

Forum

In *Eos*, September 29, p. 770, Editor in Chief A. F. Spilhaus, Jr., invited readers to respond to the Forum piece **Space Station?** by L. H. Meredith. Meredith observed that most space station discussion has been about its design and cost, arguing that objectives and use also merit discussion. He asserted "that the only basis on which the Space Station can be justified is that of reasserting U.S. pre-eminence in space by creating a permanent manned presence and of providing a required step toward the manned exploration of the Solar System." Letters in this Forum are replies to Meredith's opinions.

Space Station, Yes

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After 3 years of service on a National Aeronautics and Space Administration

(NASA) Advisory Committee devoted to studying the U.S./International Space Station in the context of scientific uses, I have come to the following conclusions that may be of interest to the geophysics research community:

- The currently planned manned space station modules will provide important resources to the pursuit of science in and from space. These include the possibility of relatively frequent transportation of experiments to and from space, the availability of substantial electrical power, extensive areas on the external portions of the facility for attached payloads, and the possibility of human services, including some level of interactive operations, maintenance, and repair. In addition, there is clear acceptance of the concept of telescience, which supports distributed, remote access to space station experiments from a variety of investigator ground locations. Each of these resources can be found in various unmanned satellites, but the total-

ty of such resources is a unique feature of the core space station.

- The large platforms identified as an integral part of the Space Station Program are important to scientific research and especially to those individuals concerned with Earth observations and solar-terrestrial research.

- Scientific research is only one of a number of activities supported by the planned space station. Other direct and indirect purposes are to enhance national prestige, to rejuvenate NASA and bring the civil space program into better balance relative to the massive military space program, to support manned exploration of the Solar System, to support advanced technology development, and to investigate the possibility of space-based commercial enterprises.

- The planned space station is an international undertaking that can support a variety of activities, each of which is inadequate to justify such an undertaking on its own merits or needs. International scientific perusal of

space station possibilities has been underway for more than 2 years. There is general agreement about the various types of programs that are best suited to the space station and an appreciation of the serious technical problems that must be overcome to make the planned space station a valuable research center.

- There is a clear recognition by the international space funding agencies that the space station won't be the dominant or restricted platform for conducting space research. The specter of such a terrible situation was mentioned by L. H. Meredith in *Forum* several weeks ago, but I doubt its validity. In this context, we must think of the space station as a lamb in wolf's clothing. Some scientific disciplines will have a greater stake in the space station than others. In particular, research making use of the long-duration microgravity environment of the space station will certainly benefit greatly, while disciplines content with relatively small remote sensing instruments will most certainly opt for other unmanned platforms. After the experience of science aboard the space shuttle, I am confident that the decision whether or not to use space station resources will be made on rational grounds: namely, scientific merit, cost, funds available, and suitability of the platform.

- There is increasing appreciation that large, singular space projects must be balanced by large numbers of smaller investigations. One important value of the space station for all scientific disciplines can be its use as a testbed facility for new instruments. All disciplines, including Earth remote sensing, solar-terrestrial physics, astronomy, planetary and solar observations, and so forth, can benefit from this capability. With proper planning, it is even possible that rapid turnaround projects can be accomplished. Freeman Dyson sees this situation in terms of "quick is beautiful." Many of the current generation of space researchers who have become accustomed to 10-year projects can only hope that NASA and the international partners will strongly support this concept. Of course, our experience with the space shuttle is to the contrary and must serve as the counterpoint in arguing for more rapid access to space.

- For all of its current publicity, the space station will provide little direct benefit to space researchers for almost a decade. While it might have been better to start with a smaller space station and evolve to a large facility on the basis of demonstrated experience, the decision has been made by Congress, the Administration, and the international partners to go ahead with an ambitious facility that, it is hoped, will be in full operation sometime after 1995. Thus in the near term there is little opportunity for productive work related to the space station for most scientists and certainly the younger generation seeking important inspiration. Nevertheless, senior scientists must continue to argue for the best possible accommodations, services, access, experiments, instruments, and facilities in hopes that the next generation of space researchers will have superior opportunities with a broader range of science platforms than are now available.

