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Oral history interview with H. Kurt Strass
[full name of interviewee]

Systems Evaluation & Dev. Div (SEDD)

about Advanced Projects and ~~Space Environment~~
[main focus of interview]

~~Simulation Laboratory (SESL)~~.

Title: 1962- Test Facilities Branch, Systems Evaluation & Dev. Div.
[interviewee's current and/or former title and affiliation]

1968 -

Interview conducted by Robert B. Merrifield - Staff
[interviewer's name/position]

Historian **at** MSC.
[location of interview]

Transcript and tape(s). [for inventory only: # pages 17; # tapes 2]

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CONTENTS:

Biographical - [date/place of birth; family background] ______

Education - ______

Career Path - Jan, 1959 - STG

Topics - Post-Mercury thinking; Project Boomerang idea for re-entry tests; Vega - Lewis Research Center project; STG's Advanced Projects Office; early lunar program ideas; circumnavigation then landing; Apollo Project Office; Tiger Booster solid launch vehicle; Nova liquid rocket; liquid vs. solid rockets; major study of relative design advantages of various rockets; Systems Evaluation & Dev. Div (SEDD) facilities; Space Environmental Simulation Laboratory (SESL); Construction at MSC; problems ~~for~~ Corps of Engineers; Chamber A pumpdown failure; Lunar Plaque; solar simulation devices; RCA contract problems; cost of SESL; water distribution system problems;

1391

September 12, 1968

Kurt,

The transcript of your interview, edited to remove extraneous material, is attached.

If you will, please read the statement and mark those sentences with brackets [ ] that you would not want alluded to in a Center history for reasons of embarrassment to an individual or the Center. As I mentioned during our recording session, this interview is to be part of the source material for the history, and it is doubtful that I will quote from it verbatim. Therefore, please don't worry about a sentence here or there which might not be as polished as would be desirable were it to receive public scrutiny.

If you want to add information feel free to do so. Just tack it on at the end of the statement, unless you prefer that it be inserted into the text.

After you return the transcript to me, I'll send you a copy for your personal file.

Thanks,

Bob Merrifield
Code BN

Bob -

An accurate transcription of our conversation & surprisingly coherent considering how it was obtained. Good Luck,

Kurt 9/27/68

INTERVIEW WITH H. KURT STRASS
May 21, 1968

By the end of 1958 all my friends seemed to have gone over to STG and I felt pretty strongly that I wanted to do something different, particularly to get into advanced planning. About November or December, 1958, I went to see Bob Gilruth and told him that I wanted to work for him on missions after Mercury. He told me there wasn't any activity in that area at that time. He couldn't talk about anything other than Mercury, but he certainly felt we should do something like this so along about the middle of December I got a phone call from Paul Purser inviting me to come join STG. I came over January 4, 1959, and started out with a desk and no particular title and my only job was to start thinking about what we were going to do after the initial Mercury series was concluded. We started off rather simply using the Mercury spacecraft concept as a basis for more advanced applications. In those days we were thinking in terms of single missions--certainly not the Apollo program as we now know it. Mercury in those days meant earth orbit and return. We looked into aspects of longer missions in a very similar approach to what we are now following in the AAP Program. The same kind of atmosphere prevailed. We planned for a sequence of individual missions which used the Mercury spacecraft as a research vehicle, and particularly for high speed re-entry testing.

The first thing Leo Chauvin and I more or less put together was called Project Boomerang. It was an Atlas-Agena with Mercury payload, and was lobbed around the world from the Cape and re-entered in the vicinity of Hawaii at re-entry speeds of 30,000 ft/sec. It was to be a full sized Mercury spacecraft and the first test was to get heat transfer data on heatshields at speeds approaching lunar re-entry speeds. That program received some interest but finally was scrubbed because it was too expensive.

