and automobiles all day. There was no air-conditioning, but we had one large fan pointed at the bull pen. There were private offices for the big wigs on the periphery of the bull pen along the outer wall of the building. These offices had evaporative coolers in their windows. After I left North American I was told that management decided to replace the evaporative coolers with freon based room air conditioners. Someone decided it was too expensive to remove

the evaporative coolers so they mounted the room air-conditioners over the transom with the hot side facing the bull pen.

Preliminary Design had one or two individuals representing each of the main line organizations such as design, weights, structures, thermodynamics and aerodynamics. This caused constant friction with some of the line organizations. Flanigan, head of aerodynamics, was constantly snooping around to see what our guy, Shimizu, was up to. Shimizu was amazing in his ability to conceive of a new missile or airframe shape. A priority was always eye appeal. I am sure that was important in selling the concept to the government.

The major project in Downey was the Navaho intercontinental supersonic ram jet. This was in competition with ICBM programs such as Atlas. The Navaho project was divided in three successive phases. The first phase was powered by turbojets to check out the configuration. At the time the only turbojets available were Westinghouse J40 engines. Unfortunately two of these underpowered engines were required. These were mounted on the sides of the vehicle. In order to justify the usefulness of the turbojet phase the ram jet G-26 and G-38 vehicles were designed with twin side inlets. Management was constantly trying to prove that twin side inlets were best. These inlets led to a very complex ducting system leading to the combustion chamber. In the early ground tests the strong secondary flow in the ducting caused the flame to go upstream.

The G-38 had a cruise Mach number of 3.25. I was asked to analyze the surface temperature. Supervision was not pleased with my results. They did not realize that the adiabatic recovery temperature due to friction in the boundary layer would be close to the stagnation temperature. The boundary layer is a region close to the vehicle surface where friction and heat transfer play a major role while outside this region these phenomena usually can be neglected. Of course radiation would reduce the surface temperature somewhat depending on altitude and surface emissivity, but they were not happy with my results.

Complex computations, which required more than slide rule accuracy, were done with the aid of the Friden machine. Mostly women were assigned to run these calculators. We had a real expert who could make the machine sound as if it was playing "Yankee Doodle". I thought I should learn how to program a computer. I took a class and learned the procedure. The programming method was close to machine language. Numerical calculations were done in octal. Also we had to estimate run time because it was known that statistically the computer's electrostatic storage would drop a bit about every half hour. Therefore we had to program the code to recompile every half hour. We would fill out green sheets with our program written in specified spaces. These sheets were sent across town to El Segundo for key punching. The resulting cards were fed into the IBM computer. Usually a package came back that said "Trouble". With the help of someone more experienced my program for transient heat transfer in a sphere finally ran. We had a loose leaf manual for programming. One day I received a package in the mail that replaced $\frac{3}{4}$ of the manual. At that point I decided that programming is a full time job. I decided to depend on others for programming and stick to engineering. I did not take up programming again until about 1972.

For me the most positive thing I experienced at North American was the opportunity to speak at length with Ed Van Driest. I learned a lot about boundary layers. I also took a UCLA Extension course that Ed gave on boundary layer theory. In addition

to the technical discussions I enjoyed Ed's droll sense of humor. For example, he had to brief management at an after hours meeting. He decided that it seemed like a TV interview. Later when referring to that meeting he would say, "Last night on TV ---". He always called me "young fellow" even years later when my hair was pretty gray.

Preliminary Design was given the task of preparing a proposal for an Intermediate Range Ballistic Missile (IRBM). Management called it Comanche. They liked Indian names. It was recognized that the nose cone was a difficult new problem. There were a number of German engineers working at North American who had been part of the V-2 program in World War Two. They pointed out that the Germans used Fiberglas and graphite jet vanes in their rocket exhaust region. Although they lost some material they successfully survived this extreme environment. I was asked to investigate using such materials for the Comanche nose cone. I would have preferred a metal nose cone because it would be easier to analyze. It was decided to fabricate several nose cones with various binder materials and test them in a rocket exhaust in North American Rocketdyne's Santa Susana facility. By that time we were living in a little house in Studio City. Without thinking I set out on the morning of the test directly to Santa Susana. I was stopped at the entrance and told that I needed a written permission from my supervisor in Downey. Going to Downey first would have added about 60 miles to my trip. After a long discussion by phone with Downey I was finally allowed to supervise the tests. About a dozen people had been standing around waiting to get the tests started. While walking through the offices toward the test area several men were pointed out as "paper clip" Germans. With their steel rimmed glasses and foreign appearance, they sure looked like the heavies in wartime pictures about the Nazis. In the tests some of the test cones blew up and others ablated but remained intact. Several years later ablative heat shields which dissipate reentry heat by allowing its outer layers to vaporize and/or combust in a controlled manner became the preferred type of heat shield.

I used the data from the tests to predict ablation of the Comanche nose cone by assuming that the ablation was proportional to the aerodynamic heating. This was not that far from the method of ablation analysis developed about eight years later. Unfortunately the Comanche proposal did not result in a contract.

Late in 1955 I decided that I would like to change jobs. Ed Van Driest told me that Joe Charyk, editor of the famous Princeton Series on aeronautics was looking for people at Lockheed in Van Nuys. I had to be very careful when contacting Lockheed for fear of being fired prematurely. I had seen the assistant manager of Preliminary Design, George Jeffs, in action. George was a brilliant technical person but a scary manager. When someone complained to him about working conditions or some other gripe he would shake hands with him and tell him it was nice knowing him but he did not want him around any more. I made all my calls to Lockheed by pay phone far away from our bull pen. In December 1955 I got the job at Lockheed.

Lockheed – December 1955 to November 1956

It was actually Larry Kavanau who worked for Joe Charyk that hired me. Larry, who got his PhD at Berkeley had recently hired Dan Tellep who just got his MS at Berkeley. Unlike North American we had private offices. At my level I shared an office

with Dan Tellep. As you may know Dan eventually became CEO of Lockheed and arranged the merger with Martin.

In the 50s Southern California had a large pool of aerospace engineers and scientists who moved from company to company depending on where the contracts were. Although the workers were the same, the Lockheed that I found was very different from North American. I assumed it reflected management style. The vice president had the exact same kind of badge that I had. I could come and go without permission from my supervisor. The guards were very pleasant although they did have to make sure that we were not taking home anything classified. We did not even fill out time cards. A time checker came around once a day and asked what we were working on.

Lockheed Missile Division was located on the edge of Van Nuys Airport in two buildings called lean-to buildings because of the shape of the roof. They really were nothing like the familiar greenhouse lean-to. They were large two story buildings with a high bay area on the first floor, but they were referred to as the north and south lean-to. The newly created Missile Division consisted of engineers, mostly from Lockheed Burbank, in the south lean-to and scientists, many from Los Alamos, in the north lean-to. There was a war between the north and the south. On December 13 my first child, Linda, was born. On the same day there were big black headlines in the Los Angeles Times "20 Scientists Leave Lockheed". I came to work handing out cigars and was greeted by George Sutton who said, "Life goes on" The scientists headed by Ernie Krause, including Joe Charyk and Larry Kavanau left to form a new company called Aeronutronic in an abandoned grocery store. George Sutton and Wally Warren left to join General Electric in King of Prussia, PA outside of Philadelphia. George went on to head the rival AVCO Everett Research Lab and was editor of the AIAA Journal for many years. Wally later had a lab at Aerospace Corp. before starting his own company. Dan Tellep and I stayed at Lockheed.

