VOYAGER TALES

Personal Views of the Grand Tour

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1801 Alexander Bell Drive
Reston, Virginia, 20191

Publishers since 1930

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After receiving his bachelor of science in mechanical engineering at the University of Utah in 1957, Gary Flandro worked at JPL as a Sperry employee on Sergeant missile trajectories and aerodynamics. While studying for a master of science (1960) and doctorate degree (1967) in aeronautics from California Institute of Technology, he worked part-time for JPL.

His academic career began in 1967 as assistant professor of mechanical engineering at the University of Utah. In 1985 he became a professor at the School of Aerospace Engineering, Georgia Institute of Technology. He was awarded the Chair of Excellence in Advanced Space Propulsion at the University of Tennessee (UTSI) in 1990. He has supervised over 80 masters and 20 doctoral students.

In addition to university teaching and research in such areas as aerodynamics, combustion, acoustics, and astrodynamics, Flandro has worked as a consultant for 20 aerospace corporations. He has more than 70 publications and is co-author of textbooks on aerodynamics and rocket motor combustion instability.

He is an Associate Fellow of AIAA. In 1970 Flandro received the M. N. Golovine Award of the British Interplanetary Society in recognition of the discovery of the Voyager Grand Tour multiple outer planet mission opportunity. He received the Edward M. Glass Award for outstanding research for the U.S. Air Force in 1981.

He holds a commercial glider pilot rating and the FAI Diamond soaring badge. He has built two sailplanes and is completing the design and construction of a three-quarter scale replica of the Supermarine Spitfire. Other interests include amateur astronomy, model airplanes, hiking, and cross-country skiing. He is currently collaborating with a Russian group at the University of St. Petersburg on innovative design of solar sail space missions.

I believe that the most precious natural resource in the world is its young people. I have passed up many opportunities for financial and professional gain in order to maintain a position in which I can help young people to understand the creative application of the methods of scientific research. I have attempted to convey to my students the great sense of fulfillment that can be attained in such a search for truth. I feel great sorrow for the manner in which this most important resource is now being wasted.

—Gary A. Flando

GARY FLANDRO

How did you happen to be at JPL?

I had worked there previously just after finishing my bachelor of science degree at the University of Utah. I was hired by Sperry Univac to work in a liaison role at JPL on development of the Sergeant missile trajectories and aerodynamics. So my association with JPL began at a quite tender age in the fall of 1957.

I believe that I hold the record for the largest number of goings and comings to JPL as I carried out my graduate studies, spent a short time with Sperry, and taught math and physics in the Tonga Islands for a year. After a two-year stint as an engineering instructor at the University of Utah, I continued on at Caltech studying for a doctorate degree in aeronautics. During this period I worked part-time at JPL, with the full expectation that I would seek a permanent position there when I finished my thesis research in rocket combustion instability. It was during the spring of 1965 that I first began work on the outer planet missions.

Up to that time only preliminary mission analyses had been done on outer planet exploration. This was at a time when space flight hardware, guidance, and communications technologies were stretched to the limits by the requirements of unmanned lunar, Mars, and Venus exploration. No one was seriously contemplating flights to the outer planets, since they are much more difficult to reach in terms of required launch energy and flight duration.

The task of carrying out detailed outer planet mission studies was assigned to me in March 1965 by my supervisor, Elliott (Joe) Cutting, who was at the time the head of the advanced projects group. This was to be a serious, in-depth study to determine if there were any truly useful outer planet mission opportunities; the practical type of unmanned mission that could be accomplished with launch systems, guidance techniques, and spacecraft mechanical and electronic technology available at that time.

Cutting and Fran Sturms were studying the application of intermediate planet gravity assist in missions to Mercury via close passage of Venus (that work led to the highly successful Mariner 10 Venus-Mercury flyby launched in 1973). Based on this experience, Joe suggested that a similar trajectory shaping method might be applicable in outer planet exploration.

It was widely believed at the time that employing an intermediate planet encounter would always lead to a longer trip time than could be achieved with the best available direct trajectories. This appeared to be true in the Venus/Mercury mission designs and the inner solar system round trips (e.g., Earth-Mars-Venus-Earth) first proposed by Hohmann in 1928, studied in detail by G. A. Crocco in the mid-1950s, and later by Minovitch. It seemed to me that careful application of the energy gained in a properly configured "swingby" could be used to advantage in shortening trip times to the outer planets.

I was already familiar with the idea of using intermediate planet orbit perturbations in the design of interplanetary trajectories. Most of my practical knowledge of the technique came from study of the works of Krafft Ehricke. His textbook, *Space Flight (Vol. II, Dynamics)* published in 1962, presented a comprehensive discussion of the application of the energy gained in a close gravitational encounter with an intermediate planet to either gain or lose speed relative to the sun. I used Ehricke's works in all of my mission analysis efforts at JPL and was greatly inspired by them as I began my quest for trajectories to Jupiter and beyond.

The mechanics of orbital energy modification in a planetary encounter, the "gravity-assist", or "swingby" effect were presented in Ehricke's text. He wrote, "If at all possible, maneuvers for changing the heliocentric orbital elements should be carried out during the hyperbolic encounter with a planet, rather than in heliocentric space. The greater the planet's mass, the greater the energy saving." In the preface of *Space Flight* he notes that "... hyperbolic encounter (with an intermediate planet) represents a potentially important means of central force field orbit change without propellant expenditure." He obviously saw with great clarity

the benefit of using the massive planet Jupiter as an energy source in the exploration of the outer planets.

