BY BRUCE MARGON

Small Is Beautiful, But Big Is Necessary

"Battlestar Galacticas" is what Dan Goldin, the former NASA administrator, called them—the large, heavy triumphs of 20th century space science. These spacecraft, such as Hubble Space Telescope, Chandra X-ray Observatory and Cassini, continue today to yield enormous scientific return. But they also caused endless management heartburn and budget tensions during their decades of development. Goldin believed the era of these giants was closing, and the spectacular demise of the Superconducting Supercollider seemed to support this view. Perhaps "big science" in space was over. Goldin argued instead that "smaller, faster, cheaper" projects were the answer.

Certainly there were some small programs of the 1980s and '90s, such as Explorers, that yielded spectacular discoveries at modest cost. But, inevitably, there were also high-profile disasters, typified by the loss of two Mars missions, said by some to be due to avoidable blunders made in the name of cost and schedule. As the 20th century closed, "smaller, faster, cheaper" had lost its charm.

There always will be a need for small- and medium-scale programs. But President Bush's Fiscal 2007 budget request, which would cut $3 billion out of the next five years of NASA space science, has reopened the big-versus-small controversy. Now there's a more dignified label for the multibillion-dollar programs, namely "flagship missions." But the questions are the same. Even a few large missions can place great strain on the budget.

How should the flagships be prioritized against more modest siblings when the financial going gets really tough? Do we get the best return simply by maximizing the number of missions? In early March, the House Science Committee heard from four preeminent scientists, who said that if the proposed $3-billion cut stands, small and medium missions should receive priority.

I disagree. The case for flagships has never been stronger, for multiple reasons: scientific uniqueness, productivity and, perhaps counter-intuitively, contributions to "small science."

It should be self-evident that future missions such as the James Webb Space Telescope or Space Interferometry Mission are large due to unique capabilities that are aimed at the most imperative questions. But when money gets scarce, physics is sometimes forgotten, and the seduction reappears that "smaller, faster, cheaper" can do it all.

However, many of the most vital problems in space science involve phenomena for which nature provides a tiny flux of particles or light photons arriving at Earth. As today's detectors often sense nearly 100% of incident radiation, no clever technology will induce nature to deliver more information. The only option for more signal then is a larger collecting area—implying larger, heavier and, sadly, more expensive spacecraft to carry these instruments. If we want to understand physics near the Big Bang, or find exceptionally faint traces of planets orbiting nearby stars, or return Martian samples to Earth, we do not have the luxury of claiming that the same quality of science is obtained with small or medium missions. If flagship missions end, we retreat from many of the otherwise soluble key problems, and thus from international leadership in the field.

Yes, flagships are expensive, but they are astoundingly productive. The Hubble Space Telescope has yielded more than 5,000 refereed scientific papers since launch, with the annual rate steadily increasing, to more than 600—a dozen publishable discoveries every week—in 2005. Part of this is straightforward: Significant progress on the most important problems rapidly stimulates more follow-up work and a cascade of related discoveries. But large projects also require a critical mass of human and software resources, to which by definition a low-cost project can never aspire. Calibration, reduction and archive software for flagships is usually well-standardized and portable, as it is written, tested and maintained by specialists. Any investigator with a competitive idea can use these flagships, limited only by scientific skills and imagination, and not by a raft of undeveloped analysis tools.

The "critical mass" factor also applies to issues of public science literacy, a key goal of all NASA science. While a professor can make an important discovery, she cannot employ a cadre of professionals familiar with mandated educational standards in numerous different states and grade levels. A flagship project can: Is there a K-12 school in the U.S. that does not display a Hubble image?

Finally, to our counter-intuitive point: Small science flourishes around large projects. In huge demand, flagships are used by a very large number of investigators. They are funded for analysis of results by NASA grants that support students, postdoctoral fellows and equipment. In a typical year, 200 Hubble users each receive a grant averaging well under $100,000—small science, and lots of it. The three NASA "Great Observatories"—Hubble, Spitzer and Chandra—combine to distribute nearly $70 million annually for analysis, a sum far greater than the total of National Science Foundation grants to individual investigators in astronomy. The financial health of the U.S.'s space science community depends not just on NASA's research and analysis programs, but also equally on the vigor of current and future flagships. Similarly, several dozen investigators get started or maintain footholds in the field each year with Explorer, sounding rocket and balloon projects, but the Great Observatories continually support several thousand U.S. astronomers.

NASA's space science program requires a mix of large, medium and small projects, both in times of budget sickness and health. In difficult times, the solution is not to choose which of our children to execute, but rather ensure that the scientific community, Congress, NASA and voters engage in sufficient dialogue that we emerge with a space program that is not just affordable, but inspires and challenges the American people.

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