Space Policy Reconsidered

edited by
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Chapter 11

FUTURE DIRECTIONS FOR SPACE POLICY RESEARCH

Radford Byerly, Jr., and Ronald D. Brunner

THE DOGMAS OF THE QUIET PAST ARE INADEQUATE TO THE STORMY PRESENT.

LINCOLN

The preceding essays have raised many issues. In this concluding chapter we step back from the individual essays, draw on our own experience, and consider the lessons and implications for the future of space policy research. In particular we (1) contend that the Apollo Paradigm, as a general way of seeing space policy, is increasingly obsolete, (2) suggest some specific priority research problems in a Post-Challenger Paradigm, and (3) recommend the broadening of such future research through the use of experience from other policy areas and the emerging discipline of public policy.[1]

THE BASIC LESSON

In our judgment, many of the difficulties described in these essays stem from a complex of assumptions, often taken for granted, that was described in the Introduction to this volume as the Apollo Paradigm. That paradigm has become increasingly obsolete and counterproductive as a framework for research and decision in civilian space policy.

The Apollo Paradigm emphasizes a vision that favors the selection of projects of a certain kind. Such favored projects are:

- -- large, both in physical size and in complexity, and accordingly expensive;
- -- pre-engineered according to a centralized plan, integrated with other projects, and thus vulnerable to the failure of any weak link; and
- -- dependent upon long-term, stable, and focussed political support.

In the ecological metaphor advanced by Brewer, projects of this kind are increasingly less "fit" or "adaptive" as the social, economic, and political environment changes.

Consider the dependence on stable political support and how it affects the survivability of programs. Apollo probably did have broad consensus support at its start but the support withered: Apollo funding was brutally phased down even before the first man stepped on the Moon. As part of the current budget process an attempt is underway to force a go/no-go decision on the Space Station, with the hope that if the decision is to go ahead, there will be no further debate and the requested funds will be appropriated annually for the thirty-year life of the project. But our system of government does not work that way. In the era of Gramm-Rudman-Hollings the regular annual budget debates will be even more brutal because the need is to allocate cuts, not increments, of resources.

Even after development costs have been incurred, it will still be cheaper -- in terms of Federal dollar outlays -- to cancel than to continue. And because total operating costs will be larger than development costs, there will be repeated review of the worth of continuing. Therefore Space Station, a program that exemplifies the Apollo Paradigm, strongly depends on political support that may be difficult to sustain over several decades: It will be difficult during the construction period when expenses are high and there are few results to show, and it will be difficult when "permanent presence" becomes routinized and unexciting but operational costs remain high. In these respects the Apollo Paradigm does not contribute to a vigorous space program.

The Apollo Paradigm also de-emphasizes or ignores other criteria that have become increasingly necessary for "viable" projects:

-- appreciation of unpleasant contingencies that are overlooked in success-oriented planning, such as hardware failures, changing user needs, and emerging competition;
-- resilience or flexibility to respond to events that cannot be entirely predicted or controlled; and
-- renewal of the political consensus required to sustain a program.

Each of these can be expanded with an example.

First, consider the need to appreciate unpleasant contingencies such as hardware failures. There is a reasonable chance that there will be another Shuttle accident before Station is assembled,[2] but current plans assume that four orbiters will be available throughout the period. The Apollo Paradigm encourages such "success-oriented" plans. However, plans and results need to be evaluated realistically, explicitly, continuously, and at least partly independently in order to maintain their viability.

Flexibility is needed so that feedback from such evaluations or unexpected signals from the environment can be incorporated gracefully. The Apollo Paradigm sees flexibility as a negative characteristic to the extent that it makes a program vulnerable to interference or changes by OMB or Congress. The assumption is that potential budget cuts are more easily resisted if it can be argued that they would have a catastrophic impact on the target program.

