

A Space Physics Paradox: Why Has Increased Funding Been Accompanied by Decreased Effectiveness in the Conduct of Space Physics Research?
Committee on Solar-Terrestrial Research, Commission on Geosciences, Environment, and Resources, Committee on Solar and Space Physics, Commission on Physical Sciences, Mathematics, and Applications, National Research Council

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A SPACE PHYSICS PARADOX

WHY HAS INCREASED FUNDING BEEN ACCOMPANIED BY DECREASED EFFECTIVENESS IN THE CONDUCT OF SPACE PHYSICS RESEARCH?

Committee on Solar-Terrestrial Research
Board on Atmospheric Sciences and Climate
Commission on Geosciences, Environment, and Resources
and
Committee on Solar and Space Physics
Space Studies Board
Commission on Physical Sciences, Mathematics, and Applications
National Research Council

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This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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Cover art reproduced from a batik card titled *Changes* by Susan Wexler Schneider, a nationally recognized batik artist who has been working in this medium for 20 years. Now a Seattle, Washington, resident, Ms. Schneider learned the craft of batik in a southern Ontario town and has had many one-person and group shows. Susan considers batik a "truly magical medium." It is singularly appropriate to have Susan's art represented on the cover of this report since she is the daughter of the late Harry Wexler, whose contributions to atmospheric science and to our understanding of solar influences on the atmosphere are well known. Dr. Wexler was instrumental in establishing the geophysical observatory at Mauna Loa and in attracting scientists to study solar radiation and the atmosphere.

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Preface

Traditionally, the National Research Council's Board on Atmospheric Sciences and Climate (BASC) and Space Studies Board (SSB) examine research strategies within their areas of science. In that respect this report is unusual. It looks, instead, at the health of a scientific discipline as it is affected by administrative, managerial, and funding decisions. The study originated from a perception shared by many space scientists that, although overall funding was greater than in previous years, individual researchers seemed to be having greater difficulty in obtaining support for their work. This report is the result of an investigation into that perception and the program structures within which much of U.S. space physics research is conducted.

The authors of this report are listed in the preceding committee membership rosters. Their aspirations were to help federal science managers, and those within their own ranks who help make and implement science policy, by analyzing governmental support of space physics research. The conclusions and recommendations from this study are guideposts for identifying and solving significant problems that thwart cost efficiency in the management of one corner of science. However, as the committee members soon discovered, the subject and results of this study apply to many other areas of science as well. This report should be of interest to everyone engaged in research or in the funding and organizing of research.

The two authoring committees, the BASC Committee on Solar-Terrestrial Research (CSTR) and the SSB Committee on Solar and Space Physics, meet jointly as a federated committee representing the subdisciplines of solar physics, heliospheric physics, cosmic rays, magnetospheric physics, ionospheric physics,

upper-atmospheric physics, aeronomy, and solar-terrestrial physics to provide advice to government agencies. They are concerned with the experimental (both ground-and space-based), theoretical, and data analysis aspects of all these subdisciplines.

Development of research and policy guidance is undertaken with one committee taking a lead role, as appropriate. While the CSTR filled the lead role for this report, the results stem from a sustained effort by the entire federated committee.

A particular note of appreciation is extended to two people who helped bring this study to its most fruitful conclusion: Morgan Gopnik, who skillfully edited the report and made key recommendations in response to reviewer comments, and Ronald C. Wimberley of North Carolina State University, who contributed insightful suggestions for improving the manuscript. The committees also wish to thank Doris Bouadjemi for her able preparation of the many iterations of the manuscript.

John A. Dutton, *Chairman*

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Executive Summary

The field of space physics research has grown rapidly over the past 20 years both in terms of the number of researchers and the level of investment of public money. At first glance, this would seem to portend a happy, prosperous community. However, rumblings of dissatisfaction have been building, and periodic reports have surfaced indicating that the huge investments have not produced the desired outpouring of new experimental results. To move beyond anecdotes and perceptions, this report seeks to first substantiate, and then unravel, this seeming paradox by asking:

Why has increased research funding been accompanied by decreased effectiveness in the conduct of space physics research?

BIG AND LITTLE SCIENCE

Central to this discussion is an understanding of the distinction between "big" and "little" science, both in general and specifically as these terms apply to space physics. The first thing to note is that these concepts are far from static. Whether a given project is perceived as big or little science depends on when it is observed (many of today's small projects would have seemed daunting and ambitious 20 years ago), on how it compares to other endeavors within a subfield (a small satellite project might dwarf a large ballooning experiment), and what funding agency it falls under (a large project at the National Science Foundation [NSF] might be viewed as a modest effort at the National Aeronautics and Space Administration [NASA]). Nevertheless, it is possible to distinguish broad char-

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acteristics of big and little science. Each offers particular research capabilities, and each presents certain challenges to be overcome.

Big science programs generally pursue broad scientific goals perceived to be of national importance. They are costly and technically complex and incorporate many experiments. As a result, they tend to be defined and managed by committees of administrators, and they require long planning and selling phases. Funding must generally be sought from Congress on a project-by-project, and sometimes year-to-year, which results in a large measure of uncertainty. On the other hand, the archetypal small science project is run by an individual or by a small team of researchers with its own specific research goal. These projects are less expensive and can be implemented relatively quickly. Funding for small science is typically obtained by submitting grant proposals to compete for core program funds within an agency.

Ideally, the large body of experimental results and discoveries coming out of small science help define and fashion the big science programs, which in turn provide platforms for many additional experiments. Unfortunately, many observers believe that this synergism has been deteriorating. Within the field of space physics, this report examines funding mechanisms, the nature of the research community, and the conduct of research itself to see how these factors have evolved over the past two decades.

DEMOGRAPHICS OF THE RESEARCH COMMUNITY

An examination of data from relevant professional associations, and an intriguing though limited NASA survey, reveal a growth in the space physics research community of roughly 40 to 50 percent from 1980 to 1990. The median age of academic researchers is rising significantly and most dramatically among those who describe themselves as experimentalists. Of the graduate students who responded to the NASA survey, only 10 percent were involved in instrumentation. In an empirically driven field such as space physics, this is a cause for concern.

TRENDS IN THE AVAILABILITY AND DISTRIBUTION OF FUNDS

Since 1975, overall federal research funding in all fields has shown a steady increase, resulting in greater than 40 percent growth (adjusted for inflation) from 1975 to 1990. University-based researchers have been the primary beneficiaries of this growth. Although the data are harder to come by, relevant Figures from NASA and several universities indicate that the growth in funding for space physics research has been comparable to these overall trends.

However, these figures lump together many different kinds of projects and funders. For example, one element of space physics funding is the base-funded (or core) program, which is the primary source of support for small science endeavors. This report looks at base-funded programs at both NSF and NASA

and finds, contrary to the trends described above, that they have not even kept up with inflation and have certainly not been able to keep pace with the explosion in grant requests. As a result, grant sizes have decreased, and the percentage of proposals accepted has dropped. A rough calculation shows that researchers must now write two to four proposals per year to remain funded, up from one or two in 1989. Of course, increasing the time spent searching for support means that less time is spent on productive research. Rising university overhead and fringe benefit costs, that consume more and more of each grant dollar exacerbate this problem. Clearly, the base-funded program has not participated proportionately in the overall space physics research funding increase. Although we do not attempt to quantify the effect this has had on the quality of science produced, we do find that the core program has become much less efficient during the past decade. We also infer that the lion's share of new funding has gone into project-specific funding, most of which involves big science efforts.

TRENDS IN THE CONDUCT OF SPACE PHYSICS

A detailed examination of the history of satellite launches, solar observatories, rockets, ballooning, theoretical modeling, and data analysis reveals several important trends relevant to our understanding of the space physics paradox. For each type of experimental or analytical activity, this report considers trends in technical complexity, implementation times, amounts and sources of funding, and planning activities.

Looking first at satellite launches, including space-based solar observations, we find that implementation times have soared. Is this due to their increasing size and technical complexity or to mushrooming planning, selling, and coordinating activities? Experience in other programs indicates that the latter plays a major role. Ground-based solar observatories, whose complexity has not evolved enormously, still experienced huge implementation delays over the past two decades as a result of protracted study, design, and redesign efforts and the need to extract new-start approvals and continued appropriations from Congress. One effect of long implementation times, especially in the satellite program, has been to all but eliminate new experimental opportunities. Conversely, the rocket and balloon programs, which tend to be funded from agency budgets and controlled by individual researchers, have experienced great increases in technical capability without crippling administrative delays. Technical problems do arise and must be overcome, but these temporary delays do not seem to exert an ongoing drag on progress.

In general, increased implementation times seem to be correlated with program planning and management characteristics as much as, or more than, with technical complexity. On the other hand, programs run predominantly by individual researchers who are dependent on grants (e.g., rocketry, ballooning, theoretical work, data analysis) continue to be hampered by falling grant sizes, in-

creased competition for budgets that are barely growing or are actually shrinking relative to inflation, and the inefficiencies that result from these struggles.

CONCLUSIONS

The accumulated data and findings presented in this report can be embodied in four broad conclusions.

Conclusion No. 1: The effectiveness of the base-funded space physics research program has decreased over the past decade. This decrease stems mainly from a budget that has not kept pace with demand, a time-consuming proposal submission and review process, and rising university overhead rates. An effective base-funded program is essential for the incubation of new ideas and for broad support of the scientific community.

Conclusion No. 2: Factors such as planning, marketing, the funding process, and project management have become as responsible for the increased delays, costs, and frustration levels in space physics as technical complications related to increasing project size and complexity. More complicated management and funding structures may be a natural result of the trend toward larger programs. Still, the true costs of these requirements should be acknowledged, and they should not be imposed in programs where they are not necessary.

Conclusion No. 3: The long-term trend that has led to an ever-increasing reliance on large programs has decreased the productivity of space physics research. Big science is often exciting, visible, and uniquely suited for accomplishing certain scientific goals. However, these projects have also been accompanied by implementation delays, administrative complications, funding difficulties, and the sapping of the base-funded program.

Conclusion No. 4: The funding agencies and the space physics community have not clearly articulated priorities and developed strategies for achieving them, despite the fact that the rapid growth of the field has exceeded available resources. Lacking clear guidance from a set of ranked priorities, the funding agencies have absorbed into their strategic plans more ideas and programs than could be implemented within the bounds of available, or realistically foreseeable, resources. Too many programs are then held in readiness for future funding, driving up total costs and often ending in project downsizing or cancellation.

RECOMMENDATIONS

Based on the conclusions described above, the committee makes four interrelated recommendations aimed at policymakers, funders, and the space physics research community. The committee believes that implementation of these recommendations could greatly increase the amount of productive research accom-

plished per dollar spent and reduce the level of frustration expressed by many space physics researchers without any overall increase in funding.

Recommendation No. 1: The scientific community and the funding agencies must work together to increase the proportionate size and stability of the base-funded research program. As noted above, a steady development of new ideas is necessary to advance the field of space physics. With a larger, more stable core program, agencies can increase grant sizes and durations, enabling researchers to focus more on science and less on funding.

Recommendation No. 2: The funding agencies should ensure the availability of many more experimental opportunities by shifting the balance toward smaller programs, even if this necessitates a reduction in the number of future large programs. The future of space physics requires access to new research opportunities and the ability to train and develop new scientists. Although large programs have the potential to provide many experimental opportunities, their risk of failure must be counterbalanced by more frequent small programs.

Recommendation No. 3: In anticipation of an era of limited resources, the space physics community must establish realistic priorities across the full spectrum of its scientific interests, encompassing both large-and small-scale activities. In the absence of clear priorities, programmatic decisions will ultimately be made on the basis of considerations other than a rational assessment of the value of the program to the nation's scientific progress. Scientific goals should not be lightly altered or set aside, and ongoing projects initiated in response to established scientific priorities should be insulated as much as possible from the effects of short-term fluctuations in resources. Prioritization must include an assessment of the balance between the capabilities and limitations of both big and little science.

Recommendation No. 4: The management and implementation processes for the space physics research program should be streamlined. Requirements put in place to ensure accountability and program control are now taking their toll in delays and inefficiency. Planning, reviews, oversight, and reporting requirements should be reduced in many instances, even at the expense of assuming a somewhat greater risk. Recognizing the strong self-interest of researchers to succeed, greater authority should be delegated to principal investigators, who on the whole have demonstrated their ability to get results more quickly and efficiently.

The four recommendations outlined above are highly interrelated. Streamlined management processes will further boost the productivity of a stabilized core program. Priority setting will enable the few most critical big science projects to be pursued without jeopardizing ongoing research. Taken together, we believe these recommendations provide a blueprint for a stronger, more productive space physics research community.

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1

Introduction

Recent years have witnessed a rapidly escalating debate on the process and adequacy of research funding in the United States. In this debate strong opinions have been voiced concerning the relationships and relative merits of issues such as "big" science versus "little" science, centers of excellence versus individual initiatives, and directed research versus unconstrained research. Intimately related to this debate is a perception that U.S. research capabilities have steadily eroded despite substantial increases in research budgets. Most of the discourse, generally anecdotal, has taken place at meetings, in hallways, and through the media via letters and articles. Only rarely have reports (Lederman [1], OTA [2]) addressed various aspects of the issues raised.

The same debate flourishes in the scientific fields served by the Committee on Solar-Terrestrial Research (CSTR)/Committee on Solar and Space Physics (CSSP). The committee deemed it timely to address the issues involved and, like the Lederman report, seek to resolve the basic paradox behind the question:

Why has increased research funding been accompanied by decreased effectiveness in the conduct of space physics research?

Many thorny issues lurk behind this simply stated question. Is its basic premise accurate or even verifiable? Can and do funding choices influence the effectiveness of a scientific discipline? If so, have the funding agencies spent their money unwisely? Have the research communities abrogated their responsibilities by wanting to do everything and prioritizing nothing? Is it true, as has been

suggested in popular articles¹ and the media, that "big" science (i.e., big-budget, multi-researcher, highly managed research) is battling against "little" science (typically university based, initiated by a few principal investigators, and with more modest budgets)?

If the paradox is real, it becomes important to discover its causes. Decreased effectiveness, and the accompanying widespread dissatisfaction in the research community, may be symptomatic of a system that is not serving either space science or the public interest. Consequently, members of the CSTR and CSSP set out to assemble a data base of information on grant programs and science projects supported over the past two decades by the main funding sources for these communities.²

The resulting data set consists of a combination of data from individual scientists, the funding organizations, and other supporting institutions (e.g., American Geophysical Union, International Association for Geomagnetism and Aeronomy, International Council on Scientific Union's Committee on Space Research). This report presents the trends identified in the data and discusses them in the context of the issues mentioned earlier.

No organization has collected the exact kind of data needed for this study. As a result, the committee was necessarily limited by incomplete information and by the frequent need to identify plausible surrogates for many of the actual attributes and trends under investigation. In some cases the incompleteness of the data sets allowed us to use them only as suggestive evidence, illustrative of the trends perceived by committee members and other long-time practitioners in the field. Nevertheless, the committee was able to use the data to illuminate a variety of perspectives on the many issues associated with the space physics paradox.

This report differs from others that have touched on the same topic. For example, the Lederman report [1] is a synthesis of some 250 replies from individual scientists across a spectrum of physical science disciplines who responded to a questionnaire on research funding and productivity. The resulting anecdotal data base gives a powerful and disturbing assessment of a deteriorating research capability in the United States. However, other than recommending an 8 to 10 percent per year real growth in U.S. research funding, the Lederman report does not (and was not intended to) present solutions or suggest approaches to specific issues.

¹ For example, D. E. Koshland, Jr., 1990, The funding crisis, *Science* 248:1593; and D. S. Green-berg, 1986, Fundamental research vs. basic economics, *Discover* 7:86.

² While the National Aeronautics and Space Administration (NASA), National Science Foundation (NSF), Department of Defense, Department of Energy, and National Oceanic and Atmospheric Administration all participate directly in solar, solar-terrestrial, and space plasma physics, NASA and NSF are the main funding sources for competitive research proposals.

The OTA report [2], while touching on some of these issues, is primarily concerned with the national research issues of prioritization, expenditures, changing needs, and the information required for decisionmaking. It provides excellent background material, data, and perspectives on federally funded research. It also makes clear that the established methods used for the past 40 years to fund research in the United States are changing, and changing rapidly.

The present report addresses research funding issues specifically in the fields of solar, solar-terrestrial, and space plasma physics. Like the Lederman report [1] it has been stimulated by our colleagues' anecdotes. We have tried to extend the analysis and sharpen the issues by linking these tales of frustration to trends in funding and project management. Like the OTA report [2], we look at data trends as a way of examining the different sides of the issues involved. However, where possible and appropriate, we have taken the next step—by drawing conclusions and making recommendations. All of the recommendations are made in the spirit of requiring no additional overall resources. It seems likely that the broad themes and concerns expressed in this report are not unique to the field of space physics. We hope that a detailed analysis of this specific field will help shed light on a systemic problem and at the very least open a productive dialogue between the research community and the funding agencies.

Throughout the remainder of the report, the term space physics is used as a designation for the research areas served by the CSTR/CSSP: solar physics, heliospheric physics, cosmic rays, magnetospheric physics, ionospheric physics, upper-atmospheric physics, aeronomy, and solar-terrestrial relations. The report is organized into seven chapters. Chapter 2 presents a discussion of big science and little science issues relevant to this report. Chapters 3 and 4 present, respectively, funding and demographic trends in the research community generally, with specific examples from space physics. Chapter 5 discusses the results and implications of these trends for the base-funded program. Chapter 6 presents trends in the conduct of science observed for various elements of space physics. Finally, Chapter 7 synthesizes the report's findings into a set of conclusions and recommendations.

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2

Big Science, Little Science, and Their Relation to Space Physics

It has been the nature of science to grow—and to grow rapidly, outstripping population growth. De Solla Price [7] has shown that science has been characterized by an exponential growth rate for the past 300 years. This growth, measured by various manpower and publication parameters, is characterized by a doubling period of 10 to 20 years. Data analyzed by De Solla Price included scientific manpower, number of scientific periodicals, numbers of abstracts for various science fields, and citations. While the absolute values of these growth rates display a range of uncertainty, the general result is that the growth of science has been both long and rapid. This rapid growth is characteristic of all scientific subfields, old and new.

To appreciate how rapid a growth this is, note that the exponential growth of the general population shows a doubling period of about 50 years. Using 15 years to denote the doubling period for science, the ratio of the number of scientists to the general population doubles about every 20 years. Clearly, this trend cannot be sustained indefinitely. In fact, De Solla Price suggests that the problems facing science at this time are a reflection of its unusually long and rapid growth, a growth that, when compared with the much slower growth in the general population, may finally be straining the present economic fabric of society.

In this chapter the committee presents its views on the concepts and characteristics of big and little science as they pertain to the field of space physics. Each presents unique opportunities and challenges, and we conclude that both elements must be present for a research field to advance vigorously and productively.

CONCEPTS OF BIG AND LITTLE SCIENCE

In many discussions the concepts of big and little science are presented in near mythical terms—terms that cloud the complexity of the issues involved. Little science is usually represented by the lone researcher working in the laboratory on self-chosen problems, generally oblivious to the needs and/or requests of society. Big science, on the other hand, is often envisioned as a huge project or institute, managed by a bloated bureaucracy that directs, usually by committee, the scientific paths of many researchers. These are unsatisfactory and largely inaccurate generalizations that have led to more sterile argument than productive discussion.

One of the main reasons for this situation is that there is no absolute definition of big or little science. There seems to be a tendency in experimental science for small endeavors to evolve into large ones. Therefore, the bigness or smallness of any given scientific effort will depend on when it is observed within the evolution of its scientific subfield. In addition, the perceived size of a scientific project will vary from one subfield to another, as well as from one funding agency to another. What is considered a small satellite project is a very large project for rocketry or ballooning; what is a small project for the National Aeronautics and Space Administration (NASA) is generally a large project for the National Science Foundation. Furthermore, what is considered a small project today generally was thought to have been a large project years ago. This latter effect—the time dependence of the accepted measures of big and little project sizes—is a strong function of technological advances in the field. For example, today's desktop computers far outstrip the capabilities of the best mainframes of two decades ago—the big computer of yesterday is the little computer of today. A similar evolution has occurred in the space physics experimental arena, with the result that even today's small experiments are more sensitive, capable, complex, and expensive than those considered large in earlier years.

Although it is not possible to formulate accurate, universal definitions of big science and little science, it is possible to recognize each at a given point in time, in a particular subfield, and within a specific funding agency. The discussion in this report is based on researchers' perceptions of what constitutes big and little science, even though, as mentioned above, these terms vary by agency, subfield, and time.

CHARACTERISTICS OF BIG AND LITTLE SCIENCE

Big science and little science are characterized by very different needs, capabilities, and difficulties. In order that a proper balance between them be approximated in a given subfield, it is important to recognize how their respective strengths support the research objectives of the field.

Large projects are required for that unique class of science problems that

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can be pursued only by using large, complex facilities and platforms, extensive campaigns, or multipoint observations. Small projects, typically pursued by many diverse investigators, are required for the steady progress and evolution of the field, as well as the unexpected results that often dramatically alter current perspectives. With an appropriate balance, there can be a strong synergism between large and small science that greatly enhances the productivity of the field.

Table 2.1 presents a concise list of some current characteristics of big and little science, as viewed from the space physics perspective. Because the terse

TABLE 2.1 Some Current Characteristics of Big and Little Space Physics Science

Big Science	Little Science
Broad set of goals	Specific goal
Interdisciplinary problems	Discipline-oriented problems
Scientific goals defined by committee	Scientific goals defined by individual researcher/small group
Researchers selected to fulfill program goals	Researcher sets program goals
Long implementation time	Short implementation time
Infrequent opportunities	More frequent opportunities
Large, complex management structure	Minimal management structure
High cost	Relatively low cost
Highly variable resource time line	Relatively stable resource time line
New-start funding process	Base funding
Supports project managers, engineers, administrators; science support comes at end of long planning, selling, implementation phases	Supports science community throughout project
Graduate student support data analysis phase	Graduate student support through during entire project lifetime
Dominant and increasing share of budget	Minor and decreasing share of budget

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phrasing required for the table cannot convey the full complexity of these issues, an expanded discussion of each item is presented below.

Goals

Big science programs generally pursue broad sets of scientific goals that span the interests of several subfields. These goals are often backed by an influential constituency. Such programs are characterized by size (of both personnel and sheer physical infrastructure), complexity, and/or the numbers of experimental opportunities provided. Small science programs tend to address limited scientific goals, providing answers to specific science problems of importance in their research field.

Interdisciplinary Problems

Large programs represent an effective (and at times the only) way to pursue interdisciplinary science problems. Small science programs generally focus on problems within a single scientific discipline.

Project Definition

Because of the broad set of goals involved, definition of the science in large programs is accomplished through the use of committees representing all pertinent elements of the research field. In small science programs the more limited scientific goals are defined by the individual researcher and/or the small group involved.

Investigator Selection

Investigators in large programs are selected to fulfill the scientific goals set forth by the committee defining the program. In this sense big programs are often thought of as "managed" programs. However, it should be noted that a significant amount of independent research is often supported by such programs. In small programs, investigators are funded on the basis of the science that they propose within the program they have defined.

