An Agenda for Space Physics

The National Academy of Science's Committee on Solar and Space Physics, representing the researchers who study the sun, the solar wind, the upper atmosphere, and the magnetospheres of Earth and the other planets, has released its recommendations for a 20-year program of federally supported space missions.*

The space physicists have thus followed in the footsteps of the astronomers in the Academy's Field Committee and the planetary scientists in the National Aeronautics and Space Administration's (NASA's) Solar System Exploration Committee. However, it remains to be seen whether their report will be as influential as their predecessors'. Produced by a 15-member panel chaired by Stamatios M. Krimigis of The Johns Hopkins University, it calls for a 30 percent boost in the program's annual budget at a time when the federal government as a whole is facing huge deficits, and when the NASA space science office in particular is trying to divide its budgetary pie among more and more disciplines. On the other hand, the report may well prove effective as public relations: it dramatizes the solar/space physics program at a time when its two highest priority missions have fallen into budgetary limbo.

The first of these missions is the Solar Optical Telescope (SOT), a high-resolution instrument scheduled to fly on the space shuttle in the early 1990's. SOT has been delayed repeatedly as NASA focused its money and attention on completing the Hubble Space Telescope. This year, a deficit-minded Congress has slashed the SOT budget yet again, leaving the fiscal year 1986 funding uncertain. Some researchers are concerned that another year of severe underfunding might lead NASA to abandon the project.

The second troubled priority is the International Solar Terrestrial Physics mission, which addresses the interaction of the solar wind with the magnetosphere of the earth. According to agreements signed in 1983, the mission will involve six satellites: three from the United States, two from the European Space Agency, and one from Japan. However, budgetary constraints have forced NASA to delay this project also. Agency officials still hope for funding in fiscal year 1987. But the Japanese are reportedly getting restive; any further delay may lead them to drop their portion of the project entirely. Meanwhile, there is some chance that the Europeans will go ahead with their two spacecraft on their own—thereby undermining one of the most valuable aspects of the mission, the simultaneous collection of data from multiple points in the magnetosphere.

The other two major missions recommended by the committee are less problematic simply because they are much further off. The Solar Probe, targeted for launch in 1995, would fly to within 2.5 million kilometers of the solar surface to explore the origins of the solar wind. The Solar Polar Orbiter, targeted for launch in 2000, would orbit over the poles of the sun to explore the three dimensional properties of the heliosphere.

Rounding out the recommended package is a series of moderate-sized missions. These would include free-flying explorer-class satellites, to be launched at the rate of about one per year, plus a continued program of experiments aboard the shuttle, and eventually aboard the space station.

Altogether, the report calls on NASA to boost its funding for solar and space physics from its current $300 million per year to roughly $400 million per year. However, while there will definitely be a rise if and when the solar-terrestrial mission is approved, say NASA officials, it is not at all clear that NASA will be able to keep the funding at that level afterward. Indeed, one sore point for researchers is that in 1980, after a series of personality clashes and turf battles within NASA, the solar/space physics program was broken up among four different jurisdictions. The upshot is that the community has no single, high-level voice in arguing for new missions. The committee has suggested that the program be reassembled into a new office of its own. But it is anyone's guess whether NASA will do so.

—M. Mitchell Waldrop

*_An Implementation Plan for Priorities in Solar-System Space Physics_ (National Academy Press, Washington, D.C., 1985).

from one European and six U.S. institutions, informally known as the East Coast VLBI group, has analyzed data from two transatlantic baselines (_3_). They developed a new, more sophisticated atmospheric model that seems to be an improvement over the old model, especially for sources near the horizon. They also adjusted for nutation using the actual motion as determined by the VLBI observations themselves rather than that predicted by the standard model. Using these improvements, the lengthening of the Westford-Onsala baseline was found to be 3.2 ± 0.6 centimeters per year. That was "marginally significant," according to the group, in light of the inevitably larger systematic error.

The East Coast group has since made another improvement, according to Herring. Instead of subjectively sorting out nanosecond variations in each site's atomic clocks, they now use a more objective method involving Kalman filtering. Using this method and including the eight most recent distance determinations on the Westford-Onsala baseline, they find a rate of 2.3 ± 0.3 centimeters per year. Because the primary interest in experiments at the Westford observatory has been Earth's rotation, the East Coast group considers the best determined baseline to be between the neighboring Haystack Observatory antenna, which is larger and more sensitive, and Onsala. Four years of these observations have been made as part of the Crustal Dynamics Project. The group's latest and best analysis of that data yields a rate of 2.0 ± 0.2 centimeters per year, which is the same rate as their analysis of the Westford-Onsala baseline.

A growing consensus that the true error in these measurements is about 1 centimeter per year supports privately held opinions that the detection of continental drift is imminent if not already accomplished. How best to estimate true error is as much a philosophical as a technical question, but three approaches—those of NGS, the East Coast group, and a West Coast group centered at the Jet Propulsion Laboratory—seem to be converging on the same 1 centimeter per year figure as a reasonable error estimate for the best VLBI data. The major remaining area for improvement seems to be further verification of the new atmospheric model and, within a few years, the routine use of radiometers to infer the amount of water vapor along the radio signal's path.

Additional confidence has been engendered by the preliminary results from