UV-Optical from Space

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EXECUTIVE SUMMARY

Observational capability from space in the UV, Visible and IR spectral regions will be crucial for substantial progress on fundamental astronomical problems that range from the study of planetary systems to understanding galaxy formation. The combination of uninterrupted wavelength coverage (ultraviolet through the visible to the mid-infrared), high dynamic range, diffraction-limited images and dramatically reduced near-IR backgrounds with passively-cooled systems (to $< 10^{-6}$ that from the ground and HST) would result in major advances in scientific understanding.

A balanced program of both Observatory-class instruments and smaller directed missions of Moderate and Explorer size is essential. The panel strongly recommends that both components be developed concurrently in a dynamic long-term program.

A long-term program of Observatory-class missions is recommended, starting with the full realization of the potential of HST through optimized operation and the rapid implementation of state-of-the-art image-correcting cameras and spectrographs. The critical role that Observatory-class instruments play requires that a successor to HST be flown within a few years of the end of HST's nominal life. The panel strongly recommends that such a successor be planned for launch in the first decade of the new century. This would be the 6 m LST, the Large Space Telescope. Operation beyond Low Earth Orbit would lead to large gains in the science return and substantial savings in construction and operational costs. Finally, the program has as its goal a telescope of astonishing power, the 16 m NGST (Next Generation Space Telescope).

Many high-priority science goals cannot be addressed with general-purpose large telescopes. Paralleling the Observatory program must be a vibrant program of Moderate and Small missions. Progress in the Explorer program is currently so slow that its primary goal of rapid access to space for innovative scientific programs has been lost. We recommend that the Delta-class Explorer program be enhanced to lead to more frequent missions and shorter turnaround. The missions should also be managed more directly by the PI team; substantial educational and training benefits would accrue from such a change. While the final selection of missions should be carried out through the normal peer-review process, examples of outstanding science goals include a UV imaging survey, a wide-field astrometric telescope, a multi-waveband mission and a very high spectral resolution instrument.

Interferometry promises to revolutionize the study of compact objects with small length scales such as AGNs, QSOs and interacting binaries. The panel recommends beginning a systematic program of technology development and ground-based experimentation that leads to a space-based imaging astrometric interferometer with baselines of tens of meters early in the next decade.

A more complete outline of the recommended program can be found in §II (Implementation of the Science Program). The recommended program is:

SIZE	PROJECT	Cost	Start	Finish
Large:	LST – 6 m HST Successor	$2000M	1998	2009
Moderate:	Explorer Enhancement	$300M	1993	2000
Moderate:	HST Third Generation Instruments	$150M	1994	2000
Moderate:	Imaging Astrometric Interferometer	$300M	1997	2004
Small:	SMEX UV Survey.	$30M	1995	1998
Small:	Space Optics Demonstration	$30M	1993	2000
Small:	Supporting Ground-based Capabilities	$25M	1993	2000
Technology:	Technologies for Space Telescopes	$30M	1993	2000

Large:

LST (Large Space Telescope):   The LST is a 6 m Observatory-class telescope incorporating UV to IR imagers and spectrographs. Passive-cooling and high-performance optics result in large gains in scientific capability over HST. A high priority goal is location beyond Low Earth Orbit (e.g., HEO – High Earth Orbit). This telescope is an excellent candidate for strong international participation. For operation by 2009 a start date of 1998 is considered necessary. Advances in technology and HEO operation will break away from the HST cost curve and lead to an expected cost of $2000M.

Moderate:

Rapid Explorer Deployment: The Delta-class Explorer program should be enhanced by more frequent missions and shorter development schedules. A wide range of forefront science can be carried out with such missions: contemporary examples are EUVE and Lyman-FUSE (the Extreme UV Explorer and the Far UV Spectroscopic Explorer). The programs should be chosen through peer review. An essential element is education and training of space scientists and engineers. The base of technical and managerial experience in space science must be increased by direct oversight and management of these programs by the PI team. Cost savings will accrue from such an approach. The expected incremental cost of the program is $300M.

HST Third Generation Instruments: The full potential of HST can only be realized with state-of-the-art instruments that compensate for its optical problems. A further set of such instruments should rapidly follow the WF/PC II (Wide Field/Planetary Camera) and the Second Generation Instruments. A high throughput camera is an example. The expected cost of the instruments is $150M.

Imaging Astrometric Interferometer: The potential returns from interferometry are high. An instrument with baselines beyond 20-30 m that demonstrates the needed technologies and offers substantial scientific gains should be developed for a new start late in the decade. The expected cost is $300M.

Small:

Small Explorer missions: A UV low spatial resolution all-sky imaging survey would return excellent science in this class. The UV survey would be particularly valuable for mapping diffuse UV emission from the hot and cold ISM. The expected cost of a Small Explorer mission (SMEX) is $30M.

Optics Development and Demonstration: Lightweight optics with high performance surfaces and active control are central to the goals of future telescopes, large and small. Technology developments and demonstrations are needed. The expected cost is $30M.

Supporting Ground-based Capabilities: Ground-based telescopes will continue to play an essential complementary role in support of space observations. They will continue to be particularly important for spectroscopic follow-up and for development and demonstration of new detector and instrument technologies. All-sky coverage is essential. The ground-based program should be augmented by $25M for 4+ m class telescope projects that support space observations.

Prerequisites:

Lyman-FUSE: The high scientific returns expected from the EUV and UV spectroscopic capabilities of Lyman-FUSE requires that it be completed and launched by the middle of the decade.

HST Optimization: It is crucial to realize the potential of HST through implementation of instruments that correct for its optical problems. Thus the WF/PC II should be completed as soon as possible, and the state-of-the-art Second Generation Instruments should be put on an accelerated completion and deployment schedule. Optimized operation is now even more crucial given the reduced efficiency of HST.

Near-term Science Program: Support for data analysis, ground-based observations, and modeling and theory is badly needed to give the scientific returns consistent with the capabilities and the large commitment of national resources to space science missions. Stable research funding of the science community and students is essential if the scientific productivity is to be commensurate with the investment in facilities. The ongoing missions such as HST, EUVE, IUE and ASTRO have and will make substantial scientific contributions. They require adequate support.

Technologies for the Next Century:

The Next-Century Program: Explorers, interferometers and large space telescopes depend critically on technology developments. Development of those technologies now will lead to major savings in cost, schedule and management complexity for the next generation of instruments. Such technological developments will be crucial for observatories of the scale of the NGST, the 16 m Next Generation Space Telescope.

Structure of the Panel Report.

The report is organized as follows:

I. THE SCIENCE PROGRAM.

The UV-Optical in Space Panel has been considering the science goals for a broadly-based program of space missions that cover the wavelength range from the EUV at ~ 100 Å into the near-IR at ~ 5-10 μm. The moderate (Explorer-class) and large (Observatory-class) missions outlined here allow for fundamental advances in almost all areas of astrophysics. They range from the detection of planetary systems around nearby stars to cosmology. The science program developed by the panel is outlined below.

Planetary systems.

The detection of planets beyond the solar system was given great impetus in the last decade with the discovery by IRAS of radiation from cold disks of dusty material around nearby stars. Detection of the light from planets, and the subsequent spectroscopic observations, are important goals for the coming decades and can very likely be attained through the design of space-borne instrumentation directed to that end. The technological requirements are challenging. A low background telescope with apodizing, occulting or interferometric instruments will be required. The telescope will need to be cooled (passively) to $\sim 100°$K or less, and to have extremely smooth, low scattering optics. These optics must be smoother than required for diffraction-limited performance. While the detection of planets like those in our own solar system is likely to

be beyond the reach of HST, a 6 m telescope with higher performance optics and passive cooling would be able to detect gas giants like the outer planets (Jupiter and Saturn) in our own solar system out to beyond 10 parsecs, even against the huge background from the central star.

To take the next step and detect Earth-like planets requires a substantial step in capability, especially since detection alone is not enough. We need to take spectra that can confirm whether the atmospheres of the detected planets could support life. The spectroscopic measurements must establish whether their atmosphere is in a chemical disequilibrium state that might indicate the presence of life (e.g, by detecting the 9.5 μm Ozone band).

The technical challenges confronting the detection and measurement of Earth-like planets are substantial. Such planets would be found $\approx 0.25''$ from a star at a distance of a few parsecs. An optimal approach would be to detect such an object at ~ 10 μm with a cooled, 16+ m telescope where the first dark diffraction ring corresponds to the planet's orbit. Apodization or interferometric instruments would be used to greatly enhance the contrast of the planet against the light from the star. Then the telescope's low resolution spectroscopic system (with $\lambda/\Delta\lambda \sim 100$) would be used to obtain a spectrum to search for the signature of ozone (O_3) at 9.5 μm. Other weaker features, e.g., methane, could also be the goal of such a spectroscopic analysis, albeit with data of higher S/N and spectral resolution. Even with low resolution spectra it appears likely that satisfactory discrimination could be obtained between different atmospheres. For example, the thermal emission spectrum from the inner planets in our own solar system (Mars, Earth and Venus) differs greatly.

It is probably no exaggeration to say that the impact on humanity of successful detection of a planet with an atmosphere like that of the earth would be similar to that of Galileo's detection of the moons of Jupiter, following the invention of the telescope.

Star formation and origins of planetary systems.

Stars form through a series of complex physical processes. While absorption limits the study of much of the activity in star-forming complexes to long wavelengths, substantial investigations can be carried out in the near-IR and shorter wavelengths. The resolutions available at such wavelengths are a better match to the characteristically small length scales in protostars. Of particular scientific interest at these wavelengths is the study of protostellar disks and outflow jets. The resolutions and sensitivity of space telescopes will allow the derivation of physical conditions (temperatures and densities) along with kinematical and structural maps. 1000 AU diameter disks could be mapped with ~ 15 independent resolution elements at 1 kpc with the 6 m LST.

While our own Galaxy provides a laboratory for the study of star forming complexes at a level of detail that is impossible in other galaxies, different environments in other galaxies make such regions of particular interest. We would like to know how the formation rate for massive stars and the form of the mass function at high masses are affected by physical conditions that are found only in other galaxies. These include normal galaxies like our neighbors such as M31, NGC 205 and M33, and the LMC and SMC, as well as the more massive systems such as M101. Moreover, galaxies undergoing violent kinematic interactions such as "starburst" galaxies are of particular interest since they may be representative of conditions that were common during the formation and early evolution of galaxies. Large space telescopes that combine high spatial resolution with low backgrounds are essential for tackling this problem.

In general, the spatial resolution that will become available with HST, with the 6 m and ultimately with the 16 m will be crucial for unravelling some of the complex structural and dynamical characteristics of protostellar structures. The 6 m LST would resolve 3 AU in the nearest star-forming complexes, or 8 AU at Orion at 0.5 μm in the visible. At 3 μm the resolution would be ~ 20 AU and 50 AU respectively. Furthermore, the low backgrounds and high dynamic range available with space telescopes, particularly passively-cooled telescopes, will broaden the range of problems that can be tackled. With cooling to $\sim 100°$K, the background out to ~ 10 μm can be > 18 mag fainter *per resolution element* ($< 10^{-7}$) than that from the ground.

Structure and Evolution of the Interstellar medium.

Fundamental information on the composition of interstellar gases and evidence for spectacular differences in their physical states has resulted from observations of the rich assortment of UV absorption lines in the

spectra of background stars. Unfortunately, few of the spectra have had the velocity resolution to clearly differentiate the different underlying structures within gaseous regions. These regions could be responding to a variety of undetermined physical influences, such as ionization, dissociation and recombination fronts, thermal- and photo-evaporation interfaces, subsonic wave phenomena, and ionization and cooling from shock fronts. These phenomena can trigger important chemical changes or shifts in excitation and ionization over very small scales in distance or velocity. The interplay of such processes is important in the exchanges of mass and energy between different phases of the medium, and can be influential in regulating the large scale properties of gases within the disk and halo of the Galaxy.

With the high resolution spectrograph on HST (particularly with STIS, the Second Generation spectrograph) faint objects can be measured with a wavelength resolving power of about 10^5. Evidence for complex, narrow absorption lines in the visible indicate that studies of physical processes in the interstellar medium (ISM) would be greatly rewarded by having results from UV spectrographs which could observe bright sources with a resolving power of a few million. The technologies for UV echelle gratings and 2-dimensional detectors are mature enough that it is now possible to achieve good sensitivity and resolution to study bright stars (to 5th magnitude) with a small instrument (effective collecting area ~ 10 cm^2).

Much remains to done through imaging studies of the ISM. Observations on a variety of spatial scales in the UV would also help in elucidating the composition, physical properties, and spatial distribution of dust, interstellar H_2, and supernova remnants. These observations of interstellar emissions and scattering complement the spectroscopic measurements. Instruments with wide field coverage and high sensitivity to diffuse emission are needed to complement the capabilities of ASTRO and large telescopes such as HST and LST. Explorers, both SMEX and Delta-class, could lead to major gains in our knowledge.

Just as hot stars are used to probe the interstellar medium of the Galaxy, QSOs and AGNs can be used to probe the interstellar medium of much more distant galaxies. Contained in the investigation of QSO absorption lines are fundamental astrophysical questions: What is the history of chemical enrichment in various galaxy environments? How are the physical conditions in the gaseous medium influenced by the host galaxy properties and by the extragalactic radiation field in which the host galaxy resides?

Observations with HST and Lyman-FUSE will allow direct comparison of physical conditions in low-redshift absorbers with those in high-redshift absorbers as measured from ground-based data. From observing lines-of-sight through the halo of the Galaxy and local group galaxies, the gas phase depletion pattern can be determined as a function of stellar metallicity. Because the low redshift absorbers can be associated directly with host galaxies, the problem of current gas-phase metallicity as it relates to the history of star formation can begin to be addressed.