About this time, it became apparent to me that this certainly wasn't any way to do serious advanced planning. We became more seriously involved with an entirely new approach with John Disher of NASA Headquarters who was then working briefly at Langley. The Lewis people had a project they called Vega--it was basically a two-man space station using the Vega rocket, which was an Atlas-Centaur combination. This two-man spacecraft was very much like the present concept of MOL. We worked on that for awhile and then John Disher went back to Headquarters and we moved on through several other exercises--drawing pictures of space stations, drawing concepts of a three-man spacecraft, etc. We finally decided that the ideal crew ~~past~~ SIZE AFTER Mercury was a three-man spacecraft. We drew configurations that resembled our present Apollo command module, and we began focusing our attention on what we called the Lunar Program. We envisioned a two-phase program: the first phase was to terminate with manned lunar circumnavigation. Following this there would be a development of a phase ~~two~~ II spacecraft which was primarily designed for lunar landing. About this time, Headquarters began to take an active interest, and in particular John Disher who worked closely with us in laying out what was called a ten-year program (APPARENTLY BASED ON HDQTRS PLANNING). Now it became possible to do our work out in the open and we formed an Advance Projects Office which I headed and consisted of three~~-four~~ other people.

Initially, this was a one-man effort, which was myself. This one-man phase lasted about a month or two into the early spring of 1959. At that point, I recruited several other people to work with me--Robert L. O'Neil from Langley, Lawrence "HAP" Anderson, and Dave ~~Grannit.~~ GRANA. The four of us constituted this advance planning office, and we were the only people in the STG at that time working on advanced projects. Now although this project office had official sanction, it was still not openly referred to as Advanced Planning Office nor Project Apollo. We worked closely with John Disher's office at Headquarters. Disher and his people were working real hard on this program and we were trying to coordinate our efforts with his [in dividing responsibility for the program]. We went through the usual design stages--including advanced Mercury's of serveral kinds--but it became obvious that this wasn't what we really wanted to do and what needed to be done was lay out a program that would commit the nation to a long-term development. We simply couldn't do any advanced planning of any consequence if we were obliged to prepare annual justification for each project because everything we could think of had a leadtime of at least three-five years. This meant it would be necessary to commit ourself to supporting something after it was started.

We began to dream of a ten-year program. This was also the same kind of thing Headquarters had in mind, and I really don't know where the idea originated. We decided what we really needed was a lunar program and not an earth orbital program. This caused a wide divergence between us and Langley, as Langley wanted an earth orbital program. Langley people were talking about inflatable space stations and frankly I couldn't see the space station, nor particularly inflatable ones. They all looked like so many blown-up innertubes. The inflatable structure technology left a lot to be desired and it was very heavy. Also nobody could give us any solid rationale as to what we were going to do in a space station once we got up there. We wanted something that would really catch the imagination of the country and would focus attention on the development and solving of problems of space. We thought it desirable to pick out a goal that was just a little beyond our reach and thus would force development. The main objective was a forced-blast development to get the space technology developed as rapidly as possible. The lunar program looked like the thing to do.

The phase one was a lunar circumnavigation in a three-man spacecraft very similar to the present Apollo configuration. At that time we conceived the idea of the command module and the propulsion module, now called the service module, and a mission module which was to be the cabin portion of the spacecraft.

About this time, the project came out in the open as far as NASA was concerned. Headquarters started to talk about it openly in public--a ten-year lunar program. It was decided to change our basic project in order to agree with the approach Headquarters was taking and instead of making lunar circumnavigation as the end point of the first phase of the operation, it was decided to go on to lunar landing--do it all in one pass. Headquarters got in the act to a greater degree, the Center put more people on the job, Bob Piland came back to the Center from Headquarters, and headed up what was called the Apollo Project Office. This was when we had the briefings for the contractors for the study. Caldwell Johnson headed a Preliminary Design Group, and many people working here now were then on that project. Will Taub was a key fixture, Rene Berglund, etc., all from Max Faget's Division. The Apollo history from then on is pretty well documented.

After the Apollo project got established and the Apollo Project Office was formed, I wasn't interested in all the paperwork and didn't want to be in a project office so I went over and became Assistant Chief of Flight Vehicle Integration Branch being formed under Dave Hammack whom I had met previously on my visits to Huntsville. The whole purpose of this operation was to look into the propulsion and launch vehicle problems to protect our own interest in the Apollo Program and particularly to ensure integrity of launch vehicle-spacecraft interfaces. We were also concerned about the escape devices, booster performance, etc., and gave serious consideration to other launch vehicles. We generated a study we called The Tiger Booster which was finished in 1961, and was the product of a joint effort of MSC and Langley Research Center. It was the first real look at the use of a big solid as a first stage launch vehicle and the report was called The Design of Reliability Analysis of a Solid Propellant First Stage with a Nova Launch Propulsion Vehicle.