- The costs of the space station, while large in a cumulative sense, don't relate directly to the funds available for space research. The

essential problem facing all space agencies is how to spread already thin research funds over a constituency that will be broadened by space station opportunities. Real growth in budgets is needed to maintain research programs already underway. While this growth was an early promise to NASA, its reality must be judged by current and planned budgets. In this regard, the U.S. space research community must present its needs to Congress and to NASA in clear terms. This message has already been sent by many groups over the past year. It must be repeated clearly to have effect.

- There has been a call for a simple vote by AGU members about the merits of the space station. Clearly, the issues involved span a complex range of political and scientific issues. In this situation the meaning of any result is open to considerable debate and confusion.

In summary, the U.S./International Space Station is an important event in the future of space research. Positive and negative aspects abound and the advice and recommendations of the science disciplines involved are essential if the final product is to be of value to science and technology. Skepticism is valuable, and enthusiasm must be tempered, but unreasoned rejection of what is already underway can lead to missing a unique opportunity to shape the new resources to the needs of many different research undertakings.

Peter M. Banks
*Department of Electrical Engineering,
Stanford University, Stanford, Calif.*

Space Station? We Need Two

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First, the point must be made that this nation desperately needs a space station if it is to maintain any legitimate claim to being a spacefaring nation. Unfortunately, the type space station that the National Aeronautics and Space Administration (NASA) will ultimately put up will probably not be the one best suited to our needs, and it most definitely will not be the most cost-effective one. I disagree with L. H. Meredith's assertion (statement letter in *Eos*, September 29, 1987) that there is no role in the space station for microgravity research and manufacturing. However, he hit the nail on the head by saying that its main justification would be in providing "a required step toward the manned exploration of the Solar System." However, it seems that the current configuration of the station is not really suited for serving as a spaceport.

Sen. Spark Matsunaga (D-Hawaii) had a good idea last year when he proposed that we should really have two space stations. The first would be an expanded version of Space Industries, Inc.'s Industrial Space Facility (ISF). SII (now allied with Westinghouse) is designing their man-tended ISF for launch in the early 1990s. An upgraded version of that facility could probably handle this nation's microgravity research and manufacturing needs for the 1990s. In addition to the ISF, the United States needs the type of low Earth orbit space port referred to in the National Commission on Space report of last year. Such a facility would be capable of servicing

reusable orbital transfer vehicles (OTVs) that could take commercial payloads to Clarke (geosynchronous) orbits or boost scientific (robotic and manned) missions to the Moon and beyond. Human crews will be brought up to these stations by the space shuttle, but to lift the stations themselves, it would make much more sense to use a heavy lift launch vehicle.

Realistically, it is difficult to envision NASA changing the current configuration of the space station. Nonetheless, AGU should do all that it can to ensure that we get a space station that can serve as a spaceport. It is equally important that this nation develop a heavy lift launch vehicle as soon as possible. If station elements are lifted by the shuttle, it will take many missions that could otherwise be devoted to scientific purposes.

William Farrand
Long Valley, N.J.

Way Station

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I agree with Les Meredith's statement of valid and nonvalid objectives for the space station. The problem with the space station that NASA is proposing is that it is designed to a nonvalid objective, specifically microgravity experimentation. I would support a space station that addressed the valid objective of a way station, but I cannot support NASA's current design.

Meredith states that the space station can only be justified as a political action by the United States to reassert its preeminence in space. Meredith lists a valid objective of creating a permanent manned presence to provide a "way station" for future Earth orbital activities and Solar System exploration. Meredith further argues that two other classes of space station objectives, namely a microgravity research and manufacturing facility and an Earth and astronomical remote sensing platform, are not valid because those activities can be better carried out by non-space station means.