The need to maintain a political consensus behind a project seems obvious, but the important thing is the strategy employed. James Webb -- next to John Glenn and John Kennedy perhaps the person most closely associated with the Apollo program -- recommended keeping the national interest in focus and measuring the degree to which programs are concurrent with the national interest by the support gathered.[3] If programs did not maintain support they were modified or abandoned. This was very different from the strategy employed for the Station: That is, the attempt to force a go/no-go decision (described above) and the earlier attempt to institutionalize support by giving a major role to five different NASA Centers. This splitting of responsibility complicated the internal politics and management of the program. Curiously, Webb might well reject the Apollo Paradigm, and not recognize it as related to his work.

The implication is that a new paradigm is needed for space policy and policy research. The Introduction suggests that this be called the Post-Challenger Paradigm, and that it supplement the vision of the Apollo Paradigm with more realistic analysis. Neither vision nor analysis alone is sufficient.

SPECIFIC PROBLEMS

A Post-Challenger Paradigm raises the priority of several current but relatively neglected problems, and provides a different perspective that may make these problems tractable.

(1) *Costs*, both annual and total, are more important in this era of unprecedented Federal deficits not yet under control. It is no longer reasonable to assume that escalating costs will be easily absorbed, or that costs will be discounted in favor of technical performance. If a project is allowed to exceed its budget, there will be large opportunity costs as other projects are postponed, curtailed, or cancelled; large political costs as political "chips" are expended to secure extra funds; or a reduction in program flexibility caused by mortgaging the future as was done in the case of the Tracking and Data Relay Satellite System (TDRSS).[4]

Traditionally the role of the political system has been to deliver the funds needed to carry out the vision of space exploration: Mission design was specified first and then costs were determined by the amount of money needed to achieve the desired design. The system (driven largely by Federal procurement regulations) has been sensitive to annual appropriation ceilings but insensitive to overall project costs.[5]

The problem is that the design of a science mission, for example, is typically determined by asking users "what do you need?" This generates a wish-list which is then used to justify building a large, expensive system to do many of the things on the list. (This preserves broad support in the user community.) To make cost a more significant criterion one must ask instead, "if you had a dollar, how would you spend it?" This very different question allows, indeed forces, fundamental consideration of alternatives for meeting needs.

Policy research is called for: How can we put incentives into the procurement system to hold down costs and get the most for each dollar invested? The Congress has tried caps through Gramm-Rudman-Hollings, and Moore has considered their potential impacts. Struthers has considered how both the Administration and Congress have promoted "commercialization" of space activities in part to reduce costs. However, it may be difficult to achieve the anticipated benefits of "commercializing" critical activities which the government will not allow to fail. For example, if the government is determined to build a Space Station then it cannot reasonably allow *critical* elements to be built commercially, if the definition of "commercial" includes some real risk. What other approaches might work?

(2) *Performance* is also increasingly important, and the criteria of performance are changing. NASA has a good record with respect to the ultimate technical performance of its missions but there is a disturbing tendency to defer delivery of results. Missions are taking longer and longer between initiation and culmination. Galileo began in 1976 but data from Jupiter will not be available before 1995. Station "began" in 1984 and the first element launch is planned for 1995 with complete assembly of Block I in 1998, and Block I is only a fraction of what was envisioned in 1984. The deferral of payoffs into the future discounts the present value of a mission, both politically and economically, which makes program support and funding more problematical.

In addition, "technical" criteria of success are becoming less important: It is not enough merely to fly hardware successfully; *de facto* standards will be set through competition, especially if users are allowed to choose. The fact that the Shuttle is a great technical success[6] and copied by the Soviets is less important than whether users believe that it can take their payloads into space reliably and efficiently, compared to the alternatives. One user, the U.S. Air Force, has decided that the Shuttle is not its vehicle of choice for many payloads,[7] and Giacconi has described in this volume how scientists want to be free to choose other launchers. In the beginning it was enough merely to get into space. But reliability and efficiency become more important once the uses of space have been demonstrated. The barnstorming era described by Roland cannot last forever.

Another performance criterion falling into disrepute is "space leadership." By itself "leadership" is directionless; if unqualified it allows others to set directions. In the past we were clearly leaders but others have caught up, in part because we deferred payoffs.

