## THE FIRST EXPLORER SATELLITES

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This paper summarizes events culminating in the launch of the early U.S. Earth satellites, Explorers I and III and the discovery of the Van Allen Radiation Belts. A companion paper, <u>James Alfred Van Allen</u> – From High School to the Beginning of the Space Era, provides additional biographical background. My lecture for this birthday celebration, <u>Building the Early Explorer Instruments</u>, is a condensation of this paper.

These two papers and lecture were assembled from material being written for a full-length book relating the story of the University of Iowa's role in the development of the space research program during the decade of the 1950's. The book is expected to be available in time for the celebration of the 50<sup>th</sup> Anniversary of the Explorer I launch in January 2008.

The papers, lecture, and planned book all draw heavily upon many published papers and unpublished sources. Some of those sources are cited in the notes at the ends of these papers, and the book will contain a more complete list of citations. The contributions by the authors of all of those sources are gratefully acknowledged.

### The Pre-Vanguard Era

As James Van Allen and his students at the State University of Iowa were building, flying, and analyzing the data from balloon, rocket, and rockoon instruments during the first half of the 1950's, Van Allen had his sights set on higher targets. His first serious proposal for a satellite-borne cosmic ray instrument was prepared well before there was an approved program for launching an artificial Earth satellite. In November 1954 he prepared a preliminary outline for a satellite-borne cosmic ray experiment to measure total cosmic ray intensity above the atmosphere, and to measure its variations and correlation with solar activity.<sup>1</sup> How did this come to be, and what happened to the proposal?

It is commonly perceived that the Army's Redstone-based satellite program came into being only after Sputnik I was launched on 4 October 1957. In fact, it already had a long and frustrating history, which might be thought to have started in Germany before and during World War II.

As in the case of many rocket pioneers, dreams of flight into space guided the work of Germany's Wernher von Braun throughout his life. As a sixteen-year old teenager in 1926, he decided that he would devote his life's work to developing powerful rockets with active guidance systems that could achieve that objective. His group's most notable and controversial achievement was the development of Germany's V-2 rocket (the *Vergeltungswaffe Zwei*, translated in English as Vengeance Weapon Number Two) that came into play near the end of World War II. Although developed for the wrong reasons, the V-2 marked a major milestone in the history of rocketry. It was the first practical, guided, mass-produced rocket with substantial lifting and performance capabilities.

Thoughts of using this rocket for space travel and scientific research were present in von Braun's thinking from the beginning. In fact, von Braun, along with two of his other top staff members, was arrested and imprisoned for two weeks in March 1944 by the German Gestapo, with the charge being that they were developing the rocket out of their interest in space travel and research, and that these interests were diverting their energies from the task of developing it for war. It took an appeal by von Braun's boss, Major General Walter R. Dornberger, to free them.<sup>2</sup>

After the German scientists and engineers were brought to the United States following the War, they continued with their rocket development under von Braun's leadership, but now under U.S. military sponsorship. This environment continued to foster his dreams of space flight.

By a chain of decisions by enlightened leaders in the post-war V-2 test program in the United States, most of the flights of V-2s assembled from captured components carried meaningful instruments prepared by leading scientists. Van Allen, then a young upper atmosphere scientist leading a research group at the John's Hopkins Applied Physics Laboratory, was one of those researchers. His group prepared a number of experiments for those flights, among them, some of his own cosmic ray investigations.

An important liaison was formed during this period. Ernst Stuhlinger had rejoined the German V-2 group in the U.S. as von Braun's senior scientist, and served as the ombudsman between the rocket experts and the scientific researchers.

Most of the German group were eventually moved to the Army's Redstone Arsenal at Huntsville, Alabama. There, one of their first substantial tasks was to develop the Redstone rocket. Its first flight took place on 20 August 1953. Even in 1952, as the rocket design neared completion, von Braun believed that, with the addition of appropriate upper stages, it could launch an artificial satellite. Using his authority to make somewhat limited feasibility studies, and teaming with the Jet Propulsion Laboratory under William Pickering's leadership in Pasadena, California, they began unofficial preliminary planning for that eventuality. In 1954, in a private discussion about the project with Stuhlinger, von Braun expressed his belief that they should have a "real, honest-to-goodness scientist" involved in their little unofficial satellite project. "I'm sure you know a scientist somewhere who would fill the bill, possibly in the Nobel Prize class, willing to work with us and to put some instruments on our satellite." Stuhlinger, himself a cosmic ray researcher during his college years, and having worked with Van Allen at White Sands, was ready with his reply: "Yes, of course, I will talk to Dr. Van Allen." <sup>3</sup>

Stuhlinger followed this by a visit with Van Allen at his home in Princeton, New Jersey, where Van Allen was on sabbatical leave from Iowa to work on stellarator design. Van Allen later recounted, "Stuhlinger's 1954 message was simple and eloquent. By virtue of ballistic missile developments at ABMA, it was realistic to expect that within a year or two a small scientific satellite could be propelled into a durable orbit around the earth.... I expressed a keen interest in performing a worldwide survey of the cosmic-ray intensity above the atmosphere."<sup>4</sup>

Much later, Stuhlinger remarked that Van Allen was taciturn at that meeting. "When I had finished my sales talk and waited for Dr. Van Allen's show of interest, he only said: 'Thanks for telling me all this. Keep me posted on your progress, will you?' I was disappointed by this apparent lack of interest, but then I remembered from our meetings at White Sands that Dr. Van Allen was a very cautious scientist, far too careful to jump to any conclusions."<sup>5</sup>

Stuhlinger has also recalled that, when he saw Van Allen shortly after the Explorer I launch, Van Allen confided, "Do you remember that evening in Princeton, three and a half years ago? I was sitting in my corner, listening to you through my screen of smoke, and I said to myself: Either, these guys are crooks. Or, if they are not, they have something absolutely fantastic on their minds, and even on their hands. At that time, I did not know the correct answer. Now, I do!" <sup>6</sup>

This off-the-record conversation greatly heightened Van Allen's excitement about extending his cosmic ray research further into space. He soon sent Stuhlinger the outline for the satellite-borne cosmic ray experiment that was mentioned at the beginning of this section.

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As the building of the Army's Redstone rocket was nearing completion, the ABMA group undertook the development of the Jupiter rocket, a much longer-range Intermediate Range Ballistic Missile. As a

part of that program, in order to test nosecones for reentry through the atmosphere, they developed a special test vehicle to achieve the necessary high velocity. Since it was part of the Jupiter program, it was called the Jupiter C (Jupiter Composite). It consisted of a Redstone first stage, topped by clusters of small solid propellant rockets forming an additional two stages. It was consciously designed from the beginning so that an additional stage could be added in place of the test nosecone to make the vehicle orbit-capable. The orbit-capable three-stage version was referred to at Huntsville as the Juno I, although the Jupiter C label is often applied to it as well as to the two-stage version.

The story of the development of the Juno I is resumed in a later section.

### **The Vanguard Project**

After early planning for the International Geophysical Year by its international and national participants, President Eisenhower announced, in July 1955, the intent to launch an Earth satellite as part of the United States IGY program. A fierce competition developed between von Braun's group and a Navy group centered at the Naval Research Laboratory in Washington, DC. After vigorous debate and consideration of a recommendation by a special committee led by JPL's Homer J. Stewart, the Secretary of Defense, on 9 September 1955, established the Navy's Project Vanguard as the chosen alternative.<sup>7</sup> Van Allen later summarized the reasoning behind this order as being "military-political in nature - to avoid revealing the propulsive capability of the United States and to avoid alarming foreign nations with the realization that a U.S. satellite was flying over their territories."<sup>8</sup> Controversy about this decision continues to this day.

Van Allen lost no time in proposing his cosmic ray experiment for the newly approved satellite program. Anticipating a fast-breaking situation he, on 25 September 1955, submitted a more completely developed and definitive version of his earlier proposal to Joseph Kaplan, by then chairman of the U.S. National Committee for the IGY.<sup>9</sup>

As Van Allen was preparing this Vanguard experiment proposal, I was completing work on my Bachelor's degree and beginning to search for a graduate research topic. The two of us began a series of discussions about specific details of the satellite design. This was to develop into my work in designing the cosmic ray instrument.

