But that's just the beginning, because, as Turner notes, "for astrophysics and cosmology, this guy is a disaster." Even the supernova of 1987 gets into the act. Unless unknown interactions trap massive neutrinos in the supernova, they would come streaming out and—according to astrophysical theory—cool the supernova faster than was actually observed. Yet endowing the neutrino with new interactions creates problems for cosmology. Big Bang theory says the universe expanded very rapidly during the first seconds of its existence, until it had cooled enough for disparate particles to join into elements such as hydrogen, lithium, and helium. The postulated neutrino interaction would alter the universe's expansion rate, ultimately causing these elements to form in ratios different from what is now observed.

"It's not easy to get around this dilemma," Turner concedes. One way would be to postulate a lifetime for the heavy neutrino of only a millionth of a second, so that by the time elements started to form almost all the neutrinos would have been gone. A more remote possibility: the Big Bang model of how the elements formed is off the mark.

And if the Big Bang might have to be retooled to accommodate this new player among subatomic particles, it's not the only theory that will. The Standard Model would also need work. In pristine form, the Standard Model assumes neutrinos have zero mass; simple extensions of the theory postulate masses still too small for the recent results—by factors ranging from ten thousand to ten billion. Says Glashow: "There have been suggestions—over a dozen over the last few months—of how to accommodate something like this, but it's not obvious.... It's possible to add junk to the Standard Model to save the phenomenon, but none are particularly attractive."

But all of this assumes that the 17keV neutrino is for real. And a lot of people, including some harsh critics like Felix Boehm, say that idea is a lot to swallow. As usual in experimental physics, the answer is going to be more, and more definitive, results. As Glashow says: "More and better experiments can still be done. And they will."

Indeed, some of those presenting the new findings say it's too soon to become true believers. When Norman presented his results to the Berkeley physics department in February he said, "I believe the result is positive, but I'm not trying to sell you a bill of goods. We should be closer to an answer in about 6 months." The physics community will be waiting attentively.

■ PAUL SELVIN

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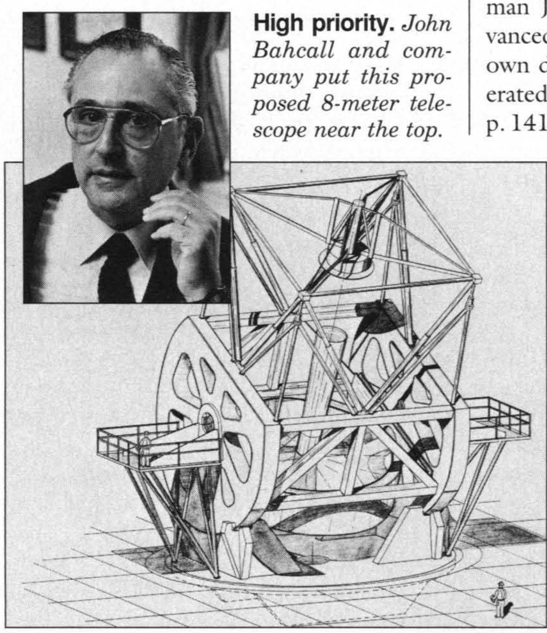
# Astronomers Forge a Consensus for the 1990s

*The Bahcall committee is getting high marks for making tough choices about astronomy research priorities.*

ASK ANY GROUP OF SCIENTISTS WHAT Washington should give them in the coming decade, and 99 times out 100 their answer will be utterly predictable: "Every project is top priority—send more money!" The U.S. budget deficits being what they are, nobody wants to admit that his or her pet project is less deserving of funding than someone else's.

But now comes one of the rare exceptions. In a 200-page report* released on 19 March, the 15 members of the National Research Council's Astronomy and Astrophysics Survey Committee have explicitly listed their research recommendations for the coming decade in order of priority—the third time that astronomers have done so since 1972. And even more remarkable, considering the ample potential for conflict, they have agreed to those priority rankings unanimously.

