FINAL REPORT

OF THE

HUBBLE SPACE TELESCOPE

POST-SM4

SCIENTIFIC REVIEW PANEL

April 25, 2003

EXECUTIVE SUMMARY

Recent NASA Congressional Appropriations language directed NASA to conduct studies partly in response to the loss of the Space Shuttle Columbia. The part of that language that is relevant to this document is: "*The conferees direct NASA to carry out an in-depth study of an additional servicing mission (SM5) in the 2007 timeframe that would study operating HST until the Webb Telescope becomes operational. The study should address the costs of an additional servicing mission and the potential scientific benefits.*"

The OSS therefore chartered the *HST Post SM4 Scientific Review Panel* to evaluate the existing HST Program and OSS plans, and studies upon which they are based. The panel was to consider the potential scientific benefits and the costs of such an additional HST servicing mission, SM5, and comment on the preliminary concepts for new instrumentation. In particular, the panel was asked to address the following questions:

1.) What is the scientific potential of HST if an SM5 is launched (e.g., in 2007-8) which includes engineering refurbishments/upgrades (e.g., gyros) but no new instruments? Would the scientific return of lengthened operations with the post-SM4 instrument complement (ACS, NICMOS, STIS, COS, WFC3) be worth the cost of such a mission, in the context of the overall Origins Programs? In its response, the panel should include an assessment of the post-SM5 uniqueness of HST scientific capabilities under this scenario.

2.) Would either of the candidate new instrument possibilities studied in late-2002 (a very wide-field imager and an optimized coronagraph), make an SM5 especially valuable in ways beyond simple life extension discussed above? Would such instrument(s) be feasible and of minimal risk while at the same time offering new performance capabilities not before realized on HST or on the ground? In its response, the panel should include an assessment of the post-SM5 uniqueness of HST scientific capabilities under this scenario.

The key assumptions for the Panel's deliberation were as follows:

- *That SM4 was conducted in the 2004-05 timeframe and was fully successful (this implies full functionality in all ORU's including a full complement of six gyros resulting in a 50 percent probability of undiminished scientific operability of four years following SM4 in addition to any instrument updates).*
- *The cost for an additional service mission in the time frame of interest is ~ $300M (adjusted to FY '03).*
- *The cost for a new instrument is near the average cost (i.e., $85-100M) for previously developed HST instruments and two instruments (COS & WFC3) still in development for installation during SM4. This estimate assumes that any new instrument would require no significant, specific technology development.*
- *That the current OSS share of "marginal shuttle costs", approximately $120M, would be covered under full cost accounting (note that the cost of SM5 to OSS could be much higher if OSS were required to fund the mission fully).*

The Panel approached its deliberations consistent with a set of ground rules provided by NASA. The top-level ground rules were:

- *Funding for an SM5 must be found within the current Origins Program.*
- *Funding for an SM5 must not come at the expense of the JWST development schedule (See congressional language).*

The Panel worked over a roughly three-week period beginning in late March. The Panel met twice via teleconference and there was a daylong face-to-face meeting in Houston on April 7, 2003. The Panel report that is summarized here was submitted to the Astronomy and Physics Division of NASA's Office of Space Science on April 25, 2003.

NASA provided the Panel with an extensive and useful set of documentation for review, and the members were encouraged to talk with anyone they believed could provide insight or additional factual information germane to the Panel's charter. The Panel extends its appreciation to many people who provided input to our deliberations. We particularly acknowledge the helping hand of Dr. Eric Smith, the HST and JWST Program Scientist.

The Panel developed an extensive detailed set of written materials in response to our charge. These are embodied in the Final Report. We summarize here the top-level findings and comments from that Report. We have organized this document in terms of the Panel's response to each of the questions contained in our charter. The Panel notes that its charter was of necessity sharply constrained. For this reason, we strongly urge the reader to read our "Caveats, Comments, and Concluding Remarks" at the end of this Summary.

CHARTER QUESTIONS AND PANEL RESPONSES

Q: *What is the scientific potential of HST if an SM5 is launched (e.g., in 2007-8) which includes engineering refurbishments/upgrades (e.g., gyros) but no new instruments?*

A: The Hubble Space Telescope is arguably the most effective and productive scientific facility ever launched. It has produced discoveries that have revolutionized our scientific understanding of the universe in which we live, and engaged the public in the business of scientific inquiry in a captive and informative way that has few equals in the annals of science.

The Panel concluded that with the possible exception of one instrument, STIS, none of the instruments aboard HST in the 2007 timeframe would be at discernable risk of failure. ***On the assumption of a fully successful SM4, the Panel was unanimous in its view that HST would continue to provide the highest quality scientific return at and beyond the time of a proposed SM5.***

Q: *Would the scientific return of lengthened operations with the post-SM4 instrument complement (ACS, NICMOS, STIS, COS, WFC3) be worth the cost of such a mission, in the context of the overall Origins Programs?*

A: The history of science is clear. The qualitative scientific return of any facility evolves with time. The opening of new capabilities leads quickly to a period of discovery, as well as the resolution of questions that could only be posed by results from previous facilities. The Panel sees HST in this mode through 2007 and beyond with the instrument complement assumed to be in place at the time of SM5. At some time in their history all scientific facilities move to a phase of continuing high productivity, but no longer operate at the forefront of discovery and rapid scientific progress. This could be because of a lack of natural phenomena to study and/or in the case of an astronomical facility a limit posed by sensitivity, resolution (spatial and/or spectral), or wavelengths accessible to the instruments. For example, HST provides unique capabilities for observing faint objects at high spatial resolution and provides unique access to the space-ultraviolet. When in the post-SM4 era HST would reach this state of being is unclear, but it is the view of some of the Panel that in the context of the

Origins Program, it would occur at some time after 2007 and before the currently advertised availability of JWST (2011) if no new instruments were placed on HST at the time of SM5.

The charge to the Panel required us to assess the "worth" of an SM5 in the context of the Origins Program. ***Based upon the Panel's understanding of the cost of a minimal SM5 (i.e., no new instruments), and the expected gradual reduction of HST's contributions to the major scientific objectives of the Origins Program if no new instruments were installed, the Panel concluded that in the absence of <u>full</u> additional funding, we could not endorse an SM5 at the expense of currently budgeted missions in the Origins Program. The Panel concurred unanimously with the Congressional conferees that funding for an SM5 should <u>not</u> be taken from JWST development. The relatively short duration of our study and the expertise mix of the Panel did not allow us to make similarly unqualified judgments regarding other Origins missions.***

Q: *Would either of the candidate new instrument possibilities studied in late-2002 (a very wide-field imager and an optimized coronagraph), make an SM5 especially valuable in ways beyond simple life extension discussed above?*

A: The Panel assessed a concept for an optimized coronagraph that was a derivative of an instrument proposed previously, but not selected, for flight on HST. The Panel also assessed a concept for an Ultra Wide-Field Imager (UWFI). As noted in the Panel's response to the following question, we had technical reservations about both of these concepts and that invariably colors our response to this question.

