Invited Paper

The Next Generation Space Telescope: a large UV-IR successor to HST.

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ABSTRACT

The Next Generation Space Telescope (NGST) is a uniquely powerful scientific tool. UV-Visible-IR space observatories like NGST offer the unparalleled advantages of low background, uninterrupted wavelength coverage and a stable point-spread function for high dynamic range observations. Many fundamental astronomical problems can be tackled only with filled-aperture telescopes that combine high spatial resolution with large light-gathering capability. An 8 m-class passively-cooled telescope in high earth orbit would have unprecedented power for problems as diverse as planet searches around nearby stars to the way in which galaxies formed in the young universe. It will build upon the discoveries and astronomical understanding of many decades of research with astronomical observatories, and is the natural successor to HST and the new generation of large 10 m-class ground-based telescopes. The concept of NGST has developed as a result of workshops, studies and the consideration given future instrumental programs by the *UV-Optical in Space* Panel of the *Astronomy and Astrophysics Survey* Committee.

1. INTRODUCTION

It is clear that a large space telescope is needed to tackle a broad range of astrophysical problems. Many of these scientific issues cannot be adequately addressed with current or planned telescopes. The Hubble Space Telescope is clearly a powerful telescope, though its potential will not be fully realized until instruments that use contemporary technology and corrective optics are implemented. Even then, since it is the first of its kind, it does not fully exploit the potential of space telescopes in the crucial 0.1–10 μm range.

The primary advantages of large space telescopes are the combination of continuous spectral coverage unavailable from the ground, low background, a stable point spread function, and extremely high dynamic range from the ability to apodize low-scattering optical surfaces. The gains from the lower background are particularly large in the IR. Background reductions of 10^6 times can be expected out to ~ 10 μm in systems passively-cooled to $\sim 100°$K. Contemporary telescopes would not be competitive. Adaptive optical systems on ground-based telescopes have the potential for substantial gains in imaging performance, but will not result in telescopes that have the overall capability of space-based instruments. The Strehl ratio (a measure of the concentration of energy in the diffraction-limited core) will vary both with time and across the small field defined by the atmosphere, making high dynamic range, quantitative measurements impractical. HST is limited by its small aperture, which leads to low sensitivity for spectroscopic observations and low spatial resolution in the IR, and its warm optics and structure that lead to high background in the IR for $\lambda > 2$ μm.

The science presentations at the *Next Generation Space Telescope* Workshop in Baltimore[1] gave excellent examples of both the breadth of the scientific issues that could be tackled, as well as the unique opportunities that such a telescope would bring for certain key problems. It was clear that many central problems in astrophysics require a large space telescope with forefront imaging and spectroscopic capabilities across the $\sim 0.1 - 10$ μm wavelength range. Examples of such problems are: the nature and structure of high redshift galaxies; planet detection and spectroscopic analysis of their atmospheres; structure in star forming regions; and analysis in the UV of current-day counterparts of high-redshift objects.

The question of planet detection is one of great interest, but one that is extremely difficult from the ground or with HST. Angel, Cheng and Woolf [2] analysed the problem of detecting and spectroscopically measuring earth-like planets. They concluded that the optimum configuration was a filled-aperture, 16 m diameter, passively-cooled telescope with low-scattering optics for use at 10 μm. Angel[3] has further refined this argument and suggested that a close-packed array of four 8 m telescopes would be better. Whichever approach is finally chosen, it is clear that the problem is

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an extremely challenging one. However, it is one that must rank high as a goal for any long-term program of space astrophysics.

Together these scientific goals have led to the development of a concept of large, passively-cooled filled-aperture space telescope with $\sim$ 8 m diameter primary utilizing advances in technology since HST was conceived. This is an ambitious program, but it appears to be practical for launch in the first decade of the 21st century.

2. SCIENCE GOALS

As noted above, the Baltimore NGST workshop[1] volume contains several papers describing some of the scientific goals for space telescopes of the 8-16 m class. Further discussion of the science programs that could be carried out with a large space telescope can be found in the Report of the *UV-Optical in Space* Panel of the Astronomy and Astrophysics Survey Committee[4]. One of the striking features of NGST is the range of astrophysical problems for which it would provide substantial gains in knowledge; almost every major area of astronomical research would benefit greatly. A few examples of key problems to be tackled with NGST are:

- Detection and spectroscopy of gas giant to earth-like planets around nearby stars. The size of the planets and the distance to which they can be studied depends upon the size, optical configuration and the temperature of the optical system, but planets of a broad spectrum of of sizes could be detected and studied spectroscopically with 8-16 m telescopes passively-cooled to $\sim 100°$K.
- Study of nearby star-forming complexes with resolutions from 5-50 astronomical units (AU) in the visible and IR. Star formation is another key problem that is greatly compromised by the limitations of ground-based telescopes in the IR. The complexity of the structure associated with star-forming processes and protostellar disks, and the very high dynamic range required, make this a problem that will benefit greatly from the capabilities of large space observatories. Protostellar disks 1000 AU in size could be detected throughout the disk of our galaxy. Such a disk in Orion could be studied with 40 AU resolution, i.e., with 25 independent spatial resolution elements.
- Measurement of stellar populations in a variety of environments. The past history of our galaxy and nearby galaxies centers on the study of faint, mostly unevolved stars. With its ability to detect stars with a S/N=10 in 10^4 s in the visible to 31 mag, NGST could age date the oldest populations by measuring below the main sequence turnoff anywhere in the Local Group of galaxies.
- Spatially resolve and map complex structures in the inner narrow line regions for active galactic nuclei and quasars (AGNs and QSOs). The complexity of the structure in such regions, and the high dynamic range pose problems for ground-based observations with adaptive optics and for interferometric systems. The difficulty of this problem will benefit from observations with large telescopes like NGST that can resolve $< 10^{20}$ cm at the nearest QSO, 3C273.
- Structure and evolution of (forming?) galaxies at redshifts $z > 1$. Large space telescopes can give resolutions on galaxies ***at any redshift*** z comparable to that for galaxies in the nearest cluster of galaxies, the Virgo cluster. These resolutions range from $\sim$ 100 parsec (pc) to 1 kpc from the visible to the IR. The ability to resolve the dominant structures in galaxies (spiral arms, star-forming complexes, merger or interaction filaments or "tails", characteristic disk and bulge length scales, etc) will be crucial if we are to understand the evolutionary events that occur as galaxies form and change with redshift.

