Statement from Colleen Hartman of the Outer Planets Program Directorate

On February 21 and 22, 2001, NASA's newly-created Outer Planets Program Directorate convened a conference at Houston's Lunar and Planetary Institute on "Innovative Approaches to Outer Planetary Exploration, 20001-2010." I would like to express my gratitude to the five individuals whose reports from that conference's three Focus Groups are compiled here. Drs. Michael J. Drake, Jonathan I. Lunine, William McKinnon, William Jeffrey, and Lisa Porter have promptly met their obligations to the conference's 80+ participants with an accurate and thoughtful joint report, and I would like to thank them on behalf of myself and the outer planetary community.

The joint report contributes to the mission of the Directorate in at least three ways.

First, it provides a usefully detailed summary record of the activities of the Houston conference itself. Members of the broader community of persons interested in solar system exploration can inform themselves on the current state of play in the Directorate by studying this document. They are furthermore invited to remain involved in the Directorate's efforts by offering their feedback on the report.

Second, the report provides us with a crucial and significant step in the direction of a clearly integrated prioritization of outer planetary science drivers and mission objectives. With such integration, the goals of outer planetary exploration merit significant public support. By achieving consensus on prioritization, the community can rightly claim long-term funding, even when compared to already robust portions of NASA's space science program.

Finally, the joint report provides the first in a series of snapshots by which the progress of the Directorate can by evaluated over time. The success of our efforts over the long term depends on our mutual accountability in the face of sophisticated challenges both in the realms of space science and technology, and in the realms of national budget and space policy priorities.

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Report from the *Forum on Innovative Approaches to Outer Planetary Exploration 2001-2020*

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Introduction

Why explore the Outer Solar System? Numerous reasons drive human curiosity. At one level, the Outer Solar System is the “Frontier,” the “Edge of the Planetary Neighborhood.” Another diving motivation is the age-old question “Are We Alone in the Universe?” The intriguing prospect that life may have started on Europa, that pre-biotic chemistry may have occurred on Titan, that Pluto is a “deep freezer at the edge of the solar system, and that comets may contain the inventory of the organic and inorganic building blocks available for Europa and Titan as the starting points in their evolution, all drive our interest. Finally, there is the oldest imperative all for human exploration of , “because it is there.”

In response to this enormous interest in the Outer Solar System, a Workshop was held at the Lunar and Planetary Institute on February 21 and 22, 2001. There were three focus groups; one on primarily astrobiological targets, a second on primarily non-astrobiological targets, and a third on technology.

In addition, there were two plenary sessions. Eugene Levy, Provost of Rice University, gave an inspiring presentation with an important message: A disproportionate number of abstracts were submitted by NASA centers compared to non-NASA organizations such as universities. This distribution points to NASA needing to open competition for advanced technology funds to competition through an Announcement of Opportunity process, to see if this currently disproportionate distribution withstands the scrutiny of merit review. Timothy Kreider, a cartoonist, gave a humorous talk based around his space-theme cartoons, providing a much-needed interval of levity.

What follows is a brief summary of the Workshop. For a detailed recitation of each of the papers the reader is referred to the abstract book (LPI Contribution 1084).

Focus Group 1: Strategic Objectives and Key Capabilities for Current Mission Concepts

Overview:

The present outer solar system mission queue consists of Cassini already in space, and a Europa Orbiter and a possible Pluto-Kuiper Belt flyby. It has long been recognized that follow-on missions to Cassini-Huygens (arrival at Saturn and Titan in 2004) and Europa Orbiter (possible arrival to Titan and Europa in 2013), respectively, would have enormously compelling scientific rationales as well as great public appeal. Many innovative ideas on such future missions, their objectives, and possible instrumentations were presented. Technologies and instrumentation for comet nucleus science were also discussed. There was less discussion of future Neptune-Triton missions, reflecting the inherent difficulty at present of mounting such an ambitious deep space mission.

