State of the Profession Study Groups

A white paper submitted to the Decadal Survey Committee

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State of the Profession Position Paper
Primary Panel: Facilities, Funding and Programs (FFP)
Secondary Panel: International and Private Partnership (IPP)

1 Introduction

The past decade has produced great new observational results in astronomy from ground based and space facilities and has posed fundamental new questions in cosmology and physics which give us exciting prospects for the future of the discipline.

US astronomers and astrophysicists have played an important role in these advances and all of us hope they will continue to do so in the future. I assume that the questions posed to the State of the Profession Study Groups are intended to insure that there will be favorable conditions for this to happen. While I can not contribute quantitative data to these studies as currently structured, I would like to express some concerns which could contribute to the discussion. I will primarily comment on institutional structures and on management issues.

My main concern is the loss of technological and instrumentation capabilities by the US astronomical community. The trend in the last decades has been towards a majority of astronomers becoming largely data analysts, with only a small minority receiving the training and exposure necessary to build the new telescopes and instruments of the future. Many of my concerns are directed towards improving this imbalance.

2 Institutional Structures

We can recognize several institutional structures which are relevant to astronomical research: Universities, Academic and Research Institutions (including FFDRC such as SAO), The National Academy of Sciences, Funding Agencies (primarily DOE, NASA and the National Science Foundation), National Observatories, NASA Centers, and International cooperative efforts. It would be impossible to comment on the full gamut of interactions which take place within and between these institutions, thus I will restrict my comments to issues for which I have direct personal experience and that I believe are important for astronomy.

2.1 Universities and Academic and Research Institutions

Academic Institutions in the US have continued to provide outstanding training in physics and astrophysics. However Academic Institutions in the US have not been able to develop on their own a unified, cooperative, national vision which would guide activities at the national level as well as their participation in international ventures. Such a vision would presumably be developed according to the best approach for the pursuit of scientific excellence. The closest effort toward such an approach is represented by the decadal surveys of the NAS.

However while these surveys are extremely worthwhile they are purely advisory to the Academic institutions and to the funding Agencies (NSF, NASA and DOE). Other factors come into play. The academic institutions have come to depend more and more on NSF and NASA grants for their research funding; this has resulted in a lack of disposition to critically evaluate the policies, strategies, management competence and goals of the federal institutions in the execution of the programs. This same reluctance does not affect only individual scientists and Universities, but also the scientific advisory committees of the agencies and the National Academy of Sciences as a whole.

The net effect is that the Survey itself becomes a tool for marketing by the federal agencies of the programs which they find to be in accord to other institutional or political goals, and the scientific priorities established by the Survey are disregarded when they prove inconvenient. I hope that the US astronomical community will use the opportunity of this

decadal survey and the climate of greater interest in science and technology that seems to prevail in the new administration to ask some of the fundamental questions that need to be addressed, for example:

- How can the Decadal Survey separate itself from the self interest of the agencies that are funding the survey?
- How can the committee avoid issues of conflict of interest?
- How can the survey assure that its priorities are respected in the programs of the agencies?
- How can the performance of the agencies be monitored?
- Have the funding agencies structured programs which are suited for the effective achievement of excellence in science and education in the short and long run?

2.2 National Academy of Sciences

The NAS has accepted as its role that of advising the funding agencies on strategic goals for astronomy. It's most important contribution, for which the decadal surveys have been highly esteemed, has been its ability to enlist the cooperation of the community in establishing priorities for a program that cuts across the many sub disciplines in astronomy. However NAS has taken no responsibility to insure that the priorities thus established are maintained through the many vicissitudes which occur during the execution of the programs, and it has in fact allowed ad-hoc NAS committees to validate changes in the program priority at the urging of particular groups strongly supported by the funding agencies, without a recourse to the more general consensus by the astronomy community which characterizes the surveys.

Furthermore the program of scientific studies of the decadal survey itself is being influenced by the decision of the funding agencies to fund studies of missions in which they feel a particular interest to bring them to a higher level of preparation and give them a competitive advantage over equally worthwhile missions. This bias becomes particularly important when issues of cost are being considered. The funding agencies would like the community to believe that cost estimates, and feasibility plans that they have studied are more certain than those produced by individual groups of scientists or institutions. The record of performance shows that this is not the case.