In Washington, where scientific advisory reports come and go by the dozens, officials are impressed. "This is one of the most effective decision-making processes in science," declares NASA space science chief Lennard Fisk. It is especially effective, he says, because the astronomers have produced one unified list for two very different agencies: The National Aeronautics and Space Agency (NASA), which funds space-based astronomical facilities, and the National Science Foundation (NSF), which supports the ground-based facilities. "That means they've looked at the entire discipline of astronomy and asked how the scientific issues can most effectively be dealt with," says Fisk. "In that sense, it's a very far-reaching strategy."



NOAO

**High priority.** *John Bahcall and company put this proposed 8-meter telescope near the top.*

Take, for example, the committee's list of recommended "large" projects—"large" being defined relative to what is typical at each agency. The top billing goes to a $1.3 billion NASA project known as the Space Infrared Telescope Facility, a liquid helium-cooled observatory designed to make ultrasensitive infrared observations of star-forming regions and newborn galaxies. But right behind it comes a ground-based facility costing about one sixteenth as much: An 8-meter infrared telescope to be built by the NSF on Mauna Kea in Hawaii.

And on a separate list of "medium"-sized projects, a host of ambitious space-based and ground-based missions were beaten out for the top spot by a relatively modest, $35-million program of laboratory research in adaptive optics: A set of innovative techniques that promise to reduce greatly the distorting effects of atmospheric turbulence and allow ground-based telescopes to achieve much of the clarity originally advertised for the Hubble Space Telescope.

From all accounts, much of the credit for the committee's achievement goes to chairman John Bahcall of the Institute for Advanced Study at Princeton, who records his own description of how the committee operated in his accompanying Policy Forum on p. 1412. As a scientist with long experience in Washington—in the mid-1970s, for example, he was a leader in lobbying for the Space Telescope—Bahcall knew it was important that the committee's final report be accepted by the funding agencies and the political powers-that-be, as well as by the astronomical community. So in 1989, before the committee even started its deliberations, he made the rounds of top officials at NASA, NSF, the White House, and Congress, asking them what con-

* "A Decade of Discovery in Astronomy and Astrophysics," National Academy Press, Washington, D.C., 1991.

cerns they would like to see the committee address.

At NSF, then-director Erich Bloch said he was disappointed that researchers weren't making better use of supercomputers and high-speed networking. The report accordingly has a full chapter on that subject: Since theoretical astronomers are constantly coming up with numerical simulations that strain the biggest and fastest supercomputers, says the committee, and since the observers' new digital detectors are churning out data fast enough to choke the largest existing archiving system, astronomers should be at the forefront of the Bush Administration's new High Performance Computing Initiative.

At NASA, administrator Richard Truly wanted to hear how astronomy would fit in with the president's initiative to send humans to the moon and Mars. The Bahcall report accordingly has another full chapter on that: The moon is indeed a promising platform for astronomy, says the committee, but take it one step at a time; experiment first with remotely operated observatories in places like Antarctica.

Everyone, however, urged Bahcall to make his priorities explicit. As House science committee chairman George Brown (D-CA) noted in his speech to the American Association for the Advancement of Science meeting last month, scientists who can't get together and define their own priorities are nothing but a headache in Washington—especially when their perennial anxiety over tight budgets leads them into backbiting, badmouthing, and individual lobbying. That sort of spectacle has been seen all too often of late, said Brown: "In the eyes of many members of Congress it relegates scientists to the level of every other special interest group."

As it happens, this last request was one that Bahcall and his colleagues were well prepared to heed. Astronomers had already drawn up prioritized wish lists in their two previous decade surveys, in 1972 and 1982, and their intention was to do so again. "We've gotten into the habit," explains panel member Sidney Wolff, director of the National Optical Astronomy Observatories. "In the past we've had good success. A reasonable fraction of the recommended projects actually got done. So now we take the process very seriously."

