A Partial History of Orbital Debris: A Personal View[1]

by
Donald J. Kessler

On February 11, 1988, President Reagan signed a Presidential Directive on National Space Policy. This Directive established the policy that all space sectors will seek to minimize the creation of space debris. The Directive also established a process to implement that policy. For some, this Presidential Directive established the basis of the orbital debris work presently being conducted. For others, it was simply a milestone in an existing orbital debris program. As one of those who looked at the Presidential Directive as a milestone, I would like to give my perspective of how we got to where we are today. This perspective is by no means complete, or even unbiased; I am aware of many significant contributions to understanding orbital debris that is not included in this history. The contributions I have included are those that, in my view, were most significant leading up to the Presidential Directive.

To fully understand the history of orbital debris requires an understanding of some of the meteoroid hazard issues that were developed during the early part of the space program. Before the mid-1970's, orbital debris and meteoroid hazards were thought of and treated differently. However, it wasn't until meteoroid skills and perceptions were applied to orbital debris that the orbital debris problem could be placed into a the context that is currently accepted today. Consequently, I have included a brief background of some of those early meteoroid activities.

Orbital debris activities can be broken into four periods of time: 1. The Limited Studies Period (1966-1972) 2. A Transition Period (1974-1979) 3. The Program Development Period (1979-1988), and 4. The Post Presidential Directive Period (1988-present). The first period is characterized by a preoccupation with calculating the collision probability of catalogued objects with relatively large spacecraft. The Transition Period consisted of work at Langley Research Center which established a foundation for predictions of an uncatalogued population and work at the Johnson Space Center where a significant future uncatalogued population was first predicted. It was during this period that the orbital debris hazard was compared to the meteoroid hazard. During the Program Development Period, the hazards from uncatalogued debris were emphasized together with the need to change certain types of operations in space. The current "post directive" period is one of cooperation among national agencies and international groups to more accurately characterize the orbital debris environment, and to prioritize future techniques to control the environment.

DEVELOPMENT OF AN UNDERSTANDING OF THE METEOROID HAZARD

Associated with the commitment to send people into space was also a commitment to understand the hazards associated with the environment of space. One of those hazards was from meteoroids. Large meteoroids were observed when they collided with the Earth's atmosphere and caused a meteor; meteoroid dust could be seen as a milky-way type of glow in the sky, known as the zodiacal

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light. However, it was not known what size meteoroids caused these phenomena, at what speed meteoroids may be traveling, nor what type of damage they may cause if they collided with a spacecraft. Consequently, some of the first studies, ground experiments, and spacecraft experiments associated with the space program had the objective of characterizing the meteoroid hazard.

By the early 1960's nearly every NASA center had a meteoroid research group. Marshall Space Flight Center, the Langley Research Center, Ames Research Center, and the Manned Spacecraft Center (now called the Johnson Space Center) built light-gas guns to simulate meteoroid impacts onto spacecraft surfaces. Goddard Space Flight Center used a Van de Graaff generator to accelerate dust to hypervelocities. In order to characterize the environment, each of the NASA centers was conducting various combinations of studies and experiments. These combinations included theoretical studies to understand the size of meteoroids responsible for meteors and the zodiacal light, ground observation stations to either photograph or measure with radar the intensity and orbits of meteors, and space flight experiments to measure the meteoroid impact rates. Early flight experiments both in the United States and the Soviet Union measured a meteoroid environment that was much higher than any predictions. The measured environment was so high that if this environment had been characteristic of interplanetary space, then the zodiacal light would be much brighter. This led to the theory that the measured meteoroids were in Earth orbit rather than in interplanetary orbits. This theory began to fall apart with the launch of Explorer 16 in December, 1962.

Explorer 16, followed by Explorer 23 and Pegasus launched over the next 3 years, all included a more reliable type of meteoroid sensor...detection required penetration of the sensor by the meteoroid. These satellites measured a meteoroid environment that was between 2 and 4 orders of magnitude smaller than had the acoustic sensors on the earlier satellites. Examination of the recovered Mercury and Gemini windows for hypervelocity impact pits, as well as data from additional spacecraft, all supported the lower meteoroid hazard. By 1967, the early sensors had been shown to be subject to thermal noise, and the lower environment was generally accepted as representing the actual hazard to spacecraft. Consequently, the theory of a natural meteoroid environment in Earth orbit was no longer supported. Since 1967, there have been few changes to our understanding of the meteoroid hazard to spacecraft.

