New Directions for Space Astronomy

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THE BURGEONING OF THE U.S. SPACE PROGRAM IN THE 1960s and 1970s created new opportunities for scientific research in, from, and about space. Those years produced triumphs of exploration and discovery as the nation's astronauts and spacecraft observed the earth system, the planets and sun, and the cosmos beyond. These activities have benefited the world community with many peaceful contributions to science and technology.

The astrophysical sciences, in particular, have been stimulated by space-based observations that are free from atmospheric absorption, distortion, and background. In each wavelength range, as the instrumentation has improved and as more classes of objects have been observed, studies have evolved from narrower, subdisciplinary interest toward broader scientific scope. Exploratory investigations have been superseded by logically coherent, problem-oriented attacks on fundamental scientific questions relating to the creation of matter, the emergence of structure in the early universe, the formation and evolution of galaxies, stars, and planetary systems, and the conditions for the emergence of life itself. The progress in this century toward understanding our origins and context in astrophysical space and time will remain one of the outstanding achievements in the history of the human mind.

Because the National Aeronautics and Space Administration (NASA) phased out unmanned launch vehicles in the 1970s to create a market for Shuttle services, the space science program became totally reliant on manned space flight. The overly optimistic predictions concerning the Shuttle flight rate produced long delays for science missions, even before the Challenger loss. Studies of the Shuttle program subsequent to the accident have clarified the factors governing a sustainable flight rate, and the future availability of the Shuttle can be predicted to fall far short of the estimates used to plan existing programs. The current lack of assured access to space is the primary problem confronting the space sciences, and it is generally agreed that recovery will be based on the use of unmanned launch vehicles for missions not requiring the Shuttle's unique capabilities (*1*).

Adapting to these new realities will induce profound changes in the space sciences. The space astronomy program provides an instructive paradigm because of its highly planned and well-articulated symbiosis with the manned program. We suggest that future nonreliance on the Shuttle is not only a practical necessity but it will bring technical and scientific benefits. Program savings could offset the costs of unmanned launch services for future missions.

Foundations of the Current Space Astronomy Program

In the 1970s, when the Astronomy Survey Committee (the "Field Committee") charted the course of astronomy for the 1980s, the power and interest of space astronomy were already apparent (*2*). The Field Committee could look back to a series of distinguished accomplishments: x-ray observations by Uhuru, culminating with the series of High Energy Astrophysical Observatories; solar observations by Skylab; the gamma-ray survey by the Small Astronomical Satellite-B; and ultraviolet studies by the Orbiting Astronomical Observatory-3. The more sensitive ultraviolet program of the International Ultraviolet Explorer (IUE) was under way, and the international Infrared Astronomy Satellite would be launched in 1982. The Field Committee considered as given that the Hubble Space Telescope (HST) would be launched in 1985, the Gamma Ray Observatory (GRO) in 1987–1988, and, as part of the Spacelab Program, the Solar Optical Telescope (SOT) in 1987–1988, followed by the Space Infrared Telescope Facility (SIRTF).

On the basis of that schedule, the Field Committee proposed a balanced mix of ground-based and space-based facilities as a new program for the 1980s. The highest priority recommendations for major new programs in space were the Advanced X-Ray Astrophysics Facility (AXAF) (a 1.2-meter x-ray telescope) and the Large Deployable Reflector (LDR) (a 10-meter mirror for infrared and submillimeter wavelengths). Recommendations for moderate space programs included an augmentation to the Explorer Program, a Far-Ultraviolet Spectrograph, a Very-Long-Baseline Interferometry Antenna, and an Advanced Solar Observatory.

A study, "Space Science in the Twenty-First Century," was initiated by the National Academy of Sciences in 1984 to chart the longer range future, and it has been widely discussed in the community (*3*). The Task Group on Astronomy and Astrophysics framed its deliberations assuming (i) the ample availability of Shuttle launches, (ii) the existence of the "Great Observatories" (HST, GRO, AXAF, and SIRTF) as well as SOT, and (iii) the availability of astronauts for assembly, test, repair, and fine-tuning of large structures in space.

