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Report of the Terrestrial Bodies Science Working Group

Volume IX. Complementary Research and Development

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OVERLEAF: Complementary Research and Development—The Foundation of Planetary Exploration

Often less glamorous than the conduct of space missions, complementary research and development efforts provide the foundation and framework for planetary exploration. Theoretical models, laboratory experiments, field experience, computations, instrument development, telescopic observations, advanced studies and many other science activities derive their support from research and development funding. Four areas of R&D are illustrated: the "solar sail," with its large space structure and exotic material; the "ion drive," with advanced solar cells and power systems; a Mars rover with artificial intelligence, advanced autonomous guidance systems and advanced science experiments; and computer facilities for modeling atmospheric processes in space and time on a variety of planets

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PREFACE

This volume is one of a nine-volume series documenting the work of the NASA-sponsored Terrestrial Bodies Science Working Group in developing plans for the exploration of Mercury, Venus, the Moon, Mars, asteroids, Galilean satellites, and comets during the period 1980-1990. Principal recommendations and conclusions are contained in Volume I (Executive Summary); reports and working papers of the study subgroups are presented in Volumes II-IX.

This volume is the report of the Complementary Research and Development subgroup, whose members and contributors are F. P. Fanale (chairman), W. M. Kaula, T. B. McCord, and J. L. Trombka.

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SECTION I

INTRODUCTION

The review this committee has carried out of the existing and potential capabilities for solar system exploration, the time scale for the preparation and execution of spacecraft missions, and the history of past missions indicates once again that stronger emphasis must be given to the conception and development of a wide spectrum of experiments, instruments, and vehicles in order to derive the proper return from an exploration program. Of equal importance is the effective use of alternative methods of data acquisition involving ground-based, airborne and near-Earth orbital techniques to supplement spacecraft missions. The use of these alternative methods to achieve important science objectives allows more sophisticated spacecraft missions to be designed. Continued reduction and analysis of existing data including laboratory and theoretical studies are required in order to benefit fully from experiments and to build on the past programs toward a logical and efficient exploration of the solar system.

We recommend a more vigorous and broad Supporting Research and Technology (SRT) program in support of terrestrial bodies exploration in order to better prepare for future mission opportunities, and as a cost-effective means to derive the maximum value from data already returned.

SECTION II

AREAS OF SRT REQUIRING SUPPORT

The need for SRT programs covers many areas and is continually changing as the exploration program continues. We attempt here only to point out a number of general areas of importance and a few examples within these general areas. This should not be taken as a "shopping list" but as an attempt to illustrate the needs.

SRT recommendations have been made in the past and have usually resulted in limited action. This committee feels that a more coherent and effective SRT program might be achieved if a special NASA committee were established to conduct reviews and recommend on overall SRT needs. It is our recommendation that establishment of such a committee be considered.

In the meantime, the following areas are among those where we see a special need for support:

A. USE OF ALTERNATE METHODS OF ACHIEVING SCIENCE OBJECTIVES

The more that can be learned about an object or a phenomenon the more exciting, productive and sophisticated future missions can be. There is little danger of these alternative sources of data eliminating the need for new missions, but there is considerable danger of flying poorly conceived experiments on a given mission with the results predicted and duplicated well before the mission arrives at its destination.

1. Ground-Based Observation

The past efforts in ground-based and airborne astronomy have continually proven valuable in planning missions. Topography on Mars and Venus, surface temperature and pressure, structure and composition of Venus's atmosphere, composition of asteroid surfaces, compositional information about the surface of Mercury and Mars, surface chemistry of Mars, the existence and approximate strength of the Jovian magnetic field and radiation belts -- all were examined first and often only through ground-based observations. Some of the significant achievements of ground-based observations are listed in Table 1. As techniques improve and if astronomy is allowed to grow with technology, many more important results could be obtained. We point out the need for continued and increased support for the operation of ground-based and near-Earth orbital observations, the special need to insure telescope time for planetary studies, and the need for continued rebuilding of instrumentation and development of new techniques.

2. Data Analysis

A successful mission is defined partly by the new information obtained about an object and the suggestions of new experiments to further improve our understanding. Unless data returned by a mission are fully reduced and analyzed by a wide range of scientists, the mission cannot be considered a complete success. We recommend that more emphasis be given at the mission level to support the returned data analysis.

3. Theoretical and Laboratory Studies

Laboratory and theoretical studies are required to understand the implication of results obtained from past missions. These background studies are also necessary to understand or anticipate the environment new experiments might encounter, as the Viking surface chemistry results illustrated. A concerted effort should be made to support laboratory studies that will be needed to understanding future remote sensing data in terms of the mineralogy, chemistry and in situ state of planetary surface material. In the case of the Viking lander results, it was found that studies of the effect of the Martian surface environment (e.g., UV radiation) in producing a unique surface chemistry have played a crucial role in the interpretation of Viking data. Yet such "simulation" studies had only been supported at a very low level.

Another example is spectral data: investigators are actually observing IR bands on some objects that they are having difficulty understanding in terms of surface mineralogy because of a lack of analogous laboratory data on minerals in the same wavelength range! These are only examples of the need for laboratory analog studies of the remotely obtainable properties of materials as they may occur in their space environment.

To fully support the envisioned program of exploration, laboratory studies should include major efforts in emission spectroscopy of minerals at Venusian temperatures, UV and IR reflection spectroscopy of minerals, gas-gas and gas-mineral reaction kinetic studies at all temperatures (including non-thermal activation), absorption spectroscopy of gases, cloud and aerosol studies. Other studies should include such topics as equilibrium between gases and surface materials and exospheric escape processes. Theoretical modeling of atmospheric circulation and planetary internal thermal evolution are basic. Attempts to constrain planetary interior and atmospheric evolution to explain data reflecting directly on atmospheric and magnetic chronology should be emphasized as should those which attempt to synthesize planetary chemical and physical data to constrain speculation on the bulk composition of objects.