The war between the north and south continued at a reduced level after the exodus. We were working on the Air Force X-17 project which was sold to the Air Force by the departing scientists. It was decided that all of the three stage vehicle except the nose cone would be the responsibility of the engineers in the south lean-to. Dan and I, residing in the north lean-to under the direction of the remaining scientists, were responsible for the nose cone. The object of the project was to learn how to penetrate the "heat barrier" during reentry of ballistic missiles. The first two stages of the X-17 were fired upward and the third stage was fired down toward the earth to simulate high speed reentry.

The full-scale X-17 employed three solid-fueled stages. The first stage was powered by a Thiokol Sergeant rocket which could produce a thrust of 48,000 pounds. The second stage employed three Thiokol Recruit rockets which could each produce a thrust of 33,900 pounds. The third stage was powered by a single Thiokol Recruit rocket which could produce a 35,950-pound thrust by utilizing an enlarged exhaust nozzle.

The early test flights failed a few seconds after second stage ignition. One day Dan and I took a look at the inter-stage hardware at the base of the second stage. I said to Dan. "How come those nozzles are buried so deep in the skirt? I was reading in Courant and Friedrichs² that exhaust plumes expand at altitude. I bet the exhaust plume

² R. Courant and K. O. Friedrichs, "Supersonic Flow and Shock Waves" Interscience Publishers, Inc., New York, 1948

is damaging the skirt". We had several really great consultants available to us. One was Anatol Roshko from Caltech. We asked Anatol if this idea was right. He agreed. The job of convincing the engineers in the south lean-to was much more difficult. The project manager was Frank Bednarz and the person in charge of thermodynamic analysis was Lloyd Wilson. We talked to Dick Hognlund who I think worked for Wilson and he understood right away. Wilson, however, insisted that he had tested the system and the flame only singed a screw. I asked where he did the test and he admitted it was on the ground. I got pretty angry and we stormed into the assistant project manager's office. Fred O'Green was a brilliant electrical engineer who later became head of Litton Industries. He wore hearing aids and often asked in a meeting, "Are you listening or do you have the gain turned down?" Dan exhibited the diplomacy with O'Green that must have helped him rise in the company. I think it was the Chief Engineer, Willis Hawkins, a very competent guy, who made the final decision. The result was that Lockheed would propose to change the inter-stage at the next meeting with Ramo Wooldrige who were technical managers for the Air Force. They were convinced. It took Lockheed about 10 days to make the change and fly again. Today it would take the better part of a year to do that. The result was about 20 successful flights.

The scientists at Ramo Woolridge were headed by George Solomon assisted by John Sellars. Lester Lees of Caltech and George Carrier of Harvard were prominent consultants. Other people that we had contact with included Budd Cohen and Jerry Fox. Our project manager, Frank Bednarz, hated having them tell him what to do. He actually had an alarm system set up to warn him whenever they entered the plant. We had review meetings in a large room with a huge round table which must have been built there because it could never be moved in or out. The meeting usually began with Frank entering late, slamming down his papers and declaring, "OK Let's fight!" A meeting that I remember had to do with Lockheed not being able to meet performance requirements. I am afraid that was my fault. The scientists at Ramo Woolridge and ours that had left originally thought that it would be impossible for a vehicle to reenter the atmosphere unless it experienced only laminar flow. The X-17 nose cone was designed as a nickel plated copper spherical shape with a 2 micro-inch finish to prevent transition to turbulence. The wall thickness was thinned out away from the stagnation point in accordance with the predicted drop off in heating rate in laminar flow. I thought what if it becomes turbulent can we handle that? I developed a turbulent analysis based on what I had learned from Van Driest and some ideas of Lester Lees on the effective Reynolds Number on a three dimensional body with variable flow outside the boundary layer. The results showed that unlike laminar flow where maximum heat transfer was at the stagnation point, in turbulent flow the maximum heat transfer would be near the sonic point of the flow outside the boundary layer analogous to the throat conditions in a nozzle. On a sphere the sonic point would be about 45 degrees from the stagnation point. Therefore we redesigned the nose cone with a constant wall thickness. This made it heavier than the original design and led to a degraded performance. George Solomon asked, "Why doesn't Lockheed want to meet the required Mach Number of 15?" Bednarz said, "Lockheed wants to, but we can't." Somehow it was decided to fly anyway.

The data that we obtained from the flights consisted of temperature measurements from an array of about ten thermocouples. We located the

thermocouples very close to the surface of the nose cone. To interpret the results we really wanted to obtain the heat transfer rate rather than just the temperature. We solved the transient heat transfer problem backwards to get the instantaneous heat transfer rate. Of course this problem is inherently unstable so that we got wiggly bands of heat transfer rates. Nevertheless, it was obvious that the largest heating rate was near the sonic point. To interpret flight data we used three IBM 650 computers and about a dozen women running Friden machines. There was a flight about every 10 days and a Quick Look Report was required a few days later. There were a lot of people walking around with red eyes whenever we had a flight. Once something went wrong with the computers and the person who programmed them was not around. Bednarz moaned, "My whole project depends on a girl named Irene!"

Although we were allowing for turbulent flow the Ramo Woolridge people were still hoping for laminar flow. One day John Sellars came over to inspect a nose cone that was about to fly. It was impossible to manufacture these nose cones without some flaws. John worked out an elaborate analysis for allowable length width and depth of a pit. The nose cone flunked. In order not to delay the flight someone got the brilliant idea of taking the nose cone to a dentist to fill the pit with gold. The nose cone flew and performed as usual.

Dan and I had lots of fun encounters with our friends at Ramo Woolridge. In one case we were going to fly a sphere cone configuration. The RW people were worried that the abrupt change in the radius of curvature at the joint between the two shapes would trigger turbulence. They gave us an algebraic power series formula for a transition. When we worked out the geometry it turned out that although the radius of curvature had a smooth transition the region appeared to have horns facing forward. They called with new constants but now the horns faced backwards. We abandoned this exercise.

Another fun episode had to do with trying to reduce stagnation point heat transfer. It was well known that stagnation point heat transfer on a sphere is inversely proportional to the square root of the sphere radius. Therefore if we make a nose cone with a flat stagnation point region (infinite radius of curvature) there would be no heat transfer. Professor Wally Hayes was another of our consultants with whom we discussed this idea. He came back the next day and said, "I was thinking of your problem while taking a shower. As the water hit my nose I realized that it is not the body radius but the shock shape that governs the flow." He worked out a simple theory that Dan Tellep implemented for analyzing various shapes. We designed some pretty weird shapes including a 5th degree paraboloid. This shape had a very flat nose and no discontinuity in radius of curvature, but the peak turbulent heating was still near the sonic point.

Lloyd Wilson was a very aggressive manager. He constantly tried to get Dan and me to join his organization. After he left Lockheed he became head of Brush Beryllium. Later he was one of the founders of RDA which was a spin off of Rand Corp. During his time at RDA he was shot and killed by his wife.

We had lots of fun going to lunch. Often Professor Sol Penner, who was still at Caltech at the time, did the driving. Others who went to lunch included Henry Aeroeste, who was getting a PhD at Caltech, and Marcel Vinokur. Marcel always ordered bread and ate the whole loaf.