It was the assessment of the Panel that these capabilities on HST could produce useful, indeed significant science, particularly an UWFI. However, we were unable to reach a consensus that either of these instruments would "make SM5 especially valuable in ways beyond simple extension" of HST. ***Therefore, the Panel cannot fully endorse a justification for an SM5 based solely on potential added science that might derive from these instruments.***

Q: *Would such instrument(s) be feasible and of minimal risk while at the same time offering new performance capabilities not before realized on HST or on the ground?*

A: The Panel had a number of technical reservations regarding these concepts. In the case of the coronagraph, these include, but are not limited to, the ability of the instrument to achieve the required diffracted and scattered light rejection/contrast to the level required to address its principal scientific goal of detecting mature (i.e., cold) Jupiter analogs revolving around nearby stars. This is a three order of magnitude improvement over current coronagraphic capability on HST. ***The opinion of the Panel was that it would be a substantial challenge to overcome these technical difficulties in time to make an SM5 launch date.***

The Panel also had technical concerns regarding the UWFI concept. The choice of detectors, i.e., CCDs or HgCdTe arrays, is important, as it will set the maximum workable wavelength range for the UWFI. The scientific potential of a UWFI is likely to be maximized if it has an infrared capability to detect high-redshift galaxies and supernovae. This choice could increase the technical challenges associated with fabrication, testing, and qualifying the large number of arrays needed to cover the large focal plane. Experience with the WFC3 array and those developed for the JWST provide a basis for this concern. ***The Panel also noted that there are competing concepts (e.g., SNAP) that could accomplish many of the scientific objectives of the UWFI.***

We found it useful to try and capture the findings of our assessments in a single Table. While this is fraught with peril due to oversimplification, we offer it here. The Table is almost self-explanatory. Instrument uniqueness is characterized by numerical rating as follows: "1" => Unique, "2" => Competitive concepts exist, but are unfunded, and "3" => Competitive funded facilities exist. The exception is the shorthand used to identify the Origins Science topics that are addressed by each instrument. These topics are designated according to the major scientific Objective (X) and associated specific Investigation (Y) as given in the recently published Origins Roadmap.

Table I: Summary of Instrument Status, Origins Science, and Uniqueness

Instrument	SM5[1]	2010[1]	Origins Science (X.Y)	Uniqueness
ACS	+	+[2]	1.3, 1.4, 2.3	1[3]
COS	+	+	1.2, 1.3, 1.4, 2.1, 3.1	1
NICMOS	+	-[4]	2.1, 2.3	3[5]
STIS	?[6]	?[6]	See COS[7]	3[8]
WFC3	+	+	1.1, 1.3, 1.4, 2.1	1(UV-Vis), 3 (IR)[9]
Adv. Coron.	N/A	+	2.3, 2.4	2[10]
UWFI	N/A	+	1.1, 1.3, 1.4, 2.1	2[11]

Table Footnotes:

1. Status of instrument at this time, "+" => no major problems foreseen, "-" => potential technical issue
2. Loss (~ 5 – 10 percent) in effective area of the ACS WFC due to "hot pixels".
3. Key strengths come from synergy with WFC3.
4. Concern is available spacecraft power for parallel operations.
5. Competing facilities are WFC3 and JWST.
6. Major unknown is the continuing health of Side 2.
7. The contributions of STIS are largely accomplished by the times of interest or can be done by COS.
8. Competing facility is COS.
9. Competing facility is JWST.
10. Competing facilities are coronagraphic DISCOVERY missions.
11. Competing facilities are SNAP and LSST.

CAVEATS, COMMENTS AND CONCLUDING REMARKS

It was noted above that the Panel had a sharply defined charge. A full assessment of the value of an SM5, with or without new instruments, is a multi-dimensional issue. We worked under a number of key assumptions and ground rules that on one hand simplified our task, but on the other qualifies our conclusions.

Among the caveats that we would like to identify is the uncertainty in the time to a fifty percent likelihood of diminished scientific operability for HST systems following SM4. Estimates obtained by the Panel for this key time ranged from roughly two and a half to roughly four years. While we were constrained to consider an SM5 in the 2007-08 timeframe, the time, indeed the existence, of SM4 remains unknown in light of the Columbia disaster and it's effect on shuttle manifesting. The former "time to failure" is consistent with our assumed time for SM5, while the latter could mean that one might wait until 2009 for such a mission. That is close to the nominal end of mission time for HST and only a few years from the advertised launch of JWST.

The Panel was unable to reach a consensus that either of the instrument concepts for installation at the time of SM5 was compelling. That is not to say that better concepts might not be found. A Request for Information (RFI) might be considered to see if alternative concepts exist. This would have to be done on a fast track to have any hope of meeting a 2007-08 launch date.

We have tried to emphasize that, in accord with our charter, most of the scientific metrics used in our assessments are those set out in the recently published Origins Roadmap. We would be remiss if we did not point out the tremendous scientific value of HST and its complement of instruments post-SM4 for the other leg of the astronomy and astrophysics stool at NASA, the Structure and Evolution of the Universe (SEU).

The "value" of an SM5 must be viewed in as broad a context as possible. It involves the assessment of the contributions and longevity of the post-SM4 HST in the context of real, not planning, time. It involves a reasonable accurate understanding of the budget and development profiles of JWST, SIM, and possibly other missions.

Contemplating and planning for the end of a mission that has been extraordinarily successful is a difficult exercise. The challenge for those responsible for HST will be to recognize when that time has arrived. This Panel is convinced that time will not have arrived by 2007-08, provided COS and WFC3 are emplaced at the time of SM4. Our vision beyond that time is less clear. The Panel was of differing views regarding the overall scientific value of HST in the second decade of this century, when it must be compared with other missions that would stand on the shoulders of discoveries made by HST.

INTRODUCTION

Background

The tragic loss of the Shuttle Columbia and associated delays in resumption of shuttle operations led to inclusion of the following language in the recent NASA Appropriations:

> "The conferees commend NASA for the continued success of the Hubble Space Telescope and the extraordinary contributions it has made to the advancement of science. The recent success of the Hubble servicing mission has underscored the continued importance of the Hubble Space telescope (HST). NASA's plan for HST has been to discontinue servicing missions after 2004 in order to create a funding wedge for the next generation space telescope (NGST), the science community's highest priority, and return HST to earth in 2010. Due to the loss of Columbia, the conferees are aware that the current schedule for servicing HST has potentially been delayed and could possibly cause degradation of HST earlier than currently anticipated. The current situation may also require additional funding for HST. The conferees direct the program manager to maintain the current schedule for NGST development and not reduce NGST funds to cover HST shortfalls. The conferees direct NASA to carry out an in-depth study of an additional servicing mission (SM5) in the 2007 timeframe that would study operating HST until the Webb Telescope becomes operational. The study should address the costs of an additional servicing mission and the potential scientific benefits. Further, the conferees direct NASA to study the means for disposing of Hubble following the deployment of the Webb telescope in the 2010 timeframe. This study should examine the full range of options for disposal of the Hubble including relative costs and mission constraints."