In summary, our space program has emphasized technical challenge and performance -- Oppenheimer's "sweetness" -- over real and solid utility. The policy research problem is to devise ways to stimulate and challenge the people in the program to achieve both technical improvement and cost-effective utility. Because the latter is more difficult, it could be even more challenging and stimulating.

(3) *Support for a manned space program* is broad and continuing, neither fragile nor intense in the general population. Opposition to specific manned activities arises when they are bungled or interfere with other activities. Nevertheless it seems that there has been a loss of nerve, a lack of faith in the popular support for manned space programs *per se*, and this doubt in turn leads program managers to overpromise.

The prime example is the Shuttle, which was overjustified as a cargo booster. This overjustification meant that science missions were forced to fly on Shuttle. Making this manned vehicle serve as our only launch provider and thus as the only way for science payloads to get to space harmed the overall program in three ways: Astronauts were put fatally at risk merely to launch cargos such as unmanned science missions and communications satellites; the Shuttle was overcommitted and faltered;[8] and a million pounds of science payloads were idled.[9]

A similar lack of faith in public support is manifest in the Station program which now offers something to large numbers of diverse users and contractors in many congressional districts. This is one way programs become large, complex, costly, slow-to-fruition, and inflexible.

The point is that a manned program might be justified on its own, without connecting it to other programs and activities. The research problem is to decide whether and how this might work. A low risk way to begin would be to stimulate a broad, open, informed, and vigorous debate on this question.

(4) *Large projects* take longer to develop, which means they may become technically obsolete before they are flown. In this way old technologies get locked into flight projects, as Wheelon and Giacconi each have described. This together with insensitivity to cost may make such large projects irrelevant or even counterproductive both to U.S. industrial competitiveness and to the education of the next generation of engineers and scientists.

Large-scale goals do not necessarily require large-scale, centralized projects. Some large-scale goals can be realized efficiently by small evolutionary steps which take advantage of feedback to delete unpromising alternatives and to develop the promising ones. In situations where we can neither predict nor control the future of a project nor the environment on which it depends, such an evolutionary approach is probably the best course.

There are assertions that the observed trend to larger science missions is inevitable, that it results from the nature of space science.[10] A research question: Is this true, or is the trend a result of budget and procurement policies, incentive structures, and other factors not directly related to space science per se? Projects like Station might be kept smaller by empowering users -- for example, by expanding the opportunity for them to advocate smaller projects that are quicker to fruition. More generally the questions are, what drives projects toward giantism in our system, and how can it be changed?

Some projects, such as a manned mission to Mars, are unavoidably large. A research question is how to modularize them so they can be accomplished effectively even if problems arise. Initially, modules may cost a little more in the short-run, but in the long-run might avoid the domino effect of costly failures rippling through a tightly integrated system. How much modularity is enough, and how can we achieve it?

(5) *Single-point failures* should be avoided where possible in programs as well as in physical systems. The decision to make Shuttle our sole launch vehicle put a large, unnecessary single-point failure mode into the U.S. space program. Similarly the decision to plan the Space Station so that it can be assembled and serviced only by Shuttle puts *the same* single-point failure mode into the Station program. There is a significant chance that a Shuttle accident will lose a Station assembly payload, and as of now there are no spares provided in the program plan. It is even possible that if certain assembly elements were lost, the partly-built Station could re-enter.

Another example is TDRSS. When our ground tracking stations are shut down the U.S. civilian space program will be relying largely on TDRSS to control spacecraft. Our ability to communicate with spacecraft that have degraded attitude control will be greatly reduced. And we will have reduced redundancy in the U.S. space program.

In each of these cases short-term budget considerations have been a major driver in the decision not to provide backups. If everything works as planned, the costs of backups are avoided. However, the aftermath of the Challenger accident showed that the costs of failure can be extremely large when there is no backup. Some redundancy is often efficient and effective.

In the recent past plans have assumed success, and when the inevitable failures occurred Congress has delivered the funds to fix the program or the program has simply suffered (as is the case of space science after Challenger). This was not always the case. In the early planetary program missions were typically planned in pairs so that if one spacecraft failed the other could serve. As mission costs escalated this practice was dropped.

