Dr. James Van Allen

18 November 1997

Brian Shoemaker

Interviewer

(Begin Tape 1 – Side A)

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BS: *This is an oral history interview with Dr. James Van Allen in his office at Iowa City, Iowa. And it's the 18th of November, 1997. Dr. Van Allen.*

JV: Good morning. What do you have in mind today?

BS: *Well, this is your interview, Dr. Van Allen. It's an oral history for the American Polar Society. We're interested in things that have driven science and exploration of the Polar Regions and one of the things that certainly drove science in the polar regions was a meeting of people at your house in 1950, when you decided upon a program to begin the third Polar Year which eventually led to the International Geophysical Year. So, where did you get, how did your interests in the polar region get started?*

JV: Well, Captain, the meeting, I checked the date April the 5th, 1950 was in Silver Spring, Maryland, where the idea of the International Geophysical Year first crystallized under the discussion of the evening we had there in my home. I'd been interested, of course, in polar exploration, for many years previous to that. But let me first make it clear that I do not consider myself a polar explorer in the traditional sense. I have been on two expeditions into the Arctic and one into the Antarctic, all on shipboard. Not on the continent of Antarctica itself, but on shipboard. I was on shipboard to the Arctic on both occasions, so I have some personal experience in both regions, but not at the level that would really entitle me to be considered an explorer in the traditional sense. But I have done exploratory work in both polar caps of the Earth and have been a keen advocate and follower of the scientific work, especially that in the Antarctic, in recent years.

As a young student in Iowa Wesleyan College in Mt. Pleasant, which is my hometown – Mt. Pleasant, Iowa – I fell into an extraordinarily good fortune to have a professor there named Thomas C. Poulter who was an extraordinary individual. And he was THE professor of physics at the Iowa Wesleyan College at that time.

BS: *What year was that?*

JV: Well, I started as a freshman at Wesleyan in September 1931 and I was already very interested in science and had done many simple experiments in electricity and mechanics and radio and so forth.

BS: *So that was before Tom went to Antarctica*.

JV: Yes, that's correct. And Tom was a son – one of the three sons and I think one daughter in the Poulter family there in Mt. Pleasant, Iowa. Tom's father was a mechanical genius. He devised and built all sorts of heavy equipment, most importantly a successful ditch-digging machine. He constructed a large plant in Mt. Pleasant and manufactured ditch-digging machines there. Tom was one of three sons, all three of whom took after their father. Tom Poulter himself was one of the most ingenuous and creative and inventive persons I've ever known in my life either before or since. He really stands out in my mind. A truly extraordinary person. His brother John later became the chief engineer of the Koehring Company, I think it was, in Milwaukee, which was a huge company, devoted to building earth-moving equipment of one kind or another. His other brother, Bill, became a Ph.D. biologist and taught biology for many years at Iowa Wesleyan College. They were three extraordinary individuals.
(50)

Tom took a liking toward me. I took his physics course right away when I entered as a first year freshman. I loved the course. He taught in a beautifully clear way. And you felt you were really getting the authoritative story from Tom. He wasn't just kidding around. And I did well in the course. I loved it and did well in the laboratory. And by the second semester, Tom asked me if I'd be a lab assistant. So I then took on the job of setting up the experiments in the lab. In the following summer, Tom said he'd like to have me work in his research lab. I was delighted to do that. He was, at the time, beginning to develop plans for joining Admiral Byrd on an Antarctic expedition which, I think, was originally scheduled to leave in 1932, but actually left in October 1933. He has been associated with the planning the scientific work for this expedition. He had been appointed Chief Scientist and second in command for this Byrd Expedition which was the second Byrd expedition to the Antarctic.

And so in the summer of ’32 – I had just finished my freshman year – Poulter had borrowed on an indefinite loan a magnetometer from the Department of Terrestrial Magnetism a the Carnegie Institute of Washington. This was one of the most beautiful pieces of equipment I’ve ever seen in my life. He gave me the job of taking it out in the field and checking it out and doing observations on the magnetic field of the Earth in Mt. Pleasant and the Henry County area. Also, part of the instrument was a theodolite with which we could observe the sun and Polaris and determine the latitude and longitude of the place where we were, so I learned how to do all of that. I had a good instruction manual and I studied that very hard and I was soon pretty good at doing the navigational fixes as well as the magnetic observations. I did a series of those observations during the summer – very illuminating to me. For the first time I recognized that there was such a science as geophysics as contrasted to what we sometimes called “indoor physics,” or laboratory physics. And that was my introduction.

Also, later that summer, I helped Poulter with some observations of the Perseid meteor shower. Using devices that he had developed - simple triangulation devices - we did observations over a 50 mile baseline of the height of appearance and disappearance of meteor trails in the atmosphere. That was the Perseid shower of August 1932. The magnetic observations and the Perseid observations were the first original scientific work that I had ever done.

BS: *So you did that with Tom.*

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JV: Yes. Yes. Those two projects helped launch me on my professional career. So I was very devoted to Poulter and then I worked with him during the subsequent year. I helped build a tilt meter which was a sensitive device for measuring the tilting of an ice cap. It was later used in the Antarctic. Meteoric observations were also carried on in the Antarctic as well as the magnetometer ones. So everything I did that year or two had a consequence in the scientific work in the Antarctic. And then, of course, after Poulter left, in late summer or early autumn of ’33...

BS: *We know.......*

JV: Anyway, I followed the expedition very assiduously and about once a week there was a radio broadcast from Little America after they got there and got established. There was a man by the name of Harry [Vonzell] who was an announcer at a radio station in Chicago, I think it was. On Sunday night or some other standard time of the week, they would establish communications with Admiral Byrd at Little America and he would describe what was going on and how things were going. It started. . . something like . . . "This is Little America . . ." It was a great thrill for me because I knew somebody who was there. And so that continued for at least a year or so. . . perhaps more. Another highlight of that experience for me was the fact that Arthur Collins, who was later a friend of mine, had developed the short-wave radio equipment for Byrd and was the founder of the Collins Radio Company in Cedar Rapids, now the monstrous Rockwell-Collins Plant there. He was an extremely talented radio engineer – a self-made engineer. He had developed the radio equipment, short-wave equipment, which was used for those transmissions. A transmitter and receiver were in Antarctica and he had another set in, I think, Chicago or maybe it was Cedar Rapids. That was a revolutionary development in radio communications and global radio communications at that time.