An additional great benefit of space observations with a passively-cooled telescope is that "sky"-limited observations can be made in the zodiacal background "window" around 3.5 μm. This "window" occurs between the scattered solar spectrum and the thermal emission from the zodical dust. This may well prove to be the wavelength at which detection could occur of the youngest, highest-redshift objects in the universe. Opening up the full wavelength region from $\sim 0.1 - 10$ μm to sky-limited observations with resolutions 10 to 100 times smaller than that from the ground would have a dramatic impact on galaxy studies in the young universe. Even with adaptive optical systems ground-based telescopes will not be competitive. These galaxies are low surface brightness objects and the structures in them will be swamped by the 10^6 times brighter background in the thermal IR from the ground.

To further demonstrate the power of large space telescopes for investigating galaxies at high redshifts, some simulations have been developed and are shown in Figure 1. They were made by J. E. Gunn for his paper in the NGST workshop in Baltimore[5]. They are of a "typical" spiral galaxy at a redshift $z \approx 1$ as it would appear through the 10 m Keck

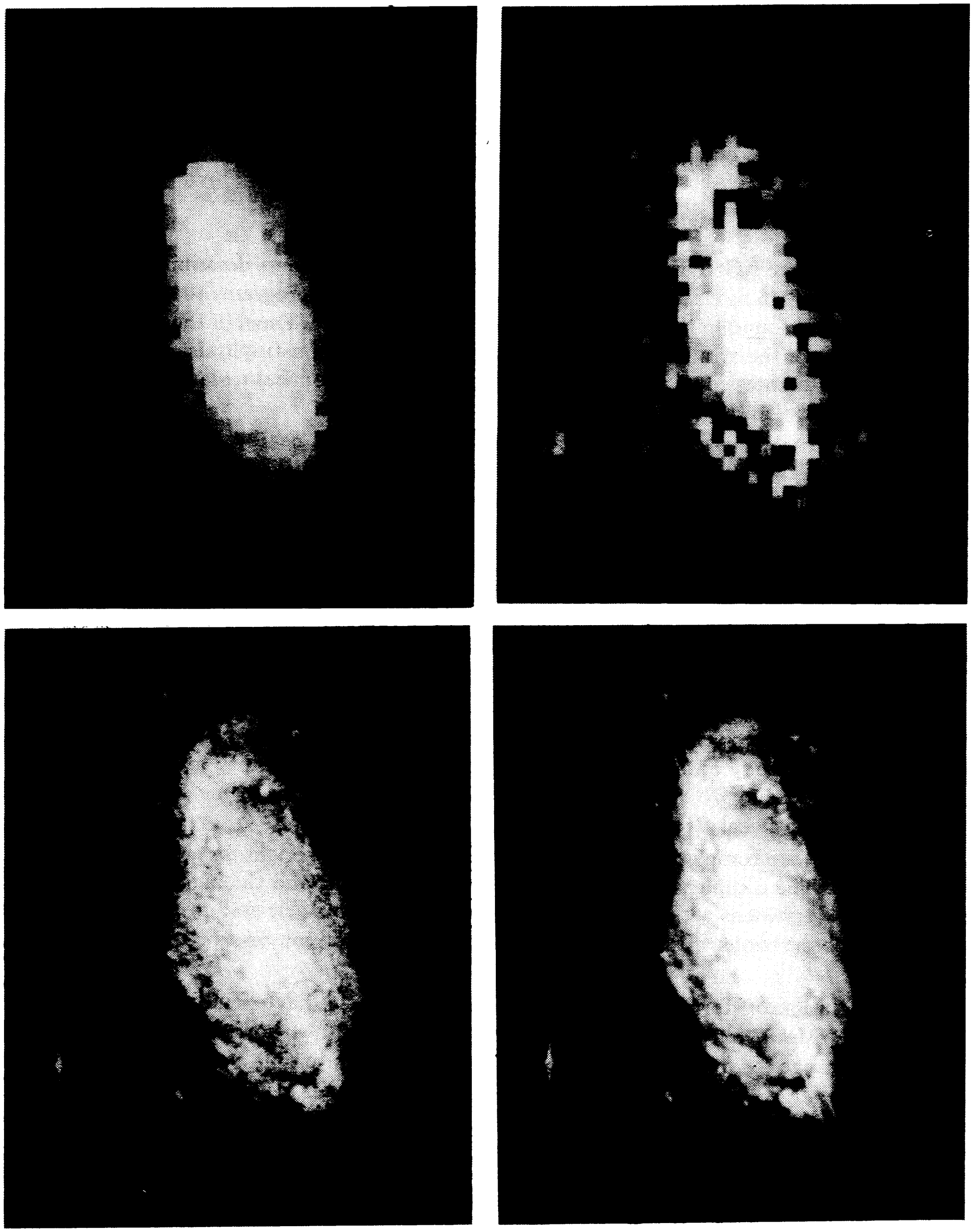


Figure 1. Simulations of the imaging performance of the 10 m Keck telescope, HST and of a 16 m diffraction-limited telescope for a $z = 1$ Sc spiral. a) Upper left. A two-hour integration with the Keck telescope. The image has high S/N but low resolution. b) A three orbit (approximate 2 hours) exposure with the Wide Field Camera from HST. The (undersampled) resolution is about 1 kpc. The image has much higher resolution, but lower S/N. c) Lower left. The 16 m with a 2 hour integration. The imaging performance is spectacular. This is the resolution with which we image ***Virgo cluster galaxies from the ground.*** Except for noise it is almost indistinguishable from the original CCD image of the 10 Mpc galaxy. d) Finally a 24 hour integration, with a superimposed spectrograph slit. The slit width is ***ten times*** the resolution of the telescope. Gunn noted that his original data of this nearby galaxy taken with a large ground-based telescope barely has the S/N and resolution needed for this simulated observation with the 16 m. An 8 m NGST would have intermediate performance, but would be much closer to the 16 m than to HST.

telescope in Hawaii, through HST and through NGST (in this case a 16 m NGST). Galaxies at $z \approx 1$ would be seen when they are about 8 billion years younger, when the universe was between one-third and one-half the age it is today. While the gains with HST are impressive, those with NGST are truly astonishing. An 8 m NGST would have gains much closer to the 16 m than to HST.

3. NGST CHARACTERISTICS

Discussions extending back to the early 1980s about the power of large space telescopes have focused on the combination of collecting area and resolution as being the appropriate figure-of-merit for astronomical problems. It was realized in the mid-1980s that cooled systems (passively or radiatively cooled systems, as opposed to cryogenic systems) would make a major improvement in the sensitivity of such a telescope[2,6]. The key characteristics of the NGST observatory and its supporting systems that have developed over the last few years are summarized here:

- 8-16 m diameter filled-aperture telescope with lightweight optics.
- Wide bandwidth system: 0.12 μm to $\sim$ 10 μm.
- Diffraction-limited in visible and IR; high Strehl ratio in UV if technologically feasible. Resolutions of 10-20 milliarcsec (mas) at 0.6 μm.
- Low-scattering, smooth optics for clean, easily-apodized point spread function for extremely high dynamic range.
- Passively cooled to 100°K or less for reduction of background by 10^6 in IR beyond 2 μm.
- Active optics to compensate for thermal and aging effects. Fine pointing by optical element motion in lieu of body pointing.