My time at Lockheed was the most fun I had before or since. Unfortunately it had to come to an end. Lockheed decided to move the missile research work to Palo Alto and the engineering to Sunnyvale. We could not sell our house in Studio City. Looking back on that cute little house in the San Fernando Valley I call it Denison's Folly. It was about 8 feet from a steep hill composed mostly of shale. I tried to plant ivy on the hill by hammering holes in the rock. Not only did the ivy fail to grow, but I got a bad case of poison oak. Some time after we did sell the house the hill came down and filled up the master bedroom with mud. Fortunately no one was hurt.

I gave Larry Kavanau a call at Aeronutronic and he hired me again.

Aeronutronic – December 1956 to June 1961

When I joined Aeronutronic it had become a subsidiary of the Ford Motor Company and occupied the old Glendale Air Terminal on Air Way in Glendale. The president was Gerry Lynch who came from Ford and the vice president was Earnie Krause. They and other major managers such as Joe Charyk had their offices with outside windows on a second floor balcony. Larry Kavanau had a private office on the ground floor near the one I shared with Steve Scesa who had a PhD from Berkeley. The building was ornate Spanish style, but piles of sawdust announced the presence of termites. There was a private dining room on the north side of the building. The tables had table cloths and service was on China. Waitresses would stand unobtrusively behind poles waiting to collect our written choices for a meal. When Gerry Lynch wanted something he waved his coffee cup and someone came immediately. He liked to live in style. He was living in one of the cottages at the Huntington Hotel where he gave a reception for all employees complete with receiving line and waiters with white gloves. He also gave a Christmas party at the California Club. At this time there were about 200 personnel at Aeronutronic. I knew virtually everyone in the company.

In addition to Larry Kavanau, who was in charge of aerodynamics and related fields, Dave Altman also worked for Charyk in the fields of chemistry and propulsion. Dave had recently run a Lab at JPL. After Aeronutronic he went on to become an executive vice president of United Technology in Sunnyvale. While we are discussing these people I should note that Larry Kavanau eventually became Special Assistant (Space) to Director Defense Research & Engineering, a vice president at North American, and started a small company called SYS. In 1963 Joseph Charyk became the founding president of the Communications Satellite Corporation, and under his leadership, COMSAT ushered in the age of commercial satellite communications.

Steve Scesa grew up in Grass Valley. He had worked at Ramo Woolridge in propulsion monitoring. He laughed at the naiveté of his fellow monitors when presented with a drawing of a rocket nozzle by the contractor. It was not a flyable version. Steve was a bachelor who rated restaurants based on the number of old people eating there. He said that if there are a lot of them the price is reasonable and the food is easy to digest. Although the Glendale airport was closed and the runway had a big X on it there were a few planes out there. A Ford tri-motor sat right next to our window. Every once in a while someone would start up the engines. It was quite noisy. Steve and I got various inventions to evaluate that people wanted Ford to buy. Several were perpetual motion machines that we advised the company to reject.

One day I went to lunch over on Riverside Drive with Seymour Lampert and Joe Neustein. The waiter warned us not to eat the hamburgers because some fly spray might have gotten in them. It was too late. Sy called his lawyer and Joe called his doctor. Nothing came of it but the reaction was perfect if you know those two guys.

Don Dooley who I knew at North American showed up in an office down the hall. He was just finishing up his PhD with Professor Frank Marble at Caltech. His thesis was in the new field of Aerothermochemistry promoted by Theodore von Karman. Dooley got Frank Marble to come over as a consultant. This began a relationship for me that lasted virtually the rest of my career. Frank is a brilliant scientist-engineer with all sorts of insights and perspectives on technical problems and people. Also he can tell a good story. I should mention that Joe Neustein had worked in his group at what used to be called NACA Lewis Lab in Cleveland. Frank along with Sol Penner was a student of von Karman. Penner was a prolific writer of scientific papers. Frank had a bit more careful style. Frank used to quote von Karman's assessment of Penner, "First he talks, then he writes, and then he thinks."

Dooley introduced me to the ideas of Aerothermochemistry, and we thought maybe we could explain the behavior of a graphite nose cone during reentry which might be better than the metal heat shields that were used on all the US ballistic missile programs. We did an analysis for a laminar boundary layer and wrote it up³.

On October 25, 1957 my second daughter, Joanne, was born. No exodus of personnel occurred this time when I gave out the cigars.

A few other events occurred while we were in Glendale. One day the management style of Ford reared its ugly head. I entered the building somewhat late and was greeted by some administrative person at the door who wanted me to sign in and explain why I was late. I told him if they want me to do that I was not coming in, and I went home. They never did that again.

At the time that the Russians launched Sputnik Aeronutronic had one of the few space projects in the country. It was called "Project Farside". A missile was supposed to be launched from inside a balloon provided by General Mills. The nose of the missile was supposed to cut a hole in the balloon and fly off to take pictures of the far side of the moon. Unfortunately none of the attempts succeeded in getting out of the balloon.

Aeronutronic decided to bid on the Atlas missile. This was pretty gutsy with only 200 employees no matter how smart they were. They decided to show pictures of the Ford factories and said they could build anything. This was a big effort with a lot of comic relief. There was a guy named Charlie Wong who was our only structures person. Larry Kavanau asked him, "Charlie how many men do you need?" Charlie did not have a clue but he answered, "Fourteen." There was an administrator named Ghant who was running the printing machine. He looked at the size of the proposal and yelled, "I need 50 more pages of technical information!". Needless to say we did not win, but we got a booby prize. Don Dooley, Frank Marble and I had been talking to Bob Bromberg and Budd Cohen at Ramo Woolridge about the idea of using graphite for nose cones. Frank came up with the idea of testing graphite combustion in pure oxygen to simulate the greater heating rate in reentry that we could not reproduce on the ground. We had sent in a proposal to do such testing and briefed them on our boundary layer theory. I do not

³ Denison, M.R. and Dooley, D.A. *Combustion in the Laminar Boundary Layer of Chemically Active Sublimators*, Aeronutronic Document No. U-110, September 23, 1957

know if there was any connection to the Atlas proposal, but it was the same agency. We got the job.

Winning that contract caused a big problem. The experiments required a large tank farm of oxygen containers and a blow down facility to get the oxygen into a graphite cylinder for combustion. We could not do that in Glendale. Nicholas Bortinsky, born in Russia, was the company architect. He had been trying to get the management to decide on a site for an expanded company. They had rejected every proposal. He laughed. He thought it was the required investment. He found the perfect spot on the Irvine Ranch in Orange County. No investment would be required. The land would be leased. The Irvine Ranch was considered grazing land. If they sold part of it there would be a huge tax. Nick asked Don Dooley and me to go down there with him and inspect the site to see if it would be suitable for our experiment. It really was grazing land. There was a herd of buffalo on the site. We walked around kicking buffalo chips. At least they were dried out. We found a spot on the edge of the property where we could do our experiment. That is why Aeronutronic moved to Newport Beach.

This time we were able to sell our house in Studio City. We moved to Newport in March 1958. Our house in Westcliff was ten minutes from the site of our new offices and laboratories on the newly built Ford Road. While we waited for the offices to be built Aeronutronic rented space at the Jamaica Inn on the Coast Highway. One day I got an angry call from Gerry Lynch. He was showing the Ford Chairman of the Board, Ernie Breech, the new site when they spotted my secretary, Peggy Lenny, washing her car at the site. Furthermore it was a Buick. This is not the Ford public posture. The story grew and grew until people said Peggy was wearing a Bikini. I saw her. She was wearing blue jeans.