Implementation Time

[Chapter 6](#) shows that along with the growth in program size a major increase in program implementation time has occurred. While small programs generally require shorter implementation times, [Chapter 6](#) also shows that even they are experiencing implementation delays.

Frequency

Large programs necessarily occur infrequently because of their cost and long implementation times. Small programs can be supported at a much higher frequency.

Management

Large programs generally use large and complex management structures, while small programs are more often characterized by smaller, streamlined management structures. (Recently, large program management requirements have been increasingly applied to small programs with unfortunate results, as discussed in [Chapter 6](#).)

Costs

Large programs are expensive; small programs are less expensive. However, it is important to remember that big science and little science vary from subfield to subfield, agency to agency, and notably in time. For example, the NASA Global Geospace Sciences mission, costing approximately \$400 million, was thought of as a large mission when it was formally defined in 1988; today it is still considered a large mission in many quarters of space physics. However, a 1991 planning study [9] indicated that, at that time, NASA considered missions in the \$300 million price range to be moderate, again showing the relative nature of big and little in time. Similar trends appear in other funding agencies.

Time Line for Resources

Because of the long implementation times and large costs involved, big programs result in a peak/valley resource time line, especially with respect to the science community supported by the mission. Small programs, because they are usually supported from base funding, can provide in the aggregate a relatively stable resource time line and are thus an important factor in maintaining the science infrastructure of the field.

Funding Process

Large programs generally require new-start funding approval by Congress on a program-by-program basis. This represents a large infusion of new resources into the field. Small programs are almost exclusively funded from an agency's base or core research funds. At times, however, large national initiatives, such as the Global Change Research Program, have included support for both large and small programs.

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Community Support

Big science programs, during their planning and implementation phases, support an extensive management, administrative, and engineering infrastructure. A smaller operations effort is required during the mission's operational phase. Direct research support becomes available at the end of a long planning, selling, and implementation phase. Small science projects generally support the science community directly throughout the project's lifetime.

Educational Support

Due to their long planning and implementation times, large space physics programs generally are not appropriate training grounds for students (graduate and undergraduate) until these programs enter their operational and data analysis phases. At this time a substantial opportunity becomes available for data analysis and interpretation. (Data in [Chapter 4](#) suggest that one result of this characteristic is an increasing average age for experimentalists and a decreasing average age for data analysts in space physics over the years.) Student support in small science programs is possible, and often required, throughout the planning, implementation, and data analysis phases. This provides excellent hands-on experience in experimental research and scientific program management, as well as in data analysis and interpretation.

Resource Share

Over the past decade, big science programs have come to command a dominant and increasing share of available funds (see [Chapter 5](#)). Conversely, small programs now represent a minor and decreasing share of the budget. Perhaps more importantly, the research efforts of small science are very vulnerable (sometimes to the point of extinction) to even small percentage cost overruns in big science projects.

Historical Interactions

A look at the history of the field of space physics shows that both large and small science projects have been used to advance the field to its present state of knowledge. First, as will be shown in [Chapter 4](#), the field itself has grown over the past few decades from a small band of pioneers probing the mysteries of outer space with rockets and balloons to a community of several thousand researchers using sophisticated tools to study the space environment from the earth to the stars. As further indicated in [Chapter 6](#), both small and large projects (as defined at the time) were used from the earliest days of what we now call space physics. Rockets and balloons played (and continue to play) a vital role in

studies of the upper atmosphere, ionosphere, aurora, and cosmic rays. These studies were greatly extended by the early satellite programs, judged large at the time. These satellite projects in turn evolved into big and little components, with the large platforms providing a capability to perform larger and more complex measurements than ever dreamed of before.

Space physics advanced rapidly during this period. Little science not only supported big science with the results of its research and discoveries but often itself evolved into big science operations. In a complementary fashion, big science supported little science by providing the platforms for experiments and data for many additional researchers and/or groups. A synergistic relationship existed whereby everyone seemed to benefit. Over the course of the 1980s, this symbiosis broke down. In large part, this report attempts to answer the question: "What went wrong?"

SUMMARY

Science grows, and in the past it has grown rapidly. Little science efforts often grow into larger ones, with the result that the recognition of what is big science and little science changes not only from field to field and agency to agency but also continuously with time. Current characteristics of big and little science, in the field of space physics, can be loosely defined. Big science projects attack broad problems with sophisticated technology and bring with them complex management structures, long implementation times, and high price tags. Small science projects involve individuals, or small teams of researchers, pursuing specific research goals via relatively inexpensive experiments that can be rapidly implemented.

We have seen that in the past big and little science projects in space physics were supportive of one another and synergistic in the research being pursued. Results of small science programs often motivated and formed the rationale for big programs; these big programs in turn provided many additional opportunities for the individual researcher.

Over the past decade tensions have developed between large and small science in the space physics community. Clearly, there can be no either/or; an appropriate dynamic balance between the two approaches must be found. This balance will vary both in time and from subfield to subfield. It is a balance that must be established and continually updated by the space physics research community and reinforced by the funding agencies.

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3

Research Funding Trends

The main purpose of this chapter is to document the earlier statement that funding for all scientific research has substantially increased over the past decade. The bulk of the data were taken from the OTA report [2], which used the research expenditure compilations prepared by the National Science Foundation (NSF) in the report *Federal Funds for Research and Development: Detailed Historical Tables, Fiscal Years 1955-1990* [10].

Figure 3.1 shows a history of federally funded research from 1960 through 1990. Associated with the advent of the space age, there is a large increase in federal research funding that peaks in 1965 and then gradually declines until 1975. Since 1975, the nation's research funding has shown a steady increase, resulting in better than a 40 percent growth in constant-year dollars.¹ It is the past 15 years, since 1975, that the present report addresses.

This increase is also observed as an increasing share of the Gross National Product (GNP). Figure 3.2 shows both total and nondefense research and development (R&D) expenditures as a percentage of GNP for the United States and four other countries, Federal Republic of Germany (FRG, now Germany), Japan,

¹ Note that Figure 3.1 shows not only a steady increase in federally funded research since 1975 but also that the bulk of the increase occurred in the area of basic research, resulting in a doubling of funds in this area over the 15 years prior to 1990. However, the actual division of resources between basic and applied research is somewhat uncertain since such a distinction is ambiguous and varies among the federal agencies. Even with this ambiguity, however, it is clear that the U.S. research program has experienced a substantial real-dollar increase since 1975.

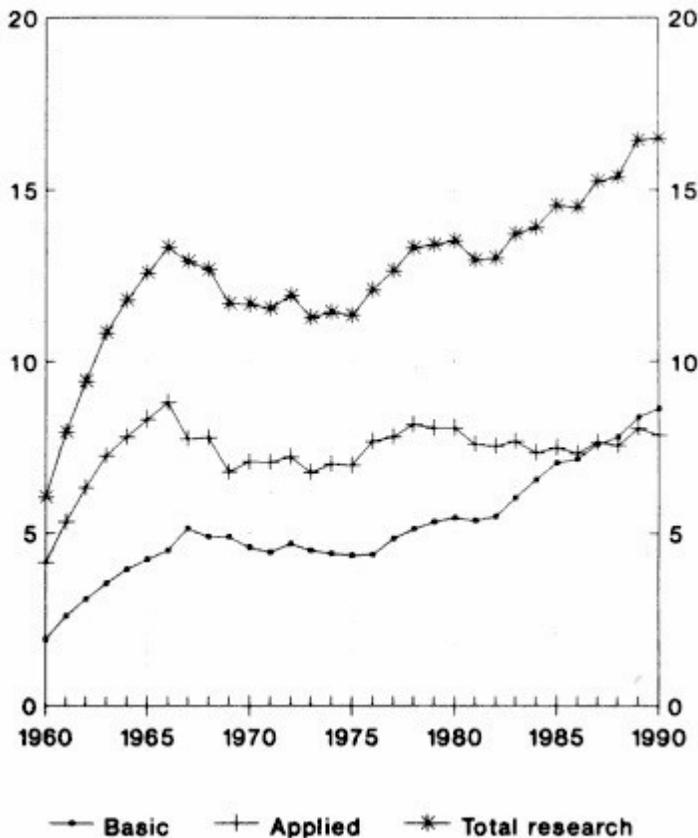


FIGURE 3.1 Federally funded research (basic and applied), fiscal years 1960-1990 (in billions of 1982 dollars). Source: National Science Foundation, *Federal Funds for Research and Development, Detailed Historical Tables: Fiscal Years 1955-1990* (Washington, D.C., 1990), Table A; and National Science Foundation, *Selected Data on Federal Funds for Research and Development: Fiscal Years 1989, 1990 1991* (Washington, D.C., December 1990), Table 1.

Note: Figures were converted into constant 1982 dollars using the Gross National Product (GNP) Implicit Price Deflator. For 1990 (current dollars), basic research = \$11.3 billion, applied research = \$10.3 billion, and total research = \$21.7 billion. Figures for 1990 are estimates.

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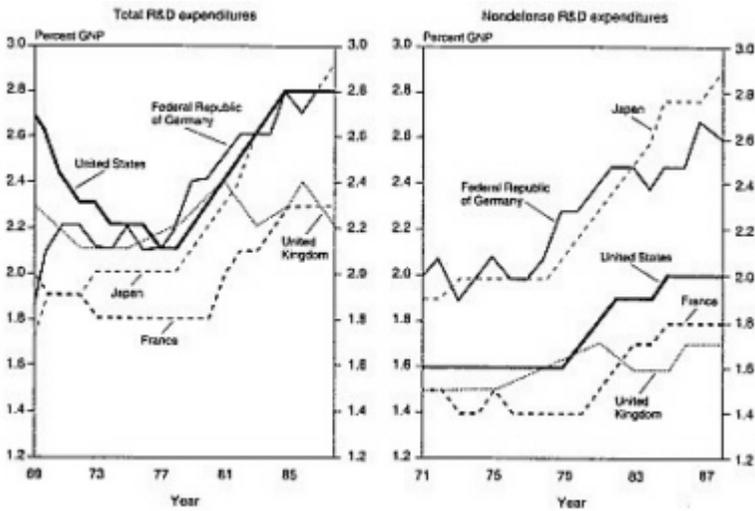


FIGURE 3.2 R&D expenditures as a percentage of gross national product, by country.

Source: National Science Foundation, *National Patterns of R&D Resources: 1990*, Final Report. NSF 90-316 (Washington, D.C., 1990), Tables B-18 and nbB-19.

France, and the United Kingdom. The fraction of the GNP represented by total R&D expenditures in the United States has increased by about 27 percent since 1975, while the nondefense R&D fraction has increased by about 25 percent. Thus, the growth in research funding in the United States has exceeded the growth in the GNP. The higher percentage for nondefense R&D in the FRG and Japan seen in Figure 3.2 reflects the fact that these countries have not had to support a defense-related R&D program.

Figure 3.3 shows federally funded research by performer for the years since 1969. It can be seen that the university research community, the main beneficiary of the increase shown in Figure 3.1, has received an additional 2.5 billion constant-year dollars (an increase of over 70 percent) since 1975. Note that the federal government's share has remained steady and at a relatively high level.

Figure 3.4 shows the overall research funding trend for the National Aeronautics and Space Administration (NASA) and NSF, the two main funding sources for space physics research. There was a steady funding increase through the 1980s, preceded in the case of NSF by a flat funding profile and in the case of NASA by a somewhat more variable funding profile. The overall inflation-adjusted increase since 1975 has been over 140 percent for NASA and a more modest 30 percent for NSF. Note that by 1990 funding for basic research had reached essentially the same level at both institutions.

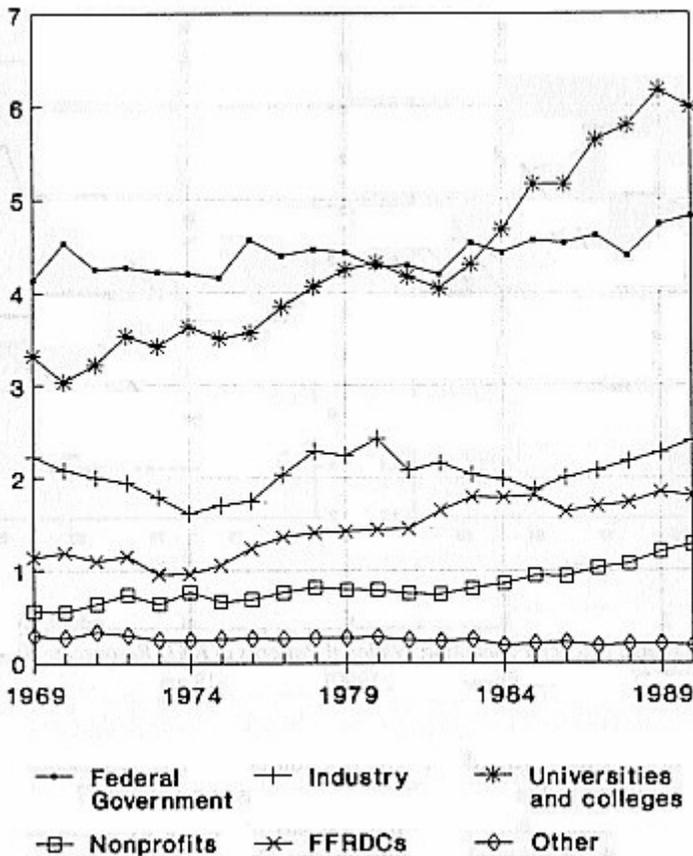


FIGURE 3.3 Federally funded research by performer, fiscal years 1969-1990 (in billions of 1982 dollars). Source: National Science Foundation, *Federal Funds for Research and Development, Detailed Historical Tables: Fiscal Years 1955-1990* (Washington, D.C., 1990), Table 17; and National Science Foundation, *Selected Data on Federal Funds for Research and Development: Fiscal Years 1989, 1990 and 1991* (Washington, D.C., December 1990), Table 1.

Key: FFRDCs include all Federally Funded Research and Development Centers that are not administered by the federal government. "Other" includes federal funds distributed to state and local governments and foreign performers.

Note: Research includes both basic and applied. Figures were converted to constant 1982 dollars using the GNP Implicit Price Deflator. Figures for 1990 are estimates.

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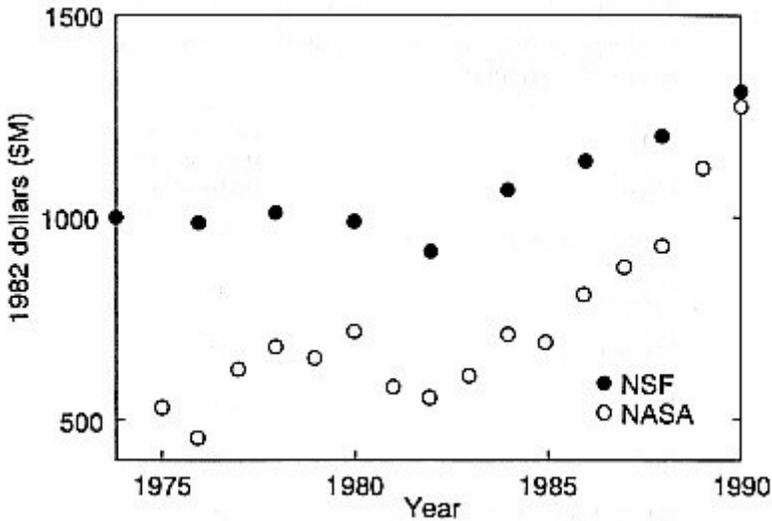


FIGURE 3.4 NSF and NASA research funding (in millions of constant 1982 dollars).

Related to the basic research funding issue is the question of how much of this funding actually leaves the funding agency and reaches the nonfederal research community, primarily universities and colleges (e.g., see Figure 3.3). It might be expected that agencies whose main responsibility is to support research would allocate the majority of their resources to the university research community. On the other hand, agencies with strong mission and operational responsibilities may perform much of their research in-house. Using data from the NSF report on federal research expenditures [10], we find that for the past 15 years the ratio of in-house research obligations to total research obligations ranged from approximately 10 percent for the NSF, 30 to 50 percent for NASA and the Department of Defense (DoD), 70 to 80 percent for the National Oceanic and Atmospheric Administration. This is in agreement with the qualitative expectations mentioned above. Data for the past several years also show that for NASA and DoD this ratio has been decreasing, showing an increased fraction of their research funding leaving these agencies.

Table 3.1 summarizes various elements of the growth in research funding in the United States from 1981 through 1989. The percentage values shown have been adjusted to the closest 5 percent increment. The top five elements are taken from the general funding data presented earlier. The bottom three elements have been synthesized from data received by a NASA-sponsored University Relations Task Force² and refer specifically to NASA research funding to universities closely related to and including space physics programs.

² These data were made available by the NASA Advisory Council University Relations Task Force (Steven Muller, Chair).

TABLE 3.1 Summary of the Percentage Growth in Constant-Year Dollar Funding of Various U.S. Research Elements

Element	Constant-Year Dollar Growth, 1981-1989 (%)
Federally funded research (FFR)	30
FFR basic	55
FFR in universities	55
NASA basic	100
NSF	30
NASA/OSSA with flight projects	60
NASA/OSSA without flight projects	45
Seven-university sample (funded by NASA/OSSA)	20-40

Funding for NASA's Office of Space Science and Applications (OSSA) includes substantial amounts for project-related activities. As these activities often include substantial support for industry, they were removed from the university funding estimates to obtain a lower limit to that funding, and the result is shown in Table 3.1, where funding growth, with and without projects, is given.

An illustration of these research funding trends can be found in data from seven universities receiving NASA/OSSA funds over the 1981-1989 period. Funding changes ranged from a 35 percent decrease to a 245 percent increase, with an average value of 40 percent growth, as shown in Table 3.1. If the possibly anomalous 245 percent increase is removed from the sample, the average growth would be 20 percent. Although the sample is small and the growth experienced from university to university varied widely, it does appear that the average growth in NASA/OSSA funding in space physics fields at these seven universities has been consistent with the national trend in research funding.

In summary, we conclude that research funding in the United States, both generally and in the field of space physics, has increased substantially over the past 10 to 15 years.

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4

Demographics

This chapter presents general demographic trends within the space physics community and relates them to the funding trends discussed in [Chapter 3](#). To assess the health of the field, the committee sought data that might indicate trends in the number of scientists studying space physics, the age distribution of these scientists, and the kinds of projects they are engaged in. Of course, scientists in the various disciplines that fall under the purview of the Committee on Solar-Terrestrial Research and the Committee on Solar and Space Physics are not conveniently listed in a central registry, but they do tend to belong to certain professional societies. One particular organization—the American Geophysical Union (AGU)—has elements of all the subdisciplines and should therefore give a good overall indication of the field's growth. [Figure 4.1](#) shows the membership of the AGU and its Space Physics section (now called Space Physics and Aeronomy) from 1974 to 1991. Very steady growth is seen for both the union and its space physics component. During the 1980s, both grew at a much faster rate than in the 1970s, with the space physics fraction growing slightly more slowly than the parent organization. The number of graduate student members in the AGU also is shown in [Figure 4.1](#). Although this number may be a less reliable measure of the actual population, it does show similar growth trends.

A similar rapid growth during the 1980s is seen in the membership of the American Astronomical Society's Solar Physics Division (AAS/SPD). [Figure 4.2](#) shows a steady increase in division membership, with some 50 percent more members in 1991 than in 1981. It is of interest to compare both the AGU space physics membership and the AAS/SPD membership growth with the funding trends presented in [Chapter 3](#). We show this comparison in [Table 4.1](#) for two

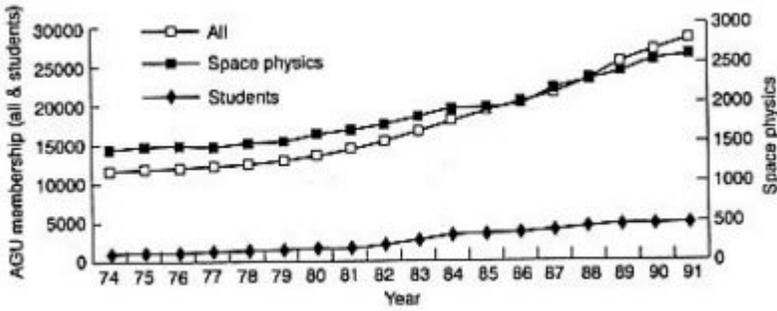


FIGURE 4.1 Growth of space physics community within the American Geophysical Union.

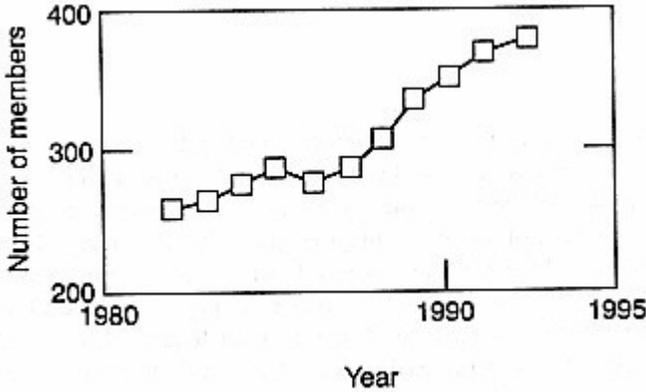


FIGURE 4.2 Growth of American Astronomical Society's Solar Physics Division membership.
 Source: Karen Harvey, AAS/SPD Treasurer.

time intervals over which data are available. The data on percentage growth for federally funded basic research, for the National Aeronautics and Atmospheric Administration's (NASA) Office of Space Science and Applications (OSSA) research funding, and for the National Science Foundation's (NSF) research funding are taken directly from Chapter 3, Figure 3.1, and Table 3.1. Table 4.1 shows that the growth in the size of the space physics field occurred at essentially the same rate as the funding increases experienced over the past 10 to 20 years.

The growth in AGU membership presented above is also reflected in levels of activity over the same period. Figure 4.3 shows the total attendance at national AGU meetings, together with the number of abstracts submitted. The number

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TABLE 4.1 Percentage Growth in Real-Dollar Funding and in the Size of the Space Physics Research Field

Item	Percentage Growth	
	1974-1990	1981-1989
AGU Space Physics Community	90	50
AAS Solar Physics Community	—	40
Federally funded basic research	100	55
NASA/OSSA research funding	—	45-60*
NSF research funding	—	30

* See Table 3.1.

