Challenges and Potential Solutions to Develop and Fund NASA Flagship Missions

Robert E. Bitten
The Aerospace Corporation
2310 E. El Segundo Blvd.
El Segundo, CA 90245
310-336-1917
robert.e.bitten@aero.org

Stephen A. Shinn
NASA Goddard Space Flight Center
8800 Greenbelt Road
Greenbelt, Maryland 20771
301-286-5894
stephen.a.shinn@nasa.gov

Debra L. Emmons
The Aerospace Corporation
2310 E. El Segundo Blvd.
El Segundo, CA 90245
310-418-7892
debra.l.emmons@aero.org

***Abstract*—Large, strategic "Flagship" missions have unique characteristics that lead to challenging developmental difficulties for the National Aeronautics and Space Administration (NASA). Missions such as the Hubble Space Telescope (HST), James Webb Space Telescope (JWST), and the Mars Science Laboratory (MSL) had technical and programmatic challenges that led to significant schedule delays and subsequent cost growth. Although NASA has instituted policies that have reduced cost growth for more "typical" NASA science missions, NASA Flagship missions remain a distinct challenge due to their requirement to provide unprecedented science or tackle bold exploration goals, typically while concurrently developing new technologies. The unique challenges presented by Flagship missions make it extremely difficult to fully predict cost and schedule given that the technical and programmatic advances needed to meet performance requirements are unprecedented. This paper addresses why Flagship missions are unique and proposes a new programmatic approach to develop and fund Flagship missions.**

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1. DEFINITION OF FLAGSHIP MISSIONS

According to Miriam-Webster's Dictionary, a Flagship is: 1) the ship that carries the commander of a fleet or subdivision of a fleet and flies the commander's flag, or 2) the finest, largest, or most important one of a group of things. [1] In many ways, National Aeronautics and Space Administration (NASA) Flagship missions incorporate both of these definitions as they are the most visible because of their potential for scientific discovery and expense and are also the finest, largest, and most important of NASA's science missions. NASA Flagship missions are unprecedented in the science that they enable as they provide exquisite measurements that cannot be done otherwise and typically require new technology or advanced engineering developments to acquire the measurements.

Figure 1 shows typical examples of NASA Flagship missions such as Viking, Galileo, the Hubble Space Telescope (HST), and others relative to their launch date and development cost. As shown in Figure 1, Flagship mission are typically developed on the order of every 10 years and have a cost greater than $2B fiscal year 2017. The development cost is from the start of preliminary development at the beginning of Phase B through launch and is taken from NASA historical public budget documents and then inflated to fiscal year 2017 equivalent dollars (FY17$). [2, 3] The relatively high cost of Flagship missions is due to their significant complexity with examples such as HST, as it was the largest pointable space telescope of its time, and the James Webb Space Telescope (JWST), which is larger than HST and operates at significantly colder temperatures while still meeting unprecedented stability requirements.

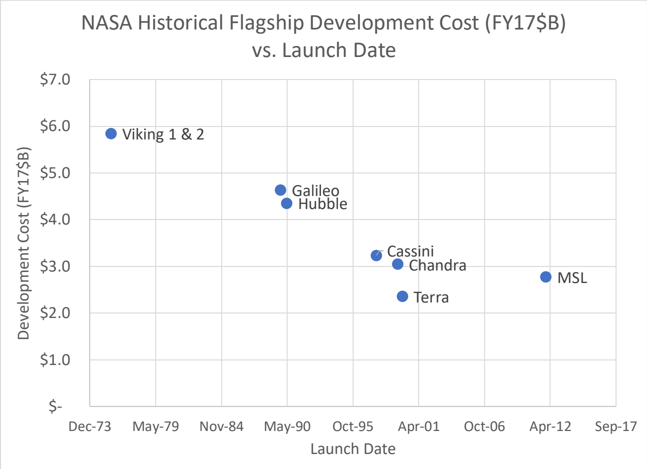

Figure 1. NASA Flagship Mission Cost vs. Launch Date

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2. BENEFITS OF FLAGSHIP MISSIONS