The meteoroid environment as understood in 1967 represented only a minor hazard for most spacecraft. Adequate protection against the environment could be handled by the spacecraft structure, or a relatively minor amount of additional shielding; for example, a very minor amount of protection was added to the Apollo LEM and space suit. That was not the case for large spacecraft that were in space for long periods of time, especially where a high reliability was desired, as for Skylab. Skylab, which was designed in the late 1960's and launched in 1973, required a meteoroid bumper shield to be added to the design in order to meet a desired reliability. The current Russian MIR space station uses a similar shield to protect against meteoroids. However, after the meteoroid environment was defined and Skylab was designed, there was no longer any reason for NASA to continue to research the meteoroid hazard...the environment hazard had proven to be low for all but space station types of missions, and NASA had no future plans to construct a space station. By 1970, funding for meteoroid research begin to disappear. Eventually, nearly all of the NASA meteoroid research team members moved on to other jobs. Some members later returned to study the contribution of meteoroids in the origin and evolution of the solar system, while others returned to research orbital debris. Those that returned to address orbital debris did not do so until after the Limited Studies Period of orbital debris.

LIMITED STUDIES PERIOD OF ORBITAL DEBRIS

The earliest serious calculations to characterize the orbital debris hazard were made in about 1966. These calculations were performed by specialists in orbital dynamics, with the purpose of determining hazard for manned spacecraft by calculating the collision probability resulting from other man-made objects known to be in Earth orbit. These earliest studies assumed that the largest uncertainty in the hazard resulted from the technique of calculating collision probabilities rather than from the actual number of objects in Earth orbit. Under this assumption, the hazard was found to be relatively small; consequently, these studies were always limited in their scope.

One of the first such studies supported the Apollo Program and was published as an Internal Note in 1967 by the Flight Analysis Branch at NASA's Manned Spacecraft Center. The study used two different techniques to calculate collision probabilities; these two techniques resulted in collision probabilities that were less than a factor of 2 apart. However, the study also indicated "that the number of untrackable fragments, which result from explosions of satellites in Earth orbit and whose radar cross section areas are too small to be tracked by NORAD, constitutes an insignificant increase in the total number of objects in Earth orbit and hence can be neglected in the calculation of collision probability." This type of thinking contaminated all of the early orbital debris work.

A 1970 Internal Note by Michael Donahoo, also from the Flight Analysis Branch, updated these results to apply them to Skylab and a space station. Whereas the assumed 12-day Apollo mission had a very low probability of collision, the larger size and longer duration of Skylab and the even larger 10-year space station, not surprisingly, was found to have a significantly higher probability of collision. At about the same time, James McCarter at NASA's Marshall Space Flight Center was also calculating collision probabilities (his results were later published in a NASA TMX). McCarter assumed "that a miss distance of 50 m from the center of the Space Station would represent either a collision or a very near miss." Although this may seem like an obvious truth, it was quickly forgotten that a miss is a miss, and not a collision; consequently McCarter's results was interpreted that the "collision probability" for a space station was nearly 10%. These higher collision probabilities got the attention of NASA Headquarters. In 1971, Morton Shaw at NASA Hq recognized that an uncatalogued population could increase these probabilities farther, so he expressed plans in a file memo to organize a working group to pursue a program plan. The responsibility for future orbital debris work was given to Marshall. Marshall continued to develop computer programs to calculate collision probabilities, but did not consider the possibility of a significant uncatalogued population. I can find no significant results from this program after 1972.

These early studies did not develop, I believe, for a number of reasons. First, they missed a major point...the main hazard from orbital debris was not from the catalogued objects, but from smaller, uncatalogued objects. This may have resulted from the fact that these studies were conducted by specialists in orbital dynamics; their main emphasis was to understand collision probability rather than other key elements, such as satellite breakups, detection of small debris, the consequences of hypervelocity collisions, and debris management. Finally, there were many false impressions by both the public and high level DOD and NASA management about orbital debris that were simply untrue. Some of these false impressions were "NORAD is tracking all man-made objects. Space is infinite. Space is self-cleaning. Objects in space float relative to one another rather than collide at high velocities. Objects placed in space remain intact. Debris control means limiting the number of payloads in orbit." These impressions had to change before a program could develop, and began to change during the Transition Period.

THE TRANSITION PERIOD

By 1974, another NASA group began working orbital debris. Dave Brooks at Langley also developed a technique for calculating collision probabilities. However, he combined his orbital knowledge with some data from ground fragmentation tests gathered by Dale Bess. The resulting 1974 presentation at the International Astronautical Federation (published in 1975 by AAS) not only included collision probability calculation results, but for the first time, included a predication for an uncatalogued population of millimeter-size debris which was 2.5 times the catalogued population...a small factor by today’s standards. Unfortunately, due to a computer program error, Brooks's published collision probabilities for millimeter debris were less than previous calculations for catalogued debris. However, follow-on work by this Langley group provided a valuable transition from the Limited Studies Period to the Program Development Period. Dave Brooks, Dale Bess and Joe Alvarez, all working under Bill Kinard at Langley, developed a foundation for future orbital debris work. This group of researchers was fundamentally different from previous researchers in that some had a previous history of research in defining the meteoroid environment. They developed the techniques of meteoroid research to conduct tests and gather data, and they sponsored studies that later became valuable tools in developing the orbital debris program. Dale Bess compiled more data on ground fragmentations and conducted some hypervelocity impact tests into spacecraft structures to determine the amount of debris generated. Langley sponsored a study by General Electric to determine the feasibility of using various remote sensing techniques to detect smaller orbital debris...one such technique, the use of ground telescopes, is successfully used today. A study by the Naval Research Laboratory described the orbital motion of fragments resulting from an explosion. However, by 1976, funding from NASA Hq for the Langley studies was no longer available, and no further work was completed at Langley. The reason for this may be that even though the Langley studies were obviously in the right direction, they were not yet able to show that orbital debris might have a significant effect on current, or future, space operations.