Thus it does not seem inappropriate to ask NAS to pay attention not only to the overall scientific strategies that are proposed but also to the institutional and societal conditions that can make those goals achievable. Two specific examples are the current approach to large optical telescopes and the expense of large NASA programs conducted largely within NASA centers. It is useless for NSF to advocate a generation of 30 meter optical ground based telescopes, which we know will lag behind what is planned in Europe. The community should ask for a truly national ground based telescope (competitive with the ESO 42 meter) and recommend that no NSF money should be given to the 2 "private" 30 meter projects, instead all that the NSF can spend should be concentrated in only one facility. If NASA continues to carry out programs as expensively as it is currently doing in its centers the net result is large waste. If NASA could cut its currently planned inefficient expenditures by 1 billion/year, one could devote that money to smaller and medium programs of great scientific interest. Here we are fighting in house NASA institutional interests and the Survey

Committee should ask NAS to look into it. In what follows I will pose some of the questions that I consider appropriate.

2.3 National Science Foundation

During the last decade the US has lost its dominance in ground based optical astronomy which it had enjoyed since the construction of the 5 meter telescope at Palomar. The 10 meter Keck Telescopes that continued the tradition of private funding of the largest optical facilities in the US, maintained the lead till the late 90s, but lost it with the completion by the European Southern Observatory (ESO) of the Very Large Telescope. This was due to the comparable total area of the 4 telescope array(slightly larger than Keck I +Keck 2), but even more to the superb complement of instruments provided for the telescopes, the state of the art operations and data handling and archival system, and the interferometric capabilities of the array.

This event places in question some of the sociology of ground based optical astronomy in the US. The telescope mirrors on which so much attention has been focused in the US turns out to be a relatively small part of the cost of a fully operating facility like HST or VLT. Thus the cost of a competitive facility in a world wide context may well exceed what can be afforded by a single university, and this would seem to offer a great opportunity to NSF to provide the leadership toward a truly national facility for the US, just as ESO provides that leadership for Europe.

It appears however that this opportunity is being missed by NSF. In order to satisfy regional or institutional ambitions two 30 meters telescopes are planned to be built in the US to compete with the planned 42 meter ESO telescope (the advantage goes of course as the square of the diameter or a factor of 2). Given its experience with VLT, ESO is very likely to be able to successfully complete this project and to produce the necessary data system and instrumentation that will consent its proper scientific utilization. Thus Europe will continue to provide the largest optical ground facilities for at least the next two decades. The race between the US 30 meter telescopes, and the ESO 42 meter is much closer than it was thought. What the US should do is to let "private collaborations" build one (in collaboration) or two 30 meters as fast as they can, and start on a national telescope bigger than the 42 meter, properly equipped and operated, to be completed soon thereafter.

The question arises as to what the NSF sees as its mission in astronomy. I would assert that it is to provide the US community with facilities that go beyond those that can be constructed and operated by private institutions, to compete and/or cooperate on the international scene. This does not seem to be what NSF considers as its task. In the past it has treated its National Observatories with a great deal of neglect.

The NOAO has forever designed and built facilities inferior to those available through private financing and was essentially excluded from the design and execution of the most recent project GEMINI, thus loosing technical and managerial competence. NRAO has maintained with great efforts and inadequate funding its technical competence with VLA and VLBA, and is currently been given the opportunity to participate in a world-wide project of the first rank: the Atacama Large Millimeter and Submillimeter Array. What has been missing at NRAO over the years has been the continued support for data analysis and operations that will be crucial for the full utilization of ALMA. Will this deficiency be made good by NSF in the future?

A very important question regarding the governance of NSF has to do with the responsibility of the National Science Board toward the scientific community in setting NSF policies and in providing oversight and accountability of the program execution.

2.4 National Aeronautics and Space Administration

The mission of NASA has always been understood to include both manned space exploration and support of scientific space research. It is quite clear that grievous errors have been made by NASA in the technical and strategic choices regarding the implementation of the manned program. Perhaps one of the few illuminated insights of the Bush administration has been the recognition of the futility of pursuing the Shuttle and Space Station Programs. But the pursuit of these dual goals has negatively influenced the space science program for decades.

As a simple example we can recall the decision to make HST serviceable by Space Shuttle. It is true that manned servicing of Hubble was one of the most useful missions of the Shuttle and that it restored and improved the HST capabilities with great scientific results and with concomitant reaffirmation of the utility of man in space to enable scientific missions. The truth of the matter however is that a new fully automated Space Telescope with current technology could have replaced Hubble at a fraction of the cost, and in a better orbit. Such is the case for Chandra and Spitzer.