Continuing vigorous activity in the area of theoretical modeling is needed to improve our understanding of the implications of current data and to develop ideas for new observations and critical measurements to be performed by the next generation of missions.

Table 1. Some Significant Achievements of Ground-Based Observations

Microwave determination of Venus surface temperature
Radio determination of existence and approximate strength of Jupiter magnetic field
Determination of composition and pressure of Mars atmosphere
Discovery of water ice on the surfaces of Ganymede, Europa, the Rings of Saturn and Saturn's satellites
D/H ratio in Jupiter's atmosphere
T/P structure in Jupiter's upper atmosphere
Determination of Vesta's surface composition and the classification of asteroids
Discovery of the extended atomic clouds around Io
Determination of the diameters of Io, Ganymede, and Titan
Radar images of Venus' surface
Photo-oxidation in surface chemistry on Mars
Characterization and extent of geochemical units on the Moon
Presence of water in the Mars atmosphere and surface material
Determination of the rotation rate of Mercury and Venus
Basaltic composition of Mars' surface
Constraints on the size of ring particles from radio, optical and radar observations
Presence and nature of iron oxide in Mars' surface material

B. NEW EXPERIMENTS, INSTRUMENTS, AND DELIVERY SYSTEMS

The lead time required to build and certify an instrument for flight is about that usually available when a mission is first defined. As a result, new experiments have been very difficult to develop. It is necessary to begin developing experiments and instruments well before a mission is officially begun. Additionally, early support should be given to adapting existing space instruments to new or unusual space environments (e.g., radiation, thermal) and spacecraft requirements (e.g., shock, volume, and deployment). Alternative approaches, and a variety of techniques, should be explored to insure the most effective payload. Long-range planning is needed to define mission concepts well before new starts so that appropriate time for instrument development is available.

1. Entry Science

There are probably no unsolved problems in safely entering any of the planetary atmospheres including that of Jupiter. In addition, nearly all instruments which one might want to include in a typical entry mission will soon exist as flight hardware through Pioneer Venus or PAET. There are however three specific areas which require further development: (1) procedures for attaining greater sensitivity (e.g., better than 0.1 ppm) in mass spectrometer/gas chromatograph analyses, particularly for inert gases; (2) high-temperature (e.g., >500 K) technology for survival of electronics on entry probes and balloons and for survival of the balloons themselves; (3) techniques for *in situ* chemical analysis of volatile and involatile particulates in the atmosphere.

2. Orbital Geochemistry Systems

a. *γ-Ray Spectroscopy for Elemental Analysis*. Although lower energy resolution detectors ıch as NaI (TR) can and have been used in space (Apollo, Mars 4, 5, ·nd Luna series), significant improvement can be achieved using solid-state (intrinsic Ge) detectors. Thus large intrinsic detectors and both passive and active cooling systems need continued development for planetary exploration programs. Radiation background problems need to be studied, including those due to natural radioactivity in surrounding materials, induced activity from cosmic ray primary and secondary radiation, and trapped planetary radiation belts. These effects greatly influence the detection sensitivity and the feasibility of application to a given planetary body. Orbital gamma-ray spectroscopic mapping methods show most promise for application to Mercury, Moon and Mars missions.

b. *X-Ray Spectroscopy*. Both dispersive and nondispersive techniques can be used for X-ray geochemical mapping of planetary surfaces. Nondispersive systems are used for near-approach observation while dispersive systems are most useful at large distances from a planetary surface. The major area of research required for nondispersive X-ray range is in the development of large-area high-resolution detectors.

Research in the development of Si Li and barrier-type detectors will greatly enhance X-ray mapping capabilities. Because the X-ray emission is soft (i.e., 1 to 6 keV), this technique finds greatest application to planetary bodies lacking significant atmospheres (e.g., Mercury, the Moon, Galilian satellites, asteroids and comets). The radiation fields in the Galilean satellites may be so high as to eliminate the utility of these nondispersive systems. In these cases, it may be possible to observe these bodies at large distances from the body. Focusing dispersive systems may be considered for this application.

c. Alpha Spectroscopy. This technique has been used on the Apollo orbital science package. Owing to system sensitivity and poor counting statistics the experiments were only marginally successful. The importance of the information that can be obtained from such measurements urges further research in alpha spectroscopy.

d. Multispectral Imaging and Reflectance Spectroscopy. Geochemical units can be characterized in terms of mineralogy using reflectance spectroscopy, and the spatial extent of these units can be determined by multispectral imaging. The spectral region of interest covers the IR, visible, and UV spectrum. Some work already has been completed to develop image detector systems for orbital application. More work needs to be done in the instrumentation area (e.g., spectrometer development, IR area detector, CCD detectors and passive cooling). More laboratory and ground-based spectroscopic observations must be obtained in order to maximize information concerning elemental composition and mineralogy. These techniques are applicable to all the planetary bodies with solid surfaces except those with dense atmospheres.

e. Neutron Albedo Measurements. Measurements of the neutron albedo spectrum from a planetary body is related to the hydrogenous content of its surface material, its surface density and atmospheric composition and density. High-altitude studies of the neutron albedo of Earth have been carried out. Theoretical studies of the capability for detecting hydrogenous materials on the Moon and Mars have also been carried out. Development of neutron albedo flight instruments is required (e.g., BF^3, He^3 acid proton recoil detectors).

f. End-to-End Data Processing System. End-to-end data processing systems have been studied in the development of Lunar Polar Orbiter instruments and their integration aboard that spacecraft. The use of distributed intelligence systems by means of microprocessors now available can greatly simplify the problems of instrument integration, on-board data processing, command systems design, telemetry formatting and transmission, and development of test equipment from instrument design to integration. These studies are of general application and should be continued. Early software engineering studies of the on-board to ground-based system can later affect the design of the total system. Decisions on tradeoffs between such problems as the extent of on-board processing vs ground-based processing and software versus hardware can thus be handled more intelligently.