So, I followed those transmissions with great interest. One of the most tense periods was when Byrd had gone off to an inland station as an experiment in personal psychology under isolated conditions. I think that would be the way to describe it. He'd decided to go off and live by himself in a small hut. He was to be at this little station for a prolonged period to see how he reacted to life in isolation. Things went pretty well for awhile. But then his transmissions got weaker and weaker. That is, Admiral Byrd's speech on the radio got to be more and more feeble. Poulter was very alert to that and he led a rescue party which went to the advanced station in August 1934, in the midst of the Antarctic winter, over 90 miles from Little America and found Byrd in an emaciated condition, which they diagnosed as being carbon monoxide poisoning – slow carbon monoxide poisoning from a kerosene stove or something of that nature there in very confined conditions. And so Poulter rescued Byrd from almost certain death otherwise, if he had not been alert and hadn't taken the initiative to travel over the ice cap under very hazardous conditions.

BS: *100 miles south of Little America*.

JV: Yes, yes, it was a long trek, to say the least. Poulter had enormous physical energy and courage as well as intelligence. A really extraordinary person.

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Poulter and the other members of the expedition returned to the United States in May 1935. I was then a senior in college and was scheduled to graduate in early June. Both Poulter, who was a local hero in our area, and Byrd, who was a national hero, were invited to our commencement. Both accepted. So, in June of '35, the little town of Mt. Pleasant staged a huge parade. Byrd and Poulter were honored together as returning explorers. They rode in a big old Cadillac convertible in the parade. Both received honorary degrees at the Wesleyan commencement.

BS: *Sat up on the back of the convertible*.

JV: Sat up on the back and greeted the bystanders. Byrd was a very impressive figure, of course, in his sparkling white uniform – full dress uniform. Very impressive national hero. He gave the commencement address, I believe.

I then went on to graduate school at the University of Iowa and studied more or less conventional physics and completed a Ph.D. in nuclear physics in 1939. I then went to the Carnegie Institution of Washington as the Research Fellow in nuclear physics, although I still had a strong interest in geophysics or what I like to call outdoor physics. At the Department of Terrestrial Magnetism of the Carnegie Institution I fell in with a number of very distinguished persons – Scott Forbush and Harry Vestive and several others there who were geophysicists in the full professional sense. And though I worked in nuclear physics, I associated more with the geophysical people there. Then, in 1941, with World War II already underway in Europe and threatening the U.S., I converted to what we then called war work, namely, helping to develop a device for naval anti-aircraft guns called the radio proximity fuze. This was a device that, if successful, would detonate a projectile in the immediate vicinity of an enemy aircraft irrespective of timing errors. Our fuze was for the large 5”/38 guns. The Navy also had short range guns 40 mm and 20 mm guns, which had a high rate of fire and depended entirely on impact fuzes.

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You had to hit something fairly solid to make it go off. The long range anti-aircraft guns the 5 inch 38 and also the 3 inch 50 guns had time fuzes which were set in a fuze pot alongside the breach of the gun. The time would be set by computer from down below and the gunners mate would lift the 64 pound 5”/38 projectile and drop it in the breach of the gun, close the breach and then fire it. During that process there is always uncertainty in the handling time, setting time, etc. not to mention basic tracking and computer errors. The fact of the matter was that the 5 inch 38s and the 3 inch 50s almost never hit an aircraft because of range error, also partly because of pointing error, but mostly the range error which was typically on the order of 500 feet – maybe 1000 feet – corresponding a fraction of a second. And so for the most part, you made a lot of air bursts, but you didn’t hit anything. So that was our problem, trying to eliminate the range errors. You’d still have the pointing errors, of course, but if a proximity fuzed projectile passed an aircraft within 50 – 75 feet, we could get it to burst automatically.

So I did a lot of the basic research on that – the development of that fuze. And I was one of the co-patentees of the so-called rugged vacuum tube which is the heart of the fuze. This was long before the days of the transistors, of course. I developed one of the basic features of the vacuum tube which survived firing from a naval gun at about 20,000 g. It’s a real achievement, I think, to have a vacuum tube survive that.

The first group of these fuzes to have a reasonable level of operating reliability was produced in the summer of 1942. And they were then in the small scale production

in several plants and the responsible officers of BuOrd decided that a reliability exceeding 50% was adequate for initial service use. That sounds pretty poor, but in that day, it was considered adequate. We had so much at stake, it was probably a good decision. I never made the decision. I was well acquainted with it, but it was the officers of BuOrd who made the decision. I think it was a good decision. Three of us who were young scientists/engineers at the Applied Physics Lab of Johns Hopkins, where the work was being done, were commissioned as naval officers in November 1942, to go out to the fleet and accompany an initial shipment of these proximity fuzed projectiles to the Pacific Fleet, to confer with the captains and gunnery officers on the various combatant ships in the area, and to issue, or parcel out, the projectiles to ships most likely to be engaged in anti-aircraft combat in the near future. And then, to go along with them on the ships to monitor their operation and to advise the gunnery officers on the techniques and expectations for them. I was one of the three that did that. We were all commissioned as Lieutenants, jg. and were given spot commissions. Our total instruction in how to become naval officers consisted of two pamphlets they gave us to read while we were en route.
(250)

BS: *The things on your collar right?*

JV: Those are always . . . I'm just rambling on here.

BS: *You're fine, you're fine. We're getting your background here and that's kind of important leading up to what you did*.