The point here is not that the programs carried out by NASA in Space Science are wrong, but that in order to satisfy other than scientific priorities they have become so very expensive as to limit severely the number of missions that can be carried out. The very large missions such as JWST on which NASA has focused its attention are carried out by NASA centers which have experienced delays and overruns thus adding to the costs. Due in part to these circumstances, NASA has not achieved the balance between very large, medium and small missions that the community has advocated for years. This lack of smaller, principal investigators and university led missions makes it very difficult to train the experimental scientists of tomorrow.

While for NASA it sufficient to obtain success in any of its missions to claim a successful overall program, this does not take into account the need for the different disciplines of astronomy to maintain a minimum of continuity to remain healthy. Thus NASA's interests and those of the astronomical community are often at variance; one of the most disturbing trends is the shift of NASA center from enablers of academic research to well funded competitors.

I have personally advocated a separation between Space Science and Space Exploration since the mid 80s and I believe that space science would be better served by the creation of a National Space Science Institute along the lines of NIH, which in the past has devoted 80% of its budget to extramural research. Such a National Institute would include the scientific portions of the NASA activities (including the existing NASA centers), but it would endeavor to carry out most of its research out of house, following the NIH example. With such a structure a Space Science Institute would attract people into science and technology, and allow mobility for these scientists and engineers across academic institutions, to train and educate as well as carry out research. I believe that this separation would also better serve manned Space Exploration by focusing NASA's attention on a clearer strategic plan and achievable short and long term objectives with careful planning on an incremental program at a level of effort basis.

I believe that this decadal survey gives us the opportunity to better study this issues and

to request a greater participation of NAS in setting up the institutional arrangements for a more efficient and productive space research, as was done successfully in the 70s regarding the institutional arrangements for space telescope, but on a much broader scale.

2.5 National Observatories

I have already discussed some of the issues regarding National Observatories under NSF. I find in general the following problem areas:

- Lack of clarity on the purpose of the observatory reflecting the lack of clarity at NSF. Are the Observatories to simply provide for the have- not or to lead the national enterprise? National Observatories (particularly optical) have accepted to play a secondary role in the astronomy of the country.
- Weak governing boards. Many astronomers on these boards have severe conflicts of interest, having to do with competition between their institutional agenda and the national needs.
- Weak management exacerbated by the lack of clarity at NSF, the lack of a stable budget, the lack of a common vision. NSF wants to keep the largest number of scientists happy, the Boards leave the setting of policies to the Observatory Director, and none insists on prioritization of scientific programs. When funds are scarce the reaction is always that of protecting jobs rather than cutting programs.
- Poor overall management. Lack of appreciation of the need to perform work within time and funding constraints in order to maximize science. This is further exacerbated by NSF attempts at bypassing management, without the assumption of responsibility.

2.6 NASA Centers

The assignment of tasks to NASA centers is primarily due to institutional or political interests. Past performance and experience does not seem to play a role in the selection. It is important to keep in mind that performance either with respect to time or resources is not of particular interest to the Center management whose main concern appears to be to provide continuity of work for its numerous staff and it's equally numerous subcontractors; thus there is a conspicuous lack of incentives for efficiency and punishment for failure. I would like to add that with notable exceptions there are very few capabilities which now exist in NASA centers which are not available in industry or academic laboratories.

Over the years there has also occurred a very substantial shift in the attitude of some of the NASA centers, particularly GSFC, which see their role not as enablers of scientific research by the community, but as competitors using their privileged position to advance their own research agenda. The real problem is that by utilizing the scarce available resources toward these larger and larger in house NASA programs there is less and less left for medium sized and small programs led by individual principal investigators or institutions, which are vital for the continued health of the profession. The reduced opportunities at academic institutions leads to smaller departments, less interest in science and technology, and an overall reduction in the training of the next generation of scientists and engineers to build and operate the facilities of the future. Thus the cost of these very large programs must be

reduced to make room for an expanded explorer program which includes mid sized missions particularly directed to academic and research teams.

I think that what is necessary is a revaluation by the scientific community (NAS?) of the role of NASA centers in space science.

2.7 International collaborations

It is necessary to distinguish between NASA and NSF performance in international cooperation.