At this time in the program--between the period of 1959 and 60, we were largely concerned with a direct landing on the moon. We did not like the idea of orbiting around the moon, use of a LM to the lunar surface, and later rendezvous with the spacecraft. We didn't want to develop another spacecraft. But our approach which would cut out the need for another spacecraft, required a bigger booster, and the Saturn V at that time appeared too small. The Nova, then appeared to be the most promising concept for a launch vehicle. The Nova, however, was a frightening big liquid thing and we were considering using a big solid. Anyway we did a rather advanced study for those days considering the relative design advantages of the Saturn, the big solids, and the Nova using a solid as a first stage. We originated some concepts of how to build the solid, discussed them with companies like Newport News Ship Building and Dry Dock Company and Letournean Corporation, and investigated means of handling and barging it to the site, erecting it and checking it out. Our team was headed by Dave Hammack and it involved Guy Thibodaux's people from Langley and people from PARD. The motor design aspects were summarized in our report by Robert L. Swain, the Control Dynamics Analysis by Homer G. Morgan, the Structural Dynamics Analysis of the vehicle was by William C. Walton, Jr., Materials requirements and testing by Edward M. Gregory, the Stress Analysis by Les St.Leger, the development schedule, handling and transportation by myself, the Trajectory Analysis by Robert L. O'Neil, Failure Modes and Reliability Analysis by Lawrence Anderson, Jr., and Design Drawings by Joseph Pryor, Jr. Our group put together this rather lengthy study, worked very hard on it, and I think we did a good job. It went pretty far--finally to the so-called Golovan Committee, under Dr. Nicholas Golovan. In the process we got into several tangles with the Air Force over the concepts. The Air Force was pushing segmented rockets, while we favored the big monolithic cast-in-place rockets, but both of us were strongly in favor of big solids. Unfortunately the liquid propellant school won out. The big Saturn was designed the way it is now and that's the way we have gone. The study by the way is still a good study and still applicable and the same material is being looked at today by companies like Chrysler and Aerojet. Not a heck of a lot new has been added in the last five-six years.

About this time we moved to Houston. I came to arrange office space for our people and it was obvious after we arrived that our propulsion oriented branch wasn't going to last. I don't think MSC really wanted us to become competent in the design of boosters or in integration. This was considered a Marshall prerogative. The prevailing attitude was that we would take all our advice from Marshall. Eventually our group got out of the launch vehicle business entirely, but it kept looking at advanced spacecraft problems.

(The portion of the interview bracketed between asterisks has been reconstituted from notes as this section of the recording was garbled.)

* * * * *

It was evident that advanced planning wasn't going anywhere. At this time, SEDD was being organized, and since Hammack's area was becoming sterile, I decided to go with SEDD and participate in the design, construction and checkout of test facilities, expecting that planning could be as challenging for facilities as hardware. In SEDD I became Chief of the Test Facilities Branch with responsibility for the centrifuge, SESL, thermochemical test facility, and the structures lab.

The general configuration, size and capacity of these facilities were already in being, having been established in 1961 by a group under the guidance of Bond and Kotanchik. Strass came just as criteria ~~was~~ WERE being formalized. The centrifuge was an outgrowth of the Johnsville test facility of the Navy. The structures lab was a bigger and better version of a similar unit at Langley Research Center, as was also the thermochemical test facility. The only facility that was not like something somewhere else was SESL.

Ultimately, the functional responsibility for two of these test facilities was transferred to other E&D divisions: the centrifuge to Crew Systems Division and the Thermochemical Test Area to the Propulsion and Power Division. The structures lab and SESL remained under SMD.

As the major technical development of this generation, SESL was a personal challenge in facilities design. It was the largest and should
236 be the best. There were many problems, and the first of these was the fact that everyone participating in the design was inexperienced in this type of technical facility. Thermo-vacuum conditions had not been reproduced except under small scale test conditions. High vacuum technology was in its infancy; 10^{-6} to $^{-8}$ torrs was near the upper limit in the state-of-the-art. Only a handful of people had experience in any type of comparable work. One of these was Rich Piotrowski, who had worked at Guardite ~~(?)~~ Corp. on small vacuum chambers and pumps. Piotrowski carried the major burden of responsibility for the early work on the SESL, putting out the specifications and in selecting the Bechtel Corporation of San Francisco as the engineering/design firm.