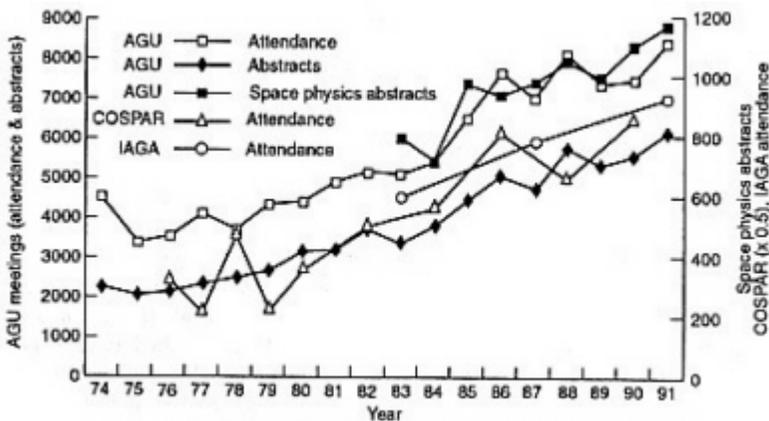


FIGURE 4.3 Measures of space physics growth.

of space physics abstracts is also shown for the last nine years. The meeting attendance increased from 1974 to 1991 by the same factor of two that describes the growth in AGU membership (Figure 4.1) with the number of abstracts increasing by an even greater factor of about three. Figure 4.3 also shows the attendance at meetings of two prominent international scientific organizations that include space physics as a major element of their activities—the Committee

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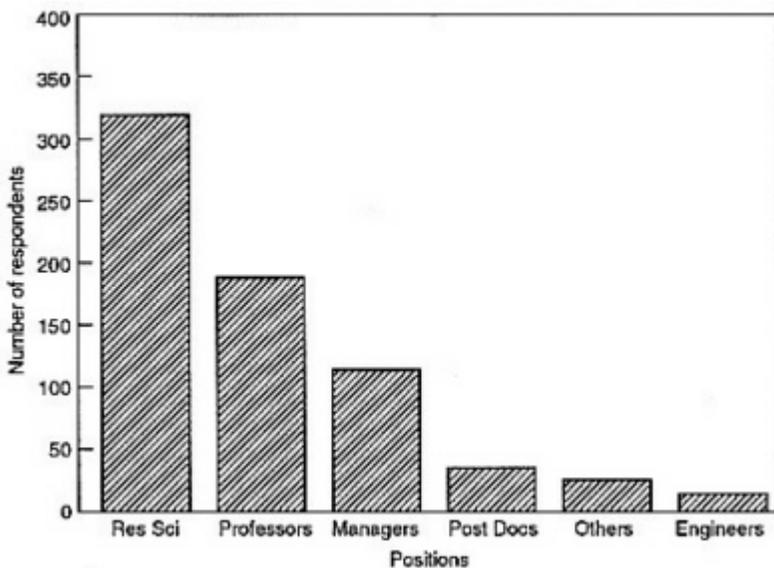


FIGURE 4.4 Space physics community survey positions of primary responsibility.

on Space Research (COSPAR) and the International Association of Geomagnetism and Aeronomy (IAGA).

NASA's Space Physics Division [5] recently attempted to gather additional information concerning the demographics of the U.S. space physics community. A questionnaire was sent to 1,770 members of the community on January 2, 1991, and 686 replies were received. While no attempt was made to ensure a representative sample, and the response rate was only about 39 percent, it is instructive to look at the trends that emerge. Figure 4.4 shows the breakdown by position of the respondents. The responses were dominated by persons categorizing themselves as research scientists, followed by university professors.

The 1991 National Science Board (NSB) study [11] of science and engineering indicators noted that "the average age of academic researchers increased in the past decade, continuing a trend that began in the early 1970s. The median age of academic researchers rose from 38.7 years in 1973 to 39.7 years in 1979; it was 43.8 years in 1989." (The impact on these results of university hiring practices, such as limits to tenure-track appointments, is not known.) This conclusion is consistent with the results of the NASA survey [5]. The age distribution of the respondents to the NASA survey is shown in Figure 4.5. The median age falls into the 46-to 50-year bracket, almost the same age as for the academic researchers addressed by the NSB study. The age distribution for each of the

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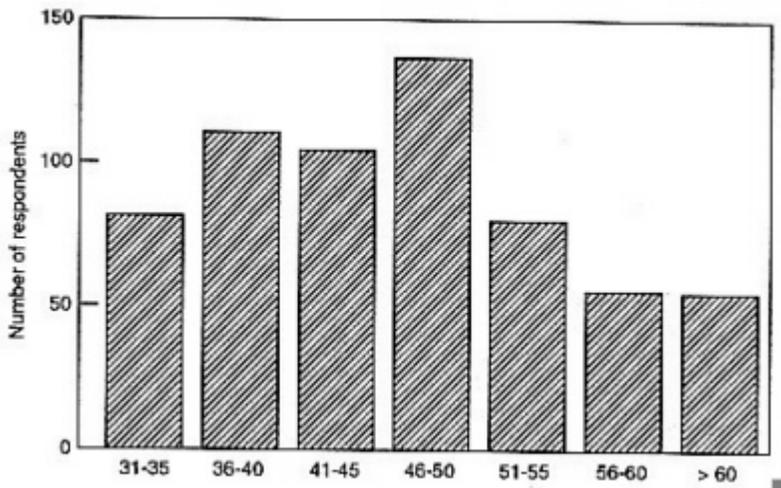


FIGURE 4.5 Space physics community survey age distribution.

pertinent subdisciplines of research (cosmic and heliospheric physics; ionospheric, thermospheric and mesospheric physics; magnetospheric physics; and solar physics) is quite similar. As a whole, the median age is a few years higher than that of the AAS membership, largely due to the greater number of respondents over age 50. This trend is particularly noticeable for NASA employees, who represent some 10 percent of those under age 40, some 15 percent of those in the median 40 to 50 bracket, and some 20 percent of those over age 50.

Figure 4.6 shows the survey breakdown by research technique and institution. Other than a slightly larger fraction of theoretical research in universities, the three research subdiscipline areas appear to be fairly evenly distributed in each research environment. A disturbing trend is illustrated, however, in Figure 4.7, which shows the fraction of each age group involved in the subdiscipline techniques of data analysis, theory, and instrumentation. The fraction of each group represented by experimentalists increases dramatically with age.

This of course leads to the question: Are we training a sufficient number of new experimentalists? A partial answer can be gleaned from the results of a separate NASA questionnaire completed by 130 graduate students. These results are also shown in Figure 4.7. Only about 10 percent of graduate student respondents are involved in instrumentation. The remainder are split approximately equally between theory and data analysis. One can speculate that the ever-increasing time scales associated with experimental research (see Chapter 6) are driving experimentally oriented students toward dissertations primarily involving theory or data analysis.

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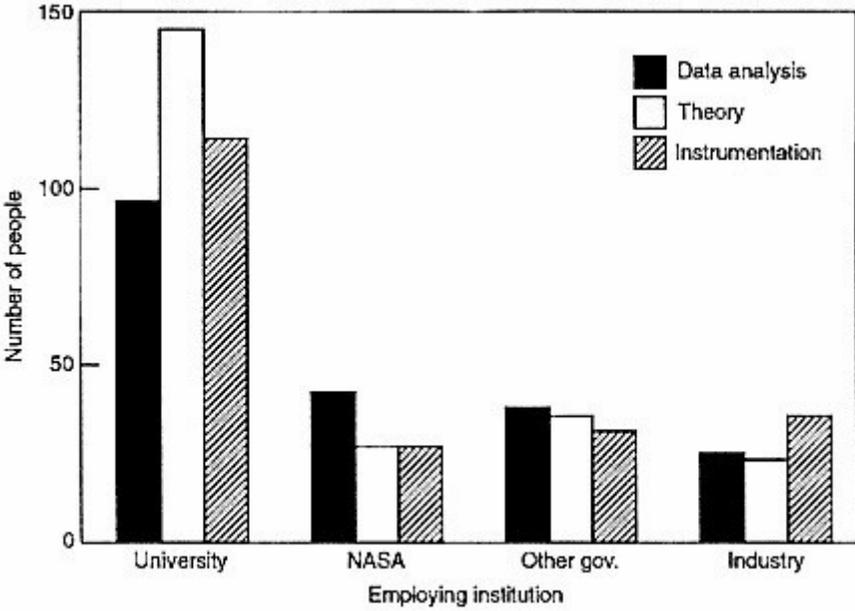


FIGURE 4.6 Space physics community survey distribution of techniques by institution.

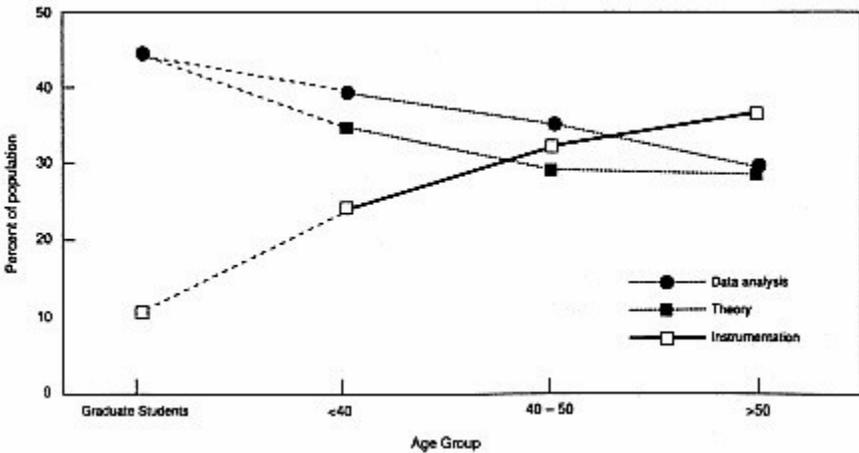


FIGURE 4.7 Age distribution by prime technique in the space physics community. Points on left edge of plot represent present graduate students' prime technique.

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Graduate students represent the future of the field, and it is important to assess whether enough are being trained and are remaining in the field. Once again, precise data are difficult to obtain, but the committee has assembled some interesting indicators.

In responses to the NASA survey [5], 185 professors indicated that they were advising 342 graduate students working toward space physics dissertations. (These numbers refer only to graduate students who have started their research and do not include first- and second-year graduate students not yet committed to a space physics dissertation topic.) Half of those students were supported by funds from NASA's Space Physics Division; nearly 80 percent received some form of government funding.

Data available from the University of California at Los Angeles and the University of Chicago show that of 28 space physics graduates in the years 1964-1969, 12 are still in the field (43 percent); seven of these are university faculty members (25 percent). The corresponding numbers for the 1970-1979 period are 50 graduates, with 20 still in the field (40 percent) and 11 faculty members (22 percent). For 1980-1989 there were 29 graduates, with 23 still in the field (79 percent) and five faculty members (17 percent). Very few students (nine out of a total of 107 graduates) moved abroad after graduation. (It is interesting to compare these numbers to results of the NASA survey, where three out of four graduate student respondents indicated that they expected to remain in the field after graduation.) The high ratio of 1980-1989 graduates still in the field may be due to the increase in funding in the 1980s (Figure 3.1, Table 3.1). On the other hand, it may simply be a recent phenomenon, requiring a substantial input of fresh funds to retain these people in permanent jobs and make their current soft money positions available to the graduates of the 1990s.

The number of graduates that in time become faculty members indicates that "zero population growth" (ZPG) of faculty would occur for a lifetime number of around five students graduated per faculty member. The NASA survey [5] response showing an average of two graduate students supported per faculty member at a given time in space physics appears to be well above this ZPG level, consistent with the growth trends presented earlier in this chapter.

SUMMARY

The size of the space physics community has grown along with the funding increases of the past two decades, as discussed in Chapter 3. The percentage growth in funding levels and in various measures of the size of the field are similar.

The average age of the field is increasing. More significantly, a decreasing proportion of young researchers are entering the experimental side of the field. In a field as empirically driven as space physics, this is an ominous trend.

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5

Base Program Funding Trends in Space Physics

Chapter 4 has shown that the increase in size of the space physics community has, within the accuracy of the available data, correlated with both the national and institutional funding trends presented in Chapter 3. All have increased measurably. This expansion may appear to contradict one assumption behind the space physics paradox, because it seems to imply that the field has benefited directly from increased funding.

In referring to "benefits" to the field, it is important to reiterate that this report examines the *conduct* of space physics science, that is, the funding and administrative processes that provide the foundation for research activities. It is beyond the scope of this report to judge the relative *value* of the discoveries made over the years or the power of the new ideas that have been generated. However, the committee does believe that there are connections between the conduct and the content of science. Because breakthroughs can come unpredictably from unexpected sources (and are often unrecognized until much later), the overall advance of knowledge can only be assured by giving a variety of researchers the opportunity to pursue their ideas.

In this chapter we look in more detail at how this era of increased funding has affected the base-funded research program in space physics. As discussed in Chapter 2, it is the base-funded program that has offered the primary support for small science opportunities and, through this support, has provided the foundation for many large science programs. Long-term base-funding data sets for the relevant fields were obtained from the Upper-Atmospheric Section (UAS) of the National Science Foundation (NSF) and the Space Physics Division of the National Aeronautics and Space Administration (NASA). The NSF UAS covers

TABLE 5.1 NSF Proposal Statistics for the Upper-Atmospheric Section

	Fiscal Year						
	1985	1986	1987	1988	1989	1990	1991
Number of competitive proposals	142	115	109	127	120	163	205
Number funded	101	78	76	80	67	90	129
Percent funded	71	68	70	63	56	55	63
Average duration (years)	1.9	2.1	2.0	2.5	1.9	2.0	2.1
Average size (\$1,000)	56	52	47	46	52	52	49
Average size (\$1,000 1985 dollars)	56	50	44	42	46	45	41
Total grants (\$1,000 1985 dollars)	5,656	3,900	3,344	3,360	3,082	4,050	5,289

aeronomy, magnetospheric, solar-terrestrial, and upper-atmospheric facilities. The UAS data on competitive proposals over a seven-year period are shown in Table 5.1. For this space physics portion of NSF, the total number of competitive proposals increased substantially in FY 1990 and FY 1991. The FY 1991 increase is due, in large part, to the start of the Geospace Environment Modeling (GEM) program. The fraction of proposals funded decreased slightly over this seven-year period, and the average grant duration remained at approximately two years. Of more significance is the observation that the average grant size in constant 1985 dollars decreased by about 25 percent over this interval.

Figure 5.1 compares these results with those from the other major source of space physics funding, the Space Physics Division of NASA. This division covers aeronomy, ionospheric, magnetospheric, cosmic rays, heliospheric, and solar physics. NASA and NSF data are shown in the Figure for an overlapping time period of several years, with both data sets normalized to FY 1985 dollars. The data all show the same downward trend, although the NASA average grant size is about \$20,000 larger than the average NSF grant size. The actual buying power of an individual grant has decreased by \$15,000 to \$20,000 over the last seven years. (As discussed later in this chapter, this decrease is exacerbated by increasing university overhead costs for the same period.) Thus, what scientists have been saying about a shrinking grant size is confirmed by the data in Figure 5.1: the average grant buys much less today than it did seven or eight years ago.

This downward trend in the average size of an individual grant (in fixed-year dollars), combined with the actual size of each grant, suggests that an indi

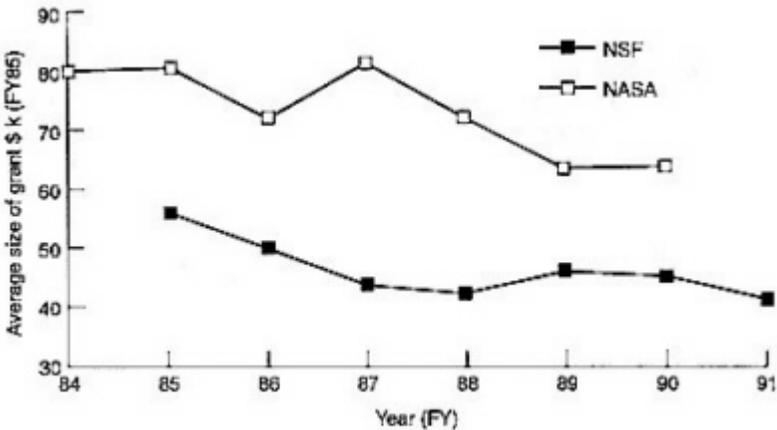


FIGURE 5.1 Space physics grant trends.

vidual researcher must write several proposals per year to remain funded and that this number is increasing. To get a rough yet quantitative measure of this trend, we can divide the annual cost of a research scientist (salary, overhead, travel, etc.) by the proposal success rate (number funded/number submitted), the average grant size (in dollars per year), and the average grant duration. For convenience, let us call this the Proposal Index (PI), where

$$PI = \frac{\text{cost of scientist}}{\text{proposal success rate} \times \text{average grant size} \times \text{average grant duration}}$$

Assuming a research scientist cost range (as defined above) of \$120,000 to \$180,000 and using the NASA- and NSF-supplied values for the remaining parameters, we find that to remain funded with NASA support "Dr. Average" had to write one and one-half to two proposals in FY 1989 compared to two and one-half to four proposals in FY 1992. With over 100 additional proposals submitted to NASA's Supporting Research and Technology program in 1993 as compared to 1992, the PI will continue to grow. From the NSF data we obtain a similar PI value of two to three for FY 1991. Thus, the available data indicate that full-time researchers applying for NASA or NSF core funding must write an average of two to four proposals per year, with the number expected to grow in the future.

What is the effect of having scientists spend this much time writing proposals instead of doing research? One way to address this question is to estimate the costs of writing and reviewing submitted proposals and to express these costs as

a fraction of the funds being disbursed. We assume that the average proposal takes two weeks to write and that each proposal can be reviewed in one day by each reviewer. By multiplying the number of proposals received by the average writing time, and by the average cost of a researcher (as given above in the calculation of the PI), adding the cost of the review, and dividing by the available funds, we obtain a measure of this overlooked cost burden. Let us call it the Hidden Overhead Index (HOI). Using financial data supplied by NASA, and simple assumptions about the cost of the review process, the NASA HOI was between 20 and 30 percent in FY 1989 and between 30 and 50 percent in FY 1992. For NSF, the HOI is around 10 to 15 percent for both FY 1989 and FY 1991. (This difference may be due in part to the manner in which NSF and NASA interact with their respective research communities.) Nonetheless, the magnitude of this index is alarming. It indicates that the current funding cycle is very costly. It also appears that, in the case of NASA, as proposal success rates or grant sizes drop, the entire process becomes even less cost effective.

We attempted to gain further insight into the trends in grant funding and its role in contemporary research activities by examining the results of a survey sent to over 300 members of the American Astronomical Society's (AAS) Solar Physics Division.¹ Unfortunately, the response rate was low (18 percent), and no attempt was made to normalize the sample. However, in the absence of more rigorous data, it is instructive to look at the trends for 1981-1991 as extracted from the 55 responses received. The respondents reported that during that period the number of submitted proposals per scientist per year rose from 1.1 to 3.0, while the success rate dropped from 94 percent to 62 percent. Factoring in the trend for granting only a fraction of the requested budget (91 percent in 1981, versus only 45 percent in 1991), we find that for every dollar requested by these scientists, 85 cents was realized in 1981 and only 28 cents in 1991. The respondents estimated that the fraction of time spent doing actual science dropped from 74 percent in 1981 to 46 percent in 1991. (This result must be interpreted with caution since other factors may play a role as a researcher matures and accepts broader responsibilities.) Over the 1981-1991 time period, about one-third of the respondents were 100 percent dependent on research grant income for their salary support. The number of Ph.D. graduates grew by 59 (more than one per respondent), consistent with the 10- to 15-year doubling period noted in Chapters 2 and 4. The reports from these 55 individual scientists in solar physics are consistent with the other data we examined: more proposals need to be written today for scientists to stay gainfully employed, resulting in less time for research.

¹ The survey was developed for the Committee on Solar-Terrestrial Research/Committee on Solar and Space Physics by D. Rust and G. Emslie and was presented in the AAS Solar Physics Division Newsletter, #2, June 1992.

In an attempt to compensate for the erosion of the base-funded research program, NSF science managers have concentrated on establishing a number of "directed" research projects. In this context, directed research supports a specific, larger science program or an *a priori* defined science problem. In contrast, core program funding generally supports "undirected" research within a broadly defined discipline. In some cases several undirected projects are grouped based on their similar thrusts to form a larger synergistic program, thereby providing a sense of direction to this group of projects.

Recent NSF projects belonging to the directed project category are the ongoing Coupling, Energetics, and Dynamics of Atmospheric Regions (CEDAR) program, the Geospace Environment Modeling (GEM) program, and the Radiative Input from Sun to Earth (RISE) program. Of interest within the context of this study is how the awards within such directed projects compare with the more traditional core program awards. Figure 5.2 shows the new award size distribution in the CEDAR program for 1991 and 1992. We see that the average size of the CEDAR awards has decreased significantly, similar to the trend shown for regular NSF grants in Figure 4.1.

As mentioned earlier, any increases in overhead expenses will further reduce the actual dollars received by the research scientist. Chapter 3 (Figure 3.3) showed that academia has been the largest benefactor of federally funded research. Figure 5.3 shows an estimate of the cost components associated with U.S. academic research budgets. The senior scientist component of Figure 5.3 indicates that the research community has expanded, consistent with our findings for space physics discussed earlier in this chapter. Figure 5.3 further shows a marked increase in indirect costs. To see how such an indirect cost increase affects the space physics researcher, we present data from the University of California at Los Angeles (UCLA) Institute of Geophysics and Planetary Physics, the primary administrative home of space physics at UCLA. UCLA overhead charges have increased by more than 50 percent since 1978, as shown in Table 5.2. Figure 5.4 shows the effect of this adjustment on NASA research funding at UCLA. The funding level, adjusted to FY 1991 dollars (using the consumer price index [CPI]) has fluctuated, with an overall increase of 40 percent. However, because of the increase in overhead charges, the actual increase in research support is only 25 percent. Figure 5.4 shows that the fraction of grant and contract funds that actually reach the researcher dropped from 78 percent in the late 1970s to 69 percent in the early 1990s.

The increase in overhead costs is consistent with data on employment patterns at UCLA [12]. In the decade from 1977 to 1987, the academic support staff (including environmental safety staff, contract and grant officers, affirmative action officers, etc.) grew by 44 percent and the executive staff grew by 36 percent. In the same time frame, secretarial staff grew by only 1.4 percent and faculty decreased by 6.8 percent.

If these data are representative, it appears that the cost of administering

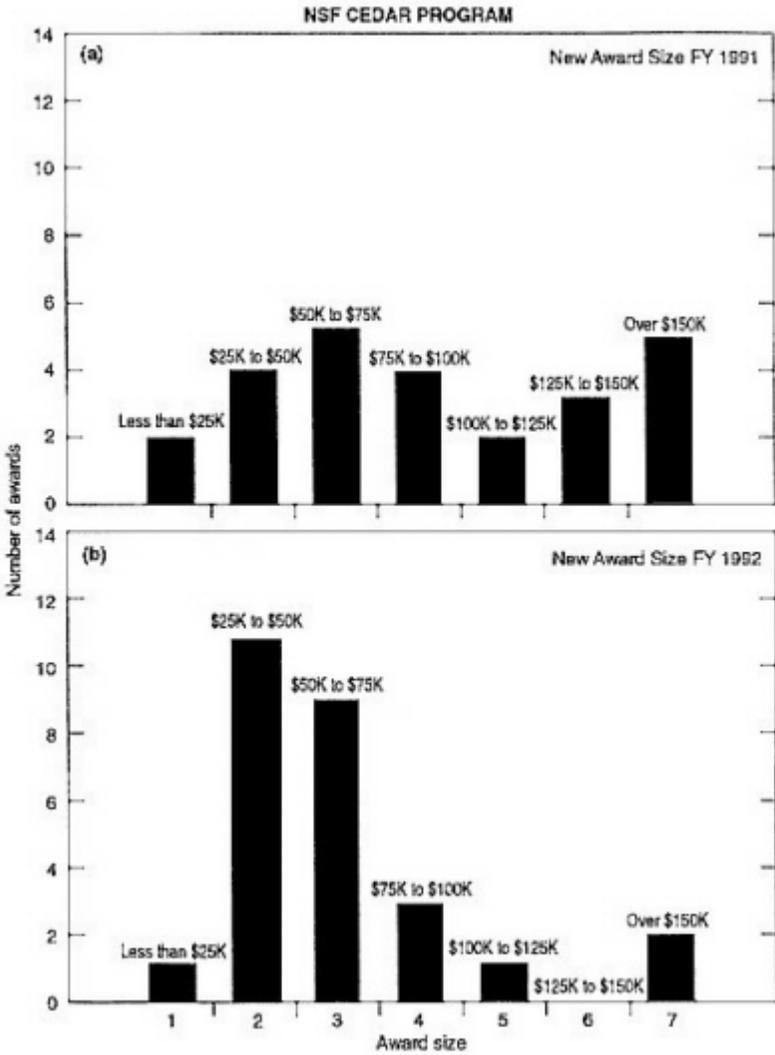


FIGURE 5.2 Award size in NSF CEDAR program, 1991 and 1992.