As a part of its response to the congressional directive given above, NASA formed a scientific review panel to assess an additional service mission (SM5). This report communicates the findings of that review panel.

Panel Charter

The baseline Hubble Space Telescope (HST) servicing/operations plan developed by the HST Program Office under the NASA Office of Space Science (OSS) direction contains a final servicing mission in 2004 November (SM4), a retrieval/return-to-Earth mission carried out by Space Shuttle astronauts in 2010, and post-SM4 scientific operations extending as far toward 2010 or beyond as the onboard hardware allows. Both the 2004 and 2010 missions are in the OSS budget.

As noted above, Congress has directed NASA to study an additional servicing mission ("SM5") that would permit HST operations until the start of James Webb Space Telescope (JWST) operations in approximately 2011.

The OSS therefore chartered the *HST Post SM4 Scientific Review Panel* to evaluate these existing HST Program and OSS plans, and studies upon which they are based. The panel should consider the potential scientific benefits and the costs of such an additional HST servicing mission and comment on the preliminary concepts for new instrumentation. In particular, the panel should address the following questions:

3.) What is the scientific potential of HST if an SM5 is launched (e.g., in 2007-8) which includes engineering refurbishments/upgrades (e.g., gyros) but no new instruments? Would the scientific return of lengthened operations with the post-SM4 instrument complement (ACS, NICMOS, STIS, COS, WFC3) be worth the cost of such a mission, in the context of the overall Origins Programs? In its response, the panel should include an assessment of the post-SM5 uniqueness of HST scientific capabilities under this scenario.

4.) Would either of the candidate new instrument possibilities studied in late-2002 (a very wide-field imager and an optimized coronagraph), make an SM5 especially valuable in ways beyond simple life extension discussed above? Would such instrument(s) be feasible and of minimal risk while at the same time offering new performance capabilities not before realized on HST or on the ground? In its response, the panel should include an assessment of the post-SM5 uniqueness of HST scientific capabilities under this scenario.

Assumptions, Ground Rules, and Process

Assumptions

Any review of this type must have a clearly stated set of assumptions and ground rules. The key assumptions for the Panel's deliberation are as follows:

- *That SM4 was conducted in the 2004-05 timeframe and was fully successful (this implies full functionality in all ORU's including a full complement of six gyros resulting in a 50 percent probability of undiminished scientific operability of four years following SM4 in addition to any instrument updates).*
- *The cost for an additional service mission in the time frame of interest is ~ $300M.*
- *The cost for a new instrument is near the average cost (i.e., $85-100M) for previously developed HST instruments and two instruments (COS & WFC3) still in development for installation during SM4. This estimate assumes that any new instrument would require no significant, specific technology development.*
- *That the current OSS share of "marginal shuttle costs", approximately $120M, would be covered under full cost accounting (note that the cost of SM5 to OSS could be much higher if OSS were required to fund the mission fully).*

Ground Rules

The Panel approached its deliberations consistent with a set of ground rules provided by NASA. The top-level ground rules were:

- *Funding for an SM5 must be found within the current Origins Program.*
- *Funding for an SM5 must not come at the expense of the JWST development schedule (See congressional language).*

Process

NASA provided the following items as input to the panel for their consideration:

Origins Roadmap 2003
HST Servicing Strategy Interim Report
HST Reliability Assessment Model
HST Program plan for an Observatory return mission
Study of post-SM4 science prospects, new instrument candidates
OSS figures for HST instrumentation costs and program budget projections

During their discussions the panel sought additional technical details or clarifications through the OSS and/or the HST Program Office as it deemed necessary and relevant to its charge.

Panel membership (see Appendix A) was finalized the week of March 17, 2003. The Panel was asked to conclude its deliberations and provide a report to NASA by April 25, 2003. In order to meet this schedule the Panel operated in accord with the following schedule:

- March 21: Panel kickoff teleconference
- March 27: Second teleconference of Panel and NASA officials
- April 7: One-day Panel meeting in Houston, TX
- April 18: Draft Final Report sent by Panel Chair to Panel members for review and concurrence
- April 22: Comments on Final Report due back from Panel members
- April 25: Final Report submitted to NASA Office of Space Science, Astronomy and Physics Division

Because of the schedule constraints, the Panel worked on the various aspects of its charter in a parallel fashion. At the end of the March 21 teleconference, a Panel member was assigned one of the five instruments assumed to be onboard the HST following SM4. Their task was to assess the likely status of these instruments, using the NASA-provided reference material listed above and any other formal or informal information channels at their disposal, at the time of SM5 and roughly 2010. They were asked to characterize the state of the instrument at those times (identify as best they could the nature of any statistical degradation/failures) and the measurement/scientific capability of the instrument at those two times. Consistent with the Panel charter, they were asked to focus on those science goals highlighted in the recent Origins Roadmap, but they were asked to also identify any scientific value outside the Origins Roadmap (e.g., to SEU or SSE) that they feet the instrument would have at the two reference times. They were also asked to identify possible synergies between instruments that make the whole greater than the sum of the parts. Finally, they were asked to identify the "uniqueness", if any, of HST scientific capability in the time frame of interest or if none is apparent so indicate. The remaining two Panel members reviewed the Origins Roadmap to make an independent assessment of top-level scientific Goals and Investigations to which they felt the HST could make a significant contribution.

The findings of each Panel member were shared with all Panel members in advance of the March 27 teleconference and discussed extensively during the teleconference. At the end of this teleconference, two teams of three Panel members were formed to conduct the same type of assessment as conducted for the five instruments onboard following SM4 on two new instruments (a very wide-field imager and an optimized coronagraph) that are considered for possible installation at the time of SM5.

The Panel met in Houston, TX on April 7, 2003 to review the assessments by the two teams formed March 27 and to review our overall assessment of the value of the putative SM5 mission, including options to add instruments. Particular attention was paid to issues of the relative scientific merits of HST and its instrument complement at SM5 and roughly 2010 vis-à-vis those programs that would have to provide funding to cover the cost of SM5. The Panel also examined the unique contributions of HST at these times and under the varying assumed instrumentation relative to other space missions as well as ground-based facilities.

The material to follow contains a summary of the key Origins Science Objectives and Investigations as described in the recent Origins Roadmap. That is followed by an assessment of the state of HST, as indicated in the Panel charter, at the proposed time of an SM5. A similar discussion is then presented on the Panel's assessment of the state of HST in 2010-11. The report concludes with a Summary and Conclusions.

ORIGINS SCIENCE OBJECTIVES

The third "Origins Roadmap" was published in 2002. It establishes, based upon extensive input from the Origins science communities, the primary Scientific Objectives for NASA's Origins Program and it identifies the key Scientific Investigations that should be conducted in order to make substantive progress in achieving these Objectives. These Investigations in turn suggest generically the type of measurement(s) that need to be made and these can then be mapped into a particular NASA mission.