In October 1955 the Upper Atmosphere Rocket Research Panel, an outgrowth of the original V-2 Upper Atmosphere Panel which was still active and, by then, under Van Allen's chairmanship, organized a symposium to discuss the scientific aspects of planning for the IGY satellite program. This especially noteworthy tenth anniversary meeting was held on 26 and 27 January 1956 at Ann Arbor, Michigan. Thirty-three proposals for both passive and active satellite experiments were presented at that meeting, and Van Allen later published them in book form.<sup>10</sup>

At this meeting, Van Allen presented three proposals in two separate papers as further elaborations of his earlier ideas. His first paper, Cosmic-Ray Observations in Earth Satellites,<sup>11</sup> actually contained two proposals. The first was for the use of a simple single detector for a first-time study of the cosmic ray intensity above the appreciable atmosphere on a comprehensive geographical and temporal basis. He envisioned that these data would be especially valuable in helping to interpret the extensive ground, balloon, and rocket observations being planned for the approaching IGY. Soon after the meeting, he submitted this proposal to the U.S. National Committee for the IGY as a more definitive update to his earlier one.

The second proposal in Van Allen's first Ann Arbor paper was for the use of a Cerenkov detector to study the relative abundance of heavy nuclei in the primary cosmic radiation. This portion of the proposal

was soon dropped because of the severe limitations on Vanguard instrument weight. Its scientific objectives were eventually achieved by instruments on later satellites.

Van Allen's second paper, <u>Study of the Arrival of Auroral Radiations</u>,<sup>12</sup> proposed a further study of the high intensity radiation that had been discovered on the earlier Iowa rockoon flights. Although this proposal was not included for development within the Vanguard program, (it required a much higher orbital inclination than was envisioned for that satellite) some of its objectives were met by Explorer IV in mid-1958, and others were met during the early 1960's with other satellites.

At about the time of the Ann Arbor meeting, Porter's Technical Panel on the Earth Satellite Program (TPESP) set up a Working Group on Internal Instrumentation (WGII) under Van Allen's chairmanship and a Working Group on External Instrumentation (WGEI) under William H. Pickering's chairmanship. At its first meeting, on 2 March 1956, the WGII adopted a set of criteria for assessing proposed investigations, and proceeded to oversee the selecting and prioritizing process. On 22 May, they approved an initial sum of \$106,375 for Van Allen's instrument development. (This funding was increased somewhat later, with the eventual total authorization for the cosmic ray satellite instrument being \$169,225!)

### The Vanguard Cosmic Ray Instrument

Immediately following the Ann Arbor Symposium, I began substantive laboratory work on the instrument. Van Allen and I quickly reached final decisions on the specific experiment objectives and the overall design approach for the instrument. The hardware development was a reasonably straightforward extension of our earlier rockoon work, with two essential exceptions. Van Allen's proposal, by its nature, required data recovery over a wide range in geographic position. This meant that either there must be a very extensive network of ground receiving stations, or on-board data storage would be necessary. A sufficiently dense ground network did not seem likely, and we soon concluded that on-board data storage was needed. Because of the extreme weight, size, and power limitations of the envisioned satellite, developing this recording device turned out to be one of my most challenging tasks.

The second major challenge was the need to use transistors in the electronic circuitry. Again, because of the satellite's limitations, use of the vacuum tubes that we had employed in our balloon and rockoon instruments was out of the question. This was very early in the history of semiconductor devices – less than six years after the first announcement of the junction transistor, only three years after the first transistor hearing aid was announced, and less than two years after the first commercially produced radio became available. Although I had been watching reports of the development of the new devices, I had never actually seen one. Literature dealing with the practical design of transistor circuits was virtually non-existent. I was able to find several binary scaler designs, mostly in the IRE publication Electronics, but none of them operated at the low power levels that we needed. Our instrument, as it eventually evolved during the next two years, used over 100 transistors that required less electrical power than would have been required to heat the filament in a single vacuum tube.

By 1 June 1956, my work had progressed to the point that I was able to make a detailed report to the Working Group on Internal Instrumentation. This included the instrument's complete block diagram, a description of its operation, and the first meaningful estimates of its projected weight, size, volume, and power consumption.<sup>13</sup> At the end of that meeting, four proposals, including our cosmic ray experiment, were designated as flight priority A. The other experiments in the initial group were Herman LaGow's environmental measurements experiment (NRL), Herbert Friedman's solar Lyman-alpha intensity experiment (NRL), and Maurice Dubin's micrometeorite experiment (AFCRC). This designation meant that, if the development of those instruments proceeded satisfactorily, the first few launch vehicles would be assigned to their use.<sup>14</sup>

It is a tribute to the spirit of that simpler and gentler era that respected scientists were able to participate in both experiment preparations and the assigning of their flight priorities. This was a

straightforward evolution of the practice that had begun in the days of the V-2 rocket flights. Van Allen, as chairman of the Working Group on Internal Instrumentation, was concurrently involved in proposing and preparing a key experiment, evaluating proposals, selecting some for flight, and generally overseeing the progress of the experimental program. The scientists were successful in carrying out those potentially conflicting responsibilities so that, to my knowledge, there was never a serious claim of a conflict of interest resulting in an unfair decision.

Although our experiment and the preparation of its flight instrument were our responsibility, it constituted only one element of a broad collaborative Vanguard satellite program. The Naval Research Laboratory was responsible for its overall technical management. At the same time, they developed the launch vehicle, developed and fabricated the satellite shells, their thermal control, instrument mounting arrangements, the antenna and connecting harness, the transmitters, and the battery power supplies for all of us experimenters. They tested all of the satellites to ensure that they would withstand the rigors of the launch and space environments. Finally, they designed and built the network of Minitrack ground tracking and receiving stations and a computing center to derive the satellite orbits and produce magnetic data tapes containing the experimenter's flight data. In addition, they always made the results of their extensive circuit development efforts freely available, and helped us with technical questions and problems whenever needed.

NRL carried added responsibilities in support of our particular instrument, namely, the development of a command receiver and the installation of command transmitters at the tracking and receiving stations. The Army Signal Corps Electronics Laboratory (SCEL) at Fort Monmouth, New Jersey contributed to our instrument by developing the 700-volt power supply required for our GM counters and providing kits of pre-tested parts that we assembled on our circuit boards. Another important collaboration was with Maurice Dubin and Edward Manring of the Air Force Cambridge Research Center (AFCRC). We integrated some of their micrometeorite instrument electronics into our package.

As the program progressed, the meetings of the Working Group on Internal Instrumentation provided a forum for the exchange of information between all participants. Those meetings also served as key milestones in the preparation of our instruments, as they caused us to pause in our laboratory work to reflect on progress and prepare our reports. By the time of its 9 October 1956 meeting, Van Allen had arranged with Nicholas Anton to design our GM counters, they had delivered the first of them, I had worked out the physical form of our instrument, the switch had been made from germanium to silicon transistors, and good progress was being made in designing all of the electronic circuits. The data recorder had progressed through several evolutionary designs and a major change had been made from the use of the originally envisioned magnetic drum to magnetic tape as the storage medium.

By the time of the WGII meeting of 3 December, James Heppner's Magnetometer experiment (Naval Research Laboratory) and Verner Suomi's Radiation Balance experiment (University of Wisconsin) had been added to the flight priority A list. A Cloud Cover experiment by William G. Stroud of the Signal Corps Engineering Laboratories (SCEL) appeared at the top of the flight priority B list, and had also been funded for development. The National Advisory Committee for Aeronautics (NACA) had provided most of the funding and was developing an Atmospheric Density experiment using a simple inflatable sphere, as proposed by William O'Sullivan at that organization's laboratory at Langley Field, Virginia.

After it was reported at the December meeting that the development of all high priority instruments was proceeding satisfactorily, a more definitive flight priority was assigned. This was: (1) a NRL/Lagow/Friedman combination Environmental Measurements and Solar Lyman Alpha experiment, (2) the SUI/Van Allen Cosmic Ray experiment, combined with the AFCRC/Dubin Micrometeorite experiment, (3) the NRL/Heppner Magnetometer, combined with the NACA/O'Sullivan Air Density experiment, and (4) either the University of Wisconsin/Suomi Radiation Balance experiment or the Stroud/SCEL Cloud Cover experiment, depending on subsequent developmental progress. This priority

assignment remained unchanged until events following the launch of Sputnik-1 in October 1957 shifted our cosmic ray experiment to the Army's Jupiter C program.