But that doesn't mean it was easy. "It took tremendous self-discipline," says Princeton University's Jeremiah Ostriker, who has served on each of the past three committees. When money is as tight as it is now, he says, and when people see that their own pet project isn't going to be first, their immediate impulse is to declare, "Don't rank anything—make all the projects the same, and maybe they'll all have a better chance!" And that is in fact what most scientific advisory panels do. The counterargument is that somebody is going to be making those choices, says Ostriker, "and better us than bureaucrats and politicians." That argument prevailed in the previous astronomy survey committees, he says, and it prevailed again this time—"but it required some tenacity on the part of the chairman."

To make that commitment to priorities stick, Bahcall had to minimize the obvious potential for conflict as much as possible, both in the committee itself and in the astronomical community at large. The latter task was made somewhat easier by the fact that astronomy is a comparatively small discipline, with only about 5000 researchers in the country; physics, by contrast, has about 50,000. By the time Bahcall had appointed 15 specialty panels to draw up recommendations in their own subdisciplines, each with about 30 members and a number of consultants, he had gotten almost 15% of the astronomical community directly involved in the committee's operations. Each of the subpanels, in turn, sought out input from hundreds or thousands of researchers.

If nothing else, says Ostriker, the result was a process of community self-education that was at least as important as the final recommendations themselves. Other advisory committees have used subpanels, of course. "But if you know you're going to prioritize," he says, "then you know you have to familiarize yourself with more than your small area. People were forced to look at where their field is going, where the scientific and technical opportunities are going to be, and what the trade-offs are between different disciplines." Or, as Wolff puts it, "It's as if the whole community goes on retreat."

***"This is one of the most effective decision-making processes in science."***

—Lennard Fisk

Meanwhile, to avoid bloodletting within the committee itself, Bahcall did his best to keep people from hardening their positions too soon. To achieve this, he kept the committee looking at options for the better part of a year. Then toward the end, Bahcall held a series of nonbinding straw votes designed to winnow out those projects that had little chance of getting support—and, not incidently, to let individual members know where their own favorite projects stood in the eyes of their colleagues. The result was a rapid convergence of opinion on a handful of items in areas such as infrared astronomy, where the prospects seemed particularly ripe for scientific and technological advances.

Then, at the urging of Wolff and committee member Wallace Sargent of the California Institute of Technology, Bahcall drew up a proposed list of final recommendations based on the previous balloting. The idea was that the committee members would be free to make changes, but that they would at least have a coherent proposal to start from instead of debating and voting on each project individually. And at this point, it became clear that Bahcall's strategy had paid off handsomely: After one day of debate in July 1990, the committee made only minor changes before accepting the list unanimously.

At the research council's parent organization, the National Academy of Sciences, president Frank Press is gratified by the Bahcall report, to say the least. He's been urging scientists to set their own priorities for years now, with little success; this report, along with astronomy's previous surveys, is one of the few exceptions he can point to. "It's going to be a model for how future studies will make recommendations," he declares hopefully.

Granted, he explains, some fields are so broad it's hard to imagine coming up with a single list of priorities. In physics, to take an obvious example, the divisions between subfields such as particle physics and condensed matter physics are far deeper than anything in astronomy. But it's not so hard to imagine scientists achieving more of a united front than they have in the past, with broad agreement on the weight to be given to such issues as infrastructure or the support for young people. Nor is it so hard to imagine them making their case in terms of where the opportunities are for greatest progress, instead of from an aggrieved sense of entitlement.

"The importance of that is not to be underestimated," says Press. The Bahcall report is certainly not all happy-talk. Echoing a theme heard in every discipline, the committee pointedly tells the NSF that it should "restore the infrastructure" before it does anything else—that is, raise its individual grants program and repair the damage done by a decade of deferred maintenance at the national observatories. But overall, says Press, the emphasis is not on pain, but on excitement. "The astronomers are saying, 'If you make these investments, here's what we think we can do.'"

"If everybody can be as eloquent and as convincing as [the Bahcall report] is," adds Press, "then they will be making a much better case for increasing science funding overall." ■ M. Mitchell Waldrop