After a while all that calmed down. Then I got a message from Mr. Lynch telling me to move in to the new building. I went up to the site to take a look. Then I sent him a message telling him that we need a few things first like walls, windows, plumbing and electrical. Communication with the construction crew was not very good. They did have the framing up. One problem was that all of the design was done in Dearborn Michigan. I guess that is how we got all those deciduous trees. Eventually we did move in. Frank Marble referred to my office as a meat locker because it was so cold. We had to comply with a lot of Ford regulations. The wall behind the secretary was the official Ford blue. By this time I was a Department Manager. To signify this so that everyone would know my status upon entering my office I was required to place a company issue double pen set on the center of my desk.

The social life at Aeronutronic was pretty good. Newport Beach was a pretty small town so we spent a lot of time with other Aeronutronic people. We had sumptuous dinners prepared by Sue Green, Leon Green's wife, and Shirley Kavanau. These were sophisticated gatherings with good conversation to go with the meal. A few years later Shirley died of cancer at a young age.

George Carver designed and supervised construction of the blow down facility. Chuck Hallum and Dale Jefferies were the technicians. In the process of checking out the facility we had a bad accident. Chuck was turning the valve manually to allow oxygen to flow through the test section. He said later that it seemed a bit harder to turn than usual. There was a flash fire. Chuck was burned on 70% of his body but he survived. Recovery was a very long process. In the accident investigation it was learned

from the oxygen supplier, Linde, that they had several similar accidents. We never really figured out what happened. The only fuel was the material of the valve itself. After a long investigation some changes were made and we were able to conduct the tests.

The test results were in pretty good agreement with our predictions. We wrote up a company report⁴ and later sent the results in for publication⁵. My co-author Gene Bartlett, who was in my department, went on to become one of the founders of Aerotherm, Inc. The theory was published in a company report⁶ and later published in the IAS Journal⁷. The theory was based on an extension of a Van Driest like model to include the chemical and ablation ideas that were influenced by Don Dooley and Frank Marble.

Some of the other people in the department included Dick Hoglund, Skip Byron, Bill McCauley, and Eric Baum. Dick Hoglund, who went to Northwestern to get his PhD after I met him at Lockheed, concentrated mainly on advanced propulsion. He took my place after I left Aeronutronic. Bill McCauley was an expert on decoys. Eric Baum and I worked together for many years at various companies. Managing such a high powered group is an acquired skill. These are self motivated people. It is important to get them interested in the work. However, working on government contracts is sometimes an impediment. One day a young Physicist came in all upset and said to me, "Fermi did not have to fill out a time card." I replied, "You are not Fermi."

Around this time I was working for Dave Altman instead of Larry Kavanau. Dave had a PhD in Chemistry from Cornell and also worked on the Manhattan Project. He had a Chemistry Lab and a Materials lab in the building. Dave was a terrific scientist and a great salesman. He usually made his briefings simple. "Item number one so and so, Item number 2 this etc."

Dave had a pet problem that he often explained to many of Aeronutronic's Physicists and other scientists such as Frank Marble. The problem was unstable burning in solid propellants. The Physicists included those that came over from Lockheed with Ernie Knause, such as Montgomery Johnson and Harold Hall. Johnson was an elderly highly respected scientist. Harold Hall was a great guy who did not mind asking elementary questions about any field new to him. Another Physicist was Ernie Bauer. Ernie wrote long papers on Fluid Dynamics which was not his field. He would only quote Landau and Lifshitz, the Russian Physicists, on this subject while completely ignoring more than 50 years of work in Western Europe and the United States.

I sort of figured out from talking to Frank Marble what Dave's problem was all about. When a burning solid propellant experienced a fluctuation in chamber conditions it would sometimes suddenly go into unstable combustion. A part of the problem was describing the burning rate based on physical principals. Marble introduced me to laminar flame theory which had been pioneered by von Karman. Rocket developers

⁴ Denison, M.R. and Bartlett, E. P., *Experimental Ablation Rates in a Turbulent Boundary Layer*, Aeronutronic Document No. U-702, November 1, 1958

⁵ Bartlett, E. P. and Denison M. R., *Experimental Ablation Rates in a Turbulent Boundary Layer*, Paper No. 60-WA-208 presented at ASME Heat Transfer Division Meeting, New York, NY, November 27-December 2, 1960

⁶ Denison, M. R., *Combustion in the Turbulent Boundary Layer of Chemically Active Sublimators*, Aeronutronic Document No. U-166, March 10 1958

⁷ Denison, M. R., *The Turbulent Boundary Layer on Chemically Active Ablating Surfaces*, Journal of the Aerospace Sciences, Volume 28, No. 6, June 1961

used more empirical methods to predict burning rates, but the flame theory was a more fundamental way to describe the burning rate. As I mentioned Dave Altman was a great salesman. One of the projects that he sold to the Navy, which was being monitored by Johns Hopkins, had an unusual problem. The contract was almost over and there was a problem finding a way to spend the remaining money. I had an idea how to solve Dave's pet problem. I got the go ahead to spend the remaining contract money.

My idea was that the unstable burning was controlled by heat transfer in the solid phase. Because of its much lower density the gas phase, including the laminar flame, would respond to changes in the solid phase almost instantly. The conservation equations in both the gas phase and the solid phase were perturbed. The resulting transient heat transfer problem was solved by Laplace Transforms. Not many problems are solved this way today. In this method the problem is transformed into an ordinary differential equation in the transformed plane. This equation is then solved with the transformed boundary conditions. The killer is transforming the solution back to the physical plane. Eric Baum had just joined us after getting his PhD at UCLA. He did not see anything unusual about doing the transformation. After working out the solution we obtained stability conditions that depend upon a few dimensionless parameters of the steady state conditions. We eventually published the analysis in the American Rocket Society Journal⁸. We made a lot of people angry. Johns Hopkins did not like what we did because they were working on the same problem. The contract was not extended. The ARS Journal was run by people at Princeton. This paper must have snuck into their journal when they were on vacation. For eight years they tried to prove something was wrong with it. Finally the paper was recognized as the classic paper on the subject. Professor Fred Culick of Caltech told me that he had tried many ways to come up with something different but it all led back to our work. Professor Forman Williams of UC San Diego told me something similar. I am very glad that I stayed away from that field the rest of my career. Nothing could top that.

Ed Zukoski like Frank Marble was a professor of Jet Propulsion at Caltech and another one of our consultants. In fact he was one of Marble's students. One day he told me that his job was to escort von Karman, who was in his 80's, around Pasadena and look out for his safety. For example he said that he had to launch von Karman in a swimming pool. One day Ed and Frank brought von Karman to visit our laboratory. On the outside of the building on the granite wall was a sign that read *Aerothermochemistry Laboratory*. Von Karman was pleased. He said, "Dot iss my word." Later I showed him the results of our experiments on graphite combustion. He was a bit skeptical. He asked. "Iss dot a log-log plot?" He felt too much error is hidden on a log-log plot of an experimental correlation.

On July 3, 1960. My son Jimmy was born. Vicki had told the doctor that she did not want natural childbirth which was becoming popular. She had no training for it. When she got to the hospital she was told they do not give anesthesia. The good thing about it was that I could be with her most of the time. When the girls were born my role was pacing outside the delivery room for the 18 hour labor period. In this case I knew Vicki was OK because I heard her screaming and cussing out the doctor. It got worse

⁸ Denison, M. Richard and Baum, Eric, *A Simplified Model of Unstable Burning in Solid Propellants*, ARS Journal, page 1112, January 1961.

when he left to get his horse off a trailer. When it was all over we had a healthy baby and Vicki had a beautiful view of the harbor from her room at the Hoag Hospital.