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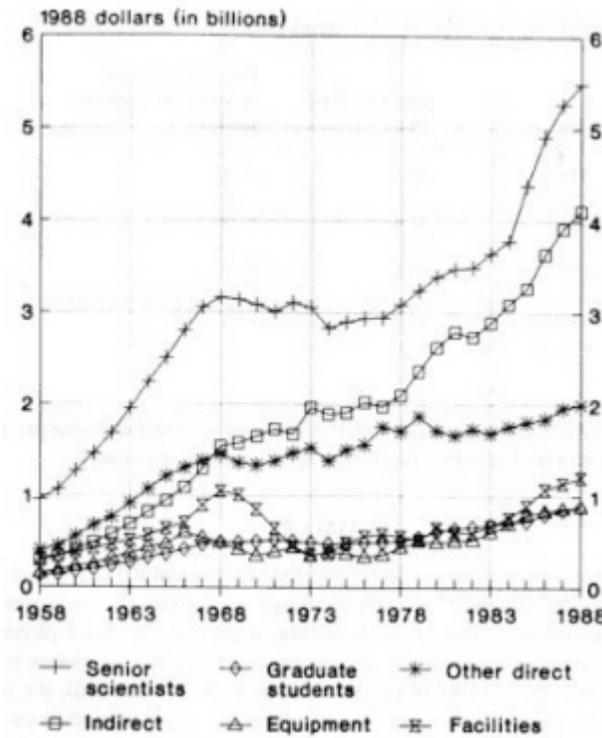


FIGURE 5.3 Estimated cost components of U.S. academic R&D budgets, 1958 to 1988 (in billions of 1988 dollars). Source: Government-University-Industry Research Roundtable, *Science and Technology in the Academic Enterprise: Status, Trends, and Issues* (Washington, D.C., National Academy Press, 1989), Figure 2-43.

Note: Constant dollars were calculated using the GNP Implicit Price Deflator.;

Definition of Terms: Estimated personnel costs for senior scientists and graduate students include salaries and fringe benefits, such as insurance and retirement contributions. Other direct costs include such budget items as materials and supplies, travel, subcontractors, computer services, publications, consultants, and participant support costs. Indirect costs include general administration, department administration, building operation and maintenance, depreciation and use, sponsored research projects administration, libraries, and student services administration. Equipment costs include reported expenditures of separately budgeted current funds for the purchase of research equipment and estimated capital expenditures for fixed or built-in research equipment. Facilities costs include estimated capital expenditures for research facilities, including facilities constructed to house scientific apparatus. **Data:** National Science Foundation, Division of Policy Research and Analysis. Database: CASPAR. Some of the data within this data base are estimates, incorporated where there are discontinuities within data series or gaps in data collection. Primary data source: National Science Foundation, Division of Science Resources Studies, Survey of Scientific and Engineering Expenditures at Universities and Colleges; National Institutes of Health; American Association of University Professors; National Association of State Universities and Land-Grant Colleges.

TABLE 5.2 UCLA Overhead Rates

Year	Overhead Rate (%)	Fraction of Grants Available to Support Research
1978	28.1	0.78
1982	35.4	0.73
1985	42.5	0.70
1989	43.5	0.69

contracts and grants has grown, and this increase in overhead charges has reduced the fraction of research funds that actually support research.

SUMMARY

Previous chapters demonstrated that total funding and the overall size of the space physics community have both increased at basically the same rate. This chapter looks more closely at the base-funded program in space physics, the traditional source of support for little science, and finds that the picture is not so rosy. Although the number of proposals submitted has increased, the fraction receiving funding and the average grant size have decreased significantly over the past decade. At the same time, university overhead rates have claimed a growing chunk of grant monies.

These factors combine to paint a picture of research scientists struggling to remain funded. The average space physics scientist must now write two to four proposals per year, twice the number required five years ago. Translating this time drain into dollars, we estimate that the writing and reviewing of submitted proposals currently represent a hidden cost that may be as large as 50 percent of the funding being awarded.

Clearly, the base-funded program has not participated proportionately in the overall space physics research funding increase. Although we do not attempt to quantify the effect this has had on the quality of science produced, we can conclude that the core program has become much less efficient during the past decade and that this trend is likely to continue in the future.

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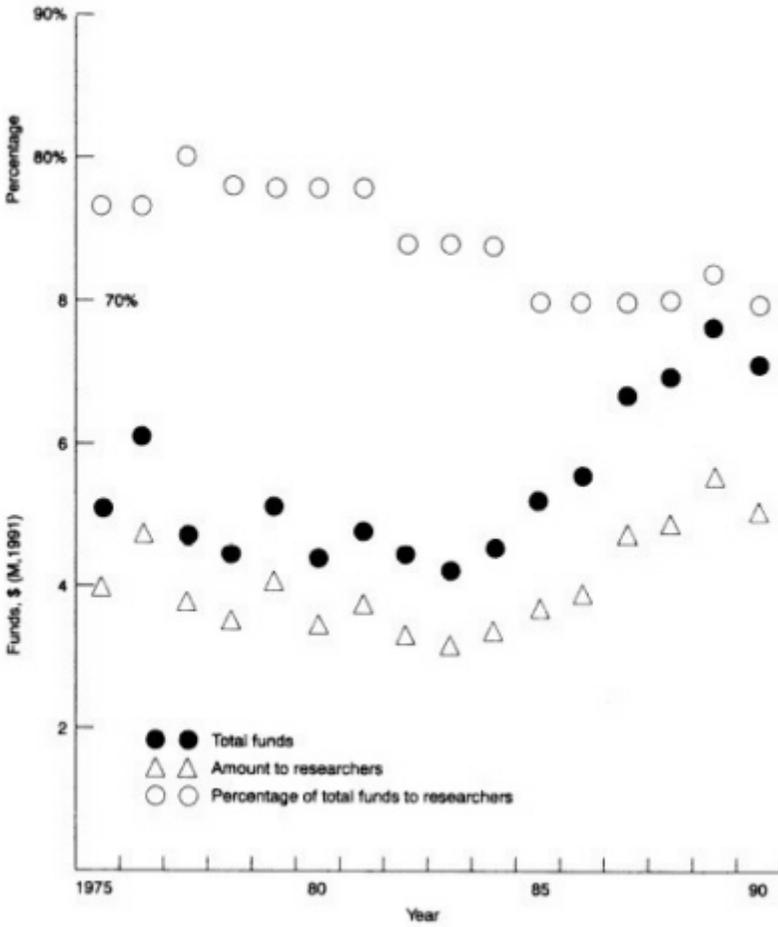


FIGURE 5.4 NASA grants and contract history at UCLA broken down into total funds, amount actually available to the researcher, and the percentage of total funds available to the researcher.

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6

Trends in the Conduct of Space Physics

This chapter identifies further trends in the implementation of scientific research in areas of interest to the Committee on Solar-Terrestrial Research (CSTR)/Committee on Solar and Space Physics (CSSP) and relates them to the funding and demographic trends discussed in Chapters 3 to 5. Data have been provided by the funding agencies or assembled by CSTR/CSSP committee members, and individual examples are used where appropriate. Much of the data on past programs were difficult to obtain, especially with respect to parameters defined in hindsight. Nevertheless, trends can be discerned that contribute to a historical perspective.

SATELLITE OBSERVATIONS

Explorer Program

A vital element of space physics research is the availability of in situ data. Without intending to imply that quantity equals quality, one relevant measure of space physics research opportunities remains the number of launches and experiments. A mainstay of the space physics research program, the National Aeronautics and Space Administration's (NASA) Explorer Satellite Program provides one accessible and well-defined data set¹ that illustrates long-term trends in the availability and implementation of space physics research opportunities.

¹ Explorer launch data were made available by T. Perry, NASA Headquarters.

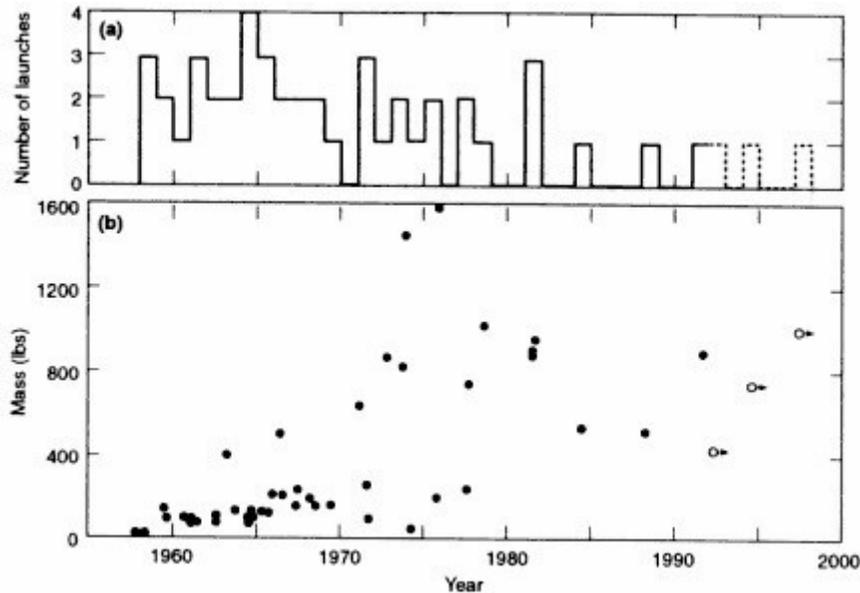


FIGURE 6.1 Space-physics-related Explorer launch history: number of launches and satellite mass.

Figure 6.1a shows the number of Explorer launches from 1958 to the present, along with projected launches for programs approved through 1997 (projected launch times should be considered uncertain). The data show a clear and continuing decrease in space-physics-related launches since the 1960s. However, we are not able to conclude from this result alone that the number of research opportunities has decreased. If, for example, satellites increased in size and thus carried more experiments, the number of actual research opportunities (as measured by experiments flown) may not have decreased.

Figure 6.1b shows the mass evolution of the Explorer satellites. Again, the masses shown for future launches should be considered uncertain. The Figure shows a general increase in Explorer size since 1958. Further, it appears to be possible to separate the data into two categories: small and large Explorers. (Interestingly, this implies that at a very early stage, the space physics community saw the need for *both* small and large missions.)

How do these factors affect the number of experiments flown? Figure 6.2 shows a repeat of the number of space-physics-related Explorer launches since 1958, along with the number of experiments flown onboard those Explorer satellites. The decrease in number of launches from the 1960s through the 1970s is compensated for by the increasing satellite sizes, giving a comparable number of experiments flown in both decades. However, the launch frequency became so

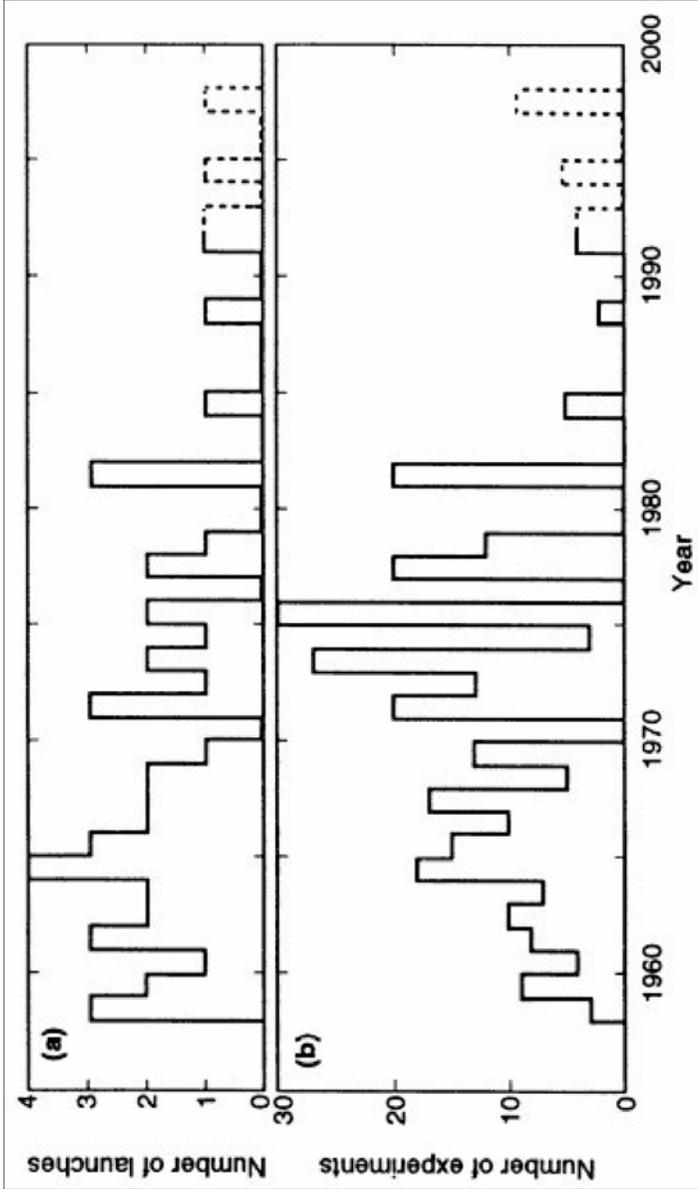


FIGURE 6.2 Space-physics-related Explorer launch history: number of launches and number of experiments.

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TABLE 6.1 Space-Physics-Related Explorer Launches and Experiments

Time Interval	No. of Launches	No. of Experiments
1958-1969	23	119
1970-1979	11	125
1980-1989	5	27

low in the 1980s that the number of experiments flown experienced a drastic reduction. Table 6.1 summarizes these results.

The preceding Figure and table show that experimental research opportunities for space physics within the NASA Explorer satellite program have become much more scarce since the 1970s.² This trend was recognized in the early 1980s and discussed in a previous National Research Council report [13] that argued strongly that NASA return to its earlier philosophy of making available to the scientific community more small and rapidly implemented satellites dedicated to focused scientific problems.

Implementation Times

The desire for rapid implementation discussed in the Explorer program report [13] also represents an important parameter in the conduct of space physics. Ideally, implementation should be on time scales that allow support of contemporary science questions, experimental research teams, graduate students, timely data analysis, and theoretical studies. The CSTR/CSSP has assembled a data base of space-physics-related launches in order to investigate the time required to implement these satellite projects. (The data base is described more fully in Appendix A.)

The implementation time is defined as the time from mission start to satellite launch. The start date is usually set at the date that investigators wrote proposals to place instruments on the spacecraft. This date was chosen because it represents a well-defined starting point that exists in some form for most missions. Where only the proposal year was known, July 1 was used as a start date. In other cases the start dates were obtained directly from the principal investigators. These different start date estimates may vary by a few months, but the variation is small compared to the implementation times obtained.

² The addition of non-Explorer NASA space physics-related launches does not change the trend shown by Figures 6.1 and 6.2. The total number of all launches and experiments has decreased since the 1970s.

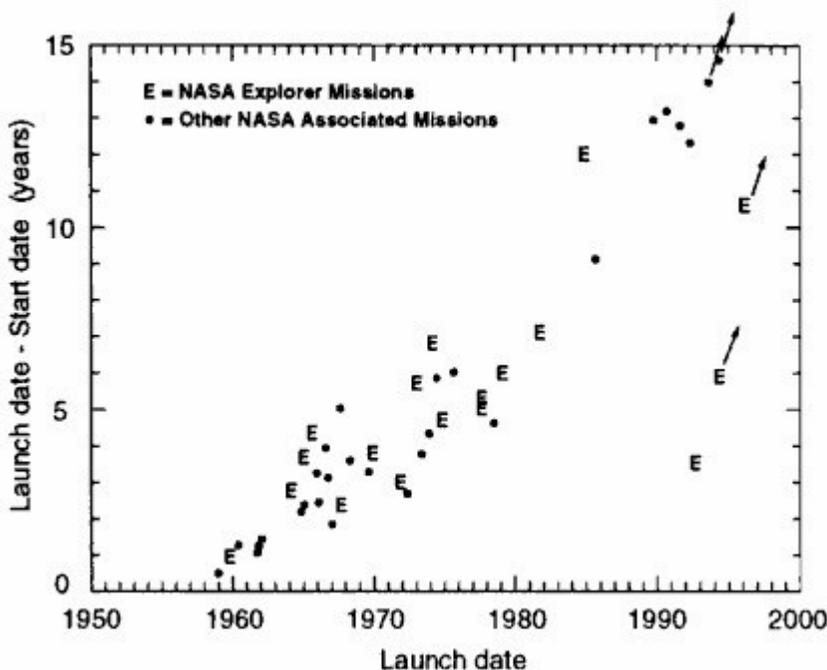


FIGURE 6.3 Implementation times for space-physics-related missions, 1958-2000.

Note: Launches for which reliable start dates could not be obtained are not included in this Figure.

Figure 6.3 shows the implementation times for the space-physics-related missions in the CSTR/CSSP data base. Arrows showing the effect of a one-year delay are given for projected launches of approved programs. Explorer missions and other NASA space-physics-related missions are identified separately. The Figure shows a striking increase in implementation times, from one to two years in the early 1960s to 10 to 12 years in the late 1980s. All mission types show a steady increase in implementation time.

As so eloquently argued by Freeman Dyson [14], this large an implementation time (over 10 years) represents "a terrible mismatch in time scale between science and space missions." It does not provide support for contemporary science questions, graduate students, instrument engineering staff, timely data analysis, theoretical studies, or stability of experimental research teams. Because of these and other ramifications (e.g., increased administration, management, planning activities, costs), large implementation times represent one of the leading reasons that space science researchers perceive that too much time is spent on activities other than research.

Increased Planning Activities

The start dates as defined above actually give only a lower limit for the implementation time. This is due to the extended preproposal planning activities that have become normal for most satellite missions. Consider, for example, the Global Geospace Sciences (GGS) element of the International Solar-Terrestrial Physics (ISTP) program.³ The two U.S. GGS satellites are scheduled for launch in 1994 and 1995 and are represented in [Figure 6.3](#) by implementation times (based on proposal submissions in 1980) in the 14- to 15-year range. However, formal planning for this mission actually began in 1977 when NASA established an ad hoc committee to define a program named the Origins of Plasmas in the Earth's Neighborhood, or OPEN. In parallel with this effort, the CSSP developed a research strategy in space physics for the 1980s [16]. The OPEN ad hoc committee issued its report in 1979 [15], describing a major NASA program that would pursue an important part of the research strategy developed by the CSSP [16]. NASA issued an Announcement of Opportunity in 1979, proposals were written in 1980, and experiments were selected in 1981. Following extensive policy and programmatic planning activities, the OPEN program evolved into the ISTP program, wherein the NASA contribution was sharply limited, and ESA, ISAS, and IKI agreed to provide major contributions. Formal approval of the ISTP program finally occurred in 1988. The net result was that not only were there an additional three years of planning activities prior to proposal submissions, but a large portion of the approximately 12- to 14-year implementation time shown in [Figure 6.3](#) was taken up by planning, policy, and political activities.

Such planning removes resources from the direct support of research, is often spent on missions that are not flown, and rarely reduces the cost of a mission. Indeed, it can be argued that excessive planning *increases* mission costs. [Appendix B](#) presents a detailed case study from the field of solar physics that illustrates these effects. It provides insight into how excessive planning and study activities arise and shows the potentially devastating effects to a research field and its relations with funding agencies.

Reliance on New-Start Approvals

The ISTP program illustrates another trend in space physics programs, namely, the increasing reliance on major programs that require new-start approval on a project-by-project basis by Congress. A recently completed NASA Strategic Plan [9] for space physics describes a research program through 2010 that is

³ The ISTP program is a multisatellite program being conducted by NASA, the Institute for Space and Astronautical Sciences (ISAS) of Japan, the European Space Agency (ESA), and the Institute for Space Research (IKI) of the former Soviet Union.

dominated by such projects. Although divided into major-, moderate-, and intermediate-class missions, all require new-start approval, and together with their mission operations and data analysis costs constitute 70 to 90 percent of the planned NASA space physics program through 2010.

One reason for this increasing reliance on major new-start programs has been the search for additional flight opportunities to compensate for the greatly reduced number of opportunities available to space physics through the Explorer program after 1980. A second incentive for this shift has been the emergence of scientific problems requiring experimental platforms of greater size and complexity than before. The net result of these effects is a space physics program that relies on big science projects to a much greater degree than in the past.

Partly because of budgetary and community pressures, NASA has recently begun to rejuvenate the Explorer program. The Small Explorer Program (SMEX) was initiated with the successful launch of the Solar Anomalous and Magnetospheric Particle Explorer (SAMPEX) mission. Two additional SMEX missions are being built, and NASA expects to continue selecting and launching these missions on a regular basis. Further plans, not yet funded, call for university-class and medium-sized explorers; it is too early to predict the future of these plans. An expanded Explorer program would be a positive and welcome step toward alleviating the serious problem of infrequent access to space.

SOLAR OBSERVATIONS

Solar physics presents a unique opportunity to analyze trends in the implementation of its scientific requirements. It requires large-scale facilities both for its space-based and its ground-based observatories. Although the evolution of solar satellites and instrumentation has moved toward larger, more complex and costly systems, as described in the previous section, this is not true for the ground-based observatories. Large solar observatory projects conducted over 30 years ago were major undertakings fully comparable to present programs. For this reason the comparison of solar satellite and ground-based implementation trends may provide a measure of the relative contributions of technical (e.g., size, complexity) and nontechnical (e.g., administration, management, funding procedures) factors to increasing implementation times.

Implementation of Solar Satellite Missions

This section presents data on the prelaunch duration (implementation phase) of solar physics space missions in order to study how long scientists prepare for space missions, whether prelaunch duration shows a secular trend, and, if so, what the underlying causes might be.

Only missions primarily devoted to the study of the Sun are considered. Although this criterion excludes some missions that carried solar instruments,

most of those missions are included in the preceding section on satellite observations. We do, however, include the Ulysses mission, which, in its original concept, consisted of two spacecraft, one of which carried an instrument for imaging the solar corona from above the solar poles. More than three years after the release of the Announcement of Opportunity for the mission, the U.S. spacecraft was canceled, leaving only the European spacecraft with its instruments for studying solar fields and particles intact but with no capability to image their solar sources.