The key to relating QSO absorption lines to galaxies lies in obtaining observations in the redshift range in which both can be well studied ($z < 1$). HST will only begin to pursue such studies at high spectroscopic resolution because of photon limitations and the low surface density of bright QSOs. Larger aperture space telescopes such as LST will result in very substantial increases in the number of sources that can be studied. The physical conditions in the absorbing interstellar medium of galaxies could be directly related to other observable properties of the host galaxy. Unreddened B stars could be used to examine the interstellar medium of Local Group galaxies for velocity structures in star-forming regions, for metallicity as a function of star formation rate and history, and for gas phase depletion as a function of local metallicity.

With their high throughput and their capability for high-dispersion UV spectroscopy, LST and even larger telescopes such as the 16 m NGST are well-suited to such studies. A strong scientific motivation for LST is to raise QSO absorption line studies to the level where the chemical and dynamical evolution of interstellar and halo gas can be determined as a function of cosmic time.

Stellar astrophysics.

One of the great triumphs of modern astrophysics was the development in the 1960s and 70s of a broad understanding of the evolution of single stars from the gaseous cradle to the dense stellar grave. This rush of progress followed from parallel developments in observation and theory. Technical advances made it possible to measure stellar luminosities and temperatures accurately and simply, and to model the evolution of stars in the Hertzsprung-Russell diagram with detailed numerical calculations.

Our understanding of the evolution of close binary stars, however, is still at the phenomenological level. We can recognize main sequence stars at the start of their joint lives with the primary filling its Roche lobe; we notice peculiar chemical signatures in some red giants that could be explained by binary mass transfer;

we see spectacular displays from the accretion disks of dwarf novae; we suspect that the origin of Type I supernovae is an endpoint of close binary evolution. A physical understanding of close binary evolution, however, still awaits the crucial observations, in this case high angular resolution images of stars interacting gravitationally.

Near-term observational progress in this field will come from recognition of hot and compact companions of stars in the UV with HST, and from resolution of binaries with milliarcsec separations with the first imaging interferometers. In the longer term we can look forward to imaging the nearest W UMa and U Gem systems and investigating the accretion disks of cataclysmic variables. These observations call for 10 to 100 μas resolution.

High angular resolution astronomy will also challenge our understanding of single stars by revealing surface structure and variability. These observations will call for more sophisticated models than the current one-dimensional approach.

Much more insight into the cooler phases of stellar evolution (most importantly, the evolution of young pre-main sequence stars) followed the extension of observational techniques into the infrared. The low background in space will allow substantial further gains. Near-IR observations could place valuable constraints on the existence of baryonic dark matter.

There are important areas of stellar evolution that similarly await improved capability in the ultraviolet. The evolution of very massive objects remains a subject of intense theoretical interest from the points of view of both chemical evolution and black hole formation. The UV excesses in elliptical galaxies clearly call for a real understanding of post-asymptotic-giant-branch evolution. A suitable laboratory for studying the evolution of massive stars and post-AGB evolution exists in the Magellanic Clouds. An appropriate survey is needed, as the hottest post-AGB stars currently go unrecognized because their optical energy distributions are insensitive to temperature. An excellent example of the significance of the Magellanic Clouds for stellar evolution was furnished by SN1987A, an event to which space astronomy was able to respond remarkably well, and which produced a major leap forward in our knowledge of the evolution of massive stars. High angular resolution observations are of special importance in the study of massive stars (such as R136a in the 30 Dor star formation region) which need to be separated from their coeval companions.

Stellar populations.

In the standard Big Bang cosmological model, baryonic matter forms as diffuse gas consisting almost entirely of hydrogen and helium. Due to remarkable effects associated with gravity and nuclear processes, baryons at the present epoch have become concentrated into stars within galaxies containing a full range of chemical elements. Since lower mass stars have nuclear burning lifetimes that exceed the age of the universe, information about the full evolution from a gaseous to a stellar universe is retained in the stellar populations of nearby galaxies. Conversely, conditions at the time of galaxy formation, when the universe was making the transition from gaseous to stellar states, can be ascertained from a solid understanding of stellar populations in combination with sensitive observations of faint, high redshift objects.

Both of these approaches require an extension of knowledge of fundamental properties of normal stellar populations. In the Galaxy the description of the stellar mass distribution function needs to be completed; e.g., by reaching stars below the nuclear burning mass limit through determinations of the space densities of brown dwarfs. A census of low luminosity, compact stellar remnants is also required, especially in very old systems such as globular star clusters. Ancient white dwarfs, neutron stars, and black holes are significant, both as semi-dark forms of baryonic matter and as indicators of the properties of stellar populations when the Galaxy was young. Explorations of the extremes of the Galactic fiducial stellar population will require multi-wavelength, high sensitivity observations, e.g., in the near infrared with HST and its successors to find brown dwarfs, and across the EUV-UV-Visible to find old white dwarfs. Explorer-class missions could also contribute to aspects of this problem. The search must reach V = 29 in the nearest globular clusters and a magnitude fainter in the halo far from the Galactic disk. *Claims that the halo of the Galaxy is non-baryonic cannot be taken seriously until a census is complete to at least this level.*

A direct complement and check on the study of galaxy evolution by "looking back" at galaxies of significant redshift is the determination of the star formation history of nearby galaxies. By measuring the main sequence luminosity function down to one $L_\odot$ it is possible to determine the time-history of the star formation rate in a stellar system over the last 5 billion years. To date the experiment has only been performed in the solar neighborhood and in the Large Magellanic Cloud, but our understanding of the star

formation history of all spiral galaxies is generalized from these results. An important goal is to extend the experiment to the full sample of galaxies available in the Local Group. This requires imaging to V = 30 with spatial resolutions of $\mathcal{O}(10\text{-}20)$ milliarcsec – these requirements are beyond what will be achievable with HST.

With such a limit, color-magnitude diagrams could be obtained in the nearest galaxies (M31, M32 and M33) to $M_V > +5$, below the main sequence turnoff for globular cluster-like populations. This would enable age determinations to be obtained in the oldest populations of these galaxies. These ages constrain the formation epoch of galaxies, and also constrain cosmologies. The initial mass function (IMF) could be determined through much of the Galaxy (to masses ~ 0.1 $M_\odot$ out to several kpc). IMFs could be derived from the luminosity function to $M_V \sim +18$ in the nearest globular clusters and to $M_V \sim +12$ in the LMC and the SMC. The LST would meet the goal of imaging to V=30 mag. With a high-throughput camera it would give S/N=10 measurements in 10^4 s in the visible (V) for large numbers of stars fainter than 30 mag over a field of several square arcmins.

Once stellar populations have been properly sampled in nearby galaxies and the results placed in a physical model, we will be in a better position to interpret hard won multi-waveband measurements of the light from distant galaxies. Empirical models of stellar populations can be obtained most directly by resolving individual stars throughout the UV-Visible-IR. Detailed physical studies of stellar populations are currently carried out within the Galaxy. High angular resolution, multi-wavelength observatories in space will extend these studies to Virgo supercluster galaxies, where a full range of morphological and evolutionary galaxy classes can be sampled.

In addition, our knowledge of the frequency of formation of massive stars is biased to those which are luminous at visible wavelengths. Really massive stars would not necessarily be those which are most luminous visually; most of their radiation is emitted in the ultraviolet region of the spectrum which is accessible only from space. Wide-field surveys in the UV would not only determine the distributions of hot white dwarf and subdwarf stars in the Galaxy, but would also map the massive star distribution in the Magellanic Clouds and other Local Group galaxies.

The galactic and extragalactic distance scale.

For the best part of this century astronomers have been ham-strung by the need to estimate the distances to stars outside the solar neighborhood by indirect methods. This has often led to circular arguments by assuming the astrophysical theory we were trying to test by observation! In the next decade we can begin to find our way out of this historic impasse. By means of optical interferometry it will be possible to measure the distance of every detectable star in the Galaxy geometrically. Ten μas precision in positions is a realistic goal for an astrometric Interferometric system. If we can determine the distances of a sample of Cepheids and RR Lyrae variable stars in the Galaxy, we will improve the precision of the extragalactic distance scale (which will be studied by HST in the coming decade), and refine our estimate of the expansion age of the Universe. Such astrometric precision would also permit measurement of the proper motions of Local Group galaxies (and far beyond – even to Virgo!) and yield an accurate dynamical age and mass of the Local Group.

For megaparsec distances the most reliable distance indicators are Cepheid variable stars. Using Cepheids, HST with the WF/PC II will measure distances to the Virgo cluster (redshift 1000 km s^{-1}). In the last decade significant peculiar velocities have been measured for galaxies at redshifts of 3000 km s^{-1}. An accurate determination of the Hubble Constant requires that the expansion field be corrected for large scale flow velocities. These are at present mapped with secondary distance indicators. The LST with its significantly higher angular resolution would permit detection of Cepheids out to Coma. The LST would permit our model of these flow velocities to be tested directly with Cepheid distances, an important check on HST's H_0.

A new technique that is based on measurement of the fluctuations in surface brightness that arise because of statistical clumping of stars appears to result in very precise measurement of distances – possibly to accuracies of 5%. This technique also has the potential for allowing valuable constraints to be placed on the age and metallicity distribution in the stellar population. While HST, in conjunction with observations from large ground-based telescopes, will probably allow precise measurements out to Coma, extension beyond that will require larger space telescopes. The possibility that galaxies are clumped and structured

on scales larger than 10,000 km s^{-1} and so will require accurate distance determinations to beyond such velocities, is a further argument for LST, and its successor, the 16 m NGST

Nature of galaxy nuclei, AGNs, and QSOs.

It has been known for over a quarter of a century that many galaxies possess highly luminous nuclei. QSOs are the most dramatic examples. More recently, it has become clear that this is a widespread phenomenon and that the majority of galaxies, including our own, exhibit activity at some level. The most common explanation of these phenomena involves black holes with masses between a 10^6 and 10^9 M$_\odot$ orbited by extensive accretion disks. However, the black hole models remain unverified. A new generation of UV-Visible space telescopes would place our understanding of active galactic nuclei on a firm footing and should help us to develop theories of galaxy evolution that parallel existing theories of stellar evolution.

A 10^8 M$_\odot$ black hole has a size comparable with that of the Earth's orbit. So for the closest active galaxies, at distances of order 10 Mpc, this subtends an angle $\sim 10^{-7}$ arcsec, far too small to be resolved by any telescope. However, its presence can be detected indirectly, through its dynamical influence on the surrounding stars which move at $\sim 10^{-3}$ the speed of light and are therefore influenced out to a 10^6 black hole radii, or ~ 0.1 arcsec for nearby galaxies with a 10^8 M$_\odot$ black hole. Observation of this effect has been attempted, with promising results, on a few nearby galaxies both active and inactive, using ground-based observations where only a few resolution elements are possible. HST, with its tenfold increase in resolving power but a lower sensitivity than ground-based telescopes, should allow some improvement to be made in these measurements.

Observations with high dynamic range and angular resolution superior to ~ 0.1 arcsec will be necessary in order to weigh nuclear black holes and elucidate their fueling in a variety of galaxies (or to the contrary, demonstrate that black holes are not responsible for nuclear activity).

Most AGN radiation appears in the UV and probably originates from the inner parts of an accretion disk. UV emission is also responsible for exciting the broad emission lines that are the primary indication of nuclear activity. Without data in the UV it has not been possible to measure AGN powers to better than a factor of ten. In order to understand the compete spectrum of an AGN, it will be necessary to observe high-redshift QSOs in the UV with magnitudes fainter than ~ 19 mag, too dim to be seen with Lyman-FUSE. This would enable photon energies as large as 60 eV in the QSO rest frame (within a factor of two of the energies of photons observable in nearby AGN with X-ray telescopes like AXAF) to be detected. These measurements should also provide crucial tests of theories of accretion disks.

Most QSO and Seyfert infrared radiation is now believed to be re-radiated by dust in orbit around the active nucleus. For the closest active galaxies, the expected size of the 10 μm infrared source is ~ 10 pc or ~ 0.1 arcsec. Fainter infrared emission may be detectable from closer low-luminosity AGNs.

Radio-loud QSOs and radio galaxies produce non-thermal jets, some of which have been detected at visible and UV wavelengths. Lower power bipolar outflows have been observed from Seyfert galaxies and are known to be related to the narrow emission line regions. Continuum imaging of visible and UV jets could greatly clarify the particle acceleration mechanism. Line imaging may allow a determination of the outflow speeds, which cannot be measured using radio techniques.

The required advances in angular resolution needed for both the spectroscopic and the imaging studies must come from space-based interferometry. For example, a 50 m baseline deployable interferometer could yield images with resolutions of 2 milliarcsec at 400 nm (5×10^{17} cm at Virgo). Ultimately 10 km baseline interferometers on the moon may resolve accretion flows at optical wavelengths.

Formation and evolution of galaxies at high redshifts.

A high-priority scientific objective for large space telescopes is the study of galaxies at high redshift. The imaging and spectroscopy of galaxies at redshifts $z > 1$ is crucial for understanding how galaxies form and evolve. While the epoch of galaxy formation is very uncertain, existing data suggests that the redshift range of 2-4 is a particularly promising region for exploration. The recent results for radio galaxies, the deep galaxy counts and color surveys, the long standing results from QSO studies, and the significant evolution seen at redshifts $z < 1$ all support such a view, as do evolutionary N-body models.