About this time, the Center was informed that the Corps of Engineers would supervise the actual construction. MSC would be the customer and technical contract manager. Although the Corps was able to assign people who were competent in brick and mortar construction, they were completely at sea when it came to contributing anything useful in the design and con-
240 struction of a vacuum chamber. Bechtel in general did a good design job--although there were several notable failures--and the design phase was completed on schedule. The construction contract was awarded to a combine of three firms, Chicago Bridge and Iron (steel work), Ets Hokin and Galvan (bricks

and mortar) and Industrial, Fisher and Diversified (mechanical systems). The size of the facility was only one of many problems. It was pushing the state-of-the-art, the amount of funding that had been allocated was extremely modest for such an undertaking, and the schedule was very tight. Bechtel overestimated the cost, so we had to cut the size of the facility. One of the four chambers was cut out and the others reduced in size.

Despite all the difficulties (and the creation of the Facilities Division as an administrative organization to guide the Corps was a miserable arrangement), the construction moved along fairly smoothly and on schedule. Of the organizations participating, only the Structures and Mechanics Division had a reservoir of knowledgeable personnel. Both the Facilities Division under Zbanek and the Corps were small organizations with Center-wide construction responsibilities. The Facilities Division had the authority to deal with the Corps, but had no real capability in the design of such a technical facility and was dependent on the SMD for guidance. The Corps had no previous experience in such construction and also was not fully manned. The Corps had almost a veto power over technical decisions, and channels of authority were ill-defined. It was a hard way to do a job.

The most outstanding event in the construction of SESL was the failure of chamber A to pass the initial pumpdown test. Crumpling took place in the vicinity of the door, which proved to be a design deficiency on the part of CBI--it was simply under-designed. To correct this deficiency took almost a year and an additional three to four million dollars.

* * * * *

The Lunar Plane was another major design problem of the facility and

it was a headache throughout the whole design construction phase. It's a very complex piece of apparatus--very large, has very stiff operating requirements, was designed to carry a 150,000 pound spacecraft very smoothly, and be capable of being used and cooled at the same time. It has a rotating shaft vacuum seal and liquid nitrogen must be pumped up the shaft while it is rotating. We had major problems in two areas. The
240 vacuum seal which was designed by the contractor after much testing had to be redesigned. Hargraves' people in Facilities, and a few of our guys in SESL came up with a new concept and successfully designed, built, and tested the shaft seal and the liquid nitrogen joints. They were successful. If this device hadn't worked, the whole chamber would have been inoperative. The second problem, and single biggest problem outside the chamber failure during the pumpdown, was the operation of the solar simulation devices.

Solar simulation was then and still is the biggest single problem in operating the thermal vacuum facility. Not only is it necessary to
240 have a vacuum but the thermal characteristics of outer space must be simulated, especially solar radiation. In order to do this it is necessary to simulate the intensity and the spectral characteristics of the sun. It is also necessary to simulate the degree of collimation of the sun. The LIGHT FROM THE ~~earth and~~ sun ~~are~~ IS almost parallel and ordinary searchlights or illumination devices have rather diffused radiation, are not collimated, and do not cast sharp shadows as the sun does out in space. The device which simulates the spectrum characteristics of the sun, with the same energy distribution, the same degree of parallelism or collimation is extremely sophisticated. We had about four million set aside in our original budget