2.7.1 NASA

In general NASA has been reasonably successful. Cooperation with other space agencies are normally based on memoranda of understanding which define the contributions expected from each side. Each side follows its own national laws, contractual arrangements etc to provide the necessary hardware, software and operation support. The necessary interfaces are spelled out and administered through joint ad-hoc teams also spelled out by memoranda. Such arrangements have been quite successfully employed in the case of HST. They work in part because one or the other side has more than 50% of the cost and takes the bulk of the responsibility for integration.

The only substantial area of weakness (some see it as an advantage for the US) has to do with the reluctance of NASA to promote these understandings to the level of treaties, which would require involvement by the Department of State and ratification by the Senate. The problem for our foreign partners is that NASA memoranda obligations can be changed unilaterally without penalty. For the US side, the problem is that there is no obligation by the Congress or the Executive to continue funding of the programs.

We are now entering an era when international cooperation becomes essential in carrying out the very large and long term programs which are undertaken, and the US role may also become less dominant. Clear understandings on the responsibilities and rights of the partners and of the obligations of their governments will be more important. A case in point might be the IXO collaborative program, whose structure should be carefully studied prior to approval.

2.7.2 NSF

The performance of NSF in international ventures in astronomy has been quite poor. In the case of Gemini the attempt to build an observatory for the community without adequate funding for instruments and data systems, and without adequate management oversight has resulted in a loss for the community and for NOAO.

In the case of the the Atacama Millimeter and sub millimeter Array being constructed in Chile, the situation is improved since the participation of the foreign partners has set a high level of performance and of operations as a requirement. Still the ALMA governance was established by an agreement between the ESO and NSF(the agreement was later expanded to include Japan), largely following the GEMINI agreement, and it establishes a Governing Board, which is an open invitation for management interference by a group that bears little or no responsibility or accountability for its actions. The trouble in the program execution occurs when NSF (which alternates with ESO in the chairmanship of the Board) agrees to decisions of the Governing Board which affect the scope or the duration of the program, but does not recognize its responsibility for the programmatic and monetary consequences.

Delays in procurement decided by the Board have for instance increased substantially the cost of ALMA, as was recognized by the appropriate review committee.

3 MANAGEMENT ISSUES

It is characteristic of scientists to consider management as a necessary evil whose function is not well understood. In general this is a reflection on the performance of the management entities with which they have come into contact. Their management has typically not helped with the job at hand. In part this is due to the fact that their management has focused on recovery of cost rather than on the support the entire gamut of the institutions activities. Yet one of the biggest problems we face today in the execution of the Astronomy programs is represented by the continuously escalating costs of the missions, which severely limit the science that can be pursued.

It is my opinion that a great deal of these costs increases are due to causes on which the people that execute the actual program have little or no control. To clarify this statement let us consider only three elements of cost that should be considered in the planning phase of a program.

- Personnel costs are determined primarily by the number of staff required, the duration of their involvement, and possibly escalating salary costs.
- Hardware or software costs are determined by the scope of the program, its technical difficulty and careful planning of critical items acquisition and of the critical path of the program. Timeliness of procurement actions on the critical items is extremely important.
- Overhead costs and management costs. (Management costs can be substantial and are generally underestimated)

These costs can estimated at the beginning of a program under certain assumptions which are primarily: the technical difficulty, the availability of the staff, the ability to only pay for the staff during the time during which they are actually working on the project, the availability of funding when needed during the various phases of the program. The overhead costs are generally set by the proposing institution and the management costs must be evaluated at the time of the proposal to take into account the complexity of the program.

With these assumptions one can plan the most effective way to carry out a program. Provided one has the management capability to be promptly informed of problems as they arise and to devise work around approaches the costs can be maintained within a 10-15% contingency in time and money. The ESO Very Large Telescope was executed within these bounds .A great deal of the overruns and delays which occur are due to the breaking down of these assumptions or the inability to foresee and fix problems.

Some of the biggest problems results from the inability to hire and fire technical staff as needed. The marching army effect, as it is called, means that once the staff reaches a full complement a delay in the program will simply increase the personnel costs. This can be due to internal difficulties but it is most often due to outside causes: when a specific program gets in trouble both NASA and NSF have been extremely reluctant to cancel the program. In some cases the scientific scope is reduced, but most often it is not, which is due to the active lobbying of scientists and/or professional lobbyists in the pay of scientific institutions

who bring the case directly to congress. This then results in an overrun which can affect the conduct of all other programs in the queue.

