# Astronomy and the Realities of the Budget

***The Field Committee Report set forth a remarkable consensus on research priorities; but other disciplines have priorities too***

When the report of the National Academy of Sciences' Astronomy Survey Committee* was released in the spring of 1982, it was immediately hailed as a premier example of scientific statesmanship. By hammering out a consensus on the new facilities needed in the coming decade—and by listing them in priority order—the astronomers seemed to have gained enormous credibility in official Washington. And given the success of the committee's previous report, in 1972, there seemed every reason to think they would have an inside track on funding for the 1980's (*Science*, 16 April 1982, p. 282).

Three years later, however, the astronomers' recommendations are having a rough time of it, both within the National Science Foundation (NSF), which is generally responsible for ground-based astronomy, and within the National Aeronautics and Space Administration (NASA), which is responsible for space astronomy. In retrospect it was probably too much to expect that projects would be funded just because a report had asked for them—especially when a lot of other disciplines have a claim on the science budgets. But the astronomers are frustrated nonetheless, as is reflected by George B. Field, former director of Harvard-Smithsonian Center for Astrophysics and chairman of the Astronomy Survey Committee. "They all embraced the report and said 'Hallelujah!'," he says. "But as the years ticked by, it became apparent that when they solve their other problems *then* they'll solve your problems. So the promises are empty."

Be that as it may, there are a number of similar priority-setting efforts under way in such fields as physics, chemistry, and the earth sciences. So it is worth taking a look at what really has happened with the Field Committee recommendations and what lessons might be drawn.

To begin with, the tradition of cooperative priority-setting is well established in astronomy. In 1972, the previous astronomy survey committee, chaired by Jesse Greenstein of the California Institute of Technology, produced *Astronomy and Astrophysics for the 1970's*; its recommendations were a key factor in winning approval for such facilities as the Very Large Array of radio telescopes in New Mexico, the Hubble Space Telescope, the Multiple Mirror Telescope, in Arizona, the Einstein x-ray satellite, and the Gamma Ray Observatory satellite.

Operating in that same tradition, the Field Committee developed a list of projects estimated to cost somewhat under $2 billion 1980 dollars. The major projects included, in order, a satellite observatory known as the Advanced X-ray Astronomy Facility (AXAF); a continent-spanning assemblage of radio telescopes known as the Very Long Baseline Array (VLBA); a 15-meter class New Technology Telescope; and a Large Deployable Reflector in space. Separate lists covered such moderate- and small-sized projects as an enhanced Explorer satellite program, and a submillimeter wave telescope. And finally, the report pointedly included a list of inexpensive but vital "prerequisites," such as theory and data analysis, computational facilities, and technical support.

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One reason this report was so well received in Washington was that it appeared in the midst of the Reagan Administration's early attempts to cut back on the federal budget. Agency administrators, faced with painful choices, were grateful for any guidance they could get. (Then NSF director Edward Knapp said at one budget briefing that he wished every community would produce such a list.) The report also fit in with the predilections of presidential science adviser George A. Keyworth, II, who was insisting that tight budgets would actually improve research if scientists would only quit expecting the government to fund everything and start setting some priorities for disciplines at the cutting edge. Astronomy, he added, was a prime example of the latter.

The upshot was that the astronomers seemed to be in better shape than most to weather the budgetary storms of the 1980's. But it has not quite worked out that way, for several reasons:

- Soaring costs and competition for funding. In relative terms, science budgets have not done too badly in recent years. But they are not growing all that much, either. Thus, as the cost of a state-of-the-art astronomical facility keeps going up, fewer and fewer projects are getting started.

At the same time, there are more and more claimants to the science budgets. NASA, for example, is trying to maintain its programs in astronomy, planetary science, and solar-terrestrial physics, while making room for expanded efforts in earth observations, zero-gravity materials science, and life sciences.

NSF, meanwhile, has been diverting money from astronomy and other fields into its new supercomputer initiative. "The NSF has done a lot to fund the Field Committee's prerequisites," says Peter Boyce, executive director of the American Astronomical Society, "but this year that's started to erode very badly."