In particular, these models and the data suggest that galaxy formation involves the interaction and assemblage of galaxies from clumps. Galaxies as we know them today would build up through interactions

and merging of such clumpy subunits. While such a view should be adopted with caution, it does give guidelines as to the capabilities needed to detect, map and establish the physical and kinematic conditions in protogalaxies. What is clear is that both resolution and sensitivity will be crucial, for spectroscopy as well as for imaging. The characteristic scale of structure in galaxies is hundreds of parsecs. Resolutions that match these scales are of particular importance for understanding the evolution of galaxies, since much of the structure in galaxies occurs on such length scales of 100 pc to 1 kpc (e.g., star forming regions, spiral arms, merger "arms and tails").

Thus resolutions substantially better than 1 kpc are required, with sufficient collecting area to obtain images and spectra of objects fainter than 27 mag. IR observations will be important as the redshift moves much of the information out beyond 1 μm. Cooled, low background telescopes are therefore necessary.

HST will offer significant gains over ground-based observations when it is equipped with image-correcting instruments, but will typically be limited by its light-gathering power to objects at $z < 1$ for this extremely difficult problem. The 6 m LST and particularly the 16 m are highly suited to this task. They have the combination of resolution and sensitivity needed to detect and measure structure in $z > 1$ "galaxies". LST will provide an essential intermediate step in capability from HST to allow continuing progress in this fundamental area.

LST gives resolutions of 15 milliarcsec at 400 nm. A resolution of 15 milliarcsec corresponds to $\sim$ 150 pc resolution in *galaxies at any redshift*, given currently accepted cosmological parameters. This is remarkable. This is within a factor 2 of the resolution with which we see Virgo galaxies from the ground (1 arcsec is $\sim$ 75 pc at Virgo). The resolution on high z objects would still be $\sim$ 1 kpc in the zodiacal background minimum at 3 μm (see below). The 16 m NGST would improve on this, and bring its greater collecting area to bear as well.

Simulations of the imaging power of the 16 m NGST are shown in Figure 1. These simulations were made by Jim Gunn for "The Next Generation Space Telescope" workshop held in Baltimore in 1989. Galaxies at a redshift $z \sim 1$ appear as though they were nearby objects being observed from the ground! While HST's abilities in this area are impressive compared to that available from the ground, the contrast with the 16 m is striking.

The simulations were made using images for a typical nearby ScI-II spiral galaxy. They were made assuming no luminosity evolution at a redshift $z = 1$ and for images taken in the rest-frame B-band (observed at 9300 Å with a 20% wide filter). They were assumed to be taken with a CCD camera with realistic throughput and noise characteristics. With the wide field of the 16 m and mosaics of CCDs similar to those now being obtained, hundreds of such galaxies could be imaged in each exposure. The galaxy frame (of NGC 2903 at 10 Mpc) used in the simulations was taken in 0.8 arcsec seeing conditions; any worse and the image would not have been adequate to simulate the capability of the 16 m at $z = 1$!

In addition to the resolving power and sensitivity of these telescopes, the low IR background will become increasingly important for the study of high redshift objects. It is in the near-IR at around 3-4 μm that the sky is darkest from space. This minimum occurs in the transition zone between zodiacal scattering and emission. The background at 3-4 μm with a passively-cooled space telescope will be $\sim 10^{-6}$ of that from the ground, a substantially greater gain than the 10^{-3} decrease expected in the 1-2 μm region accessible to HST.

The light from the stellar populations in very distant high-z galaxies will be redshifted into this region, as will many of the important emission lines. Observations at these wavelengths are then of particular importance since comparisons with nearby well-studied galaxies will require spectra and colors that correspond to visible light in the rest-frame. Already, but with great difficulty because of the high background, 2 μm observations from the ground are being used to try to determine the contribution of an "old" stellar population in high-z objects.

The Second Generation IR camera for HST (NICMOS) with its 1-2.5 μm imaging capability will allow very substantial gains to be made, but the background from the "room temperature" HST and the limited resolution ($\sim 0.25''$ at 2.5 μm) will not allow HST to fully exploit the potential of space observations in this region. The combination of low background, resolution and spectroscopic performance from a large collecting area will likely prove essential if we are to probe this redshift range in a direct way. In other words, telescopes such as LST and the NGST will probably be essential if we are to understand fully galaxy formation and evolution in the redshift range $z \sim$ 1-5 (or beyond to $z > 5$?).

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Figure 1. Simulations of the potential imaging performance of HST and of a 16 m diffraction-limited telescope, for a $z = 1$ Sc spiral. a) Left panel. A three orbit (approximate 2 hours) exposure with HST and the WFC of the replacement camera, the WF/PC-II. The (undersampled) resolution is about 1 kpc. b) Right panel. The 16 m with a 2 hour integration. The imaging performance is spectactular. 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. The LST would have intermediate performance.

Cosmology.

The fundamental cosmological parameters remain uncertain. Accurate measurements of the geometric parameters (the Hubble constant H_0 and the "deceleration" parameter q_0), the age of the Universe t_0 and the density of the universe Ω_0 are needed. In addition, accurate abundances of the cosmologically important elements, deuterium, lithium, and helium are also needed. Several of these problems have been addressed earlier, particularly the determination of the Hubble constant and the limits on ages from age-dating the oldest stellar populations. A key issue is the comparison of the cosmological clock with the stellar nuclear clock.

The Deuterium abundance is a high priority for Lyman-FUSE, and one of its outstanding scientific goals. The abundance of deuterium is a sensitive diagnostic of conditions in the early universe and its determination is of particular interest for issues relating to the baryonic content of the Universe.

New approaches to establishing distances to distant galaxies that will utilize the spatial resolution of HST, and subsequently LST, would allow tighter limits to be placed on q_0. Examples are the use of distant supernovae and those techniques that depend upon combinations of structural parameters and kinematic data, such as the Fisher-Tully relation for spirals. A similar relation is now known for ellipticals. Comparisons of q_0 with Ω_0 (determined locally from large-scale motions), and also of t_0 with H_0 and Ω_0 are likely to have interesting cosmological implications (i.e., the value of Λ). Fundamental tests of the simplest Big Bang cosmological models will be made by combining data on the formation epoch of galaxies, inferred from observations of distance galaxies and from the ages of populations in nearby galaxies, with these determinations of H_0, q_0 and Ω_0.

The nature of the dark matter remains enigmatic. While many tests will be performed by ground-based

surveys (e.g., for brown dwarfs and microlensing) the spatial resolution available from space telescopes will play a crucial role in defining the dark matter distribution in galaxies and clusters (and elsewhere) through gravitational lensing studies.

Another area of considerable interest for its cosmological implications is the question of structure in the universe. The large-scale clustering of galaxies poses particularly challenging theoretical constraints. These are becoming increasingly difficult to reconcile with current models. If significant structure really exists on scales greater than 10^4 km s^{-1} careful mapping of the distances of large samples of galaxies will be required. Developing accurate distance measurements will require application to samples at $z \sim 0.1$. Many of these techniques require high spatial resolution and so need space telescopes, such as HST, LST and interferometers.

Such telescopes will also have a particular role to play in investigations of the evolution of structure with time, both directly at high redshift and through measurement of QSO absorption lines. The characteristics of galaxies at higher redshift as a function of galaxy density will be of particular interest.

The determination of the shape, amplitude and origin of the fluctuation spectrum will be potentially the most significant link between astrophysical constraints and work of the particle physics community with the supercollider. The observational difficulty of answering these cosmological questions and deriving the fundamental parameters will require a concerted long-term ground and space-based program.

II. IMPLEMENTATION OF THE SCIENCE PROGRAM.

The UV, Visible and near-IR region lies at the center of contemporary astrophysics. This is due in part to the vast historical base of information and understanding that has been developed over the last century of observations with ground-based telescopes, and the last few decades of exploratory UV observations. It is also due to the physical nature of the universe, in that much of the baryonic matter in the universe can be studied and diagnosed through its radiation in this region. Space astronomy in the 1990s and into the next century offers opportunites in this regime for fundamental improvements in our ability to probe and understand the universe. The Great Observatories program, with HST at its heart, exemplifies the central role that UV, Visible and near-IR region plays. EUVE and Lyman-FUSE further demonstrate the potential of missions in this region.

With the launch of HST a new era has begun for space astronomy. The initial steps have proven to be more difficult than should have been the case. However, HST's optical problems, while serious, can be overcome in part, and will likely result in a telescope that can meet most of its original objectives. The importance of large space observatories should not be lost in the discussions that result from this failure. The problem did not result from the requirements outstripping the available technology, but was a failure of testing and oversight. The complexity of the management of large projects should not be underestimated. The HST experience indicates that the management, review and oversight process needs to be given as much visibility as the technical challenges that face a project of such scope. The success of the high energy physics community both here and in Europe in managing large, complex scientific projects at the forefront of technology shows that such projects can be successfully carried out. It is crucial that the lessons from these projects and the experience with HST be taken to heart by all the participants in future projects. In particular, the full involvement of the scientific and engineering resources of the science community is essential.

A coherent long-term program driven by scientific and technical opportunites needs to be developed and implemented to build upon this first Observatory mission. A central element of such a program would be a continuing series of long-lived missions that are directed at tackling and solving the fundamental issues of astrophysics. These include the formation of stars and planetary systems, the formation and evolution of galaxies, and the structure and geometry of the universe. To do so will require that we fully exploit the potential of space observatories. While new technologies and launch capability will be needed, substantial gains can be made through incremental utilization of developments in these areas.

While observatories like HST are central to the program, a crucial element is a parallel program of continuing missions that explore and exploit areas that are not possible with large observatories. Examples are spectroscopic and imaging systems in the far UV and the EUV; the potential of high spatial resolution offered by interferometers; the gains from precision astrometry of large samples of objects; concurrent observations across a broad wavelength region, particulary of time-dependent phenomena; and all-sky

surveys for both point sources and for low-surface brightness structures. As COBE has recently shown and as EUVE and Lyman-FUSE will show, data of great importance to astrophysics can be obtained within the very cost-effective directed missions of the Explorer class.

Such missions also can play a critical role in both the technical and managerial training of scientists, instrumentalists and technologists. Such training and experience is necessary for the synergism needed between the science community, industry and government that is critical for the long term health of a space science program, and for future Observatory-class missions. A complementary, viable and dynamic program of Explorer-class missions managed and implemented by PI teams is an essential component of the overall program.

The Observatory-class mission program begins with HST. Its potential must be realized through the implementation of image-correcting instruments, starting with the WF/PC II. The Second Generation instruments, the near-infrared camera and spectrograph NICMOS, and particularly the imaging spectrograph STIS should be redesigned, developed and implemented as quickly as possible. A state-of-the-art Third Generation instrument set should follow the Second Generation instruments. An advanced camera would be an example of such an instrument. The importance of optimizing the operational effectiveness of HST should not be overlooked. It will pay handsome scientific dividends. HST must be supported until its successor is available, so that its crucial capabilities in the UV, Visible and the near-IR are not lost in the era of the Great Observatories.

The problems associated with HST emphasize the need for an early development of its successor, the Large Space Telescope, LST. The LST would be a 6 m-class passively-cooled UV-Visible-IR space telescope. Substantial gains in capability and science productivity are possible through implementation of developing technologies, and through its location in High Earth Orbit (HEO). It is recommended that this be a truly international partnership amongst the major space nations. With the global interdependence of nations, and the dramatic changes which are leading to common interests, such a scientific venture could act to cement these common goals in this last frontier.

Planning and development of its successor must begin soon if it is to be available within 5 years of the nominal end of HST's life. The experience gained with HST and the maturing of instrumental and optics technologies should allow us to build a substantially more powerful successor to HST on a shorter timescale and at reduced cost relative to HST. *We recommend that a compact, passively-cooled telescope be planned for deployment in 2009.* With concept development and planning beginning in 1993 and a new start in 1999, the launch of LST in 2009 is a realistic goal.

The compact design of LST in HEO and the use of optics and control technologies unavailable for HST should lead to an overall weight comparable to HST. Its instruments can be developments of the mature technologies that will be used for the Second and Third Generation HST instruments. By passive-cooling to $\sim 100°K$, LST will have a background at the zodical light minimum at 3 μm that is *six* orders of magnitude less than from both HST and the ground. Images of 10 milliarcsec at 0.25 μm could be expected from improved optical performance.

Finally, the program leads to the implementation of an observatory of astonishing power, the Next Generation Space Telescope (NGST), a 16 m UV-Visible-IR telescope. The technical and logistical challenges of such an observatory are substantial, but they are by no means insurmountable. We should also utilize some of the technological developments for the 16 m in the 6 m successor to HST. By so doing we will not only obtain a telescope of substantially improved capability, but will further refine the technologies in a working observatory. The modular nature of a 16 m telescope and the required long life make the Lunar Outpost a natural location for the NGST, particularly if it is combined with a very large km-scale interferometer. The km-scale interferometer complements NGST and allows for study of outstanding astrophysical phenomena on microarcsec to milliarcsec scales that cannot be reached by other means.

The goal of these wide ranging efforts is deeper understanding of the universe. Scientific understanding comes through careful data analysis, extensive modeling and concurrent theoretical developments. Only if such activities are adequately funded can the great investment in facilities be appropriately redeemed. Long-term funding for students, researchers and for the equipment necessary for their research programs is a crucial element of the program. We applaud the very substantial gains made in this area within NASA over the last few years. Such funds should continue to be routinely associated with new missions and observatories at a level that allows the scientific returns to be commensurate with the cost of these missions.