Our next major report to the WGII was made at its four-day meeting on 24-27 April 1957. By that time I had built a full prototype instrument package (with a few of the circuits still in mock-up form), completed numerous temperature tests of individual circuits, and conducted a first vibration test of the tape recorder and GM counter at NRL.<sup>15</sup> I projected (optimistically, as it turned out) delivery of our complete engineering test package to NRL for vibration testing on 15 June, and of three flight units on 1 August. The NRL was, at that time, projecting the launch of our instrument in February 1958 (also proving to be optimistic).

Work continued at a hectic pace through the summer of 1957. The Naval Research Laboratory delivered a succession of components, including an aluminum prototype satellite shell (to be replaced later by a magnesium one), transmitter, receiver, and radio frequency harness, so that we could fit the full mechanical assembly and verify the radio frequency performance of the complete satellite. We incorporated a new high voltage power supply design from the Signal Corp Electronics Laboratory into our package. The micrometeorite circuits from Air Force Cambridge Research Center were incorporated. This much more complete instrument package was subjected to a succession of tests, including various electronic system tests and, in partly assembled form, vibration testing at NRL.

On 2 October, at the CSAGI Conference on Rockets and Satellites in Washington, DC, I gave another complete report on the status of our work.<sup>16</sup> Just two days later, the Sputnik-1 launch was announced, as detailed in the next section.

After attending the CSAGI meeting, I stayed in Washington for the following week for additional work on our instrument. The NRL engineers and I calibrated the radio frequency portion of our completely assembled satellite, using the NRL developmental model Minitrack ground station. A test of the isolation between the transmitter and receiver, and planned helicopter flights over the Blossom Point, Maryland Minitrack station were deferred, as the necessary equipment and facilities had been preempted for receiving the signal from the new Soviet satellite.

My last notebook entry dealing with the development of the prototype cosmic ray package for Project Vanguard was on 29 October 1957, a little over three weeks after the Sputnik launch, only a few days before the second Sputnik launch, and following serious discussions in Washington about authorizing the Army to proceed with the launch of their Jupiter C-based launch vehicle. My final act in closing out the Vanguard phase of our work was to prepare a complete progress report to its sponsors.<sup>17</sup>

Thus, by the end of October, I had developed a fully operating prototype cosmic ray instrument package. It began to appear that our instrument's preparation was about to take the fast track.

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Before leaving the discussion of the Vanguard instrumentation effort, it should be reported that our working relationships with that program's many scientists, engineers, technicians, and managers were outstandingly cordial and helpful. Our shift from the Vanguard to the Army's Jupiter C program never dimmed this relationship, and we continued many collaborative efforts with NRL and the other Vanguard experimenters throughout and after that transition. This remains an especially bright feature of the work leading up to the first U.S. space flights and, in fact, throughout the entire IGY program.

### Sputnik I!

The announcement of the launch of Sputnik I by the Soviets, like a bolt from the blue, immediately changed the complexion of the U.S. space program. Although much of the gentlemanly collaborative

international spirit remained throughout the IGY, a competition to demonstrate technical superiority in the Cold War became a prominent element.

This surprising demonstration of what was viewed as the superiority of Soviet launch capabilities alarmed everyone in the U.S., from military planners to the general public. In looking back from fifty years later, there is little doubt that the Soviets, by this triumph, made a major contribution to advancing the world's entry into the space era. It is probable that, without this National humiliation, we would not have a NASA today, man would not have set foot on the moon, and we would not be operating huge telescopes in space.

Adding to the shock, the Soviet launch occurred during the week when scientists from countries all over the world had gathered to continue planning for the budding space program. The Comité Speciale de l'Année Géophysique Internationale (CSAGI) had organized this first CSAGI Conference on Rockets and Satellites in Washington, DC. Occurring on 30 September through 5 October 1957, the agenda included general reports from all participants, meetings of the various IGY Working Groups, and the presentation of a wide assortment of technical papers.

During that week, the official delegates and a scattering of newsmen received invitations for a Friday evening cocktail party at the Soviet Embassy. By an accident of circumstances, I attended this momentous event, when I was substituted for Van Allen who was busily launching rockoons in the South Pacific. After the meetings on that day, I freshened up at my hotel room, walked the short distance to the old USSR Embassy near Scott Circle, and was among the early arrivals. I was greeted warmly and escorted to the grand ballroom on the second floor. During the course of the next half hour or so, most of the other guests arrived. As the assembly grew, the babble of mingled voices swelled, as small animated knots formed and reformed around the elaborate tables of *hors d'oeuvres* and the several bars. Most of the discussions centered on IGY planning and technical information provided at the Conference, and the three Soviet delegates and many of the Embassy senior staff mingled freely with the guests.

Just as the party was reaching full swing – as if on cue from an unseen master of ceremonies – there was an interruption. Lloyd V. Berkner, the CSAGI Vice President and Reporter for Rockets and Satellites, climbed on a chair. He clapped his hands loudly to get our attention. As we hushed, he proclaimed,

# "Radio Moscow has just announced that the Russians have placed a satellite in orbit 900 kilometers above the earth."

There was a short stunned silence, and then applause gradually spread as we began to realize the reality and significance of the moment. Reporters rushed out of the room for telephones to contact their papers. The Soviets beamed with obvious pleasure during the first of the toasts with the excellent Russian vodka.

The United States Navy Vanguard Program (based on the strictly non-military Viking and Aerobee research rockets) had been chosen over the Army Jupiter C Program (based on the clearly military Redstone rocket), among other reasons, because it was believed to carry less of a Cold War threat to the Soviets. But the Soviets had used a military launch vehicle, their satellite was passing freely over the U.S. and many other countries, and no objections resulted.

The Soviet launch immediately made space an open, non-military arena.

### Pre-Sputnik Development of the Army's Satellite Launcher

During the entire period when the Vanguard program was evolving, dreams of using the Jupiter C as the basis for a satellite launcher had continued at Huntsville and Pasadena. The first Jupiter C test flight on 16 November1956 for its official purpose, the testing of nosecones, further increased their enthusiasm

for the satellite application. Even though an active final fourth stage had not been included in the test flight (by direct order from the Pentagon), it reached a height of 682 miles and a distance of 3,400 miles. If the final stage had been added, this flight could have entered Earth orbit.

Knowing that, and in light of the decision by the Secretary of Defense to proceed with the Vanguard proposal, the Pentagon brass instructed the Army to stop work on the satellite program. Von Braun at Huntsville reacted by setting aside one of the Jupiter C first stage vehicles and carefully subjecting it to a "long-time storage test." Jack Froelich at JPL, likewise, put a number of the Jupiter C Sergeant solid-state rockets into a program to "test them at normal room temperature, pressure, and humidity until destruction, to determine the long-term characteristics of the propellant." <sup>19</sup> These rockets made the later rapid response to the Sputnik I launch possible.

Following the successful nosecone reentry test flight, von Braun made new attempts to obtain approval to complete the design of the satellite-launching vehicle and a satellite payload. Among other actions, he sent Stuhlinger to Washington to offer the now-proven rocket to the Army brass, and to the Vanguard officials. He even offered to write the name "Vanguard" in large letters on the side of the Redstone rocket.<sup>20</sup> All of these offers were rejected – the decision had been made to launch the Vanguard independently of any help from the Army.

In spite of that, the space enthusiasts at Huntsville, quietly and off the record, continued to develop their design, with much of the work being done on their own time. Ernst Stuhlinger quickly informed Van Allen of the successful test flight. During this call, he also expressed his grave doubts about the realism of the Vanguard rocket launch schedule, and encouraged Van Allen to suggest a cosmic-ray instrument that could be used as a payload for the Jupiter C. Van Allen did this informally during that long telephone conversation, and followed it in February 1957 with a more formal letter proposing a specific instrument package. In that letter Van Allen stated, "We are delighted to know that there is a possibility of flying some scientific apparatus on one or more of your orbiters." After outlining the range of investigations that might be undertaken, his letter continued, "Needless to say, our group here at the State University of Iowa is very eager to participate in your program. We now have all the appropriate elements of a suitable cosmic ray apparatus well developed, as well as the foundations for interpretation of the observed data. We can make several sets of flight gear...within about a month after receipt of definite packaging details. The only other significant factor which is not presently known to us is the impedance, voltage and pulse width of our signal for modulating the transmitters. We shall await further information with great interest." His enclosure to that letter contained the major electronics parameters, block diagram, and physical dimensions of the cosmic ray instrument package that he envisioned.<sup>21</sup>

The dialog between Stuhlinger and Van Allen continued during the following months, as substantive plans for the unofficial Army satellite continued to evolve at Huntsville and Pasadena.