Some time in my last year at Aeronutronic Dave Altman left for bigger and better things at United Technology. The person who replaced him was Frank Denison, no relation, who had come from JPL. It is a tough job to come in and try to lead an organization that was built in someone else's image. Frank had trouble doing it. He called a meeting of department heads and senior staff at midnight on a Saturday night. Those attending included Sy Lampert, Joe Neustein, George Carver, Leon Green, George Mills and me. Frank opened the meeting with, "Why do you hate me?" We spent many hours explaining that we did not.

One of the major fringe benefits of working at Aeronutronic was that we could lease Ford cars at very low rates. The time period was one year. I remember I once leased a fancy Mercury station wagon for \$35 per month. The reason we got this nice benefit was that Ford had a policy of giving the same benefits to all employees at the same level. My salary of about \$8000 per year was equivalent to some of the plant managers in Detroit. There was a downside to this for some. Our Division President, Lloyd Smith, was caught trying to set up a company of his own on the side. Two guards came to his office and told him to turn over the keys to his leased Lincoln. He was then ushered out of the plant without being allowed a phone call or to take any of his papers or books.

The fun was beginning to disappear at Aeronutronic. It was getting too big. Also I did not want to send my children to school in Newport Beach where the PTA was holding bonfires to burn books that they did not approve of. Joe Neustein had already left to work at a small company in Pasadena. He wanted me to join him.

Electro Optical Systems – June 1961 to June 1965

Electro Optical Systems was founded and run by Dr. Abraham Zarem. Abe was a unique personality. He was once voted as the most outstanding young Electrical Engineer after he got his PhD at Caltech, but it was his personality and insight that really made him special. I heard that when he would meet someone he made an immediate judgment about the person that never changed. I was pretty nervous about that when I went for an interview. Apparently Joe Neustein's recommendation was sufficient. I heard that people were getting stock options so I asked and I received some. I did not think options on stock of this company of less than 200 people were worth much.

We moved to Tarzana to a \$30,000 house on half an acre. We built a 20ft by 40ft swimming pool in the back yard that the kids and I loved. Joe Neustein and some others were annoyed that we did not locate in Pasadena to show company loyalty. We had the house in Newport Beach for sale at about the same price as the one we bought in Tarzana. The Real Estate market was weak at the time. We owned two houses for about a year.

Abe ran the company like a Hollywood mogul. He used what he called the star system. He hired people who were outstanding in their field and then promoted them in proposals to the government. Ted Forrester was the star of ion propulsion, Gordon Cann was the star of plasma jets, and I guess I was supposed to be the star of reentry phenomena. Abe slept no more than 4 hours per night. If he went to Washington he

would call people in the company as soon as he got there. Abe's office was the only one in the building with windows. He did not want people to be distracted. Gordon Cann and Rolf Buhler both came from Plasmadyne but they did not get along. Therefore, Abe made them share an office. There was an emphasis on experimental work. We all had lab books where we were supposed to record our activity for the purpose of obtaining patents. This was pretty foreign to me, but I did end up with a patent award on a fluidics device during my employment at EOS.

The first person I met when I went to work at EOS was Irv Weiman. A bad penny turned up again. I knew Irv from Penn and Philco. Irv was about the 3rd or 4th employee. Also Dave Medved from Philco was there. I did not have much to do with either one during my stay at EOS. Saul Feldman was there briefly before he went out and formed his own company, Heliodyne. I had known Saul when he was at AVCO Everett. He was the closest person to my field in the company. While he was there Saul wrote an important paper pointing out the nonlinear variation from the mean of properties in the turbulent wake of a reentry vehicle. I had fun watching Saul. I once heard him talking to one person on the phone whom he promised to meet in half an hour while he was on the other line calling a taxi to go to the airport.

With help from Abe I got a contract from Cliff McLain at ARPA to study the near wake of reentry vehicles. By this time Eric Baum had joined me and we produced another Denison and Baum paper⁹. In this paper we calculated a shear layer which began with a laminar Blasius distribution from the boundary layer on the body. For large distances downstream the solution approached the Chapman distribution. This paper became pretty popular. When I was asked, "Are you the Denison of Denison and Baum?" I had to ask, "Which one?" In later years "I thought you were dead." was added.

More people joined my department including Hartley King with a PhD from Berkeley, Don Duclos with a PhD from Northwestern, Dick Ziemer also with a PhD from Northwestern. Susan Wu who got her PhD at Caltech under Lester Lees had to stay in another building because she was born in China and did not have a clearance.

The work continued on the near wake. Art Mager held sessions at the old IAS building in Hollywood where a particular investigator would give a talk on his work before publishing it. At this and other meetings we had discussions with Lester Lees of Caltech, Marty Bloom of Brooklyn Polytechnic Institute, and Andrew Hammit and Les Hromas of Space Technology Labs.(which later became TRW) A summary of our additional work was published in the AIAA Journal¹⁰.

The ion engine project starring Ted Forrester, sponsored by NASA Lewis Lab was by far the largest project in the company. The lab at EOS had huge vacuum tanks where experiments were conducted. A large fraction of the company was working on that. Gordon Cann's vacuum tanks were smaller and not as low pressure, but the power that he required to run his plasma jet could turn the lights out in Pasadena. Somehow our department got involved in Magneto Hydrodynamics (MHD). We got a contract to study the Hall effect in crossed field accelerators. Dick Ziemer had done similar work before at another company. He built the experimental set up and it ran in Gordon's

⁹ Denison, M. R. and Baum, E, *Compressible Free Shear Layer with Finite Initial Thickness*, AIAA Journal Vol. 1 No. 2 February 1963

¹⁰ Baum, E., King, H.H., and Denison, M. R., *Recent Studies of the Laminar Base Flow Region*, AIAA Journal, Vol. 2, No. 9, September 1964

facility. The Hall effect causes the current to concentrate on the end of the electrode. We did tests with segmented electrodes to measure the current distribution. I did a little analysis to predict the current distribution. This work was presented at a conference at Northwestern¹¹.

I asked Susan Wu to work on the MHD analysis. This was not classified so she could work on it in another building. After she left EOS to move to Tennessee she worked on MHD for the rest of her career. She built a small company around that work. To me analyzing MHD meant adding Maxwell's equations for electro magnetic theory to the conservation equations for fluid flow just as we had previously added chemistry. However, electrons and ions go into very awkward directions. When I went to the AFOSR meetings on MHD I was convinced that MHD stood for Mutual Hate Department. I never saw such acrimony. It was as if the various contractors believed that he who makes the most noise gets the most money. One memorable meeting was held at the Hollywood Roosevelt Hotel. Sterge Demetriades, one of the contractors, came in costume. He had a red vest, a jacket with tails, Prince Nez glasses, and a black handlebar mustache. As he began his talk he whipped out a dagger from under his cloak, jammed it into the rostrum, and declared, "I heard some people here are not friendly to me."