The prelaunch phase is taken to begin with an Announcement of Opportunity (AO) that solicits proposals for scientific instruments to be carried on the satellite. This is by no means a general characterization of prelaunch activities; it is simply an attempt to place a lower bound on the period during which a space researcher must devote a substantial fraction of his or her professional effort to a particular space-borne experiment. The AO is usually preceded by a period of study and planning in which scientists are heavily involved. Thus, the prelaunch phase as defined here typically underestimates the period of involvement for selected experimenters, sometimes by years.

The actual AO date was used in most cases. (For some of the Orbiting Solar Observatory (OSO) missions, the AO date was estimated based on other related dates.) The instruments on the Skylab Apollo Telescope Mount (ATM) were originally proposed for the canceled Advanced Orbiting Solar Observatory (AOSO), so the prelaunch phase is taken to begin with the AOSO opportunity. The Orbiting Solar Laboratory (OSL) is discussed extensively in [Appendix B](#), but for the present purpose its origin is taken to be the AO for the Solar Optical Telescope issued in April 1980. The launch date is estimated by assuming that a new start for OSL will not occur before 1997. In the case of missions primarily sponsored by ESA (Ulysses, Solar and Heliospheric Observatory [SOHO]) or the Japanese space agency (Yohkoh), the prelaunch phase is dated from the NASA AO for participation by U.S. scientists.

The results are shown in [Figure 6.4](#). For missions yet to be launched, arrows show the effect of a one-year delay. The early OSO satellites were part of a series, with multiyear funding and a spacecraft design that remained relatively stable. At the other end of the spectrum is the OSL. If this mission eventually flies, it will be both the most delayed and the most ambitious solar physics mission ever, by a wide margin (see [Appendix B](#)). Over the entire period, 1970-1992, the trend is consistent with that shown in [Figure 6.3](#) for space physics satellite missions in general. The rise in implementation time has been from a few years in the early 1960s to the present value of over 10 years.

Of course, the duration of the prelaunch phase should really be considered in relation to the overall scope and cost of each mission. We do not attempt to include that level of detail here. We can, however, identify one characteristic that is common to several of the missions with relatively long prelaunch phases: a midcourse change in the scope or conception of the mission itself. As men

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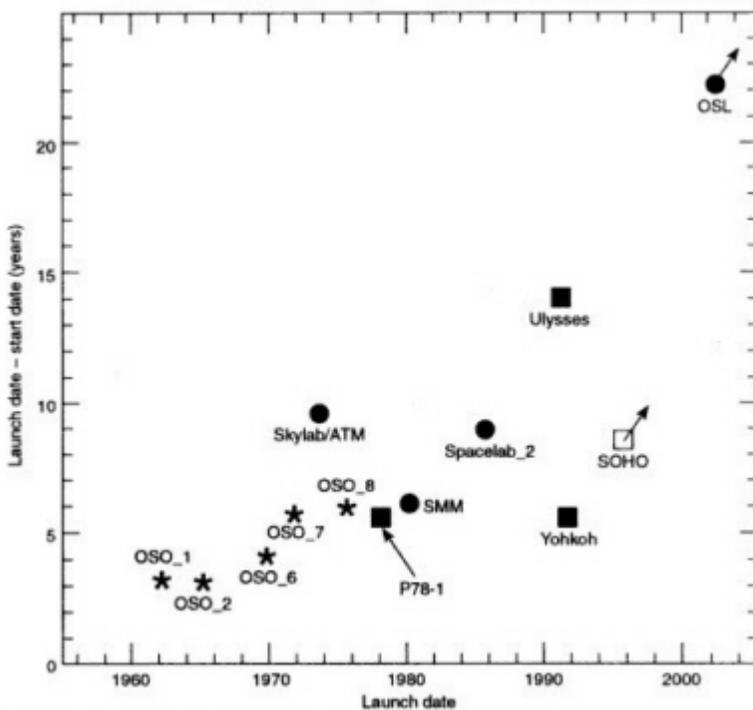


FIGURE 6.4 Implementation times for solar physics missions, 1962-2005.

tioned above, Skylab/ATM evolved from the canceled AOSO mission. The character of NASA's participation in Ulysses changed drastically after the AO was issued. In the case of OSL, the AO was issued in anticipation that the mission would be approved; if it is ever approved, over 15 years will have elapsed since the AO date. Experimenters do not play a passive role when missions are redefined or rescheduled. To maintain their participation in the mission and, perhaps, to help ensure that the mission flies at all, they may invest as much professional effort as they would have had the mission proceeded on the original schedule.

Because of the way NASA missions are funded, inefficiencies and delays connected with changes in mission concept are linked to the overall cost of the

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mission. Large missions require funding over at least five years. When this funding must receive specific congressional approval each year, the program is subject to unpredictable budget fluctuations that, in turn, necessitate continuously evolving plans to accommodate different fiscal scenarios. This shifting-ground effect is even more pronounced when the mission as a whole has not yet been assured new-start approval. Although the political process should ensure responsible government control over large public expenditures, it often has unintended negative consequences; delay and inconsistency lead to inefficient use of human and financial resources.

Solar Ground-Based Observatories

This section presents data on the proposal, design, and construction phases of ground-based solar telescopes in order to identify trends and relate them to project cost. Only national or international facilities costing at least \$4 million (1991 dollars) are considered. This makes for a homogeneous sample but excludes several major university observatories.

The beginning of a project is taken to be the date of the proposal or, if it can be clearly identified, the date of a study or site survey that led directly to the proposal. In analogy to the launch of a space mission, the end of the project is taken to be "first light," even though significant testing and improvement usually occur for some time after that.⁴

Costs were converted to 1991 dollars according to the NASA (Code BA) new-start inflation index to reflect the rates of inflation that characterize the technical sector. The cost for the Large Earth-based Solar Telescope (LEST) is taken to be the U.S. share, one-third of the total.

Summary time lines for these observatories are shown in [Figure 6.5](#). The projects divide naturally into an earlier group of three major telescopes and two ongoing projects, the Global Oscillation Network Group (GONG) and LEST.

[Figure 6.5](#) indicates that it takes longer to plan and execute major ground-based projects than it did 20 to 30 years ago. The comparison can be made more directly for these ground-based projects than for space missions because there has not been the same striking evolution in the complexity, scope, and cost of ground-based efforts. The McMath Telescope project, executed in less than five years, was a major undertaking fully comparable to current programs; the McMath and LEST were each designed to be the world's largest solar telescope, and the McMath still is.

⁴ Dated proposals and extensive chronological documents were available for the McMath Telescope, the Global Oscillation Network Group (GONG), and the Large Earth-based Solar Telescope (LEST). Estimates from project principals were used for the Sacramento Peak Tower Telescope and the Kitt Peak Vacuum Telescope. The completion dates for GONG and LEST are estimates.

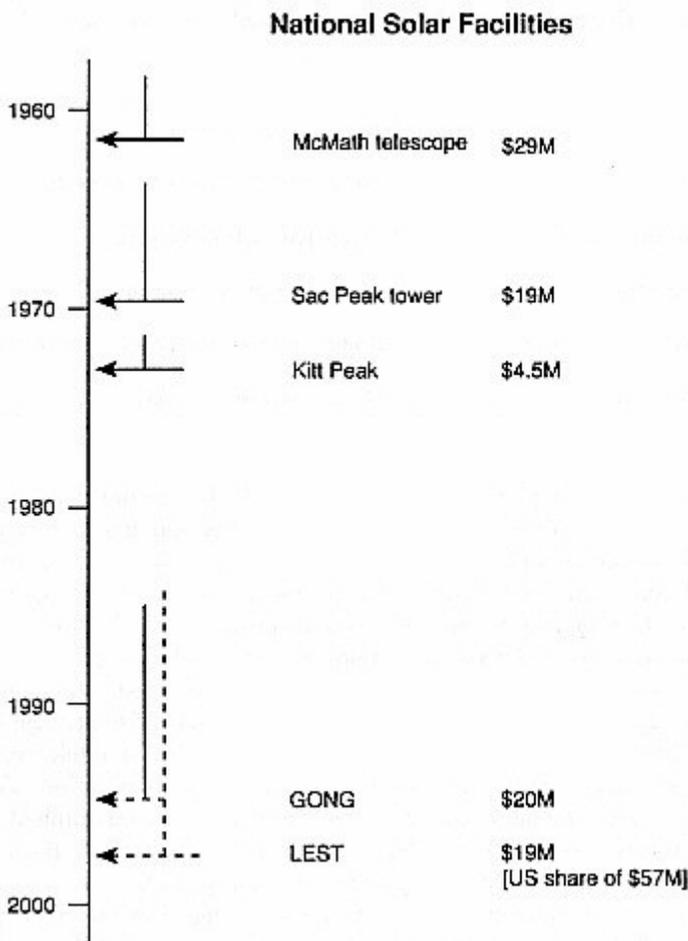


FIGURE 6.5 Time line showing implementation times for a number of ground-based solar facilities, 1957-1999. (Bars indicate development and construction time; arrows indicate first light; all costs are given in FY 1991 dollars.)

Table 6.2 compares some of the characteristics of the McMath Telescope and the LEST project. Figure 6.6 compares their project time lines in more detail. Although the total projected cost of the LEST project is twice as large as the cost of the McMath Telescope, the cost to the U.S. funding agency is smaller for LEST; this is part of the rationale behind international consortia. LEST may also entail a somewhat greater construction task; however, the LEST project *before groundbreaking* has already taken twice as long as the entire McMath project.

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TABLE 6.2 Comparison of Two Ground-Based Solar Telescope Projects

McMath	LEST
U.S. only	International consortium
\$29M cost	\$19M cost (U.S. share of \$57M total)
Single proposal (11 pages)	Multiple proposals (>> 500 pages)
Single funding transfer	Multistage, multisource funding
Informal scientific oversight	Scientific and technical advisory committees
Five-year duration	<14-year duration (in progress)

Although the international character of LEST has complicated its overall coordination and funding, increased effort in the proposal and advocacy phases can also be identified within each participating country. Table 6.2 shows that the period of active involvement for participating scientists (writing the proposal, advocating the project to funding agencies, and participating in or responding to oversight committees) plays a much more prominent role in LEST.

Probably the single most important factor behind increasing implementation times for major ground-based solar projects is the advent of multistage funding. The McMath, Sacramento Peak Tower, and Kitt Peak Vacuum telescopes were funded with a single transfer of money to the managing organization. From that point the progress of the design and construction phases was limited only by technical issues or practical considerations internal to the project. Basically, the telescopes were built as fast as they could be soundly built.

Figures 6.7 and 6.8 illustrate the effect of multistage funding on the progress of the GONG. Even though the overall project budget and timetable were judged reasonable when the project was approved, in none of the first five years did the actual funding reach the proposed profile. After three years, funds were short by a factor of two. The effect of this mismatch in resources will be a delay of at least three years and an increase of more than 30 percent in total (constant-dollar) cost.

As discussed above, funding decisions are part of a broader, often political, process with many competing demands. However, it is important for decision-makers and the public to understand the true costs, in dollars and morale, of these kinds of project delays and midcourse changes in funding.

Figure 6.3 showed that implementation times for solar satellite programs have increased from two to three years in the early 1960s to the present value of well over 10 years. In the solar ground observatory program, where there has been a much less dramatic evolution in complexity than in the satellite program,

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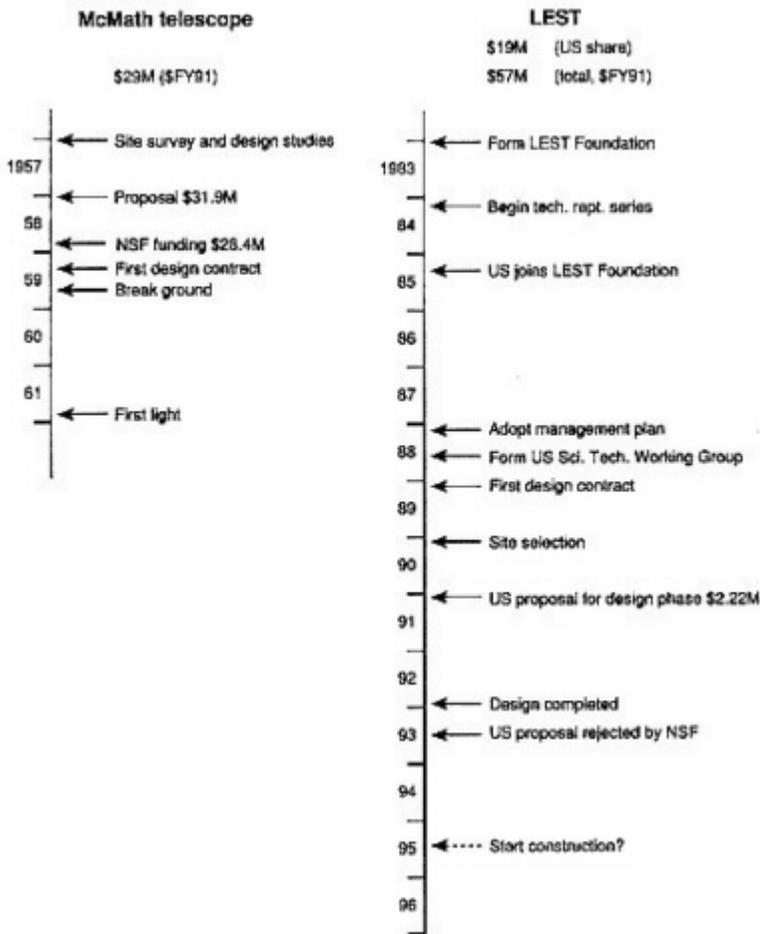


FIGURE 6.6 Detailed time lines on same scale for the McMath telescope and the LEST program.

we also see a major increase in implementation times. Much of this has been due to changing managerial and funding procedures. Much more time is spent in study, planning, selling, and oversight activities, all of which add to the final cost. Funding is apportioned on an incremental basis that usually falls short of planning expectations.

This comparison indicates that administrative procedures have had at least

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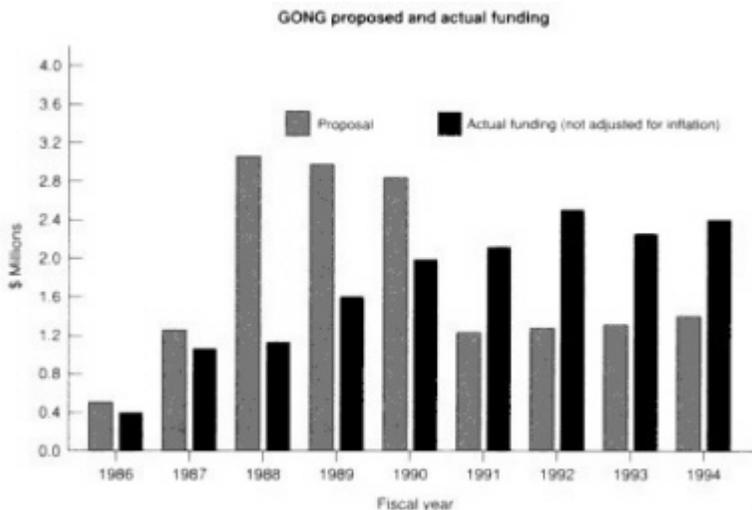


FIGURE 6.7 Budget history for the GONG program. (Estimated total cost \$20M in FY 1991 dollars.)

as large an impact on increasing implementation times as have the elements of project size and technical complexity.

ROCKET OBSERVATIONS

The NASA suborbital program supports scientific experiments carried out on airplanes, balloons, and sounding rockets. The experiments come from disciplines in astrophysics, earth sciences, microgravity research, solar physics, and space plasma physics. This section is limited to discussion of sounding rockets in space physics research.

Sounding rockets provide unique capabilities not easily attained by other means. For example, sounding rockets are the only vehicles that can launch payloads to observe space phenomena from unique geographic locations, altitudes, and times. Thus, sounding rocket experiments can accomplish specific scientific goals and have been especially valuable in obtaining information on small-scale and rapid temporal features that are difficult to obtain from rapidly moving spacecraft.

NASA currently offers 15 configurations of sounding rockets to provide the scientific community with different capabilities. Scientific requirements and the payload weight dictate which rocket to use. The average weight of sounding rocket payloads has been growing steadily since 1960, as shown in Figure 6.9.

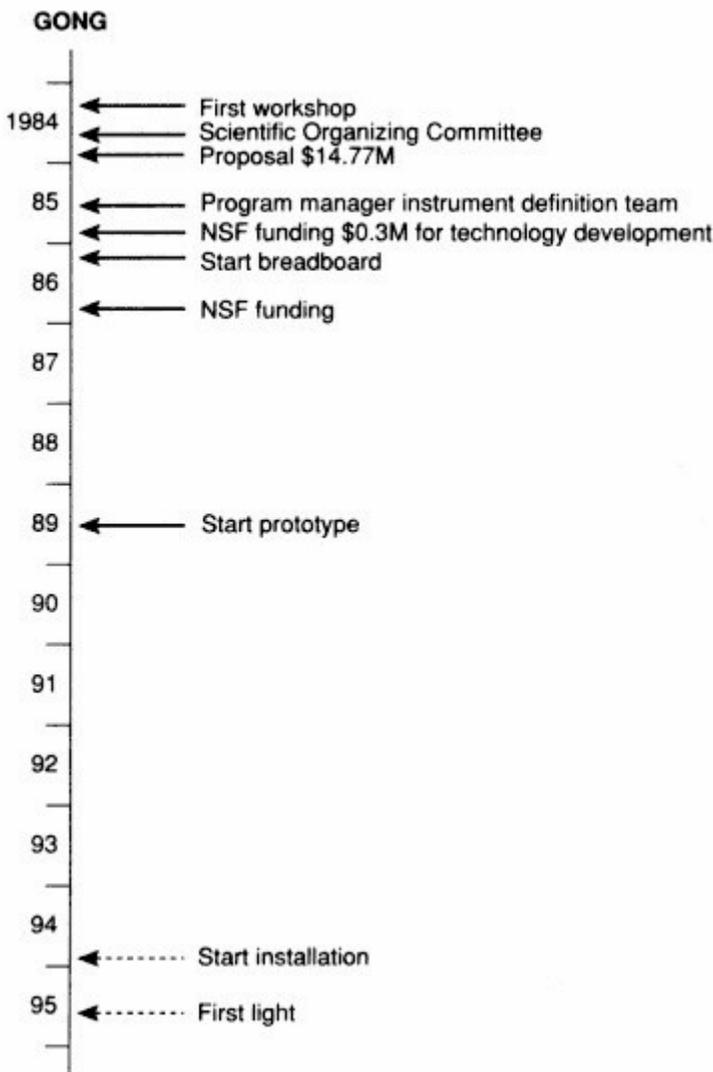


FIGURE 6.8 Time line for the GONG program.

Sounding rocket payload weight has more than quadrupled in the last 20 years, from about 125 pounds in 1970 to 600 pounds in 1990. This increase has come about as the scientific fields matured and experiments became more demanding and complex. It has also had a direct impact on the number and types of rockets launched. Figure 6.10 shows that the total number of rocket launches has de-

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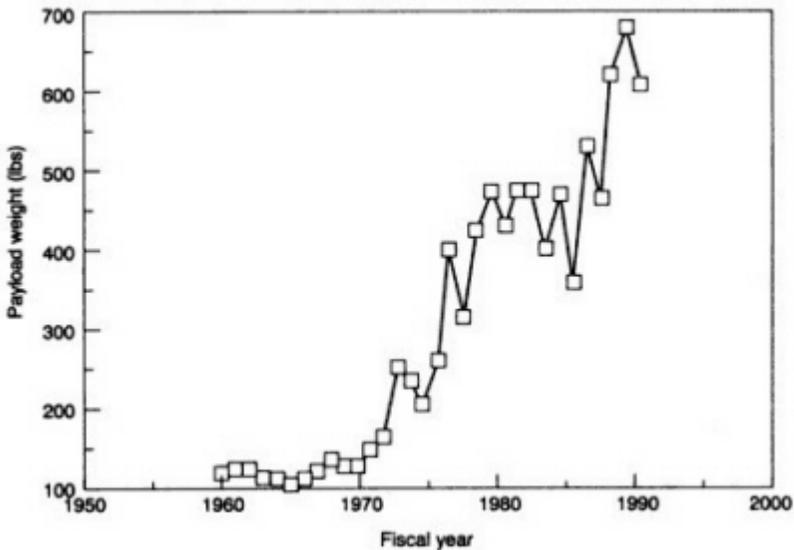


FIGURE 6.9 NASA rocket payload masses, 1960-1991.

creased over the past decade. However, the percentage of large rockets included in those launches has increased markedly, from 24 percent in 1984 to 64 percent in 1991.

Sounding rocket grants are awarded for a three-year period to cover the experimenter's expenses for fabrication, launch, and data analysis. The funding level per experiment and the number of experiments supported by the Space Physics Division were fairly steady through the 1980s (Figure 6.11). Note the change that occurred in 1990: the funding level per grant was reduced from the 1989 average of \$200,000 to \$150,000. This occurred because the total number of grants supported increased from 29 to 41, without an increase in the overall funding level.

NASA's suborbital program serves an extremely useful function to the scientific community. It provides the opportunity for (1) research groups in industry, university, and government laboratories to develop space-borne instruments for orbital missions; (2) university research groups to train graduate students in experimental methods in space physics; and (3) research groups to conduct inexpensive research programs that continue to yield original results. About 261 papers were published in refereed journals based on results obtained from sounding rocket experiments during the period 1986-1990.