All-sky access to ground-based telescopes for a variety of observations, but notably spectroscopic follow-up, will continue to be needed. Spectroscopic observations are invariably time-consuming, but provide the physical information needed for understanding. The unique capabilities provided by high-performance spectrographs on space observatories, will need to be complemented by efficient spectrographs on large ground-based telescopes; they will be needed into the indefinite future. Continuing access to large 4+ m ground-based telescopes in both hemispheres, and the funding necessary to support those programs is an essential element of the astrophysics program.

High performance optics are a central component of all instruments in this wavelength range. The scientific goals demand more difficult, larger and smoother optics. Weight will continue to be a concern, particularly as the gains to be realized from HEO operation and beyond become a driver for missions. A further concern driving one beyond LEO (Low Earth Orbit) will be the increasingly high probability of damaging collisions with space debris. As optics and hence structures get larger the difficulty of pointing and tracking increases. Moving away from fine pointing by body pointing to fine pointing with an optical element is a very desirable goal. All these concerns lead to the need for lightweight optics with high performance surfaces with active elements. The need for a technology development and demonstration program is clear. It will return great value across missions of all scales in the UV-IR region.

III. THE OBSERVATORY-CLASS MISSIONS.

The science objectives call for an enhanced program of long-lived, large telescopes with imaging and spectro-scopic capability from the UV to the mid-IR. HST is the first of this new class of Observatories. It would be followed by the 6 m LST and ultimately by the 16 m NGST. The crucial elements for this decade, beyond HST's image-compensating instruments and its optimization, are the third-generation instruments for HST and the start of LST, the 6 m successor to HST.

HST.

HST is the premier instrument of NASA's Great Observatory series and will be the cornerstone of the space UV, Optical and Near-infrared program for the 1990s. In performance and overall capability, HST has the potential to surpass all other space and ground facilities operating between 0.12-2 μm in this decade. Only in visible and near-IR spectroscopic capability will it be challenged by the new 8-10 m ground-based telescopes. It is crucial that that potential be realized by new instruments that will restore and extend its imaging and spectroscopic performance. HST's sensitivity and resolution will link future observations at other wavelengths from both ground and space. Its intended lifetime (15 years) will span the entire generation of Great Observatories (HST, GRO, AXAF, and SIRTF).

The scientific returns possible from HST require that we restore its optical performance with new instruments, and maintain and refurbish the Observatory and its supporting systems until its successor has been launched or is in the final development phase (late Phase C/D).

The replacement camera, the WF/PC II, should be completed and implemented as soon as possible. It is also crucial to accelerate the development of the two Second Generation scientific instruments, the NICMOS and the STIS. These should be completed rapidly and deployed as early as possible – by 1995-6. They not only bring very large gains in capability to HST, but will help to restore much of the lost capability.

The breadth of the science program of HST attests to the power of observatories in space. For example, HST is expected to play a key role in finally determining an accurate value for the Hubble constant. The use of QSOs to explore the intergalactic medium in the young universe as well as of the gaseous components of young galaxies will also be greatly enhanced by our ability to explore like environments at current epochs. We will be able to relate the absorption-line systems to objects that can be studied directly from HST and the ground, thereby helping us to understand those objects in the young universe that contribute to the high-redshift absorption-line systems.

The parallel mode surveys that can be made with the cameras on HST have the potential for making major discoveries. While the images will be of great value for elucidating the structure of distant galaxies and for establishing the distribution of stars throughout the Galaxy, they are likely to surprise us since the images will be taken serendipitously.

The operational support will be as challenging and as critical to its scientific productivity as the performance of the major spacecraft elements. NASA and the scientific community have made considerable investments in the Observatory and the science and mission operations systems which will sustain the 15 year scientific mission. It is critical that these efforts be continued, and, in addition, that the M&R (Maintenance and Refurbishment) program maintain and improve the Observatory so that its 15 year mission is productive and fully complementary to its sister Great Observatories.

The goal of these efforts is to maximize the scientific productivity of HST over its 15 year lifetime. This involves providing responsive and efficient spacecraft and science operations, including scientific capabilities and improvements to science efficiency that are not yet available. Near-term examples that are being implemented to meet the original goals of HST are improved support of planetary observations and parallel observational capability with the cameras. By optimizing science planning though improved planning software, a 66% increase in the amount of useful observing time is potentially available. Continued refurbishment of the spacecraft, orbit and ground systems will be essential if the high level of scientific productivity is to be maintained.

A strong data analysis program is required, as is the rapid diffusion of the scientific results. Important elements of this are the development of the HST data archive, and the adequate support of General Observers and Archival Researchers working on analysis of HST data. Every effort should be made to educate and involve the next generation of scientists in HST activities.

Third Generation Instruments for HST.

A third generation of instruments should be planned for an M&R mission later in the decade. These also will play a major role in ensuring the continuing high productivity of HST. While the selection should be made through peer review, examples of likely instruments could be:

An Advanced Camera.   A more sensitive camera with better sampling of the corrected PSF (Point Spread Function) in both the UV and the visible would be a major improvement over that expected for the WF/PC II. Higher performance in the UV would require a large visible-blind array detector with good dynamic range or the development of visible-blocking filters. Higher performance in the visible requires CCDs with efficient coatings over broad wavelength ranges and lower readout and dark noise. Incorporation of a narrow-band imaging capability across a wide field could provide new insights in many areas, for example, AGNs, star forming regions and distant clusters. This might be accomplished with a Fabry-Perot system or an Acousto-Optical Filter.

A high performance Spectrograph.   STIS is an excellent example of the spectroscopic gains that can be made with contemporary technology. Further gains are possible. One potentially very valuable area for improvement is multiobject capability with a programmable multislit system. This would overcome the current restriction to observing one target at a time. The multiobject system could utilize fibers, slits, or more generic technologies that would allow arbitrary-shaped entrance apertures to be defined (based, for example, on liquid-crystal-like technology). Substantial gains in effective throughput would result from such techniques. Spectropolarimetry is also an area that could be valuable to explore. Cosiderable scientific return could result from having linear and circular polarimetric capability, far UV capability, very high spectral resolutions (10^6), and from fully utilizing the high angular resolution of HST in the UV and visible.

LST – the 6 m Successor to HST.

The scientific case for enhanced Observatory-class capability in the UV-IR region is overwhelming. The panel strongly recommends that a 6 m-class telescope be launched in the first decade of the next century.

Substantial gains in capability can be realized at a cost comparable to HST. Building upon the advances in technology and instrumentation, the 6 m LST would offer a substantial performance boost over HST, while not being strongly dependent on as-yet to be developed technology. The improvements in optics technology, the maturing of detectors and instruments, and the technical and managerial experience gained in the nearly two decades since HST was designed, will make LST an affordable project, even though the primary optic is significantly larger. Tremendous gains in sensitivity in the near-IR would accrue from a passively-cooled optical system. The operational complexity and cost can also be substantially reduced by placing the telescope in HEO.

As noted throughout the science discussion in §I, LST can have a substantial impact in almost every area of endeavor, from cosmology to planetary systems. While the testing of the existence of truly Earth-like planets will await 16 m class telescopes and interferometric techniques, very substantial gains can be made with LST in the area of planetary system studies in the Solar neighborhood within ~ 10 pc. It is clear that major issues can be addressed in the areas of the ISM and its evolution with time, in stellar astrophysics, in star forming regions, in studies of activity in galaxies, and in stellar population studies. In this latter area thresholds are passed that allow for the study of populations in nearby galaxies that will establish how such galaxies form and evolve. The constraints placed on the ages of the oldest populations also help determine the age of the universe. Since the Hubble constant will be well in hand by the time LST is launched this also allows constraints to be placed on other fundamental cosmological parameters (e.g., Ω_0). Its image quality and infrared sensitivity combine to give it the power for studying galaxies in their youth, and even during formation, that far exceeds that of HST and any ground-based facilities.

With improvements in optical fabrication techniques, the wide-angle scattered light in the UV and visible from residual small scale surface structures could be substantially decreased below that expected for HST (after correction of the spherical aberration). This would have major implications for the science program. The UV gains would result in improvements in the imaging capability – 10 milliarcsec resolution at 0.25 μm is a reasonable goal for a 6 m. The dynamic range that would result from lower scattering would have immediate advantages for detecting large planets and protostellar disks, as well as for the study of QSOs and AGNs. It would be ideally suited to stellar studies with its sensitivity (10:1 S/N) of 30 mag in 10^4 s in the V band for unresolved sources. Its power for galaxy studies, particularly distant galaxies, can be estimated from Figure 1, where its capabilities would fall between HST and the 16 m NGST.

The use of active optical elements would greatly reduce the difficulty of developing a pointing and tracking system to satisfy the more stringent requirements of this telescope. The structural requirements would be relaxed also by the use of an active optical system. The varying thermal loads of LEO would be eliminated, as would the effects of aerodynamic drag. The complexity of the power system is eased in HEO. Furthermore, body-mounted solar arrays could be used.

Passive cooling could greatly enhance its IR performance. If the telescope could be cooled to the vicinity of 150°K, it would allow background-limited performance to beyond 3 μm where the zodical background is at its minimum (between scattering and emission). Cooling to ~ 100°K would result in outstanding gains in the mid-IR at 10 μm and beyond. Passive cooling to this level would be quite possible in HEO. Its IR sensitivity would be high. If it is cooled to 100°K its 3 μm sensitivity (S/N = 1; 500 s integration) would be ~ 15 nJy, and its 10 μm sensitivity would be 2 μJy, ~ 30X and 3X, respectively, the sensitivity of SIRTF in the near-IR. With its higher resolution (> 6× that of SIRTF) it will build upon the science programs and discoveries of SIRTF.

Difficulties will arise in fabricating and testing large optics for use at those temperature, but an even more challenging goal (< 100°K) is being set for the 16 m, and so technological development needs to occur in this area. The scientific gains are large. The background would be reduced by *six* orders-of-magnitude at 3 microns compared to that typical of ground-based telescopes.

With the developments in detector technology, particularly with CCDs and near-IR detectors, photon-limited wide field imagers and spectroscopic systems that approach theoretical limits appear to be quite feasible. Particular attention will need to be given to the operation of detectors in the higher radiation environment of high earth orbit. This is an open question, but it has been argued that the low read noise and the high charge transfer efficiency of contemporary detectors allow multiple short exposures that minimize the effect of high particle rates. The Second and Third Generation instruments for HST will lead to mature technologies for detectors and instruments that can readily be applied to a smaller complement of versatile instruments for the LST.

The complexity of the ground system for HST is driven largely by the many constraints of LEO operation (which in turn was dictated by the use of the Shuttle for deployment and on-orbit maintenance). A substantial fraction of HST's cost has occured as a result of these constraints and complexities. Operations costs and the costs of maintenance will eventually dominate the total program cost. Not only does operation in LEO incur large cost penalties, but the intrinsic efficiency loss also compromises the science program and the science productivity of the telescope. The cost of operating and maintaining HST will exceed $150M per year, not including the support for data analysis. A high priority goal for future space science missions
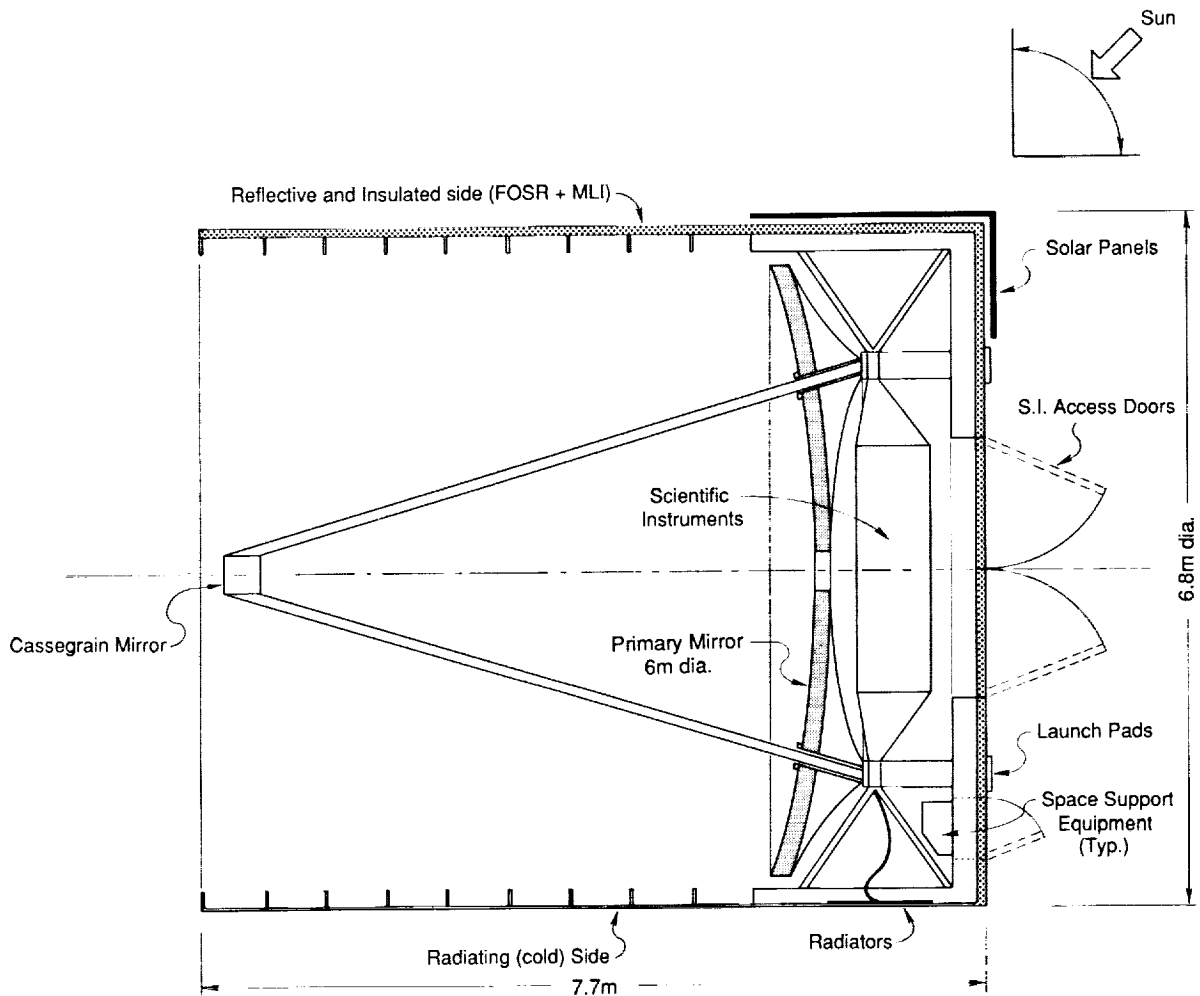
Figure 2. A schematic of the 6 m HEO LST. Apparent are the compact dimensions that result from the use of both a fast primary and the short baffle that is practical in HEO. A preliminary analysis of the weight of such a telescope indicates that it would be *comparable to that of HST.*

should be access to HEO. Operations costs for a HEO LST would be reduced to a small fraction of that above.