JV: The three of us went out to the South Pacific Fleet under the supervision of a man named W.S. (Deke) Parsons who was at that time a commander – later a rear admiral and later the weaponeer on the *Enola Gay* which dropped the first atomic bomb in Japan. He was a great leader. He went out ahead by air. And the three of us went out on a troop ship, with about 6000 of the proximity fuzed 5 inch 38 ammunition which was very highly classified. We went non-stop from Mare Island, California to Noumea in New Caledonia. Noumea was then the headquarters of COMSOPAC, it was called – Commander of the South Pacific Fleet. That was our working headquarters. And so to make a long story – shorten it up a bit – the three of us fanned out and I was responsible for destroyers and one of the other two was responsible for cruisers and the third for battleships. We didn't furnish any ammunition to the carriers. I had duty on a number of different destroyers for several months. My two comrades went back to the States fairly early on, but I thought I really had so much more to do, that I asked to have my duty extended. So I stayed on for a total of about 8 months. Admiral Willis A. Lee was the Commander Battleships Pacific. Also he was Task Group Commander within Task Force 38 or 58 depending on who the Task Force Commander was. He asked me to join his staff. Admiral Lee was a gunnery expert and he was very interested in these new fuzes. He asked me to be on his staff temporarily and he made me an assistant staff gunnery officer. And I was on his flagship, the *USS Washington*, for a fairly prolonged period at sea during that time. Lee was one of the most intelligent and interesting persons I've ever encountered in the Navy.

We had lots of problems with the fuzes. Just the mere fact that only about 50% of them worked was enough of a problem already to undermine gunnery officers' confidence in them. I always had to explain that despite this failure rate the proximity fuze was still five times as effective as a time fuze. That's kind of a subtle concept and it's not so clear that you aren't giving them a snow job and it's a difficult thing to establish actually. We didn't really know for sure anyway. But it was a reasonable estimate. An additional problem was the useful life of the batteries. I disassembled samples of the fuzes and diagnosed the problem. The problem was the dry batteries which had a limited lifetime under the hot conditions on ships in the tropics.

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Often the shipments of the fuzes took several months to get to the combatant ships in the holds of cargo ships at 110, 125 degrees. By the time they got there, some of the batteries were dead. So I then went back to the States as a kind of missionary to get something done about this. I took on the personal responsibility to bring this to the attention of the people who were at the supply end and to get something done about it for the sake of the Fleet. I was making a nuisance of myself around the Washington D.C. area, but I did persuade the Bureau of Ordnance to (a) send me back out, (b) supply at least for awhile, fresh batteries by air shipment and (c) authorize me to set up re-batterying depots at various advance stations. We set up one at Noumea, one in Tulagi, one in Espirito Santo, and one on Manus Island. Also on some ammunition barges in Eniwetok, and later in Brisbane, Australia. We set up these re-batterying depots where we could take projectiles off of, say, a destroyer, exchange them for ones with fresh batteries, and then re-battery the ones we took off. And so I established that operation on a wide scale in the Pacific. I had priority orders. I kept in touch with COMSOPAC and went wherever I was needed. My basic orders were called TAD for Temporary Additional Duty. And I could get priority on any military flight. I went all over the Pacific by military aircraft. I also rode on ships if more convenient. One ship I rode on was a YP boat from Guadacanal to Noumea during which we were attacked by a Japanese aircraft en route. We were just slogging along at about 10 knots – the YP boats were former tuna fishing boats out of San Diego which were commissioned as what the Navy called reefer ships. They were floating refrigerators. And we ate well on those ships. The Japanese plane dropped several bombs near us but never hit us. During the Battle of the Phillipine Sea, I was back on the battleship *Washington*, at Admiral Lee's request. In helmet and life jacket, I stood right beside him elbow-to-elbow on the bridge during a major air attack. Fortunately the *Washington* was not hit, although we had been singled out for special attention. We had at least two or three low level torpedo bombers attack us and one that was intended to be a dive bomber attack. And we shot them all down. I was never 100% sure if the proximity fuze did it because every anti-aircraft gun on the ship was firing. All the 40s and 20s were firing. The air was full or bursts from a mixture of time fuzes and proximity fuzes, so I never could swear on a stack of bibles that our fuzes really did the job but I thought they probably made a significant contribution.

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I came back to the States in December of ’44. I was assigned to the Bureau of Ordnance, as a liaison officer to the Applied Physics Lab to try to get some further improvements in the fuze. It was distressing to me how detached from the problems in the Fleet the manufacturers were by then. They were cranking out the fuzes and never knew what happened to them and it seemed that I was the only one who knew what happened to the fuzes and how they we really working out in the Fleet. No one else in the Bureau of Ordnance shared my specific experience in the Fleet. I did my best to encourage improvements in the fuze. So that was pretty much my Naval background. I was placed on inactive duty in May of ’46 as a lieutenant commander, with three battle stars on my Pacific ribbon.

Then I went back to the Applied Physics Lab as a civilian. I had earned my spurs around there, and I was authorized to build up a high altitude research group using German V-2 rockets which the Army had captured and brought back to the States in late ’45. I built up a group doing scientific research with high altitude rockets. Originally the V2s and then with the American Aerobees whose development I oversaw. The latter were unguided vertically fired sounding rockets.

BS: *You oversaw the development of those?*

JV: I oversaw their development. I was the guru of the Aerobee, yes. We started it in the autumn of '46 and had the first firing in '47. I oversaw the whole development including putting them into operation at the White Sands Proving Ground in New Mexico and having many bruising fights with a Colonel Turner, who was the Commander of White Sands Proving Ground at that time. He was professionally friendly, but he really gave me a hard time on launching those unguided missiles at White Sands. Colonel Harold A. Turner was a crusty guy but I had a lot of respect for him. At the same time, I could never get through to him in some respects. I recall having a battle with him to get permission to conduct the first firing of an Aerobee at White Sands. The Aerobees were built by Douglas Aircraft Company and the Aerojet Company. The Aerobee and its successive upgrades continued to be the work horse of high altitude research for many years thereafter. A total of 1049 Aerobees had been flown up until their retirement in 1988.