3. Penetrator Science

The emplacement of an array of penetrators on a planetary surface would offer an outstanding opportunity to investigate the surface, atmosphere and interior of that planet. An array of 6 penetrators with seismometers could (because of their number and superb coupling to the object) provide evidence on seismic velocity vs depth, distribution of seismic sources, and identification of chemical or phase changes associated with planetary zonation. There do not appear to be any major problems in penetrator seismology; this area is exceptionally promising.

Penetrators offer the advantage of providing (afterbody) imaging of several widely separated sites on a planetary surface. There appears to be promise that sufficiently low weight and power cameras can be made for inclusion in the afterbody and can provide stereo multispectral imaging with resolution comparable to the high resolution mode of the Viking cameras after the (17,000 g on Mars) afterbody impact. Panoramic views are obtainable and seasonal changes in each scene could be monitored. The key to global meteorology is a network of observation posts. On Mars, for example, several simple shock-resistant packages designed to measure wind speed, temperature and P_{H_2O}, that are widely separated, could provide outstanding data. In the forebody, the total water in a volume of soil could be measured by a simple radiometeric experiment, or the gross distribution of H_2O among several mineralogic sites could be discerned using a slightly more complex soil heating/effused H_2O analysis instrument.

Interesting possibilities for chemical analysis also exist since the penetrator offers a chance to measure deep subsurface composition. An α backscatter/X-ray fluorescence device could provide analysis of major elements (>10%) to ±3% and minor elements (<1%) to ±10%. Light elements would be included. A γ-ray device would provide analyses of additional key elements and would be essentially immune to potential contamination problems.

The opportunity of obtaining six widely separated heat flow measurements is important since the heat flow is (along with seismology and bulk density) one of the most significant measurements in that it bears on both the bulk chemistry and the thermal evolution of the object. Severe difficulties are anticipated with this experiment and moderate difficulties must be overcome with some of the others. We recommend that the following questions be more thoroughly investigated so that a meaningful evaluation can be made of the relative value of penetrator sensor, rover science and hard lander science for each of several bodies:

(1) We need an evaluation of the handicap to imaging science caused by lack of even near-field mobility or ability to manipulate objects in the scene. Also, the handicap due

to the cameras being only 40 cm above ground should be evaluated using, for example, the Viking II landing site boulder distribution and topography model or field tests.

(2) The problem of impact heating and RTG heat for the heat flow experiment should be further studied along with means for circumventing it. Without heat flow measurements, the special capabilities of penetrators may be seriously compromised.

(3) Ways to prevent the chemical contamination of soil immediately surrounding the penetrator and/or mitigate its effect on the X-ray results should be studied, as should the effects of the RTG on the γ-ray experiment.

(4) Impressive progress has been made in sample acquisition by drilling, but the degree of possible "housekeeping" control (i.e., knowing what is being sampled) may never be comparable to that offered by a rough lander or rover. The seriousness of this problem (i.e., blind and predetermined drilling) should be studied.

(5) The general tradeoffs between the value of a number of widely separated sites vs near-field or longer-range mobility should be studied as well as the relative value of subsurface sampling vs control of detailed sample selection, for each body. This would aid in evaluating penetrator vs rover or rough lander science. Also needed are more studies of how penetrators would be used on various-sized airless bodies.

4. Rough Landers

Rough landers (deceleration <300 g as opposed to 20,000 g for a penetrator) have been proposed as alternatives to penetrators for several landing applications. Initially proposed for the Moon (Ranger Block V) and later for Mars (Capsule System Advanced Development-CSAD), recent studies of Mercury and Ganymede landers have revitalized interest in rough landers.

The advantages of the proposed rough landers over penetrators include:

(1) Greater science payload (at least a factor of 2 larger than penetrators).

(2) Less rigorous deceleration.

(3) Greater data storage capacity.

(4) Greater ease of thermal control.

(5) Higher data rates.

(6) Larger and less restricted volume for science instruments.

Many of the disadvantages of rough landers lie in the area of science instrument development and deployment, and stem primarily from the lack of supported studies of these items. Some key questions which should be addressed by future SR&T efforts include:

Can effective seismic coupling be achieved?

Can adequate surface and/or subsurface sampling techniques be developed to supply material to geochemical instrument?

Can planetary heat flow be determined using a drill-hole or other form of subsurface implantation?

Can geochemical or biological instruments which cannot fly on penetrators owing to acceleration, mass, or volume constraints be incorporated into rough lander payloads?

What are the relative advantages of rough landers and penetrators for obtaining panoramic multispectral images?

The most important problem remains in providing sufficient support to evaluate all potential lander configurations -- both penetrators and rough landers -- in light of engineering and science instrument constraints.

5. Rovers

Planetary rovers can provide a link between the type of detailed information which can be derived from returned samples and global information obtained by orbiters. Rovers now under study could someday provide a set of reconnaissance data (e.g., morphology, chemical composition, spectral reflectance) along traverses conceivably thousands of kilometers in length, encompassing several types of terrain or geological or geochemical provinces. It is not expected that rovers will ever be able to return detailed information such as the precise isotopic analyses that have been conducted on lunar samples returned by astronauts or the Russian unmanned sample return spacecraft. However, the type of knowledge obtainable by a less sophisticated set of experiments conducted over a traverse of great length cannot be replaced by the type of knowledge which can be gleaned from studies of a returned "grab sample," however sophisticated those studies may be. To some extent, even a grab sample can provide considerable information: especially in cases where wind or impact transports material over wide areas on the planet. However, some properties (initial morphology, for example), are often largely destroyed by such transport, and the complementary value of wide-ranging *in situ* studies should be obvious.

Mars is the most pressing example that concerns us. It is envisioned that a Mars rover payload would contain at least (1) α-scatter/X-ray fluorescence and γ-ray instruments for surface elemental analysis,

(2) high spectral resolution reflectance and transmission spectroscopy and multispectral imaging for characterization of the mineralogy and morphology of exposed surfaces (an X-ray diffractometer is an elegant but plausible addition), and (3) some means (possibly a microscope) for examination of individual grains and surface textures. Also, means for manipulating (e.g., breaking, scraping or drilling) rocks is desired. A mass spectrometer for soil volatile experiments and simple biological and surface chemistry experiments is advisable. Indirect biological experiments (those which allow biological deductions from chemical or morphological studies) should be emphasized.