The total cost of HST has been quoted as being in the vicinity of $1.5B to $2B. What is usually not realized is that a very substantial fraction of the cost of HST has been incurred by the ground and operational system software and hardware costs, and by the engineering analysis, spare parts inventory and additional management needed for the 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 ~ $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 LST.

Why should the cost of LST not be proportionally larger by the usual scaling laws? There are several very good reasons why the LST as discussed here 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 LST. Another useful guide to cost has been spacecraft weight. A preliminary analysis of the likely weight of an
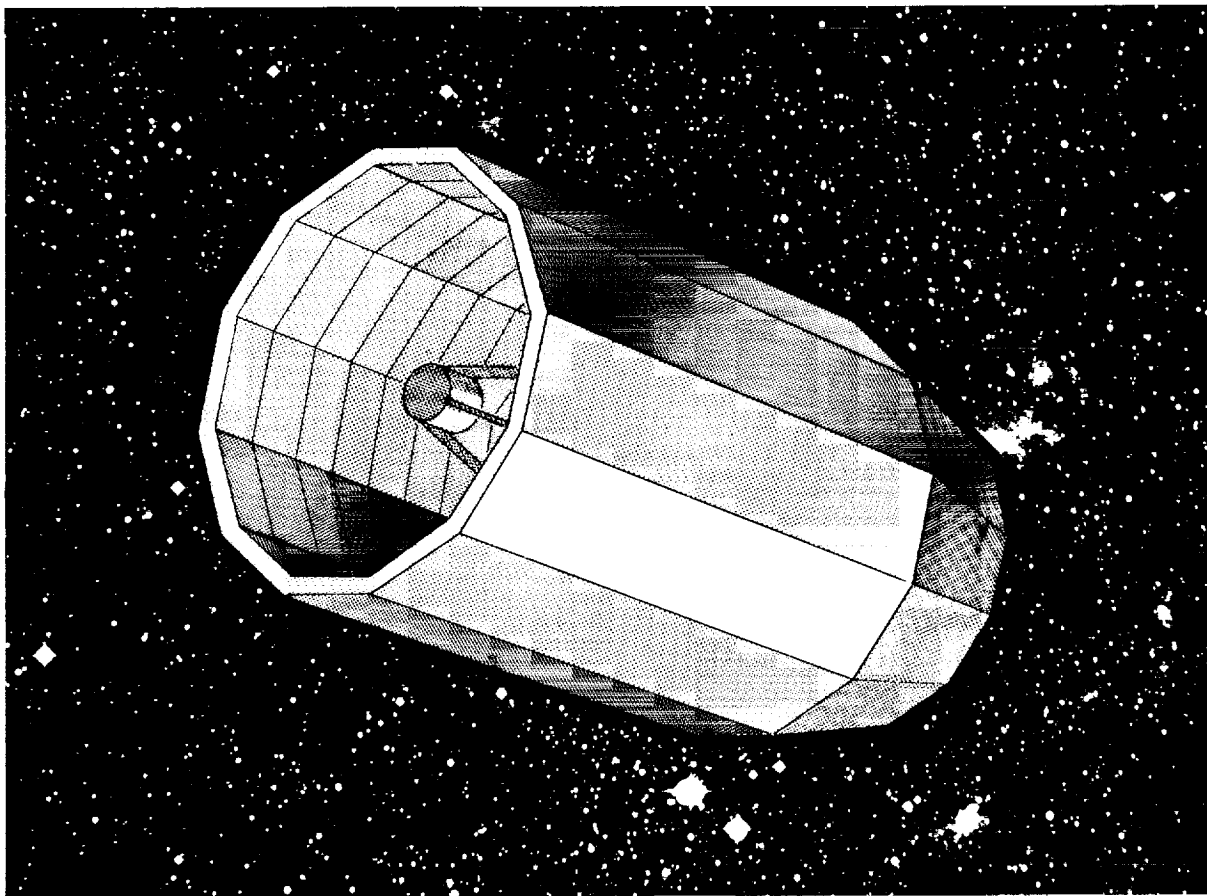
Figure 3.   An artist's conception of the 6 m LST in HEO.

HEO LST, (of the design shown in figures 2 and 3) 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 (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 because of the HEO location. Earth and Sun angles of 90°are 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 will greatly simplify the PCS (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, for example, if active cooling systems do not reach maturity) considerable further savings can accrue.

While we have discussed the LST 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. Cost tradeoff studies need to be undertaken to

establish the most cost-effective and timely route to fruition of the program. Meanwhile the LST will be discussed as being a single UV-Visible-IR telescope.

The surfacing of a major optical problem with HST has emphasized again that the successful completion of a project of this scale is more than just the application of sophisticated technology. The optics problem is but the most obvious of many such examples throughout the HST program. 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 occurences. The complexity of such projects brings with it a challenge for the oversight, review and testing that is comparable in difficulty to the technology developments. This challenge must be faced squarely, and must be given the same level of attention as that required for the technological developments. The lesson to be learnt from HST (and from the success of even larger high energy physics projects and others) is that the management must be done by experienced people with deep technical backgrounds and a long-term commitment to the program. 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 LST.

The current state of launch vehicles is in flux. The envelope and weight requirements for such a spacecraft could easily be met by the larger members of the US ALS/HLV (Advanced Launch System/Heavy Lift Vehicle) ELV (Expendable Launch Vehicle) program. Shuttle-C also could be configured to accommmodate such an envelope. Whether it will is unknown. The Soviet Energia could easily launch such a payload to HEO. The options for an appropriate upper stage for orbit circularization need to be investigated further. Further orbit tradeoff studies along the lines of those done for SIRTF need to be carried out.

The essential and central role of an UV-Visible-IR observatory in astrophysics makes this a natural program for an 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.

Next-Generation 16 m Telescope.

Some of the most demanding scientific goals are beyond the power of even a 6 m space telescope. For a range of questions of fundamental significance it is clear that a telescope as large as 16 m is required. The NGST, Next Generation Space Telescope, would be a passively-cooled, diffraction-limited, telescope instrumented for the UV-Visible to the mid-IR. It would have unprecedented power for tackling a wide range of the most fundamental astrophysical problems, from the detection and spectroscopic observation of Earth-like planets to the structure of (forming?) galaxies at redshifts z approaching those of the highest redshift QSOs.

The investment in such a telescope would require it to be a long-lived facility. As such it would naturally form a part of the science program for the Lunar Outpost. In addition, very real advantages accrue for such a telescope from the use of the Moon as a base. The stability and isolation from noise sources afforded by the moon could be crucial if its ultimate performance is to be achieved. It could also be combined with the proposed very large interferometer. The combination of the NGST and the km scale interferometer is one that pays substantial scientific dividends. The NGST would enhance the sensitivity of the interferometer with its large collecting area. The ability to provide data for complex strucutres on scales of 5 milliarcsec to arcminutes also enhances the utility of the interferometer. It then provides a natural overlap in resolution with data from ground-based telescopes and from space telescopes in other wavebands.

While initial studies may focus on the development of such a telescope as part of the Lunar Outpost program, these studies should be generic in nature. If the Lunar Outpost program fails to develop an attached, large, long-term scientific capability, the developments can be utilized for space missions.

The telescope would have a lightweight, segmented primary with active wavefront sensing and control for diffraction-limited performance, into the UV if technically feasible. The optics are particularly challenging because of the objective of detecting and studying Earth-like planets. This will require low-scattering optics for high dynamic range. As for the 6 m, the structure and optics would be passively-cooled but now to less than $100°K$ to maximize the contrast for the planet detection and measurement program. Per resolution element the background would be 21 mags *less than that on the ground* to beyond 10 μm, i.e., $\sim 10^{-8}$ that for ground-based observations of unresolved sources.

With a collecting area that is 44 times that of HST, the sensitivity for unresolved sources is remarkable. The visible background is $\sim$ 32 mag per resolution element, i.e., $\sim 10^{-4}$ that for ground-based observations. In the V-band, unresolved objects could be measured with 10:1 S/N in 10^4 s to 32.5 mag! The sun could be detected with 10:1 S/N at 3.5 Mpc, and a 10-day cepheid at 200 Mpc. A $z = 2$, $M_V = -21.5$ *normal* galaxy containing an 18th mag QSO could be detected at 5:1 S/N, all for 10^4 s integrations. The simulations in Figure 1 graphically demonstrate the power of this telescope. Its 3 μm sensitivity (S/N = 1; 500 s integration) would be $\sim$ 3 nJy, while at 10 μm it would $<$ 0.5 μJy.

With reasonable projections from current detector technology, mosaics of detectors could be used in wide-field cameras to give fields of several arcmin in size with diffraction-limited images. Resolutions would range from less than 3 milliarcsec in the UV to 50 milliarcsec in the zodical background "window" at 3-4 μm. High performance spectrographs would complement the imagers. As in HST, multiplexed operation of the instruments would allow for surveys of unprecedented sensitivity.

IV. MODERATE AND SMALL MISSIONS.

The Observatory-class missions cannot stand alone. Many scientific programs call for specific capabilities that cannot be accommodated in the large versatile telescopes. Examples such as EUVE and Lyman-FUSE abound. Frequent access to space for high scientific merit programs that can address a specific problem will remain central to the goals of the UV-Visible-IR space astronomy program. Specific examples follow.

Delta-class Explorers.

Missions of the Delta-class have played a crucial role in many areas of astrophysics. They will continue to do so. The panel strongly recommends that the Delta-class Explorer program be enhanced so as to allow more frequent missions with shorter development times.

The following §V, which deals with the ongoing program, discusses some excellent examples of current Explorer missions (EUVE and Lyman-FUSE).

Examples of possible future Delta-class Explorers clearly show that the scientific payback is very large from an enhanced Explorer program. While the panel believes that the following examples would all provide excellent and much needed scientific returns, they also feel that the most appropriate approach to the selection of Explorer missions, both Delta-class and smaller, is through peer review. The examples of forefront scientific missions (in no priority order) are an all-sky UV Survey that matches the resolution of the current ground-based Schmidt surveys, a wide-field astrometric system with sub-milliarcsecond accuracy on small and large scales, a multi-waveband system with the ability for simultaneous observations from the X-ray through the UV, and a very high spectral resolution instrument for ISM studies.

The lack of an all-sky survey in the UV is striking. While EUVE will map the region below 900 Å, the 1000 Å to 3000 Å range remains unmapped. Ideally the $\sim$ 1 arcsec resolution all-sky Schmidt surveys now available in the visible from the ground should be matched. A 1 m-class wide-field instrument would reach hot stars as faint as $m_V = 26$, and would provide a vast database of scientific value in its own right, as well as providing sources for further study with HST and LST. It would provide a quantitative database of UV point and diffuse objects that will enable statistical studies of hot stars, UV-bright extragalactic sources and diffuse sources throughout our own and other galaxies. Such a quantitative survey also needs to be undertaken from the ground in the visible with CCD detector mosaics. The resulting large color baseline (UV-V) would have particular value for stellar population studies. While the demands on detector and data handling technologies are large and may have limited the feasibility of such a program in the past, the rapid maturation of such technologies has made such a program much more realistic.

A similar survey should also be carried out at 2 μm. Future emphasis on the IR through SIRTF, LST and the NGST makes this an important goal. The first such survey should be carried out from the ground. However, the reduction in sky brightness in space by three orders of magnitude from that on the ground would result in very substantial gains in the depth of such a survey. Continuing development of IR array detectors will make a second space-based survey a practical proposition in the future.

Accurate reference frames, distances and space velocities are fundamental. Beyond a certain threshold the opportunities for very significant and even fundamental gains in knowledge are available. An astrometric system that can determine positions of stellar objects with submilliarcsec accuracy is an example. This would

ideally be usable both for establishing a wide-field reference frame and for small scale mapping. It would have substantial impact on our confidence in the distance scale, age dating and evolutionary results drawn from stellar population studies and on the structure of the Galaxy. It would be a very valuable precursor to the more ambitious and more costly interferometric systems which can lead us into the microarcsecond range.