Mission Strategies:

Even at our present level of understanding of Europa, it is possible to conceive and design a scientifically credible instrument package for a future lander. It is less clear what shape the landing system itself should take. The highest resolution Galileo images show a very rough surface compared to that of the Moon, presumably reflecting the very geologically active surface of that body and the general lack of erosion and fine-scale impact gardening. Galileo images also show Europa to be geologically heterogeneous, so we cannot be sure of what hazards to landing the local-scale geology of a future site will present. This is a problem that is unlikely to be solved until Europa Orbiter data are in hand, so innovative landing systems that would work in a variety of environments and degree of targeting accuracy should be sought. One possibility presented was an airbag landing system (rather than the traditional retrorocket and landing legs). While designed as a proof of concept to see what the minimum landed mass would be, it clearly could be scaled up to apply to the entire Europa lander strawman science package as previously proposed by the SSES (or to version of the Mars Pathfinder for Europa).

All missions involve risk, and because Europa is such a compelling exobiological target, it is logical to think of a multi-decadal Europa program, echoing the architecture of the Mars program, but recognizing the longer flight times, greater costs, and more limited resources of Europa exploration compared with Mars exploration. The present paradigm involves an orbiter, followed by a lander, and if it is deemed feasible, a subsurface exploration vehicle capable of reaching the ocean (the presence of which is building towards a scientific consensus). What we may learn about Europa in the future may indicate an ice shell so thick that the subsurface mission would be deemed impractical, or a shell at least locally so thin and/or geologically active, that the lander mission would need to rethought. Again, an innovative way to accommodate these possibilities into a lander mission design should be sought.

A possible way to mitigate risk *to a program* is to return to an older idea: building and launching a second spacecraft. For this to realize any savings, however, the second build must be done in parallel with the first, not later. For Europa Orbiter, the second spacecraft could follow months or years later, and if the first orbiter was deemed a success, the second orbiter could be retargeted to Ganymede or Io, bodies also of enormous scientific interest, and ones for which an orbiter or multiple flyby mission (with a similar instruments to Europa Orbiter) would logically be the next step after Galileo. Such a retargeting scheme would require certain innovations in programmatics. A second spacecraft would be an unlikely choice in a severely cost constrained program, but it is an option that could prevent truly long (>20 yr.) delays in exploring Europa as well as allowing a possible return to Ganymede and Io, bodies that that are presently not in the future possible mission queue.

The future of a robust Outer Planets Program requires better access to all outer solar system bodies. Current chemical propulsions systems are presently stretched to their limits, unless one is simply trying to fly by Pluto or go into a extended orbit around Jupiter, as examples. It has long been recognized that advanced propulsion systems offer the possibilities of more rapid and hence achievable missions such as a comet-nucleus sample return or a Neptune system orbiter. Presentations were made on the present state of advanced electric (sub-kW) ion engines and Stirling radioisotope power converters. Both technologies hold great promise for outer solar system exploration, and have the distinct environmental advantage of not requiring space reactors.

Innovative Instrumentation:

A number of innovative in situ analytical techniques were presented, all of which show great promise. A laser ablation time-of-flight (TOF) mass spectrometer offers enormous potential for sampling various target, obviating the need for sample acquisition and preparation. Even limited mobility on the surface of a body like Europa and Titan for such a system could be invaluable. Such an instrument could measure major element ratios, isotopic ratios of some elements to the few percent level, and organic mass spectra. A reflectron TOF mass spectrometer sampling satellite atmospheres is also a technique of enormous potential, and obviates the need to necessarily go to the surface. Gamma/neutron mass spectrometers can measure natural radioactivities of outer planet objects and provide evidence on the distribution of H and the mean composition of the regolith. Neutron-alpha activation spectrometers can analyze major elements, some trace elements, and some light elements (H, C, O, N) present in water, ices, and biological or prebiotic materials. Laser-induced breakdown spectroscopy in concert with Raman spectroscopy can deliver elemental, mineralogical, and biological information. While the real capabilities of these instruments remain to be demonstrated, all laser techniques have the common characteristic of allowing analysis at some distance from the platform on which the instrument resides. Finally, age dating of outer planetary body surfaces was proposed - see below.