The three Scientific Objectives for the current Origins Roadmap are:

1. To understand how today's universe of galaxies, stars, and planets came to be.
2. To learn how stars and planetary systems form and evolve.
3. To explore the diversity of other worlds and search for those that might harbor life.

The Scientific Investigations associated with Objective One are:

1. Study how pristine gas from the Big Bang condensed into the first generation of stars, and how their supernovae produced the first heavy chemical elements.
2. Observe the enormous release of energy during the building of the first massive black holes that combined with energy from the first stars to change the structure of the universe.
3. Describe the assembly of galaxies and their subsequent evolution from generations of stars, leading to the diversity of galaxies in today's universe.
4. Study how the lifecycles of stars in the Milky Way and other galaxies build up the chemical elements and galactic environments needed for planets and life.
5. Observe when and where habitats for life emerged in the Milky Way and other galaxies.

The Scientific Investigations associated with Objective Two are:

1. Investigate molecular clouds as cradles for star and planet formation.
2. Study the emergence of stellar systems.
3. Determine how protoplanetary dust and gas disks mature into planetary systems.
4. Search for evidence of planets in disks around other stars.
5. Conduct the census of planetary systems around stars of all ages.

The Scientific Investigations associated with Objective Three are:

1. Study the properties of giant extrasolar planets using the combined light of planet and parent star.
2. Detect giant planets by direct imaging, and study their properties.
3. Which nearby stars host terrestrial planets that might be suitable for life?
4. What are the compositions of the atmosphere of terrestrial planets orbiting nearby stars? Which of these planets are suitable abodes for life?
5. Determine optimal biosignatures for life on other worlds.
6. Search for evidence of life on habitable planets orbiting other stars.

The set of three science objectives and the associated 16 scientific investigations listed above provided the basis for the Panel's assessment of the likely contribution of observations with HST to the Origins Program. In the material to follow we refer to investigation Y in objective X as (X.Y).

ASSESSMENT OF HST AT THE TIME OF SM5

Consistent with the assumption of a fully successful SM4, the instrument suite on HST at this time would consist of ACS, COS, NICMOS, STIS, WFC3 (and FGS1R, though not specifically germane to the Panel's discussions or Charter). The Panel, using all of the reference material provided by NASA as well as extensive personal contact with a variety of people familiar with each of these instruments, has assessed the likely status of each instrument with specific attention on an instrument's capability to continue to gather data that substantially informs the strategic scientific objectives and investigations delineated in the Origins Roadmap.

The Panel has also assessed the uniqueness of each instrument. In this regard, we identified other space-based platforms, or ground-based facilities that contribute to these issues at a comparable level of significance. The Panel developed the following numerical factors to characterize uniqueness. A rating of **1** signifies that the HST instrument uniquely addresses a particular science issue. A rating of **2** signifies that the instrument is not unique and that its competitors are not yet funded programs. A rating of **3** signifies that the instrument is not unique and that its competitors are funded programs. Examples will be cited in each case.

Status of HST Instruments at SM5 and Beyond

Prior to summarizing the panel's instrument-by-instrument assessment, it is important to restate the charge to the panel. In particular, we were asked to consider an SM5 in the 2007 timeframe. It must be noted that during the Panel's deliberations we were informed that the latest estimates of the so-called HST survivability timescale (i.e., a 50 percent probability of undiminished scientific operability) is four years after SM4. Under any set of assumptions, this would mean that HST has a reasonable chance of working well until at least 2009 and perhaps beyond that **even if there is no SM5**. We will comment later on what we see as possible ramifications of this, but our assessments, per our charter and the congressional language, are predicated on a presumed 2007 date for an SM5.

ACS

The three most vulnerable components to catastrophic failure are the entrance door, the filter wheels, and the thermal electric coolers that support the Wide Field Camera (WFC) and the High Resolution Camera (HRC). The entrance door has a second motor that can permanently drive it out of the light path if the positioning motor shows signs of failure. And, like all of the motors in ACS, the entrance door motor has dual (redundant) windings. Likewise, although the two motors on the two WFC filter wheels are not redundant, they do have dual winding. If a motor began to fail on one set of windings, it could be operated with the other. The camera could be operated with the filter wheel fixed to do single-band imaging in the WFC and HRC.

If either of the thermal electric coolers that support the WFC and HRC should fail, the ACS would be lost. However, the largest risk of this happening was at launch. The ACS has redundant electronics, and there is also some redundancy in the calibration lamps and, if needed, sky flats will suffice. In the past, there has not been a catastrophic instrument failure on Hubble and such a failure is unlikely to occur with the ACS.

The two likely most important causes of degradation are in the CCDs, namely declining charge transfer efficiency (CTE) and an increasing number of hot pixels.

The ACS CCDs are undergoing a continuous loss of CTE. Empirical estimates of the effect of CTE loss on WFC photometry suggest that for a relative bright source on non-trivial background in the middle of the chip, the effect would increase the photometric uncertainty by 10%. This would increase to 50% for a faint source in low background at the edge of the chip.

A photon preflash method of CTE mitigation was added to the ACS to mitigate this problem To first order, CTE loss is likely to produce more traps rather than deeper traps. If this is this case, a preflash or deep sky background that is sufficient to fill the traps in 2005 will be sufficient in 2010. The penalty will be increased shot noise in the preflash (read noise from 5 to 7.5e).

The HRC is much less susceptible to CTE loss than the WFC because of its smaller CCDs and because readout can be done via four amplifiers, reducing the maximum number of shifts to 512.

Unless a better technique is developed for annealing hot pixels, by 2005 ~ 2.5% of the ACS WFC pixels will be "hot" (e/s > 0.04), and ~ 5.5% by 2010. Dithering (eg by 4 steps) the images helps, because each pixel on the sky is sampled by 4 ACS pixels. Combining dithering with hot pixel maps should eliminate most bad pixels in the resulting image. But, effectively, by 2010 the effective area will have been reduced by ~ 6% in addition to the 1to 25 percent loss due to the effect of cosmic rays.

In summary, the performance of the ACS will undergo continuous decline, but the performance loss in 2010 relative to 2005 will have not have a substantial impact on its ability to pursue the fundamental science themes outlines in Section 4.

<u>COS</u>

COS was designed for a seven-year lifetime, including life-testing critical components for use between 2003 (the original date for SM4) and 2010 (original end of mission). The spectrograph grating and delay-line detectors are not subject to the usual degradation of CCDs or MAMAs, and the electronics were designed to be robust. It is likely that COS could last beyond seven years, and could continue to provide high-throughput UV spectroscopy beyond 2010.

Given the more substantial concerns for the longevity of STIS, it is important that COS be installed on SM4. In the last part of the HST mission, COS might be the only available spectrograph, providing unique space access to the ultraviolet.