Many astronomical objects emit simultaneously across a wide range of wavelengths. For objects in which such activity occurs on timescales of seconds, minutes and even hours, concurrent observations are difficult to obtain because of the problems of scheduling multiple instruments. An instrument which allows simultaneous spectroscopic observations from the X-ray to the UV, and possibly into the Visible would play a particularly valuable role in understanding time-variable phenomena in a variety of astrophysical systems. The scientific objectives would include understanding the physical processes occuring in accretion disks, such as boundary layer phenomena, and the conversion of magnetic energy to relativistic particles and heat, the characteristics of the matter and radiation environment around highly condensed stars, the physics of the activity on stellar surfaces and within their coronae, and, on longer timescales, the dynamics and physics of the activity in active galactic nuclei. Simultaneous observations by co-aligned modest-sized instruments on the same satellite in HEO will permit uninterrupted long-duration observations of the intrinsic variability of plasma at 10^4 °K to 10^8 °K in these sources. This is not feasible with separate satellites in low Earth orbit. Furthermore, a quick response could be made for targets of opportunity such as novae, supernovae, and outbursts in cataclysmic variables and X-ray binaries.

Another area which could benefit from an Explorer-class mission is a spectroscopic study of the Interstellar Medium at very high resolving powers. At the velocity resolution needed, less than 1 km s^{-1}, the spectral resolutions required are of the order 10^6. Even though bright stars can be used for such observations, any substantial mapping requires many sources and considerable time to carry out the program. This area has been the focus of numerous rocket experiments, but an orbital mission would clearly result in substantial gains in the number of lines of sight and features that could be studied.

The advantages of orbits beyond LEO for Observatory-class misions also apply to these smaller missions. The returns from IUE attest to the value of HEO. The scientific return from the HEO missions is sufficiently high that as the plans for future launch capability are developed every effort should be made to include the means for ready access to HEO for Explorer-class vehicles. This is particulary prudent since the probability of damage in LEO from collisions with space debris is increasing significantly and is projected to approach unity for long-lived missions.

Another issue is the currently planned use of a single Explorer platform for three Explorers, namely EUVE, XTE and Lyman-FUSE. An expanded Delta-class Explorer program would provide the funds to allow the acquisition of a dedicated spacecraft for Lyman-FUSE. This is an attractive option given the potential scientific productivity of Lyman-FUSE and the risks associated with two manned Shuttle missions to service the platform (see also the Lyman-FUSE discussion in §V).

The primary goal of the moderate-sized program is to improve access to space for the many high-priority scientific objectives. Enhancement in this area would be directed towards:

(1) Improving the frequency of missions;
(2) Reducing the time to implement the mission;
(3) Giving the PI more direct oversight and responsibility for the mission, with the goal of greater training and reducing time and cost.

The scientific case for more frequent Delta-class Explorer missions is very strong. While augmentation of the program is primarily focused towards a higher mission frequency, two other elements, (2) and (3) above will play a crucial role in containing costs and improving the extremely high educational and technical training value of such missions.

Reducing the time an Explorer mission spends in the queue preparing for launch is very important. The timely response to new scientific initiatives through the exploitation of contemporary technology is a defining goal of the Explorer program. The rationale for the program is undercut when high-priority missions strech out well beyond a decade, as Lyman-FUSE has done. To lessen the lag before missions can be carried out will require both an increase in resources for the program as a whole and changes in program and budget planning. Ideally, the length of an Explorer Project should be determined only by the technical development schedule, but in practice the time to implement a project is determined by programmatic and budgetary considerations. As a consequence, the lifetime of a project is often much longer than it needs

to be. The Explorer Program should be examined to find planning and management techniques which will reduce the average time in the queue. This will probably mean fewer missions in the queue at any moment, but more rapid motion through the queue. As part of this change, projects presently in the queue should be accelerated in order to reduce the lag time. Acceleration will require more overall resources. However, it will also require good planning and finding the best way to concentrate resources on a limited set of missions, although for a shorter time.

Acceleration of the Explorer Program has several advantages. Shorter implementation times lead to more rapid scientific progress. In addition, the cost of a project will be reduced for two reasons: first, there is always a core group of critical personnel which must be supported despite programmatic changes and delays; and second, long implementation times increase the probability of changes (e.g., in launch vehicle or mission operations) which increase the total cost of the mission. More rapid progress will also encourage the most talented and creative instrumentalists to participate. Indeed, their participation is necessary to produce the highest quality instrumentation for this program. Thus, a different approach to the programmatic and project management will not only speed the development of the science, it will produce better missions and reduce costs per mission.

The third element of the enhanced Explorer program is the much deeper involvement of the PI and his or her team. With such involvement, these programs can educate those involved and develop the experience base for the management and oversight of technically complex missions. The PI team should have greater autonomy and decision-making authority in Explorer-class programs. This also implies that the implementation plan will play a significant role in the selection process. High scientific merit and technological readiness will need to be supplemented by a thorough and convincing implementation plan. The deep involvement in these programs of the science community at universities, labs and centers is crucial for the long-term vitality of the field. Explorers provide a means of training and of developing the technical and management skills that are needed if the UV-Visible-IR space science community is to continue as a vital element of the space science program.

Imaging Astrometric Interferometer.

Long-baseline interferometry would have a revolutionary impact on the study of compact astronomical objects such as AGNs and interacting binaries. A first-generation orbiting interferometer would provide a major scientific step in imaging power and provide an essential demonstration of the demanding technology required for the further development of such systems. An early phase of technology development and demonstration is required that would naturally lead into a deployable 20-50 m baseline instrument late in the decade. In particular, this would entail demonstrations of the technologies for large stable structures.

Long baseline synthetic aperture UV-Visible-IR telescopes offer the potential of many orders of magnitude improvement in two areas, angular resolution and astrometric accuracy. In order to realize significant gains over existing and planned space-based observatories, the baseline of the interferometer should be a factor of 5-20 beyond the 2.4 m aperture of HST. A traditional problem of large optics is maintaining submicron tolerances for structures many meters in size. Current space interferometer proposals advocate the use of a laser optical truss as the primary tool, to control the dimensions of the optics. The accuracy of the optical components in the laser system is the key to the astrometric application of interferometry.

Technology for space interferometry is being developed and should be accelerated so that a moderate mission can be started in the latter half of this decade. Further technology development in very large structures (hundreds of meters), hyperprecision (sub-Å) metrology, large area optics, and low temperature optics offer additional orders-of-magnitude improvements in sensitivity, angular resolution, and astrometric precision. A moderate interferometer mission would be the first step in a sequence of space observatories that would provide order-of-magnitude increases in performance in one or more directions in parameter space.

An example of one concept that would have high scientific payoff is a moderate-sized UV-optical interferometer with three collinear baselines, each of order 10-20 m, with 30-50 cm apertures. Two of the baselines lock onto reference stars to stabilize the platform allowing long coherent integration times on the third baseline. Laser metrology would be used to monitor instrumental effects. Astrometric accuracy would be 10 μas for narrow fields and 100 μas for wide fields. Such accuracies would allow for planetary searches, determination of the cosmic distance scale from parallax measurements on Cepheids, and linkage of the

optical and radio reference frames. In its high dynamic range imaging mode (10^3:1) it would allow study of such objects as AGNs, QSOs, extended atmospheres, interacting binaries, and gravitational lens objects fainter than 20 mag at resolutions less than 4 milliarcsec at 4000 Å.

The science goals include imaging of the narrow line region in AGNs and QSOs and measurements of the dust tori, searches for gravitationally lensed objects, and imaging of unique objects such as SS433. It would have great value for stellar astrophysics for the study of interacting binary systems, for the measurement of stellar diameters for most bright stars, and for the measurement of limb darkening in supergiants. Its resolution would also allow continuing studies of planetary and solar system objects (e.g., asteroids) at scientifically-interesting resolutions.

The astrometric potential is also very exciting. Astrometric searches for extrasolar planets and dark companions would benefit from the dramatically improved resolution. The distance scale and its many error-introducing steps could be placed on a much firmer footing by precise parallax measurements to Cepheids and other objects, and by allowing the distances to Local Group galaxies to be determined through a combination of kinematic and proper motion observations. Knowledge of the space motions of the constituents of the Galaxy will also have important implications for the formation and evolution of spiral galaxies.

Major scientific returns will result from the high spatial resolution that could be obtained only through interferometry. However, the development and operation of interferometric instruments will be a very challenging one. This is particularly true of the very large baseline, multi-element systems required for elucidating the very complex structures that form a major part of the scientific objectives of interferometry (e.g., AGNs). A realistic program of technology development and demonstration, initially on the ground and then in space is essential. While likely to be substantially larger in cost than traditional Explorers, an interferometric system such as that discussed here should be considered as the technological hurdles are overcome. *The panel strongly supports a program of technological development leading to a space-based imaging astrometric interferometer.*

Small Explorers.

Several disparate but important programs comprise the SMALL category of our recommended program. While small, they have an impact far beyond their size and so deserve strong support. They are: a UV all-sky survey; optics development and demonstration; and enhanced support for related ground-based observations.

SMEX UV Survey.

Most of the emphasis in this wavelength domain has been placed on the larger Delta-class Explorers. This reflects the relative maturity of observational capability in this waveband. However, there are a few areas where the smaller Scout-class Explorers (SMEX), or a somewhat more capable Pegasus-class Explorer, could yield very interesting scientific returns. One such program would be a UV low spatial resolution all-sky imaging survey. This appears to be scientifically very meritorious and is well-matched to Pegasus or Scout capabilities.

While there is a clear need for a 1″ resolution point-source survey that matches the multicolor Schmidt surveys available from the ground, a survey at low spatial resolution that is particularly sensitive to low surface brightness diffuse emission would pay substantial scientific dividends.

An Ultraviolet Background SMEX whose goal is a low spatial resolution survey primarily for low surface brightness diffuse emission would return substantial science at a very reasonable cost. This could be a small free-flying Scout-launched mission in an equatorial orbit, dedicated to an all-sky imaging survey at modest resolution (e.g., 15 arcsec). It would result in a large database of faint ultraviolet point sources, and would also survey the diffuse ultraviolet background over the entire sky. The inclusion of spectroscopic capability with modest resolving power ($\Delta\lambda \sim 5$ Å) would also be desirable.

Such a program would make important observations of hot interstellar gas, interstellar dust, fluorescence radiation from interstellar molecular hydrogen, intergalactic ultraviolet radiation, hot stellar populations in our own and external galaxies and QSOs; it would also provide a cornucopia of targets for the Great Observatories.

Optics Development and Demonstration.

Central to all instruments in this wavelength range are high performance optics. As the field progresses, the scientific goals will demand not only larger but also smoother and more complex optics. A technology development and demonstration program for lightweight optics with high-performance surfaces would benefit missions of all scales in the UV-IR region.

A quantitative, deterministic approach to optical figuring has been the goal of optical development programs for decades. Only recently have major developments taken place that indicate that such a goal is within reach. Two developments indicate that the ability to polish extremely smooth, strongly aspheric surfaces could be at hand. The techniques are the stressed-lap approach and ion polishing. Both appear to offer the possibility that surfaces such those needed for future generations of UV-Visible-IR telescopes could be obtained within a reasonable amount of time and for a reasonable budget.

While such deterministic polishing methods are important for the production of the required optical elements, many other steps are also needed. In particular, additional challenges are lightweighting of the elements, generation of surfaces, support of the surfaces, active compensation (in some cases), and fabrication and testing of optics to be used in passively-cooled systems. However, the polishing process and the attendent testing is a key element, and if the new approaches are proven they will greatly alleviate the overall difficulty of the task. For example, ion polishing offers a way to deal with the very difficult problem of "print-through" of the web-structure in lightweighted optics.

The nearly 20 years since the HST design was set in place sees us on the verge of major advances in optics technology. A program that focuses on the demonstration of these techniques with realistic optics will affect missions on all scales throughout this spectral region. *Optics development and demonstrations should be at the forefront of a technology program early in this decade.*

Supporting Ground-based Capabilities.

Ground-based observations have played a major role in bringing the science programs of space missions to fruition. The need to place such observations into the context of the development of nearly a century of astronomical understanding usually requires observations in the visible region. In particular, the need for spectroscopic observations and optical "identification" have driven great demand for ground-based facilities. As space observatories become more sophisticated and cover progressively larger ranges of the spectrum this latter demand will probably decrease. However, the need for spectroscopic follow-up will remain far into the future. Even with very large telescopes in space the cost-effectiveness of large ground-based facilities for time-consuming spectroscopic observations will require that such telescopes be built and supported. In general the rule that "anything that can be done from the ground should be done from the ground" is a wise and cost-effective one.

The results from an increased Explorer program, HST, and the other Great Observatories will place substantial pressure on ground-based facilities. This will be particularly the case for spectroscopic observations of faint sources. All-sky access to large ground-based telescopes for spectroscopic observations will be crucial if the desired scientific returns are to be made. Telescopes of the 4+ m-class will be under the most pressure. Additional support for the construction and instrumentation of such telescopes should be seriously considered as part of the overall space science program. In addition, support of the related use of such facilities should be available through funding of the analysis of space science data. The goal is clearly to maximise the scientific return and all the necessary steps need to be supported in a unified way.

Instrumental development and demonstration is another area where ground-based programs are valuable and cost-effective. Much can be learnt and many problems solved before the start of the very difficult, time-consuming and expensive process of fabricating instruments for space missions.

From a scientific viewpoint the need, effectiveness and value of a balanced and synergistic program of ground and space missions is obvious. Every effort should be made to ensure that such an approach is taken. The costs are minimal compared to the space missions and the scientific returns are large.

V. PREREQUISITES – THE CURRENT SCIENCE PROGRAM.

The ongoing program with IUE, ASTRO, EUVE, Lyman-FUSE, STIS, NICMOS, and HST operational

improvements is a strong one. It must be well-supported to ensure that the scientific returns are made. The key elements are Lyman-FUSE, HST Optimization, and the near-term missions and their support.