The problem with the space station being sold by NASA is that it appears to best address the objective of microgravity research and manufacturing, one of the nonvalid objectives. NASA's present design does not include facilities to allow the space station to be used as a way station, so it cannot satisfy the one valid objective.

It makes good technical, economic, and political sense for the United States to be active in space. I would gladly urge Congress to fund a space station that addresses valid objectives. Unfortunately, the present design does not, and it should not be funded.

Jeffrey L. Warner
*Chevron Oil Fields Research Company,
La Habra, Calif.*

Space Station: Microgravity Design Is Best First Step

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Les Meredith's recent statement in *Eos* (September 29, p. 770) on objectives and uses

of NASA's proposed space station argues that microgravity research, manufacturing, space physics, astrophysics, and Earth observations are not good justifications for the present zero-gravity design of the station. In my view, he is correct for the general issues of remote sensing, whether it is toward Earth or the planets and beyond. Such observations are indeed better carried out by automated platforms.

But in the case of microgravity and space biomedicine, his arguments lose force. Science in the microgravity environment is a field of basic research ranging from materials science to studies of structure of large organic molecules. Today, and for the foreseeable future, the fundamental experiments to be done in the microgravity environment require close interaction with human experimenters, as in a ground-based laboratory. The space station, with its microgravity environment and open access to astronauts, provides an environment consistent with the needs of that field of basic research. As we move to manufacturing, the needs for human interaction may become less, but that is probably decades away.

Space biomedicine provides another striking example. One of the key aspects of astronauts in space is the need for space walks. Deployment of arrays, repair and refurbishment, and construction will all require astronaut activity in zero *g*. Could they go easily from a station with artificial gravity to a zero-*g* environment to work? The evidence suggests otherwise, that a significant fraction of astronauts experience severe motion sickness when going from a 1-*g* environment to a zero-*g* environment. Thus, if we are to have a station where work outside is to be done, and this is one of the continuing justifications for a station, it must be the current microgravity design.

There is no question that a long-term zero-*g* environment is debilitating. We know, from Soviet experience in space and from our own ground-based studies, that long periods of weightlessness can cause severe bone mineral loss. It is not clear whether this loss is reversible in a 1-*g* environment. This is a subject that will require much study as the space station and its successors are built and used.

The fact is that one space station cannot do everything. We need to have an initial focus that is workable and affordable, and we need to ensure that those observations and activities that can be operated from automated platforms are carried out that way. Eventually we may have a space station that can have variable gravity and that can serve many purposes at once. For now, however, the present design appears to be the best first step.

D. James Baker
Washington, D.C.

On the Future of the United States in Space

PAGES 1578-1579

It is no news that the space programs of the United States, both civil and military, have been acutely disabled since the *Challenger* accident in January 1986. Billions of dol-

lars' worth of high-priority commercial, scientific, and military spacecraft are backing up in the launching queue, but within the civil sector, no effective effort has yet been made to remedy this disgraceful situation. In fact, the present inflexible policy runs in the opposite direction — that is, to worsen the situation.

The likelihood of such a national disability was foreseen in testimony before a subcommittee of the Senate appropriations committee on June 29, 1971, by then-Senator Walter Mondale, Thomas Gold, Brian O'Leary, and myself. At that time, advocates of the development of a fleet of manned space shuttles promised the Congress that each of the proposed shuttles would deliver 50,000 pounds of payload into low Earth orbit at a launching cost of \$100 per pound; that each shuttle would have a useful lifetime of 100 missions; that by the early 1980s the United States would be conducting 50 shuttle flights per year (i.e., about one per week) for civil purposes alone; and that all expendable launch vehicles would thereby be rendered obsolete.

The outrageous optimism of those promises was described in the above-cited 1971 testimony; it is now evident to everyone. In contrast, the unmanned elements of our space program have consistently delivered on their promises and have usually gone far beyond them.