One important contribution that rovers (even those of limited mobility) could make is in concert with sample return missions. The possibility of selecting a suite of samples for return which appear of outstanding interest based upon preliminary examination or analysis is scientifically exciting. One of the best features of a sample return mission is the opportunity to "gear up" after preliminary examination of the sample to specially examine some unforeseen property of that sample that appeared on preliminary examination. In an analogous way, an advantage of a rover is that it is possible to "high-grade" a previously gleaned suite of samples to accommodate any new or unexpected discoveries.

The following is a brief discussion of rover technology requirements, status and problems:

Rovers obviously must survive the extremes of the Martian environment, including duststorms, high winds and low temperatures. Total electrical power demands must be compatible with RTG capabilities. Sizes and masses must be compatible with launch vehicle payload constraints. In all rover concepts they must be able to acquire and manipulate rock and soil samples. For long range rover conceptual missions, onboard computerized hazard avoidance systems seem required. In such a case, onboard locomotion and manipulation functions imply ability to extract information about surroundings from onboard sensors, such as TV cameras and microwave or laser ranging devices, to plan sequences of action based on this information and to detect and correct errors.

Can the above requirements be met? Mobile systems and manipulators are being developed for a growing field of Earth-based applications, ranging from coal mining and undersea exploration to assembly-line operations and prostheses. A large variety of equipment is available. Most of it has not been designed with regard to stringent limitations on size, weight, and power, but a wealth of experience exists to guide the design of suitable manipulation and locomotion systems. Similarly, for the more conventional rover subsystems (e.g., structure, communications) there would seem to be no major technological advances demanded (although prototype development remains to be done).

The most difficult aspects of moderate-to-long-range rover missions are those relating to rover control. Integrated semiautonomous systems with capabilities of the kind being described in this short discussion have received attention at a number of universities and industrial research laboratories in the US and in several foreign countries. Most of the robotics work in the US has been supported by the Defense Department (although the funding has recently been drastically reduced).

To date, primary emphasis has been on the automation of vision and manipulation and their integration. Many hand/eye systems exist, and research is centered on improving them. Automatic interpretation of images--"scene analysis"--presents a number of challenging problems that are far from being solved. Much early work in this field dealt with simple regular objects in highly constrained settings. Recently, there has been a shift in emphasis to complex, rich scenes characteristic of natural environments. Progress has been slow, and it is hard to predict at what time fully autonomous scene analysis will become feasible, but the capabilities are steadily growing.

NASA's principal activity in robotics is a research program being conducted to demonstrate integration of mobility, locomotion, and scene analysis. Mars surface exploration is one application of the results of this program. The equipment is presently able to use data obtained from a laser range finder or from TV analysis, or both, to identify and pick up objects. Limited locomotion will be demonstrated soon. Within one or two years, mobility in a natural setting will be achieved, perhaps with occasional human assistance in scene interpretation or in task execution.

For a rover/sample return mission, an inertial navigation system may be used to traverse up to 250 km of Martian terrain to acquire data and return to the lander vehicle to return samples. The Department of Defense has designed such systems. Terminal descent should contain an automated hazard avoidance system. Versions of both of these systems are available for other applications from the DOD.

While most (although not all) of the rover requirements can be met through the application of existing knowledge, much engineering development is needed to produce flightworthy hardware. The present work is limited to a few rover functions. Before an actual Mars mission can be conducted, an engineering prototype must be built to demonstrate successful solution of the many engineering problems presented by the Martian environment, by the performance requirements, and by the physical constraints. Several areas in which work is needed are singled out here for emphasis:

(1) Power. A radioisotope thermoelectric generator or other power source that can meet the specific demands of the rover and be integrated with it must be designed. Development lead times are long. Power management also presents problems.

(2) Onboard Data System. The computational requirements of the rovers will be different in quantity and type from those imposed by previous spacecraft. Suitable architectures for their data systems must be developed and demonstrated. Reliability will be of paramount importance. Techniques of fault tolerant design will have to be applied and extended to the other elements of the rover system.

(3) Mission Operations. The partial autonomy of the rovers and the number of independent systems to be controlled will require departures from methods presently used for planning and conducting mission operations. Examination of these problems should begin well before the total mission system is designed.

(4) Robotics. The capabilities needed for semiautonomous control of manipulation and locomotion, while under development, are not yet in hand. Substantial advances are needed in the acquisition, processing, and interpretation of images and other sensory data. Specifically, both the hardware and the algorithms must be carried to the point that analyses can be completed rapidly and efficiently, with limited onboard computer resources, for the complex natural scenes that the Martian surface will present. It is unrealistic to expect that all such analyses can be done without some human assistance, but the savings in mission operations time and money and the increase in mission return that will result from increased autonomy in the performance of locomotion and some manipulation functions can be substantial. Additional research and development on the software that integrates the robot sensory and motor equipment and provides for onboard error detection and recovery is also needed.

6. Sample Return.

It is clear that many long-lead-time development items must be worked out well in advance of a sample return mission launch date. Perhaps the most crucial is the sample quarantine system. A JPL study has identified crucial considerations for this area and should begin to be implemented immediately, as funding permits. There is a clear possibility of useful interaction between technological advances required for a Mars quarantine system and current interest in protective systems for research.