I was of course aware of Michael Minovitch's efforts, and it was my impression that he had been concentrating on the inner planets—Mars and Venus round trips. He had just published a JPL technical report that described solar system escape, out-of-ecliptic, and close solar probe missions utilizing Jupiter swingbys. In fact these had already been proposed by Ehricke. On pages 1107–1109 of *Space Flight, Vol. II* are detailed diagrams showing the trajectory designs just mentioned.

Anyone interested in the early history of solar system exploration should examine Ehricke's marvelous astrodynamics texts. They contain not only a vast array of astrodynamics tools, mission analysis techniques, and applications of the type described, but also a comprehensive history of space technology.

Dr. Krafft Ehricke had been one of von Braun's colleagues at Peenemünde before coming to this country under Operation Paperclip. He was described by Grayson Merrill [editor of the Van Nostrand series on Guided Missile Design in which Ehricke's books appeared] as "... a beloved humanist as well as a fine scientist." Interestingly, during the time these books were written, Ehricke was working at General Dynamics/Astronautics as the program director of the Centaur upper stage rocket, which was to play a key role in launching the Voyagers on their way to the outer solar system.

Minovitch later claimed he had invented the concept of gravity assist, or what he termed "gravity thrust", but in fact, I believe Ehricke should be given careful consideration if an inventor is to be identified. He clearly understood rather completely the application of energy gained or lost in a hyperbolic planetary encounter in modifying the orbit of a spacecraft. I've endeavored to motivate historians to pay more attention to Ehricke's early contributions in this regard.

In reality, the basic ideas behind gravity assist were known as far back as the 1800s. Astronomers (Leverrier, the discoverer of Neptune among them) interested in comet motions were quite familiar with the gravitational modification of the orbit of a comet or asteroid when passing close to a massive planet like Jupiter. The mathematical formulation of the orbital energy change that these astronomers developed for estimating the amount of energy gain or loss and the resulting modification of the orbit were the same ones that we used later in our outer planet mission analyses. They were already over 100 years old when we put them to work in space flight applications.

So that is where I started. I was looking for ways to reach the outer solar system efficiently, making practical missions possible. The essence of the problem was that a mission design approach was required that both shortened the flight duration and provided for a payload of significant size using available launch vehicles. I began with the idea of employing gravity assist as a means for supplementing the energy imparted to the spacecraft by the rocket booster at its departure from the Earth. With that in mind, the key element was to examine the geometry of the solar system to see if practical flight paths would be available at not too distant future times.

Thus began the search for actual time periods during which outer planet flights would be feasible. The first step was to graph the heliocentric positions of all the planets versus time (Fig. 1).

I noticed that the traces for all four of the major (outer) planets crossed in the 1980s. I was intrigued by the conjunction of Saturn, Uranus, and Neptune and found by computation that this planetary configuration would not occur again for about 175 years. This number later appeared in many places, including infamous discussions of the so-called "Jupiter Effect".

It therefore became quite evident that, if you are looking for trajectories first passing Jupiter on the way outward, this was the correct time period on which to focus. I was struck by the fact that all of the outer planets, Jupiter, Saturn, Uranus, and Neptune, were on the same

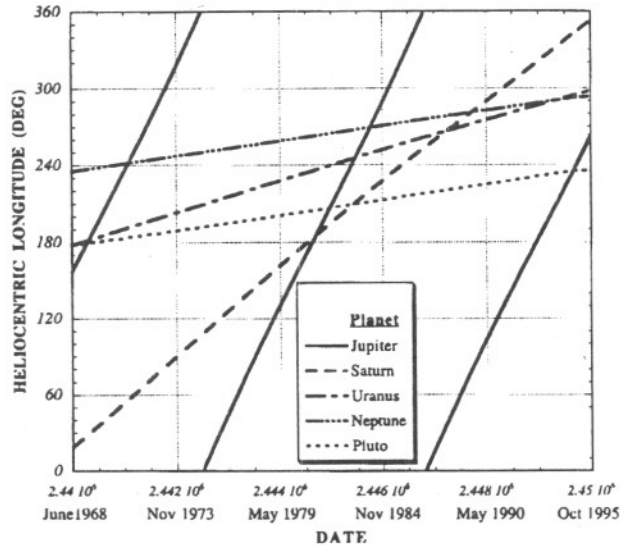

Fig. 1 Heliocentric positions of the outer planets vs time.

side of the sun (within a range of about 50 deg). So, why not look for a *single trajectory* that would pass each planet with the shortest possible trip time between?

Study of the planetary position diagram indicated that a spacecraft launched from the Earth in the mid-1970s on a two-year trajectory to Jupiter would enable access to all of the major outer planets (except Pluto) with a single vehicle. It was also clear that trajectories including Jupiter, Saturn, and Pluto could be designed, but I was more interested in Uranus and Neptune as final targets.