The overall NASA funding for rocket sciences increased by approximately 20 percent (in constant-year dollars) from 1979 to 1991. The number of rocket

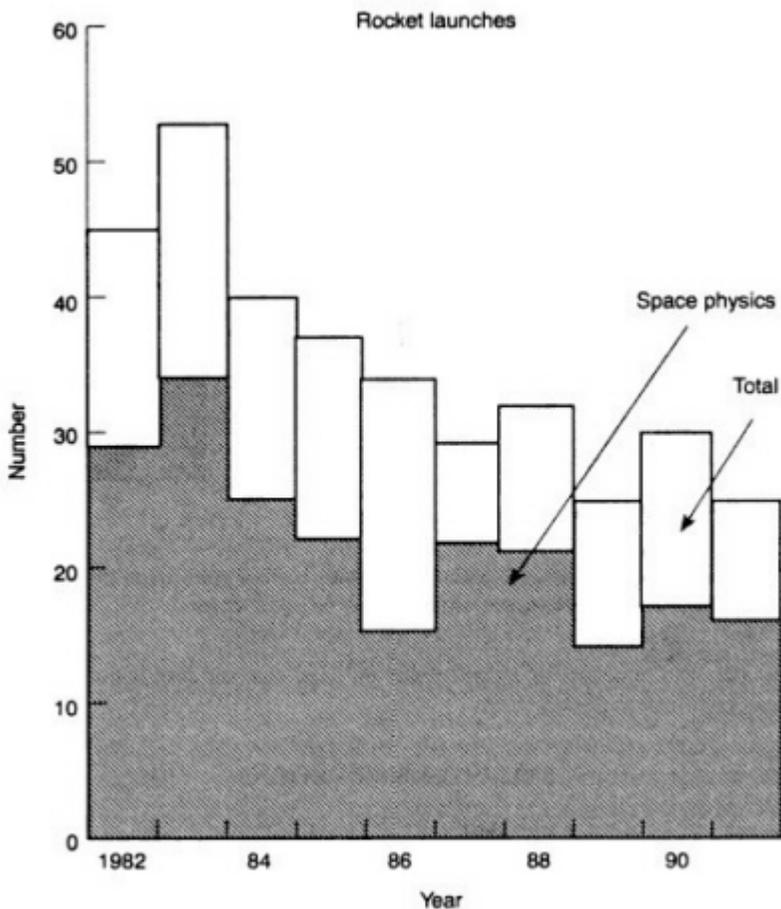


FIGURE 6.10 Number of NASA rocket launches, total and for space physics, 1982-1991.

launches decreased by approximately 45 percent, but payload weight increased substantially. The demand for sounding rocket support has been increasing steadily since the mid-1970s due to decreased opportunities in orbital missions. Recently the support level per experiment in solar and space physics has decreased substantially in order to fund more of these projects. Increasing university overheads (described in [Chapter 5](#)) further decrease the funds directly available to the rocket researcher. Finally, rocket experiments continue to be managed

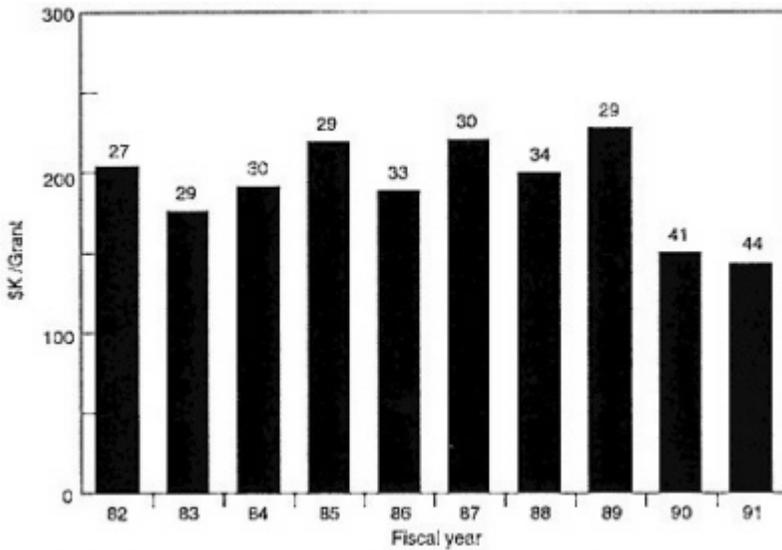


FIGURE 6.11 NASA space physics rocket program: average grant size in 1991 dollars (solid bars) and number of grants funded (numbers on top of the bars).

by individual principal investigators and on fixed cost and time schedules despite their increasing complexity.

BALLOON OBSERVATIONS

NASA's balloon program has historically provided a mechanism to obtain exposures to energetic particle radiation. When high-altitude balloon technology was developed in the late 1940s, balloons were used to discover the major constituents of the steady flux of cosmic rays bombarding the Earth. Many of the scientists who went on to play a major role in NASA's flight programs learned how to operate a payload in the harsh space environment by doing thesis or postdoctoral research using a balloon-borne payload.

This section draws on data about scientific ballooning as a whole, and on a small sample from a very specific and narrow discipline area, namely studies of high-energy cosmic rays. The trends in the two data sets are consistent. Three cosmic-ray groups provided information about flight rates and implementation times: the California Institute of Technology, University of Chicago, and Goddard Space Flight Center. Figure 6.12 shows the number of NASA balloon flights per year. The trend toward decreasing flight frequency is very clear. Figure 6.13 shows the trend toward increasing payload complexity, as reflected

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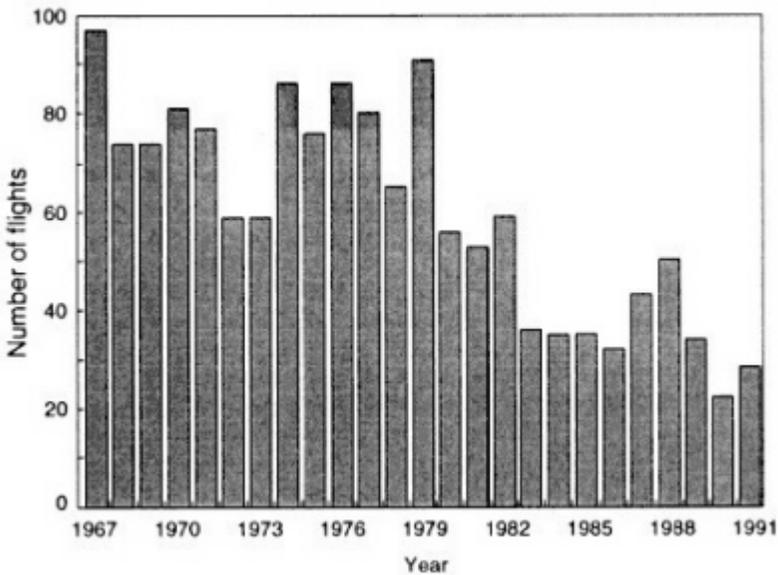


FIGURE 6.12 NASA balloon flights, 1967-1991.

by payload mass, during the same time period. Although payload weight increased, the reliability has remained relatively constant (Harvey Needleman, Balloon Projects Branch, Wallops Flight Center, personal communication, 1993), except for periods of specific difficulty in the late 1970s discussed further below.

Based on limited information from the cosmic-ray groups, [Figure 6.14](#) shows the trends in implementation time (i.e., conception to first successful flight). Understanding these data requires some guesswork based on knowledge of the history involved. In the 1960s there was a major expedition each summer to Ft. Churchill, Manitoba, known as the Skyhook program, through which most of the balloons, all with relatively small payloads, were flown. In the 1970s, balloons that could reliably lift a few thousand pounds came into being, bigger experiments were possible, and the flight frequencies declined. This growth in balloons and payloads finally ran into technology limits in the last half of the 1970s. Difficulties with balloon materials and flight reliability began to manifest themselves and were not resolved until the mid-1980s. Meanwhile, the size and complexity of payloads steadily increased; it now can take several years of funding to build and fly a complex balloon payload. Even with these factors, the time from conception to successful flight recovered in the late 1980s and is as good now as it was in the late 1960s and early 1970s—a testimonial to the

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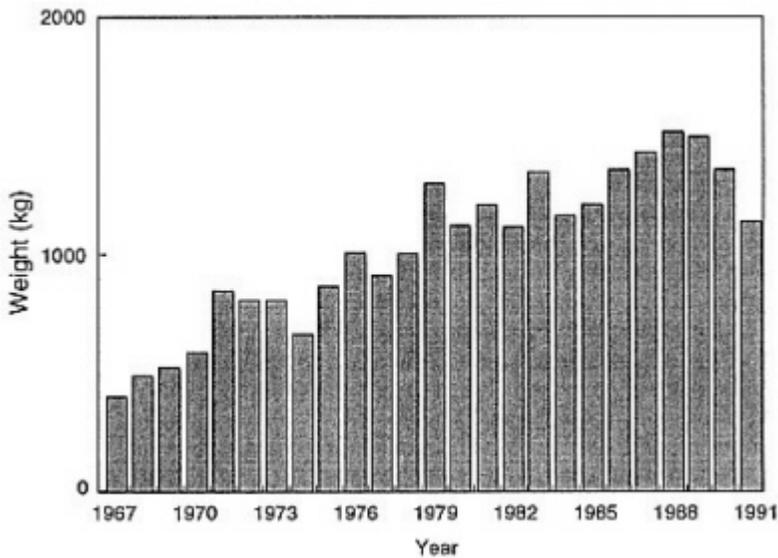


FIGURE 6.13 NASA balloon payload weight, 1967-1991.

attention paid to balloon problems by NASA. This is a success story that has led to demands for more and longer-duration flights.

Data on total funding and funding per grant for cosmic ray balloon payloads are shown in Figures 6.15a and b. (Unfortunately, these data only go back to 1979, and further historical data were difficult to obtain.) The number of grants and the number of projects are shown in Figure 6.16. The increase in payload complexity has led to multiinstitution (and hence multigrant) collaboration on individual projects.

The data support the recollections of some researchers that in the 1950s and 1960s one could rely on steady funding as long as meaningful results were forthcoming. Programs proposed were multiyear ones and began returning results within a year or two of conception. In the 1970s the time taken to accomplish research seemed to increase: payloads became bigger and more complex, taking longer to build and fly. The late 1970s and early 1980s were a period of poor balloon reliability, when a failure usually meant a year's delay. Following the technical problems of the early 1980s and the corresponding dip in funding, the number of groups flying balloons, as well as the funding, began to increase again.

Ballooning is a viable method for conducting galactic cosmic-ray studies. The long-duration balloon capability currently emerging has promise for the

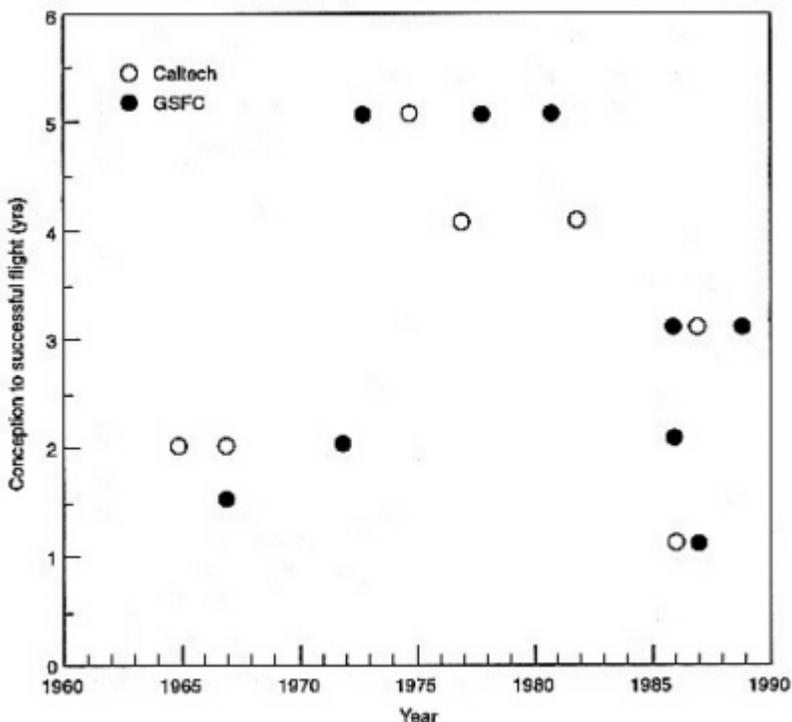


FIGURE 6.14 Sample balloon implementation times for cosmic-ray payloads, 1964-1990.

1990s. The trend toward increasing payload complexity has generally been absorbed by the space physics community without loss of reliability, with the exception that occasional balloon reliability problems arise and must be solved. The increase in complexity has not itself given rise to an increase in implementation time for the balloon programs surveyed.

There has been a trend toward increasing numbers of small grants, mitigated in part by an increase in multiinstitution (and hence multigrant) collaborations. These collaborations have been managed by allowing several institutions to prepare a single proposal, accompanied by separate institutional endorsements.

The time scales for ballooning have remained compatible with the education of students, especially those familiar with space hardware. This program is, relatively speaking, a success story and may contain lessons for others.

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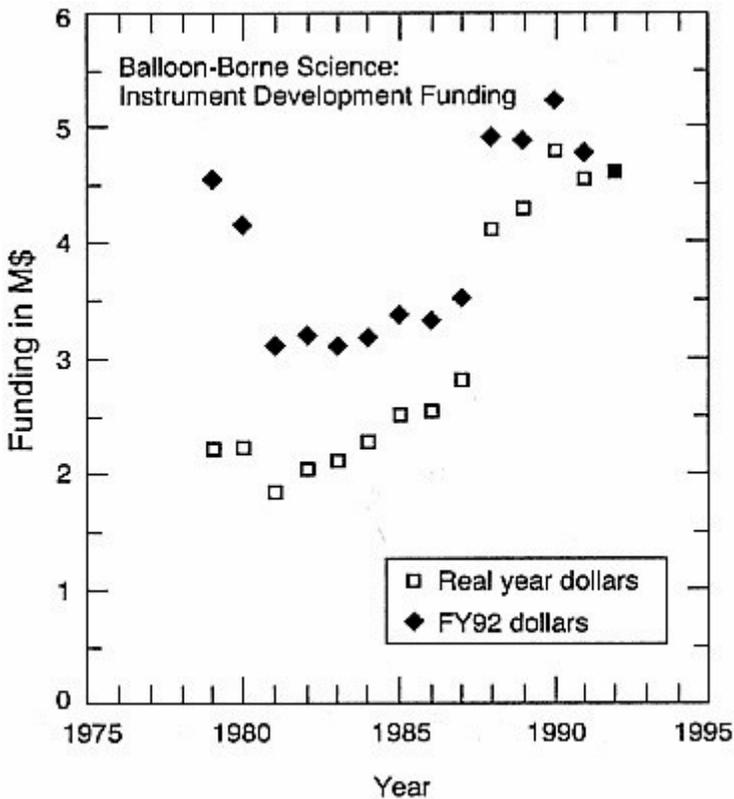


FIGURE 6.15a Total NASA funding for cosmic-ray balloon payloads. Source: Data provided by NASA's Cosmic and Heliospheric Physics Branch.

THEORY

Progress in science results from the interaction between its theoretical and experimental (or observational) branches. This interdependence holds as much for space physics as it does for the rest of science. About a third of all space physicists consider themselves to be theorists or modelers, according to one survey (see Chapter 4, Figure 4.6). This percentage is larger for younger physicists (40 percent for those under age 40). Both aspects of space physics science have evolved over the past few decades. As discussed earlier in this chapter and in Chapter 2, *experimental* space physics has been getting steadily "bigger," starting in the early 1950s with sounding rockets and progressing to larger and more expensive satellite programs. The way in which *theoretical work* is carried out also has changed over the years.

Traditionally, theoretical work has been carried out at an individual level or

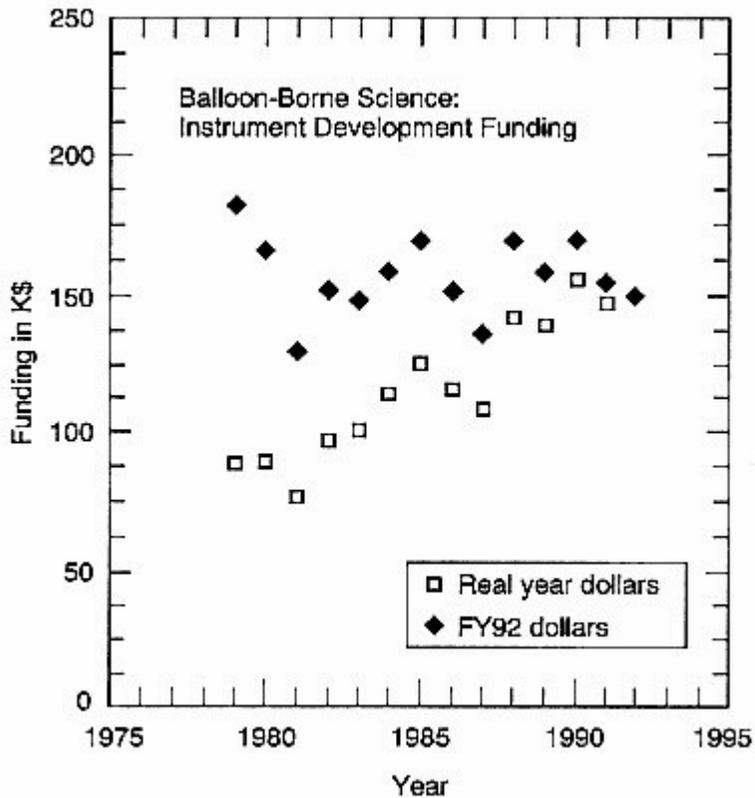


FIGURE 6.15b Average NASA funding per grant for cosmic-ray balloon payloads.

in very small groups. Most support for theoretical work comes from small, short-term grants from NSF and from NASA's Supporting Research and Technology (SR&T) program. Consider the theory and modeling component of NASA's Space Physics Division SR&T funding for FY 1991 (excluding the balloon and suborbital programs). The total number of SR&T grants was 288, of which 140 (or 48.6 percent) were for theory and modeling. The total division SR&T budget was \$17.046 million, of which theory and modeling accounted for \$7.614 million (or 44.7 percent). (This does not include the Space Physics Theory program, which is discussed below.) Thus, the average SR&T grant size was \$59,000 for the division as a whole, and \$54,400 for theory and modeling. This average grant size has remained the same for about a decade, thereby falling significantly behind the typical 5 percent rate of inflation. Proposal pressure on the SR&T program was high at the end of FY 1991. For example, in the solar

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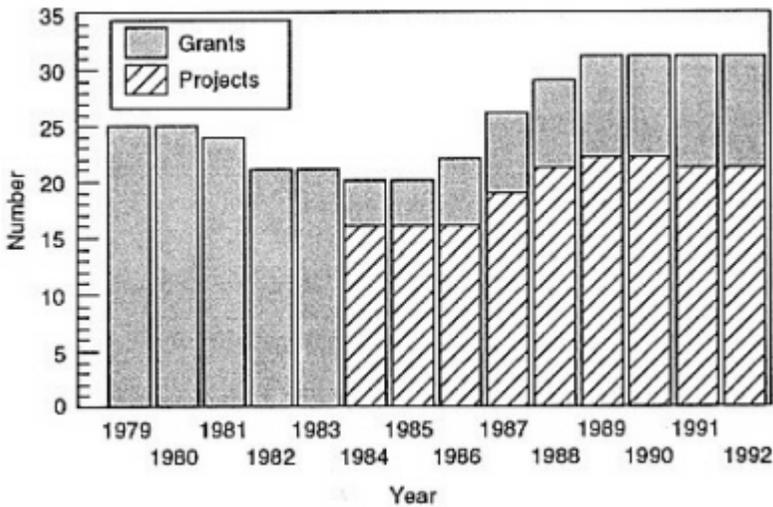


FIGURE 6.16 NASA cosmic-ray balloon grants and projects, 1979-1992.

branch, \$2.5 million was available for new grants for FY 1992, but proposals totaling \$11 million were submitted.

There has been some tendency for theoretical work to be put into larger packages. Part of this is associated with advances in numerical simulations of space plasma phenomena, which require a larger infrastructure than traditional theory. One example of this is NASA's Space Physics Theory program (previously the Solar Terrestrial Theory program). One of the purposes of this program is to assemble "critical masses" of theorists to work on certain key problem areas. A large number of these groups emphasize numerical plasma simulations. Currently, there are 17 such groups, each with several senior scientists, several junior scientists, and graduate students. The total FY 1992 funding is \$4.3 million. The program started in FY 1980 with 13 groups and \$2.27 million. The overall program grew at a rate of 5.5 percent per year, thus approximately keeping pace with inflation, but the rate of increase per group was only 3.1 percent. These groups have been very productive and important for space physics, but most theoretical work still takes place elsewhere.

Some theoretical work has also been supported by large NASA missions under various guises: (1) as theorists included on instrumental proposals as coinvestigators and (2) as interdisciplinary scientists. The NSF also supports theory and modeling via relatively small grants, comparable in size to NASA's. However, over the past few years "new" money at NSF has gone not to the "base" program but to new initiatives, such as the magnetospheric GEM pro-

gram and the upper-atmosphere CEDAR program. Both GEM and CEDAR have significant theoretical and modeling components but are "managed" programs, in the big science mold.

Numerical simulation of space plasma phenomena using computers has become an important theoretical method over the past two decades. Examples include particle-in-cell simulations, hybrid simulations, and three-dimensional magnetohydrodynamic modeling. Numerical models have increased in size and complexity over time, taking advantage of technological developments in computers, such as the CRAY-YMP and other supercomputers. Large computational facilities have been created, such as the four NSF supercomputer centers, the National Center for Atmospheric Research's Scientific Computing Division, and government laboratories such as NASA's Ames and Goddard centers and the Los Alamos and Livermore national laboratories. The largest space (and nonspace) plasma simulations have been run at the national laboratories, where large blocks of computer time are available. In this sense, too, also theory has become "big" science.

Some relatively large groups have grown to support the development and running of these plasma simulations and models, but overall these groups are still smaller than the large experimental teams that are put together to design, build, and use space-based instruments. In fact, even large modeling efforts such as 3-D magnetohydrodynamic simulations are frequently undertaken by only one or two senior scientists. A recent trend running counter to the dependence on supercomputers is the increasing use of powerful workstations that permit all but the largest simulations to be run locally.

Theory is an important part of space physics, and numerical plasma simulations have played an increasingly important role in this field. The emphasis on simulation has resulted in some increase in the scale of operations for theory over the past couple of decades, but theoretical space plasma physics seems to have found a balance between big and little science.

The chief difficulty for theory and modeling lies in the small grant sizes (\$54,000 on average), which are not keeping up with the rate of inflation, as well as the decreasing probability of a proposal getting funded and rising overhead costs. As discussed more generally in [Chapter 5](#), these factors lower the effectiveness of the Space Physics Theory and Modeling program.

DATA ANALYSIS

The reason for flying scientific space missions is to obtain and analyze new data. NASA currently funds mission operations and data analysis (MO&DA) efforts separately from spacecraft development, in part to protect the postlaunch funds from being used to solve development problems or overruns. This effort at protection has not always been successful, however.

There are several requirements for ensuring appropriate data analysis: (1) adequate funding of the principal investigators (PIs) responsible for instrument development and operation and the subsequent reduction and analysis of the data; (2) adequate funding and the commitment to enforce documentation and archiving requirements so that data are available in a format that can be used by scientists other than the original PIs; and (3) broadening the base of researchers who know of, understand, and can use the data. The first two requirements are self-explanatory. This section addresses the third requirement, especially as it relates to the interplay of big and little science.

There has been a recent trend toward broadening the base of researchers working with space data through the use of interdisciplinary scientists and guest investigators. This trend is driven by the increasing breadth of many of the space physics missions and by the abundant correlative data available from both ground-based observations and spacecraft operating in different locations in the solar-terrestrial system at the same time.

One space mission that was particularly successful in attracting the participation of many researchers was NASA's Solar Maximum Mission (SMM). Launched in 1980, SMM was an Earth-orbiting satellite that carried six instruments for studying the Sun. For its time, SMM could be considered a moderate-cost space mission (prelaunch cost of \$125 million in FY 1979 dollars). In addition to the six principal investigators and their teams, about 25 Guest Investigators per year were funded, at an average of \$34,000 per investigation, for a total cost of roughly \$6 million over seven years. About twice that number of investigators obtained SMM data for their own research, either in person or over the phone. SMM also provided modest support for correlative ground-based observations of the Sun. Summaries of selected SMM data were published in broad-circulation periodical data reports to make those outside the program aware of the data resource. In addition, the SMM project sponsored a series of workshops that focused on SMM data and were open to all interested investigators. By 1989 the data obtained by SMM had led to over 700 scientific publications. Over 75 percent of U.S. solar physicists and a large number of non-U.S. researchers have participated in SMM in one way or another.

SMM was not the only successful guest investigator program in space physics. Over the period 1984-1987, for example, NASA's Dynamics Explorer (DE) guest investigator program funded 39 investigations at a total cost of \$1.45 million, resulting in roughly two new publications per investigator per year.