The Vanguard project had settled very early on a 3-1/2-inch-diameter instrument package. We felt uncomfortable with that constraint. Based partly on our extensive experience with our six-inch diameter instruments for the Deacon-based rockoons, we felt that a six-inch configuration for the satellite would permit more efficient packaging. It must be added, however, that we were also strongly influenced by the six-inch diameter of the final rocket stage being planned for the Juno I launcher, and wished to remain compatible with that configuration.<sup>22</sup> Van Allen formally expressed his preference for the six-inch envelope in a letter to the Technical Panel on the Earth Satellite Program in late-January 1956. He proposed that half of the IGY payloads be built in the original 3-1/2-inch Vanguard configuration, identified as Mark I, and that the other half be of a new Mark II configuration in the shape of a right circular cylinder six inches in diameter and eighteen inches in length.<sup>23</sup> Following that recommendation, the Vanguard program changed their specifications to permit either configuration to be housed within the Vanguard 20-1/2-inch diameter satellite sphere.

On 19 April 1957 ABMA's Stuhlinger, Arthur Thompson, Charles Lundquist, and Joseph Boehm visited Van Allen, Frank McDonald, and me at Iowa City for further discussions. During that meeting,

they gave us the full technical outline of a possible satellite payload. During the rest of that month, while continuing my work on the Vanguard instrument design, I also worked out further details of an abbreviated version of our Vanguard instrument for their program. This work culminated in my sending detailed drawings of the instrument that we envisioned to Boehm on 1 May.<sup>24</sup> This was followed by a visit on 23 May by either Henry L. Richter, Jr. or Eberhardt Rechtin (my notes and memory are inconclusive on this point) from JPL. He came to discuss the use of their Microlock communications system as an alternative to the Minitrack system, and we laid out a block diagram for a continuous channel on that system. This design was the one used later for the low power transmitters on Explorers I, II, and III. A call from Ernst Stuhlinger on 12 June indicated that he was checking into the use of the Vanguard/NRL command receiver. Thus, plans were being made even then for the flight of our complete Vanguard instrument, including its in-orbit data storage, as well as for the simpler, abbreviated instrument.

From 9-12 July, I was at Huntsville to work out further details of the Jupiter C payload with the ABMA engineers. As our ideas evolved during that meeting, the ABMA engineers created a sequence of drawings. The first, dated 9 July, showed some details of our instrument mounted in the nose of the satellite. The second, dated the following day, showed layout details of the mounting ring to which our instrument would be attached. The third drawing is annotated, "Proposal. Payload instrumentation. For purpose of radiation measurements. July 11, 57." Signed, "Wag." (Wag. refers to Herman L. Wagner, a senior engineer at Huntsville.) This historic drawing shows a cutaway view of the complete satellite, with our cosmic ray instrument package in the forward section, and the Microlock transmitter at the aft end. The payload is shown attached to the final scaled-Sergeant rocket stage to form the complete satellite assembly.

This drawing is remarkably like the final design of Explorer I. Thus, the preliminary Explorer I design was "in our pockets" nearly three months before the Soviets launched Sputnik 1.

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This work was, however, behind the scenes, as the Juno I satellite effort was not an officially recognized part of the U.S. IGY satellite program. Had it been sanctioned at that time, the preliminary plans could have been converted to finished hardware in a very short time.

### Switching to the Jupiter C, Deal, and Explorer I

News of the Sputnik 1 launch reached von Braun at Huntsville while they were hosting a reception for Neil H. McElroy, the incoming Secretary of Defense. After expressing his extreme frustration, von Braun told the Secretary to remember that, when he returned to Washington and "found that all hell has broken loose," his team could have a satellite up in 90 days.

The shocked reaction of the public, scientific, and military communities to the Soviet coup had a very salutary effect on national decision-making. With the change in the nature of international politics introduced by the Sputnik launch, all constraints against using the Jupiter C-based rocket instantly vanished. Immediately, there was tremendous interest in backing up the then-lagging Vanguard program in order to join the Soviets as a space-faring nation as quickly as possible, and to increase the chances of a successful U.S. satellite launch during the IGY.

The last organizational details of the close collaboration between the ABMA and JPL organizations for proceeding with the Juno I satellite project were completed immediately. Von Braun and JPL Director William Pickering rushed to prepare a concrete proposal to Defense Secretary McElroy. They insisted that the satellite should be launched, not just as a technological demonstration, but also to meet a meaningful scientific objective. Because of the history of advanced collaboration and preparations between SUI and ABMA, and because we had packaged our cosmic-ray instrument to fit into both the Vanguard and the Juno I configurations, our instrument was the obvious choice for inclusion.

I was abruptly brought into this planning on 22 October by a telephone call from Eberhardt Rechtin at JPL. By that time, it had been decided that JPL would take over the satellite design.<sup>25</sup> Henry Richter brought a small delegation to Iowa City the next day (Rechtin was ill that day) to discuss our instrument's incorporation into the Jupiter C-based satellite. We agreed at that meeting that there would be a first flight of a limited instrument providing continuous transmission only, followed by a second flight with the full cosmic ray instrument with its data storage recorder. With this agreement, SUI became a full partner in the endeavor. By this time, the project was being referred to as Project Deal, with the first configuration known as Deal I, and the second, with the full instrument package, as Deal II.

Rechtin presented this ABMA/JPL/SUI proposal to Defense Secretary McElroy two days later, and McElroy gave his preliminary approval on 28 October. The plan was also presented to the National Committee for the IGY and the Vanguard Project, and they quickly agreed to it.

Also on 28 October, Rechtin finally asked me if I had the full authority to switch our project from Vanguard to the Jupiter C. I was taken aback by this question, since I had been proceeding fully in accord with our earlier planning and with what I believed would be Van Allen's wishes if he were present. As related earlier, Van Allen was essentially unreachable, as he was on the icebreaker *Glacier* in the Pacific Ocean.

When the question was phrased in that manner, I had to respond that I did not really have that authority. I arranged for JPL to contact Van Allen on his ship, but this turned out to be quite difficult. Because of the secret classification that had been imposed on the project to keep it out of the public limelight, the question could not be asked directly over the unencrypted communication channels. As a result, Van Allen was not sure what change was being proposed. Among other things, he wanted assurance that we would not lose control of the scientific experiment.

Having confidence that Van Allen would be in full accord with the change once he understood it, I continued with detailed design work. I made a hurried trip to JPL on 1-4 November, and we worked out many of the technical details and organizational arrangements. Among other things, the earlier plan for exclusive use of the Microlock communications system was expanded to include transmission over two channels, with reception and tracking by both the Microlock and Minitrack systems. This would provide additional redundancy for increased reliability.

While we were holding that meeting, on 3 November, Sputnik 2 was launched. It weighed 1,120 pounds and carried the dog Laika. This, of course, only increased the pressure for a quick U.S. response.

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With this added incentive, the Secretary of Defense gave his formal go-ahead for the project on 8 November. From that day forward, the JPL engineers proceeded at full speed to build the abbreviated version of the cosmic ray instrument for the first satellite. I was on the phone repeatedly to help extract the design of my detector and part of the electronic circuitry from the full package for this purpose. Although JPL took over primary responsibility for producing this instrument, we at the State University of Iowa maintained full control over the scientific performance of the instrument, including calibration of the detectors.

It was obvious that I would have to go to JPL if we were to be able to prepare both instrument packages in the short time envisioned. Eb Rechtin suggested that JPL could hire me and move me with my family to Pasadena for the duration. Although the terms seemed reasonable, I felt truly crippled by Van Allen's absence. Ernie Ray, as acting Department Head, did not believe that he had the authority to offer me University of Iowa employment while I was physically at JPL. Therefore, I accepted the JPL offer as the most expeditious solution, immediately withdrew from my classes, and began to arrange for the move.