<u>NICMOS</u>

No degradation in performance has been seen in the three NICMOS detectors due to radiation exposure or other causes despite six years in orbit (i.e., no increase in dark currents, number of hot or inoperable pixels, well-depths, or non-temperature related changes in quantum efficiencies). The increase in dark currents over cycle 7 is as expected due to the higher operating temperature of the arrays with the NICMOS Cooling System (NCS). These dark current levels are replicated both in pre-flight testing of the flight detectors at these temperatures, and post-cycle 7 testing of a flight-spare detector of the same pedigree at Steward Observatory's NICMOS detector lab.

Unlike CCDs these detectors need no periodic "annealing" to reduce dark currents, and suffer no incremental performance degradation with repeated radiation exposure. There are no credible mechanisms for degradation in detector performance for the remainder of the HST mission (i.e., out to 2010). Also, unlike CCDs, the unit cells of the

NICMOS HgCdTe detectors are directly accessed when non-destructively "read out" and do not suffer from charge transfer inefficiencies (as charge is not sequentially transferred), and thus no creep in CTI with time.

Approximately 0.2% (~ 120) pixels in each NICMOS camera have reduced throughput (some effectively inoperable) due to surface contamination from post-launch particulate deposition. It is fairly well established that these particles (known as "grot"), are typically tens of microns, in size, and arose from the scraping and subsequent flaking of the low-IR scatter paint at the point of contact of the thermal short in the optical entrance post baffle assemble at the front end of the cold well. There had been some concern that the warm-up and subsequent cool-down of NICMOS (between SN2 exhaustion and NCS activation) would induce new particles which would adhere to the detectors by cold-stiction - but this was NOT seen. The mechanical configuration of the dewar at the point-of-contact appears unchanged (as evidenced by both its affect on the optical alignment, and the heat flow into the dewar). Hence, there is little concern over future (incremental) grot growth if /when the NCS is power cycled.

The NICMOS detectors are performing in all respects as anticipated for 77K operation.

The NCS, though of only a one-year on-orbit pedigree has performed flawlessly and continuously, is apparently completely sealed (no leakage of neon coolant per results from a December 2002 engineering study) and has maintained positive thermal control and stability over the NICMOS detectors with higher precision than in the cycle 7 era using a passive SN_2 cryogen. The "tall pole" in a catastrophic NICMOS+NCS failure would likely be a failure in the NICMOS Cyro Cooler (NCC) Power Conversion Electronics (PCE). Once initial turbine spin up and infant mortality period for the PCE passed, which it has, the probability of failure for the PCE was placed at nine percent over its first five years of operation.

Based upon the post SM-3B solar array performance and models of their long-term degradation, the spacecraft power system should be able to sustain all five post-SM4 science instruments (plus NCS) in normal operate mode concurrently until after mid-2007.

Hence there is no power resource necessity for decommissioning NICMOS prior to a proposed SM5. Beyond this epoch, NICMOS {+NCS} would have to be operated in a power-sharing management or campaign mode would preclude unrestricted parallel operations, or with more constrained allowable spacecraft off-nominal roll orientations which and would further constrain the scheduling of many programs.

With a successful installation of the Aft shroud Cooling System in SM4, the aft shroud instrument sink temperatures in the environment of the post-SM4 instrument complement will be sufficiently low, with only minimal change projected out through 2008 (or beyond in the absence of additional instrument change outs), to permit parallel operations in an unrestricted manner.

Our evaluation of the current and projected state of the NICMOS instrument, and NICMOS+NCS, indicates a high degree of probability for continuing operability with little or no loss of capability, degradation in performance or decline in scientific return into the 2007 - 2010 time frame, given a programmatic decision to continue its operation, and spacecraft power availability. NICMOS thermo-mechanical, electrical, and detector/electronics systems show no signs of wear and continue to perform to pre-launch expectations.

STIS

There are no consumables to limit the STIS lifetime. STIS was designed to have a fair amount of redundancy. In particular, if either MAMA detector were to fail, then another detector can provide partial backup. If the FUV MAMA failed completely, then the NUV MAMA can provide similar spectral resolving power, although some of the modes would have about half the efficiency. If the NUV MAMA were to fail completely, then the CCD can provide partial redundancy through the G230LB and G230MB modes, which are already supported as prime modes because the CCD is more efficient than the MAMA above 250 nm. These modes have less long-wavelength scatter protection than the prime NUV MAMA modes. The NUV echelle modes would be lost if the NUV MAMA failed. No detectors have failed to date.

The STIS electrical system was designed to be redundant, consisting of side 1 and side 2, which are each able to receive power, and data and command handling, from either side of the HST C&DH. Each side of STIS can control the entire instrument independent of the other side.

This redundancy has proven to be valuable as side 1 has failed and STIS is now fully functional, and dependent upon side 2. The cause(s) of the side 1 failure is not known. One possibility is that a capacitor was wired backwards. If that were true of side 2 we could expect a similar lifetime of 4 years of use, ending in 2005, but this is pure speculation. The risks of failure at or soon after launch of a new instrument are higher than that of a mature instrument in steady conditions in space.

There are eight mechanisms in STIS: the mode select mechanism, the slit wheel, the CCD shutter, the corrector mechanism, the external shutter, the calibration insertion mechanism, the mode isolation shutter, and the echelle blocker. None of these have failed to date, but now have single side electrical dependence.

The UV performance of STIS is changing only slowly. Optical degradation of ~3% per year has been observed at 1216A, with less at longer wavelengths, so changes from now (2003) to 2007 and 2010 should be <12% and 21% respectively. Detector backgrounds will fluctuate, increasing slowly until SM4, and then should decrease drastically with cooling applied.

Any optical degradation so far is negligible. CCD radiation effects continue, with charge transfer efficiency (CTE) losses and an increase in the number of hot pixels. Offering a spectroscopy slit position near the output, for short slits, is minimizing CTE losses. Hot pixels can be removed partially by tracking and subtracting them, or better by dithering along the slit. Both problems represent an operational nuisance rather than a net change in performance.

WFC3

As with any imager the critical lifetime components of WFC3 are its mechanisms, electronics, and detectors. WFC3 is built for a minimum 5-year lifetime in all components, projecting high reliability to the canonical 2010 de-orbiting mission. The instrument is also fully redundant, so most failure modes would allow the instrument to continue operations in one or more channels. The engineering design is conservative, and most components have proven their long-term reliability on other instruments on HST or elsewhere. There are few if any untested technologies or components (e.g. the cryo-cooler on NICMOS) used in the design.