The burden of my statement is an assault on prevailing national policy for the conduct of our civil space program — a policy that ignores the lessons of experience, emphasizes things that do not work, and neglects the things that do work. I make this criticism in the spirit of the "loyal opposition," as that term is used in British politics. *There is no one who wishes the continued success of our civil space program more than I do.*

But I am not inclined to suggest that everything can be fixed by an additional few billion dollars per year. On the contrary, I consider that the federal budget for civilian space activities is already at a reasonable level and that the primary fault is one of policy and not one of appropriations.

On January 25, 1984, I listened very carefully to President Reagan's State of the Union address and later got a transcript of it from the *Congressional Record*. He singled out space exploration as the second of his four great goals for America. That was very pleasant to hear, but I was appalled by his emphasis as represented by the following verbatim excerpt from his address:

We can follow our dreams to distant stars, living and working in space for peaceful, economic and scientific gain. Tonight, I am directing NASA to develop a permanently manned space station and to do it within a decade.

A space station will permit quantum leaps in our research in science, communications, and in metals and life-saving medicines which can be manufactured only in space.

He continued with remarks on the enormous potential for commerce in space.

A year later the President reiterated his enthusiasm for space as the "next frontier" and emphasized the bright prospects for manufacturing exotic medicinals for curing disease and extraordinary crystals for revolutionizing electronics — all in a permanently manned space station. These statements were so hyperbolic and so ill-founded as to leave an in-

formed person gasping for breath. Indeed, they resembled science fiction much more closely than they did sound national policy.

In refutation of the President's declaration of space policy, I submit that the principal elements of our 40 years of experience in space are as follows:

First, despite much speculation, only one truly commercial application of space technology has emerged thus far. That is represented by our great global network of communication satellites and the associated industry, including manufacture of the necessary launch vehicles. In addition, private companies have, of course, the central role in selling space equipment and services to the federal government. But these latter functions do not qualify for the usual understanding of the term commercialization, which means providing products and/or services that pay their own way in the nongovernmental marketplace.

Second, there are many other important utilitarian applications of space technology: weather observation and forecasting; remote sensing of Earth's surface resources; marine and aircraft navigation; the applied sciences of the Sun and of Earth's ionosphere, magnetosphere, atmosphere, and oceans; and military reconnaissance, surveillance, and other applications that are technically similar to those for civilian purposes. But all of these applications remain wholly or principally in the area of governmental services.

Third, nearly all of the above-mentioned applications of space technology and nearly all of the spectacular advances in the space sciences, including planetary exploration and space astronomy, have been, are being, and will continue to be accomplished by unmanned, automated, commandable spacecraft. Moreover, most such spacecraft have been and can be launched by expendable launch vehicles and at less cost than by the shuttle — despite the grossly optimistic promises to the contrary in the early 1970s — and with minimal risk to human life.

Fourth, materials processing in space is still an embryonic science. It has significant research interest but, as of the present date, its demonstrated commercial potential is wildly incommensurate with the cost of a space station that has materials processing as one of its principal declared purposes.

A massive 2-year study entitled "Space Science in the Twenty-First Century: Imperatives for Two Decades, 1995-2015," under the auspices of the National Academy of Sciences has been completed recently. Apart from biomedical research on human subjects, this study has again found few first-order scientific objectives that require human crews in space, much less a permanently manned space station, during the adopted time frame.

The cost/benefit ratio of secondary purposes such as repair, refurbishment, refueling, and replacement of equipment in Earth orbit is, I think, greatly underestimated by advocates of the space station. Indeed, it is much more cost-effective to improve the reliability and versatility of space equipment by preflight development and test work and, if necessary, to service it by remotely controlled, unmanned spacecraft. The true "heavy-hitters" in space science and applications have been long-lived satellites and spacecraft based on intrinsically good design, good engineering, and rigorous preflight testing.

There has been a truly astonishing difference in the responses to the present national emergency in space by the Department of Defense and by the National Aeronautics and Space Administration. Soon after the *Challenger* accident, Secretary of the Air Force Edward Aldridge made the forthright public statement: "We made a great mistake in planning exclusive reliance on the shuttle for the delivery of military satellites into space. Believe me, we will not make that mistake again." True to his word, he placed the Vandenberg shuttle launch facility in caretaker status and immediately resumed the procurement of expendable launch vehicles for defense purposes. The same position has been taken by the National Oceanic and Atmospheric Administration.