The task group outlined an ambitious program that would provide qualitative improvements in angular resolution and gathering power. These initiatives would require extensive space structures and substantially greater collecting areas. On the basis of anticipated capabilities, such telescopes could not be deployed without the direct participation of man in space.

The Field Committee's vision for space astronomy in the 1980s has not yet come to fruition. Of the major initiatives taken as given in the late 1970s—HST, GRO, SOT, and SIRTF—none have yet flown, and for the latter two, development has not begun. Of the major new programs recommended by the Field Committee—AXAF and LDR—neither has yet been initiated. Before Challenger, this backlog of basic capabilities was perceived as the frustrating result of delays and overruns of the Shuttle program. Now, the quality and quantity of Shuttle services that will be available to space astronomy not only invalidate the basic assumptions of the 21st-century study, but also put the planned program at significant risk.

Lost Choices and Discovered Risks

The mode of space transportation—manned or unmanned—is a strategic choice that enables or precludes satellite servicing, direct communications, and free selection of orbit parameters based on merit. For example, a science satellite in Shuttle-accessible, low earth orbit is visible from U.S. communications stations only a fraction of the time. It requires the Tracking and Data Relay Satellite System for continuous communications access at high data

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rates, whereas a direct link can communicate with a satellite in high, geosynchronous orbit without interruption. Only an unmanned system could place a heavy observatory into orbits outside the narrow range of the Shuttle orbits, but that option could improve scientific productivity and reduce environmental hazards. On the other hand, only the Shuttle could bring astronauts to repair faulty spacecraft, restore expendable resources like cryogens, or assemble complex structures in space.

The advantages of alternate orbits and an incremental development approach based on engineering prototyping have been traded off in the current space astronomy program for the Shuttle's ability to lift great weights and for the apparent benefits of satellite servicing. The result is expensive, one-of-a-kind observatories that develop total reliance on astronaut servicing during construction. The servicing option is effectively a contingency reserve that may be used by project managers to meet cost and schedule goals by postponing the resolution of technical problems from the development to the operational period. In the prototyping approach, the difficulties would be identified on the engineering model and solved on a second system that is launched first. The engineering model would be a backup that could be subsequently refitted and flown with enhanced components.

Servicing reliance has been implemented for HST and GRO, and it is currently planned for AXAF, SIRTF, and the Explorer missions, for which until recently it was intended that the Multi-Mission Spacecraft would be retrieved and reinstrumented several times in orbit. The principal flaw in the current approach is the demonstrable inability of manned spaceflight to satisfy the needs of space science generally and those of space astronomy in particular.

Shuttle Support for the Space Astronomy Program

A panel of the National Research Council has identified and assessed technical factors that bear on the achievable Shuttle flight rate (*4*). Assuming problem-free ground and flight operations, the panel concluded that a total of 8 to 10 flights per year were possible for three Shuttles, or 11 to 13 for four Shuttles if improvements were made in ground facilities and logistical support. On this basis, we believe that eight launches per year is the most realistic rate. In the first instance, this is because delays will inevitably occur as a result of ground-processing inefficiencies and precautionary launch postponements. Second and more fundamentally, the eight-launch-per-year rate follows from a recognition that a three-Shuttle fleet should be the basis of planning at least into the mid-1990s. Currently, NASA has three Shuttles and is procuring a replacement for the Challenger, which is scheduled for first flight in mid–FY 1992. The median Shuttle lifetime measured in launches is that exponent of the system reliability (or one minus the probability of failure) that yields a value of about 0.5. For more than 27 total launches before mid–FY 1992—far fewer than NASA currently plans—the median Shuttle lifetime would be exceeded for system reliabilities less than 0.975. Assurance at the 90% level that the new Shuttle would restore a four-Shuttle rather than a three-Shuttle fleet would require a system reliability above 0.996 for the same number of flights. The achievability of such high reliability values has not been demonstrated (*5*).