- High sensitivity. The small PSF (point spread function) and low backgrounds result in remarkable sensitivity. Can measure >31 mag (V-band) and <25 nJy (3 μm) objects in 10^4 s at 10:1 S/N.
- State-of-art UV-IR imaging and spectroscopic instrumentation with wide field, large format detectors and multiplexed operation.
- Compact optical system (e.g., small baffle for high earth orbit (HEO) operation to lessen constraints on launch vehicle).
- HEO operation results in savings on size, weight, power, operational complexity plus gains in performance. Preliminary assessment indicates that it is comparable to HST in weight, thereby breaking away from the HST cost curve.
- Siting and size tradeoffs. Discussions regarding siting have noted that a 16 m diameter primary is highly desirable but impractical in orbit. A site on lunar surface is likely to be the only practical one for such a large facility. It would be near the lunar base for assembly and maintenance.
- Near term 8m HST successor in HEO. Precedes lunar 16 m.

International participation is a highly desirable goal. Observational data in the UV-Visible-IR plays an essential and central role in astrophysics. Thus a unique space observatory in this wavelength region naturally becomes a program for international collaboration. It should receive very widespread scientific support. Furthermore, it has the capability, longevity, and "presence" to be attractive as a truly international scientific venture for the first decade of the new century. Subsequent discussion will expand on many of these points.

4. MILESTONES

As noted above, the concept of a large space telescope that could truly exploit the advantages of observatories in space with mature technologies has been brewing for many years. A key step was the realization in the early 1980s of the power of 8m-class filled-aperture space telescope for cosmology/galaxy formation questions (plus a broad range of other astrophysical problems). A step that strengthened this view was the analysis of the problem of detecting earth-like planets[2]. This led to an optimum configuration of a filled-aperture, 16 m diameter, passively-cooled telescope with low-scattering optics for use at 10 μm.

Further discussion of the concept occurred as part of a Space Science Board study. The result was a recommendation for a 8-16 m filled-aperture telescope in the 1988 report[7] *Space Science in the 21st Century: Imperatives for the Decades 1995-2015.* The report noted that: "Given a well-directed technology development program, the task group anticipates that an 8 to 16 m telescope will prove to be within closer reach than a simple extrapolation from HST would suggest". This report was followed by an invited presentation[6] at the 1988 IAU (International Astronomical Union) triennial meeting in Baltimore on the concept. The presentation focused on an 8 m filled-aperture, passively-cooled, wideband UV-Visible-IR telescope for operation in High Earth Orbit.

More detailed evaluation of the concept of NGST occurred at the workshop[1] that was held in Baltimore in 1989. The baseline concept for discussion at that meeting was similar to that previously discussed but more ambitious at 10 m in size for HEO. The scientific merit of a 16 m filled-aperture telescope and planning for SEI (the Space Exploration Initiative, i.e., the lunar base/Mars exploration program that resulted from the "90-day study") led to inclusion of a 16 m lunar-based telescope in discussion. As noted previously, the scientific discussion demonstrated the outstanding capabilities of NGST for tackling forefront scientific problems over a huge range of areas. Technologies were assessed, as were launch and support capabilities. There was widespread agreement at the meeting that the concept was technically challenging, but within range of reasonable extrapolations of contemporary technology.