This realization occurred in the spring of 1965, while I was working in my office on the third floor of building 180 at JPL. I distinctly remember the feeling of awe as it first dawned on me that this mission was available at just the right time, leaving about 10 years to market the mission concept and to design and build a spacecraft. The next such opportunity would not appear until about 175 years later! I then hurriedly sketched the Earth-Jupiter-Saturn-Uranus-Neptune trajectory configuration and excitedly described the potential mission to my office mate, but as I recall his only response was a condescending smile. Joe Cutting, however, was very interested in these preliminary findings and encouraged a detailed study of the opportunity.

Since there was no guarantee that such flight paths were actually available until it was demonstrated that passage distances at each target were close enough for scientific measurements but not too close, I began a careful search for the actual trajectory profiles. I studied these at first, using so-called "conic" trajectory approximations. I did all of my trajectory work with a computer program that had been written for JPL by the Lockheed Missile and Space Company in northern California. It was a simple interplanetary ballistic trajectory program that approximated the flight paths with conics (ellipses and hyperbolas).

I did not set up the computer runs myself because in those days JPL provided a closed-shop computer system. I would work each day, setting up the launch dates and flight times for a number of runs, and I would then take these to Don Snyder, the cognizant programmer, to be run overnight. The next morning, if all went well, I would have a stack of computer printouts representing my next increment of data to analyze. Helen Ling and her outstanding group of "human computers" sometimes aided me in plotting and cross-plotting results.

One of the most difficult parts of the computation was the "matching" of trajectories at an intermediate planet. I would run a set of, say, Jupiter-Saturn trajectories and then match these

with continuation Saturn-Uranus orbits. These had to jibe properly on both the incoming and outgoing legs.

All of the detailed mission analysis for this first four-planet study was accomplished by graphical methods; I did not use a computational method such as the one that Mike Minovitch developed. I did not feel that a blind, large-scale searching technique would give me the physical insight I was looking for. After all, the determination of the correct time period had already been accomplished. All that remained was to carry out the detailed calculations to prove the concept and accurately portray the best launch dates and flight times.

Mine turned out to be a sufficiently accurate and yet quite practical multiplanet trajectory design method given my self-imposed time constraint: the work had to be done by the end of the summer. By July of 1965 I had a quite complete idea of the best launch dates. I then began writing a paper describing the findings, which appeared later in *Astronautica Acta* (Vol. 12, 1966). This was the first publication outside of my internal JPL memoranda describing what would later evolve into the four outer planet Voyager missions. I'm sure it would have been academically wiser to publish the results in one of the more widely read technical journals of the time. I later found that very few readers in my target audience paid much attention to *Astronautica Acta*.

My Ph.D. advisor at Caltech, Professor Frank Marble, had been approached by the editor-in-chief, Martin Summerfield, in search of some appropriate material for publication. I had described my findings in a graduate seminar at Caltech, showing some of the multi-outer-planet trajectory results I was getting. After observing that this was somewhat removed from the work I was supposed to be doing for my thesis, Frank suggested that I should publish in *Astronautica Acta*.

Were you working on this all by yourself at this time?

There was no one else at the Lab other than a few advanced-mission specialists much interested in such matters. The focus at that time was mainly on implementing the Mars flights (Mariner and Viking) and soft lunar landings (Surveyor). An overwhelming portion of the Lab's resources was devoted to carrying out the exploration of the moon, Mars, and Venus. Just a few of us were looking at some of the more distant opportunities. To my knowledge I was the only person at that particular time doing outer planet work.

How did it come about? Did your supervisor say, "Gary, why don't you look at this?"

No, not really. Although he would have allowed me to select any research topic I thought worthwhile, Cutting felt that gravity assist in general showed great promise in making the outer solar system accessible. I was just given the general theme of "outer planet missions," so I took it upon myself then to search out the practical means for accomplishing them. The Grand Tour was the outcome.

At what point in your thinking did you have the idea "Aha! Maybe I can get all four planets"?

That came right at the very beginning of the study, in the spring of 1965, when I had just made the plot showing the heliocentric longitudes of the outer planets. I could see that the paths all crossed in the late 1980s. That is, the outer planets would all be on the same side of the sun. I knew at that point "Yes! Here is the way to do it!" All that remained was to prove by means of detailed trajectory calculations that a practical mission could be designed.

When you were first looking at Jupiter and what you could do to go on to Saturn, was that thought of getting out to Neptune in the back of your mind?

Absolutely! That was there in the thinking, along with Uranus, right from the outset. I could see that doing flights with a final target of Saturn (or in fact any of the other outer

planets including Pluto) by means of Jupiter gravity assist would be quite interesting. Given the economics and expected tight budget constraints, it seemed more likely that a mission proposal would succeed if we could encounter several outer planets in a single flight. So that was the goal right from the beginning.

I carried out detailed trajectory calculations for Earth-Jupiter-Pluto, Earth-Jupiter-Uranus, Earth-Jupiter-Neptune, and Earth-Jupiter-Saturn as well. I felt that these were necessary, since it seemed likely that the Grand Tour should be backed up by some less ambitious exploratory opportunities that would serve as a fall-back position.

While I was still at JPL, there was much concern that missions as ambitious as the Grand Tour could not actually be accomplished; I received considerable negative response at first. One of the activities I engaged in was the "marketing" of the multi-outer planet mission concept to those who might later be involved in the required guidance, communications, and spacecraft mechanical and electrical design. Most felt that it would not be possible to build a machine with the longevity for such a mission. I think it is important to note here that there was at this time considerable skepticism regarding the potential of the Grand Tour as a practical mission undertaking for JPL. Many openly scoffed at the idea.