It may well be that the best decision is to tolerate the risks implicit in the decisions to build in such single-point failures, but the risks and costs should be openly debated -- these are not merely technical decisions. Research could inform such a debate.

The science community is well aware of this situation and has proposed approaches at least to mitigate it. The core program for planetary exploration is one example.[11] There are at least two kinds of research questions. The first has to do with risk tradeoffs, basically asking the question of when the risk of failure becomes too costly. As planetary missions become rarer, failure becomes a bigger risk to the community. The second kind of question asks why the system works as it does. What drives decisions to rely solely on Shuttle? Why are alternatives dismissed? Why is open debate avoided?

(6) *Competition* with our allies, not just the Soviet Union, is increasing and must be dealt with. Competition may directly and simply affect outcomes: For example, Ariane's effectiveness and aggressiveness may determine whether a commercial U.S. ELV industry survives. Competition may also compound other problems. For example, large projects may be too slow to adapt to competition. Thus the Europeans may learn what they need to know from our Station program, then develop an independent, low-cost alternative, leaving us with massive, expensive infrastructure in space and an institution needing lots of attention and support.

Foreign competition presents many intriguing research questions. The first is, what can we learn from our competitors? Our allies at least are quite open and we could learn how they operate. Second, are we willing to learn? That is, why can not or will not we adopt foreign practices or approaches that seem to work better than ours? Third, if we can't "join 'em," how can we "beat 'em"? That is, does our system have any underutilized strengths? Fourth, are there some areas in which we should yield to the economist's concept of comparative advantage and merely buy from foreigners? For example, when earth resource images are needed should we buy them from SPOT-Image and Soyuzkarta?

(7) *Efficiency* has been sought through centralization and integration (interdependence) of projects and through economies of scale (large size). Thus unwittingly our program has adopted

a Soviet model, vitiating the strengths of our system -- individual initiative, redundancy, and flexibility. The perverse result is that we have become less efficient. We need to learn from ecology, as Brewer pointed out, that successful systems tend to be those with natural redundancy and resilience.

Operating the Space Station will fundamentally change our understandings of what is feasible and worth doing in space. That is, when we have done experiments on the Station we will find some things more interesting than others. Surely we will find some unanticipated things we want to do. (If we don't it will be, at best, disappointing.) In other words, by definition and by intent we can't tell beforehand what our interests in space will be after we operate the Station. We can only know in advance that they will be different.

But assuming we follow our present track, by the time we learn what is truly interesting we will have spent at least $30 billion and will have a very expensive facility to operate in space. In turn this will generate great pressure to decide that the most interesting activities are the ones that can be done on Station, somewhat analogous to the pressure to put science payloads on Shuttle.[12] Already astronomers are being advised to design observatories that can fly on Station.

Clearly a less risky alternative approach would have been to evolve from Shuttle, to Extended Duration Orbiter, through various Man-Tended Platforms, to a Skylab-type Station, and then to a larger, perhaps permanent Station, learning as we go. In the past, a lack of competition and abundant resources have allowed us to proceed despite our programmatic inefficiency. Neither condition prevails now. How can we take advantage of our strengths to proceed more efficiently? Should we enlarge our criteria of efficiency to encompass more than merely the efficient use of existing or planned space resources?

(8) *Emphasis on operations* may conflict with NASA's charter as an R&D agency. With Shuttle, the great observatories, the Centers, TDRSS, and Station are in place, most of NASA's budget could be devoted to operating infrastructure. Of course the infrastructure supports R&D, but it tends to develop a life of its own, as Waldrop has pointed out. The tendency is for infrastructure operations to increase their share of resources in a ratchet-like manner because they are seldom terminated, and because new programs are difficult to start under severe budget constraints.

The trade-off between R&D and operations can be detrimental to both. For example, if engineers want to continue to make improvements to Shuttle and this conflicts with launch schedules then both the improvements and the operations might be adversely compromised.