The next major activity was the *Astrophysics from the Moon*[8] workshop in Annapolis, early in 1990. Discussion of the 16 m telescope and its relationship to other lunar observatories[9], particularly the lunar interferometer, was a notable part of this meeting. Further discussion on the relative merits of large observatories and smaller missions occurred at the meeting on *Observatories in Earth Orbit and Beyond*[10] held later in 1990. Interest developed at Marshall Space Flight Center (MSFC) in carrying out preliminary studies of a lunar 16 m telescope and a 4 m lunar precursor. This activity was ably led by Max Nein and Billy Davis and has been summarized in their paper in this volume. This is the most detailed engineering study that has been carried out to date.

Concurrent with these studies and meetings were the deliberations of the *Astronomy and Astrophysics Survey Committee* (AASC – the "Bahcall" committee) and its Panels. This committee, and thus the Panels, were charged with evaluating and prioritizing the major astrophysics programs of the 1990s. The *UV-Optical in Space* Panel of the AASC discussed the HST successor and future large telescopes, among many other important issues. There was great concern about the timescale for the development of observatory-class missions, and about need for capability to follow HST. The Panel developed a long-range plan that included a 6 m HEO telescope to follow HST, with the long-term goal of a lunar-based 16 m telescope. The Panel recommendation to the AASC committee was for a new start on a 6 m large space telescope in 1998, for launch in 2009.

The focus of the AASC report was on completing outstanding missions (many left from recommendations in the 1980s *Survey Committee* report) and rectifying outstanding infrastructure problems. While some new initiatives were supported, the group was unwilling to support major new "Great Observatory-class" missions since SIRTF has yet to receive "new start" status and AXAF is being watched with some concern. A initial favorable reaction to the 6 m telescope faded after concerns about the current unfinished mission set arose. There was widespread support for a technology development effort to strengthen the case for future space science missions. The *UV-Optical in Space* Panel report can be found in the *Working Papers*[11] section of the AASC report.

Along these lines, a program of great importance to future missions is the Astrotech21 program. This is being implemented to develop technologies that will be needed for future programs. It is a NASA HQ Astrophysics/Advanced Technology joint initiative with planning support from JPL. NGST needs are being considered by that program. The mechanism for prioritizing NGST technological developments was a Workshop that was organized through JPL and held in Pasadena March 4-5 1991. Recommendations and priorities for NGST from that workshop were passed through technology planning meetings and into the development of an overall plan. The NGST workshop was held immediately before the JPL Astrotech21 Optics workshop to ensure appropriate input on large filled-aperture telescopes.

5. "UV-OPTICAL IN SPACE " PANEL RECOMMENDATIONS

The actual program recommended by the *UV-Optical in Space* Panel[4] is summarized in Table 1. The start date and the launch or completion date is also shown. The projects are grouped by their expected costs, into small, moderate and large programs. A line for technology development was explicitly highlighted, since it is clear that budget and schedule problems can be minimized for projects of any scale by having the demonstrated technologies in place *before* the start of a project.

SIZE	PROJECT	Start	Finish
Large:	6 m HST Successor	1998	2009
Moderate:	Explorer Enhancement	1993	2000
Moderate:	HST Third Generation Instruments	1994	2000
Moderate:	Imaging Astrometric Interferometer	1997	2004
Small:	Small Explorer UV Survey.	1995	1998
Small:	Space Optics Demonstration	1993	2000
Small:	Supporting Ground-based Capabilities	1993	2000
Technology:	Technologies for Space Telescopes	1993	2000

The Panel took the view that large missions, in particular, require a long-term plan that incorporates appropriate technology developments and demonstrations and precursor missions. They noted that the scientific case was strong for a successor to HST. The panel then recommended:

- that a successor to HST be ready to fly within a few years of end of HST's nominal life;
- that this be a 6 m UV-Visible-IR, passively-cooled telescope in HEO;
- and that, in the long-term, there be 16 m telescope on the lunar surface, sited near the large lunar interferometer.