We went on several sales trips. John Teem, a brilliant physicist, born in the Ozarks and educated at Harvard was a vice president. One time John Teem, Gordon Cann, Rolf Buhler and I went on a trip to Wright Field in Ohio. As we were approaching Chicago the pilot came on the speaker, "According to TWA policy of informing the passengers ----we've got a little problem up here. We can't get the landing gear down. If you do not hear from me in the next half hour everything is OK." We did not hear from him and the Convair 880 jet landed safely. Another wearing but less scary trip was one that I took with Abe Zarem to Washington. We took a red-eye to Baltimore airport, cleaned up at the airport and drove to the Pentagon. We finished at the Pentagon and took a 5 PM flight back to LA. Abe did this all the time. He spent a lot of time selling to and briefing the government. His famous quote was, "Dealing with the government is like pushing on a wet noodle."

After I was at EOS for a couple of years Abe sold the company to Xerox. He made about \$20 million which was a lot of money back then. Another \$2 million was shared by those of us who had stock options. Abe was very generous. After my options vested I decided to go to TRW where I thought there were more people with my background.

TRW – June 1965 to October 1972

I called John Sellars whom I had known at Lockheed. He remembered my work on turbulent flow on nose cones. He offered me a position on his staff. I arrived at TRW on Aviation Blvd. near Imperial Hwy. during the Watts riots. When I looked to the left as I drove down the 405 I could see the smoke of what the newscasters were calling "Charcoal Alley". On my first day Budd Cohen called a meeting on some subject. 50 people showed up for the meeting. That would have been a large fraction of EOS

¹¹ Denison, M. R., and Ziemer, R. W., "Investigation of the Phenomena in Crossed-Field Plasma Accelerators", Proceedings of the Fifth Biennial Gas Dynamics Symposium, pp. 201-232, Northwestern University Press, Evanston, Nov. 1963

employees. We were in what was called building 80, an old McDonnell Douglas building. We had cubicles rather than offices. The noise level approached that of a bull pen. Soon we moved to temporary quarters in trailers. This was a much better environment. We had individual offices. After that we moved to trailers in Space Park and finally to a new building that was being completed.

Les Hromas was in this organization. He had weekly meetings with Lester Lees to go over aspects of the reentry wake problem. The wake was important to ballistic missile defense because of radar and optical observables which occur during reentry. It was hoped that these observables would provide a means of distinguishing the reentry vehicle from decoys. Lees and Hromas had written several papers on the subject. One day Chuck Hebel and Ray Markle of Bell Labs came to see Les Hromas, whose work on the wake they knew, and offered TRW a sole source subcontract to their Reentry Measurements Program Phase B (RMPB) contract with the Army. The purpose of the contract was to fly a full scale instrumentation package on about 10 Atlas flights. TRW did not say no thanks. It was estimated in the 1990s that the RMPB project would cost about \$2 billion if it were active then. The project that we got was called Reentry Measurements Instrumentation Package (RMIP). It was as large as the major satellite projects at TRW, but it was always a bit of a step child because it was not a satellite project.

Sam Aschuler was designated as the Project Manager, Terry Bateman was Assistant Project Manager for Integration and I became Assistant Project Manager for Experiments. I played the role of Chief Scientist. I was supposed to make sure that the needs of the experiments did not get overwhelmed by hardware systems considerations. Tommy Thompson, who was in Les Hromas' organization, helped define the experiments and write specifications for them. The hardware systems people wanted the requirements for the experiments very quickly. They did not want to hear from the scientists after that unless something went wrong.

Although I did not really like being assigned to a program office I learned a lot from the experience. Bill Towle who worked in the program office did a lot of stock market investing. He introduced me to Value Line which I still use today. I learned a lot about how a major project is run at TRW. The program manager could spend a limited amount (I think it was \$25,000 back then) without going through formal procedures. In most cases if we wanted to obtain something we would hold a make or buy session. If we were to make it at TRW a work package and a work package manager would be set up. We would get weekly reports of charges. One time the report showed that 25 people charged to the acoustic sensor last week. I went over to the line organization where the work package manager was and asked how 25 people could get near the device that was only about 1.5 inches in diameter. The manpower problem was corrected. If the decision was to buy something we had to go through Subcontract Management. These people had their rolodex files of subcontractors. For specialized material, like a spectrometer, we could insist that they buy it from someone that we had confidence in. TRW had all sorts of special organizations with authority over the project. Quality Assurance was one of them. One day someone from QA came over and told Sam, "I am shutting down your project." He could do that but Sam talked him out of it.

The experiments consisted of instruments flush mounted on the sides of the reentry vehicle to measure properties in the body boundary layer and instruments at the

base of the vehicle to observe the wake properties. In addition to the stationary wake instruments a wake probe was deployed into the wake with data transmitted back to the vehicle along a long cable which was cut at the end of the deployment. Wake probes were deployed at three altitudes during reentry. Three different heat shields were flown on the Atlas vehicle: silica phenolic, teflon, and beryllium with a graphite tip. We set up a Principal Investigator (PI) system for the experiments. Bill Shackleford was the PI for UV and Visible Spectrometers, Ron Watson was the PI for Infrared Interferometers, John Chang was the PI for the Wake Probe Experiments, and Gerherd Grohs was the PI for Electrostatic Probes. We also had Acoustic Sensors, Radiometers and various housekeeping measurements. I found that for any obscure scientific subject I could always find someone at TRW who was an expert on that subject.

Chuck Hebel was the Chief Scientist and Ray Markle monitored Systems Integration for Bell Labs. We had many meetings with Bell Labs both at TRW and at Whippany, NJ. Chuck Hebel was a brilliant Physicist. I remember once when reviewing our method of transmitting a data spectrum he said, "We have to be careful that the cellos do not drown out the violins." In other words make sure that low frequency data does not obscure high frequency data. Some of the others at Bell Labs were interesting but somewhat unrealistic. We did a lot of analysis to go along with the experiments. On one of our trips back to Bell Labs one of their scientists was not satisfied with the number of chemical reactions we were allowing in our model. I told him that adding additional reactions that we did not believe to be controlling the phenomena would be too expensive. He said, "Cost is no object." We called his desire for 79 reactions the 79 trombones analysis. Of course today with much greater computer power available this would not be very expensive, but I still believe in thinking rather than throwing everything in. Another Bell Labs employee came over from Murray Hill, NJ where the really high powered scientists at Bell Labs worked. He said, "I think it is my duty to devote one day a week to military work." At one of the meetings in Whippany a decision was made about a particular instrument. About ten days later a delegation from Bell Labs visited TRW. One of their people, a French scientist, began screaming at us because the instrument was not yet built and flight tested. It took Chuck Hebel and me about a half hour to get him calmed down. One day we had a major review of the experiments. I described and defended our work for about 8 hours.

In the flight program there were several Atlas failures or reentry vehicle anomalies, but when we were able to obtain data the TRW experiments were highly successful. The data, especially the optical data, were still being used well into the 1990s. A project this elaborate will probably not be repeated.

In my spare time I got interested in the electrostatic probe experiment. These probes were flush mounted in an array on the sides of the vehicle. I predicted the decay of boundary layer electron and ion density with downstream distance in the boundary layer that would be measured with these probes¹². This turned out to correlate well with the data. Eric Baum did some more detailed analysis for the flush probes and with Bob Chapkis he analyzed the conical electrostatic probes that were deployed on the wake probe.