Should the civilian space program be split to have one agency for operations, another for R&D? Splitting the program might give users more freedom in choosing launch vehicles. On the other hand Smith, in this volume, argues for again consolidating all civil space activities in NASA.[13] These are issues meriting research.

(9) *Consideration of alternative* policy and program choices is important. At present most policy and program analysis is done within NASA and with emphasis on technical considerations. The agency has the initiative: Typically other players react to what NASA proposes or does. Although within NASA alternatives may be considered at an early stage, every effort is made to solidify an agency position before a proposal surfaces and is presented to OMB or Congress. For example, alternative approaches to a Space Station were held close and Congress was presented with the approach of building all at once a large, central facility. A measure of the strength of support for NASA programs is the fact that they are largely supported in spite of the take-it-or-leave-it form of presentation.

Other entities, perhaps even commercial ventures, could be enabled and encouraged to introduce competitive alternatives. This is presently resisted by the program culture. Another way to generate alternatives might be to encourage competition between NASA Centers. The idea of making NASA Centers more independent -- somewhat like the DOE national laboratories -- has been raised.[14] Such a step might *allow* intercenter competition. A further step would be to *encourage* competition, not just in proposal-writing but also in results achieved for resources expended.

Such intercenter competition could turn the present organization of the space program into a major asset. The duplication of effort would be justified by an increase in efficiency through competition. In this volume Wheelon has described how competition between Department of Energy labs has helped our national strategic weapons programs. Certainly a basis for the strength of most U.S. science is competition in the peer-review process for project funding, which takes into consideration the previous accomplishments of the principal investigators.

There is a need for research to evaluate these and other ways of generating alternatives. Certainly any proposal to restructure NASA should be carefully explored. Part of the task is to understand more deeply the reason why few alternatives have been generated.

(10) *Policy planning and appraisal* should be emphasized in the space program. Of course there is great emphasis on technical planning of missions, but as discussed above little open consideration of alternatives or strategic planning.[15] Similarly there is a great deal of technical evaluation of hardware, but little program evaluation.[16]

Criticism of programs and policies is not necessarily destructive, even if it appears to threaten a fragile consensus within or outside the space program. A fragile consensus is unlikely to last and, in any case, hiding flaws eventually destroys credibility. Neither the public nor Congress expects perfection unless it is promised -- either explicitly or implicitly through lack of debate. Defensiveness and inflexibility only dissipate political support.

Given the problems discussed above, it seems likely that civilian space policy would be improved if a more vigorous planning and appraisal capability were organized and encouraged within NASA and left open to participation by outside groups. This would provide some redundancy in policy-making, which as Wheelon argues is just as important as redundancy in hardware. How to achieve this is a policy research problem.

BROADENING RESEARCH

We recommend that the foregoing problems of civilian space policy be considered in broader perspective.

Space policy research traditionally has proceeded with little regard for parallel experiences in other areas of policy, or for the various principles of policy inquiry distilled from such experiences. Meanwhile, over the past two or three decades, a new discipline of public policy has begun to emerge from the traditional policy-relevant disciplines.[17] It provides a rich store of practical and theoretical insights into problems of decision and decision process in any area of policy. For space policy, it suggests new observations, new interpretations, and new alternatives that are very much needed in the post-Challenger era, and broader perspectives from which space policy can be reconsidered.

To illustrate what we have in mind, consider rationality as a major theme underlying the ten problems reviewed in the previous section. In the emerging discipline, a rational decision is one that selects the action alternative expected to realize the greatest net benefits. It therefore depends upon the *alternatives* available and upon two kinds of estimates: Estimates of the future *consequences* of each course of action, and estimates of the future *preferences* (or goals) for evaluating those consequences.

Modern theories of rationality recognize, however, that humans are limited; that is, human beings are at best "boundedly rational."[18] For any policy decision, no one can imagine all the possible alternatives, resolve all the uncertainties and ambiguities inherent in the two estimates of the future, and then make an objectively rational decision -- or even come close. Instead, each of us considers carefully only subsets of the possible alternatives, consequences, and preferences, with various degrees of creativity and dependability. And different groups of people tend to consider different subsets in different ways according to the special interests they share. Consequently, in an open and competitive political arena, decisions tend to be controversial and the alternative eventually selected is at best an hypothesis about what is rational.