What time are we talking about here?

The 1965–66 period. At that time I was seeking a really fast trajectory that would take less than eight years to travel all the way to Neptune via Jupiter, Saturn, and Uranus. It should be pointed out that in conducting a mission study, one does not seek just a single trajectory solution. There must be an entire set of solutions to yield an appropriate "launch window" during which the mission can be initiated. In fact, one endeavors to define sufficiently long launch windows in several successive years to provide a contingency for any problems in readying the spacecraft and its launch vehicle. I found that the best launch windows for the Grand Tour were in the fall of 1977 and '78.

Since my *Astronautica Acta* paper presented a rather broad picture of outer planet exploration, including a discussion of the mechanics of gravity assist and no fewer than six different classes of missions including the Pluto opportunities, there was not room for very extensive plots of the Grand Tour trajectory results. I chose to display the launch date/arrival date (at Neptune) contours only for the 1978 mission opportunity (Fig. 2).

I had, however, done all the detailed calculations for the 1976 and 1977 opportunities as well. This involved hundreds of trajectory profiles for each of the launch years. It is important to see that there is not just a single trajectory, but a very large family of them. It was necessary to investigate the entire family of trajectories in order to determine the following items: 1) the launch dates yielding the shortest times of flight to the final target planet; and 2) the sensitivity of the trajectory to the launch energy from the Earth.

The latter requirement was met by constructing drawings of the launch date vs arrival date as contours of constant launch energy, traditionally called the "C3" by JPL engineers. In simple terms, C3 is the kinetic energy of the spacecraft relative to the sun as it leaves the gravitational field of the Earth per unit spacecraft mass. So the contours show exactly when you must launch to achieve the lowest required launch energy, or in other words which trajectory yields the highest potential spacecraft mass for a given mission duration.

On this basis, the Voyager mission designers later selected the Titan Centaur launch vehicle because it could impart the necessary launch energy to a payload of appropriate mass, about that of a Volkswagen "Beetle." Ehricke's contributions must be mentioned again here, since he led the team at General Dynamics that developed the Centaur liquid hydrogen/oxygen upper stage that made the mission possible.

If you examine Table 1 you will see that I had identified 1977 as the best year for the Grand Tour launch. This is the launch opportunity that was chosen for the actual mission.

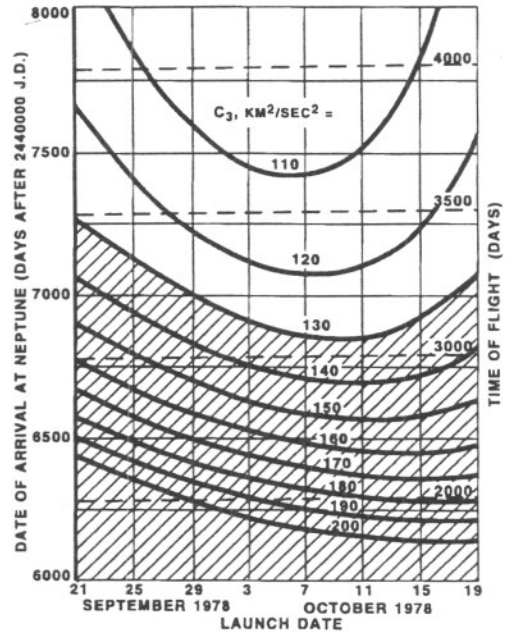

Fig. 2 Neptune launch and arrival dates during the 1978 launch window opportunity.

To make a fast flight (seven to eight years), a very close passage of Saturn was required, with the trajectory passing between the planet upper cloud layers and the rings to achieve the extra gravity assist boost. That particular possibility was rejected later on because there was concern that there would be ring material in that zone, the almost invisible inner rings, that would be a hazard to the spacecraft. Therefore, the future Voyager mission designers opted for a considerably slower trajectory that went outside the rings. Looking back on things, this choice seems ironic in view of the concern then that even the shorter flight times could not be accommodated.

I studied a whole range of trajectories and flight times, but my favorite was that one that took you out to Neptune in about eight years. This represented a remarkable time savings. A direct optimal flight to Neptune would require a flight duration of about 30 years! But when I suggested such things, many people at JPL would say, "This is ridiculous, we can't build a spacecraft that will last over two years, let alone eight years!"

Table 1 Multiple-planet trajectories to the outer solar system

Mission	Launch years				
Earth-Jupiter-Saturn-Escape		1976,	1977,	1978[1]	
Earth-Jupiter-Uranus-Escape	1977,	1978,	1979,[1]	1980,	1981
Earth-Jupiter-Neptune-Escape	1977,	1978,	1979,[1]	1980,	1981
Earth-Jupiter-Pluto-Escape	1975,	1976,	1977,[1]	1978,	1979
Earth-Jupiter-Saturn-Uranus-Neptune		1976,	1977,[1]	1978	

[1]Optimum launch year.

There was much concern over the practicality of the mission from the standpoint of the longevity of the required electronics and mechanical components in the interplanetary environment. Even an eight-year trajectory was considered to be a very difficult task for the mechanisms and electronics that were available then.