Then there is the case of the VLBA, the Very Long Baseline Array. At an estimated cost of $61 million, the array will produce ultrahigh resolution imagery of quasars, galactic nuclei, and other objects; NSF thus asked Congress last year for $15 million to start construction of VLBA in fiscal year 1985. It was accordingly passed along through the committees without much comment—until June, when it reached the House Appropriations subcommittee chaired by Representative Edward P. Boland (D–Mass.). The VLBA money could not be obligated, said Boland, until NSF's science education budget exceeded 8 percent of the total.

"It was a gimmick to force the issue," admits a subcommittee staffer. "We've spent more than $2.5 billion on telescopes in the last 10 years and another $2.5 billion is in the works, while science education is slowly starving to death. VLBA was simply the next proposed telescope."

Whatever the merits of science education, the astronomers were not amused

**Astronomy and Astrophysics for the 1980's* (National Academy Press, Washington, D.C., 1982).

by the linkage. The result was a flurry of outraged letters and telephone calls; a spirited defense by Senators Jake Garn (R–Utah) and Pete V. Domenici (R–N.M.) (VLBA will be headquartered in Domenici's New Mexico); and expressions of grave concern by the House–Senate conference committee. In the end, a compromise was struck: fiscal 1985 funding for the VLBA was set at $9 million, to begin no earlier than April 1985. "The April date is so we can see if there are any recisions for science education [in the NSF's fiscal 1986 budget to be presented in early 1985.]," says the staffer. If there are, he adds, the committee will go after VLBA again.

- Obsolete assumptions. Of necessity, the Field Committee had to assume that projects already in the pipeline would be completed as planned. In practice, however, that has rarely been the case.

A prime example is SIRTF, the Space Infrared Telescope Facility. It started out in planning phases nearly a decade ago as the Shuttle Infrared Telescope Facility, a liquid-helium–cooled observatory that would regularly be carried aloft in the space shuttle cargo bay during the mid-1980's. The community was looking forward to it eagerly: infrared radiation is the ideal probe of such things as the cool gas and dust in the star-forming regions of the galaxy, although most of it, unfortunately, is screened out by the earth's atmosphere. SIRTF will thus have some 1000 times the sensitivity of infrared telescopes on the ground.

As a precursor to SIRTF, however, NASA had planned to fly a relatively low-resolution survey mission to map the infrared sky. And IRAS, the Infrared Astronomy Satellite, turned out to be one of the nastiest technical challenges in NASA's experience (*Science*, 24 June 1983, p. 1365). SIRTF thus fell into limbo while agency engineers struggled with its precursor's liquid helium cryogenics and recalcitrant detector arrays. "There was no way we could start a new infrared project before IRAS was finished," says Charles Pellerin, head of NASA's physics and astronomy division, "especially since it wasn't even clear what we would find up there with IRAS."

As it happened, IRAS was an enormous success. But that very fact changed all the assumptions that had gone into SIRTF. For one thing, the infrared sky turned out to be extraordinarily rich. "The infrared cirrus, the Vega cloud—for a while it was discovery of the week," says Pellerin. Clearly SIRTF was going to need far more than an occasional 5-day shuttle flight to follow up. At the same time, IRAS had operated beautifully in space for nearly 11 months, which made it possible to contemplate operating the cryogenically cooled SIRTF as a permanent, free-flying space observatory.

Add in the fact that the shuttle was turning out to be a terrible platform for astronomy—not only would the launch costs be exorbitant, but the shuttle's own thermal emissions would wash out any observations in the near infrared—"and the upshot," says Pellerin, "was that [project scientist] Nancy Bogess and I decided in March 1984 to make SIRTF a free flyer and get on with it."

The decision was accepted and generally applauded in the community. But it also meant accepting yet another delay while SIRTF was redesigned. And it meant an inevitable strain on intradisciplinary good will: SIRTF has now been delayed so long that it overlaps the AXAF x-ray project, and certain partisans have taken to backbiting and name-calling as they scuffle to see who flies first. The infrared community argues that the Field Committee assumed their mission as a given; therefore NASA should do SIRTF first and *then* do AXAF. The x-ray astronomers maintain that they haven't had data since the Einstein mission in 1979, and that their whole discipline is on the verge of becoming moribund. Says one congressional champion of SIRTF, "I've gotten so sick of listening to them that I'm beginning to think I don't care any more."