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We used a military Nike booster for the first stage. We had a tough fight with Colonel Turner on whether we could launch them from White Sands. One of his famous statements to me was, "You're not going to launch that blankety-blank unguided missile from MY range." He was fearful of its falling out of range. So he set us back quite a bit. But he was right. So I got together with a man by the name of Bob Kent who was the chief ballistrician of the Army Ordnance Department based in Aberdeen. And Kent was a great guy and I got on with him very well. I had worked with him during the War. He helped me work out what we call windage tables. What we did was to release a free balloon and track it with a theodolite in order to measure the wind velocity. We then fed that information into a table so that we could tilt the launching tower by the proper amount to keep the impact point within the government range. The Aerobee has no active guidance whatever. Hence, it tended to weather cock into the relative wind. So if the wind was blowing from, say, the north, then the rocket would tend to line up with relative wind and fly off to the north. The worst thing was to fly east and west out of the range and that was what Turner had a responsible concern for and a very proper one, actually.

So I worked with Kent on these tables and we built a 140 foot high tower in White Sands for launching Aerobees. It was a tiltable tower. It had three legs with a jack screw under one leg and a hinged support for the other two legs. We tilted the tower in accordance with the calculations from the windage tables. Colonel Turner finally approved this arrangement. The scheme worked very well and we kept all of our rockets inside the range despite the fact they went up to an altitude of 100 miles. We seldom had more than a couple of miles deviation from the calculated impact point. I've always been amused to recall that one of the guided V2 rockets which Colonel Turner was launching went awry about two weeks after our conversation and fell into a cemetery in Juarez, threatening an international protest. But when the Army recovery team got over there, a group of young Mexicans was selling souvenirs from the thing, and as far as I know, it never resulted in an international incident or embarrassment but I did mention to him later that one of his guided missiles, I heard, had gone outside the range.

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I continued research on cosmic rays, atmospheric ozone and high altitude photography of the Earth by flights from White Sands for the Applied Physics Lab. I also led two shipboard expeditions using Aerobee rockets. The first expedition was to the equator on the *USS Norton Sound*. We put the *Norton Sound* into commission as a rocket launching vessel. We went to the equator off the coast of Peru in March 1949 and did some very nice cosmic ray measurements and measurements of the magnetic field in the upper atmosphere. We did similar measurements with Aerobees launched from the *Norton Sound* in the Gulf of Alaska in January 1950.

In the spring of 1950, a man by the name of Sydney Chapman, a distinguished British geophysicist, was visiting the United States and he heard about our work and he was very interested in our rocket measurements. He came to the Applied Physics Lab one day for a visit and I went over all of our work with him. I then suggested, “Sydney, why don’t we get together with some of your colleagues in the Washington area?” He said that would be great. So my wife and I arranged a dinner party at our home in Silver Spring, Maryland. Chapman was particularly eager to meet with Lloyd Berkner and Wallace Joyce. The dinner was on the 5th of April 1950. I have a picture of our house around here somewhere. It was a modest little house in Silver Spring, Maryland at 1104 Meurilee Lane.

BS: *Was Alan Shapley there?*

JV: Sir?

BS: *Alan Shapley?*

JV: No.

BS: *He wound up being Deputy for IGY*.

JV: I know him quite well, but he was not present at that dinner.

BS: *Lloyd was*.

(500)

JV: Lloyd was, Berkner, Lloyd Berkner. Also Wallace Joyce, who I think was in the National Research Council at that time – a very bright and distinguished geophysicist. Others were Fred Singer, who was one of my assistants at the Applied Physics Lab, E. H. ("Harry") Vestive of the Carnegie Institution, a former student of Chapman's, my wife Abigail and myself.

So we had a very good dinner which my wife prepared and then she hustled our then two daughters off to bed while the men gathered in the small living room and shared some very good conversation. For dessert, my wife had prepared a multi-layered chocolate cake which was made of about ten thin pieces of cake-like batter, each one of which was frosted with chocolate frosting. It made a fantastic dessert which she served that evening. I thought that her dinner was one of the important bases for planning the IGY.

During the course of the evening Berkner said, “Well, Sydney, don’t you think it’s about time for another polar year, say on the 25th anniversary of the Second International Polar Year, 1932-33?”

BS: *Yes. Second Byrd Expedition*.

JV: That was about a year late.

BS: *Yes, 1933*.

JV: ’33, OK.

BS: *Byrd jockeyed his time around to fit in there.*

JV: OK.

BS: *Good for publicity. Good for fund raising*.

JV: Yes. He was good at that. The first International Polar Year was in 1882-83, I believe.

BS: *'82*

JV: '82, '83.

BS: *The Greeley expedition where he was left stranded up on Ellesmere Island. And then they had another group go to Point Barrow under Captain Ray.*

(550)

JV: Yes.

BS: *Army Captain Ray. The building's still there. He didn't have any problem. They put him up to do the job and they bought him out without fanfare.*

JV: . . .OK, I can check all that. That was the first International Polar Year and the emphasis, obviously, was on the Arctic and on what we now regard as a fairly restricted field of scientific specialties. The second one was 50 years later in '32 – '33. I remember, Berkner remarking, "Well, the way things are going now, don't you think 50 years is too long to wait for the next one? Why don't we make it 25?" And Chapman immediately agreed. I think Berkner was essentially addressing Sydney Chapman with this remark. We, obviously, were all parties to the remark. Sydney said, “Good idea, Lloyd. Why don’t we get together on that?” That was essentially the genesis of the International Geophysical Year. I think Sydney had already had the same idea because he immediately remarked, “1957 – 58 is going to be a time of maximum solar activity.” Which was another good reason for that choice. My own impression was that Chapman already had this idea in mind. But at least on the surface of things, Lloyd was the one who opened the conversation. I’m quite clear about that. But, my impression is that Chapman was so immediately responsive as though he was just waiting for Lloyd to make the suggestion. But I think it had already passed through his mind as well because he knew right away that was the period of the expected maximum solar activity. And he concurred completely on the general assessment that geophysics was moving along so rapidly in so many different areas that the time was ripe for an international coordinated effort after a much less lapse of time than previously. Then, of course, Chapman went on to become President of the International Geophysical Year, also called CSAGI, the initials of the French term.

(600)

BS: *Who ran with the idea out of your house? Someone pushed it.*

JV: Chapman was the key person, understandably, and Berkner was also very influential in the U.S. and internationally.