So all of this wonderful work was as a student?

Yes, that's correct. I was at the final stage of my course work in aeronautics at Caltech, had completed the two required foreign language exams, and had begun serious thesis research for the doctorate degree.

I must admit that I was disappointed that I got very little recognition for my work in light of later events, but I guess that sort of thing is natural. I did receive some recognition, but not from sources within the United States. The British Interplanetary Society (BIS) awarded me its annual Golovine Award in 1970, in recognition for the outer planet work. The British were, I think, more likely to be following things in the *Astronautica Acta*. Based on suggestions from Elliott (Joe) Cutting, Dr. William Pickering, the JPL director at the time, wrote a letter to BIS recommending that the Golovine award be made to me in recognition of my outer planet mission research. This was not just for the Grand Tour, but for the entire set of outer planet missions I had proposed.

I did not manage to raise the funds necessary to travel to England to accept the award. So the Dean of Engineering at the University of Utah, Max Williams, who was in England on business at the time, attended the award ceremony for me and reportedly enjoyed the whole thing immensely. I was pleased that someone had noticed my work. All of this took place, of course, before any of the actual planning for the Voyager mission had begun at JPL. I know that Arthur C. Clarke, a longtime member of the British Interplanetary Society was aware of my work; he used the idea of the Jupiter-Saturn gravity assist in the original screenplay for *2001, A Space Odyssey*. Apparently the final script had to be shortened so all the important final events took place at Jupiter instead of Saturn as he had originally written it.

No recognition from NASA?

Most certainly nothing from NASA; nor anything from JPL other than Dr. Pickering's kind recommendation for the Golovine Award made long before the mission had become a reality.

Several of the people who have written things have done an excellent job of portraying the events as they happened. There's a fine book, *Planets Beyond* by Mark Littman, that received several literary awards. This book describes both the discovery and the later exploration by spacecraft of the outer planets.

Littman gave you some credit by name?

Yes. In fact I was invited by him to contribute a vignette on the history. You should also consult Joe Cutting and the other members of the original Mission Analysis Group for their views on the history. I think because I left the Lab before the Voyager project was underway that many JPL people have forgotten the true origins of the mission. Their minds were elsewhere at the time. Part of the problem was the 10 year time span between my discovery of the mission opportunity and the actual implementation of the concept.

Charley Kohlhase, who worked on the standard trajectories, appeared not to have remembered who did the original work until I reminded him around the time of the Neptune encounter. In his intense concentration on the trajectory design, he lost sight of the fact that had I not discovered the mission when I did, it is most likely that he would never have had the chance to do his part. The timing was critical. That entire 10-year interregnum before he started work in 1975 was needed for the turbulent political and technological stew to settle into something like a realistic approach to the mission.

Many myths have arisen about the origins of the Voyager mission. Some of these were the result of the political nature of the mission during the early days of its marketing by JPL to NASA and by NASA to the federal government. Since I was pretty low on the totem pole and had already left JPL, no one felt much need to mention my connection with the discovery of the mission. I accepted these misconceptions, but it was sometimes difficult. In looking back, I can see that what happened is a lesson on human behavior. I have found it interesting that there was little contention until the Voyager mission became a reality. Then the trouble really began.

I left JPL as a rather naive young fellow, I could not conceive of the possibility that I would not in time be acknowledged in some fashion for the work I had done. I thought this would happen automatically, since I had properly documented my work and presented it to many people both at the Caltech campus, at JPL, and in various technical meetings.

Those at JPL who brought everything together certainly deserve major credit for the magnificent job that they did.

What was the origin of the term "Grand Tour"?

About mid-1965, Joe Cutting recognized that we had indeed identified a practical mission opportunity. Therefore he set up an appointment with the JPL chief scientist, Homer Joe Stewart, to describe the possibilities to him. I showed Dr. Stewart slides for a presentation I made earlier to the Los Angeles AIAA section. This was a detailed description of the available mission profiles including flights to Pluto.

By that time I had been able to determine the view angles and passage distances at each of the target planets, demonstrating that useful photography and scientific measurements could be made at each planetary encounter. It also had become clear that the mission would be politically correct. That is, each leg of the trajectory required less than four years; therefore we could probably count on the support of highly placed politicians who, of course, are always on a four-year schedule.

During this meeting Homer Joe recalled the earlier work on multiplanet trajectories carried out in the 1950s in Italy by G. A. Crocco (father of Professor Luigi Crocco of Princeton University fame). Crocco had proposed an Earth-Mars-Venus round trip gravity-assist mission, which he had called the "Grand Tour". This name was of course a perfect description of our new mission proposal, and it was at this moment that the name "Grand Tour" was adopted to describe the four-planet trajectory.

I have been amused over the years as many other people have claimed that they were responsible for attaching this name to the mission. Dr. Stewart gets the credit for this one. He issued a press release within a day or two after our meeting, and the first steps in publicizing and marketing the Grand Tour mission had begun.

How were the final trajectories chosen?