Van Allen on the U.S. *Glacier* finally arrived in Port Lyttleton, New Zealand on 10 November, where he found another in the series of increasingly pressing messages from Pickering, "Urgently need your approval on proposed change of Ludwig experiment. Porter committee has given their approval to proposal to change experiment to JPL and to modify experiments as agreed upon between Ludwig and JPL." Still uneasy about his lack of specific information, Van Allen addressed a commercial cable to me at Iowa City on 13 November, asking, simply, "Question: Is Pickering plan for our experiment agreeable with you? Please cable answer IGY rep Christchurch." The following morning Ernie Ray answered the cable on my behalf, as I was fully occupied in preparing for the rush move. He wired, "After high level approval and obvious rearrangement of old program, George left town for extended stay. George quite happy [with] Pickering plans. Hope you say yes." <sup>26</sup>

With this reassurance, Van Allen immediately wired his approval to Pickering. This finally cleared the way for me to move my base of operation to California. Early the next morning, Friday, 15 November, I, with Rosalie, our two daughters Barbara and Sharon, personal possessions for a temporary home, my prototype instrument, and a store of flight instrument components, departed Iowa City for the drive west. Arriving there three days later, I set aside all of the usual formalities of signing in with a new employer and was immediately consumed by the task at hand.

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Van Allen returned to the Iowa City campus on 22 November, and I was able, at last, to brief him fully on the details of the change. I felt tremendously relieved to hand over all further responsibilities on the programmatic and political fronts, so that I could concentrate on the instrument preparation.

The JPL engineers, in addition to building and testing the instrument package for Deal I (the operating name for Explorer I before it was launched) were, of course, also responsible for many other program components. These included the satellite physical structure, its thermal control, batteries, the transmitters, the three upper rocket stages, the network of Microlock ground stations, integrating and testing all assemblies, and the necessary wide-ranging liaison with ABMA. I monitored the overall Deal I instrument preparations, and retained full responsibility for detector calibration.

However, my primary effort quickly became that of preparing our much more complex complete instrument package for Deal II, the second mission. My first action in that regard was to draw a complete set of the final circuit schematic diagrams that, until then, existed only in fragmentary form in my notebooks and other papers.

The JPL staff's challenge to produce the satellites was daunting. They had to (1) study my plans and learn all design details of the instrument (in effect, reverse engineer it), (2) breadboard the circuits and test them to assure themselves of satisfactory design (making some minor modifications in the process), (3) develop several modifications of the physical arrangement to accommodate the higher satellite spin rate, (4) order, process, and test the components and supplies, (5) design and integrate a Microlock-compatible transmitter as a replacement for NRL Minitrack transmitter, (6) set up a production line and assemble four copies of the hardware (an engineering prototype and three flight payloads), and (7) perform the full range of environmental and operational tests. They also had to integrate the elements produced by other collaborators. These included the NRL command receiver, the Army Signal Corps Electronics Laboratory's high voltage power supply, the Air Force Cambridge Research Center's micrometeorite instrument, and the University of Iowa-produced on-board tape recorder.

My personal responsibilities for Deal II were to: (1) transfer all of the information about our instrument design to the JPL engineers, (2) consult with them throughout the project, (3) coordinate with the outside organizations providing the elements of the Deal II instrument packages mentioned above, (4) manage the assembly and testing of several of the electronics circuit boards, (5) calibrate the GM counters

(for both Deal I and II), and (6) generally monitor all activities related to the successful scientific performance of both sets of flight instruments.

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The widely publicized and spectacular failure of Vanguard Test Vehicle 3 on 6 December added even more to the growing pressure for a quick and successful launch of the Deal satellites. The Deal I payload had to be ready for launch within less than three months from its approval in early November, and the complete Deal II cosmic ray package had to be ready less than a month later.

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This was an extremely hectic period, with the JPL staff and me working through many nights as the situation required. Rosalie carried most of the burden of running our household, even though she was in the third trimester of her pregnancy with our third child. She delivered our third child between the second and third Deal launches, and continued to carry most of the burden of running the household while caring for the new arrival.

### The Deal I Instrument and Explorer I

The progress in preparing the cosmic ray instruments for the first flight was very rapid by any standard. By 11 December, JPL had made excellent progress in breadboarding and testing the Deal I instrument and beginning the assembly of its flight units. Many of the arrangements for the network of Microlock and Minitrack tracking and data acquisition stations had been made.

A few key milestones will convey an impression of the rapid progress during the rest of December and January.

- 1 January 1958: The fabrication of the three sets of Deal I flight hardware components was essentially complete. Calibration of the GM counters was in progress. Over the Christmas holiday, we had resolved a high voltage corona discharge problem.
- 7 January 1958: Testing of the prototype payload at the type approval test levels was complete. These levels verified that the design margins were adequate for withstanding the launch and orbit environments.
- 11 January 1958: The first Deal I flight payload was completely assembled and weighed.
- 16 January 1958: A meeting was held at JPL to work out the final details for handling and distributing the telemetry data from both Deal I and Deal II.
- 17 January 1958: The launch rocket was hoisted into vertical position on Launch Pad 26A.
- 22 January 1958: I provided a full report on the status of our instrument preparations to Van Allen. All work on the Deal I instruments had been completed.

I delayed my flight to Cape Canaveral until the last possible minute because of the difficulties in calibrating the Deal II GM counters at JPL. I finally arrived there on 29 January, to find that all of the crew posts had been occupied. The first launch attempt that evening had to be scrubbed because of high upper-atmosphere winds. The next day I arranged with Roger Easton and Marty Votaw, senior NRL engineers in the Vanguard Project, to join them at their jerry-rigged Vanguard receiving station in Hangar S for the remaining attempts. I followed the progress during the second launch attempt by listening to the telemetered signal from my instrument via their Minitrack receiver while, at the same time, hearing the progress of the vehicle countdown from a loudspeaker connected to the Cape-wide intercom system. This second attempt also had to be scrubbed for the same reason.

For the third attempt on Friday, 31 January, the ABMA and JPL launch crews assembled once again in the blockhouse and other operational posts, and I took my position in Hangar S. Time was running out,

as the days during which the satellite could be launched into the desired orbit were waning. The launch time was set for 10:30 that evening, and at 8:30, the crews started to fuel the rocket.

The third countdown progressed steadily, with two short holds to investigate problems. At 10:45, Launch Director Kurt Debus gave the word to throw the switches to start the automatic firing sequence. A few seconds of silence, a burst of orange flame, a mighty roar, and at 10:48:16, the Jupiter C was airborne.

Sixty miles up, 156 seconds after takeoff, the first stage burned itself out. The three upper stages with the satellite payload separated from the booster and zoomed upward, spinning in their tub-shaped assembly in free-coasting, unpropelled flight, toward the apex. Ernst Stuhlinger had built a special "apex predictor" to determine the instant that the assembly would reach the top of its trajectory, at which instant the remaining three rocket stages had to be fired. His device, using inputs from radar, Doppler measurements, and rocket telemetry, was set up in one of the Cape's hangars. As backup, observing the various inputs, and using an array of complex charts prepared in advance, Ernst pushed a button hooked up in parallel with the computer-like predictor at the correct instant. This sent a radio signal to the speeding missile to fire the second stage. Off went the first cluster of scaled Sergeants, which quickly boosted the speed by thousands of miles per hour. Seconds later, the next cluster of rockets ignited, pushing the final-stage rocket, with its satellite, ever closer to that critical orbital velocity. Then the single rocket in the final stage ignited. Its thrust drove the 18.13-pound payload over the 18,000 mile per hour mark. Post-launch analysis revealed that his timing was impeccable, and Ernst has been known by his colleagues ever since as "the man with the golden finger."