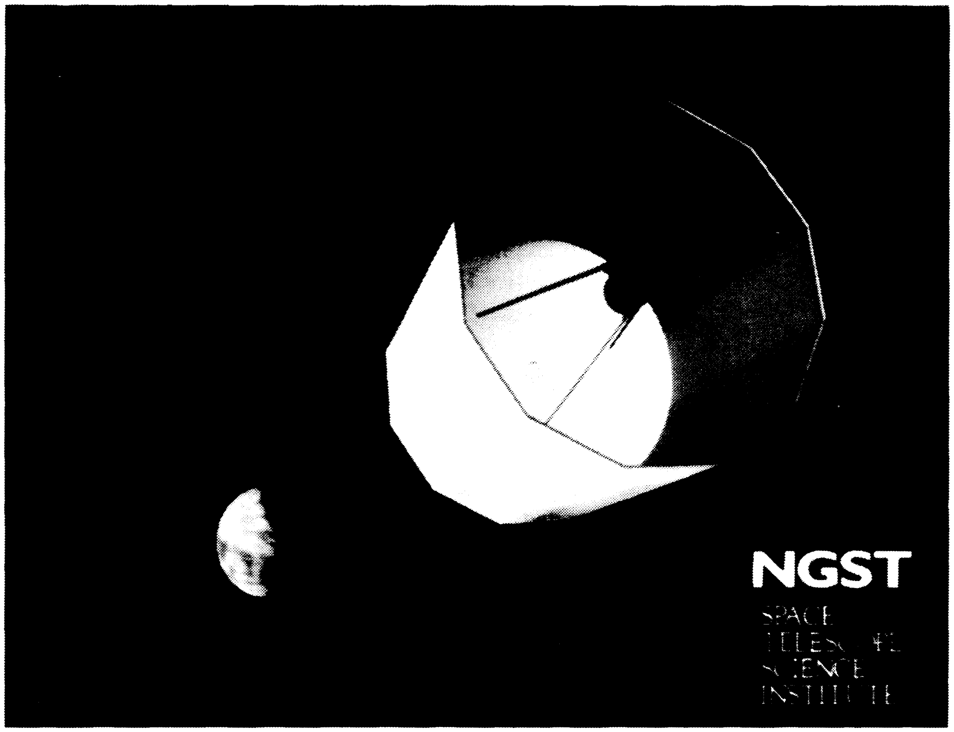
6. NGST LONG-TERM STRATEGY

In essence the view that has been developed is that of a long-term program with the goal of an orbiting NGST in HEO by the first decade of the 21st century, followed by a 16 m lunar-based telescope. This approach has come about in large part because of a realization that the Space Exploration Initiative (SEI) program will develop slowly. Several factors have played a role in the development of this view, some of which are Federal budget concerns, space station development, and the complexity and the cost of the SEI program. To accommodate one of the key objectives of the Panel – that a successor be developed to follow HST – it is clear that we must focus now on an NGST concept. This results directly from the very long timescale for development and acceptance of observatory-class missions. An additional factor is that the science community support for an orbiting successor to HST stems from the hope that launch will occur within a few years of the end of HST's life.

While the *UV-Optical* panel recommended a 6 m NGST, the choice of size was the source of considerable discussion. There was some dissension from 6 m, and a desire for a larger telescope (8 m?) to accomplish the science goals. Further technological evaluation needs to be undertaken, but an 8 m may well be practical for a monolithic mirror (March 4-5 JPL workshop discussions) and so we have adopted the baseline concept of a HEO 8 m passively-cooled telescope. Since some of the key factors involved in the decision tree for SEI are political ones, the timescale for that program is quite unpredictable. A resurgence of interest in the SEI program could lead to its implementation within the time frame being considered for NGST, and so it seems very appropriate and desirable to develop initially technologies that are generic for a variety of orbiting and lunar telescopes, *where it is technically rational to do so.* In particular, such developments should consider HEO orbiting telescopes of the 6-8 m size, a 4 m lunar precursor and the 16 m lunar telescope. Such an approach was broadly adopted in the Marshall Space Flight Center (MSFC) study[12].

An artist's view of of NGST for HEO operation is shown in Figure 2a, and a schematic view of such a telescope is shown in Figure 2b. The schematic view is one of the several conceptual models that have been considered for an orbiting NGST. The dimensions are for a 6 m telescope, but could be scaled for 8 m or larger (see the Baltimore NGST workshop[1]). Optical considerations may result in a longer focal length system, but this change has minimal impact on the overall weight. The key features are a short baffle, body-mounted solar panels, radiative cooling, lightweight optics and structures, and the less demanding support system requirements of HEO. Together these lead to a telescope whose weight would be *comparable to that of HST* ($<1.5\times$).

There will clearly be pacing items for lunar-based telescopes that are unique to such facilities and so it is appropriate