In 1991, NASA's Space Physics Division initiated a plan for the archiving and analysis of data from six Explorer missions. The plan had three elements:

1. documentation and archiving of Explorer data at various investigator facilities and at the National Space Science Data Center to make the data accessible to the scientific community.

2. phase-out of direct funding for data analysis by the Explorer principal investigator teams, and
3. initiation of a guest investigator program to which all interested scientists could apply for continued study of these valuable data bases.

The first two elements above were implemented quickly. As for the third element, 41 guest investigators were supported in 1991, but the funding for continuation of this important program is highly uncertain.

The space physics community is united in its support of vigorous guest investigator programs for many reasons: (1) they are very cost effective, (2) they bring different talents and interests to the analysis of costly space data, (3) they provide the PIs and their teams with new collaborative capabilities and opportunities, (4) they help bridge the gap between major missions, and (5) they provide a natural way to encourage the interplay between big and little science that enriches space physics research.

Sometimes individual investigators or small teams find it advantageous to coordinate their data analyses in order to achieve their respective research objectives. As an example, it has been suggested that the time is ripe to undertake a retrospective analysis of existing and complementary sets of space-based and ground-based data dealing with magnetospheric substorms. The relevant data have now been archived, and the theoretical models of substorms have changed considerably since the individual data sets were first analyzed. Another example is the Coordinated Heliospheric Observations (COHO) program, which seeks to support theory, modeling, data analysis, and guest investigations aimed at understanding the heliosphere and its boundaries through coordinated data obtained from spacecraft now widely distributed throughout the heliosphere. The COHO initiative was approved in NASA's Space Physics Division program for FY 1993, but no funds were appropriated to support it.

Data analysis is but one example of the strong synergism that can exist between big and little science. It shows, as discussed in [Chapter 2](#), that both are essential for the advancement of space physics. Furthermore, large and small efforts must be carefully balanced and coordinated to optimize the scientific return.

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7

Conclusions and Recommendations

In this chapter we attempt to unravel the space physics paradox by reviewing the findings from our earlier chapters and revisiting the big science/little science controversy. We have assembled our findings in a manner that naturally leads to four major conclusions and four important recommendations that address the essence of the space physics paradox. We have relied more on the trends derived from our data than on absolute values for any given epoch. Our conclusions therefore reflect these trends, and our recommendations seek to change them—a change that can be made with no increase in overall funding for space physics.

Other reports have touched on these matters but in different contexts. For example, the Lederman report [1] delivers a disturbing anecdotal survey on the health of university research. The National Aeronautics and Space Administration (NASA) Advisory Council report [3] recognizes a decreasing number of research opportunities, increasing time scales for research projects, and the scientific community's need for a variety of research opportunities of different sizes. The Committee on Space Policy [4], in its recommendations to then-president-elect George Bush, discusses the need for a balanced program consisting of a stably funded base program supplemented by large, long-term projects. Such general reports complement the more specific case presented in this report for the field of space physics.

THE REALITY BEHIND THE PARADOX

Before discussing our conclusions, let us reexamine the paradox itself. On the basis of individual case studies, the data base assembled for this report, and

anecdotal evidence from colleagues, the committee concludes that the *problem* underlying this paradox is real—that despite substantial funding increases in space physics over the past 15 years, the conduct of research has become less effective, leading to increased levels of dissatisfaction in the research community. However, by asking questions about *where* the money has gone, *why* inefficiencies have developed, and *who* is feeling the dissatisfaction most keenly, this seemingly paradoxical situation can be explained.

Chapter 3 showed that overall research funding, as well as funding for space physics research, have increased at a pace well beyond inflation and now represent a larger share of the gross national product than they did 15 years ago. Furthermore, Chapter 4 showed that the size of the space physics community has grown at a similar rate. On the other hand, Chapter 5 established that in the core program the percentage of proposals funded and the funding per grant have generally decreased over this time, concluding that the base program has not kept pace with either the increasing size of the field or the general funding increase. Thus, even though total funding has increased at a rate similar to the growth of the research community, individual "small science" researchers must now write significantly more proposals to support their work than they did a decade ago. Chapter 5 also showed how increasing university overhead rates are compounding these problems. Finally, Chapter 6 discussed the changes that have occurred over the past two decades in the selection, management, and implementation of space physics research projects. The data show that these projects have become larger, more complex, and more expensive, which suggests greater opportunities for the research community. However, other findings are more sobering: launch frequencies (and total experiments deployed) have decreased; project implementation times have risen across the board, dramatically in some cases; heavy documentation requirements have been imposed; and projects increasingly require individual, new-start approval from a strapped U.S. Congress. As overall funding levels increased, with more dollars targeted for large projects, many of these changes were unavoidable. Some even seemed reasonable and necessary to maintain an appropriate system of checks and balances. However, the net effect has been the establishment of a system that causes major implementation delays; disproportionate study, planning, selling, documentation, and administrative activities; inadequate funding profiles for planned programs; and a less effective core research program. All of these findings are consistent with the increased levels of frustration sensed through discussions with colleagues throughout the space physics community.

REVISITING THE BIG SCIENCE/LITTLE SCIENCE ISSUE

We examined the ongoing big science/little science controversy in Chapter 2, described the general characteristics of "big" and "little" science, and reviewed the debate concerning the balance between the two. As we saw, there is

a tendency for science to grow, and to grow rapidly. As a consequence, the perceived size of any science effort will depend on when it is developed within its respective subfield. Similarly, the relative size of a scientific project will vary from subfield to subfield, as well as from agency to agency. Despite these changing perspectives, it generally is possible to distinguish between big and little science at a given point in time, in a particular subfield, and within a specific funding agency.

When we do this in space physics we find that both big and little projects have been used to advance the field to its present state of knowledge. Chapters 4 and 6 illustrated how the field of space physics has evolved from a small group of pioneering researchers probing the edge of space with balloons and rockets to a community of several thousand researchers using state-of-the-art tools to study the space environment from the earth to the stars. Chapters 2 and 6 also showed that both large and small projects have been used together from the earliest days of what we now call space physics. When things go well, little science supports big science through the results of its research and discoveries, and often itself evolves into big science endeavors. In a complementary fashion, big science provides platforms for larger and more complex experiments, and often supports little science directly by providing experimental opportunities for many additional researchers and groups.

At one time a strong synergistic relationship existed whereby everyone seemed to benefit. However, the extensive experiences of the committee members, confirmed through discussions with colleagues, and substantiated by the data assembled for this report, indicate that this synergism has broken down. In struggling to explain what went wrong, we have found what we feel are important clues for understanding the paradox underlying this study. The consequences of our findings are embodied in four major conclusions presented below.

CONCLUSIONS

Conclusion No. 1: The effectiveness of the base-funded space physics research program has decreased over the past decade.

We saw in Chapter 4 that the size of the space physics research community has increased at a rate roughly commensurate with the general increase in research funding described in Chapter 3. However, Chapter 5 showed that the average grant size in the base-funded program (the source of support for most small science) has decreased during this time. This was brought about by an effort to fund a growing number of proposals from a budget that, while increasing slowly, has not kept pace with demand. In other words, the base research program has not participated fully in the overall funding increase. We estimate (Chapter 5) that researchers must now submit two to four proposals per year to remain funded, even more if graduate students, a research group, and instrument

development staff are to be supported. This contributes to the greatly decreased efficiency of the present core program. Much more effort is now expended per dollar on writing and reviewing proposals and on contracting for the research being done. Increased university overhead costs further exacerbate this inefficiency.

In an attempt to quantify this phenomenon, we estimated in [Chapter 5](#) that the dollar value of the effort expended in writing, reviewing, and granting funds in the core program can reach up to 50 percent of the amount being awarded. From any perspective this is an unreasonably high (and generally overlooked) cost burden for an already stressed core research program.

Small science, carried out by an intellectually diverse, flexible community of independent investigators, provides unique capabilities for performing certain kinds of research. Because of this, it is our conviction that a strong, effective, base-funded research program is essential to the health of the field.

Conclusion No. 2: Factors such as planning, marketing, the funding process, and project management have become as responsible for the increased delays, costs, and frustration levels in space physics as technical complications related to increasing project size and complexity.

[Chapter 6](#) showed that many space physics programs, both space-based and ground-based, exhibit the same trend of increasing time from conception to implementation. This trend is most pronounced in what we characterize as big programs, but similar problems are creeping into the smaller programs as well. For example, satellite mission implementation times have increased from two or three years in the early 1960s to the present value of 10 to 15 years. We also looked at NASA's Explorer program, where implementation times have become so great ([Figures 6.3 and 6.4](#)) as to undermine the original intentions of the process itself—namely, to do high-priority science in a timely manner.

[Chapter 6](#) and [Appendix B](#) show that accompanying these increased implementation times has been a major increase in project management functions such as study, planning, review, and selling activities. These activities occur well before the start of a program and continue far into the implementation phase. They are time consuming, expensive, and often do not contribute much to the science being pursued. In some cases these efforts are for naught, and the planning never comes to fruition (see [Appendix B](#)). In too many other cases [8], these efforts have extended through such long study and implementation phases that a sense of disillusionment arises.

The management system further imposes documentation requirements that represent a substantial part of the experimental team's effort. The extent to which this requirement has been imposed is seen as excessive by the space physics community. For example, in many cases a separate manager is assigned the responsibility of collecting each set of required documentation.

Using examples from solar observatories, rocketry, and ballooning, Chapter 6 goes on to show that increased size and complexity are not the only, or sometimes even the major, factors in the increased implementation times and costs of research projects. For example, balloon and rocket program experience has shown that success rates and scientific productivity can be maintained despite significant growth in the size and complexity of instruments. By contrast, ground-based solar observatories show a major increase in implementation times, even though their size and complexity have remained comparable over the past two decades. In the latter example the increased implementation times are due to the extra management activities described above, combined with the vagaries of incremental funding that rarely matches the planned profile.

This management structure may be a natural result of the trend toward large programs. Because of the huge investment of resources in these large projects, government and other managers feel a responsibility to closely monitor every aspect of their progress. Unfortunately, the indiscriminate application of this management system to programs and projects of all sizes has reduced the effectiveness and increased the cost of the overall space physics research program.

Conclusion No. 3: The long-term trend that has led to an ever-increasing reliance on large programs has decreased the productivity of space physics research.

The time period that saw a growing reliance on large science projects also witnessed several disturbing trends. As described above, there has been a steady increase in the implementation times of space physics projects, both ground and space based. This has been accompanied by a steadily expanding effort by the research community on planning, study, and selling activities.

In parallel with these trends, the average grant size in the base-funded program and its constant-dollar buying power have decreased relative to inflation, despite the fact that overall funding for space physics has increased markedly over the past decade. We conclude by implication that the bulk of the funding increase experienced in space physics research over the years has gone to large programs. Because of their broad goals and national visibility, these projects have been easier for funding agencies to sell than the base research program.

However, the complex and ambitious character of big science projects also has its downside, including frequent cost overruns. The infrequency of largescale activities produces pressure to add on additional experiments that may not be essential to the primary goal of the project. The addition of these lower-priority components contributes to rising costs.

Unfortunately, small budget perturbations in big programs can have major effects on small programs. The OTA report [2] aptly illustrates this concern for the broader science community by considering cost projections that include the possible effects of four megaprojects: the Human Genome Mapping Project, the

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now-cancelled Superconducting Supercollider, the Earth Observing Satellite, and the Space Station. It shows that in a scenario in which the *total* science budget is held to a specified rate of increase, constant-year dollar funding for the nation's science base will stay level or decrease through the 1990s, squeezed out by the big projects.

Big projects are also much more vulnerable to shifting political winds and competing science priorities. Tying science goals to new-start approval decisions with incremental funding allotments introduces a high level of uncertainty and risk. It has also led to a steady decrease in experimental opportunities (including balloon, rocket, and satellite flights and instruments) and a space physics community too often diverted from direct scientific research into peripheral management activities, or writing multiple proposals for the scarce dollars left in the base-funded program. A disturbing, but telling, side effect of the processes described above is that a steadily decreasing percentage of experimentalists are entering the space physics field, as noted in [Chapter 4](#).

We believe that these trends must be halted and reversed in order to restore the health, and safeguard the future, of the field. We also believe that the space physics story may contain lessons of value to the broader academic community.

Conclusion No. 4: The funding agencies and the space physics community have not clearly articulated priorities and developed strategies for achieving them, despite the fact that the rapid growth of the field has exceeded available resources.

The number of researchers in space physics has grown considerably over the years ([Chapter 4](#)). The growth has been accompanied by a marked increase in the number and complexity of new research problems proposed within the field. Lacking clear guidance from a set of ranked priorities, the funding agencies have absorbed into their strategic plans more ideas and programs than could be implemented within the bounds of available, or realistically foreseeable, resources. Many of these programs were then maintained in readiness, awaiting the availability of formal approval and funding in the face of competing national priorities. Some of these projects were never started ([Appendix B](#)), or were canceled in midterm ([Chapter 6](#)), wasting resources and failing to achieve the scientific goals that drove them. Even those that do see completion now take longer and cost more as a result of this process ([Chapter 6](#)). The funds consumed in maintaining programs in readiness do not represent an effective use of resources and often impact the core science program as well.

RECOMMENDATIONS

Based on the four major conclusions presented above, we have developed four interrelated recommendations. It is worth repeating the caveat expressed in

Chapter 5 that this report examines trends in the *conduct*, not the content or *quality*, of space physics research. Nevertheless, we believe that implementation of the recommendations in this report could greatly increase the amount of productive research accomplished per dollar spent in the space physics community and could significantly reduce the level of frustration without increased funding levels in the overall space physics research budget.

Recommendation No. 1: The scientific community and the funding agencies must work together to increase the proportionate size and stability of the base-funded research program.

As has been discussed, a productive space physics program cannot survive on large new-start projects alone. An active and synergistic program of small projects is also needed to incubate new ideas. To support this family of small projects, there must be a stable and effective base-funded research program. Furthermore, a revitalized core program must adopt procedures to decrease present inefficiencies (**Chapter 5**). With a larger, more stable core program, the funding agencies can increase grant sizes and durations, enabling researchers to focus more on science and less on funding. Other improvements in the funding process, such as requiring shorter proposals, providing a faster, more efficient review process, and delegating greater authority to the principal investigator where possible, are discussed in more detail in our last recommendation.

Recommendation No. 2: The funding agencies should ensure the availability of many more experimental opportunities by shifting the balance toward smaller programs, even if this necessitates a reduction in the number of future large programs.

The current frequency of experimental opportunities is insufficient to sustain space research into the next generation. As explained in **Chapter 2**, some scientific investigations can only be done via large science initiatives. In fact, we have shown in **Chapters 2** and **6** that a strong synergism existed at one time between large and small space physics programs. However, the future of space physics requires frequent access to new research opportunities and the accompanying development of new scientists capable of carrying out the missions of the future. These goals have been more efficiently achieved through small science programs.

In one survey of the space physics community by NASA's Space Physics Division [5], researchers were asked to characterize their desires for the future of space physics with regard to small and large missions. Although these results must be qualified in light of a low response rate, over 90 percent of respondents expressed a preference for more frequent access to space through small mis-

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sions, even if it meant a reduction in the number of large missions. Our recommendation concurs with this view.

Recommendation No. 3: In anticipation of an era of limited resources, the space physics community must establish realistic priorities across the full spectrum of its scientific interests, encompassing both large-and small-scale activities.

Unless the scientific community itself is willing to make difficult choices and set priorities at the outset, programmatic decisions will ultimately be made on the basis of considerations other than a rational assessment of the value of the program to the nation's scientific progress. The needs and vitality of the discipline as a whole, and not necessarily equity among subdisciplines, are of paramount importance in setting such priorities. Thus, these overall priorities must be considered before it becomes productive to prioritize specific programs and missions. Long-term scientific goals should not be altered lightly or set aside before they are achieved. Scientific priorities should change only in response to changing scientific perspectives. Ongoing projects initiated in response to established scientific priorities should be insulated as much as possible from the effects of short-term fluctuations in funding.

Prioritization must not only cross subdisciplines within a field but also include an assessment of the balance between "big" and "little" science. In particular, it must consider the unique value of a large project relative to the promise of the ongoing base-funded program. A clear assessment of this delicate, yet important, balance must be made by the research community at the outset.

Prioritization is always a wrenching process, since it necessarily involves postponing or eliminating the pursuit of some interesting ideas. It is beyond the scope of this report to recommend a specific process for making these hard choices; however, the space physics community might be able to learn from other academic disciplines that have been forced to undertake similar priority-setting exercises.

Recommendation No. 4: The management and implementation processes for the space physics research program should be streamlined.

Management and implementation processes must be tailored to the size of a given program: big science management techniques should not be applied to little science programs. Politicians, funders, and managers need to acknowledge that their understandable desire for accountability and program control exacts a price in inefficiency, delay, and, occasionally, failure to achieve scientific goals.

Oversight and reporting requirements should be reduced in many instances, even at the expense of assuming a somewhat greater risk. Risk is an everpresent and essential element of the scientific enterprise and should be accepted

at levels in keeping with program size. Study, planning, and selling activities should be reduced and implementation times should be shortened to provide increased experimental opportunities, allow for the timely pursuit of research problems, and ensure the training of students. Proposal reviews also should be streamlined, particularly within the core program.

Many of these steps will be easier to accomplish if a set of priorities is developed by the scientific community. Such priorities will also aid the funding agencies in their programmatic decisionmaking. For their part, funding agencies must work with the scientific community to streamline management procedures, consult with the scientific community as they make programmatic decisions, and clearly convey, and then fulfill, their level of commitment throughout the development of a program.

We feel that major progress can be made by recognizing and relying on the powerful self-interest of researchers to succeed. For example, the continued success of the rocket and balloon programs, despite their increased size and complexity, can be traced in part to the fact that management of these programs has been left in the hands of the principal investigators (Chapter 6). We believe that delegation of greater authority to principal investigators will generally lower the direct and indirect costs of oversight and reporting while improving success rates and scientific productivity.

The four recommendations outlined above are highly interrelated. Streamlined management processes will further boost the productivity of a stabilized core program. Priority setting will enable the few most critical big science projects to be pursued without jeopardizing ongoing research. Taken together, we believe these recommendations provide a blueprint for a stronger and more productive space physics research community.

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APPENDIX A

Space Physics Missions (1958-2000)

INTRODUCTION

This appendix examines the processes involved in obtaining observations from space, which were discussed briefly in [Chapter 6](#). This examination was motivated by a concern over the long time delays between the start of a mission and the return of scientific data which became common in the late 1980s and early 1990s. We start by considering the time from the start of a mission to launch of the spacecraft. We take the starting date as the date that investigators submitted proposals to place instruments on the spacecraft. We chose the proposal date because it is a well-defined starting point that exists in some form for most missions. However, we recognize that it is not the "actual" starting time for the ideas that led to the mission. For instance, NASA administrators first have to be convinced to start, or at least investigate, a mission prior to issuing an Announcement of Opportunity for investigators. In several cases we discuss this preannouncement development stage as well. (A date of July 1 is used when only the year is known. When a proposal date is not available, we use an estimated date on which the "concept" development started for the mission.)

INTERVAL FROM PROPOSAL TO LAUNCH FOR MAGNETOSPHERIC MISSIONS

Missions Started in the 1960s

In this section we consider missions for which one or more investigators submitted proposals in the 1960s. [Table A.1](#) shows a representative list of

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TABLE A.1 Space-Physics-Related Launches in the 1960s

Mission	Start	Launch	Time to Launch	Lead Agency
Pioneer 2	5/1958	11/1958	6 mo	NASA
Explorer 6	7/1958	8/1959	1 yr 1 mo	NASA
Pioneer 5	1/1959*	3/1960	1 yr 2 mo	NASA
Discoverer 31	7/1960*	8/1961	1 yr 1 mo	NASA
Discoverer 33	7/1960*	9/1961	1 yr 2 mo	NASA
Discoverer 36	7/1960*	12/1961	1 yr 5 mo	NASA
IMP 1	12/1960*	11/1963	2 yr 11 mo	NASA
OGO 1	7/1962*	9/1964	2 yr 2 mo	NASA
IMP 2	1/1961*	10/1964	3 yr 9 mo	NASA
Mariner 4	7/1962	11/1964	2 yr 4 mo	NASA
IMP 3	12/1960*	5/1965	4 yr 6 mo	NASA
OGO 2	7/1962*	10/1965	3 yr 3 mo	NASA
Pioneer 6	7/1963*	12/1965	2 yr 5 mo	NASA
OGO 3	7/1962*	6/1966	3 yr 11 mo	NASA
Pioneer 7	7/1963*	8/1966	3 yr 1 mo	NASA
ATS 1	2/1965	12/1966	1 yr 10 mo	NASA
OGO 4	7/1962*	7/1967	5 yr	NASA
IMP 4	12/1964*	5/1967	2 yr 6 mo	NASA
OGO 5	8/1964	3/1968	3 yr 7 mo	NASA
OGO 6	3/1966	6/1969	3 yr 3 mo	NASA
IMP 5	7/1965*	6/1969	3 yr 11 mo	NASA
IMP 6	7/1968	7/1971	3 yr	NASA
Pioneer 10	7/1969	3/1972	2 yr 8 mo	NASA
IMP 7	11/1966	9/1972	5 yr 10 mo	NASA
Pioneer 11	7/1969	4/1973	3 yr 9 mo	NASA
IMP 8	11/1966	10/1973	6 yr 11 mo	NASA
AEC	7/1969	12/1973	4 yr 5 mo	NASA
Mariner 10	7/1969*	11/1973	4 yr 4 mo	NASA
ATS 6	7/1968	5/1974	5 yr 10 mo	NASA
Viking	7/1969	7/1975	6 yr	NASA

* Mission concept date used.

missions from this period. An asterisk indicates missions for which the concept date is used. The time from proposal to launch for our sample varied from less than two years to nearly seven years. Several of the missions took three to four years. One of the shortest was ATS 1, which took one year and ten months. ATS 1 was designed to test communications technology and was not originally intended to carry any scientific instruments. However, in early 1965 the decision was made to include a small number of scientific instruments. These had to be completed quickly to keep the project on schedule. The spacecraft with the longest development interval was IMP 8. IMP was a scientific spacecraft, and its development was originally planned to take six years, with launch in 1972. It

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should be noted that IMP 8 was still providing valuable solar wind data 18 years after launch.

Missions Started in the 1970s

Table A.2 lists several missions started in the 1970s. The Voyager and the International Sun Earth Explorer (ISEE) missions took about five years from proposal to launch. Both were scientifically successful missions. ISEE reentered the Earth's atmosphere in 1987 after 10 years in orbit, while the Voyagers are still returning heliospheric data after having probed the magnetospheres of Jupiter, Saturn, Uranus, and Neptune.

The Dynamics Explorer (DE) mission provides a good example of the effort required to define and sell a mission concept in the 1970s. The DE mission was designed to study the atmosphere, ionosphere, and magnetosphere as a system. Conferences laying the foundation for the DE mission began as early as 1972. During the fall of 1973 the scientific concepts on which the DE mission was based were presented to the Office of Space Science at NASA Headquarters. In April 1974 a planning and feasibility study group was established at the Goddard Space Flight Center (GSFC), and in July 1974 an Announcement of Opportunity (AO) was released that solicited proposals for Explorer-type payloads. Many of the proposals submitted in response to this AO were for the Electrodynamics Explorer (EE) program.