Improvements in rationality depend upon recognizing this reality and taking advantage of it. More alternatives can be generated if competing interest groups have both the incentive and the ability to take broad and ambiguous goals -- the only goals likely to be widely accepted -- and resolve them into programs that reflect their respective interests. Estimates of future consequences and preferences can be improved through action on multiple alternatives as quasi-experimental trials, conducted in series or in parallel. Decisions can be reconsidered and modified if the commitment to any one alternative is contingent upon the test of experience and limited with respect to the time and resources required to obtain that experience.[19]

In short, since we cannot know in advance what is an objectively rational decision, then we should at least evolve better approximations through procedures that are self-correcting. Such procedural rationality is implicit in the design of the American political system.[20] It is also implicit in the various procedures of modern science, including experimental methods and continuous peer review.

In public policy as in science, the best answers are not handed down from the keepers of a vision. The best answers are those which survive the tests of experience and open debate among informed representatives of different viewpoints. Institutions based on such self-correcting procedures are more rational than any single group participating in them -- provided the participants respect such institutions and allow them to work.

NOTES

1. Both the Apollo and Post-Challenger paradigms are described in the first chapter of this volume. The Apollo Paradigm, briefly, is the set of attitudes and practices, the institutional culture, that developed in the U.S. civil space program largely as a result of the Apollo Program. A core assumption is that large manned space missions are necessary to maintain political support for the civilian space program. The Post-Challenger Paradigm remains to be completely defined but is, or hopes to be, characterized by a more rigorously critical analysis of programs in their actual context.

2. If Shuttle is 98% reliable, the simple probability is 50% that there will be an accident in the next thirty-five flights, i.e. in 1993 before first element launch of Station.

3. Webb, James E., *Space-Age Management: The Large-Scale Approach* (New York: McGraw-Hill, 1969).

4. The Tracking and Data Relay Satellite System was financed by a loan from the Federal Financing Bank which NASA is now paying off, with interest, from its limited budget. See U.S. House of Representatives, Committee on Science and Technology, 98th Congress, *United States Civilian Space Programs, vol. II, Applications Satellites*. Serial M (Washington, D.C.: U.S.G.P.O., May 1983), p. 372 ff.

5. Because funds for government programs are appropriated, costs and cost overruns are treated politically, not economically. Annual spending must fit within the amount appropriated. However, if a project runs into difficulty, the typical response is "stretchout," pushing work into future (appropriation) years, which inevitably raises the total cost of the project in order to hold down the cost in the current year. Most contracts for space projects are (for good reasons) "cost-plus," and lack strong incentives to keep costs down. Finally, the natural tendency for government managers is to ask for *more*, not *less*, money from the appropriations process. See papers by Wheelon and Waldrop, this volume; see also (a) Space Exploration/Cost, Schedule, and Performance of NASA's Galileo Mission to Jupiter, GAO/NSIAD-88-138FS. (b) Space Exploration/NASA's Deep Space Missions are Experiencing Long Delays, GAO/NSIAD-88-128BR. (c)

Space Exploration/Cost, Schedule, and Performance of NASA's Magellan Mission to Venus, GAO/NSIAD-88-130FS. (d) Space Exploration/Cost, Schedule, and Performance of NASA's Ulysses Mission to the Sun, GAO/NSIAD-88-129FS. All U.S. General Accounting Office, May, 1988, Washington, D.C. These GAO reports document typical cost overruns by factors of two to three.

6. J. Fletcher, Proceedings of the Fourth National Space Symposium, U.S. Space Foundation, April, 1988, Colorado Springs, CO, p. 160. "To be sure, the Shuttle has done what it was meant to do; it remains the most versatile, flexible, and useful flying machine in the world."