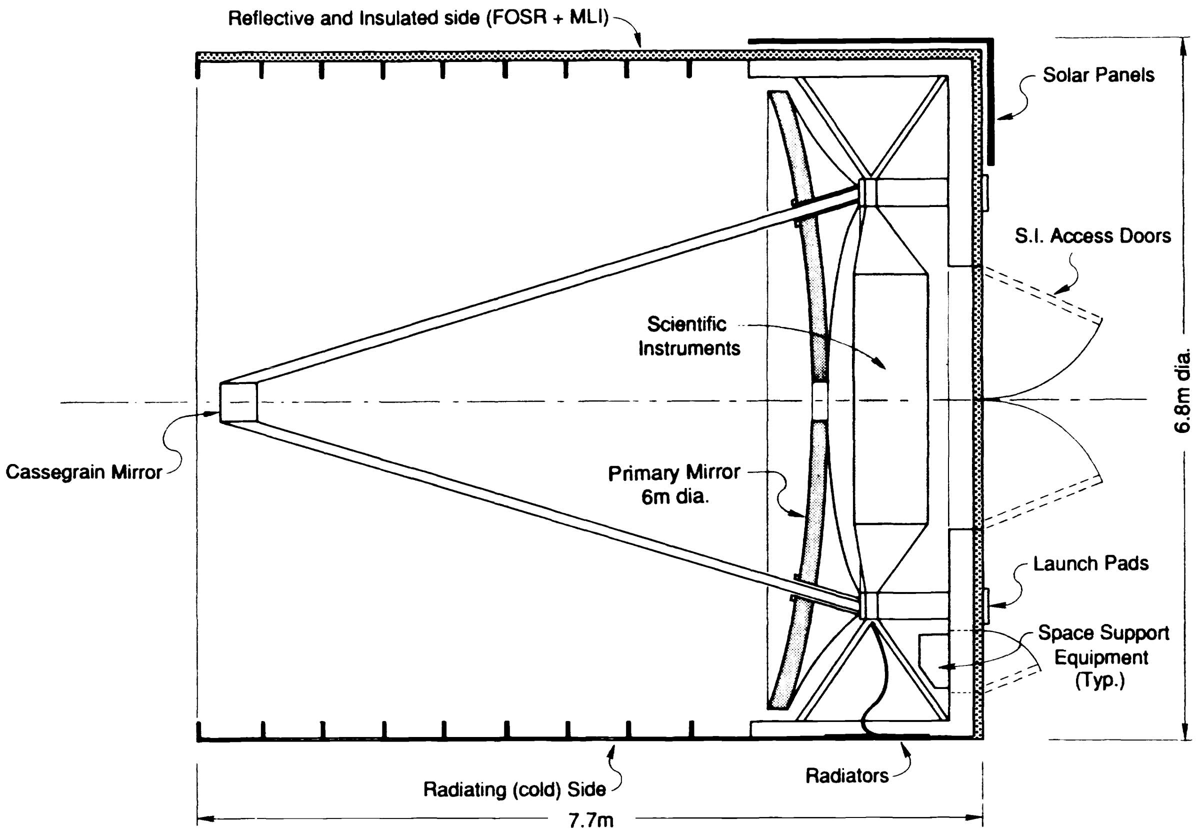


Figure 2. a) An artist's view of the 8 m NGST in HEO. b) A schematic view of a HEO NGST (6 m – see text).

to continue evaluations of such questions (e.g., dust control, shielding, precision motion under low-gravity vacuum conditions, optical designs for feeding sub-surface instruments). Broadly, the outstanding technical issues for all these telescopes are optics, pointing and control systems, UV performance in a passively-cooled system (the contamination issue), and detector performance under high background particle event rates.

7. 8 M HEO NGST

Given a strategy that has as its immediate goal an 8 m wide-band, passively-cooled, telescope in HEO, there is clearly a need to begin the definition of the characteristics of the telescope and its supporting instruments. The overall goals are a telescope with diffraction-limited optics in the visible and the IR, and a "high" Strehl ratio in the UV, that covers from 0.12 μm to $\sim$ 10 μm by passively-cooling the optical system to $< 100°$K. The PSF would have a core with a FWHM (Full Width at Half Maximum) of about 15 milliarcsec at 0.6 μm that scales directly with wavelength. A few other key characteristics should be noted, since they are crucial for many of the high priority science goals.

- Low scattering optics for high dynamic range, particularly for AGN and QSO studies, for planetary searches and for star formation studies. This constraint is much tougher than the nominal $\lambda/20$ for diffraction-limited images. For certain problems, notably the planet search problem, surfaces as smooth as $\lambda/1000$ may be necessary, though this requirement may only have to be satisfied at 5-10 μm. Both the goal of diffraction-limited UV images and the low-scattering optics lead to the challenging requirement of surface errors that are < 5 nm.
- The pointing and control system (PCS) requirements are also demanding. The jitter in the tracking should be at 10% (or better) of the resolution. This translates into $\sim$ 1 milliarcsec or better. The PCS system will need to be able to acquire and track features on planets and planetary companions. Spectroscopic apertures and slits will need to be located to a fraction of the width of the images, i.e., to < 5 milliarcsec.
- The focal plane field should be large. The ability to multiplex the operation of imagers and spectrographs will prove to be a very valuable feature of HST that should be retained for NGST. Ultra-deep imaging surveys can then be carried out in parallel with spectroscopic observations.
- The imager fields should be large so as to accumulate efficiently high S/N observations over field sizes of astronomical significance (e.g., high redshift clusters and young embedded star clusters). Fields of 2-3 arcmin in the visible and IR and 0.5-1 arcmin in the UV should be attempted. This will require mosaics of arrays to ensure adequate sampling of the PSF over those fields ($> 10^4 \times 10^4$ pixels in the UV and visible; $> 2\text{x}10^3 \times 2\text{x}10^3$ in the IR).
- High-throughput spectrographs, with multi-object capability, particularly for very faint resolved/partially resolved objects or structures, are also a crucial component of the system. Wavelength resolutions ($\lambda/\Delta\lambda$) from 10^3 to $\sim 10^5$ will be needed.
- The particle background will be substantially higher in HEO than in low earth orbit (LEO). Detectors with low read noise will need to be combined with on-board processing to minimize the data volume to be transmitted. The small PSF and low backgrounds will result in remarkable sensitivity for point sources. The visible (V-band) background is $\sim$ 31 mag per resolution element, i.e., $\sim 4 \times 10^3$ times fainter than ground-based observations of unresolved objects, while the 3-4 μm background is 18 mags per resolution element *less than that on the ground*, i.e., $\sim 10^7$ times fainter than ground-based observations of unresolved objects. This means that for integrations of 10^4 s, 10:1 S/N can be achieved for 31 mag visible objects and for 25 nJy objects at 3 μm (50$\times$ the sensitivity of SIRTF at $> 8\times$ the resolution).

While these are challenging goals, they are not out of line with reasonable developments of contemporary technology.

8. BREAKING AWAY FROM THE HST COST CURVE

The total cost of HST has been quoted as being in the vicinity of \$1.5B to \$2B. What is usually not realised is that a very substantial fraction of the cost of HST has been incurred by the software and hardware costs for the ground operational system and by the engineering analysis, spare parts inventory and additional management needed for the Maintenance and Refurbishment (M&R) program. Both these elements are driven primarily by HST's location in LEO. An additional factor was the lack of maturity of instrumental and spacecraft technology and the lack of experience with such a large, complex spacecraft. While it is not clear what the actual costs are, reasonable estimates place the ground and operational system costs at $\sim$ \$400M and comparable amounts for the M&R program. Thus the actual cost of the flight hardware system of HST is closer to \$1B than \$2B. This provides a valuable baseline number for discussions related to the cost of NGST.