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In the middle, meanwhile, is Pellerin, who has responsibility for the program as a whole. "The fact is," he sighs, "AXAF is ready to go and SIRTF is not."

- The Hubble Space Telescope. The seemingly endless delays and the enormous cost overruns on this $1.2 billion project have effectively put the rest of space astronomy on hold. The last major astronomical new start in NASA was the Gamma Ray Observatory in 1981. And that will be it for several years to come. The outlays for Gamma Ray Observatory and Space Telescope are both now at their peaks, and in fiscal 1985 will total more than $300 million. AXAF will not get a new start until 1987 at the earliest, and SIRTF may have to wait until 1989.

Nor is the budgetary squeeze confined to NASA's big ticket items. For scientists at the universities the most urgent concern is for NASA's chronically low research and analysis budgets (R&A), which cover such things as data analysis and graduate student support. One result has been the creation of the Space Science Working Group, an ad hoc consortium of universities that is, in effect, a lobby for R&A: every year, the NASA budget comes out with R&A that the members believe is grossly inadequate, and every year they get Congress to put the money back in.

Quite a few of the scientists seem to imagine that NASA administrator James M. Beggs pencils out the research and analysis budgets every year as a gesture of pure meanness. In fact the cuts originate further down, at the working level in the science divisions. It is partly a budgetary ploy: why fight to get the money past the White House Office of Management and Budget when Congress will put it in anyway? But more serious, say division managers, are the long-term bureaucratic implications of R&A: start a hardware project and it eventually comes to an end; you can go on to something else. Increase R&A and you have added to a marching army of university researchers who will demand to be supported forever.

Looking back over all this, there are some lessons to be drawn.

First, it is obviously not enough to drop the finished report on a few credenzas. Somebody from the community has to be willing to make the case again and again, to follow up, to monitor progress—in short, to be a lobbyist. In this case Field himself has taken the lead. He was active in setting up the Space Science Working Group, and more recently, he has been involved in setting up an informal network of AXAF and SIRTF supporters who will make phone calls on a moment's notice. "The VLBA awakened me to the fact that we have to be ready in the future," he says.

Second, the recommendations are not carved in stone. Assumptions do have a way of proving faulty. And priorities, scientific or otherwise, do change. Keyworth, for one, has suggested that the community find some mechanism to review and update the Field Committee every few years.

Third, statesmanship cannot stop when the report is finished. Spectacles like the AXAF-SIRTF fight can make a scientific community look like just another set of contending interest groups. "It's worse than useless," says Giovanni Fazio of the Center for Astrophysics, a key supporter of SIRTF. Pellerin, for his part, plans to start presenting Space Telescope, the Gamma Ray Observatory, AXAF, and SIRTF as a unified package, a complementary set of space observatories spanning a whole wavelength range.

Finally, the community has to have realistic expectations about what is possible. There is simply not enough money in the science agencies to do everything at once, not when astronomy has to compete with geophysics, science education, and all the rest.

This is admittedly frustrating, especially since scientists by their nature care passionately about their work—and especially since, as in the VLBA fight, the federal funding process so often results in cross-disciplinary priorities being set for ad hoc and political reasons.

On the other hand, there is no realistic hope of seeing the science budgets rise much in the foreseeable future. It might help, a little, if the scientific community could find some more systematic way of making those cross-disciplinary tradeoffs—perhaps as a Field Committee writ large.

But then, no one has yet found a good way to do that. As one veteran of the NASA adsvisory panels says, "You're asking for something that no human being can accomplish."