As was the case with WFPC2 and ACS, the performance of WFC3 is expected to deteriorate gradually due to the combined effects of radiation damage (UVIS) and thermal cycling (IR). However many lessons learned from the previous imagers have been incorporated into the design of WFC3. For the CCDs the primary deterioration is in hot pixels and degraded charge transfer efficiency (CTE). It is believed that the hot pixel annealing problems encountered with ACS is unique to those detectors, and will be much less severe with the Marconi detectors used on WFC3 (and as has been the case with WFPC2 and STIS). Significant CTE degradation is expected, and to help reduce its long-term effects a charge-injection capability is being implemented along with the more traditional solution of pre-flash/post-flash. The charge injection will increase the effective readout noise to 15 electrons (from a nominal three electrons), but this compares to about 40 electrons for post-flashing (as with ACS). This may have some programmatic impacts (e.g., giving high priority to low-background and narrowband applications early in the life of the instrument), but will not significantly impact the large majority of deep broadband imaging science.

For the IR channel thermal cycling can degrade the detector performance after a large number (dozens) of cycles, and the instrument will be operated to minimize the number of cycles (few per year at most). This could become an issue, for example, if this channel needed to be turned off frequently to provide power for other instruments.

Contamination control of optical surfaces has been a risk factor for the UV performance of WFPC1 and WFPC2, but it is not expected to be a significant factor for WFC3. The short wavelength limit of 200 nm avoids the most problematic region of the spectrum, and monitoring of STIS throughput in the UV gives the WFC3 team confidence that degradation over 5 years will be of order a few percent at most, even at the shortest wavelengths.

In summary the extension of WFC3 operation beyond its 5-year design lifetime would result in modest degradation in performance (and significant degradation for low-background applications), but projected risk of a major instrument failure within 7-8 years of launch is small.

New Instruments at the time of SM5

The panel was asked to comment on two instruments for possible emplacement on HST at the time of SM5. These are some form of advanced coronagraphic instrument and an ultra wide-field imaging instrument (UWFI). The panel considered two surrogate concepts in their assessment. We used the "Extra-Solar Planet Observer" (ESPO) – a derivative of the CODEX concept that was proposed previously but not selected as an HST instrument, as the baseline for the coronagraphic instrument. Our baseline for the UWFI is a concept put forward by the Hubble Space Telescope Program. In this concept, the UWFI would be placed in the focal plane at the location of FGS, and would replace FGS. It must be stressed that these are both just concepts at this time.

Advanced Coronagraphic Instrument

The current HST imaging instruments with coronagraphic modes are limited in their diffractive light rejection due to uncompensated systematics in the HST optical telescope assembly and optics. As a metric for comparison, NICMOS coronagraphy at 1.6 microns achieves azimuthally medianed per-pixel (76 mas pixels) diffractive and scattered light rejection from an occulted target of 10E-7 of the total stellar flux density 1" from the occulted star. This is obtained after subtracting the rotationally invariant point spread function (PSF) by rotating the field about the target axis during observation of the target star. ESPO would improve upon this by at least 2-3 orders of magnitude.

The systematics which dominate in limiting the rejection of circumstellar diffracted light currently are due to:

a) Temporal instability in the fine structure of the HST PSF due to "breathing" (from axial motion of the HST secondary mirror support of several microns due to orbit/attitude driven thermal inputs).

b) Phase errors in the HST primary mirror, primarily at mid-spatial frequencies (resulting in a "fixed" speckle pattern in the absence of temporal instabilities).

c) Line-of-site pointing authority of the PCS of ~ 4 mas RMS, with a dominant frequency of ~. 0.5 Hz.

We note that item (c) is "lost in the noise" of (a) and (b) for current HST instruments.

The ESPO concept relies on closed-loop control with a DM to mitigate the mid-frequency phase errors in the HST primary mirror. As proposed a DM with 96x96 or 128x128 elements with 1mm actuator spacing densities is called for to achieve the image contrasts required for imaging of gas giant "exoplanets." Such a DM/control system with >~ 10,000 elements significantly exceeds even the most "extreme" DMs used (for AO applications) today, and the technology development for a flight qualified system may pose a not-insignificant risk to mission success given the timescale for the fabrication of an instrument in time for the proposed SM5 (~2007). A demonstration unit from Xinetics with 21x21 actuators has been produced and has achieved a surface flatness of 0.025 nm rms (1/13,000 wave @ 0.63 microns) controlling to ~ 5 cycles/cm. While this is promising, scalability from an ~ 400 actuator to an ~ 10,000 actuator system to satisfy the ESPO/HST requirements is not assured, and likely not linear. The non-linearity certainly holds in the control system in solving the reconstructor matrix. The temporal stability of the mid-frequency ripple at the level to be to be controlled is not well characterized. Hence, the requisite bandwidth for the control authority to sense (integrate with sufficient S/N rapidly enough), solve, correct, and control for such a large number of actuators is questionable.

A simplifying alternative to a DM is to use a static phase mirror to correct the HST primary phase errors - also under the presumption that the dominant residual phase errors (from an imperfect optic) are temporally stable. Such a demonstrator optic has been produced by Tinsley based on the best-known prescription of the HST primary from phase-retrieval maps. Tests have shown that this optic performs extremely well. However, it fails by a factor of about five from achieving the needed correction, just due to the uncertainty in the maps of the primary, which are not currently characterized to sufficient accuracy. If the HST Project were to approve a campaign to map the primary mirror to the required accuracy level (using focus sweeps/phase-retrieval, similar to the HARP campaign), then an optic could be produced on a fairly short timescale (months) to incorporate that information (Tinsley has now automated the process). It is expected that this specially designed optic will still fall somewhat short of providing the necessary wavefront correction. Separation of planets from speckles could be further aided by using spectral information – i.e., by taking images in several broad bands.

The ESPO design includes a Gaussian shaped pupil mask, which they propose to obtain from Photosciences/UHawaii. Trauger has a small mask produced by this firm, but it is not as big as required by ESPO. Producing density-graded masks with the required high degree of precision is extremely difficult. This aspect of the development is not presented in the ESPO material that was available to the panel.

In summary, the panel has significant technical concerns about this concept, some of which are intrinsic to the concept and others of which derive from the fact that the HST is not particularly well suited to this type of observation. We explore this later in addressing the uniqueness aspect of this instrument.

<u>UWFI</u>

The choice of detector technology (CCD or HgCdTe arrays) will set the maximum workable wavelength range (300 - 1000 nm or 500 - 1800 nm respectively) for UWFI, and this was still being debated within the Space Telescope Science Institute at the time of this review. The uniqueness and discovery potential of UWFI is likely to be maximized with an infrared capability: it is required for detecting the highest-redshift galaxies and supernovae of interest, and it is where the gain in background performance over the ground is maximized. This choice may increase the technical challenges associated with fabricating, testing, and qualifying the large number of arrays that would be needed to cover the large focal plane of UWFI.

The panel expressed concern regarding the fabrication, procurement, and testing of detector arrays for the UWFI. UWFI would need four 4K x 2K plus two 1K x 1K CCDs, or eight 2K x 2K HgCdTe arrays.