Later, the analysts who designed the standard trajectories for the Voyager chose a longer flight duration of about 12 years. This was largely predicated on the need to achieve the best view angles, passage distances, and lighting for photography at as many of the satellites of the outer planets as possible. In fact, the Voyager 1 trajectory was designed with the objective of acquiring pictures of Saturn's large moon, Titan. To accomplish this, it was necessary that the trajectory was deflected upward out of the ecliptic plane and off the flight path needed to continue on to Uranus and Neptune (Fig. 3).

What other concerns were there?

There was much concern about "how do you get any useful data back from those distances?" Communications engineers were at the time still grappling with the data transmission problems

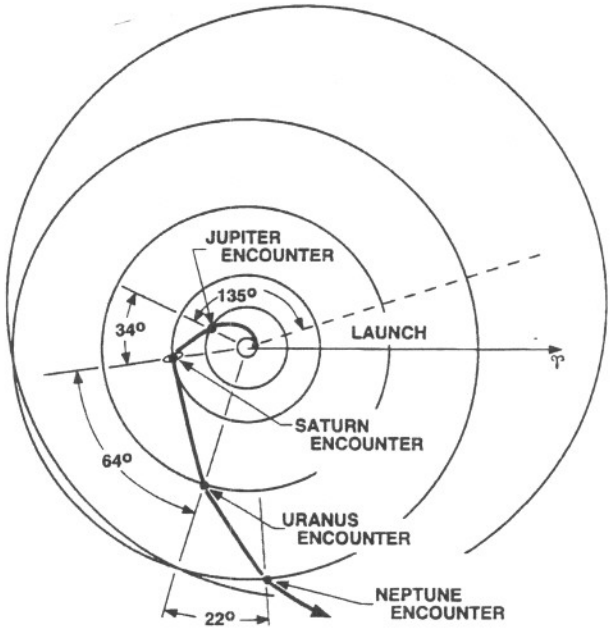

Fig. 3 Later version (1966) of a Grand Tour slide shown to Dr. Stewart.

from Mars. I recall Bruce Murray's "Martian Horror Story," describing the grave limitations imposed by constraints on the rate of data transfer to the Earth. It looked quite difficult even from that distance, so in going to Jupiter and beyond, it appeared that the data transmission and handling would be extraordinarily difficult.

However, about that time the deep-space communication net was becoming a reality, and that was an important ingredient in making long-range planetary exploration feasible; the large Earth-fixed antennas made it possible to implement practical data transmission rates. There were also, of course, rapid improvements in electronics, magnetic data storage, and so on.

Another worry was how to accurately guide the spacecraft at extreme distances. An oft-expressed concern was that we were using simple conic trajectory programs in the mission design. How well did these represent the actual mechanics of the solar system? However, those fears proved groundless. It was quite practical using Earth-based radio guidance and midcourse propulsion maneuver strategies developed by JPL to carry out the necessary accurate guidance—as the final Voyager trajectories so elegantly demonstrated.

There was also concern that Jupiter's magnetic field and radiation belts were so powerful that the spacecraft electronics could not survive a close passage with the planet, but that worry was based on some misconceptions about how such fields affect the spacecraft systems. It turned out not to be an insurmountable problem.

Yet another imagined hazard was the asteroid field between Mars and Jupiter that must be traversed by outer planet vehicles. It seemed that this might be a major threat to the spacecraft. It turned out not to be. All that was needed was to carefully design the final standard trajectories so that any large bodies were avoided.

When did the atmosphere at JPL shift from skeptical to supportive?

That happened after I left JPL to accept an academic position at the University of Utah in the mechanical engineering department in September 1967. This part of the story can best be

told by those who later continued the Grand Tour work at the Lab. I will attempt only to briefly describe important events as I watched them develop from afar.

In the same time period my mission analysis interests had shifted to application of solar electric low thrust and solar sails. I did trajectory studies that employed solar electric propulsion on the Earth-Jupiter leg of missions like the four-planet Grand Tour. This approach greatly enhanced the payload, but required application of a promising new space flight propulsion technology, electric propulsion, that has not yet even as we speak reached the stage of practical application.

Before leaving JPL, I enjoyed working briefly with Al Joseph, in the design of a computer program that could be used to accomplish the analysis of multiplanet missions in a more automated fashion. Al produced an excellent program that enabled the complex searching procedures to be carried out without the tedious handwork that I had used in the first Grand Tour study. Later versions of Al Joseph's program were used, I believe, in the detailed Voyager trajectory design work. Such efforts as these in the late 1960s demonstrated to me that there would soon be an explosion of interest in outer planet exploration at JPL.

A study to implement outer planet exploration started in June of 1969 and was based on an extension of the successful Mariner technology. In 1970 James van Allen, who had by then become a devotee of the proposed outer planet missions, chaired a NASA team examining the scientific opportunities that could arise from implementing the Grand Tour. Successful Mars exploration had been accomplished, and I suppose everyone saw that the time had come to reach for more distant targets and for even more ambitious missions. So began a serious effort to bring about outer planet unmanned exploration. Bruce Murray, the Caltech planetary scientist, worked hard to get the planners to devote attention to scientific observations of the moons of the outer planets as well as the planets themselves.

At first, NASA enthusiastically supported the Grand Tour, and a rather grandiose scheme was envisioned in which several spacecraft would fly the mission and would drop off atmospheric entry instrument packages and orbiters at each of the target planets. These were going to be heavy spacecraft, to be carried on the expensive Saturn V of Apollo fame.