7. This is reflected in their decisions to develop, purchase, and use the Titan IV Complementary Expendable Launch Vehicle (which is approximately equivalent, i.e. complementary, to the Shuttle) and the Delta II and Atlas-Centaur II. See B. Davis, "With its Titan IV, Air Force at Last Takes Helm of Space Program, Putting NASA in the Backseat" *Wall Street Journal*, November 29, 1988, p. A20.

8. Of course this refers primarily to the Challenger accident, but the Rogers Commission report makes it clear that the Shuttle system was breaking down under launch pressure. See Presidential Commission on the Space Shuttle Challenger Accident, *Report to the President*, June 6, 1986, Washington, D.C., Chapter VIII and Appendix J.

9. W. Hively, "A Resurgent NASA Woos Scientists Back to the Space Program," *American Scientist*, March-April, 1989, p. 132.

10. NASA Advisory Council, Space and Earth Science Advisory Committee, *The Crisis in Space and Earth Science* (Washington, D.C.: NASA, November, 1986).

11. Solar System Exploration Committee, NASA Advisory Council, *Planetary Exploration Through Year 2000; A Core Program* (Washington, D.C.: U.S.G.P.O., 1983).

12. This attitude was clear in the agency. For example: "In the coming decade, scientific investigations conducted in earth orbit will be the most important because these take the best advantage of the unique properties of the Shuttle." H. Mark, *The Space Station* (Durham: Duke University Press, 1987), p. 239.

13. A NASA advisory committee is also reported to recommend this approach. See *Defense Daily*, May 26, 1989, p. 321.

14. Committee on Space Policy, National Academy of Sciences, National Academy of Engineering, *Toward a New Era in Space* (Washington, D.C.: National Academy Press, 1988).

15. "Strategic planning is a structured, ongoing process that

systematically identifies an organization's mission and establishes the goals and objectives that need to be achieved to accomplish that mission....strategic planning has not yet been fully implemented throughout the agency....NASA has not yet developed an agency wide strategic plan." From U.S. General Accounting Office, *Civil Space: NASA's Strategic Planning Process*, GAO/NSIAD-89-30BR (Washington, D.C.: November, 1988).

16. There are, however, a few outstanding examples of program evaluation: For example, in reports that followed the Challenger accident; see (a) Chapters VI, VII and VIII of the Rogers Commission report, (see note 8); (b) Chapter II and V of Committee on Science and Technology, U.S. House of Representatives, 99th Congress, *Investigation of the Challenger Accident*, House Report 99-1016 (Washington, D.C.: October 29, 1986), p. 3; "...NASA's drive to achieve a launch schedule of 24 flights per year created pressure throughout the Agency that directly contributed to unsafe launch operations." p. 119; "The Congress and the Executive Branch jointly developed the policy that the Space Shuttle should...provide for most of the Free World's space launch needs. By and large, both Branches failed to appreciate the impact that this policy was having on the operational safety of the system." (c) NASA Advisory Council, *Report of the Task Force on Issues of a Mixed Fleet* (Washington, D.C.: NASA, March 1987). This report recommends that "The Shuttle must be recognized as a national resource of enormous importance to the U.S. space program. NASA policy should evolve from one that has maximized the use of the STS [Shuttle] to a policy that preserves the Shuttle for those missions requiring its unique capabilities." See also *The Crisis in Space and Earth Science* (note 10). This report is in effect an evaluation of our national space and earth science program.

17. See for example, such journals as *Policy Sciences*, the *Journal of Policy Analysis and Management*, and the *Journal of Public Policy*, which represent distinguishable components of the new discipline.

18. See, for example, Herbert A. Simon, *Reason in Public Affairs* (Stanford: Stanford University Press, 1983); and James G. March, "Bounded Rationality, Ambiguity, and the Engineering of Choice," *Bell Journal of Economics* 9 (1978), pp. 587-608.

19. Under a budget constraint, a limited and contingent commitment to any one alternative also increases the feasibility of similar commitments to multiple alternatives.

20. See, for example, Martin Landau, "Redundancy,

Rationality, and the Problems of Duplication and Overlap," *Public Administration Review* 29 (1969), pp. 346-358.