Then came a long waiting period. The rockets had fired, but was the instrument in orbit? The new satellite had to complete the major portion of a full orbit before that could be determined with any certainty. By this time, I had moved from Hangar S to the Microlock ground station trailer that was in direct communication with the rest of the ground network. The time of expected signal acquisition came amid great expectation and excitement, but passed with the disappointing absence of any signal at any of the ground stations. During the next few minutes, we all waited with growing fear that the rocket or instrument might have failed. Finally, at about 42 minutes past midnight, just as my own worst fears were peaking, a voice from the trailer shouted, "Gold [code name for Earthquake Valley, California] has it!" There followed immediately reports that the other west coast stations were receiving the signal. After a brief silence as the reality set in, there was an outburst of shouts and hand shaking as the pent up emotions exploded. A few minutes later, at about 12:46 AM, the signal was picked up in the trailer where I was standing, where the new Earth satellite had completed its first full orbit.

Van Allen, von Braun, and Pickering, were in the Pentagon in Washington for the launch. They experienced a similar roller coaster of emotions. Von Braun's later account reads:

"The bird was due in California about 12:30 A.M., EST. We had four tracking stations there poised to pick up its signal, and Bill [Pickering] had them on the long-distance phone.

"Twelve-thirty came. There was no signal.

"A minute went by. And another. And another, without a beep from the satellite. Eight minutes elapsed and still they didn't hear a thing.

"We were miserable. Obviously, we'd been mistaken. The Explorer had never really gone into orbit. Then, all at once, within 30 seconds, all four California stations reported hearing the Explorer's signals! America's moon was definitely in orbit. There'd been just a slight error in our quick estimate of the satellite's initial speed and period of revolution."<sup>27</sup>

We all realized immediately that the rocket had provided a larger than expected thrust, resulting in a higher than planned orbit, and a longer orbital period. The orbit had been expected to have a perigee (lowest height above the Earth) of about 220 miles and an apogee (greatest height) of about 1000 miles. The perigee and apogee heights were actually 223 miles and, more significantly, 1592 miles, respectively, with an orbital period of 114.7 minutes rather than the 105 minutes that had been originally anticipated.

Van Allen provided his own typically succinct account of the emotional wait at the Pentagon in his book on the Origins of Magnetospheric Physics.

"...The burning of all four stages was monitored by down-range stations and judged to be nominal. The final burnout velocity of the fourth stage was somewhat higher than intended, and there was a significant uncertainty in the final direction of motion. Hence, the achievement of an orbit could not be established with confidence from the available data. The telemetry transmitter was operating properly, and the counting rate data from our radiation instrument corresponded to expectations... The reception of the telemetry signal after the lapse of one orbit was necessary before success could be confirmed. The nominal period of the orbit was ninety-five minutes,<sup>28</sup> and the first pass from west to east over northern Mexico was expected to provide the first clear opportunity for reception of the signal by stations in southern California.

"By previous arrangement I was a member of a group in the War Room of the Pentagon, which served as a center of communications. Others present included Wernher von Braun, Secretary of the Army Wilber M. Brucker, General Lyman L. Lemnitzer, General John B. Medaris, and William H. Pickering. For about an hour following receipt of the down-range station reports, there was an exasperating absence of information. Then there began a trickle of affirmative, amateur reports from around the world, none of which withstood critical scrutiny. The clock ticked away, and we all drank coffee to allay our collective anxiety. After some ninety minutes, all conversation ceased, and an air of dazed disappointment settled over the room. Then, nearly two hours after launch, a telephone report of confirmed reception of the radio signal by two professional stations in Earthquake Valley, California, was received. The roomful of people exploded with exultation, and everyone was pounding each other on the back with mutual congratulations."<sup>29</sup>

Immediate steps were taken to brief the press corps in Washington. The U.S. National Committee for the International Geophysical Year had insisted that the event be viewed not simply as an achievement of rocket technology, but as an achievement for science as well, and as a step in meeting the U.S. scientific commitments for the IGY. Van Allen's account of events immediately following the launch summarizes this event.

"...Pickering, von Braun, and I were whisked by an Army car from the Pentagon to the National Academy of Sciences and smuggled through a back door, where we made our preliminary report to Porter and the IGY staff. We were next led into the Great Hall of the Academy (by then about 1:30 A.M.) to report to the press. To my astonishment, the room was nearly filled with reporters, photographers, and many other interested persons who had been waiting there since about 10:00 P.M. The ensuing press conference was a spirited one. The successful launch of Explorer I was an event of major national and international interest, coming as it did after three humiliating launch failures of Vanguard."<sup>30</sup>

### **Deal II and Explorer II**

Back to work! How had the preparations for the more complete instrument progressed?

Throughout most of December and January, I concentrated most of my attention on finishing and testing circuits for the Deal II instrument package. By 11 December, the JPL engineers had completed some minor repairs of the prototype Vanguard package that I had brought from Iowa, put it in a fully operational condition, and begun a series of operational tests on it.

On that date, JPL held an all-hands meeting of the managers and senior engineers involved in the project. The participants were given their specific assignments and the instrument configuration, weight, and power allocations. The schedule was also announced, calling for the first flight satellite package to be shipped to Cape Canaveral on 10 February, for completion of environmental testing on the engineering design unit on 15 February, and for shipment of the second flight payload to the Cape on 17 February.

We completed a satisfactory temperature test of the Deal II instrument's scaling, timing, and recording circuits on 2 January 1958. I continued to clean up a few design details on the playback amplifier and tape recorder control circuits during the next two weeks.

Calibration was a top concern throughout the Deal instrument preparations. Van Allen underscored its importance on several occasions. In a note to me on 2 December 1957 he wrote,

"...Principal purpose of this note is to remind you of the essential importance of (a) Good effective length measurements on Geiger tubes, (b) Absolute efficiency of Geiger tubes for cosmic rays, (c) Counting rate vs. voltage curves (temp. fixed) and counting rate vs. temperature curves (voltage fixed)(properly for cosmic rays, but since data come in so slowly that way, at least for radioactive source!). <u>These tests must be made!...</u>" [Underline Van Allen's.]<sup>31</sup>

Getting good, repeatable, calibrations turned out to be vexatious. I was reasonably satisfied with the Deal I counter calibrations by the time those payloads were shipped to Cape Canaveral in mid-January. It was not until the end of January, just a few days before the Explorer I launch, that I obtained fully consistent results with the Deal II counters. The problem was that, until then, the laboratory test equipment available for my use at JPL was inappropriate for that purpose. It was only after receiving a special random pulse counter from Iowa that I was able to obtain fully reproducible and believable results.

By the first of February, the fabrication of the Deal II instruments was nearly complete. The first Flight Payload was fully assembled and had passed all environmental tests. I completed its final GM counter calibration on Saturday, 8 February, and they shipped that payload to the Cape on schedule the following Monday.

There, the JPL engineers made several last-minute modifications to the satellite's mechanical structure. These included the installation of a shorter cylindrical outer shell, substitution of a lighter magnesium instrument case for the aluminum one, and a lighter fretwork support for the low-power transmitter assembly. We were still concerned about achieving a sufficiently high orbit with a payload that was somewhat heavier than Explorer I, and they took every step imaginable to reduce its weight.

I arrived at the Cape on Sunday, 23 February, a week and a half before its scheduled launch. The JPL satellite crew and I concentrated on the detailed checkout of the three instrumented payloads. These tests included electrical performance, spin, and radiated power tests. I read and analyzed a seemingly endless stream of ground station recordings of the on-board data recorder throughout the pre-launch period.

My Journal entry at 4:20 AM on launch day, 5 March, reported:

"Have the equipment turned on. Payload activation in 40 minutes. Sky is light cloudy and broken – rather high. This is the day for which I have been working since January 1956. If successful, this is to provide my PhD thesis. I'll have to give that payload a goodbye pat." <sup>32</sup>

My Journal also reported somewhat later that there had been several difficulties during the countdown. At X minus 300 minutes the on-board tape recorder double-stepped, that is, for each drive pulse, the recorder tape advanced two steps. All later tests revealed normal stepping. The most serious problem from my point of view was the difficulty in commanding playback of the tape recorder. When the spin-up of the upper rocket stages was started at X-11 minutes, the recorder operated normally at first. But by the time the spin rate reached 550 revolutions per minute (out of 750 rpm needed for flight) we were unable to get a response from our radio commands for playback. The Launch Director interrupted the spin-up, slowed it down, and then increased the rate gradually. Playback was successful at 450 rpm but not at 500.