My own work in orbital debris began independent of these earlier studies. In some way, this work began in 1963 as a cooperative education student, the day I reported to a new boss, Burton Cour-Palais. Burt was part of the Manned Spacecraft Center's meteoroid research group to characterize the meteoroid hazard for manned programs. Burt explained to me that NASA was beginning to plan a manned mission to Mars, and he wanted me to determine the meteoroid environment for a mission that would pass through the edges of the asteroid belt before arriving at Mars. I knew nothing about fluxes or calculating collision probabilities; however, because of my interest in astronomy, I understood a little about asteroids, meteoroids, and orbital mechanics. I remembered that environments were sometimes described in terms of number density, so I set out to determine the spatial density of asteroids, and then extrapolated the size distribution to smaller meteoroid sizes. When I completed this assignment, I was given a new one: explain what, if anything, spatial density had to do with fluxes and collision probabilities that result from the meteoroid environment.

Anyone who was familiar with the kinetic theory of gases would know that fluxes are related to spatial density through the relative collision velocity; however, this concept had not yet been applied to orbiting objects. By 1970, I had derived my own set of collision probability equations using the concept of spatial density, and was able to show that these equations were a more general form of Öpik's equations, which were published in 1951 and were widely used to predict asteroid, comet and meteoroid collision probabilities with the Earth. I had also found that the spatial density approach allowed for accurate approximations which made the calculation of fluxes resulting from a large number of objects easier. These equations were used to define an interplanetary meteoroid environment, a key part of which results from random collisions between large asteroids. I became curious about Earth-orbiting satellites...I wondered how long it would take for a miniature asteroid belt to form around the Earth from the collisions of man-made satellites. I began to realize that orbital debris might be important when I calculated that the probability for a spacecraft to collide with another orbiting spacecraft was about the same as the probability of being hit by a 1 gm meteoroid. However, I was not able to continue this work because of NASA cut-backs. In October, 1970, my Division was abolished. In order for me to continue to work at NASA, I had to work in areas that I found less interesting. As Sub-system Manager for Apollo Meteoroid Protection, Burt was the only person in the Division to continue his job for a few more years, before he too had to find a less interesting job.

In 1976, as a result of the energy shortage, NASA was looking into the possibility of constructing large solar power stations in low Earth orbit, and then transferring them to geosynchronous orbit where they would be operated. Both Burt Cour-Palais and I were working in the Environmental Effects Project Office...an office set up to understand the environmental impact on the Earth's troposphere and stratosphere from Space Shuttle launches. The office was headed by Andrew Potter. I was asked by Drew Potter to look into the environmental consequences of building and operating these solar power stations. Reasoning that a break-up of one of these stations would not be good for the space environment, I saw this as an opportunity to continue looking into orbital debris. It quickly became obvious that orbital debris was an environmental issue not only for large stations in orbit, but one that could affect all spacecraft in the relatively near future. In August, 1976, I completed an Internal Note "Space Debris - Environmental Assessment Needed", concluding that unrestrained launch activities would inevitably lead to an uncontrollable increase in debris due to collisional fragmentation. Through Joe Alvarez at Langley, I learned of a PARC radar test by NORAD that proved that there already was an uncatalogued population. In September, 1976, I attended a briefing discussing the results of that test, and met Preston Landry from NORAD. Close examination of this data and discussions with Preston concerning NORAD's capabilities suggested that a significant uncatalogued population might already exist, and NORAD could help acquire that data. Drew Potter suggested that I request funding through the Assistant to the Center Director, Joe Loftus, in order to continue this work. Joe immediately supported the orbital debris research and helped establish a closer relationship with NORAD. However, he was not able to develop a funding source at NASA Hq. Burt Cour-Palais and I were able to complete the orbital debris work which predicted that random collisions alone could cause an environment of fragments that would be more hazardous than the meteoroid environment by as early as the 1990's. However, just after that work was submitted for publication in the Journal of Geophysical Research (JGR), my division was again abolished, and I again had to work in an area that I found less interesting. On December 19, 1977 the Center Director was briefed on the orbital debris work and accepted a Joe Loftus recommendation that I be allowed to spend 10-20% of my time on orbital debris. My new boss limited my time to 10%.