Some specific areas of importance that require research and development efforts that can be initiated now are as follows:

a. Develop and Analyze Mission Options for Science Content. A variety of mission options have been studied, including direct return and Mars orbital rendezvous transfer as the Mars-Earth mode and direct entry or capture in Earth orbit as Earth return options. Each of these differs in complexity, risks, and capability in terms of such qualities as landed weight and ease of back-contamination problems. The best option will maximize the probability of successfully accomplishing mission science requirements within the budget for the mission. This will involve tradeoffs between engineering, science, and quarantine constraints.

b. Develop Systems for Increased Landing Accuracy and Landing Safety. The accuracy with which a chosen landing site can be reached and the ability to avoid hazards on landing will govern the site selection strategy and the mobility requirements for the lander. As some of the more interesting scientific sites are apparently the more dangerous to land at with present techniques, improvement in accuracy and hazard avoidance will significantly increase the number of potential landing sites that can be selected.

c. Develop Mobility Options. Mobility options range from tethered short-range rovers that are directed by the lander to totally autonomous long-range systems. The sampling and analysis capability of the various concepts should be developed. Either autonomous or joint lander-rover sample preparation and analysis systems should be investigated (e.g., complete rover laboratory vs rover capable of picking up large rocks, with sample preparation or analysis on lander).

d. Extend Capability of Surface Sampling System. Concepts should be developed for making a sampling system such as the Viking sample arm more versatile. Candidates are a rock chipper or short core drilling device which could be used as a soil or rock borer. A rock chipper would have been highly useful on Viking; a rock crusher is not as important for sample return but is desirable for in situ analysis and for discrimination of soil clods. The extended flexibility of sampling will be important for sample return as well as future planetary landers.

e. Develop Sample Sealing, Containment and Monitoring System. Vacuum seals of 10^{-10} ccHe/sec STP are obtainable routinely in a controlled situation on Earth. The design of a sample cannister that can be sealed remotely at that level requires development of new concepts. Maintenance of the cannister at Mars ambient P and T and monitoring internal conditions, while meeting requirements of sturdiness, will be significant challenges. Preparation of containers that do not build up internal atmospheres through outgassing is required. The general problem of survival of delicate soil constituents through any transport or quarantine/sterilization scenarios should be studied.

f. Develop Receiving Laboratory Containment System and Quarantine Protocols. The concept definition and verification of this entire system requires early work. The requirement to maintain systems at Mars ambient temperatures is a major difference with respect to previous technology for biological or radioactive material containment.

g. Continue Reduction and Analysis of Viking and Mariner Data to Enhance Landing Site Selection. The thorough evaluation of orbiter images obtained by Mariner and Viking is necessary to provide the best basis for selecting landing sites and establishing the framework for interpretation of sample data.

h. Continue Support of State-Of-The-Art Laboratory Analytical Capability. The amounts of Martian material returned may be small compared to that returned from the Moon and will be more complicated mineralogically. Under these conditions, high-sensitivity experiments on small subsamples will be required. Developments supported by the lunar program have revolutionized surface analysis and high-precision mass spectrometry techniques, among others. These are now being applied in many areas of science and technology. Similar developments and wide application of new techniques is a major objective of a supporting research program for MSSR.

7. Comet Instruments

Either a comet rendezvous mission or (especially) a comet flyby mission will require special designs of existing instruments and new types of instruments. We recommend early development of the following:

(1) Ion Mass Spectrometers. The large range of energies (~0.1 ev to ~1 keV) and the large range of possible masses (1 to 60 AMU or more) are difficulties for the usual ion mass spectrometers. Possible solutions are the cycloidal mass spectrometer or several mass spectrometers with different ranges.

(2) Neutral mass spectrometers. The principal problem here is the efficiency of ionization, which is 1% or less with the crossed electron beam approach. Devices using field ionization techniques may be the answer, but additional development is needed.

(3) Dust composition devices. Analysis of dust composition is very difficult but very important. The dust particles first need to be vaporized and then analyzed by means of a mass spectrometer.

(4) Reflectance spectrometer. Mineralogical analysis of the nuclear surface and mapping of mineralogical difference across the surface will tell more about what a comet is than any other measurement.

A major mission constraint will impact the orderly development of the direct exploration of comets. A complete program would include the probing of at least one of the very large comets which produce the entire gamut of cometary phenomena. Only one such comet has a known, predictable orbit: Halley's comet. Thus the era of the 1980's has special importance for cometary exploration.

C. PROPULSION

Developments in propulsion can be as important to planetary exploration as the development of instruments, and uncertainties in the former can also make long-range mission planning difficult. Several missions we recommend require low-thrust propulsion. Two approaches to low-thrust propulsion which could satisfy all our requirements are currently under study--"solar sailing" and "ion drive" (an evolutionary step from SEP). Both offer the potential of low rendezvous speeds (either could rendezvous with Halley's comet), delivery of multiple payloads, and an ideal way to use the shuttle capabilities for planetary exploration. But both are in the early design stages, and thermal or particle bombardment resistances of materials for both, and erection and stabilization problems for the sail, may yet prove to be severe. Enormous progress in ferreting out such problems, a decision between the two approaches and a firm development schedule are required. The following briefly describes each approach and its current status.

1. Solar Sail Study and Development

The solar sail is a means of using solar radiation directly as a method of propulsion. The sail is a large, flat, lightweight, highly reflective first-surface mirror. Outstanding features include its possible use as an interplanetary shuttle, its use for multiple planetary sample return, and its potential for delivery of high (possibly multi-ton) payloads to the inner planets. Three solar sail concepts are currently considered to be most promising for supporting the sail: spars, centrifugal force, and electrostatic forces. Space shuttle weight and volume capabilities improve the attractiveness of the concept. However, many questions still need to be answered concerning thermal and particle effects on the lifetime of the materials as well as problems of deployment, stabilization, orientations, and materials.

a. Principles. For a given reflectance, the inherent performance of a sail is a function of the total unit loading on the sail, i.e., total mass divided by the sail area. A heavier payload necessarily means a heavier unit load on the sail and a longer trip time. Missions to Mercury, for example, may have sail loadings as much as 50 g/m^2 or greater, while the limit for a rendezvous with Halley's comet may be as low as 6 g/m^2. If a sail were constructed of currently available materials, the resulting total unit load might range from about 7 to 10 g/m^2. Thus the mission to Halley's comet appears to require improvement in the current technology of materials processing; for other missions which are less demanding, currently available materials may be satisfactory.

b. Trajectories. Sunlight acting upon the sail results in a component of force acting in the outward direction from the Sun, unless the sail were turned edge-on to the Sun. The sail may be tilted so as to have a force component perpendicular to the solar radius line. This component may be directed along the velocity vector to increase the energy and angular momentum of the vehicle, moving the vehicle outward, or it may be directed against the velocity vector, reducing energy and angular momentum and allowing it to spiral in toward the Sun. Thus, in spite of the continuous existence of the radially outward force component, the solar sail is very versatile and may be directed to almost any target in the solar system.

c. Illustrative Missions.