for procurement of solar simulation devices. Many companies had attempted to build solar simulation devices but all failed. The most notable failure was by Bausch and Lomb who were on a contract to build a solar simulation for the Jet Propulsion Lab in their California facility. They failed miserably and that portion of the company finally went out of business. The job was ultimately taken over by JPL people in-house, and they got it to work after about three-four years. Minneapolis Honeywell next got in the act and attempted to build a solar simulator for Goddard Space Flight Center. After many failures they finally got an operating device which was satisfactory but again a massive infusion of money and effort on the part of Goddard was necessary. The solar simulation device at Goddard has never really worked well, but the JPL device has. The history of both of these devices has been characterized by over-spending on the order of several magnitudes. Development time in excess of four years was required. We came in the picture when the technology for solar simulation ~~assembly~~ had not been developed. It is not a particularly difficult technical problem, but it is a difficult engineering problem; we were asking for not a laboratory device, but a device that was very large and capable of illuminating a whole sapcecraft. It isn't possible to build very large vacuum chambers or a big vacuum solar simulation device for a vaccum chamber the same way one could a lab device. One might spend $100,000 for a solar simulation device but would expect to illuminate a specimen only six inches in diameter. We were trying to cover a spacecraft 13 feet in diameter, and 33' x 13' on the side dimension. A small scale solar simulation device might cost on the order of one million per square foot of illuminated surface. Obviously we could not afford this kind of costs for a big chamber. We not only wanted an acceptable solar simulator but we wanted solar simulation in mass quantities.

RCA Corporation won the competitive bid for this solar simulation device. Minneapolis Honeywell who had built the Goddard unit made a bid as did the Space Technology Laboratory and one or two others. RCA won the contract on a bid of a little under four million dollars. The design was based upon the carbon arc principle, which at the time was the most promising candidate and until the last year was the only device capable of producing enough energy for a large chamber. RCA really had no previous experience in solar system technology although they maintained they had. They had produced a few lab units which worked all right, but a lab unit ~~is a much~~ HAS BASICALLY different requirements from a big operation, e.g., The reliability can be lower but still adequate.

RCA had an enormous amount of difficulty in all phases--design, building and testing of their device. We had two men who supervised this contract. One was Franklin Williams who had been the design engineer on Honeywell's Goddard project. He knew about the problems of solar simulation from the ground up. The other was Arthur Johnson, who came to us from STL. He also had experience in solar simulation with STL. With these two, we had two of the most competent people in the solar simulation area. They guided and pushed RCA to the point where RCA finally produced a workable unit.

The RCA's Service Division got this contract and that was part of the problem. The RCA Service Division is not the manufacturing branch of RCA but it is in the business of contracting people and for performing services. They contract operating crews, things like Cape Operations, design sources, etc., that fall in the general category of flesh peddling. They didn't have experienced designers, they didn't have manufacturing capability and RCA wouldn't give them support from other corporate areas. It is often more difficult to get technical assistance within your company than it is to get it from a competitor. RCA's troubles with the contract were largely managerial. They followed the usual pattern which I have run across several times--they don't spend enough effort and time in the early phases of the contract. They try to cut the corners too close to start with and as they get into the contract they suddenly become aware that they have problems. Instead of spending money early to prove out the capability, budget the optimum method of doing things and get a lot of test data behind them, they waited until late in the contract and then suddenly had to have a mass infusion of money to buy time. They were well over the schedule, in fact they never met any schedule, and even with the one year dealy in the building and checkout of Chamber A they were still behind schedule. Their units never met the specifications and ultimately we had to take a compromise in specs. In part, the spec problem was due to the fact that we were pushing the state-of-the-art.

We finally got the units installed in time to meet the first test of the Apollo spacecraft. They did function although they were almost unsatisfactory. This was largely due to another incident which occurred during the checkout of the facility. During the initial pumpdown of the Chamber following its reconstruction, there was a massive failure of the water cooling and distribution system and this resulted in extensive damage to the solar simulators. The mirrors of the solar simulators were cooled by water lines. The chambers were liquid nitrogen cooled, and as a consequence we had to make certain that water kept circulating through these mirrors at all times in order to prevent it from freezing. With the plumbing stoppage, the water lines froze up, every one of the mirrors on ~~one~~ the side of Chamber A were damaged to some extent, and most damaged beyond repair. The initial operation of the Chamber was handicapped by this massive mirror failure and there was no time to get new mirrors delivered. We did the best we could with what we had available and got through the first test program with the Apollo S/C in this manner. There was some litigation with RCA, and some name calling over who was responsible for this accident. The best way to summarize the Government's position is to note that we never reimbursed RCA for this accident. Art Johnson had such an outstanding technical file on the RCA contract, that in each instance of dispute between RCA and the Government, it was reconciled in the favor of the Government because we had the facts and figures to show that we not only pointed these problems out a year or earlier, we also made suggestions for improvement that RCA did not follow. I give Art Johnson the credit for not only getting what we had but also preventing the Government from undergoing large counterclaims. RCA met its obligation so far as I am concerned, and made a massive attempt to meet the terms of their contract. While the problem was beyond their capability and certainly just plain difficult, I think RCA showed good intentions in that they put a lot of effort and a lot of their own money into the contract.