Between 1978 and 1979, many things happened that increased NASA's awareness of orbital debris as well as confidence in the predictions in our JGR paper, published in June, 1978. The reentry of COSMOS 954 caused the Secretary of State to inquire about hazardous objects in Earth orbit. One of those objects was Skylab, also about to reenter...like a 2nd debris awareness shoe falling. Dr. Brown of the Hudson Institute mentioned the JGR paper, before it was published, to both Senate and House subcommittee meetings on the future of space. This contributed to a number of newspaper and magazine articles on space debris where the predictions of the JGR paper were quoted. Also, an assistant to the NASA Administrator, Phil Culbertson, was involved with the SALT II talks, and the issue of orbital debris was raised during those meetings. One theory concerning COSMOS 954 was that it had collided with something, leading to loss of control of the satellite. This brought orbital debris to the attention of the Chief of the Space Affairs Division at the United Nations, Lubos Perek, who then researched previous work at Langley and the work in the JGR paper. Val Chobotov at Aerospace Corporation used the JGR paper to illustrate that large structures in space had to face an orbital debris problem; this resulted in the beginning of an orbital debris program at Aerospace. Also, NORAD conducted another "Small Satellite Test" using the PARC radar and found again that they were not cataloguing "all man-made objects", as generally believed. As a result of this test, I traveled to NORAD and met John Gabbard, who showed me the records of breakups that he had unofficially maintained and his plots of perigee and apogee vs. orbital period, that we later called "Gabbard Plots." These plots became valuable tools for identifying characteristics of a breakup. He also educated me on the cataloging process. Finally, using a 4% random sample (I did not have access to a computer data base), John's education of the cataloging process, and a list of which rockets were used to place various objects into orbit, I discovered that 6 Delta rocket launches were responsible for 20% of all catalogued objects in orbit. Joe Loftus passed this information to the Office of Expendable Launch Vehicles at NASA Headquarters, where they requested Battelle Institute to review the entire subject of orbital debris, including the the JGR paper. One of the reviewers was Robert Reynolds, who eventually became a key part of the current orbital debris program.

All of this activity was increasing the demand on my orbital debris time well beyond the allowed 10% level, and my new organization was actively resisting any expansion of the program. I felt it was time to establish a charter for orbital debris work, and requested help from Joe Loftus. Joe set up another formal briefing to the Center Director with his senior management. At that briefing on March 26, 1979, I presented the orbital debris technical issues including the new results and the implications to operations in space. One of the issues was JSC's role in any future orbital debris work. When asked whether we should continue orbital debris work, Director Kraft stated that "We would be crazy not to continue...go do it...forthwith!" As far as I am concerned, this was the directive that allowed an orbital debris program to be developed.

PROGRAM DEVELOPMENT PERIOD

Under Kraft's directive, an orbital debris team at JSC was formally recognized. The team was responsible for developing the program, obtaining additional data, and educating and seeking support from other US Government Agencies. For the first few years, this team consisted of Joe Loftus, Dennis Fielder, Burt Cour-Palais, Drew Potter, John Stanley and me; originally I was the only full-time member. However, there were major goals to be accomplished before any program would work. There was still no firm commitment from NASA Hq. for a program; there was no national recognition that an orbital debris problem existed...in fact, most agencies were highly skeptical. Internationally, the subject had received even less attention. Finally, and perhaps most importantly, there was little hard data supporting the idea that a significant orbital debris population did, or could, exist.

Funding from NASA Hq. begin in October, 1979, with $70K from the Advanced Programs Office, headed by Ivan Bekey, under the Office of Space Transportation and Operations. This funding source was more strongly established in June 28, 1980 in a note from Phil Culbertson to the Deputy Administrator, Dr. Lovelace. Culbertson suggested that the Office of Space Transportation and Operations should be responsible for maintaining an overview of orbital debris in low Earth orbit, and that the Office of Tracking and Data Acquisition be responsible for geosynchronous orbit; the Deputy Administrator agreed. Although there have been several reorganizations, and the Office of Tracking has transferred their responsibility, the Advanced Program Office is still the major orbital debris funding source and maintains an overview of the programs for both low Earth orbit and geosynchronous orbit.

Also in June of 1980, the AIAA Technical Committee on Space Systems, chaired by Dick Kline from Grumman Aerospace Corporation, began a formal review of space debris, with the objective of writing an AIAA Position Paper. Two of the members of that committee were Ivan Bekey and Malcolm Wolfe from Aerospace. Malcolm Wolfe was given the task of organizing the paper. Bob Reynolds, Val Chobotov and I supplied Malcolm with the necessary technical data, and the AIAA Space Debris Position Paper was released the following year. This position paper was the first to address orbital debris as a national issue; however, this by no means meant that debris was accepted as a national issue.