The original plan even earned the endorsement of President Nixon, who expressed his full support. There was an extensive publicity effort underway. Werner von Braun displayed his public backing in various articles in the popular press despite his passionate devotion to manned spaceflight. Homer Joe Stewart received much credit in the media during this time as the originator of the Grand Tour, which I suppose was appropriate since he was the chief scientist at JPL when I discovered the mission opportunity.

At this point, early 1972, ominous clouds appeared on the horizon. The first was the announcement of the space shuttle development. This was followed immediately by notification to JPL that the Grand Tour project was cancelled because all the money in the shrinking NASA budget was needed for the space shuttle and continued emphasis on manned spaceflight. There was a perceived need to preserve the manned space program at any cost. There were several highly placed individuals in NASA who attempted to bring all the outer planet work to an immediate halt along with other plans for unmanned exploration of the solar system.

Even when scaled-back versions such as the MJS (Mariner Jupiter-Saturn) were proposed by JPL, they received scant support at NASA Headquarters. By this time such proposals were also not being received favorably by the Nixon White House because of the perception that the public was no longer interested in space exploration. The novelty of the Apollo moon landings had apparently worn off. Bruce Murray aptly described the situation, "JPL was on everybody's hit-list awaiting the next big NASA cut." It seemed that JPL had no useful role in the shuttle-dominated NASA of the future. There was much resistance to the whole idea of outer planet exploration, since this required JPL to carry out the work.

On what basis?

I suppose it just took money and attention away from the manned program, and especially the shuttle space transportation system. Also, it doesn't take too much imagination to perceive the petty jealousies (probably spawned by JPL's spectacular successes with the Mariners and other unmanned missions) displayed in some of the actions of the NASA management at that time.

The MJS survived somehow and finally evolved into the Voyager project. In about 1975, former colleagues of mine at JPL began the arduous task of designing the standard trajectories to implement the Grand Tour mission opportunity. Charley Kohlhase, who later managed the Voyager flight operations with Paul Penzo and Joe Beerer, picked up where I left off and accomplished the next stage of the job in great style. This involved running thousands of trajectories in search of the ones that would yield the best photography and scientific measurements at not only the four planetary targets, but also their moons. I believe Charley deserves much credit for setting the search criteria that led to the final mission design.

In response to an RFP for scientific payloads for the MJS, Dr. Don Groom, a physics professor at the University of Utah and I prepared a proposal for a clever energetic particles telescope. We weren't selected as experimenters. However, the team of investigators finally selected came up with some wonderfully innovative techniques to get the best possible information from the unmanned exploration, as later events proved.

Meanwhile, NASA had *prohibited* the Lab from mounting anything beyond a Jupiter-Saturn mission. So when the project finally reached the hardware development stage, the Lab had only received funding to do a Jupiter-Saturn flight. But in spite of all that, the magnificent JPL team once again demonstrated extraordinary creativity and undertook to build a robust spacecraft that could accomplish the entire four-planet mission. Fortunately, later on, when it was too late for NASA to stop it without looking petty, the Voyager team was able to carry out the full Grand Tour mission (the Voyager 2 spacecraft), despite the politics. All of this on a two-planet budget!

How did NASA make its disapproval known?

They were directly opposed to it. NASA Deputy Administrator Hans Mark saw absolutely no value in scientific exploration of the outer solar system. He, in fact, was quite serious about ridding NASA of the pesky JPL entirely.

Did they say "Hey you guys, we understand you are thinking about wandering beyond Saturn; don't do it"?

Not exactly, but there is no question in my mind, that had they fully realized what was going on in the spacecraft assembly building at JPL, they would have been most displeased.

This was in the late 1960s?

No, this would be at a later time, during development of the Voyager spacecraft and the early stages of flight in the late 1970s. A fascinating 1991 book by William E. Burrows, *Exploring Space*, presents a very penetrating analysis of the politics of the time.

And your own observations support what Burrows said?

Pretty much. All of this recalls some other words by Bruce Murray, the JPL director during the Voyager years, to the effect that, "While the Voyagers functioned smoothly in space, circumstances in their terrestrial birthplace were not so harmonious."

It also brings to mind William Burrows' fascinating description of some of the events during those marvelous hours at JPL during the Voyager 2 Neptune encounter in August 1989. I was there with my teenage son Troy to witness the encounter. While I was being interviewed by a British journalist, I noticed Hans Mark and some other dignitaries on the other side of the von Kármán auditorium.

Burrows describes the response of Hans Mark as George Alexander, NASA Office of Public Information head, showed to him the Voyager spacecraft full-scale model on display in the von Kármán auditorium. As Burrows tells it:

> The fact that Voyager 2 had reached Neptune after such an arduous long-duration flight was a tribute to the quality of U.S. engineering, Mark told Alexander as he continued to appraise the spacecraft without visible emotion. He might have added that the feat was no less a tribute to its team's and the larger community's resilience, to their ability to withstand and ultimately prevail over duplicitous bureaucrats concerned only with what was politically expedient on the home planet as well. He might have. But he didn't. Instead, Hans Mark finally turned and purposefully retraced his steps to the rear of the auditorium, once more being careful to stay clear of the cables that were carrying the triumph at Neptune around the world. He did not smile.*

Do you think attitudes about space exploration have changed since then?