BS: *I didn't know Chapman was there, but . . .*

JV: Oh yes. Chapman was the kingpin.

BS: *He was at ICSU and at Paris. Who else do you make president?*

JV: OK. Chapman was clearly the best choice.

(End of Tape 1 – Side A)

(Begin Tape 1 – Side B)

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JV: It was a very pleasant and convivial evening among some, you might say, really important people. And during the course of the evening, Berkner spoke up after some general conversation about progress of the geophysics and various areas and additional fields of work which were being opened up these days and should be included in an international coordinated effort. Berkner said, "Well Sydney, it seems to me it's high time we have another polar year. But, I don't think we ought to wait for 50 years this time. Seems to me 25 years would be a better choice. And I think we ought to get something going for an International Polar Year." And Sydney said, "Well, I think that's a great idea, Lloyd. And I agree with that completely. In fact, it turns out that the 1957 –

58 period, which would be the 25th anniversary of the last Polar Year is a year of expected maximum solar activity and that's a consideration which would make it especially interesting to many scientists."

And so, all the rest of us spoke up with the feeling that it sounds like a great idea to us and why don't we get going on this. Well after this occasion, Sydney Chapman took the international leadership in going ahead with . . . he sensed the timeliness and he had the international prestige of a geophysicist and scientist, very well earned clout to make it go. He spent a good part of his life for many years on the IGY. Our meeting was on the 5th of April 1950. For the next several years, Chapman spent a good part of his time traveling internationally, going to various countries all over the world developing the idea of appointing committees for individual specialties, enlisting the support of influential scientists in all the major countries for this undertaking, and organizing the institutional structure for coordinating the work. He was, I think, essentially the unanimous choice for president of the IGY. He became the senior person for the whole IGY and continued as such for many years. That was one of his major professional undertakings but he, of course, continued to do personal research throughout that period – very important research. But certainly his most important contribution to the science of geophysics during that period was making a success of the IGY. It was a non-governmental enterprise. That was one of its distinguishing features. The adherent bodies were not government agencies in the countries, but independent scientific organizations of one kind or another.

BS: *So the Antarctic was 12 countries*.

JV: There was governmental participation and support, but the basic adherents to the agreement were non-governmental agencies, though each one of those agencies was free to solicit support by its own government for the work, which they did in all cases. But the adherents to the agreement were independent scientific societies. For example, the National Academy of Sciences in the United States was the principal society here and the National Science Foundation and the U.S. Navy provided the principal governmental support.

BS: *The Office of Polar Programs didn't get funded until 1959, at NSF. You went down there . . .*

JV: I was a member of several national IGY committees. The most important one was the one we eventually called the Satellite Panel, originally called the LPR Committee. That stood for Long Playing Rocket, Joe Kaplan's humorous cover-up jargon for our satellite panel. I was also a member of several other committees of the IGY. Within the Satellite Panel, chaired by Richard Porter, I was chairman of the Working Group on Internal Instrumentation and Bill Pickering was Chairman of the Working Group on External Instrumentation. As chairman of the WGII, I was responsible for soliciting and selecting the experiments which were to be conducted on the satellite itself. Berkner was Vice-President of the international organization and Joseph Kaplan from UCLA was

Chairman of the U.S. National Committee for the IGY and Berkner was also part of that. Hugh Odishaw of the NAS staff was a very helpful person in the entire U.S. enterprise.

So that's how we got the satellite program for the U.S. going and also that's where we generated Operation Deepfreeze and the general impetus. Polar exploration in both the Arctic and Antarctic received an enormous stimulus from the International Geophysical Year and the military services were very important in both areas. The National Science Foundation, the Office of Naval Research and a number of other military agencies both Air Force and Navy were essential in financing and in operationally supporting the Arctic and Antarctic programs. But the most notable was the Deepfreeze program which was a Navy program, a joint Navy-NSF program which became very durable and established the Antarctic continent as a major area of scientific exploration which it continues to be today. I have a fond recollection of the early work I did with Poulter in contributing to some of the magnetic field observations, our tilt meter observations and the meteoric observations of the second International Polar Year. Poulter did a large program of meteoric observations in the Antarctic with devices that he had developed when he was at Wesleyan College and which I put into commission in the summer of '32. He made a catalog of meteoric observations which I hope somebody has analyzed. He did a lot of very high quality scientific work there and, of course, the Antarctic continent is now protected by the international treaty from any kind of military aggressive action or the location of any guided missile launcher, atomic bombs or any other offensive weapons. That treaty grew out of the IGY and the internationality of the continent as a place to do research.

Also, I think, the treaty forbids exploitation of resources there. I'm not quite sure exactly how that's termed, but the idea of drilling for oil or somehow exploiting the population of penguins or whatever is strictly forbidden by the international treaty on the Antarctic. All of this was in the spirit of the IGY and I think were a direct outgrowth of the IGY. So, many different nations participated in the research program in the Antarctic. Because it's a continent rather than an ocean, of course, there's much more activity in the Antarctic than in the Arctic – scientific activity to my knowledge, but there is substantial military activity as well and particularly by submarines which have traversed the whole Arctic ocean submerged. I understand it is a very active area of submarine research.

BS: *Just released the data – bathometric data.*

JV: What's that?

BS: *We just released the data. The bathometric data*

JV: Oh, is that right? From the profiles? The bottom profiles and so forth?

BS: *And the upward profiles.*

JV: Upward profiles. Fantastic.

(100)

BS: *Al Gore is the father of satellite data.*

JV: Really?

BS: *He's taken in hand by Neil Sullivan who is an Antarctic researcher who was the last - just had a tour as the last head of the Office of Polar Programs. He's gone down to Antarctica and bent his ear. Al's Mr. Environment. He's big time.*

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JV: That's right. Great.

BS: *Great. Monstrous amount of data. We can't handle it. What we need to do is have access to the present satellites.*

JV: Well, Gore is the President's point man for science and all the environment these days and I think he is a very intelligent guy.

BS: *Well tell me about your work down there.*

JV: OK.