Why should the cost of NGST not be proportionally larger by the usual scaling laws? There are several very good reasons why NGST would lie on a very different cost curve from HST. An obvious one is that HST is the first of the UV-Visible Great Observatories, and that it is based on technology that is now nearing 20 years in age. We have learnt a lot since HST was conceived, and technologies in many areas have advanced significantly (e.g., optics, electronics, computers and control systems, and instruments). Such technological advances will make a very significant difference to the construction and operation of NGST. Another useful guide to cost has been spacecraft weight. A preliminary analysis of the likely weight of an HEO NGST is that it will be *comparable to HST*. While not obvious at first sight, this result is quite plausible.

The technological developments that lead to substantial weight savings over HST are many. In addition, substantial efficiencies accrue from operation in HEO. Together they make a dramatic difference. First, new optics polishing and fabrication technologies (e.g., ion polishing, stressed lap polishing) will lead to lighter, higher-performance optics. Second, a simpler structural support for the secondary with active location to compensate for modest thermal and aging variations leads to a lighter and less demanding optical assembly. Third, the fast focal ratio leads to a short structure, and a very short baffle is practical because of the HEO location. Constraining the telescope to pointing no closer than 90° from the earth and the sun is realistic in HEO. Fourth, the instruments can be comparable to those in HST, and could well be modest developments from the HST Second and Third Generation systems. Fifth, the power requirements are lower and much less complex because there is no rapid charge-discharge battery cycling. Sixth, more durable and reliable body-mounted solar panels would be used. Seventh, HEO operation plus an active optical element for fine pointing, combined with large area detectors for field acquisition and guiding, would eliminate the three massive Fine Guidance Sensors (FGSs) and greatly simplify the Pointing and Control System. This has been one of the most demanding elements of HST. Finally, by taking the step to a non-man-rated, non-maintainable Observatory (except possibly for robotic replacement of cryogens, if active cooling systems do not reach maturity) considerable further savings can accrue.

While we have discussed NGST as being a single all-purpose UV to mid-IR telescope, it has been suggested that it may well be cheaper to design and configure two spacecraft, one for the UV-Visible and the other for the Visible-IR. This is not obviously the case. Technical feasibility studies need to be combined with cost tradeoff analyses to establish the most cost-effective and timely route to fruition of the program. The current baseline is to consider NGST as a single HEO telescope.

9. TECHNOLOGY ISSUES

A major element of the the MSFC studies and the workshops on NGST has been the identification of areas of technological development that are crucial for NGST to accomplish its scientific goals. The workshop in Pasadena organized under the auspices of the Astrotech21 Program had as its objective the generation of a list of such issues that needed to be addressed and resolved as the project progressed. The *Top Ten Issues* that resulted from this meeting (as noted by the author at this preliminary stage of the generation of the working panel reports) were:

- Can a single passively-cooled telescope be designed for the $\sim 0.1 - 10\ \mu$m wavelength region? Even if it can be, should it be, or should two telescopes be developed, one IR/Visible, the other UV/Visible?
- Where is the break-point in size between monoliths and segmented mirrors? While not universal, it was thought practical to have a monolithic mirror at 6 m, and impractical at 10 m and beyond; they would be segmented beyond 10 m. Is 8 m OK for a monolith? Should it be?
- How does one best carry out the conceptual development? This involves interplay between optics and optical technologies, optical design, structures, system dynamics, pointing and control systems, detectors, and instruments. Then, of course, one must develop a methodology for making the inevitable tradeoffs between the science goals, cost, technology, and space logistics and infrastructure (including projected launch capability).
- What are the best materials and approaches for fabricating the optics for such passively-cooled systems? It is particularly challenging where the surface errors required are below 10 nm; surface errors in the vicinity of 3 nm would be desirable for UV performance and low scattering. What will be the test strategy? Such demanding goals require that we develop a demonstration that the technology and methodology are in hand. A large, smooth,

accurate optic needs to be polished and then tested at the operational temperature of 100°K, and iterated until the figure component of the error budget is met.

- Can contamination of the large, cold optical surfaces be controlled such that the UV transmission of the system is not compromised? In effect, is a UV-Visible-IR system practical?
- Can all the required tests during the design/demonstration phase be carried out on the ground, or do tests or demonstrations need to be performed in space? What degree of subsystem and system level testing is appropriate during construction? How can system level tests be carried out on such a spacecraft?
- What is the likelihood of the availability of a launch vehicle (Heavy Lift Vehicle - Advanced Launch System, HLV/ALS) with the appropriately-sized shrouds? The current concept for HLV and the upgraded Upper Stage has the required sizing and capacity (more below). Is servicing desirable? What are the cost, risk tradeoffs? Will the technology be available in HEO? Should it? What are the prospects for robotic systems?
- Can the required pointing, tracking and slewing capabilities be developed for a system with such demanding requirements. The structure, the PCS, and any drive sources and cooling systems will have to be such as to limit tracking jitter to less than 1 mas. Such a stringent limit is required if the 10 mas diffraction-limited images of an 8 m NGST are not to be compromised. This may require an active optical element (e.g., the secondary) instead of body pointing. A simpler acquisition and guide system is required. Would array detectors with on-board processing to measure image centroids suffice? Such a system would save substantially on the weight and complexity compared to the HST FGSs.
- Will the appropriate management, oversight and review structures be in place? A repeat of HST's problems would not be desirable.
- The radiation environment in HEO is significantly worse than that in LEO. How can high QE detectors be operated in such environments so as to minimize any effects on the data? Can on-board processors be adequately rad-hardened for reliable operation?