But the administration of NASA has steadfastly refused to recognize the full import of the existing problem and chooses to devote the principal part of its resources to the pursuit of vaguely defined objectives of a futuristic nature. Its 1987 and 1988 budgets emphasize repair of the shuttle system, the procurement of a replacement shuttle, a continuing primary dependence on manned spacecraft for all purposes, and the rapid development of a huge space station.

I grant the political face-saving importance, perhaps even the political necessity, of resuming shuttle flights with improved safety. But there is no urgency to building a replacement shuttle and far less urgency to developing a space station. The true national urgency, in my view, is to return to primary dependence on expendable launch vehicles for both civil and military purposes. Yet NASA's original 1988 budget made provision for the purchase of only one ELV.

Three years ago, NASA promised the Congress that it could build a large space station for \$8 billion over a 10-year period. This estimate, as with the shuttle program estimates in 1971, was so optimistic and so incomplete as to be deceitful — at least to the uninformed. In a paper that I wrote for the *Scientific American* in the autumn of 1985, I estimated the true cost as more like \$30 billion. A special panel of engineers and scientists assembled under the auspices of the National Research Council recently arrived at a similar lower limit on cost, but it contributed little to the more basic issue of value.

Let me make it clear that I have no intrinsic opposition to manned flight or to a manned space station if their purposes are described forthrightly and validated by critical debate. My own position is that the development of a large space station during the next 10 years is grossly inappropriate to our national predicament in space and to the public mandate for financial responsibility by the federal government.

In short, I submit that the present national space policy, if actually pursued as directed by the President, will greatly diminish our future advances in space applications of direct human importance and in scientific work of broad appeal to an intelligent citizenry. As a result, our international leadership in space will continue to shrink.

James A. Van Allen
Department of Physics and Astronomy,
University of Iowa
Iowa City

Space Station? Yes, For Biological Research

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I have, for the past several years, been the Life Sciences Team Leader on the Task Force for Scientific Uses of Space Station, where we have viewed the space station as a powerful tool for microgravity research in the life sciences. The fact that the space station is indeed to be a permanent facility makes it especially and uniquely of value to us. While I agree with Meredith's contention (*Eos*, September 29, 1987, p. 770) that the station can well be justified as a required step toward manned exploration of the Solar System, I do not agree with his apparent feeling that it has little utility as a research facility.

In the life science area, there is a broad need to study the long-term effects of microgravity on plant, animal, cellular systems, and humans. We have no other way of studying the mechanisms of bone demineralization, cardiovascular deconditioning, muscle atrophy, and other problems presently confronting us in planning for long-term manned exploration of the Solar System. To assume (as Meredith does) that artificial gravity automatically solves the problem is unrealistic. How much artificial gravity? Is the g requirement the same for all forms of life — plant and animal? Clearly what we need is a research facility where we can carefully study gravity and its impact on living systems. Then we need to look at appropriate countermeasures (for example, exercise, drugs, and artificial gravity). The most appropriate facility being planned for such work is the space station. In the case of plants, which will ultimately be required for planetary exploration as a food source, we do not yet know the effect of microgravity on their ability to produce food and oxygen. Long-term microgravity studies are required. We cannot simply assume artificial gravity solves the problem, and we certainly do not know what gravitational parameters are optimum.

As we see it, there is a requirement for basic research on the mechanisms of graviperception at the cellular and organismic level, as well as a clear-cut determination of the magnitude of the microgravity problem in the case of plants, and particularly humans. If this research indicates a need for artificial gravity, this facility is needed to determine to provide variable artificial gravity (centrifugation) as a research facility that will allow us to study lunar and Martian g levels and determine optimum levels of g for space flight. It must be remembered that the production of artificial g by means of a spinning spacecraft also induces Coriolis forces with consequent effects on human physiology and well-being.