BS: *You went down on a ship, I assume.*

JV: That's right. I have made three polar expeditions for scientific purposes. The first in 1952 was with the support of the Office of Naval Research for the launching of small rockets from balloons in order to get high altitude measurements very inexpensively. I devised and developed this technique and received ONR's support. In 1951 I had just returned to the University of Iowa and had lots of ideas on observations I'd like to do in the Arctic, but didn't have much money to work with. So I followed up on a suggestion that was made by Lt. Lee Lewis, USN during our Norton Sound expeditions. It had never been reduced to practice before. The plan was to hoist a small military surplus rocket to an altitude of 50,000 - 60,000 feet with a balloon and then fire the rocket from there. The basic idea being to eliminate the air drag of the dense lower atmosphere. The particular rocket that we chose was called a Deacon and if launched from sea level it would go to, I think, about 40,000 feet. If you launched it from 60,000 feet, you could get about 250,000 feet summit altitude. The rocket was capable of carrying only a small scientific payload, about 30 pounds. We devised and built various instruments for measuring the primary cosmic ray intensity, the magnetic field of the Earth, and anomalies in the magnetic field in the ionosphere.

BS: *You did this all over?*

JV: Yes. We called this the rockoon technique. During the one-hour ascent to some 50,000 feet the balloon drifted with the wind, perhaps 30 miles or so from the ship. Also, you wouldn't know which way the rocket was going to rotate and be fired, so there was a large uncertainty in where the impact point would be. Hence it was totally unacceptable to do that over habitated areas. So you really had to do it from a ship remote from habitated areas. Another advantage of a ship was that we could steam downwind to create a zero relative wind situation so that we could launch the balloon with the rocket safely off the helicopter deck of an icebreaker even though the true wind speed was as much as 10 or 15 knots.

BS: *Which ship?*

JV: Eastwind. Our first rockoon expedition was aboard the USCGC Eastwind (AGB279). Coast Guard Cutter Eastwind which had been loaned to the Russians during World War II. It was a good ship. The Eastwind was the first ship on which we made rockoon expeditions. There were three of us from the University of Iowa. We made five successful flights in Baffin Bay up to 77° N latitude. The primary mission of the ship was to deliver supplies to the weather station Alert at the northern end of Ellesmere Island. This was done successfully through the Arctic ice pack. Our highest latitude was 83º 38’ N.

(150)

We attempted seven flights, five of which were successful and made valuable pioneering cosmic ray measurements in the Arctic. The Eastwind broke off one blade of one of its two huge bronze screws in the ice pack. The ship returned to Thule and we flew back - our little party flew back from Thule. But the ship was significantly disabled.

BS*: Was Martin Pomerantz with you?*

JV: No. I know Martin very well, but he was not on that expedition. The next summer we got ready and had another Arctic expedition. That was the summer of '53. And in 1954 and 1955 we had two other expeditions I did not go on those. Some of my young associates did. A major discovery was made on these expeditions. We found an enormous intensity of very easily absorbable radiation in latitudes that were associated with the auroral zone and this is what I called the auroral soft radiation. We got a very good story on that, actually. It was the work of the summers of '53, '54 and '55. And so that led us onwards and upwards, so to speak, in new types of observations.

In '57 we had our next major expedition and I led this expedition to the Arctic in the early summer of '57. It was part of the IGY and we made a series of observations of cosmic rays and magnetic fields at high altitudes off the coast of Greenland, and I was particularly interested in following up on the auroral soft radiation discovery.

Meanwhile, we were also getting the apparatus ready for a large number of flights in the Antarctic, again as part of the IGY. In September of 1957 we left Boston on the USS Glacier which was then the Navy's largest icebreaker and slogged our way down the east coast, sent up two or three reckons off the east coast. We went through the Panama Canal, and then struck off across the Pacific, particularly to make measurements in the central Pacific near the equator and while we were enroute there, we had the memorable experience of being informed of the launching of Sputnik I by the Soviet Union, October 4th, 1957. I recorded the experience in detail in my logbook. I still have the logbook in which I kept strip-chart records and wrote a lot of detailed observations, which we made with the help of the radioman on the ship and authenticated the signals by the Doppler effect. I wrote a dispatch summarizing our observations and the captain sent it to the National Science Foundation. One of the first observations of Sputnik in the Southern Hemisphere. It was a very exciting event. We continued to make our rockoon flights along the equator near the Line Islands and then went on to the Antarctic and made further flights in the Ross Sea and vicinity, some of which were successful and some unsuccessful.

(200)

We got several good flights in the Antarctic and those were, I think, up to that time, the only - they were the first high altitude rocket measurements that had ever been made in the Antarctic. It was all still from the ship. I never went ashore in the Antarctic. We were in sight of the shore a number of times, but then we returned to Port Lyttelton near Christchurch New Zealand and unloaded our gear and flew back to the States from there. I think the ship later went back to the Antarctic. I'm not sure about that. That was in November of 1957 when we entered Port Lyttelton. So we had a good run of very nice scientific work. Good stuff. During the period 1952 - 1957, the Iowa group conducted a total of 109 rockoon flights ranging from Baffin Bay in the Arctic to the Ross Sea in the Antarctic with a respectable level of success. Meanwhile, George Ludwig and I had been developing an Iowa instrument to be flown on a satellite. Our proposed apparatus had been chosen as one of the four instruments to be flown on early American IGY satellites. In November 1957, the Soviets orbited Sputnik II. During the autumn there had been embarrassing failures of the Vanguard rocket, which had been selected to be the primary U.S. vehicle for the satellite program - some humiliating failures of what was basically a good system. But it was in the early stages of development. Wernher von Braun at the Army Ballistic Missile Agency in Huntsville, Alabama had a competitive system called a Jupiter C multistage rocket - liquid fueled first stage, a multiple solid fueled second and third stages, and then finally a fourth stage. This was a competitive system to put a satellite in orbit. In November 1957 after the second Vanguard failure at the Cape, the President decided to go with the Army system rather than the Vanguard. Von Braun promised the President that he'd have one in orbit within 90 days. My little apparatus was selected as the primary scientific instrument on this flight for several reasons. One is that I was a friend of von Braun. He had kept me in touch with what they were doing in spite of the official decision to use Vanguard. And so I decided to design our apparatus so it would work on either the Vanguard or the Jupiter C system. Our Iowa instrument was the only one of the primary choices that had that property. Furthermore, we had a good state of advancement on the development of our instrument by then and could have flown it at almost any time of the IGY. We were selected by the national committee to be the primary payload on the first try of the Army system. We worked through the Jet Propulsion Lab. The JPL built the transmitter, the command receiver, the payload structure and the telemetry system. That Iowa/JPL instrument became the payload of what became Explorer I, the first successful American satellite.