1) Halley's Comet. The rendezvous mission to Halley's comet is a four-year mission which would be launched in the second quarter of 1982. It would rendezvous with the comet in March or April of 1986. The mission involves spiraling into orbit around the Sun (at 0.3 AU), which inclination is increased gradually to 160^{o}, which is the plane of the comet's orbit. The vehicle then goes through an energy gain phase, at the end of which it will rendezvous with the comet (after perihelion). At the rendezvous, the sail is released and the spacecraft will continue on with the comet for an indefinite period of time.

2) Inner Planet and Solar. An interesting concept for a solar sail vehicle is that of an inner planet shuttle. This vehicle is envisioned as a reuseable sail which would deliver spacecraft to various inner planets. It may carry multiple payloads on a single mission, then return to Earth, and spiral down to a low-Earth orbit for its next mission.

The payload capability of a solar sail to Mercury is quite high by current standards. With a flight time on the order of 900 days, a sail the size of the one used for a rendezvous with Halley's comet might be capable of carrying payloads of approximately 10 tons. The sail would enable the return of a sample from Mercury and, if used at Mars, could probably provide for the return of sample significantly greater than what could be achieved by purely ballistic means. A Mars lander of 5 or 6 tons might be delivered by a sail of the design used for a Halley rendezvous.

3) Comets and Asteroids. Solar sails may provide the means of returning samples from comets and asteroids during the 1980's. It appears possible that sails could accomplish a rendezvous and sample return from any of the short-period comets with perihelions inside of 2 AU. A sail vehicle might be capable of making rendezvous with and collecting samples from several asteroids on a single mission. Samples collected from comets and asteroids would be returned by the sail to Earth orbit for recovery by the Space Shuttle.

Several concepts for sail deployment and rigidization are under study as studies of the degradational effects of high temperature, particulate (solar wind) radiation, charge imbalance, and time on sail performances. These studies should be pursued intensively. A significant increase in the intensity of these feasibility studies could lead to sail readiness for a Halley's comet (1986) rendezvous launch in 1982. This mission is to be regarded as somewhat of a driver in the sense that a Halley's comet rendezvous (in a 55 km/sec retrograde orbit) cannot be accomplished by other means (but see below). A SEP, for example, could only reduce the relative velocity of spacecraft and comet to 35 km/sec. This should not, however, detract from the main point: that the sail is, in concept, a multipurpose vehicle which could increase payloads by orders of magnitude and serve as the planetary shuttle of the future. An important qualification to the preceding statements is that, although the conventionally visualized SEP cannot compete with the sail in potential for multiton delivery and high (>30 km/sec) rendezvous speed, it appears that a moderate evolutionary stage from SEP--referred to below as ion drive--may be able to accomplish a Halley's comet rendezvous considerably prior to perihelion.

2. Ion Propulsion Capabilities

NASA's Office of Aeronautics and Space Technology has had a development program in electric propulsion for over 10 years. The technology program encompasses both smaller engines for use in satellite stationkeeping and larger (30-cm-diameter) engines for primary propulsion.

Until recently, operation of these engines was primarily matched against a 1973 state-of-the-art solar array technology. However, with several breakthroughs in solar cell technology over the last two years, it is now possible to consider a much improved power-to-mass ratio for the solar power source.

Slight modifications to the already existing 30-cm engines along with reductions in power handling* requirements (currently under study) promise a 4- to 5-fold increase in performance capability of ion propulsion.

Table 2 provides a summary of the comparison between what has been termed "conventional" electric propulsion and high-powered "ion drive."

a. Illustrative Missions.

1) Halley's Comet. These advances are currently being emphasized as interest is rising in achieving a very difficult rendezvous with Halley's comet during its scheduled 1986 return. This mission could not be achieved by the 50-kg/kW technology. However, by considering the emerging thin-cell (3-mil-thick solar cells) technology for solar arrays, along with high-powered, direct-drive operation of the existing Hughes 30-cm ion engines, the electric propulsion system becomes much more effective.

For a Halley's comet rendezvous the flight path can be kept at a comfortable distance (0.5 AU) from the Sun. Secondly, it is primarily an outbound flight toward Jupiter's orbit, but no Jovian gravity-assist

*This refers to the already demonstrated operation of some engine components, such as beam and discharge, directly from the high-voltage solar array to reduce power processing requirements.

Table 2. Comparison Between Conventional Electric Propulsion System and Ion Drive System

	Conventional	Ion Drive
Power	15-25 kW	100-250 kW
Array mass (two wings)	312 kg	416 kg
Array drive/boom/electronics	22	22
Ion-engines	49 (6 units)	106 (12 units)
Gimbals	18	36
Power processing	206	213
Structure/truss	36	50
Thermal control	5	26
Propellant storage (including residuals)	35	70
Cabling	15	30
Pre regulator	5	5
DCIU	5	5
Total propulsion system mass	708 kg	979 kg
Specific mass	$a \approx 47$ kg/kW	$a \approx 10$ kg/kW

is required. Instead, the ion drive vehicle simply remains "outside" the comet's flight path, makes a power "U-turn" at about 4.5 AU, and speeds back toward the Sun, essentially already in the comet's orbit, and awaiting the comet's "overtake." In a flight time of about 3.75 years, ion drive accomplishes rendezvous with Halley's comet at some 50 days before the comet reaches maximum activity.

2) Multiple-Asteroid Rendezvous. The performance increase of the ion drive system now makes it possible to select from a number of multiple-asteroid rendezvous tours. For example, such missions can be deliberately designed to include different asteroid compositional classes. These missions are available with ion systems at only half the power required for the demanding Halley's comet rendezvous. Sample returns are being studied.