To this list it would be very reasonable to add a few more specific areas which were not directly identified during the workshop, but which are necessary to carry out the scientific program. These are:

- Use of active optical and possibly structural components to correct for figure errors and for pointing control. This requires a sensing system for wavefront errors and control of the optical surfaces at low spatial frequencies. The primary mirror would then have actuators for active control of its figure. The system also would require active control of the location of optical elements (e.g., secondary) for compensation and for fine pointing.
- Attention will need to be paid to minimizing the mechanical noise in the system. If an active cooling system is to be used for the IR detectors, it must have very low levels of mechanical noise. The PCS system will clearly need low "noise" components.
- The instruments and detectors also pose a challenge. The overall throughput must remain high, and be combined with low read noise and on-board processing to ensure adequate removal of particle events. This may even involve close arrays for coincidence detection. The detectors will need to be mosaics of individual units to get the areal coverage required.

10. LAUNCH CAPABILITY

The current state of launch vehicles is in flux. A new generation of large capacity launch vehicles is being discussed and design decisions are being made. This is a joint Air Force/NASA ELV (expendable launch vehicle) program, which has variously be known as the ALS (Advanced Launch System) or the HLV (Heavy Lift Vehicle) program. Conceptually it is a modular system with a wide range of carrying capacities – to >100K kg in LEO. As well as its large lift capacity, a design goal is to ensure low cost access to space, a goal that could not be accomplished with the man-rated, very capable, but necessarily complex Shuttle system. The projected cost per pound is low, varying from $600/lb for the 50K kg model to $300/lb for the largest model (one tenth that of Shuttle which is $2-5000/lb). One assessment[13] of the HLV program noted that the large HLV with an upgraded Centaur upper stage was projected to have 12-28K kg lift capability to HEO with an envelope of 13-24 m length by 10-13 m diameter. Such an envelope and weight range is more than adequate for NGST.

To be more specific, for an 8 m NGST we will need to put a 9.5 m diameter, 15-20 m long envelope with mass $\sim$ 14-

18K kg (1.2-1.5× HST) into 100,000 km HEO. It is interesting to note that the USSR Energia has such capability, with the possible exception of an adequate upper stage. The discussions and the NASA projections shown at the Baltimore NGST workshop led to a consensus that the required launch capability would likely be available for large telescopes in the 2000-2010 time frame. No guarantees can be given to such projections, but the recent Augustine Committee Report[14] emphasis on new HLV capability adds to likelihood of the availability of such capability. I think that it is appropriate to take the view, as was noted in the presentation regarding launch vehicles at the Baltimore NGST workshop, that "space telescope planning should not be constrained to current launch vehicles". However, it is clear that NGST constraints do need to be input to plans for HLV.

11. MANAGEMENT ISSUES

With the realization that the spherical aberration problem on HST was not one that derived from limitations of technology, but one that resulted from limitations in management, review and oversight[15], it would be remiss of any discussion of a major program to ignore the challenges faced in the management of such projects. This challenge must be faced squarely, and must be given the same level of attention as that required for the technological developments. Management issues are at least as demanding as the technical challenges. Problems and mistakes are to be expected in projects of such scale. The process must have mechanisms that allow for the early identification and rapid correction of such occurrences. A crucial element of the successful management of such a project is ensuring that project managers, engineers and scientists of the highest caliber and experience are involved in the program and that their involvement is a long-term commitment. In addition, it must involve fully the end users of the mission in the process. That is, it must involve the scientific and engineering resources of the scientific community. The lessons from HST should not be neglected as we move ahead with large projects like NGST.

This is a complex subject, but a key step should be the early formation of a standing science-engineering working group (SEWG) from the broad space science community. This group should comprise a core of scientists and engineers who meet for frequent insight and analysis of technical and management issues. The SEWG should be fully involved in reviews and any cost-performance tradeoffs. An essential element of the success of this group in carrying out its role will be the technical support that is available to it for analyses. The SEWG input and the issues that it raises should have solid technical underpinning. This will require that the SEWG have a technical support team that broadens the expertise base of the SEWG and interfaces with the NASA and industry groups.

The SEWG should develop a close working relationship with the project manager. Ideally this should be a supportive relationship built upon mutual respect. The project manager and his or her team should have a long-term commitment to the program, with a clear understanding that they would be appropriately rewarded for their commitment. Within the project itself there should be clear lines of authority and responsibility. Unlike previous programs, there should not be any parallel division of authority between organizations. In addition to the SEWG and the Project structure, the designated science center should be implemented early and fully involved with technical support for continuing analyses.

The analyses and studies that are performed must extend through to system-level analyses, simulations and tests, with appropriate degrees of redundant tests. It would be extremely valuable to analyze systematically and objectively the HST experience, to use that as a learning tool, and to incorporate the experience of other large successful programs, *including some of comparable scale done within other agencies and non-government supported industrial projects.* Successful high-technology projects done within DOE, DOD and, for example, Boeing could well be utilized as an source for further experience for project management. A means of coupling those directly involved in such programs in some structured way (a small workshop or retreat?) that allowed open exchange of ideas and experience would be valuable for all.

The ability to successfully carry out large space science programs is governed in part by the degree of confidence that resides within the political environment that all the elements, science, technology, and management will come together to bring about its successful conclusion. Challenger and HST will dog us all through the difficult job of making a convincing case for such major projects in the future. We may find it impractical to obtain the funding for those key projects that are at the heart of astronomy if we fail again.

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