Lyman-FUSE.

The Lyman Far Ultraviolet Spectroscopic Explorer mission will use high resolution spectroscopy below the 1200 Å HST limit to measure for the first time faint sources both throughout the Galaxy and at very large extragalactic distances. This important spectral window is virtually unexplored except for the Copernicus mission, which was limited to bright nearby stars.

The primary goal of the Lyman-FUSE mission is to obtain high resolution spectra ($\lambda/\Delta\lambda \sim 30000$) with unprecedented sensitivity (10^5 times that achieved by Copernicus almost 20 years ago) in the critical spectral window from 1200 Å down to 912 Å, the wavelength of the photoionization continuum of atomic hydrogen. Recent advances in optical and detector technology, which make the primary goal possible within a modest experimental package, also enable the spectral coverage to be extended down to 100 Å with a sensitivity much greater than that of EUVE. This can be done with only a minimal increase in complexity. The combined wavelength range thus bridges the gap between that covered by HST and AXAF at moderate cost.

The spectral window opened by this mission will provide a unique access to many critically important species for astrophysics. Interstellar and intergalactic deuterium, a crucial fossil nucleus formed in the early hot universe, can best be studied in the primary bandpass of this mission. In addition, Lyman-FUSE will obtain the direct measurements of molecular hydrogen, the primary constituent of cold interstellar clouds - the birthplace of stars and planets. For objects as diverse as planetary ionospheres, the interstellar medium, and QSOs, the unique diagnostics in the Lyman-FUSE spectral range will allow measurements of gas and plasma at temperatures over a wide range, from a few tens of degrees to greater than ten million degrees.

This mission will achieve major advances in a wide range of important scientific problems, because transitions in this spectral region include most of the important interstellar and intergalactic absorption lines. Furthermore, this region includes emissions from gas and plasma over an extremely wide range of temperatures. The most pressing scientific issues are concerned with the physical processes in three broad areas of astrophysics. They are (i) the physical processes in the early Universe, in essence the measurement of deuterium in a variety of environments, (ii) the physical processes that control the origin and evolution of galaxies, namely the examination of ISM and stellar processes that influence it, and (iii) the physical processes that control the origin and evolution of stars and solar systems, in particular study of the cold clouds in the interstellar medium, and of the dynamics of the formation process itself.

The utilization of a dedicated spacecraft for Lyman-FUSE instead of the Explorer Platform should be evaluated. EUVE will be launched on top of an Explorer Platform spacecraft by a Delta vehicle. The follow-on mission, XTE, will replace EUVE on the Explorer Platform using a Shuttle to exchange the two. The present plan for Lyman-FUSE calls for it to follow XTE as the final mission on the Explorer Platform. Because the Explorer Platform will be complete and the XTE will be in intensive development, this is a good time for reevaluation of the true costs and programmatic risks associated with this novel approach, including the use of the Shuttle. New technological approaches presently under development may make dedicated spacecraft, tailored to a particular experiment, more attractive. It is possible that the integration costs and costs associated with using the Shuttle may balance the cost of a spacecraft both for Lyman-FUSE and for future missions. In addition to reduced programmatic and budgetary risks, there is a potential for significant improvement in the scientific return by optimization of the spacecraft and orbit as well as by a longer-lived mission.

HST Optimization: Operations, WF/PC II, STIS and NICMOS.

Recovery of HST's Imaging Capability.

The highest priority must be given to recapturing HST's imaging performance. While the effect of the degraded images on the performance of the cameras is obvious, virtually all of HST's instrumental capabilities are affected by the poor images; for example, the spectroscopic throughput and the ability to distinguish between sources in crowded fields are compromised, as is the spectral resolution by the use

of large apertures. The replacement camera, the WF/PC II, should be completed and deployed as soon as possible. The modified Second Generation instruments should also be funded at a high level to allow completion and deployment as soon as is practical. The spectroscopic performance of STIS is sorely needed.

HST Operations.

The complexity of operation in LEO has resulted in HST being launched with an operational system that does not yet reach the theoretical limits for on-target efficiency. Given the high cost of HST and its limited life every effort should be made to optimize the operational systems of HST. This includes, for example, bringing into operation as soon as is possible the means to observe moving targets and the means to operate and obtain data from two instruments simultaneously ("parallel" mode). These will result in substantial improvements in the science return from HST by allowing observations of planets, their companions and other bodies in the solar system, as well as parallel surveys with the camera systems. This latter capability is likely to result in major discoveries with HST, particularly with the new cameras. Continuing support for development and refurbishment of the ground system will similarly prove valuable as experience grows with the operational systems.

The combination of the implemention of the planetary and parallel modes, higher operational efficiency, the image-correcting camera WF/PC II, and the Second Generation instruments, NICMOS and STIS, will lead to very substantial gains in scientific productivity – exceeding orders-of-magnitude in some areas.

STIS.

The Space Telescope Imaging Spectrograph (STIS) is a high resolution, high sensitivity spectrograph that will incorporate several advances over first generation Space Telescope instruments. Its availability on HST will result in substantial gains in productivity. Use of large format two-dimensional detectors will allow large multiplex advantages over the current spectrographs on HST. Both photon-counting and CCD technologies will be used in the spectrometer, so the instrument will be sensitive in the ultraviolet, visible, and near-infrared wavelength regions, covering 1050-11000 Å in 4 bands. Spectral resolving powers between 80 and 140,000 will be available. Camera modes will also be available in all 4 bands. High angular resolution will be obtained by sampling the HST diffraction-limited image with 25 milliarcsec pixels, and by compensating for off-axis aberrations.

Spectrographs of the capability of STIS will play a major role in a very wide range of scientific programs. It is expected to have particular significance for studies of the stellar and gaseous kinematics of galactic nuclei, both active and "normal". Such data will play an important role in understanding the physical properties of such nuclei and in establishing the density distributions at scales unobtainable from the ground. Answering the long-standing question of the existence of black holes in such nuclei is at the forefront of the goals for this instrument.

The structure of the ISM and the nature of young stellar objects and their associated jets and disks are also problems which are well-matched by the long-slit and UV-Visible capabilities of this instrument. The interactions of jets and winds in forming stars, the characteristics of protoplanetary disks, the nature of flares in stars and mass loss from hot stars are all areas of stellar astrophysics that HST plus STIS will tackle.

NICMOS.

The Near Infrared Camera and Multi-Objective Spectrometer (NICMOS) will add a near infrared imaging and spectroscopic capability to HST. This Second Generation Instrument will extend the wavelength range accessible by HST into the near-infrared where the background is reduced by three orders of magnitude from that on the ground. NICMOS contains three cameras in the 1-2.5 μm spectral range and three spectrometers covering the 1-3.0 μm spectrum. The spectrographs have resolving powers that range from 10^2 to 10^4. Its cameras will exploit the diffraction-limited imaging performance of HST with pixel sizes of 43 and 64 milliarcsec, while also having the option for a larger field of 51 arcsec in its PSF matching mode (0.2 arcsec pixels). A wide range of filters and polarizers will be available. NICMOS has a 5.5 year lifetime that is set by its cryogenic capacity.

The scientific goals of the system range widely. It is a versatile powerful instrument that opens up a new waveband at a cost much lower than a dedicated spacecraft. Its key scientific objectives encompass

deep surveys, distances to and beyond local supercluster galaxies, the study of dust enshrouded regions of galaxies, star forming regions, and studies of valuable spectroscopic diagnostics in our solar system. The huge reduction in background over ground-based telescopes makes it a powerful tool for studies of highly redshifted galaxies. Use of NICMOS in the parallel mode for surveys can be expected to lead to the unexpected. It can extend distance measures using the brightest red supergiants to as far as the Coma cluster, while also playing a valuable role for other distance indicators where interstellar absorption is a problem. It has great value for studying star forming regions, nuclei of active and starburst galaxies, and other regions where dust absorption severely limits the data that can be obtained in the visible.

The performance gains for HST are of such magnitude that NICMOS and STIS should be accelerated and implemented as soon after the WF/PC II as is feasible.

The Near-Term Science Program.

A wide range of very exciting and powerful new missions are in development or have been proposed in this report. However, these programs must build upon the base of previous scientific programs and ongoing missions. A healthy science program requires continuing access to space missions and support of the ongoing scientific activities. Thus the missions such as IUE, ASTRO, EUVE and a variety of small programs such as the rocket program provide crucial near-term scientific returns, as well as addressing scientific goals that are impractical or technically impossible with the larger and rarer missions.

EUVE.

The EUV region of the spectrum remains largely unexplored. The Extreme Ultraviolet Explorer (EUVE) is dedicated to an investigation of the EUV band from 80-900 Å. The complement of instruments include three sky mapping telescopes, one deep survey telescope and three spectrometers. The three sky mapping telescopes will make complete maps of the sky in four distinct spectral bands during the first 6 months of the mission. EUV sources will be catalogued and their positions determined to accuracies of a few arc minutes. The deep survey telescopes will map a strip along the ecliptic where the sky background is very low, thus allowing higher sensitivities to be achieved. The deep survey will be carried out in two spectral bandpasses spanning 80-400 Å. EUVE will obtain the first full sky maps covering the whole EUV band.

EUVE also includes three novel spectrometers which will allow spectroscopic observations of the brightest EUV sources from 80-700 Å. The resolution of the spectrometers will typically be $\lambda/\Delta\lambda \sim 300$. The EUVE spectrometers will be operated through a NASA Guest Observer program. These spectrometers will allow the detailed line spectrum of coronal emission sources to be obtained. Similar coronal studies to those carried out on the sun can be extended to nearby stars. Observations of continuum sources, such as hot white dwarfs, will allow investigation of the photospheres of these objects as well as of the intervening interstellar medium. The EUVE spectrometers will allow measurements of the HeI and HeII edges at 504 Å and 228 Å.

The EUVE mission will be a valuable precursor to Lyman-FUSE, and a valuable database for UV programs on HST and subsequent missions.

IUE.

The International Ultraviolet Explorer Satellite ranks among the most productive Explorer missions, and has introduced a broad community of astronomers to ultraviolet spectroscopy during its 13-year lifetime. IUE has a 45-cm diameter f/15 Cassegrain telescope, with two echelle spectrographs with spectral resolutions of $\lambda/\Delta\lambda \sim 10,000$ and $\lambda/\Delta\lambda \sim 200-500$, and SEC Vidicon Cameras with CsI and CsTe photocathodes covering the spectral region between 1150 and 3200 Å. Fundamental discoveries have been made in nearly every area of observational research, including planetary astronomy, stellar evolution, atmospheres and chromospheres, the physics of the interstellar medium, stellar populations and galaxy evolution, active galactic nuclei and the intergalactic medium.

Barring significant degradation of current capabilities, IUE will continue to provide a valuable complement to the scientific return from HST for a relatively modest annual operating cost. The unique advantages of IUE relate particularly to variable phenomena: broad simultaneous wavelength coverage in

its high resolution mode as opposed to the very limited range of a single HST high resolution spectrograph observation; much longer periods of uninterrupted on-target time because of its HEO location; access to targets of opportunity within an hour of notification (critical for the early phases of supernova development); scheduling flexibility that allows for simultaneous observations with other spacecraft; and opportunity for long-term synoptic monitoring not readily available to HST proposers. As ground-based observatories maintain telescopes of several different apertures to support a range of qualitatively different scientific objectives, so it is appropriate to continue an active observational program with IUE during the HST era.

ASTRO.

Four instruments make up the Astro Observatory which is flown as a Shuttle-borne sortie mission. It is the first observatory that can simultaneously take ultraviolet pictures of objects, study their ultraviolet and X-ray spectra, and determine their brightness and structure through UV photometry and polarimetry. Using the X-ray and UV spectrographs together provides spectral coverage from 1-3200 Å simultaneously with millisecond timing, a capability that is unique to the Astro Observatory. It should thus demonstrate the scientific value of such a multi-wavelength mission.

The Hopkins Ultraviolet Telescope examines the ultraviolet spectrum from 400-1800 Å. This is the only spectroscopic coverage we will have of the astrophysically important 912-1200 Å region until Lyman-FUSE is launched in the mid-1990s. It is below the HST cutoff. The Ultraviolet Imaging Telescope takes detailed ultraviolet (1200-3200 Å) images with a circular 40 arcmin field of view and a resolution of 2″. Very little imaging information is available in this spectral region. The Wisconsin Ultraviolet Photo-Polarimeter Experiment will use the polarization of ultraviolet light to measure the strength of magnetic fields, the geometry of stars, and the nature of the material between stars. The Broad Band X-Ray Telescope will make the first high-quality, high-energy (0.3 - 12 keV) spectra of many X-ray sources and will be able to measure the important 6.7 keV iron lines.

The Astro mission is expected to make about 250 pointings, yielding more than a thousand measurements on astronomical sources. Astro was conceived during an era when space science was focused on Shuttle missions, and it was expected to have a multi-mission lifetime. The move away from man-rated limited-life missions is a welcome and long-overdue one. While Astro missions are costly, comparable capability is unlikely for many years, possibly beyond this decade, and so a second mission should be supported if justified by the scientific returns from the first mission.

Small and Sub-orbital missions.

In a well-balanced space astronomy program, there is a clear need to support the use of small, innovative and sometimes very specialized instruments. Inexpensive experiments operating on high-flying aircraft, balloons and sounding rockets are important for developing and testing new technologies and performing unique observations that may be unsuitable for the larger, more generalized facilities, such as the Delta-class Explorers and Observatories that serve a broad community of observers. A key operating advantage for suborbital research is that experiments and their support systems may be simple, easily modified, and can assume risks that are unacceptable for the more major missions. Moreover, the time scale from concept to flight is usually only a few years, which is ideal for training graduate students and post-doctoral fellows who will become the newest generation of investigators with a proficiency in designing, building and flying space hardware.