3) Interplanetary or Lunar Shuttle. Here is the natural extension to other planets of the near-Earth capabilities described earlier. Supply lines for construction elements, life support, and long-term sustenance can be envisioned for a team of ion drive "shuttles" operating on a "trade route" between Earth and manned bases on the Moon or at Mars.

3. Spacecraft

New major spacecraft systems and subsystems will be required in the future to assure cost-effective high science return from missions. Research work should continue on such science-related aspects of spacecraft engineering as (1) development of dual-spin spacecraft which combine some advantages of three-axis-stabilized and spin-stabilized platforms, (2) improved data handling techniques and use of microprocessor technology to achieve onboard data processing and facilitate science integration, (3) improved telemetry capabilities and ground data handling techniques, (4) development of new lander systems for atmosphereless bodies and associated instrumentation, and (5) improved thermal control for trajectories close to the Sun.

4. Interim Upper Stage

Development completion of the Space Shuttle and planetary mission configuration of the Interim Upper Stage (IUS) is necessary to carry out the exploration program outlined in this report.

SECTION III

DATA ANALYSIS AND SYNTHESIS

A. INTRODUCTION

Past, ongoing, and future planetary missions as recommended in this report generate an increasing variety and detail of data. The effective application of these data toward such goals as inferring solar system origin, understanding the Earth, and practical utilization of space requires correspondingly greater efforts in analysis and synthesis. As planetary exploration matures, it can be expected that more and more of the significant findings will depend on detailed data compilations, analogous to the establishment of plate tectonics from sea floor remanent magnetism, detailed bathymetry, and seismic motion analyses. Furthermore, these findings will require the development of theoretical models as synthesizing contexts for the data interpretation. As the complexity of this work increases, the needs for continuity of efforts, interactions between participating scientists, and comparisons among the several planets are enhanced. While part of this analysis and synthesis would take place as a consequence of the scientists' initiative, deliberate provision for its organization and support is necessary to assure the effective utilization of NASA mission products. This support is also necessary to assure the development of the ongoing consensus essential to the orderly planning of future missions and related SR&T. A final element is that if these increases in complexity and continuity occur in a climate of fiscal constriction, then more attention must be paid to organizing the research effort in an optimum manner.

B. CONTINUING ANALYSIS

While true knowledge is far from proportionate to number or bits of information, the great increases in quantity of data which will occur (e.g., 5×10^{11} total imaging bits from VOIR, compared to 4.5×10^{10} from Mariner 9 and 6×10^{10} from Viking Orbiter) will inevitably entail a data reduction and analysis effort well beyond normal mission duration. This extension will also occur because of an increase in informed interpretation in data analysis and presentation, as, for example, in geologic mapping from images. Another factor requiring prolongation of analysis effort will be increased attention to natural transient events. It is already evident that a payoff from the Viking seismometer may depend on years of continued operation. The same consideration applies to experiments whose scientific benefit may be greatly enhanced by observations of a solar flare, an atmospheric storm, a meteorite impact, or a landslide.

NASA has already recognized that exploitation of Apollo project results justified a sizeable ongoing effort over the years in the Lunar Science Program. This program also is the natural generator of specifications for further lunar exploration, such as the Lunar Polar Orbiter experiments. As the amount of information about the other planets is increased by further missions, it is logical to extend the program to include planetary science. Other measures by which the efficiency of planetary mission data analysis might be improved include (1) greater attention in mission design from the earliest stages to extended low level operation, requiring much smaller operating staff, (2) commonality of ground support procedures, as well as instrumentation, between missions to different planets, and (3) utilization of data reduction procedures and facilities developed for the Earth oriented Landsat and Seasat Systems.

C. THEORETICAL STUDIES

The ultimate product of the space program should be an enhanced understanding of the origin, evolution, and current behavior of the planets, other solar system bodies, and their environments. Theoretical efforts in these directions are supported by NASA partly as "synthesis" and partly as SR&T, roughly in proportion to the directness with which the data are utilized. The common theme in this work, however, is association more with processes rather than with particular objects. Thus, despite the gross differences in character of the Earth, Venus, and Mars atmospheres, the same principles of thermal convection apply to them all, so that, by and large, the same scientists will be found working on circulation models of the three atmospheres. While certain theoretical modelings apply much more to one body than the other (e.g., quantum mechanical explanations of UV effects on Mars' surface chemistry), the commonality of application, as well as the complex nature of much of the work, makes it desirable that it be supported continuously independent of particular missions.

Modelings which apply to several planets include atmospheric circulation, photochemical reactions, atmosphere-surface interactions, impact cratering and thermodynamics, solid-state equations-of-state, mantle convection, interplanetary trajectories and chronology, thermochemical equilibrium and rate studies, magnetic dynamos, solar wind/planet interactions, and thermal, chemical, and volatile evolution of planets. Some of these modelings require rather large-scale computer experiments; others, more fundamental theoretical considerations. Perhaps most important to the ultimate goals are planetary evolutionary models, which lead back to the conditions for solar system origin. These evolutionary models also interact with the greatest variety of data, and emphasize that there is no "Rosetta stone," or sampling of an unevolved small object. Even the applicability of the most primitive objects, the type I carbonaceous chondrite meteorites, is dependent on models: thermochemical, to infer origin circumstances and dynamical, to infer origin location and subsequent transfer to the Earth. Finally, effective explanation of the differences among all the terrestrial bodies (satellites, asteroids and meteorites as well as planets) will require much more precise and extensive modelings of solar system origin than have taken place heretofore.

Measures which NASA should consider to assure a high-quality cost-effective theoretical effort include: (1) pooling as much as possible of the effort in a unified program separate from missions; (2) extending the scope of peer review; (3) collaborative and interactive projects complementary to individual grants (such as the Basaltic

Volcanism Project); (4) a more systematic and continuing advisory structure; and (5) coordination with Earth-oriented studies, both within and outside NASA.