As a complement to the elemental and isotopic measurements above, an innovative technique was presented by which specific molecules could be detected by an enzymatic lock-and-key method, once the template is imprinted in a stable polymeric substrate. This technique, already in use by law enforcement and national security, opens up numerous possibilities for detection of complex protobiological (or biological) molecules on Europa or Titan, as well as their chirality, which may be signature of (proto)biological evolution.

One of the fundamental unsolved problems in the outer solar system is the interplanetary correlation of geologic time. Radiometric dating of icy surfaces was discussed and judged possible from at least three perspectives: 1) long-lived potassium-40 is probably a component of the dissolved salts in icy satellites (thus allowing traditional K-Ar dating), 2) cosmogenic spallation-produced noble gases could give surface exposure ages, and 3) C-14 dating could be applied to any active geological systems on Titan. The first two offer the most general promise. If any icy satellite surface can be dated, then sophisticated dynamical models that link the impact rate at any body and its variation over time can be used to assign crater ages to surfaces of satellites throughout the outer solar system. At present, however, the necessary instrumentation is lacking, as are the necessary experiments to measure the diffusion rates of the noble gases of interest in ultracold ice. The work presented should be followed up.

Nongeochemical measurement techniques were also discussed. Accurate topographic information, derived from laser and radar altimetry, has been a boon for understanding the Moon, Mars, and Venus. Information derived from stereo imagery for the Galilean satellites has also been of great scientific value, but coverage is limited. It is clear that future missions to solid outer solar system bodies would greatly benefit from using these altimetric techniques. Landed science stations could carry seismometers, as has long been recognized. Even a single, short lived lander on Europa could make major discoveries, as the number of *tidal* seismic signals in the Europan ice shell is likely to be large (the surface is densely faulted), and reflections and frequency cutoffs could yield local crustal ice structure and thickness.

Titan Airways

This subheading is not intended to be glib, but to indicate that Titan's atmosphere, 4 times as dense as the Earth's, is a unique environment in the outer solar system that offers many possibilities for *truly mobile exploration*. Numerous presentations were made on how to explore Titan's atmosphere and surface from an aerial platform. These ranged from 1) the now-traditional balloon-like aerover, 2) an inflatable, and thus floatable surface rover, 3) an airship (mini-zeppelin), 4) helicopter (!), and 5) vertical lift (ducted fan) vehicle. All of the powered devices need far less energy to operate (generate lift) on Titan than on the Earth. All are capable of descent to and ascent from the surface, and innovative techniques to acquire samples for on-board analysis were also discussed. An informed choice of how to proceed at Titan will require greater knowledge of Titan's surface, atmospheric winds, and likelihood of storms or other turbulence, information that should be available in a few years.

Focus Group 2: Strategic Objectives and Key Capabilities for New Mission Concepts

Focus group 2 papers addressed a broad range of objects outside of the Focus 1 targets for outer solar system exploration. These targets included the giant planets, their rings and their satellites, small bodies and dust debris in the outer solar system. A variety of delivery and sensing techniques were proposed, from direct sampling through long-range radar sounding of bodies.