An EE study team was appointed in 1975. It issued a report describing the mission, and in May 1976 NASA made the final selection of investigators for the mission. A project plan was prepared by GSFC, and a cost review was conducted at NASA Headquarters. Following this review it was decided that the EE project could not be implemented as outlined. However, the scientific communi-

TABLE A.2 Space Physics Related Missions in the 1970s

Mission	Start	Launch	Time to Launch	Lead Agencies
ISEE 3	9/1972	8/1978	5 yr 11 mo	NASA
Pioneer Venus	10/1973	5/1978	4 yr 7 mo	NASA
DE 1 and 2	7/1974	8/1981	7 yr 1 mo	NASA
AMPTE	7/1972	8/1984	12 yr 1 mo	NASA,
U.K., Germany				
Spacelab	9/1976	8/1985	9 yr 1 mo	NASA
Galileo	11/1976	10/1989	12 yr 11 mo	NASA
Ulysses	8/1977	10/1990	13 yr 2 mo	ESA, NASA
UARS	12/1978	9/1991	12 yr 9 mo	NASA

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ty had made a very strong case that the scientific problems EE was to attack were of the highest priority in space physics. So in January 1977 a smaller mission, Dynamics Explorer, was started. The investigators for this smaller mission were chosen in May 1977. Funding authorization for the program was received in October 1977, and the two DE spacecraft were launched less than four years later.

The final mission during the 1970s was Galileo. The Galileo proposals were written in 1976. The original launch was scheduled for 1982, but problems with the spacecraft and with the Space Shuttle launch system caused it to be postponed until 1986. The launch was further delayed until 1989 by the Challenger explosion. However, even if the Challenger explosion had not occurred, the interval between proposals and launch still would have been nine years.

Missions Started in the 1980s

During the 1980s the time between the selection of experiments for a mission and the actual launch became very large (Table A.3). The Combined Release and Radiation Effects Satellite (CRRES) mission was started in 1981. Originally, it was an Air Force project called RADSAT. In 1982 it was combined with the NASA Chemical Release Program. After a number of delays, including the Challenger accident, CRRES was launched in 1990.

The International Solar-Terrestrial Physics (ISTP) program resulted from a series of studies conducted by committees of the National Research Council's Space Sciences Board (SSB) in the late 1970s and early 1980s. Among these was the Kennel report¹, which cited six critical regions of the terrestrial magnetosphere that needed to be better understood in order to understand the time-dependent exchange of energy and plasma between the solar wind and the magnetosphere. From this the Origins of Plasma in the Earth's Neighborhood (OPEN) program evolved. In the OPEN program spacecraft would be flown simultaneously in four key regions: the WIND spacecraft would monitor the solar wind, the POLAR spacecraft would observe in the polar region, EQUATOR would provide observations in the near-earth magnetotail equatorial region, and GEOTAIL would probe both the near-earth and distant magnetotail. Proposals for participation in the OPEN mission were written in 1980. During the design phase of the mission it became evident that the costs would exceed the available resources. Since understanding the global flow of energy throughout a system as vast as the magnetosphere required the four spacecraft at a minimum, it was decided to seek international cooperation. This led to the formation of ISTP. Under ISTP the Japanese Institute of Space and Astronautical Science

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TABLE A.3 Space-Physics-Related Missions in the 1980s

Mission	Start	Launch (actual or expected)	Time to Launch	Lead Agencies
CRRES	7/1981	7/1990	9 yr	DoD, NASA
SAMPEX	9/1988	7/1992	3 yr 9 mo	NASA
ISTP/Geotail	3/1980	7/1992	12 yr 4 mo	ISAS, NASA
ISTP/Wind	3/1980	9/1994	14 yr 6 mo	NASA
ISTP/Polar	3/1980	6/1995	15 yr 3 mo	NASA
FAST	7/1988	7/1994	6 yr	NASA
ISTP/Cluster	7/1988	12/1995	7 yr 5 mo	ESA
CRAF	11/1985	7/1996	10 yr 8 mo	NASA
ACE	7/1986	8/1997	11 yr 1 mo	NASA
Cassini	2/1990	7/1997	7 yr 5 mo	NASA, ESA
SOHO	7/1989	7/1995	6 yr	ESA, NASA

(ISAS) took the lead in the GEOTAIL spacecraft. The European Space Agency agreed to provide four spacecraft that would fly in a tetrahedral formation to probe the polar magnetosphere, magnetopause, and cusp (CLUSTER). The EQUATOR spacecraft was canceled. It was hoped that data from the CRRES spacecraft would partially fill the gap left by the cancellation of EQUATOR, but CRRES stopped operating in 1991. The ISTP mission was approved in 1988. The GEOTAIL spacecraft was launched in July 1991, 12 years after the initial proposal. Unfortunately, the schedules for the WIND and POLAR spacecraft have slipped recently, and it will be at least mid- to late 1995 before they are launched.

The Fast Auroral Snapshot Explorer (FAST) is a small explorer mission. It will provide high-resolution observations in the auroral zone. In this small explorer program the entire instrument complement was proposed as a unit with a single principal investigator. The proposals were written in 1988, and the current schedule calls for a 1994 launch.

The time difference between the proposal and launch date for each mission, which provides a measure of the implementation time, has been plotted versus launch date in Figure A.1. Explorer and other NASA missions are shown in the Figure, with future missions indicated by arrows that represent the effect of a one-year delay. The implementation time has steadily increased over the past three decades, with the result that most recent missions have taken approximately 12 years to be implemented.

¹ Solar System Space Physics in the 1980's: A Research Strategy; Committee on Solar and Space Physics, Space Sciences Board, National Research Council, 1980.

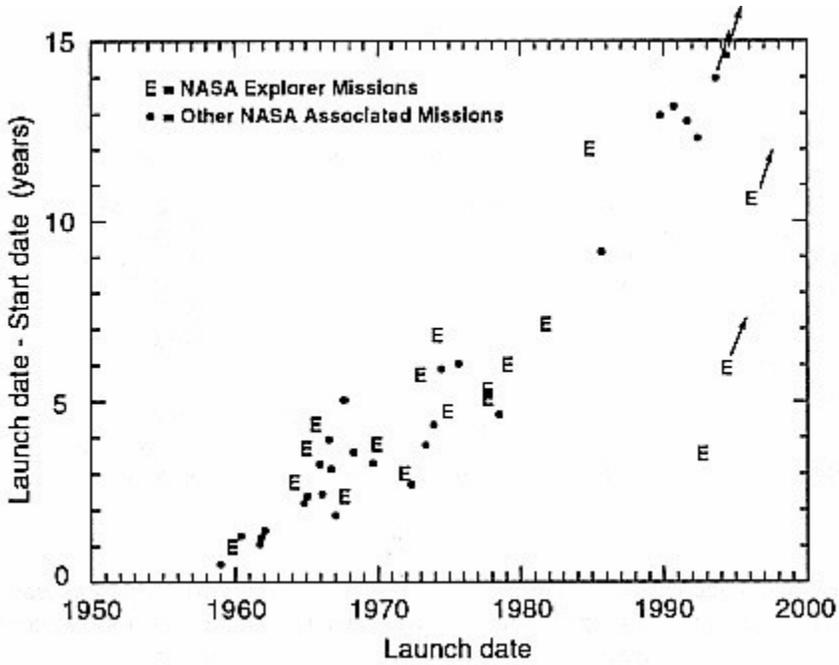


FIGURE A1.1 Implementation times for space-physics-related missions, 1958-2000.

Note: Launches for which reliable start dates could not be obtained are not included in this Figure.

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APPENDIX B

The Solar Telescope That Saw No Light (A Tale of Planning Gone Awry)

This is the story of how a proposed \$25 million solar telescope for an early Space Shuttle mission grew into a proposed \$360 million national facility for solar research. It tells how the facility further grew into a proposed \$811 million laboratory and then finally was canceled. The story takes place between 1965 and 1992, during which time an estimated 1,000 person-years of work was devoted to planning the Orbiting Solar Laboratory (OSL). It is admittedly told from the research scientist's point of view, but the committee believes that it illustrates how the trend toward "big" science and excessive planning can undermine the nation's efforts to achieve important scientific goals.

OSL started in 1965 as a modest idea. By NASA standards it was definitely a "small" science project. It was an extension of a program at the California Institute of Technology (CIT) to improve solar imagery. Two scientists would direct the project. But by the time it was canceled in 1991, OSL had grown to look like big science. About 200 solar physicists (half the world's stock) would have been needed to operate it and analyze the data. It would have inspected the Sun at wavelengths from a thousandth of a nanometer (gamma rays) to a thousand nanometers (infrared). It would have been to solar physics what a completely successful Hubble Space Telescope is to astrophysics. The difference is that OSL was never built and probably never will be, but like the Hubble Space Telescope it raises painful questions about the conduct and cost effectiveness of big science projects. [Table B.1](#) summarizes the OSL chronology.

SOLAR PHYSICS AND BIG SCIENCE

Big science is not new to solar physics and has in fact been beneficial to the field. The eclipse expeditions of the nineteenth century were major undertak-

TABLE B.1 Chronology of the Orbiting Solar Laboratory

1968	Caltech/Jet Propulsion Laboratory 65-cm telescope proposal for Skylab II
1972	65-cm prototype installed at Big Bear Solar Observatory
1973	MSVC/Itek 150-cm telescope study for shuttle
1973	Goddard Space Flight Center (GSFC) 100-cm telescope study for Spacelab
1974	Announcement of Opportunity for "Scientific Definition of Space Shuttle Missions for Solar Physics Spacelab Payloads"
1975	Initial work of One-Meter Solar Telescope Facility Definition Team
1976	Spacelab Optical Telescope proposed by Association of Universities for Research in Astronomy, Inc., to NASA
1978	Spacelab Optical Telescope top ranked of four candidate solar facilities
1979	Solar Optical Telescope project started at GSFC
1980	Facility definition teams terminated
1982	Selection of science teams, telescope and instrument contractors
1983	Phase B studies completed
1983	Phase C/D deferred due to Spacelab budget reductions and difficulties with Hubble Space Telescope
1984	Formal NASA approval for FY 86 new start but FY 86 budget capped at FY 85 study level by Congress
1985	Phase C/D funds deleted from FY 87 budget request by Office of Management and Budget (OMB)
1986	High Resolution Solar Observatory (HRSO) project started at GSFC; studied as Space Station payload
1986	Phase C/D funds deleted from FY 88 budget request by OMB
1987	HRSO redesigned as a free flier
1988	Restructuring of HRSO to restore capabilities lost in 1986
1989	New science objectives formulated to accommodate changes in hardware
1990	GSFC New Business Committee pledges center to OSL budget and manpower plan
1990	Request for proposals issued for Phase B contractors
1990	Favorable nonadvocacy review; favorable review by Space Science and Applications Advisory Committee (SSAAC)
1990	OSL listed as the highest-priority mission for initiation as early as 1992 in the Office of Space Science and Applications Strategic Plan
1991	SSAAC recommends 1998 as earliest start date for OSL

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ings, requiring extensive logistical support from the Army and the Department of the Interior. In the late nineteenth and early twentieth centuries, George Ellery Hale, a solar physicist, practically invented big science. Before the era of government funding for science, Hale convinced Charles Yerkes, the wealthy builder of Chicago's elevated railway, to finance construction of the largest telescope in the world. A few years later he persuaded Andrew Carnegie to finance the largest solar telescopes and the 60-inch and 100-inch nighttime telescopes on Mount Wilson. Each in its turn held the distinction of being the world's largest telescope. To support users of the telescopes, Hale founded the Mount Wilson Observatory of the Carnegie Institution, an early model of the Space Telescope Science Institute. Each of Hale's projects strained the technical and financial resources of the day. Hale was searching for support for the 200-inch Palomar telescope when nervous exhaustion forced him to retire.

Hale had created a new kind of institution in America, one devoted solely to scientific research. It required huge and expensive facilities, and it was successful in making southern California the world center in astronomy. His was a big science success story.

There are other such success stories as well. In 1961 the Associated Universities for Research in Astronomy (AURA) completed the world's largest solar telescope near Tucson, Arizona. Another major solar telescope for New Mexico was proposed to the Air Force in 1961, with approval in 1965. Each of these telescopes, to be used effectively, required a dozen solar physicists. Each was a successful big science project, and each moved from conception to completion in about four years.

THE SKYLAB OPTICAL TELESCOPE

In 1965 Harold Zirin and Robert Howard, two astronomers at institutions Hale built, started planning with the Jet Propulsion Laboratory (JPL) at CIT to build an orbiting solar telescope. They did not think of their Skylab Optical Telescope as big science. It was just a small experiment they would build and manage at a private institution, and they planned to oversee its scientific program.

NASA was regularly launching orbiting solar observatories, a series of small satellites each with a half-dozen bantam telescopes. It was also planning the Apollo Telescope Mount, which would carry a cluster of larger solar telescopes on Skylab. Skylab was a manned mission, and the Apollo Skylab program was definitely a big science program. Analysis of its solar data was projected to eventually employ 200 scientists for most of a decade.

But the Skylab telescopes and the orbiting solar observatories sent down pictures only of the Sun's outer atmosphere. Many solar physicists were more interested in the tiny magnetic elements on the solar surface, and Zirin and Howard's idea appealed to them. They knew that no one would ever see the

basic structural elements of the solar surface with a ground-based telescope because of the blurring effects of the Earth's atmosphere. The only trouble with the 65-centimeter telescope proposed by the CIT astronomers was that it would not resolve the magnetic elements. To do that would require a 150-centimeter telescope.

BIRTH OF A "FACILITY"

After dropping plans for a Skylab II, NASA began the first of many planning exercises for Space Shuttle payloads. In 1973 it funded two studies of larger telescopes, one through the Marshall Space Flight Center (MSFC) and one through the Goddard Space Flight Center (GSFC). Both studies concluded that the project was feasible. GSFC got the assignment for further work. MSFC and JPL were taken off the project, to the regret of solar physicists, who had strong confidence in MSFC because of its successful management of the Apollo Telescope Mount and because of its competent and growing solar physics group.

In 1976 the Associated Universities for Research in Astronomy (AURA) and CIT scientists submitted a proposal to build a Spacelab Optical Telescope and manage it as a facility for a wide range of users. The projected cost was \$25 million, although some scientists even then thought this estimate was too low. NASA thought that AURA could not possibly assure the success of the project (although it had teamed up with a highly experienced space instrument contractor), so it was renamed the Solar Optical Telescope (SOT) and designated as a NASA facility. The scientific teams were disbanded.

In the following two years (1980-1982), GSFC management built a sizable SOT project bureaucracy. Key scientists were not involved in this important phase when the project's structure and principles were developed. Finally, NASA did add scientist participation in planning the design and operation but not in the management of the SOT. All selected instruments were to become "government-furnished equipment" with virtually every detail of their design and use subject to government approval. Several who had conceived of and designed the telescope for CIT and AURA dropped out at this point.

INFLATION AND DELAY

By late 1985 the estimated cost of the SOT was \$360 million. The project had been thoroughly studied, but design and construction were repeatedly deferred, due in part to difficulties with the Hubble Space Telescope. In Congress, opposition to the SOT was building because of its cost inflation, and finally, in February 1986, the Office of Management and Budget (OMB) deleted all funds for the project. GSFC management told the SOT Science Working Group that a \$100 million mission might be acceptable.

To lower the cost the Science Working Group reduced the telescope aper-

TABLE B.2 Capabilities Deleted from SOT

1. Delete ultraviolet capabilities throughout
2. Delete articulated primary mirror
3. Use Spacelab Instrument Pointing System for pointing
4. Delete steering feature of tertiary mirror
5. Use fast active optics on M4 only
6. Add simple white-light TV for pointing control
7. Delete wave-front sensor
8. Delete stand-alone focus sensors
9. Shorten telescope or reduce alignment complexity; Coordinated Instrument Package also becomes more compact
10. Greatly simplify contamination control system
11. Consider replacement of correlation tracker with boresight or limb sensor
12. Eliminate "Facility" command and power systems
13. Eliminate "Facility" ground support equipment
14. Reduce field of view to one arc minute
15. Delete background tunable filter-graph charged-coupled-device (CCD) camera, associated optics, and shutter
16. Replace two photometric filter-graph film cameras with a single CCD camera system—thus no steering mirror
17. No spectrograph grating carousel, no UV Schmidt mirror position, and no black mirrors
18. Delete initial UV-rejection moveable window
19. Delete polarization corrector slide
20. Consider spherical optics for primary mirror rather than parabolic
21. Greatly simplify heat rejection system

ture to 100 centimeters, eliminated most of the ultraviolet capability, and removed one of the spectrographs (see [Table B.2](#)). The effect on the scientific capabilities was serious but not debilitating. Even so, the new GSFC cost estimate was still too high—\$189 million.

At this point, a team from the Naval Research Laboratory (NRL) and MSFC proposed to complete the project for \$86 million. But NASA did not want to pull the job from GSFC. Reluctant to jeopardize SOT's chance to get started in the next year, NRL and MSFC backed down.

The Challenger accident was another setback, because SOT had been planned as a shuttle payload. SOT got a new name, High-Resolution Solar Observatory (HRSO), and was studied as a Space Station payload. Then, because the Space Station berth proved to be prohibitively expensive, SOT was redesigned again, this time for launch by an unmanned vehicle. To replace the capabilities deleted earlier, NASA invited Germany, Italy, and the U.S. Air Force to supply additional experiments, at their own cost. NASA agreed to support an NRL-provided telescope for the payload.

Relations between NASA and many solar scientists were severely strained at this point because the new instruments had been added without competition, although NASA argued that a full-blown competitive selection process would take too long. It was all in vain because the OMB deleted all funds for design and construction from the 1988 budget.

Despite this history of dashed hopes and growing antagonisms, despite the Challenger accident, despite Hubble's cost overruns, the penultimate phase of the OSL project was grandiose, speculative, and briefly euphoric. Back in 1986, solar physics had moved from NASA's Astrophysics Division to the newly formed Space Physics Division, where the HRSO immediately became the biggest and oldest "gorilla" around. The SOT-HRSO was renamed the Orbiting Solar Laboratory (OSL) to emphasize its broad capabilities. Its new cost of \$500 million seemed to be a positive factor, since it could establish a precedent for other big missions to follow in the Space Physics Division. Now it was 1990 and, like the \$80 million Van Gogh paintings in the news that year, it seemed that something more expensive was better. After a series of planning sessions, the space scientists decided that big—very big—projects were most likely to succeed. The Earth Observing System and Hubble Space Telescope had paved the way. The cost estimate went to \$811 million, not counting \$53 million already spent.

FROM FIRST PLACE TO LAST

Through push and pull, plans for a truly marvelous and versatile laboratory had emerged. The scientists had broadened the scientific goals to include solar net energy and hard x-ray measurements. National Research Council committees and NASA advisory panels all agreed on the importance and urgency of getting the OSL started. Finally, there was no more planning to be done. The Office of Space Science and Applications (OSSA) moved the OSL to its first priority for the next "new start."

The bubble burst on August 22, 1991, when NASA officials met with the Space Science and Applications Advisory Committee (SSAAC) at Woods Hole, on Cape Cod. Against a background of a faltering U.S. economy and a looming election year, NASA moved the proposed OSL start date from 1993 to 1998, saying that, for the Space Physics Division, small missions would be better. On

the morning of that fateful day, Harold Zirin, the CIT physicist who had fought for the project since 1965, had felt more confident than ever of a final positive decision. But, like Alice watching the Cheshire Cat fade away, by evening Zirin knew that nothing was left of OSL but the smile.

WHAT DOOMED OSL?

The generation that invented and promoted it will probably never see it fly. Some of them struggled for 25 years to make it happen, but many forces outside their control helped doom it. In the 1980s each cost escalation of the Hubble Space Telescope amplified the SOT estimates. Then the Challenger accident forced its redesign as a free flier. The end of the Cold War meant the end of the space race and an end to large annual increases in NASA's budget. Soaring national budget deficits put all big expenditures under the knife.

From the time NASA took over in the late 1970s to OSL's cancellation in 1991, the scientists thought they had no control over cost estimates. The details were off limits. NASA argued that the numbers could reveal proprietary information or that they could tip off potential hardware suppliers about the prices the agency expected to pay. The effect was to make it impossible for the scientists to do much to bring the costs down except cut back on the scientific capabilities. No review committee ever criticized the importance of the science or the technical feasibility. After so many studies, the scientific and technical cases for OSL were strong.

The Science Working Group tried continually to gain more control over the project. Although it was generally told few details about why the costs were growing, the group did discover that data collection and analysis was a major cost driver. This issue frustrated and irritated the group for it knew that high cost estimates were jeopardizing the project and believed it could handle the data at far less cost than could the GSFC. More important, the group believed that responsibility for the quality of vital data was being taken away from it. GSFC was planning to create a Science Data and Operations Center to be responsible for management of science data processing, distribution, and archiving. The Science Working Group preferred a distributed data center, with nodes at the scientists' institutions and data banks under their direction.

Starting with its designation as a NASA facility and its early cost escalation to \$360 million, SOT-OSL was believed by some to be too expensive. The NASA chief scientist proclaimed it overpriced for the expected scientific return. After each higher cost estimate, a few more key people would privately conclude that the project would never happen. Before the end, more than \$53 million and an estimated 1,000 person-years were spent over a 25-year period in planning the project. The OSL had evolved from a small to a big science project in a bureaucratic and committee-laden environment of the sort that rarely produces excellence.

One of many lessons to be learned from the OSL experience is that a project drawn out too long loses the inspiration and determination of its inventors. Long planning periods and frequent postponements erode morale. Ultimate success becomes less and less likely as the scientists are disenfranchised by cautious professional managers. An adversarial relationship can develop between researchers and the government. Flexibility fades, factions develop, heroes depart, consensus dissolves, and everyone looks for a younger, less-scarred project.

EFFECT ON THE SOLAR PHYSICISTS

How did the scientists feel about the project as it grew through the 1980s? The surprising result of an informal survey (see [Chapter 5](#), footnote 1) is that many of them had decided as early as 1978, when NASA turned down the AURA proposal, that they would get nothing out of it. Most of the others quietly and privately wrote the OSL off after the repeated setbacks of the early 1980s.

Despite their private and sometimes public pessimism, solar physicists had tried a number of times to regain control of the project and its costs. The NRL/MSFC proposal was one example. Another was a plan by Art Walker of Stanford to set up a committee of scientists not affiliated with the SOT to try an entirely new approach. NASA opposed these initiatives. Out of necessity, most of the major players had developed alternate research objectives, and many were not even planning to use the OSL data. By 1988 most OSL scientists saw the project as a good thing if it could happen, but they were putting their own energies into smaller science projects.

EPILOGUE

In January 1992 NASA officials suggested there might be a "distributed" OSL. Couldn't much of the same science be done gradually with a combination of ground-based telescopes, theory, rocket experiments, and a balloon-borne telescope? The price of the latter would be \$20 million, part of a proposed \$38 million "Research Base Enhancement" to help U.S. solar research in space recover from the past years of frustration. Within a year, this proposal too was abandoned.

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