Sometimes, experiments developed for suborbital missions open a new field of observing and, when there is a potential for further development, they serve as an economical proof-of-concept for later, more mature facilities. Likewise, for an instrument which has operated successfully on a number of sounding rocket missions (usually lasting only about 5 minutes apiece), it may be appropriate to have it adapted for flight on an orbital mission of moderate duration, so that it can achieve a worthwhile incremental gain over its past accomplishments. While such a payload could function either as an attached payload on a Shuttle flight (as are some planned small UV payloads), or preferably, on a free flying, very simple spacecraft deployed and retrieved by the Shuttle in orbit, alternative approaches are desirable. Small experiment programs should not be constrained and burdened by the strict scheduling and costly safety requirements of the Shuttle. Inexpensive rockets which can achieve orbit, such as the Pegasus rocket, should be developed as suitable delivery systems.

From a general perspective, suborbital programs are ideal for establishing and supporting new space astronomy groups. These new research groups can play a valuable role in adding vitality and ingenuity to the national effort, as well as creating an environment for entrepreneurial activity that may not otherwise be possible. NASA should continue a vigorous flight and research program which uses small payloads and not assume that they must be supplanted by the larger, generalized Explorers and Observatories.

Data analysis, modeling and theory funding; Archives.

The added support for data analysis, modeling and theory through the enhanced data analysis program (the NASA MO&DA program) is an important change from the past. We applaud the designation of budgets for such activities that is commensurate with the very large investments that are made in the flight and ground systems. It is crucial that such support be continued, and that it be protected from overruns in ongoing development programs. The goal of the science program is to increase scientific understanding, to encourage the interest of the population at large in science and technology with the goal of enhancing our technical education level, and of sharing with the public the excitement that arises from our developing understanding of the universe. Without a timely scientific return from the science missions we run the risk of compromising the timely funding of future missions.

These goals cannot be met without strong, continuing support for the analysis of the data and its understanding through a strong theory program. These programs should be broadly-based to allow the access and input of data and results from the broad spectrum of astronomical inputs that are needed for solving a particular scientific problem. This may require supporting the use of data from several missions, from ground-based telescopes, from archives, and from sophisticated modeling. The goal is, of course, scientific understanding, and not just data analysis. Thus the funding approaches should not be too narrowly defined.

Archives are likely to play an even more important role in the future, particularly as the volume and complexity of the data from space missions increases. These archives need to be readily accessible and need to be supported by scientists with a broad understanding of the nature and limitations of the particular data. This may often be by the scientists closely associated with the initial mission. Fortunately, techological developments will allow for decentralised archives and easy access through wide-bandwidth communications. There will clearly be a need for standardisation in the interfaces and for a generic and cost-effective archive technology so that substantial volumes of data can be disseminated quickly and cheaply.

VI. TECHNOLOGIES FOR THE NEXT CENTURY.

New technologies are essential for the next generation of large telescopes, interferometers, and future smaller missions.

Substantial future progress in space telescopes and instrumentation will require technological advances. A wide variety of demanding technologies are needed for state-of-the-art telescopes and instruments in the UV-Visible-IR wavelength region. Advances in these areas can benefit essentially all missions, small and large. Any program should incorporate a development phase and a clear demonstration that the goals of the program have been met. Several areas where substantial gains in capability are needed and which appear to be feasible are given below.

High-performance optics. Diffraction-limited optics are an integral part of space astronomy. A tough but not impractical goal is improving optical surfaces beyond the current state-of-the-art as represented by HST and the ESO 3.5 m NTT mirror. The optics need to have low-scattering surfaces for high dynamic range observations (e.g., for QSO "fuzz" observations and for planet detection). A challenging component of this is the fabrication of off-axis segments. Technologies such as stressed-lap polishing and ion-beam polishing have the potential for affordably manufacturing large as well as small optical surfaces with aspheric deviations far beyond what is currently practical. A program whose explicit objective is to demonstrate the polishing of a low-scattering, strongly-aspheric off-axis element should be instituted early in the decade.

Lightweight optics. Lightweighting will remain crucial for space systems, especially as interest increases in HEO and Lagrangian point locations. The same is true of systems destined for the moon. Lightweighting techniques are intimately tied to the polishing technologies that are available. Since lightweighting can result

in "print-through" to the surface, its application has been limited by the difficulty of removing such effects. The ability of ion polishing to potentially remove "print-through" allows for more aggressive lightweighting.

Active optics. Active sensing and control of optical surfaces will be valuable in reducing the structural requirements and to compensate for thermal and dimensional changes, particularly in passively-cooled systems. Active elements can also play a valuable role in fine pointing and tracking, thereby lessening the demands placed upon the pointing and tracking control system, with attendant savings in complexity, weight and cost.

Lightweight and active structures. Lightweight structures are essential for space missions. Considerable advances in performance can be obtained through use of new techniques for passive structures and through active structures. Such structures will play an important role for control of the location of critical elements in interferometers and in large optics systems (e.g., the secondary).

Tracking and pointing. The tracking and pointing system for HST was a very demanding aspect of the spacecraft. New approaches involving large area detectors and sophisticated on-board processing have the potential for substantial savings in weight and cost. Both technologies were not mature at the time HST's design was frozen. The use of active optical elements for fine pointing would also have significant cost and technology advantages.

Passive and active cooling. Passive cooling of the whole telescope to improve IR performance can return remarkable gains in sensitivity ($> 10^6$ beyond 3 μm). Active coolers will be needed to eliminate the need for expendable cryogens for IR detectors and instruments. These pose challenging vibration and lifetime problems and need further development. Passive cooling also sets a challenging problem for the manufacture and testing of large optics. This needs to be an integral part of the optics development program.

Detectors. Detectors for the UV, the Visible and the IR are a crucial part of any system. We are making great strides in all three areas, but need to keep up the momentum. While consumer and defense requirements have been major drivers for technological developments from which we have benefited, the characteristics required for astronomical missions are usually different. Resources are needed for further development of the detectors to ensure that the required performance characteristics are met.

Instrument optics and coatings. Increasing overall system efficiency will require substantial efforts to maximize the performance of instrument optics and coatings. New optical techniques are needed to allow the development of cameras with larger fields, better images and higher throughput – particularly with wide-band coatings. The wide-bandwidth of space instruments places particular demands upon coatings and filters. Further development is needed.

Computers and electronics. The remarkable gains in computional capability over the last decade can provide sophisticated on-board processing leading to significant performance gains in the operation of the telescope and instruments. Simplification of optical-structural systems can also result with attendent cost savings. Concerns about the reliability of such systems could be alleviated by self-checking, redundant systems.

VII. LUNAR-BASED TELESCOPES AND INSTRUMENTS.

The Lunar Outpost program offers opportunities for a long-term astronomical program of remarkable scope and power. Of some concern to the astronomical community is the timescale for the implementation of such a program, and the potential for large gaps in time between orbital and subsequent generations of lunar-based facilities. It is imperative that a rational and realistic program be developed that allows for overlap between ongoing and new instruments. Such continuing capability allows for a well-planned transition and allows for contingency in case of delays. The lunar outpost program is ambitious, as it should be, but its impact on space science and the potential for damaging a vibrant scientific program which has considerable value for the scientific base of the nation should not be underestimated.

Currently the Lunar Outpost program in the UV-IR range incorporates three missions that allow phased implementation of capability. The initial project is a compact, simple instrument, the Lunar Transit telescope, which is followed by the phased implementation of an interferometer and a telescope, and culminates in a 16 m telescope of astonishing power, and a Visible-IR interferometer of some 5-10 km in size that is of comparable power.

The Lunar Transit telescope. This telescope is a wide-field non-pointed imaging telescope with a series of CCD detectors optimized for operation from 0.1 to 2 μm. The focal-plane arrays record and

transfer data at the apparent sideral rate and carry out a survey across a strip of the sky. The result is a wide-bandwidth, deep multicolor imaging survey. Repeated scans allow for the detection of variable objects. Limiting magnitudes are V $\sim$ 28 magnitude for a 2 m telescope with an image size of 0.1 arcsec at 0.5 μm. Its compact design and lack of moving parts makes it particulary attractive for the Emplacement phase of the development of the Lunar Outpost. Examples of the scientific returns range from the systematic detection and study of gravitationally-lensed objects, to deriving the structural and photometric properties of large numbers of galaxies ($\gg 10^5$), to the variability of QSOs and AGNs, and the detection of numerous supernovae.

The 16 m Lunar NGST. The NGST is a passively-cooled, diffraction-limited telescope. Its imaging and spectroscopic capabilities for a variety of scientific programs across the UV to the IR would be quite remarkable. Its scientific goals would range from the detection and spectroscopic measurement of Earth-like planets to the study of galaxies at redshifts $z > 1$ where substantial evolution and possibly even the formation of galaxies could be observed directly. Its combination of sensitivity, wavelength coverage and resolution would result in quantum jumps in knowledge in areas as diverse as the evolution of the ISM to the formation of stellar systems to the evolution of galaxy clusters.

The telescope structure and optics would be passively-cooled to 100°K or less in the lunar night, lowering the background in the 3-4 μm zodiacal "window" to less than 10^{-6} of that from the ground. Its low background, resolution and low-scattering optics at 10 μm would allow it to both image and take spectra of planets at the separation expected for Earth-like planets around the nearest stars.

The telescope would have a lightweight, segmented primary with active wavefront sensing and control for use into the UV. The segmentation would allow for a modular approach to consruction of the telescope by robotic means or by astronauts. Designs in which instruments can be placed on or beneath the Lunar surface are also being considered. This has advantages for stability, maintainability, reduced particle flux, and for instrument upgrades. With such a major facility the ability to upgrade and change the instrumentation during its presumably very long life is an important factor. The stability of the lunar surface and the ability to isolate sources of noise would improve its pointing and tracking performance.

It would be sited near the large interferometer. By combining the 16 m with an interferometer high sensitivity can be achieved from the large area of 16 m, thereby increasing the number and type of sources that can be studied at interferometric resolutions. By providing imaging on scales of 5 milliarcsecond to arcminutes, it also provides a natural overlap between space interferometry and ground-based observations.

The Lunar Optical-IR Interferometer. This km-scale interferometer exploits the stability of the lunar surface and the lack of an atmosphere to develop an interferometer of striking scale and hence resolving power. Techniques can be developed and tested on the earth and in orbit that will allow the appropriate choices of technology to be made as the Outpost develops.

The science potential ranges from submilliarcsec imaging of accretion disks around massive black holes in AGNs to images of extra-solar planets from Jupiter-sized to Earth-sized. The number of candidates could be very large within within 20 parsec. For stellar astrophysics the goals range from images of surface features of stars, both for normal stars as well as unusual objects such as X-ray binaries with neutron star companions. Elucidating the structure, the kinematics and the mass distributions close to the center of galaxies is another goal that is probably not achievable by other means.

Its astrometric performance is similarly striking with capability to sub-microarcsecond levels. This could give direct parallaxes to nearby galaxies, gravitational deflection of starlight to second-order, and determine the isotropy of the Hubble flow. Its astrometric capabilities would include the detection and determination of the masses of extra-solar planets.

The basic concept of the program is that of an optical VLA with a substantial number (> 10) of 1-2 meter-sized individual elements, with baselines to several km. This would allow a resolution for imaging of 10 μas and for astrometry of 0.1 μas. An initial 3 element system during the Emplacement phase would grow to many times that in the Utilization phase. Compared to Earth-based systems the gain is about a factor of 100 over instruments such as the VLT. It would be passively-cooled during the lunar night for good IR performance.

These telescopes have great scientific merit, and represent ambitious, but appropriate goals for the Lunar Outpost. Technology development towards these goals is needed and timely, provided emphasis is given in the early phases to demonstration projects of technologies that have broad utility for space science missions. The technologies highlighted in §VI meet these goals.

INTERFEROMETRY PANEL

STEPHEN RIDGWAY, National Optical Astronomy Observatories, *Chair*
ROBERT W. WILSON, AT&T Bell Laboratories, *Vice-Chair*

MITCHELL C. BEGELMAN, University of Colorado, Boulder
PETER BENDER, University of Colorado, Boulder
BERNARD F. BURKE, Massachusetts Institute of Technology
TIM CORNWELL, National Radio Astronomy Observatory
RONALD DREVER, California Institute of Technology
H. MELVIN DYCK, University of Wyoming
KENNETH J. JOHNSTON, Naval Research Laboratory
EDWARD KIBBLEWHITE, University of Chicago
SHRINIVAS R. KULKARNI, California Institute of Technology
HAROLD A. McALISTER, Georgia State University
DONALD W. McCARTHY, JR., University of Arizona
PETER NISENSON, Harvard-Smithsonian Center for Astrophysics
CARL B. PILCHER, NASA Headquarters
ROBERT REASENBERG, Harvard-Smithsonian Astrophysical Observatory
FRANCOIS RODDIER, University of Hawaii
ANNEILA I. SARGENT, California Institute of Technology
MICHAEL SHAO, Jet Propulsion Laboratory
ROBERT V. STACHNIK, NASA Headquarters
KIP THORNE, California Institute of Technology
CHARLES H. TOWNES, University of California, Berkeley
RAINER WEISS, Massachusetts Institute of Technology
RAY J. WEYMANN, Mt. Wilson and Las Campanas Observatory