The giant planets are important cosmogonic targets because of the rich range of phenomena that they exhibit. Although the Galileo probe, Galileo Orbiter, and the Pioneer and Voyager spacecraft made important measurements of Jupiter's key elemental abundances, magnetic field geometry, and gravitational moments of Jupiter, much remains to be understood about the planet's formation.. Further, none of the three other giant planets has been examined to a comparable level of detail--a gap that will be partly repaired by 2008 at the end of the Cassini mission to Saturn. Insofar as the extra-solar planets discovered to date are Jovian in mass, and the prospects for detecting much less massive bodies around other stars remains at least a decade away, understanding the formation of giant planets is an important Origins goal. The timing of formation, interactions with the nebular disk and hence migration mechanisms, implications for delivery of volatiles to terrestrial planets and effects on the formation of the terrestrial planets, are all key problems for the giant planets that will inform our eventual search for *habitable* terrestrial planets. And so, we must continue to explore the giant planets to measure key interior elemental abundances (e.g., O, N), fully map gravitational and magnetic fields to reveal internal structure, determine the dynamics, origins and longevity of rings, and understand the nature of atmospheric dynamical processes. From this information we determine not only the nature and evolution of the giant planets of our own system, but can also interpret the spectral signatures seen for extrasolar giant planets.

In view of these scientific considerations, the most significant outcome of the Focus 2 discussions is that *Discovery Class missions can now be flown to Jupiter and beyond, and return science addressing key NASA objectives.* There was general agreement among the participants that this situation is an evolutionary development, and not the result of a single new technology suddenly becoming available to the NASA program. Incrementally, the cost cap on Discovery and the cost of flying in the outer solar system have approached and now crossed each other. A significant uncertainty plaguing this assertion remains the availability, performance, reliability and cost of the nation's launch vehicles, and we recommend that NASA pay close attention to developments in the launch vehicle technology arena. Several Discovery mission concepts were presented. If realized these missions would make important measurements of the compositions, structures and dynamics of the giant planets, and thereby provide fundamental contributions to answering the questions of planetary origins, evolution and phenomenology.

Another theme of Focus 2 was that *the major satellites of Jupiter in addition to Europa are important and exciting future exploration objectives.* Much of the excitement about Io, Ganymede, and Callisto derived from measurements made by the Galileo

Orbiter during the very late stages of its mission, after the initial excitement about Europa motivated a programmatic response in the form of the Europa Orbiter mission. While these results do not blunt excitement about Europa as an astrobiological target, nor imply any programmatic rethinking about Europa exploration, they do point to the other three Galilean moons as dynamic and exciting targets of exploration unto themselves. Because of its high heat flow and types of volcanism, Io could be studied as an analog to the Earth during the Archean era (or even the Hadean, that earlier period of Earth's history whose geologic record has not survived), when heat flows were comparably high, and komatiitic volcanism was common. Io missions should map spatial and temporal variations in the volcanism, details of how the crust is organized, and the dynamical interaction between interior and heat sources. Understanding the tidal interactions between Io and Jupiter will help constrain the equivalent but weaker Europa-Jupiter interactions. Because of the extraordinary radiation environment at Io, such studies will need to be undertaken from a spacecraft in a high eccentricity orbit around Jupiter.

Ganymede and Callisto have intrinsic and induced magnetic fields. Ganymede's actively convecting interior, generating a magnetic field, is interesting enough, but the possibility that both it and Callisto might have liquid water interiors poses special challenges for understanding what has happened to these satellites over time. If indeed the Jovian satellite system has been subject to dynamic orbital evolution well after formation, the implications are important for understanding the evolution of giant planet systems in general, in our own and other planetary systems. Several presentations made the case for returns to Ganymede and Callisto. Interest in the Jovian satellite system is the result of major new discoveries from the Galileo mission, and the Cassini mission may lead to comparrable future interest in the Saturnian satellie system. By 2008 we may have an equivalent set of surprises and mysteries to solve for the Saturnian satellites.

Other presentations emphasized that the distribution of dust in and beyond the giant planet system may aid with interpretation of dusty disks around other stars. The dynamics of rings provide insight into processes of planetary accretion. Comparative studies of the surfaces and interiors of Triton and Pluto as large former and current members of the remnant disk of giant planet formation called the Kuiper Belt are also important. A beginning to the accomplishment of these goals was expected to come from the Pluto-Kuiper mission, which at the time of the Workshop was assumed to be safely embedded in the NASA flight program and hence itself not the topic of discussion. Had the current concerns about the mission, engendered by the first budget blueprint of the new administration, been known at the time of the abstract submission and Workshop itself, the emphasis in Focus 2 might have been very different.