After that, the Service Division of RCA went out of business following the previous history of Bausch and Lomb and Honeywell--all three of these companies went out of the solar simulation business. They were the big three in the solar simulating business to start with. None of them are now in the business, which testifies to the difficulty of the problem. Bausch and Lomb was probably the outstanding optical company in the U.S., Minneapolis Honeywell was probably one of the outstanding systems people and RCA not only had supposedly the resources of a major company, it also had the support of ~~owned the concern~~ (American Optical) Co. that designed the optics for the system.

Subsequently we were handicapped by not having enough money to really build a good solar simulation system. There was an in-house effort in conjunction with the Air Force at Tullahoma, where a space environment simulation facility was being developed. We initiated a cooperative program where we pooled the little money we had for the development and investigation of the problems associated with solar simulation and the joint effort subsequently resulted in some major improvements to our system. We now have a very effective, workable system installed in the chambers. The lesson to be learned here is that technical developments, particularly where in building large units and high reliability is required, it's not very wise to think things are going to work well the first time they are turned on. There is an ~~e~~inevitible learning curve that affects not only the manufacturer, the design, but also the operator. In the design of the solar simulation system, it has taken six years of intensive effort, only now do we have a workable system. It generally takes four years to build a major facility and iron out the bugs. Unfortunately facilities aren't built ahead of the requirement. They are built after the requirements are established, and it is almost always in the critical path of new development. There's no easy street. Just lots of hard work and recognizing the fact that there will be lots of problems that can't be solved by decree.

Most people don't realize that the SESL grew like Topsy. It suffer-
ed from cumbersome management, from limited funds, and from a tight sche-
dule. These problems were exacerbated by the inadvertent structural fail-
ure of the chamber, which really put a kink in our activities. It not
240 only handicapped the main construction of the laboratory and made the costs
go way up, but also it interfered with the installation of the solar simu-
lation. The problem of getting a major facility operational is something
that is largely subject to a great deal of naive opinion. The Lab now
represents an investment to the Government of 70 million I would guess, if
326 you count all the equipment in the SESL. The actual construction of the
hardware probably cost $30-35 million. The major investment is not in
the vacuum chamber, nor in the solar simulation system--these represent
a small part of the total. The major investment is in electronic equipment--
in the data acquisition system and the ACE system. Out of the total ex-
penditure, these account for close to $20 million.

One of the major problems in the SESL, and one which people don't
usually think about is the water distribution system. For some strange
reason it was decided that it was more cost effective to use black iron
pipe as opposed to corrosion proof plumbing such as galvanized plumbing,
246 copper, or stainless steel. The black iron plumbing, which is still in-
stalled there, rusts out faster than it can be replaced. This was a con-
tributing factor in the previously mentioned problem with RCA. In that
failure we lost approximately 50 mirrors--almost the total side sun was
wipped out when they froze up. RCA maintained that the water lines plugged

up from the rust carried through the system. This was probably a contributing factor but not the real cause. We had considerable filtration in the lines, and had gone to great expense to cut down on corrosion by adding chemicals to the cooling tower. Now the solar simulation system at Chambers A and B are closed loop systems. They are completely independent of the major water distribution system. Why anybody in his right mind would permit the installation of common black iron plumbing in that laboratory is beyond me. The water is this area is loaded with chemicals and very corrosive. We have saved an insignificant amount of money in our choice of black iron plumbing. I never have understood the rationale. We talk about saving a few dollars on the pipe but we spent far more than that later for the chemicals to purify the water and keep it clean. To my knowledge all the plumbing in SESL will ultimately have to be replaced with corrosion proof plumbing. We always had water problems and we always have had vast amounts of rust accumulate in the pipes. It plugged up pumps, it plugged up filters, it caused a great deal of embarrassment with the contractor, RCA, in operating the solar simulation device, and only by going to the great extra expense of putting a separate closed loop cooling system did we ever get away from this problem.