The panel noted that use of HgCdTe arrays allows a tailoring of the IR cutoff by means of the cooling option, but they also pose a not insignificant technical risk. Specifically, 2K x 2K arrays have just been produced for the JWST program (the WFC3 array is 1K x 1K, and it took years of effort to get a few flight qualified arrays out of ~ 70 test objects), and the butting of eight is the perceived risk.

The panel noted that the center-to-corner tiling of the UWFI field would cover an angular distance of ~ 6 arcminutes. With HST, the differential velocity aberration from the point of correction (field center) to the corner would induce time variable shifts of the line-of-site of up to approximately 1/4-pixel. Such field, pointing, and time dependent "image smear" might compromise the most photometrically challenging of UWFI's science goals.

Finally, the panel notes that the placement of UWFI into an FGS bay would relegate HST to only two FGS's for attitude and roll control. This would impact at some level the scheduability of science observations, particularly orientation constrained observations, but would also remove operational redundancy in the event of a failure of one (of the now 3) FGSs. UWFI proposes its own internal "guiding arrays", but coupling this into the HST pointing control system is an undeveloped and untested capability that also poses some inherent risks.

Origins Science

It is the view of the Panel that HST could contribute to the following Origins science investigations. While we do not explicitly address the timing or duration of these contributions relative to operation of HST, it is our impression that much, but not all, of the observations described below will have taken place by 2010 if not by the time of a possible SM5 (2007).

As a reminder, Origins science topics discussed below are identified according to the Objective (X) and Investigation (Y) in an "X.Y" format.

1.1 Study how pristine gas from the Big Bang condensed into the first generation of stars, and how their supernovae produced the first heavy chemical elements.

HST should provide the first foray into the $z > 6$ universe, search for rare very-high redshift "dropouts", and identify objects for further study by ground-based telescopes (e.g., ALMA). This is likely to depend on wide-field imaging because of the rarity of these objects.

1.2 Observe the enormous release of energy during the building of the first massive black holes that combined with energy from the first stars to change the structure of the universe.

HST can be used to study the origin of IGM structure in He II Lyman-alpha forest clouds during late-time helium reionization ($z = 2.8$-3.2) arising from the turn-on of the first black holes and quasars.

1.3 Describe the assembly of galaxies and their subsequent evolution from generations of stars, leading to the diversity of galaxies in today's universe.

Conduct more extensive imaging of $z \sim 1$□ 6 galaxies, obtaining colors, morphology, and structural information (e.g., merging) for a much larger and more complete sample of these early galaxies. This requires a new wide-field imager to make significant gains. Also, probes of gas along QSO lines-of-sight to study galaxy evolution at $z < 2$, and to conduct a census of baryons in the intergalactic medium. Probe radiative and chemical feedback from galaxies to the IGM. This requires COS with its factor of 10 – 20 greater sensitivity than STIS.

1.4 Study how the lifecycles of stars in the Milky Way and other galaxies build up the chemical elements and galactic environments needed for planets and life.

Conduct more extensive probe of the stellar populations of nearby galaxies to determine how the star-gas-dust cycle varies in galaxies of different mass or morphology via deep color/magnitude diagrams. Undertake emission-line surveys of young star-forming regions and metal-rich supernova remnants (requires wide-field imaging with appropriate filters).

2.1 Investigate molecular clouds as cradles for star and planet formation.

More extensive spectroscopic probing of the interstellar medium with COS. Measure abundances and physical attributes with COS (turbulence, line widths and temperature) and molecular abundances of CO and perhaps H_2O and small molecular clusters, through UV absorption transitions to cloud depth A_V = 5-10 mag, complementary to radio studies. Also, conduct more extensive NICMOS and WFC3 (infrared) imaging of star-forming regions. Where possible, penetrate into the protostellar environment and compare with CO observations. Can be coordinated with SOFIA and SIRTF observations.

2.3 Determine how protoplanetary dust and gas disks mature into planetary systems.

Direct examination by spatially resolved imaging of circumstellar disks which may harbor recently formed planets and planetary systems in evolution. HST high-contrast coronagraphy permits probing the close environs of unobscured young stars looking for signatures of planets and planetesimals in *nacent (gas dominated), transitional (e.g., Herbig AeBe) and older (dusty debris) circumstellar disks through their dynamical interactions with disk grains. Characterizing the underlying dynamics may constrain our understanding the process of planet formation. SIRTF will provide a rich set of candidate systems, identified through thermal emission, which can be imaged coronagraphically at high spatial resolution with HST.*

2.4 Search for evidence of planets in disks around other stars.

Direct imaging of Jovian planets around nearby stars. Ties in with ground-based radio velocity surveys. Competition from specially designed, dedicated coronagraphic 2-meter class telescopes soon to be proposed.

3.1 Study the properties of giant extrasolar planets using the combined light of planet and parent star.

Utilize STIS and COS spectroscopic observations of planetary transits to learn about the atmospheres of "hot Jupiters."

Uniqueness of Observational Capability

The panel was asked to assess the uniqueness of HST to contribute to the Origins scientific Objectives and Investigations described above. Our assessments, along with a characterization of the state of the instruments at the two fiducial times for those assessments, are given in Table I. Instrument uniqueness is characterized by numerical rating as follows: "1" => Unique, "2" => Competitive concepts exist, but are unfunded, and "3" => Competitive funded facilities exist.

Table I: Summary of Instrument Status, Origins Science, and Uniqueness

Instrument	SM5[1]	2010[1]	Origins Science (X.Y)	Uniqueness
ACS	+	+[2]	1.3, 1.4, 2.3	1[3]
COS	+	+	1.2, 1.3, 1.4, 2.1, 3.1	1
NICMOS	+	-[4]	2.1, 2.3	3[5]
STIS	?[6]	?[6]	See COS[7]	3[8]
WFC3	+	+	1.1, 1.3, 1.4, 2.1	1(UV-Vis), 3 (IR)[9]
Adv. Coron.	N/A	+	2.3, 2.4	2[10]
UWFI	N/A	+	1.1, 1.3, 1.4, 2.1	2[11]

Table Footnotes:

1. Status of instrument at this time, "+" => no major problems foreseen, "-" => potential technical issue
2. Loss (~ 5 – 10 percent) in effective area of the ACS WFC due to "hot pixels".
3. Key strengths come from synergy with WFC3.
4. Concern is available spacecraft power for parallel operations.
5. Competing facilities are WFC3 and JWST.
6. Major unknown is the continuing health of Side 2.
7. The contributions of STIS are largely accomplished by the times of interest or can be done by COS.
8. Competing facility is COS.
9. Competing facility is JWST.
10. Competing facilities are coronagraphic DISCOVERY missions.
11. Competing facilities are SNAP and LSST.