All of this was happening within the final minutes of the countdown, while the rocket sat there fully fueled and ready to go. The pressure for a final go/no-go decision was intense, as further delay would have meant canceling the launch for that evening and recycling for the following day or later. While we held up the launch for 18 minutes, the payload manager, other payload engineers, and I had a spirited

discussion, and concluded that the problem was with the on-pad commanding link, not the recorder itself. Specifically, we believed that there was a problem with the grounding path for the interrogating signal, and expected that operation would be normal once the rocket was free of the cluttered pad environment. We all agreed to proceed based on that assessment.

The official launch time was 1:28 PM, EST on Wednesday, 5 March 1958. Performance of the Redstone first stage booster rocket appeared to be normal throughout its burning. Later analyses indicated that the firing of stages one, two, and three were all normal. However, the fourth stage apparently failed to ignite, for reasons that were never completely determined, and the launch attempt failed. The satellite payload plummeted into the Atlantic Ocean about 1900 miles downrange from Cape Canaveral.

I left from Cape Canaveral for a one-day stop in Iowa City, and then quickly proceeded on to Pasadena to help prepare for the second attempt to launch our full satellite instrument.

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The first order of business upon arriving home following the unsuccessful Explorer II launch was to check on Rosalie, whose time for delivery was fast approaching. I had been away from 20 February until 9 March, a long two and a half weeks considering that our third child made his appearance only nine days after my return.

### **Explorer III**

On Monday, 10 March, it was back to the Laboratory. It had been agreed immediately that a second launch attempt would be made as quickly as possible. Beginning at that time, the failed Explorer II mission was referred to as Deal IIa, and the new attempt was dubbed Deal IIb.

We began work on the three remaining Deal II satellite instruments, with only two weeks to prepare the payloads for the new launch attempt. As a high priority, we addressed the tape recorder difficulties encountered during the Deal IIa countdown. I discovered that the double-stepping of the Deal IIa tape recorder was due to over-travel of the tape-advance solenoid, and we readjusted stops on the remaining recorders to prevent a recurrence. We also spent considerable time in fine-tuning other adjustments in the recorders to assure more reliable operation.

As for the difficulties in interrogating the tape recorder during the final stages of the Deal IIa launch countdown, the JPL engineers made many tests and analyses of the radio frequency system. One possibility, that the radiation pattern of the satellite high power antenna might have been distorted, was quickly eliminated. The second possibility was that receiver sensitivity might have been too low, at least partly a byproduct of electrical noise generated by the spin motor for the upper-stage tub. The command receiver sensitivity was increased, and other arrangements were made at the Cape for increasing the signal-to-noise ratio in the neighborhood of the launch gantry.

A substantial change was made in one of the two satellite antennas. Explorer I had employed a socalled "turnstile" antenna for the high-power transmitter. It consisted of four flexible stainless steel cables, with the base of each cable anchored at the antenna insulator. It had been expected that these flexible whip elements would be held in axially semi-rigid positions by the centrifugal force resulting from the spinning of the satellite. It turned out that the whips quickly began a swinging motion that dissipated the satellite's rotational energy, causing it to tumble eventually end over end. This problem had not been fully understood when Deal IIa/Explorer II was launched, so that payload retained the original whip antenna. The situation was better understood by mid-March, and the JPL engineers undertook a crash program to change the antenna for the high power transmitter and command receiver. This change was designed, fabricated, tested, and installed on all three payloads during the short time available. The Vanguard Program finally had its first success, with the launch of their small spherical satellite. Launched on 17 March, it was six and a half inches in diameter, and soon became known in the press reports as the "grapefruit" satellite.

Our third child arrived on Tuesday, 18 March 1958 in the midst of the hurried satellite preparations. Rosalie returned home from the hospital with son, George Vickers Ludwig, three days later. Two days after that I left again for Cape Canaveral for the Deal IIb launch preparations. Thankfully, Rosalie's mother arrived at our Pasadena home that evening to help Rosalie during my absence.

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I arrived at Cocoa Beach late that Sunday, 23 March, and made my way to the Cape the next morning, where I learned that the JPL engineers had experienced continuing payload problems. Several of the instrument packages appeared to interrogate themselves spontaneously – the result of the increased satellite receiver sensitivity combined with the complex radio frequency environment at the Cape. Additional filters were installed in the payloads to prevent this. There was a series of problems with the data tape recorder in Payload III. Fortunately, the one in Payload II worked perfectly throughout the prelaunch testing, and that payload was selected for launching. It continued to operate perfectly on launch day, and its operation in orbit was flawless throughout the satellite's operating lifetime.

The launch countdown was a reprise of the Deal IIa launch, except that it proceeded with even fewer hitches. Liftoff occurred at 12:38 EST on Wednesday, 26 March 1958, only a few minutes after its scheduled time. The rocket disappeared quickly into low cloud cover, and final injection into orbit occurred at 12:46. As the payload disappeared from radio range, the Doppler shift looked good, signifying a proper velocity in its path away from the Cape. We soon learned that the downrange Antigua Minitrack station made a successful interrogation of the on-board tape recorder immediately after the final rocket firing. A Microlock tracking station at Johannesburg, South Africa acquired the low-power signal as the satellite passed overhead, but it was too early in the orbit to reveal whether or not the satellite had achieved a durable orbit.

As in the case of Explorer I, there was a long delay before we could confirm that the payload was in orbit. That confirmation came about two hours after liftoff. It was provided by the reception of the low-power transmitter signal at the Microlock stations at JPL, Earthquake Valley, and Temple City, and at the Vanguard project's west coast San Diego Minitrack station. Determining whether the full cosmic ray instrument package was operating properly took more time, however. Only the Vanguard Minitrack stations had the capability for commanding the readout of the on-board recorder. San Diego attempted repeatedly to interrogate the recorder playback on this first pass, but they were unable to detect any response. Hearing this, my heart sank, as recovery of the on-board stored data was essential if we were to achieve our full objectives. For the second pass, the Minitrack station at Quito, Ecuador had the primary ground station responsibility. They made a series of ten command transmissions, but were unable to detect any the Lima, Peru station, marginally within range, might have heard a faint response.

That was the situation as I left Cape Canaveral. It was not until the following early afternoon in Huntsville, when I was able to get back in contact with knowledgeable ground station personnel in Washington, that I learned with tremendous relief that subsequent attempts to read the in-flight recorder had produced better results. By that time, receipt of on-board recorder data dumps had been reported from five out of twelve satellite passes, and I was ecstatic.

Explorer III's final velocity was higher than the planned value, so that its maximum orbital height (apogee) was quite high. We found, however, that the final stage was pitched up slightly from the horizontal when it fired, resulting in a somewhat lower minimum height (perigee) than planned. The first computations at the NRL Computing Center on Washington's Pennsylvania Avenue indicated that the

perigee height would be about 60 miles, which would result in only a two-week's orbital lifetime before the satellite's velocity would be slowed by atmospheric drag and it would plunge earthward. By the next day, however, a greater accumulation of tracking data and hard through-the-night work by the orbital computation team yielded more accurate orbit parameters, including a perigee height of about 125 miles and an apogee height of 1750 miles. With these new values, the satellite orbital lifetime would be from three to six months, plenty of time for us to conduct our experiment. With the higher than planned apogee, the satellite actually turned out to be even more useful in detecting and delineating the Earth's radiation belts.

My return home was slowed by the need to stop at Huntsville for planning on what eventually became Explorer VII. I finally reached Iowa City on Saturday, 29 March, leaving again on Monday morning. During that weekend I had a number of conversations with Van Allen and the others, during which our main topics of conversation were the new satellite instrument that I was designing, and, of course, the anomalous data from Explorer I.

The amount of Explorer I data received by the time of this gathering was still meager, and only fragmentary snapshots of the counter readings in space were available. These did continue to show, however, the unexplainable pattern of normal, high, and zero counting rates. The first data from Explorer III had not yet arrived, and we were still unable to account for Explorer I's strange counting rates over several of the South American receiving stations.

A few days after I returned to Pasadena, the first Explorer III data were read by the Iowa group, and the puzzle of the strange satellite data was quickly resolved.

The story of the discovery of the Van Allen Radiation Belts will be resumed in the next lecture by Carl McIlwain.