Although a significant amount of work had been completed to understand the Delta 2nd stages explosions, no operational changes had yet been made. On January 30, 1981, Preston Landry and John Gabbard called me to report that on January 27, another Delta 2nd stage had exploded, this one after 3 years in orbit. Joe Loftus had me prepare a memo giving all details of the explosion, which he passed on to NASA Hq. By March, 1981, the Director of Expendable Launch Vehicles, Joe Mahon, requested Goddard take action to reduce the probability of future explosions in orbit, which they did. Since 1981, after completing the primary mission, all Delta 2nd stages have depleted any remaining propellants. Since Japan used a rocket similar to the Delta, Joe Loftus suggested to Japan that they adopt a similar procedure, which they did. No Delta 2nd stages launched after 1981 nor any Japanese upper stages have exploded in orbit.

In 1981, Joe Loftus requested that we prepare an orbital debris program plan which would outline funding requirements and establish program activities over the next 10 years. Dennis Fielder, Burt Cour-Palais and I wrote a "Space Debris Assessment 10-year Program Plan". The plan called for funding of $15M to be spent over the next 10 years (most of which was to be spent on defining the environment) with major milestones of establishing a US position in April, 1988, followed by an International Space Management Agreement in 1990. This program plan was sent to NASA Hq. on December 22, 1981. Although many of the details of this plan were not met for a combination of funding and technical reasons, the plan did establish realistic objectives and proposed funding levels that all could work with, even if reduced.

Nick Johnson at Teledyne Brown, learned about our program from John Gabbard; Nick exposed us to some DOD hypervelocity tests that were analyzed by Physical Sciences Inc (PSI) and some additional insight to the causes of some breakups. This led to a contract with Teledyne Brown, where Nick combined his knowledge with John Gabbard's to publish the first "History of On-Orbit Satellite Fragmentations". Nick also had PSI re-analyze their data to provide us with an improved satellite breakup model. This contract eventually supported studies at the University of Colorado, where an AF graduate student, Darren McKnight, also had some ideas about breakup models.

However, most of our contracting went to the JSC support contractor, Lockheed, toward developing a comprehensive orbital debris model...Shin-Yi Su led this effort. Jeanne Crews joined the JSC team in 1982, originally to help with experiments to measure the orbital debris environment; however, when money later became available to do hypervelocity testing, she became more interested in this activity...developing the JSC hypervelocity lab into a major facility at JSC today.

Some data on the small orbital debris population began to appear in 1981. Herb Zook at JSC and Burt Cour-Palais had examined the Apollo spacecraft windows from the 84 day Skylab mission for meteoroid impacts. Herb used this measured cratering rate to calibrate the exposure time of return lunar rocks; however, some of Herb's critics questioned the validity of such an uncontrolled experiment. To answer his critics, Herb used the scanning electronic microscope to determine the chemical composition of material found within the impact craters. To his embarrassment, half of the crater contained an aluminum liner...certainly not the result of meteoroid impacts.

Another data set resulted from the Explorer 46 Satellite. This satellite was launched in 1972; however, because Don Humes at Langley suffered from the same cut-backs in meteoroid research as at JSC, the results of the Meteoroid Bumper Experiment on Explorer 46 was not published until 1981. Like most of Don Humes's publications, this paper included raw data. I was privileged to review his paper, and in the process discovered that the data pointed to a directional flux that could only result from orbital debris impacts.

With new data becoming available and new people becoming involved with various levels of interest, we needed to develop a common starting spot. Therefore, I organized a 3 day "Orbital Debris Workshop" at JSC. Beginning on July 27, 1982, the workshop was attended by about 100 representatives from industry and government. Perhaps the most significant result of that workshop was the shift in emphasis of our proposed program to obtaining early data using lower-cost ground sensors rather than the proposed flight experiments.

For me, this meant using ground telescopes. During the 1960's, I had worked with Herb Zook, at JSC, and Bob Soberman and Sherm Neste, from General Electric, all investigators of an experiment on the Pioneer spacecraft to measure the interplanetary meteoroid environment using an optical technique. Sherm Neste conducted the Langley study on using ground telescopes to detect small orbital debris. I was convinced that the conclusions of the study were correct, and a ground telescope could see orbital debris in low Earth orbit much smaller that 10 cm, perhaps as small as 1 cm. The USAF GEODSS telescopes had the proper optical properties for this type of search. After some research, we concluded that the GEODSS proto-type constructed and operated by MIT, called the Experimental Test Site (ETS), offered the best combination of factors.