Since both NASA and NSF tend to initiate more programs that can confidently be accomplished within the resources available to the agencies, this is guaranteed to happen quite often. It seems to me that in addition it has become customary to underestimate the cost of programs at the level of the proposal and of agency planning to receive a favorable acceptance, fully expecting to recover the real costs when the program is underway through effective lobbying.

The deleterious effect of these practices is hard to over- estimate. The setting and resetting of schedules due to lack of resources brought about by outside events makes a mockery of the attempts by individual organizations to do an efficient job, and affects all aspects of carrying out a program. Apart from the staff costs, the inability to place contracts when appropriate can also be very deleterious. It was the reason for the ALMA project cost increase of almost a factor of two. The continuous delays in the execution of JWST, as well as internal management problems, have increased its cost by a factor of ten with respect to the original (marketing inspired) NASA statements and a factor of several with respect to later estimates.

If we could ameliorate this situation we would all profit a great deal and better and more science could be done. One of the difficulties is that there is no punishment for lying or for incompetence, or any reward for competence. Thus there is no incentive for improved performance.

This situation severely affects the work of this decadal survey since we do not know who to trust as to the feasibility and costs of the proposed programs. I should add that in my opinion NASA and NSF do not have the expertise or perhaps the interest to assess these issues, as demonstrated by their record of performance. In trying to confront these difficult problems I have come up with a suggestion which could at least be discussed to focus our attention.

I would recommend that all costs of programs which are examined for prioritization should be understood to be carried out within *clearly spelled out cost limit* whether done by private academic or research institutions, National Observatories or NASA Centers.

As soon as the shock wears off, I would like to point out that this is not such a novel approach. The satellite Uhuru was carried out by my group at AS&E under a fixed price contract. The Sloane Survey in a sense was also carried out on a fixed donation, the rest of the cost being a problem of the proposing institutions. While the precise cost of the proposed programs could not perhaps be determined today, I would propose that the Decadal Survey only prioritize projects that would be negotiated by the agencies following this approach, both for those proposed by academic institutions or by NASA Centers or National Observatories.

The advantages of such an approach appear to me self evident:

- Fixed price bids would reduce the incentive to underbid and it would transfer the risk to the proposing institutions.
- We could institute a regime of zero tolerance for overruns except under extreme conditions when the advice of a body fully representative of the entire astronomical community would be sought (not the lobbying by interested groups or institutions)

- We could have accurate and reliable planning by NASA and NSF of their budgetary needs. Such bids would permit a fair prioritization by the Decadal Survey Committee.

- It would reduce the influence of politics or lobbying on programmatic decisions.
- It would create incentives for executing the programs effectively thus making available resources for the pursuit of additional research.(in the ESO VLT program the unspent contingency allowed the start of VLTI)
- It might help to reduce micromanagement by the agencies.
- It would secure a better awareness in the community of the responsibility we have to each other. Since the resources are always limited when we do poorly we not only damage our own program but astronomy in general. If we adopted this approach the effects could be much reduced.

Perhaps it might help to reestablish confidence in the capability of the funding agencies and the community to manage our multibillion dollar program.

4 Recommendations

1. Insure that the recommendations of the Decadal Survey Committee are followed in practice. Establish a mechanism for the continued involvement of the community in any re-prioritization of the Survey recommendations by the funding agencies. The Decadal Survey Committee (or at least a significant subset) should become a standing committee, charged to oversee the response of the funding agencies to the recommendations of the Survey, and to assure that the priorities of the Survey are maintained.

2. Request a study by the National Academy of Sciences of the conduct of Space Science, similar to the Horning Committee study for the Space Telescope, but much expanded in scope. The subject of this study should include: the role of NASA centers in science, their staffing levels, and whether their purpose is to be enablers or executors of research. It should also include the question of the separation of Space Science from Space Exploration, and the possibility of creating an independent Space Science Institute.
3. Request NASA to consider with high priority an expanded explorer program which should include medium class, PI lead, missions particularly directed to academic and research institutions, administered by them and to the exclusion(in general) of NASA centers.
4. Review NSF goals and objectives in astronomy. Consider the issue of NSF interference in the management of projects. Study the degree to which the National Science Board is representative of the scientific community.
5. Consider carefully the issues of institutional conflict of interest for scientific representatives on National Boards of Observatories, and on NAS, NASA and NSF advisory committees.
6. Introduce a fixed price philosophy approach.