There is much to do in the outer solar system. It is a region that is vast in scale, with an enormous diversity of objects ranging from the most to least massive planetary bodies in the solar system, with important connections to observable structures and bodies around other stars, and with much to teach us about how planets form and where life ought to be sought. And contrary to prior prejudice, it may not be a particularly difficult region to explore, at least if the goals are properly constrained and missions carefully designed. If there is any general lesson to be drawn from Focus 2, it is that there remain great mysteries to solve and discoveries to make in the realm beyond the asteroid belt, and a plan of exploration to do so need not be prohibitively expensive or technologically too challenging for the nation's space program.

Focus 3: Technology for Outer Planetary Exploration

The fundamental question that must be addressed prior to defining a technology roadmap is: Why explore the outer planets? What is the long-range vision? Is the objective to conduct surveillance of the physical environment and transmit the information back to earth – or is it to develop the capability and infrastructure to operate with impunity at the farthest reaches of our solar system? Enabling a specific vision will ultimately decide which of many paths the technology takes. Without this focus, the technology will flounder – good science and engineering will be accomplished – but without direction the majority of the efforts will not transition to the Outer Planets Program or to NASA. We must avoid building yet larger castles in the sandbox of technology.

If the nation's commitment to the outer planets is an occasional surveillance mission (e.g., launching one new mission every few years) – then one could argue that no new technology is needed. This heretical position is based upon the fact that we can build systems today that can survive the harsh environment of outer space. Leveraging the advancements in commercial industry and other government agencies will allow enhancements to future missions without the concomitant NASA investment. These systems will be large, heavy, and expensive – but the life-cycle cost (i.e., adding the cost of the research and development) might justify such a view.

If, however, the nation committed to a long-term exploration and exploitation of the outer planets – then a robust and innovative technology program will be required to allow for cost-effective missions. The outer planets is the next frontier – and as such, we as a nation need to decide what potential benefits might accrue from "planting the flag" – and what cost we are willing to pay for taking the high ground.

Fundamental Tenets

In spite of the current uncertainty in the long-term commitment to the outer planets, there are some fundamental "truths" that can be used to guide the technology investments:

a) Cost is the major driver
b) Cost is proportional to the weight of the system (certainly true for the launch)
c) The speed of light provides the fundamental latency for command and control and has implications for the required reliability of components and level of autonomy in the system
d) The operational environment varies widely – with radiation and temperature being the primary drivers – again with major implications for reliability

In addition to the above, there are additional constraints that are not fundamental – but tend to impact the ultimate success of the mission:
a) Faster is better – funding cycles, research agendas, and national interest tend to have "attention spans" of a few years rather than decades

b) Adaptability is the key – be prepared to have a single system conduct multiple missions or have the flexibility to adjust the mission on the fly based upon enhanced knowledge of the target

Technology Components

Any mission to the outer planets will consist of a few fundamental building blocks. These building blocks include propulsion, power, electronics and bus, sensing, and communication. Added to these might be mission-specific capabilities such as sample collection and possible return. And threading through each category is the need for reliability. In each category, a plan must be developed that meets the mission objectives subject to the fundamental constraints listed above. We will assess each component in turn.