These telescopes demonstrated some of their unique capabilities while observing solid rocket burns in geosynchronous orbit. In October, 1982, MIT video recorded an IUS upper stage burn in geosynchronous orbit. The sunlight reflecting off the aluminum oxide exhaust from this burn was so dramatic that we asked MIT to observe the IUS burn associated with NASA's TDRS-1 geosynchronous satellite on April 5, 1983. We had hoped to better understand the dispersion of the aluminum oxide dust from this observation. Instead, the telescopes recorded a failure in the mission. About halfway through the burn, a tumbling rocket and aluminum oxide plume was clearly visible, even though the sky was partially cloudy. This video tape of the failure led to an early indication of the cause of the failure. Since TDRS had been launched from the Shuttle, many of the people associated with TDRS were in Houston, including the new Associate Administrator for Space Transportation and Operations, General Abrahamson. General Abrahamson had not yet been exposed to the orbital debris program that was being funded out of his Advanced Programs Office, and this also seemed like a good opportunity to do so. Within 2 days after the failure, Joe Loftus had set up a series of briefings at JSC where I showed the video tape of both the normal and failed IUS burns. Each new briefing seemed to include more VIP's. One of the briefings was almost totally comprised of VIP's, except for one young person in a knit shirt, sitting in the back. I guessed that this person was a special "whiz kid" brought in by NASA management to understand the failure, because he kept asking a lot of technical questions. Although they were good questions, I was concerned because upper level management does not usually like to hear all the technical kinds of details that this person was asking. After the briefing, Joe said "Well...that's it...no more briefings." I said, "but I thought I was going to brief General Abrahamson." Joe said, "You just did...that was him asking all of the questions." General Abrahamson's understanding of the orbital debris program became even more important after he became head of the Strategic Defense Command (SDI).

We began to plan how to operate these telescopes to obtain data on low altitude orbital debris. We were particularly pleased when we discovered that the ETS included two identical telescopes so that we could use parallax to identify meteors which might look like orbiting objects. The concept of obtaining statistical data by letting debris pass through the telescope field of view was new, so I worked closely with Larry Taff from MIT toward setting up the observing procedures and developing the data analysis techniques. Even so, Taff's early analysis of his 1984 data appeared to include some questionable data. For example, a large number of the objects he had classified as orbiting objects had very high angular velocities, requiring orbital altitudes below 500 km. In addition, many of these low altitude objects had direction of motions corresponding to orbital inclinations where no object had ever been launched.

In 1985, Herb Zook, Loretta Weiss (a summer graduate student at JSC), and I reanalyzed the MIT data tapes. We found that a number of the objects that Taff had called orbital debris were meteors. However, when we adopted a policy of "when in doubt, assume it is not debris", we were still seeing a detection rate between two and three times that expected from the catalogue alone. About 5 years later, Karl Henize at JSC reanalyzed Taff's calibration data and found another error. This calibration error, combined with new knowledge about the low albedo of orbital debris, placed the detection size for the MIT telescopes at a size not too much smaller than 10 cm. Although it took some time to fully understand these measurements, the measurements were showing by late 1984 that there was an uncatalogued population that may be more important to spacecraft operations than the catalogued population.

More data resulted from a window on the Space Shuttle. In June, 1983, the window from the 7th Shuttle flight had to be replaced because of a 4 mm diameter crater. This STS-7 window crater was large enough that the window could not be reused without the risk of breaking on the next launch due to the high pressures that the windows are subjected to on every launch. We used the scanning electronic microscope again and found that the crater contained titanium melted into the bottom. It had to have resulted from a man-made object hitting the window in orbit. This was the first time that orbital debris could be proved to have damaged an operating spacecraft. Hard data describing the environment added credibility to a USAF Scientific Advisory Board Study which started in late 1983. The Ad Hoc Committee on Orbital Debris was chaired by Dick Kline; Drew Potter and I were members. When the study started, there was only the Apollo windows and Explorer 46 data to support any predictions. By the time the results of the study were presented to AF and NASA Headquarters in late 1984, the results of the MIT observations, the analysis of the STS-7 window were available. Dick Kline gave the briefings in November, 1984...emphasizing the theory of why we thought a large population of uncatalogued objects might be a hazard to spacecraft. One of the briefings was to the Under Secretary of the Air Force, Edward Aldridge. During the briefing, it was obvious that he wasn't quite understanding the points that Dick was trying to make. Then Dick said "...and we have an example of debris damage...", and I pulled out of my pocket the window plug containing the STS-7 crater. Mr. Aldridge took the window plug, examined it as I explained that it was caused by a 0.2 mm particle containing titanium, traveling about 5 km/sec. Mr. Aldridge began to smile...as if to suddenly understand. He then said "You know, one person's data point is worth a thousand people's speculation." A point worth remembering.

All of this data was also presented and discussed at the first international workshop at COSPAR in Graz Austria in 1984. Following COSPAR was an IAU meeting on interplanetary dust in Marseille, France, where I presented my interpretation of the Explorer 46 bumper experiment. Fred Singer presented his interpretation of another experiment on Explorer 46...the "micrometeoroid experiment", which detected short periods of a very high flux of very small impacts. Fred tried to associate these high flux periods to meteoroid showers; however, a discussion followed, suggesting that orbital debris may have been measured. In a later trip report, Fred concluded that orbital debris needed to be considered in interpreting meteoroid measurements in Earth orbit. Both the COSPAR meeting and IAU meeting increased both national and international awareness of orbital debris.