In any case, research is what is required, and a spacecraft with the capability of the space station is badly needed.

Richard S. Young
RCA Service Company,
Satellite Beach, Fla.

EOS: The Earth Observing System and Polar Platforms

PAGES 1579, 1586

The statement about the Space Station by L. H. Meredith (*Eos*, September 29, 1987) did not mention the Polar Platform component of the space station. Insofar as the planned Earth Observing System (EOS) will make extensive use of the Polar Platform, the following description is provided so that more complete information is available to the *Eos* readership in the invited discussion on the space station. EOS, with participation by the National Aeronautics and Space Administration (NASA), National Oceanic and Atmospheric Administration (NOAA), European Space Agency (ESA), and Japan, is the cornerstone of the Mission to Planet Earth initiative. It will use the Polar Platform for the most massive undertaking in characterizing the Earth system and in studying global change that has ever been attempted. The description here has been abstracted from a NASA report that will soon be available for public distribution: "From Pattern to Process: The Strategy of the Earth Observing System."

The objectives of EOS are to provide much of the global data and the supporting information system necessary to develop a comprehensive understanding of the way Earth functions as a natural system. This includes the interactions of the atmosphere, oceans, cryosphere, biosphere, and solid Earth, particularly as they are manifested in the flow of energy through the Earth system, the cycling of water and biogeochemicals, and the recycling of Earth's crust driven by the energy of the interior of Earth. The comprehensive global approach to the study of these processes in an integrated context has been termed Earth System Science and has a strong focus on the development of the capability for accurate prediction of the future evolution of the Earth system on time scales of a decade to a century. Thus the goal of Earth System Science has been enunciated as

To obtain a scientific understanding of the entire Earth System on a global scale by describing how its component parts and their interactions have evolved, how they function, and how they may be expected to continue to evolve on all time scales.

The particular objectives to be delivered by EOS are

- A comprehensive data and information system to provide the Earth science research community with easy, affordable, and reliable access to Earth science data.
- An observing system to provide a fairly complete set of the global Earth science data available from low Earth orbit on a long-term, sustained basis in a manner that maximizes the scientific utility of the data and simplifies its analysis.
- The study of Earth science today clearly requires that EOS obtain a long-term data record. Scientists cannot perform controlled

experiments in the laboratory when dealing with the forces of Earth. They must watch Earth evolve over significant time periods to deduce from the patterns of change the processes that govern Earth's behavior as a natural system. There are also important phenomena such as El Niño that only occur at intervals of several years and whose occurrence has not been predicted sufficiently in advance to permit mounting specific observing efforts.

A general list of the required data is as follows:

- The global distribution of energy input to and energy output from Earth;
- The structure, state variables, composition, and dynamics of the atmosphere from the ground to the mesopause;
- The physical structure, temperature, detailed elevation, chemical or mineral composition, and surface moisture content of the land surface.
- The circulation, surface temperature, wind stress and sea state, and the biological activity of the oceans;
- The extent, type, state, health, biological productivity, and composition of land and inland water ecosystems;
- The extent, type, state, elevation, roughness, and dynamics of glaciers, ice sheets, snow, and sea ice and the liquid water equivalent of snow;
- A sample of the rate and extent of precipitation;
- The dynamic motions of Earth as a whole, including both rotational dynamics and the kinematic motions of the tectonic plates.

The EOS space segment will begin to be deployed in the mid-1990s, with growth in the number and quality of remote sensing instruments on additional platforms for several more years. The full system would then operate for about the next decade to obtain time series of at least 10 years' duration for all of the observations. Many of the crucial measurements would be obtained for a period of at least 15 years and will build on observations that are already being taken and on long time series of conventional observations. By the mid-1990s, these data records will include the measurements of upper atmospheric chemical species, temperature, and winds, sea ice and snow extent, ocean color and vegetation extent, the total and spectral irradiance of the Sun, the radiative balance of Earth, and measurements of sea surface characteristics including temperature, elevation, wind stress, and wave heights.