While the dollar level involved in these theoretical syntheses and SR&T efforts is relatively low, they are essential to the achievement of the ultimate goals of the planetary program (including improved understanding of the Earth) and have a payoff in mission results many times their cost.

SECTION IV

EARTH-BASED AND EARTH-ORBITAL STUDIES

A. INTRODUCTION

A rational approach to solar system exploration requires the prudent and timely use of the many sophisticated tools available to collect fundamental information about the planetary bodies. Integrated applications of ground-based measurements, measurements from balloons and high-altitude aircraft, observations from Earth-orbiting telescopes, and appropriate experiments on spacecraft encountering planetary bodies are necessary to achieve the highest scientific return from our efforts in planetary sciences.

The limited funds available for spacecraft exploration and the high cost and long time scale for deep-space missions require us to make sure that any important information which can be obtained by cheaper and quicker means is promptly collected so that the use of spacecraft capabilities in deep-space missions can be optimized.

NASA has for many years supported, at a modest level, ground-based optical and radio planetary astronomy, with the result that a significant amount of time has been made available at the largest ground-based observatories for carrying out planetary studies. These studies have been extremely fruitful (see Table 1).

Limitations on ground-based and airborne observations have provided the main impetus for the Space Telescope (ST) project. We anticipate that a significant improvement in our planetary exploration capability should result from that project.

B. JUSTIFICATION

1. Earth-based observatories provide basic information on solar system objects, often not easily obtained by spacecraft studies. Global and synoptic data such as average compositions and weather patterns, low light level observations which require large collecting areas, and surveys of a large number of objects such as asteroids are examples of experiments best carried out on the ground. Such knowledge is vital and necessary to the planning and design of overall solar system exploration, including spacecraft missions. In addition, these studies provide a broad context for interpreting spacecraft data, frequently allowing more general conclusions to be drawn than would be possible from the spacecraft data alone.

2. Earth-based observatories are a very cost-effective method of improving our knowledge of the solar system. The advantages of these studies include low cost, rapid response time to new observations or conditions, and a long time base for studying variable phenomena.

3. Earth-based studies are needed to advance the art of measurement techniques and instrumentation. In addition to the flexibility they allow, ground-based facilities can accommodate instruments that cannot be used in space missions: instruments of greater weight, power consumption, and complexity. Instruments or concepts developed for possible spacecraft use can be proven by use first on the ground, where design requirements are less strict, and then in near-Earth facilities.

4. Near-Earth optical observatories, while not having the advantages mentioned above to the degree possessed by Earth-based facilities, will provide higher spatial resolution and broader spectral coverage than Earth-based optical observation of solar system objects.

C. RECOMMENDATIONS

Specifically, we recommend that there be support for the following:

1. Observing facilities (optical, radar and radio) maintenance and operation so that observing time is available and planetary observations can be effectively carried out.

The present state of many major ground-based optical observatories is one of obsolescense, disrepair and decay. Support for operation of astronomical observatories has not kept pace with inflation, the rapid advancements in technology, and the state of the sciences. New funds are required both for basic operation of many observatories and for a new instrument development program. The infusion of $200,000 per year into each of five observatories for operation, plus another $200,000 per year per observatory for five years for instrument development would make a spectacular difference in the quality and quantity of planetary science coming from ground-based observatories. The new knowledge would benefit future spacecraft missions in better developed and more sophisticated experiments far beyond this cost.

Sufficient telescope time is not often available for critical observation. For example, only half the possible radar images of Venus will be obtained during the final observing opportunity before the Pioneer Venus Mission because the Goldstone antenna will be taken out of service for maintenance at the critical time. Detection and compositional measurements of Apollo and Amor asteroids are well below desired level because of the need for large optical telescope time. Radar observations of the Galilean satellites, which would provide important information on surface properties, are not possible because of required telescope and instrument upgrading. Again the estimated cost for a serious improvement in capability and output is well under $1 million.

2. The development of new instruments and techniques to take advantage of technological and scientific advancements and make more effective use of existing facilities.

Detection and data handling technologies have advanced rapidly in the past five years. At the same time funding for instrument development has decreased in real dollars. Orders-of-magnitude in several areas of astronomy are possible with new instruments that could be built.

3. A strong research program which includes data analysis, theoretical, and laboratory studies to develop and extend the base of knowledge needed for interpretation of observations (made possible by 1 and 2) and for designing new programs.

For example, spectra are being obtained for all the terrestrial bodies which have absorption features having important implications for surface composition and surface chemistry processes. But these features cannot be properly interpreted because the required laboratory work has not been done and will not be done under present conditions.

4. Use of Earth-orbiting facilities.

While we do not foresee a quantum jump in knowledge of the terrestrial bodies from the use of orbital facilities such as the Space Telescope, OAO and infrared satellites, we do feel that a number of very important observations will be made, and we feel strong support is justified for this use of these facilities for solar system exploration. We urge that the planetary significance of such missions be fully explored. For instance, the IRAS as planned may rule out the use of asteroid data which will be obtained automatically as a result of other astronomy programs. The lack of input by planetary astronomers in this more traditional astronomical activity is partly responsible.

Further, we urge the development of suitable instrumentation for orbital planetary observations (complementary to and not supplanting Earth-based instruments), and that adequate observing time for planetary work be made available.

D. SUMMARY

Based on the premise that the spacecraft used in planetary encounters in deep-space missions should be devoted to critical observations that are not possible from ground-based or earth-orbital observing platforms, we recommend that a more active program be developed using Earth-based and Earth-orbital systems for planetary observations to complement and aid deep-space missions, with a substantial increase in support for observatory operations and new instrument development to reverse the trend toward inefficient, outmoded and decaying facilities. Such a program should include increased support for ground-based telescope studies and for a modest balloon activity and substantial support for instrumentation and observations with high-altitude aircraft.

Further, we strongly recommend that a significant portion of the ST and other space telescope schedules be made available for planetary studies and that NASA develop for these telescopes instruments that are oriented toward planetary studies.

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