The "Mixed Fleet Study" by NASA's Office of Space Flight counted some 130 "equivalent Shuttle payloads"—including many partial ones—that require NASA launch services in the period from FY 1988 to FY 1995 (*6*). Of these, some 37% were scientific, 17% for Space Station, 38% for national security purposes, and the others miscellaneous. When Space Station operations begin, a minimum of six to eight Shuttle launches per year are planned for rotation of the crew.

If we assume there will be eight flights per year after restarting Shuttle operations, the cumulative number of Shuttle launches from FY 1988 to FY 1995 will be less than 60, and the shortfall would exceed 50% of the identified requirements. Some payloads could be put on unmanned launchers, but that option applies only to a minority, including perhaps 25% of the NASA payloads, according to the Mixed Fleet Study. The situation may be improved by reductions in the Department of Defense requirements, but such gains will be more than offset by expected inefficiencies in combining partial payloads. The problem is exacerbated by two factors not identified in the Mixed Fleet Study. First, the available Shuttle fleet is smaller than the Shuttle inventory because only Atlantis and Discovery and not Columbia can meet the special requirements of some payloads, particularly the heavy ones. Second, no reserve was set aside for unexpected requirements such as servicing a failed critical system on a scientific satellite or reboosting a rapidly decaying orbit. The Shuttle program's exposure to such unexpected requirements would be compounded with multiple serviceable observatories in simultaneous operation.

The space astronomy program cannot be carried out as planned by using the current approach based on manned space flight because the Shuttle support will not be available. In the near term, some fraction of the already developed missions that are Shuttle-symbiotic, such as HST, can be flown and maintained with confidence—if their space transportation requirements are clarified and receive adequate priority. In the long term, an expanded manned program with a larger Shuttle fleet could support the innovative program envisioned by the 21st-century study. Although the immediate future offers little flexibility and the distant future depends on external factors, the 1990s offer opportunities to recover lost scientific momentum by adapting the space astronomy program to the emerging realities of space transportation.

An Alternative Space Astronomy Program for the 1990s

We propose that the future space astronomy program be planned for unmanned launch services, the only mode predictably available to new missions. The particular orbit for each could then be selected on the basis of scientific merit. The potential benefits of orbit selection are illustrated by the operational complexity, degraded observational quality, and environmental hazards that the HST will experience in low orbit.

Since it must be launched eastward and later landed in the continental United States, and because of its limited maneuvering power, the Shuttle can deploy and rendezvous with satellites only in low orbits that are inclined at about 28.5° with respect to Earth's equator. The approximately 90-minute orbital period of Shuttle-accessible orbits causes frequent earth occultations, interrupting observations and reducing the operational efficiency, which is the ratio of time on target to total time. The HST orbit limits the operational efficiency to less than 35%—almost three times lower than for IUE in geosynchronous orbit. The radiation interference of the rapidly varying trapped particle environment, especially the South Atlantic Anomaly, disrupts telescope operations. The requirement to communicate through the Tracking and Data Relay Satellite System can cause bottlenecks in data reception and commanding.

Orbital parameters can affect the quality of scientific data. For example, unwanted background light and noise can interfere with observations. In low orbit, space debris will cross the HST field of view with significant frequency and brightness (*7*), and the thermal infrared signal of this material would be observed by SIRTF. The spectrum and variable intensity of Shuttle-induced atmospheric "glow" are not well understood, but the phenomenon could contribute unwanted background to HST observations. These effects would be alleviated in higher orbits. For satellite telescopes with detectors sensitive to very energetic particles, such as the AXAF, a zero-inclination low orbit would offer the benefits of shielding by the earth's magnetic field and of avoiding the South Atlantic Anomaly.