### Perspectives

My participation in the Explorer I and III programs was the supreme highlight of my professional life. Never since then have I been so completely immersed in a project so demanding, and yet so exciting and rewarding.

I marvel to this day that I, as a largely untested beginning graduate student, was given so much freedom and responsibility for such an important endeavor. My admiration for James Van Allen, with his willingness to take great risks with his graduate students, is unbounded.

Thanks, Van, for the wonderful opportunity.

<sup>&</sup>lt;sup>1</sup> Memorandum: <u>Outline of a Proposed Cosmic Ray Experiment for Use in a Satellite</u>. Marked preliminary, dated 1 November 1954, with the signature block by J. A. Van Allen. No addressee indicated, but probably sent to Ernst Stuhlinger.

<sup>&</sup>lt;sup>2</sup> Much of this and additional material about the V-2 program is contained in several excellent published works. The first that came to my attention remains among my favorite accounts of early German rocket development: Dornberger, Walter. <u>V-2</u>, Translated by James Cleugh and Geoffrey Halliday, with introduction by Willy Ley, The Viking Press, New York, NY, 1958.

See also: Bergaust, Erik. <u>Wernher von Braun.</u> National Space Institute, Washington, DC, 1976. For an excellent more recent work, see: Stuhlinger, Ernst and Ordway, Frederick I. <u>Wernher von Braun</u>, Crusader for Space: A Biographical Memoir, Krieger Publishing Company, Malabar, Florida, 1996.

<sup>&</sup>lt;sup>3</sup> Stuhlinger, Ernst and Ordway, Frederick I., III. <u>Wernher von Braun – A Biographical Memoir</u>. p. 124. Krieger Publishing Company, Malabar, Florida. 1996.

<sup>4</sup> Van Allen, James A. <u>Origins of Magnetospheric Physics</u>, p. 49. Smithsonian Institution Press, Washington, DC, 1983.

<sup>5</sup> Stuhlinger, Ernst. <u>Discovery of the Van Allen Belts – Memories of an Old-Timer</u>. Lecture delivered at the University of Iowa, Iowa City, 27 June 1998.

<sup>6</sup> Stuhlinger, Ernst and Ordway, Frederick I., III. <u>Wernher von Braun, Crusader for Space</u>, p. 139. Krieger Publishing Company, Malabar, Florida. 1996.

<sup>7</sup> A full authoritative story of the competition and decision is contained in: Green, Constance McLaughlin and Lomask, Milton. <u>Vanguard – A History</u>, Chapter 2. National Aeronautics and Space Administration Special Report SP-4202. Scientific and Technical Information Division, NASA, Washington, DC. 1970.

For the same story from the Army's perspective, see: Stuhlinger, Ernst and Ordway, Frederick I., III. <u>Wernher von Braun – A Biographical Memoir</u>. Chapters 5 and 6. Krieger Publishing Company, Malabar, Florida. 1996.

See also: Koppes, Clayton R. <u>JPL and the American Space Program – A History of the Jet Propulsion</u> <u>Laboratory</u>, Chapter 6. Yale University Press, New Haven. 1982.

<sup>8</sup> Van Allen, James A. <u>Origins of Magnetospheric Physics</u>, p. 54. Smithsonian Institution Press, Washington, DC, 1983.

<sup>9</sup> Van Allen, James A. <u>Energetic Particles in the Earth's External Magnetic Field</u>, History of Geophysics, Vol. 7, p. 238. American Geophysical Union, 1997.

<sup>10</sup> Van Allen, James A., Ed. <u>Scientific Uses of Earth Satellites</u>. University of Michigan Press, Ann Arbor, 1956. Second Edition, 1958.

<sup>11</sup> Van Allen, James A., <u>Cosmic-Ray Observations in Earth Satellites</u>, Chapter 20, p. 171 ff., in Van Allen, James A., Ed. <u>Scientific Uses of Earth Satellites</u>. University of Michigan Press, Ann Arbor, 1956. Second Edition, 1958.

<sup>12</sup> Van Allen, James A., <u>Cosmic-Ray Observations in Earth Satellites</u>, Chapter 21, p. 188 ff., in Van Allen, James A., Ed. <u>Scientific Uses of Earth Satellites</u>. University of Michigan Press, Ann Arbor, 1956. Second Edition, 1958.

<sup>13</sup> Ludwig, George H. and Van Allen, James A. <u>Instrumentation for a Cosmic Ray Experiment for the Minimal</u> <u>Earth Satellite</u>. Unpublished. State University of Iowa Department of Physics, 30 May 1956.

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<sup>14</sup> Van Allen, James A. <u>Origins of Magnetospheric Physics</u>, p. 42. Smithsonian Institution Press, Washington, DC, 1983.

<sup>15</sup> Ludwig, G. H. and Van Allen, J. A. <u>Status Report on the SUI Cosmic Ray Instrumentation</u>. Unpublished report, State University of Iowa. 19 April 1957.

<sup>16</sup> Ludwig, George H. and Van Allen, James A. <u>The State University of Iowa Cosmic Ray Earth Satellite</u> <u>Experiment</u>. Paper presented at the Conference on Rockets and Satellites, Session C on Orbiting Satellite Devices and Internal Instrumentation, held on 2 October 1957. The paper is dated 24 September 1957.

<sup>17</sup> Ludwig, George H. <u>Quarterly Progress Report on ESP-1, Grant Number Y/32.1/147</u>. State University of Iowa, Department of Physics. 30 October 1957.

<sup>18</sup> An interesting more detailed account of this event is contained in: Sullivan, Walter. <u>Assault on the</u> <u>Unknown</u>, pp. 1-2. McGraw-Hill Book Company, Inc., New York, 1961.

<sup>19</sup> Stuhlinger, Ernst and Ordway, Frederick I., III. <u>Wernher von Braun – A Biographical Memoir</u>. p. 128. Krieger Publishing Company, Malabar, Florida. 1996.

<sup>20</sup> Letter, Ernst Stuhlinger to George H. Ludwig, 3 December 2002.

This event is also related in: Stuhlinger, Ernst and Ordway, Frederick I., III. <u>Wernher von Braun – A</u> <u>Biographical Memoir</u>. p. 130. Krieger Publishing Company, Malabar, Florida. 1996.

<sup>21</sup> Van Allen, James A. Letter to Ernst Stuhlinger, dated 13 Feb 1957.

<sup>22</sup> From an interview with Van Allen, cited in: Green, C. M. and Lomask, M. <u>Vanguard - A History</u>, p. 113 ff., NASA Historical Series publication SP-4202, 1970.

<sup>23</sup> Van Allen, James A. Letter to the Technical Panel on the Earth Satellite Program, dated 23 Jan 1956 and revised 31 Jan 1956.

<sup>24</sup> Ludwig, George H. Letter to Commanding General, ABMA, Attn: Mr. Boehn (sic), dated 1 May 1957.

<sup>25</sup> This was the subject of some debate that is described in: Stuhlinger, Ernst and Ordway, Frederick I., III. <u>Wernher von Braun – A Biographical Memoir</u>. pp. 135-136. Krieger Publishing Company, Malabar, Florida. 1996.

<sup>26</sup> Van Allen, James A. <u>Origins of Magnetospheric Physics</u>, p. 56. Smithsonian Institution Press, Washington, DC, 1983.

<sup>27</sup> Von Braun, Wernher. <u>The Story Behind the "Explorers."</u> Des Moines Sunday Register, This Week Magazine, p. 38, 13 Apr 1958.

<sup>28</sup> Actually, this was the time required for the satellite to approach the west coast, not the full orbital period, i.e., the time required for the satellite to reach its starting latitude.

<sup>29</sup> Van Allen, James A. <u>Origins of Magnetospheric Physics</u>, pp. 57-59. Smithsonian Institution Press, Washington, DC, 1983.

<sup>30</sup> Van Allen, James A. <u>Origins of Magnetospheric Physics</u>, pp. 59-60. Smithsonian Institution Press, Washington, DC, 1983.

<sup>31</sup> Handwritten note, JAVA to George, dated 2 December 1957.

<sup>32</sup> Ludwig, George H. Journal, Jan. 1958 to 23 Aug. 1960, entry dated 5 March 1958.