¹² Denison, M. R., *Analysis of Flush Electrostatic Probes for Reentry Measurements*, TRW Report 06488-6065-R0-00, September 1967

In June 1968 we moved our residence to Beverly Hills to get a better education for our children. In Tarzana we were happy that the local elementary school was in walking distance for our kids. No busing or driving was necessary. I had the attitude that they really only need one good teacher and they will be fine. Linda went through all six years, but it did not happen. We had to do something. Private schools would be difficult if not impossible to get into. Furthermore, the ones in the LA area did not measure up to those that we were familiar with in the East. Beverly Hills would cost us like a private school because housing was so expensive over there. Although Linda had to repeat the first half of seventh grade because Beverly Hills did not have half year classes it was well worth the sacrifice. All three of our kids did extremely well in the Beverly Hills system. They took lots of Advanced Placement (AP) college level classes and were well prepared for college. All three graduated from UCLA. Linda got her MBA from UCLA, Joanne got her MBA from USC, and Jim got a JD from USC. Incidentally, they were able to walk to El Rodeo grammar school and Beverly Hills High.

The work on RMIP went on after the flights in order to analyze the data. At this point all the systems engineers were off the project and I became Project Manager. By the fall of 1972 the work was completed. Unfortunately this coincided with a downturn in the Aerospace business. I had to pursue other opportunities.

Denison Development, Inc. – October 1972 –July 1982

My father in-law built apartments in Pasadena starting about 1954. He built many buildings on South Orange Grove near the Tournament of Roses Building. He would tear down old mansions and build his apartments. The zoning on South Orange Grove was designated RR. This dictated large units on a low density site while preserving as much existing landscaping as possible. Most of his tenants belonged to the Valley Hunt Club on Orange Grove. Whenever he built a new building they would discuss it at the club and several would decide to move to his new building. He had no trouble filling up his buildings soon after he got a certificate of occupancy. When things got slow for me in aerospace my father in-law offered to help me become a real estate developer.

Denison Development, Inc. was incorporated in October 1972. I decided to build condominiums instead of apartments because the turnover of capital should be faster. I needed legal and accounting help in order to do this. Gerry Marxman was a student of Frank Marble who went into the business world. On his advice I went to Haskins and Sells, one of the big eight accounting firms. I stayed with them throughout my time with Denison Development. After many name changes they became Deloitte & Touche. I asked them to recommend an attorney. They sent me to Ken Rosenberg who had worked at Haskins and Sells before he got his law degree. His partner was Ken Aran. When Ken Rosenberg left the practice of law to become a CEO I stayed with Ken Aran

I built two condominium projects in the South Orange Grove area in partnership with Bill Van Iwarden, a general contractor who had worked with my father in-law on all his projects. Fortunately, a lot of people from the Valley Hunt Club bought our units. Many of our buyers were captains of industry such as Joseph J. Jacobs of Jacobs Engineering and the Chairman of ARCO. We also had some widows of industry leaders such as Mrs. Thompson widow of the head of Whirlpool Corp. My father in – law died of cancer between these projects in February 1974. After the second project I could not find any land on South Orange Grove.

I found a beautiful tract that was just opening up in Palos Verdes Estates called the University Subdivision with views of the Pacific. I built single family houses there in partnership with Dominic Fucci, a general contractor. Based on his work in the development I chose Johannes Van Tilberg as the architect. I needed someone familiar with the area to get the plans through the Art Jury and the Building Department both of which were pretty tough. Van Tilberg stressed view in his plans. I stressed room size which I had learned from our buyers in Pasadena. The houses ranged from 3600 to 5200 square feet. There were strict rules about setbacks and the amount of dirt that could be removed. We had a little trouble with both. When the last house was completed I asked Fucci to get the brush cleared down to the property line so that a buyer could more easily see what he was buying. I looked down the hill from the balcony of the house and thought the stake that marked the corner of the property was in the wrong place. The stake was right but the house was in the wrong place. The house was located based on an erroneous topological survey and surveyor's marking can. As built it did not meet the setback requirements. Fucci and the other contractors wanted me to keep it quiet, but I decided to face the music. I called in the head of the Art Jury who told me to move the house. That would be impossible because it was built into the hill according to the minimum excavation requirements. The lot next door was empty. I decided to try to buy a triangular piece of the next door property. Ken Aran handled the negotiations. The owner wanted an exorbitant price. Ken Aran knew the owner was a doctor who wanted to be far away from his roots in the Ghetto so he said to him. "This is a stickup!" We got a reasonable price. Before finishing with that house we had to pay a fine for excavating too much dirt and deal with a law suit from the Psychiatrist who bought it. The negotiated settlement was covered by insurance. It did not help that the Psychiatrist went to my high school back in Philadelphia. He had a history of buying houses, suing, and milking the insurance.

The price of lots in the University Subdivision rose from about \$100,000 when I first started to about \$250,000 when I finished the last house. Only doctors seemed to be able to afford them. I thought we were running out of doctors. By this time I was back working part time at TRW for Les Hromas. Several of the people at TRW had seen the houses in Palos Verdes and were enthusiastic. I thought maybe a limited partnership to build a condominium project might work. Dick Tiberio, a general contractor, had worked with Fucci in Palos Verdes. I liked him very much. Dick Tiberio and Denison Development became general partners and the investors became limited partners. We found a property in West Los Angeles where the plans were already approved. We had begun construction of the subterranean garage when an inspector came by and told us to stop work. He said the first floor was too high off the ground. We showed him the stamped approved plans, but he said, "That just means it is stamped." I took pictures of a dozen similar properties on the West Side and appeared at a hearing in Van Nuys. With the microphones and the panel of the Planning Commission facing me I thought I was back fighting with AVCO. The foreman asked me, "Is this project under construction?" I said, "Yes." He said, "Continue construction." We finished construction in the spring of 1980. Shortly afterward some kids broke in, defecated in places, and set the building on fire. A neighbor called me at home. I'll never forget the sight of all those fire engines on the scene. We got through it, but for a long time people who would come to look at our units would say. "I smell something funny." To help out with the selling,

Vicki became a Realtor, a career she was happy with for many years. We were hampered with the sales by a sharp decline in the Real Estate market. The result was that many projects like ours went bankrupt, and units were sold by the lenders at a discount. There were black signs with red letters all around us saying "Bank Sale". We finally did sell out. That was the last project I did. None of the construction lenders that we used, United California Mortgage, American City Bank, and Security Pacific Bank are in business today.

TRW – March 1976 to November 1992

My work on my second tour at TRW included projects in coal combustion, laser effects on composite materials, non-ideal air blast phenomenology, and effects of solid rocket exhaust on the ozone layer.

The coal combustion project was run by Albert Solbes formerly of AVCO Everett and a student of Jack Kerrebrock at MIT. The idea of this project was to use rocket engine technology to produce clean combustion of coal for a source of energy in an MHD Generator. The combustor was finally installed at Montana State. I did several studies for this project^{13 14}

The Laser Effects project was initially run by Irv Rubin. There were both laboratory and analytical investigations of the interaction of lasers with composite materials. When Irv left I became the leader of the project. I contributed to what became the infamous ADAM code primarily developed by Dr. George Harpole at TRW. The goal of the ADAM code was to develop a predictive capability for use as a reliable tool in lethality assessments for evaluating material response, especially thermal. The approach of the modeling contained the following main features:

- Use basic principals and physical models, as far as possible, to characterize the key phenomena in the interaction of the laser with the material.
- Obtain major code inputs by direct measurements or independent observations, as much as possible.
- Validate by comparison with all available major observables in specific cases.
- Extend to a broad set of test parameters and apply to scaled predictions.

The code was set up to handle a variety of materials including metals and composites, for input pulse repetition rates and beam intensity, and for any laser frequency. The reason it became the favorite target of the government and competing contractors was that it took many times longer to run the code compared to strictly empirical codes. Of course, it had a better chance of predicting the response outside the range of available data because it was based on physical principles. With the advent of modern computer technology it may be time to revisit this approach if laser effects become important again.