a) *Propulsion* – Potential concepts discussed at the conference included chemical, nuclear, solar sailing, and more esoteric concepts such as plasma wake generators or beamed power. In addition, for small-scale orbit changes around a body with a magnetic field, energy-harvesting techniques (such as tethers) offer a viable and innovative alternative. Each of these concepts varies in mass fraction, •-V capability, and level of technical maturity. Compounding the confusion amongst these ideas is the public perception (misunderstanding) concerning the risks associated with nuclear options. Chemical propulsion is well understood and only minor efficiency gains are anticipated over the next few decades. Potential areas of improvement include use of new materials for light-weight tanks and infrastructure – but fundamental breakthroughs are unlikely. Nuclear options offer high -delta-V, reliability, and a convenient source of electrical power in conjunction with the propulsion. The downsides to nuclear are primarily political – due to public apprehension over launching nuclear material. But an additional, real concern, is the deleterious impact the radiation could have on electronic components over a long-duration mission. Solar sailing offers the most romantic option – but probably the least attractive for outer planetary exploration. The •-V is relatively low and the physical size makes the probability of damage due to micrometeorites high. Given the lower performance anticipated it is unlikely that solar sailing will be a major contender for these missions. The more esoteric concepts (e.g., plasma wake generators and beamed power) are very immature. They should be objectively assessed as to the potential payoff for future missions. The potential payoff should dictate the resources devoted to further research and development. For the short-to-moderate term, nuclear propulsion appears to be the most attractive option. It provides the requisite performance, reliability, and appears relatively technically mature. The main issue appears to be political – not technical. Where technology may become a player is in developing concepts that mitigate any risk of radioactive release or environmental damage even if a catastrophic failure occurred. For example, nuclear material could be processed in space from a lower quality (non-radioactive precursor) to make “propulsion-quality” fuel. For small satellites injected into orbits around the outer planetary bodies, innovative concepts that exploit the ambient gradients (such as tethers utilizing magnetic gradients) should be explored to provide essentially unlimited station keeping capability.

b) *Power* – Developing adequate power for the outer planetary missions divided into two competing but complementary camps. The first camp was attempting to ensure a robust and adequate source of power – the other camp was building electronics and sensors that minimize the amount of power required. For on-board power supply, radioisotopes coupled with thermoelectric converters emerged as a key player. Alpha-voltaics promise very high efficiencies, but radiation damage limits their lifetimes to about ten years (short compared to the outer planetary mission requirements). Nuclear reactors (as discussed in the propulsion section) are also a viable source of power. Once on (or near) specific outer planetary bodies, energy harvesting schemes may become practical (an example may be geothermal power on Io). In the second camp of low power electronics and sensors, substantial effort is ongoing in both other government and civilian research centers. As the era of ubiquitous computing and sensing begins to become a reality – we would envision an acceleration of innovative schemes to substantially decrease the power required to perform certain tasks. Thus a dedicated investment in this area by the Outer Planets Program may not be required.

c) *Electronics and Bus* – The major issue that surfaced was the need for rad-hard electronics. An issue remaining is how to leverage the tremendous commercial investment in silicon for applications in radiation intense environments. SOI (silicon-on-insulator) technology seems to present the answer – but failure modes need to be better understood. Realistic test beds and experiments that can accurately emulate the mission environments anticipated (including the effect of relativistic electrons) are essential. The impact of extreme low temperatures is also critical. The effect can be pronounced on silicon, which behaves like an insulator below 100 K. There are two possible approaches: one is to use radioisotopic heating units to keep the electronics warm, and the other is to develop electronics that work at low temperatures such as mica and solid tantalum, which maintain their capacitance at low temperatures. Other schemes to maintain reliability need to be investigated. For example, self-test/self-repair of the electronics and bus may be possible. An alternative approach (potentially promising but longer term) may be hardware and electronics that can evolve as the local environment changes. This is a potentially very exciting research area with applications well beyond the Outer Planets Program. One topic not adequately addressed at the conference was developing robust autonomy (beyond self-repair). Due to the large distance to the outer planets, and hence the substantial communications latency, it is critical that robust and relatively sophisticated autonomy be developed. This issue will become acute as missions start to land on the outer planetary bodies. As an example, consider the painstaking slowness with which Sojourner traveled on Mars. Due to the lack of autonomy and the communications latency, Sojourner's odometer measured in the 10s of meters – the situation will become more apparent as the distances increase. In addition, our knowledge of the local environment will be much worse – forcing the explorer to adapt to potentially radically changing conditions.