The present reality is somewhat frightening. Perhaps it is a demonstration of some of the societal trends in America that have taken us so far back from the great promise of space exploration. This is especially evident in the current lack of interest in science in the young people and in the rise of pseudoscience as a substitute for reality. I guess I can understand why typical high school students think that it is much smarter to put their efforts into things that "make money" instead of the arduous work required to prepare for a career in science or engineering.

Over the years I have felt the obligation to convey to students the great excitement and fulfillment I experienced in my early connection with outer planet exploration. Close to home, my own students, both graduate and undergraduate, have received much of value from this experience. I'm not so sure I did much good with some others. During the late 1960s there was considerable interest and apparent understanding among groups of high school students and astronomy clubs I talked to about outer planet exploration.

There has been a noticeable decline of interest and understanding with the passage of time. My most recent and most disappointing experience came in a talk I gave to a group of high school students at a rural town in middle Tennessee. I noticed during the presentation that there was considerable agitation and lack of attention. To them I was clearly just another nerd. I finished by showing some of the spectacular pictures taken by Voyager 2 at Jupiter, Saturn, Uranus, and Neptune. The first question that was asked was, "How did they fake those pictures? They aren't real are they?" This was followed by: "Do you believe in UFOs?" The media and our poorly trained and unmotivated high school math and science teachers are part of the problem.

I must take this opportunity to thank my wonderful teachers in grade school and high school who maintained my interest in science and technology. They are the ones who deserve recognition.

Speaking of pictures, do you have any photos, charts or other illustrations from the early days of the Grand Tour?

I have several of the original graphs I described earlier. I have the drawing I told you about illustrating the heliocentric longitudes of the outer planets. It was just a pencil drawing made for my own use. I had two 11-in. × 17-in. pieces of graph paper taped together so I could expand out the plots to see the details more easily. I also have copies of the original flight trajectory sketches.

While I'm talking to Roger Bourke, what particular points should I pursue with him?

I think the historical matters regarding the earlier discovery of the gravity assist by the astronomers is one of the aspects with which he is intimately familiar. That again is a fact that

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* William Burrows, *Exploring Space*, Random House, New York, 1991, p. 412.

is not very well known, and it is only right that those who did the work should get the credit. That's why I'm so adamant that some credit be given to Krafft Ehricke. I tried hard to get the Smithsonian Air and Space writers to mention these matters in their recent article on the origins of the Voyager missions; apparently they weren't listening. Some of us who were involved in the early work have grown weary of trying to straighten out the historical record as it was presented in that and other articles over the years. I hope this will be the last time I have to attempt to do so.

I think you'll find Roger a fascinating chap. He was at JPL during all events I have described. Please ask him what his perception was of how this mission began. It's my impression that he was aware of most of the happenings I have described to you.

Going back even before the times we've talked about so far, when did you first become interested in science or space?

Oh golly, that takes me right back to my earliest memories. I received a beautiful book on astronomy from my mother when I was about six years old. In the frontispiece appeared an illustration of the solar system, showing all the planets. Jupiter, Saturn, Uranus, and Neptune were placed in what I guess the artist considered a typical configuration. There they all were, lined up just as they needed to be for the Grand Tour! I must have been impressed by that because I drew similar diagrams in my later work on the mission.

I remember thinking as a youngster how great it would be to fly through space. There was in my book an artist's conception of a space vehicle of the sort you might expect in a vintage 1940s book, and I was most intrigued by it. I thought how neat it would be to go all the way through the solar system and pass each one of those outer planets shown all lined up just the right way. I suppose that might have sown the seed that later grew. What an amazing piece of good fortune to find the planets lining up in an ideal pattern at just the right moment.

I've been interested in space flight, astronomy, and engineering in all of its forms right from childhood. I built telescopes when I was a youngster. I bought my first telescope kit (for one dollar) when I was in the first grade. It was supposed to reproduce the performance of the 'scope used by Galileo. That is, it was not a very good telescope, but I built it with some help from my father and it worked. This was the beginning of a lifelong interest in amateur planetary astronomy. In my teens I spent many fruitless hours looking for evidence of the canals on Mars. I enjoyed watching the motion of Jupiter's moons and studying the cloud configurations and the Great Red Spot. However, my favorite viewing object was and will always be Saturn and its magnificent rings.

Looking back over the Voyager project, were there any lessons from it that could be applied to other projects, not necessarily just in space, but generally to any large organization?

Oh yes, absolutely. In fact, one of the things I learned was that you have to be very careful not to be discouraged by experts. I was being told at JPL that I was wasting my time with outer planet exploration. The experts would say "you can't do this, that, or the other." This important principle was expressed most elegantly by the science fiction writer Arthur Clarke in the form of Clarke's Law: "If a highly respected and well-established authority tells you something is possible, then he is probably right; if he tells you something is impossible, he is probably wrong."

I think this principle is absolutely correct, and I make all my students learn it so they won't get discouraged by being "experted". When they have an idea they should use their own mental resources and creativity to dig right to the roots. Good things almost always come from such an effort. I attribute the success of the Voyager mission to that attitude on the part of the JPL team. They followed what they saw was the correct path. They simply ignored all the opposition to the greatest possible extent.