NASA, NOAA, ESA, and Japan have considered several scenarios of instrument development and deployment. The instruments include passive sensors with much greater spectral and spatial resolution than previously possible, and active radar and lidar instruments. All scenarios considered are subject to substantial change by future events. Rather than endorse a specific scenario as illustrative of how these decisions should be made, a set of guiding priorities has been recommended by the EOS Science Steering Committee. The top priority for EOS measurements is continuity in all ongoing satellite data sets that are essential for understanding the Earth system. This priority is a direct consequence of the need for long-term consistent observations. The second priority is for instruments that address the broad multidisciplinary research questions of Earth system science or that meet specific measurement requirements in several Earth science disciplines.

The third priority is for instruments that enable and advance a specific key area of research in a single Earth science discipline. All other measurement capabilities may be regarded as sharing the fourth priority. These are the scientific priorities that must be incorporated with technological, financial, and programmatic constraints to arrive at the actual decisions on which instruments are placed in orbit as part of the initial and subsequent EOS orbital configurations.

The key to the EOS concept and to its ultimate success in meeting the needs of the Earth science community is the data and information system. This system must be the foundation upon which the rest of the mission is built; it will be the means by which all EOS results are collected and communicated. As with all other aspects of the EOS concept, the essential character of the data and information system is dictated by the nature of Earth science research.

Earth scientists must often take an eclectic approach to solving problems and assemble data of different types from a wide range of sources. The number of potential sources of useful data can, in some instances, be quite large. Thus access to a wide range of data sources is essential to the productivity of the researcher. Another characteristic of the data is the fact that different aspects of the same data set are useful in different investigations. Different research foci also lead to concern with different levels of detail in the data. This requires that the data and information system be capable of handling a wide range of data, including data with significant variations in spatial and temporal resolution and averages of various kinds. This also requires the ability to store and retrieve the results of various re-

search studies for use in other work. The EOS data and information system must also function in the more traditional sense as a data system for the EOS mission data. This function extends from the various instruments and platform subsystems through data downlink and ground-based distribution and processing to the storage of this data for the duration of the mission in a mission data base. Given the scope of the EOS mission, this is a formidable challenge.

Therefore, the implementation of EOS will start with a data and information system that unites researchers and sources of data. This system must be able to grow to the capabilities required by the expanded set of remote sensing devices that can be brought to bear on the study of Earth in the 1990s.

EOS includes an extensive set of coordinated observations and will not have to replicate the observations being made on an operational basis, since that data will be accessible through the EOS data system. EOS will make use of the new generation of spacecraft called space platforms, being developed as part of the Space Station Program. These platforms will provide expanded resources for the support of collocated instruments and more effective utilization of the investments made in instruments through on-orbit servicing. International cooperation in this venture will be essential to its success.

Coordinated Announcements of Opportunity (AOs) are planned for issue in early 1988 by NASA, ESA, and Japan. The NASA AO will solicit instrument investigations, membership and leadership on research facility instrument teams, and interdisciplinary science investigations. Proposals in this last category of investigation will be used as examples of the type of research that will be advanced by the EOS data and will be particularly valuable in guiding the development and evolution of the EOS data and information system. The detailed AO will be available in January from the Earth Science and Applications Division at NASA Headquarters. Information on how to obtain copies of all three AOs should be made available to all *Eos* readers in late November or early December 1987.

D. M. Butler
NASA Headquarters,
Washington, D.C.

R. J. Gurney
NASA/Goddard Space Flight Center,
Greenbelt, Md.

T. L. Miller
NASA/Marshall Space Flight Center,
Huntsville, Ala.

Forum

Space Station?

PAGE 770

The National Research Council (NRC) Committee on the Space Station has completed its report to the White House and NASA. Congress will hold final hearings this fall on the NASA FY 1987 budget and will decide whether to go ahead now with this major program. The NRC report stresses the need for a national commitment to the Space Station for it to be successful. Now is an appropriate time for the members of AGU to consider the proposed program and to make their views known by writing to *Eos*.