All during the early to mid-1980's, Joe Loftus, Burt Cour-Palais, and I gave a series of tutorial-type briefings to other government groups, such as the State Department, The Department of Transportation, the USAF Space Division, NORAD, other NASA centers, and the Strategic Defense Command. However, there were no major expenditures of efforts until after 1984. After 1984, three events significantly increased the research efforts associated with orbital debris. For NASA, the event was NASA's commitment to build a space station. For DOD, the event was the 1985 anti-satellite test. Internationally, the event was the 1986 explosion of the Ariane 3rd stage. Each of these events provided a focus for orbital debris research.

NASA's space station represented a large structure to be in orbit for a long period of time and, because it was manned, required a high level of reliability...a sure combination of parameters to require meteoroid protection, and perhaps orbital debris. Meteoroids were a design consideration for the space station from the very beginning; however a design orbital debris environment based on measurements did not yet exist. Beginning in 1983, Burt Cour-Palais and I discussed orbital debris issues with space station personnel both at JSC and at the Marshall Space Flight Center. Marshall was concerned with the planned habitation modules, and Burt worked closely with them toward designing the shielding and establishing an acceptable probability of penetration of those shields. It became obvious that an orbital debris reference design environment was needed, so in 1984, at Burt's insistence, I combined the available data into a reference environment for the space station, published as JSC 20001. This environment was immediately used to determine the shielding design for the space station, and established that orbital debris was a significant design consideration. Also in 1984, Drew Potter was able to obtain from Goddard some of the surfaces recovered from the repaired Solar Max Satellite. By 1985, analyses of the pits from these surfaces tended to confirm the space station orbital debris model predictions. The amount of shielding required for the space station to protect against orbital debris increased NASA's interest in orbital debris significantly.

DOD's interest increased with the USAF's anti-satellite test. On July 1, 1985, I learned that Space Division was planning to use an existing satellite to test their anti-satellite system. Later that month, at a meeting at Space Division, Nick Johnson and I learned the details of the planned test: The existing satellite was P-78, a relatively large, high altitude target, and the test was scheduled for September, 1985. I asked Shin-Yi Su to use our models to predict the consequences of the test. He concluded that the test would produce sufficient orbital debris which would remain in orbit into the 1990's that additional shielding would be required for the space station, then planned to be in orbit by that time. Nick Johnson shared this concern with me, saying, "...if I were NASA, I would be jumping up and down and turning blue not to do the test." We didn't turn blue, but Joe Loftus worked with NASA Hq to develop alternatives for DOD. Some of these alternatives included using a lower altitude target. All options were considered by DOD. However, the only real options, because of congressional constraints, were to either not do the test, or to do the test and learn as much as possible about the debris generated. The Secretary of Defense stated that the test would be conducted.

With only a few months to prepare, what could be learned was very limited. Because P-78 was in a sun synchronous orbit, neither the ETS or GEODSS telescopes were in a location to observe the fragments generated. In fact, the only US territory where the fragments could be observed was in Alaska. Drew Potter, John Stanley, and I planned an observing campaign using telescopes in Alaska. We had hoped to be able to detect objects smaller than could be detected by US Space Command; we did not. Poor weather prevented any ground observations. Drew Potter flew above the clouds in a plane with a smaller telescope; however, he detected only two fragments while the PARC radar was detecting hundreds. I remember Drew stating in frustration "...those fragments must be black." I was about to remind Drew that aluminum is shiny, when I remembered handling the fragments at PSI from the DOD hypervelocity ground tests. Those fragments were covered with a black soot...a condition I had previously thought resulted from the hypervelocity gun. I later discussed these tests further with Peter Nebolsine at PSI, and concluded that there was reason to believe that the P-78 fragments were black. Consequently, Drew Potter, John Stanley and Faith Vilas followed up with a program using AF IR telescopes in conjunction with their visible telescopes, to determine the albedo of fragments. These tests concluded that most orbiting fragments were very dark. Therefore, as a result of the P-78 test, we were convinced by 1987 that the size of the objects detected by the MIT telescopes were much larger than 1 cm, and that the orbital debris environment that space station designers were using understated the environment; however, it was not until 1991 that the space station program office was willing to update the 1984 environment model.

Perhaps more important, the P-78 test raised the orbital debris awareness of DOD. The test did produce a large number of fragments, as predicted; however, in retrospect, the combination of unexpected high solar activity around 1990, and the delay of the space station means that the test will not effect the currently-planned space station. In addition, Nick Johnson points out that our understanding of area to mass ratio for fragments is now different and the long-term consequences of this test may not be as severe as originally predicted. Even so, when the Strategic Defense Command (SDI) planned their Delta-180 test in 1986, where two objects were again intentionally collided in orbit, General Abrahamson, then director of SDI, ordered that all fragments were to reenter within a short time after the test. To ensure that no long-term or short-term hazard would be generated by this test, an "On-Orbit Safety Panel" was formed by SDI. The panel was headed by Col. Mike Rendine from SDI, and Mike Griffin from the Applied Physics Lab., and included members from Aerospace, McDonnell Douglas, and NASA. The panel not only developed predictions and safety guidelines, it planned and conducted experiments to test those predictions. The panel was a model on how such tests should be conducted in space. A few years later, the "Space Test Range" was formed by DOD with the stated objective of ensuring safe tests in space for all DOD programs; however, this new organization appears not to be as successful as the Delta-180 panel.