SUMMARY AND CONCLUDING REMARKS

The Panel has assessed the HST science program and capabilities in general, as well as with the specific goals of the Origins program. The Panel concluded that HST will make substantial contributions to astronomy in general and to (1) the study of galaxy birth, assembly, and evolution, (2) the study of the interstellar medium in connection to the birth of stars, and (3) the formation of planets around young stars, key scientific objectives of the Origins Program. These contributions are certain to remain important through 2007, assuming the baseline complement of instruments envisioned to be in place following SM4. The Panel was unanimous in its assessment that HST would remain a scientifically productive facility through 2010, with important contributions across many areas of astronomy. A majority of the Panel believes that the contributions by HST to the Origins Program specifically will continue to be of unique and cutting-edge value in 2010, but this conclusion was not unanimous.

The Panel found it useful to consider three different options for HST. Option 1 is the present baseline plan where the final servicing mission is SM4, which we imagined would take place sometime early in 2005, and an end-of-mission visit by the Shuttle in 2010 (or later if HST is productive and can be operated safely). Option 2 would be an added SM5 mission during which HST's life is prolonged by restoring to full complement those Orbital Replacement Units that have failed or exhibited degraded performance. Option 3 includes SM5 refurbishment with the installation of one or two new science instruments. We discuss relevant points for each of these options below. For purpose of reference, we take the cost of a simple servicing mission as $400M with an added cost of $100M for each additional instrument – this includes the GSFC costs of mounting such a mission, the full-cost accounting of the launch, and plausible estimates provided to us for the costs of proposed instruments.

Option 1

Important to an assessment of Options 1 and 2 is an understanding of the expected science lifetime of HST after a fully successful SM4. This is the time when the probability of a catastrophic failure, one that would render HST fundamentally inoperative from a science return standpoint, is 50 percent. The Panel was furnished an estimate of 4.0 years for this critical time. The importance of this number is that should there be no SM5 in 2007 (i.e., Option 1), the best estimates currently available suggest that HST would continue to function, with a 50 percent probability, until roughly 2009. The Panel wishes to stress the importance of a timely and successful SM4 to the future of HST. This importance derives from refurbishment of key spacecraft components to the installation of two key science instruments, COS and WFC3.

Option 2

This Option has SM5 taking place in 2007 with no replacement of instruments. As noted above, the Panel concluded that HST would continue to provide excellent scientific return through 2010 with the SM4 complement of instruments in place. The cost, in 2003 dollars, for SM5 is roughly $400M. The Panel was asked to comment on whether the scientific return of lengthened operations with the post-SM4 instrument complement (ACS, NICMOS, STIS, COS, WFC3) would be worth the cost of such a mission, in the context of the overall Origins Programs? The Panel concluded that it would be difficult to rationalize a currently unbudgeted SM5 at the expense of funded or anticipated missions in the Origins Program (e.g. JWST or SIM, the only two Origins missions with sufficient funding in this timeframe to provide funds for an SM5). The Panel unanimously concurred with the Congressional conferees that funding for an

SM5 should <u>not</u> be taken from JWST development. The relatively short duration of our study and the expertise mix of the Panel did not allow us to make similarly unqualified judgments regarding other Origins missions.

<u>Option 3</u>

The Committee reviewed two instrument concepts, a high-performance coronagraph and a very-wide-field camera, which have risen to the fore as the prime candidates for instrumentation that would enable genuinely new science with HST. Either of these instruments could address key Origins science objectives, but the concepts as presented were viewed by the Panel as having many technical challenges and risks.

The detection of planets around nearby stars is a principal goal of the Origins program; a high-performance coronagraph on HST could take a giant step forward towards that goal as a scientific and technical precursor of TPF. However, our review of the coronagraph proposals left us doubtful that HST was in fact the proper platform to take this step. Such an instrument would have to exceed the capability of current coronagraphic capability on HST by some three orders of magnitude. The Panel felt that there were technical issues/risks associated with actuators for deformable mirrors (the proposed concept needed 96 x 96 or more). HST was not designed for such coronagraphic observations and overcoming its limitations – for example, wavefront errors in mid-spatial-frequencies, scattered light, and time-variable performance – would be a technical challenge at best with a non-negligible risk of failure. The Panel noted that substantial momentum within the Origins community has already been generated toward a dedicated telescope+coronagraph of 2-m class. Because such a mission would include, from the start, control of all these important parameters, we concluded that this would be a more cost effective approach in both developing this important new technology toward TPF and reaping a harvest of Jovian planets around stars at distances $R < 10$ pc.

The Panel judged the proposal for an ultra-wide-field imager (UWFI) to be less risky and more likely to deliver Origins science that would not be provided by any as-yet approved mission. In order to be compelling, however, such an imager would have to include a near-IR capability out to at least 1.5 μm – this would afford a huge gain in area over WFC3's IR channel and enable surveys that could provide high redshift ($z > 6$) galaxy targets for JWST. We found this to be a stronger selling-point than the promise of measuring the equation-of-state of the cosmic inflationary field through large samples of $0.5 < z < 1.5$ Type Ia supernovae. Also, using SNe Ia's for cosmology requires observations of their light curves in multicolors, and the NIR is needed for objects with $z > 1$. Although HST with a wide-field-imager would outperform any other approved missions in this domain, we did not find it credible that sufficient numbers of supernovae would be found and accurately measured in order to place a tight limit on the equation-of-state. The Panel did have significant technical questions about this concept. Noteworthy here are the fabrication, procurement, and testing of detector arrays for the UWFI. UWFI would need four 4K x 2K plus two 1K x 1K CCDs, or eight 2K x 2K HgCdTe arrays. 2K x 2K arrays have just been produced for the JWST program (the WFC3 array is 1K x 1K, and it took years of effort to get a few flight qualified arrays out of ~ 70 test objects), and the butting of eight is a perceived risk. The Panel noted the possibility of large photometric errors arising from the large field of view of the UWFI leading to a combination of differential velocity aberration and undersampling. This concern may be overcome, but we saw no comments on this in the material that we had at our disposal and as this effect would be present at the few percent level, it needs to be addressed. Again, a telescope+imager designed for this purpose, for example, the proposed SNAP mission appears much more likely to make this important measurement at a level consistent with the accuracy of cosmological parameters from CMB missions such as Planck.

The Panel had reservations that either of the two concepts could be made ready, given the technical difficulties described above, in time for an SM5 in 2007.

The Panel concluded that the scientific productivity of HST would remain high **given a successful SM4**. This means that a decision to add SM5 should not in our view be driven primarily by the desire to add new instruments, but rather to extend HST's life in its post-SM4 configuration. If the decision to add SM5 is made, then the addition of new instruments can be evaluated as a case of incremental cost for the added science capability. In other words, the Panel does not regard the addition of new instruments as the justifiable sole basis for an SM5.

Appendix A: Panel Membership

- David C. Black, Universities Space Research Association, Panel Chair
- Alan Dressler, Carnegie Observatories
- Robert C. Kennicutt, University of Arizona
- George K. Miley, Leiden University
- William R. Oegerle, Goddard Space Flight Center
- Glenn H. Schneider, University of Arizona
- J. Michael Shull, University of Colorado