Eos will conduct a poll about the Space Station in a month. Between now and then, I welcome letters on specific points intended for publication. I hope your letters will be succinct, forthright, and represent all sides of the issue. The following piece, prepared by

Les Meredith, is intended to stimulate your thoughts.

It is important to realize that the Space Station is envisaged as a central focus of the U.S. space program for at least the next 30 years (a period longer than the total life of NASA). It will cost tens of billions of dollars, and use a significant fraction of the advanced technical capability of the country. Individuals wishing copies of the NRC report can get them by writing

Committee on the Space Station
National Research Council
2101 Constitution Avenue, N.W.
Washington, DC 20418

A. F. Spilhaus, Jr.
Editor in Chief

Statement

To date much of the Space Station discussion has focused on what it costs, how it will

be managed, and its detailed configuration. There's been relatively little discussion of the objectives and requirements it must meet in spite of the fact that our experience has shown the criticality of establishing well-defined objectives prior to initiating the development phase of such a program. In this regard, at least the following points need to be considered:

1. Objectives

It's my opinion that the only basis on which the Space Station can be justified is that of reasserting U.S. pre-eminence in space by creating a permanent manned presence and of providing a required step toward the manned exploration of the solar system. Other possible justifications, such as microgravity research, manufacturing, space physics, astrophysics, and earth observations, could either be accomplished more efficiently by other means or can provide only tenuous justifications at best compared to the major scope of

this program. Whatever the objectives are, they must be clearly defined before a decision is made on what should be built. The lack of clear long-term objectives is noted in the recent NRC Space Station report.

2. Broad Usage

In hindsight, a mistake was made with the shuttle program in forcing its use by essentially all groups needing transportation to space whether it best met their requirements or not. The result has been escalated costs, increased manpower requirements, and extended schedules in many individual space programs and, with the *Challenger* disaster, the virtual shutting down of the total space program for an extended period.

The current Space Station program started with the primary charge by the President to create a permanent manned presence in space. A system was then designed that not only does this but also provides a microgravity environment and support for as many other users as possible. In view of the large amount of money and manpower the resulting station will require, it will need to produce significant results. Therefore, it can be expected that most space users will be forced to use the station to conduct major parts of their programs whether it's the most efficient method or not: The piper must be paid. The Space Station program thus seems to repeat the same mistake made by the shuttle program and could have a similar but even larger effect on most of the U.S. space research activities of the future, including those that directly affect geophysical programs

3. System Studies

The Space Station is the largest space system ever planned with major international, launch vehicle, operations, and user interface requirements extending over decades. It is far from being an independent entity and so must be planned as a total system. To do this requires not only decisions on launch vehicles and other elements but decisions on objectives. For example, if the objectives are those I've proposed above then an assessment is needed on basic issues as to whether they could be better met with the present microgravity station or with one having variable artificial gravity. This could be a fundamental change but might decrease the cost, increase the crew safety, and preclude the perhaps impossible requirement of making the human body adapt to a microgravity environment for long periods. The assessment, of course, should also address the issue of the proper station orbit, particularly if international cooperation is envisaged for either exploration or crew rescue.

4. Users

One of the lessons learned in the shuttle program is that while the engineering design might meet specifications, use of the system can be difficult and expensive due to safety, procedural, and other requirements. Examples are the review and documentation requirements, special test and training programs, experiment scheduling lead times, and number of flight units required. Since the cost of using the system depends on its engineering implementation, such user re-

quirements need to be defined prior to station development. This is particularly true since the station's user and operations costs will be much greater than its development costs.

These four points are all directly related to the question of the objectives of the Space Station. They need resolution in the study phase of the program that precedes development approval if the station is to realize its potential and not be counter productive. Once the development program is started, the very major investment will almost certainly preclude significant changes.

L. H. Meredith
*AGU Headquarters,
Washington, D.C.*