The European Space Agency interest in orbital debris increased significantly following the November 13, 1986 explosion of the Ariane third stage associated with the SPOT 1 satellite. Nick Johnson previously speculated that Ariane third stages likely had exploded several times in the past; however, because all of these earlier stages were left in low inclination, highly elliptical orbits, the fragments were difficult for US Space Command to detect and catalogue. However, the SPOT 1 third stage was left in a high inclination circular orbit at about 800 km altitude, and the fragments were easily catalogued. On November 17, 1986, Nick Johnson informed me of the third stage explosion. I immediately passed the information to Joe Loftus. Joe knew that the Director General of ESA happened to be visiting the NASA Administrator at that time, so he called the Administrator's office. The Director General appreciated obtaining the information in this manner, and upon returning to ESA, requested Walter Naumann to study the issue for ESA, and Remi Hergott for CNES, the French agency responsible for the design of Ariane. Joe also organized an "Upper Stage Breakup Conference", which took place at JSC on May 14-15, 1987. The conference emphasized the need to control breakups, and shared with ESA the operational procedures that NASA had adopted to prevent our stages from exploding. ESA organized their own Space Debris Working Group to investigate the entire issue of orbital debris. Walter Flury headed the group, and Prof Rex from the University of Braunschweig provided much of the modeling results. These two groups in Europe are even more active today.

The vulnerability of the space station to orbital debris combined with the increasing amount of both national and international interest increased the need for NASA to establish a coordinated agency program. This meant a briefing to the Administrator, Dr. Fletcher. One of the objectives of such a briefing was to expand the program in order to obtain data describing the environment in the size range of interest to space station designers. I had been pushing the use of ground telescopes, but obviously this had not worked. Drew Potter suggested that existing radars were not detecting small debris because they were not optimally designed and operated to sample the environment in the same way we had been using ground telescopes. After checking with a few radar experts, Drew was convinced that a ground radar could even detect orbiting objects as small as 1 mm; however, to meet space station needs, we only needed to sample the 1 cm environment, and a small, relatively cheap radar could do this. The need for an orbital debris radar to sample the 1 cm environment became a central issue in the briefing to the Administrator. Other issues included formalizing both internal and external interfaces and the need for a policy.

The briefing to the Administrator was given by Lee Tilton on July 14, 1987. The Administrator agreed with all of our proposals and even suggested that JPL might help in the design of the radar. With this type of support from the Administrator, NASA representatives drafted an orbital debris statement to be included in the President's national space policy, signed on February 11, 1988, beginning the "Post Presidential Directive Period" of orbital debris.

Establishing a national policy is never the work of a few people, but of many people and organizations. I have only touched on those activities that I am most aware of and felt significantly moved us toward a national policy. However, there were many parallel efforts that should not be overlooked. Both Nick Johnson and Darren McKnight made significant technical contributions as well as educated the policy makers and scientific community through their book, "Space Debris", and their numerous other publications and presentations. Darren's interest in improving breakup models led him to obtain the unused Navy satellite "Oscar", eventually used by the Defense Nuclear Agency in a hypervelocity breakup test. A large number of individuals at both Marshall and JSC worked shielding, collision avoidance, and safety issues associated with the space station, and these issues illustrated the need to control the environment. The Lockheed contractor support that I have had is outstanding...Shin-Yi Su (now in Tiawan), Bob Reynolds, Phillip Anz-Meador, and Dave Talent have all developed their own independent reputations. Val Chobotov has been a consistent resource of technical information for DOD. Technical exchanges of ideas between other individuals within my branch working on orbital debris...Drew Potter, John Stanley, Herb Zook, Karl Henize, Gene Stansbery, Faith Vilas, Jeanne Crews, Eric Christiansen, and Gautam Badhwar...frequently led to new insights and projects. These individuals and others significantly contributed toward the orbital debris program that we have today, and are still making contributions.

The Post Presidential Directive Period includes contributions from an even larger group of people. Within 2 months after the period began, Vol. 1, No. 1 of the "Orbital Debris Monitor" was published. Other contributions followed including an interagency study, international working groups, an Office of Technology Assessment Study, GAO Reports, a House sub-committee inquiry into orbital debris, new measurements of small debris by ground radars and LDEF, expanded DOD programs, and a number of other things...all of which is to be written in a follow-on history. These activities are being led by very talented people...the follow-on history is going to be more difficult to write.[2]

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[2] A detailed chronology of orbital debris through January, 1998 has been written by David S. F. Portree and Joseph P. Lotus in *Orbital Debris: A Chronology*, NASA/TP-1999-208856, January, 1999.