Environmental hazards in low orbit arise primarily from the residual atmosphere, the density of which varies in response to solar activity, tracking the 11-year solar cycle. Some spacecraft materials erode rapidly by exposure to the streaming of atomic oxygen. Frictional drag causes the orbit to decay. In the case of HST, the nominal deployment altitude is about 590 kilometers, which is a significant challenge for the Shuttle. Reboost could be required several times during the FY 1990 to FY 1993 period, depending on the magnitude of solar activity around the maximum predicted for 1990–1991. The developmental Orbital Maneuvering Vehicle has been designed to ferry satellites like HST between the Shuttle and somewhat higher orbits, where the atmosphere no longer constitutes a risk. The initial use of this autonomous spacecraft is planned for the first HST revisit by Shuttle in FY 1991.

Switching to unmanned launch services for future space astronomy missions would have budgetary consequences related to (i) the savings from Shuttle launches not made, (ii) the new costs of unmanned launchers, and (iii) the cost savings associated with reduced on-orbit servicing. Savings in (i) cannot be reckoned meaningfully, because of the Shuttle's vast oversubscription. Costs of (ii) would be in the range of $50 million to $300 million per launch, depending on the choice of launch vehicle. Neither (i) nor (ii) are current budget elements in the science programs, but on-orbit servicing is a significant burden to the space astronomy budget.

The potential savings from not designing a satellite observatory for manned servicing can be estimated from the HST budget plan. Approximately $150 million will have been invested directly in prelaunch development and preparations for in-orbit maintenance. Additional, indirect development costs arose from adopting serviceability as an overall design concept that affected most HST systems by introducing safety margins for astronauts and stringent tolerances for replaceable systems. During operations, about half the annual HST budget, or approximately $100 million per year, is allocated to sustain the servicing program, not including Shuttle launches. Further operational cost savings for a nonserviceable observatory could derive from selecting the orbit to simplify operations, to reduce mission risk, and to enhance observational efficiency and scientific data quality.

A space astronomy program based on unmanned launch services would be approximately self-sustaining within current budget levels—inclusive of launch costs. Each subdiscipline could determine the optimum ratio of operating funds to development funds for maximum scientific return. The level for such a program for optical astronomy would be about $190 million per year, providing for the following: $60 million per year for operations and science support, $100 million per year for development (one $1-billion observatory every 10 years), and $30 million per year for launch costs (one $300-million launch every 10 years). These numbers are representative of the costs for optical space observatories of the HST class. Infrared, x-ray, and gamma-ray astronomy, as exemplified by the SIRTF, AXAF, and GRO missions, may need two-thirds of these amounts. This would mean that the baseline program of the Great Observatories could be conducted for approximately $570 million per year, inclusive of launch costs. Similarly, a strong Explorer program could be supported by unmanned launchers. If missions of the $200-million class were included with $200-million launch costs, and they were launched every 3 years, this would add about $130 million per year to the space astronomy program. Of course, more frequent launches could occur for less ambitious and less costly missions. Thus, the core of the current space astronomy program would be achievable for an approximate cost of $700 million per year, inclusive of launch costs. If, as was done in the 21st-century study, each replacement observatory is assumed to cost twice as much as its predecessor, the budget would increase to approximately $990 million per year, inclusive of launch costs. This should be compared to the $1 billion per year discussed in the 21st-century study, which was exclusive of launch and servicing costs.

Continuity for the observational capabilities of the space astronomy program could be provided, not by continuous operation in orbit of each of the Great Observatories, but by a programmatic and intellectual continuity on the ground. Existing observatories would be operated while replacements were being built about every 10 years. This program would create the conditions necessary to train new generations of technicians and scientists and to establish an industrial base for the program's hardware support.

The strategically reoriented program we have outlined could span the 1990s and provide the scientific and technical basis, in the second half of the 1995 to 2015 period, for addressing the more ambitious recommendations of the 21st-century study, particularly those that may require orbital assembly. The manned spaceflight program may have expanded by that time to be capable of supporting such endeavors. Such projects would certainly require additional funds, raising the budget to the $1.5 billion per year level envisaged in the 21st-century study, or higher.

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