d) *Sensing* – Much of the effort discussed in sensing involved miniaturization of existing instruments or components, primarily to satisfy low power and mass constraints. Many of these efforts were innovative (such as the work on nanotubes for electrophoresis and the work on quantum dots) but appear to have requirements that are consistent with the needs derived from commercial or other government customers. There were very few research efforts briefed that emphasized developing new tools and techniques unique to achieving specific objectives of the Outer Planets Program. Examples of unique needs may include developing sensing modalities for recognizing non-terrestrial life, assessing the ice thickness on Europa, or finding and quantifying sources of water. It would appear that in the "mission-pull" and "sensor-push" paradigm, the connectivity between the technologists and scientists appears to be weak. The Outer Planets Program should work towards ensuring that the communication across disciplines is improved as well as force a greater justification ("mission-pull") for the sensing technologies being pursued.

e) *Communications* – The ideas presented follow the trends observed in the commercial and Department of Defense. The emphasis is on alternative frequencies (including optical), more efficient encoding, and larger collecting areas (either through inflatable/erectable dishes or through distributed apertures). An area not briefed during the conference was communications for extreme environments (such as connecting probes *under* the Europa ice cap to space). For the short term – existing technologies and capabilities can suffice for communicating between Earth and the probes. Eventually – if a large and permanent outer planetary presence becomes a reality, then the Deep Space Network will be overtaxed and new communication schemes must be considered. Ideas under consideration include forming relay stations or optical beams. In either case, significant infrastructure may be required. Building this infrastructure can be considered analogous with the investment in the early rail and telegraph networks. A reliable connectivity across the vast distances will be required prior to a large and permanent presence.

Technology Recommendations:

1) Define a compelling vision – and use this to focus the research efforts
2) Conduct an analytical assessment of potential technologies and make the investment strategy match the needs (use life-cycle cost as one of the metrics)
3) Maintain a long-term view – invest in technologies that have potential large-payoff in the future
4) Develop nuclear propulsion and power options -- this may require a public education / awareness campaign
5) Explore innovative energy harvesting technologies for long-duration presence on (or orbiting) the outer planetary bodies
6) Form a partnership with the Department of Defense and the Defense Threat Reduction Agency on developing rad-hard electronics and/or alternatives – leverage commercial manufacturing capability when practical
7) Improve the communication between the technologists and scientists – optimize the connection between "mission-pull" and "sensor-push"
8) Explore autonomous operations to the fullest extent possible
9) Explore multi-mission or adaptable mission payload configurations

Concluding Remarks

The Workshop was judged a success, based on the attendance, the enthusiastic presentations and discussion, and the innovative scientific and technology concepts unveiled. The Workshop echoed the broad support for Outer Solar System Exploration seen in the scientific community, the public (witness the thousands of letters of support stimulated by the Planetary Society), and by the Congress.

A successful Outer Planets Program should recognize the enormous interest in "life", broadly defined, that has engaged the Nation's space program. Such a Program might include:

1. The already approved Europa Orbiter.

2. A mission to Pluto and the Kuiper Belt, the repository in deep freeze of the fundamental organic and inorganic building blocks of the solar system.

3. A mission to Titan, building on the unknowable, but exciting new information learned about that satellite from the Cassini Orbiter and Huygens Probe, with a focus on prebiotic chemistry.

4. A mission to a comet nucleus, the repository of the organic and inorganic building blocks immediately available in the neighborhood of Europa and Titan and, hence, the starting point for the evolution of those Moons.

Finally, where possible and appropriate, the Outer Planets program should be competed through the Announcement of Opportunity process in order to seek out the most innovative ideas from the broadest possible community.