I enjoyed working with Vijay Kulkarny, a Caltech PhD, on this project. We documented some of our work¹⁵.

¹³ Denison, M. R., *Coal Combustion Analysis for MHD Coal Combustor*, TRW Report 99994-6365-RU-00, September 1977

¹⁴ Denison, M. R., *Slag Dynamics in a Cyclone Coal Combustor*, TRW Report 99994-6398-RU-00, April 1978

I started working on the dust lofting problem while I was still at TRW with lots of advice from Frank Marble and Eric Baum¹⁶. The subject was predicting the mechanism of dust sweep up in a nuclear cloud. It was well known that the dust strongly influences the behavior of the cloud environment. I got the idea that the dust cloud is so dense near the undisturbed ground that the particles cannot avoid impacting each other. In that case the cloud near the ground should behave like a fluid with its own kinetic theory. Fortunately some English scientists had already worked out a theory that applied. I matched this layer to the outer turbulent gas cloud.

The meetings of the contractors for the Defense Nuclear Agency (DNA) reminded me of my experience at the MHD meetings. The contractors were Titan Research, SAIC, RDA, and S Cubed. Titan Research was run by Marty Rosenblatt who did a quick and dirty analysis but always got results. The other extreme was S Cubed that did much more elegant work but had trouble getting output. All the contractors used the Cray Supercomputer. I remember one time the leader of the S Cubed group said, "We started the run around Thanksgiving. We looked at the progress after the New Year and we did not like what we saw." David Bacon of SAIC in Virginia did a pretty rational job. Alan Kuhl of RDA did not believe in turbulence modeling. He thought time dependent calculations without any turbulence modeling told the accurate story. He was fascinated by the little whirls in his output. All the others would jump on Alan because his analysis was two dimensional while it should be three dimensional. Naturally all the others would fight with each other about whose turbulence modeling was best. Usually DNA gave each contractor the same conditions to calculate in what was called a "shoot out". There was not much data to compare with since nuclear testing was banned. Old tests such as Grable or Priscilla were sometimes used for comparison.

At some point I was allowed to consult at Titan Research on this problem with permission from our vice president. By that time Marty Rosenblatt had left to form his own company. Phil Hookham, a Caltech PhD, was in charge of the work. An improved analytical model for dust lofting due to shock waves or high speed (>100 fps) winds was developed for the consulting work at Titan. The model divides the dusty boundary layer into two regions, an inner granular flow region where particle impacts dominate and an outer region where turbulent diffusion dominates. The two regions are linked by specifying approximate boundary conditions at the matching point. The effect of surface cohesion is modeled with a parameter representing the energy necessary to fluidize the surface material. The model was implemented into the MAZe (multiphase adaptive zoning) CFD code of Titan Research and Technology. As a validation of the model, the code was used to simulate a wind-tunnel dust lofting experiment conducted by Dr. Richard Batt at TRW. The model produced good agreement with experimental mass lofting results and vertical loading profiles¹⁷

¹⁵ Denison, M. R., and Kulkarny, V. A., *A Simplified Analysis of Pulsed Laser Interactions with Metal Targets in Air*, TRW Report 99994-6757-UT-00, Sept. 1983

¹⁶ Denison, M. R., *A Two Layer Model of Dust Lofting*, TRW Report 48746-6163-UT-00, Sept. 1987

¹⁷ Denison, M. R. and Hookham, P. A. *Modeling of Dust Entrainment by High-Speed Air Flow*, AIAA Paper 95-2206, Presented at AIAA 26th Fluid Dynamics Conference, San Diego, CA, June 19-22, 1995 - Published in AIAA Journal Vol. 34 No. 7 July 1996 pp. 1392-1402

Another project that I spent some time on at TRW was Effect of Solid Rocket Launch on Ozone Layer. A model was developed to examine, on a local scale, the reactions of rocket exhaust from solid rocket motors with stratospheric ozone¹⁸. The effects were examined at two different altitudes. Results of the modeling study indicated that afterburning chemistry of reactive exhaust products can cause local but transient (on the order of several minutes) loss of ozone. The modeling study included potential heterogeneous reactions at aluminum oxide surfaces. Results indicated that these potential heterogeneous reactions do not have a major impact on the local plume chemistry. Homogeneous reactions appear to be of more consequence during early dispersion of the plume. It has also been found that the rate of plume dispersion has a very significant effect on local ozone loss.

In 1979 we moved from Beverly Hills to Little Holmby in Westwood. where we lived for 18 years. Jim was able to walk to UCLA from there.

On November 1, 1992 I retired from TRW

Consulting Etc. – November 1992 to Present

In addition to consulting with Titan I did consulting with Advatech Pacific, Physicomp, TRW and GO Aircraft. The most interesting consulting job involved work on the vertical takeoff and landing aircraft being developed by GO Aircraft Ltd. The work included vehicle aerodynamics, performance analysis, fan design and duct loss analysis. The unique feature of this vehicle is the fan which is used for vertical takeoff and landing. We found that by diverting the jet engine exhaust to turn this fan we could get about twice the thrust that would be obtained by turning the engines on their tails. The DARPA SBIR Phase II, ground test program of the GOAL proof-of-concept demonstrator 15 ft diameter fan assembly was partially completed on 25 April 2000 at Wyle Laboratories in Norco, CA. The test was a cold gas flow, mechanically driven static test of the capability of the fan rotor to achieve the necessary vertical thrust for VTOL operations. In the case of the 15 foot diameter 3900 pound high speed vertical take off and landing (HSVTOL) demonstrator vehicle, the design target is 4300 pounds vertical thrust at a rotor speed of 760 RPM. Since this was a cold gas flow mechanically driven test (in the flight vehicle, the rotor is to be driven by a hot gas flow pneumatic drive without a gear train), a T-53 gas turbine provided torque for rotor acceleration through a non-flight-article gearing system. Pre-test checkout runs to 400 RPM were completed. In the first test run, as the rotor speed accelerated past 300 RPM, the test gearing drive system from the T-53 failed catastrophically limiting further data collection. Interpretation of the limited results showed the fan that I designed performed approximately 55% better than design, achieving design thrust at 610 RPM rather than 760 RPM.

In addition to the consulting work I have been active in the Los Angeles Section of AIAA where I have set up an Enterprise Chapter which has meetings with speakers on various aspects of small business and an interactive web page where small businesses can post their expertise which can be accessed interactively. Also with

¹⁸ Denison, M. R., Lamb, J. J., Bjorndahl, W. D., Wong, E. Y., and Lohn, P. D., *Solid Rocket Exhaust in the Stratosphere: Plume Diffusion and Chemical Reactions*, AIAA 92-3399, Presented at 28th Joint Propulsion Conference and Exhibit, Nashville, TN, July 1992 (see also Journal of Spacecraft and Rockets Vol. 31, No. 3 May-June 1994)

several others we have set up AeroWebSpace LLC which hopes to do business in the aerospace field.

Conclusion

I am not anxious for a conclusion. I know this is the time for younger people to carry on the work, but I think that, based on my experience, I still have a lot to contribute in the technical world. Reality has been brought home to me through swimming. I used to be a competitive swimmer in high school and college. Now I swim laps at a local Spectrum Club where I have found that it is very hard to keep up with anyone of either sex who is well trained. Still I am better than most people.