That flight was a great success. The data from our instrument resulted in the discovery of the radiation belts of the Earth.

(250)

BS: *Talking about the Van Allen Belt.*

JV: Yes. Yes.

BS: *You mention it only casually.*

JV: Well, that was a long time ago. Following our discovery there was an enormous level of activity both here and elsewhere. Perhaps thousands of investigators over the world are now engaged in investigating the radiation belts of the Earth. And we went on to be the co-discoverers of the radiation belts of the planets Jupiter and then Saturn. I've spent a major amount of my life in the more recent times as a planetary explorer.

BS: *What were your first radiation studies? Where were your interests generated?*

JV: Well, of course, I did graduate work as a nuclear physicist - an experimental nuclear physicist, using what was called a Cockcroft-Walton particle accelerator named after the two Englishman who first developed the scheme, and so we did a large number of nuclear physics experiments. We measured x-rays, gamma rays and nuclear particle disintegrations. Those were the first radiation measurements I made in a nuclear physics lab. That was in the late 30s when I was a Ph.D. student.

BS: *Right here in Iowa.*

JV: Yes. Yes that's right. I was here at the University of Iowa then as a graduate student.

BS: *Who funded your first satellite payload? Was that . . . who funded your research on that?*

JV: It had a combination of support by the Office of Naval Research, the Army Ballistic Missile Agency and the National Science Foundation.

BS: *And who was your sponsor at ONR? I ask that because* . . .

JV: A man by the name of Urner Liddell at ONR was the original sponsor of the rockoon work. I was very grateful to him. But by 1957 the baton had been passed to someone else in the office, but it was the nuclear physics branch at ONR.

BS: *Louie Kuam?*

JV: Who was that?

BS: *Louie Kuam? No? Max Britton?*

JV: . . . got me. I have them written . . .

BS: *I'm curious. That's who I worked with.*

JV: Urner Liddell was our early sponsor. He's the one who first agreed to sponsor our rockoon work. That was in '52 and believe or not, I've had a continuous ONR contract for research ever since then. Our specific preparations for the IGY were supported by the National Science Foundation. But the ONR had supported our preparatory work and development of our equipment. The foundation for our competence to do satellite work was the support ONR had provided over the years. The Army Ballistic Agency also helped and the NSF gave explicit support for the satellite program. Hence the support was multiple. But support that has been the most durable over the years has been from ONR. I mean it's really been . . . they've backed us for years. It's a great outfit. After the creation of NASA, our support for many space missions has been by that agency.

BS: *What was your connection to Martin Pomerantz?*

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JV: Martin was, of course, a cosmic ray man. He was at the Bartol Foundation in Philadelphia. I got to know him well. We were colleagues and I don't mean at the same institution, but professional colleagues in the sense that we were working in the same general field. I used to hear Martin's papers and he'd listen to my papers on cosmic rays. And we got together often socially. Martin is a very outgoing, gregarious sort of guy. Lots of fun to be around. And then after W.F.G. Swann retired from being president of the Bartol Foundation, Martin was selected as his successor as president. Martin helped develop a lot of the neutron monitors during the IGY. He was very prominent during the development of the cosmic ray program during the IGY. I'd go out at his home in

Philadelphia occasionally. Got to know Martin quite well. And especially while he was at Bartol. He had this grand office at Bartol. I don't know if you ever visited him there. But, Swann was quite a character. He had a magnificent, big office which Martin took over. The size was similar to this room, but the elegance was quite impressive. Really a grand place, in the old fashioned sense. Yes. I knew Martin well but he is retired now and he's selling automobiles in a city in Florida.

BS: *Mussel Shoals.*

JV: Isn't that amazing?

BS: *He's on the Board of Governors. He's been elected as an honorary member of the Polar Society.*

JV: Yes, well, Martin's a good man.

BS: *You can have that. I should have given it to you when I first came. That's the Polar Times.*

JV: Oh, thanks. OK.

BS: *Last spring's issue. Of course, at the South Pole, he's memorialized, maybe at Bartol . . . but he's memorialized at South Pole because everything that's going on down there is his stuff.*

JV: Yes. He definitely pioneered in cosmic ray measurements in the Antarctic.

BS: *Two-thirds of the winter-overs were involved with his projects and all the buildings. He's Mr. South Pole.*

JV: Bibber? Is that his name? Bibber. John Bibber, I think, is working there at the present time.

BS: *Martin and I were the very, very best of friends.*

JV: Martin is a great character.

BS: *He and Molly came to my retirement!*

JV: Oh yes. Great.

BS: *And his wife came with him. Did you ever go back to the Arctic for any other research or were you involved in pushing research in the Polar Regions?*

JV: Not really. No. I wouldn’t say so, Captain.

BS: *Polar satellites.*

JV: I have been looking down on the polar regions with satellites, so I like to remember how it took us years with balloon launched rockets to do a latitude survey from Thule, Greenland, southward and then through the equatorial regions to the Antarctic so we made eventually a very good survey from the Arctic Ocean to the Ross Sea. It took us years to do it and as soon as we got the first satellite up, we did that in about 45 minutes.

(350)

That realization has been fundamental to all of my work since then. To realize what an enormous boost in the efficiency and effectiveness of satellites provide over ground-based balloon or rocket observations. So I've never looked back in that sense. In fact my entire career from 1958 onwards has been in satellite and spacecraft observations.