—M. MITCHELL WALDROP

# Avoiding the Schistosome's Tricks

***Using the newest methods of molecular immunology, researchers are learning to induce immunity to schistosomes***

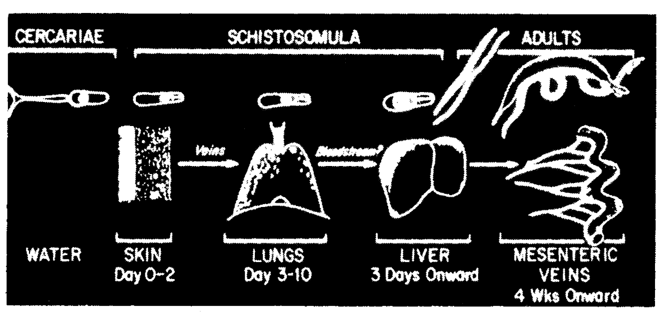
As far back as records of civilization go, there are descriptions of schistosomiasis. It is a disease that dates at least to 2000 B.C. in Egypt—the time of the pharaohs. There is even a hieroglyphic symbol for it, a penis dripping blood, which is a symptom of schistosomiasis. It is a disease that was so common in Egypt that blood in the urine was considered a puberty symbol for males. And it is a disease that even today afflicts 1 in 20 of the world's population—200 to 300 million people—in Africa, the Middle East, Central and South America, China, the Philippines, and Malaysia.

If ever an organism were well adapted to live in human hosts it is the schistosome, the worm that causes schistosomiasis. Once the schistosomes establish themselves, a person is literally defenseless against them. The immune system makes no headway whatsoever.

Like other parasitic diseases, schistosomiasis is now attracting the attention of molecular biologists and immunologists. Fascinated by the organism's ability to evade its host's immune system, investigators are searching for ways to trick the schistosomes and produce a vaccine against the disease. Their work has focused on monoclonal antibodies and anti-idiotype vaccines. Although no vaccine is imminent, all believe that one will come eventually. Because this research necessarily focuses on how the immune system is activated and inactivated, it is leading to new insights into control of immune reactions, including the isolation of substances that seemingly shut down portions of the immune system.

The current search for a schistosomiasis vaccine is only the latest in a long series of attempts to control this disease. From time to time, governments or organizations including the Rockefeller Foundation have made all-out efforts to eliminate the disease from certain parts of the world. Mao Tse-tung even tried to eradicate it from all of China and the Chinese today, disregarding patent rights, are manufacturing a new anti-schistosomiasis drug, praziquantel, and passing it out to their people. But none of these efforts has been completely successful. As Theodore Nash of the National Institute of Allergy and Infectious Diseases (NIAID) explains, "The problems with schistosomiasis are economic and political. If you had sewerage systems, if you had places to put feces, you wouldn't have schistosomiasis or you wouldn't have much."

The current best hope for stemming the disease is to use drugs and to try and prevent exposure. A number of public health experts are extremely enthusiastic about praziquantel, a drug that is effective against all species of schistosomes and is taken as a single dose. But praziquantel is expensive—it costs $30 per dose—and people, once cured, can be reinfected. Bayer, the drug's manufacturer is, however, selling the drug at cost, at $2 a dose, to Third World countries, in cooperation with the World Health Organization. Yet even $2 a dose is a lot of money for poorer countries. Moreover, if a country like Egypt spent $5 or $6 million on the drug to treat most of its infected people, the people would most likely have the disease again within the next few years unless exposure to the schistosomes can be prevented. And, of course, no drug is a panacea. As a drug is used more and more, it becomes increasingly likely that resistant strains of the organism will develop.




***Life cycle of the schistosome***

*The worm, which does not multiply in its human host, can live for as long as 20 or 30 years. [Laboratory of Parasitic Diseases, NIH]*

Like many other parasites, the schistosomes have a complex life cycle. Infected persons excrete the microscopic yellow schistosome eggs in their feces. If the feces get into freshwater, tiny embryos emerge from slits in the eggs and swim about rapidly until they find a snail host. The embryos develop and multiply in the snails, and, within a month or two, the snails start releasing thousands, or even tens of thousands of schistosome larvae per day.

The larvae home in on people who happen to be in the water, sensing people in some way that is not understood.