BS: *Space.*

JV: Because there's enormous increase in capability in many different ways. For example, I had one of the experiments on the first mission to any other planet which was

Mariner II to Venus in 1962. It was a successful mission. I also had an instrument on the first mission to Mars which was on Mariner IV in 1964 and on the second mission to Venus which was Mariner V in 1965. And a whole succession of Earth satellites and interplanetary missions. So that's been my life since '58 and most recently with the first mission to Jupiter and Saturn on Pioneer 10 and Pioneer 11. I had one of the principal instruments on each of those spacecraft. I have the only surviving working instrument on Pioneer 10 at the present time.

BS: *Where's Pioneer 10 now?*

JV: Pioneer 10 is now escaping from the solar system in the antapex direction from the Sun. The solar system is moving relative to the local star field. It's called the apex direction and we're going in the opposite direction which is called the antapex direction. We're almost 69 astronomical units from the Sun.

BS: *You're still getting signals?*

JV: I'm getting good signals.

BS: *Here?*

JV: We don't receive them here but they're received by the NASA Deep Space Network and relayed to us. I just got a fresh batch of data yesterday. I haven't finished plotting it up yet, actually.

BS: *So you're pretty busy still.*

JV: Oh, yes. We have some nice stuff going. And we're writing papers on some other things too. Got a couple of young associates here and I've got a young man I'm trying to put through a Master's degree right now. And another young associate who works with me all the time. We have a good time on analysis of some of, what we call interdisciplinary science, on the Galileo mission which is currently a spacecraft in orbit around Jupiter.

(400)

So, I'm working on the analysis of some of those data which are contemporary data. Also I'm going some retrospective analyses on some of the data from previous missions that I never properly worked up before.

BS: *You involved with the nuclear powered satellite that was just . . . that whole controversy that was just recently launched?*

JV: Well, I'm not personally, but I'm very well acquainted with the Cassini Mission and one of its principal instruments was built right next door here. Two doors down the hall.

BS: *Is that right?*

JV: That's right. By Dr. Gurnett and his group, who is a former student and is now a senior professor here. He's one of the PIs on the Cassini mission. He was distressed about the threat of cancellation or delay. And he's had, of course, all kinds of e-mail on the subject and lots of information bulletins from here and there and lots of controversial statements. So he's pretty much in the thick of it, but not operationally. The President made the final decision to proceed. There were lawsuits brought in both Florida and Hawaii to enjoin the launch, but they were both in federal courts and both judges rejected the motions to enjoin. The opponents of the launch were quite irrational.

BS: *Irrational?*

JV: Yes. At the mere mention of radioactive nuclei, they go into hysteria. But on quantitative grounds, I think the objections were absurd. Risk to human life was just trivial any way you could figure it. Any rational way of figuring it.

BS: *There are bigger nuclear accident risks like running a nuclear submarine aground coming into San Diego harbor. And they've had some bangs bumping into stuff like divers do.*

JV: Yes. The hysteria doesn't have anything to do with any rational discussion of the subject. They say, the risk may be small, but it should be zero. There is no such thing as a zero risk in my way of viewing life. And there isn't any way to talk to those people. No risk is acceptable. Furthermore they don't believe the U.S. Government. The claim the government has a record of lying about the safety of the shuttle and look at the Challenger accident. The government lied to us about poisonous gas in Iraq and facts are coming out that the government lied to us about this and that. People bring up this kind of argument and they have a lot of emotional appeal in certain quarters. I don't know what you do about all of that.

BS*: We have it out in Oregon. The [gummament].*

(450)

JV: Yes. Yes. Right.

BS: *Why don't you tell us about the Nansen Medal. How did you come to receive it?*

JV: Well, I was selected for a Nansen medal because of my contribution to Arctic geophysics, radiation belt physics and the exploration of the aurora by rockets and satellites. My wife and I had a great time in Oslo. Also I flew to one of the most northerly points in Norway.

BS: *North Cape?*

JV: Well, it wasn't that, but it was close to that at Tromsø- my third trip to the Arctic. There's a military base there and also a rocket-launching base at Andrøya nearby. The Norwegian Navy operated out of that port too. They used to go out on military patrol on big diesel powered boats. Outside my hotel window there, I'd hear them warming up about 5 o'clock in the morning and they all left on patrol about 7 o'clock in the morning. I asked what they were patrolling for. They were patrolling for Russian submarines. And then it turned out, as I was told, that they never went out on Sundays and I asked why didn't they do that on Sundays if they were looking for hostile vessels. Oh, they said, the Russians never went out on Sunday either, so they didn't need to. Anyway, I had a great time in northern Norway. One of the highlights back in Oslo was a visit to the Fram which was Nansen's ship an all wooden ship and very heavily built. A real thrill for me to go on board because Nansen had been locked in the Arctic ice, I think, through one polar night at least, maybe more.

BS: *Two nights freeze.*

JV: And eventually came through. He was not only a great explorer and a great scientist, but a great political leader of Norway, too. He's very highly regarded in Norway for his political savvy and judgment and his general quality as an individual. It was a high honor for me to receive the Nansen Medal.

BS: *One of the fathers of the country. It was, of course, Sweden, where he was born. He was one of the champions for the independence of Norway.*

JV: Yes.

BS: *And, of course, he headed the Russian Famine Relief in the 20s.*

JV: Oh he had an enormous record of personal and scientific work and politically, he was a very great man.

(500)

BS: *And, of course, Amundsen took the Fram to the Antarctic. That was Little America 0, if you like. Amundsen called it Franheim and his station manager became Byrd's Station Manager. That was the connection. Amudson set up the Base there, right where Little America was. And he and Byrd became good friends. Ronne's son, Finn, came back to little America II and III with Byrd and later Little America V (the main scientific* *base for IGY). So you can trace this lineage back from IGY to Byrd to Amundsen to Nansen, who was an inspiration for Amudson who was an inspiration for Byrd, who was an inspiration for all kinds of people – including yourself.*

JV: Yes, sure.

BS: *When you consider the magnitude of people Byrd influenced who did great things. That's his contribution.*

Here we are. All the way up through Berkner, from Byrd, to you, through the Polar Year.

[**End of Interview]**

(End of Tape 1 - Side B)