The benefits of Flagship missions are substantial. In 2017, the National Academies of Sciences assembled a panel to assess NASA large, strategic science missions. The following lists examples of the benefits identified by the panel for NASA's large, strategic missions: [4]

- "Capture science data that cannot be obtained in any other way, owing usually to the physics of the data capture driving the scale and complexity of the mission
- Answer many of the most compelling scientific questions facing the scientific fields supported by NASA's Science Mission Directorate, and most importantly, develop and deepen humanity's understanding of the Earth, our Solar System, and the universe
- Open new windows of scientific inquiry, expanding the discovery space of humanity's exploration of our own planet and the universe, and providing new technology and engineering approaches that can benefit future small, medium-size, and large missions
- Provide high-quality (precise and with stable absolute calibration) observations sustained over an extended period of time
- Support the workforce, the industrial base, and technology development
- Maintain U.S. leadership in space
- Maintain U.S. scientific leadership
- Produce scientific results and discoveries that capture the public's imagination and encourage young scientists and engineers to pursue science and technical careers
- Receive a high degree of external visibility, often symbolically representing NASA's science program as a whole
- Provide greater opportunities for international participation, cooperation, and collaboration as well as opportunities for deeper interdisciplinary investigations across NASA science areas."

In addition, the sheer number of scientific papers from Flagship missions is astounding. Figure 2 shows the number of scientific papers published from the data collected from HST. [5] As can be seen, the total number of papers exceeds 16,000 since HST's launch in 1990 with an average of over 800 papers over the last 5 years.

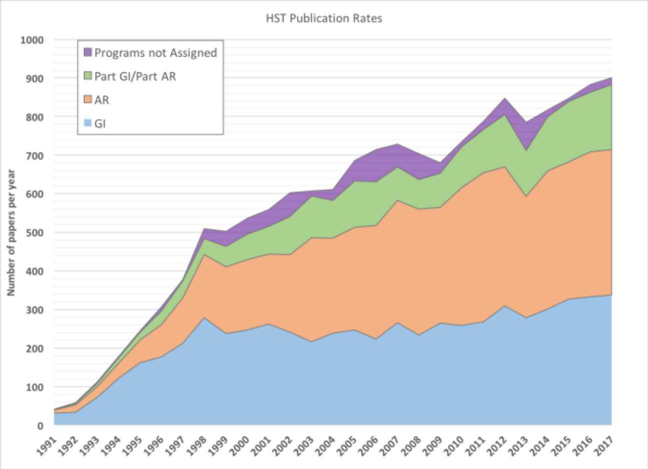


Figure 2. HST Publication Rates[5]

Further, technology developed for Flagship missions also have been transferred to commercial usages. For example, charge coupled device (CCD) development undertaken for an HST instrument was later incorporated in a stereotactic breast biopsy machine, which detects tumor positions accurately enough to steer the biopsy probe, thereby reducing the need for surgery and cutting costs by 75 percent. [6] Additionally, new improvements in wavefront sensing technology developed for JWST have led to the development of the Shack-Hartmann Sensor which has enabled eye doctors to get much more detailed information about the shape of your eye in seconds rather than hours. [7]

3. DIFFICULTY IN ESTIMATING FLAGSHIPS

Flagship missions, due to their significant complexity and unprecedented nature of their science, are inherently difficult to estimate. NASA mission costs are typically estimated given cost model or analogy cost based on historical cost and technical data. [8] Given that Flagship missions are first of a kind, there are no comparable costs to use as an estimate and, more importantly, all aspects of the mission that need to be done. Design trades and options are numerous and indeterminate through the development phase such that establishing a robust, stable technical baseline prior to the start of development, and therefore developing a robust, stable cost estimate, is extremely challenging.

A good example of this challenge is the school bus sized HST which was, at the time, the largest space telescope ever developed. It required the development of a lightweight, 2.4-meter mirror and several extremely complex scientific instruments. HST also was designed to be serviceable in-space by astronauts, something that had never been done, to replace spacecraft components and science instruments to be able to survive for its baseline 17-year mission life. Figure 3 shows a plot of the cost estimate and cumulative cost of HST during its development. The initial estimate was about one-third for the final actual cost of HST, demonstrating the difficulty of estimating an unprecedented, Flagship mission.

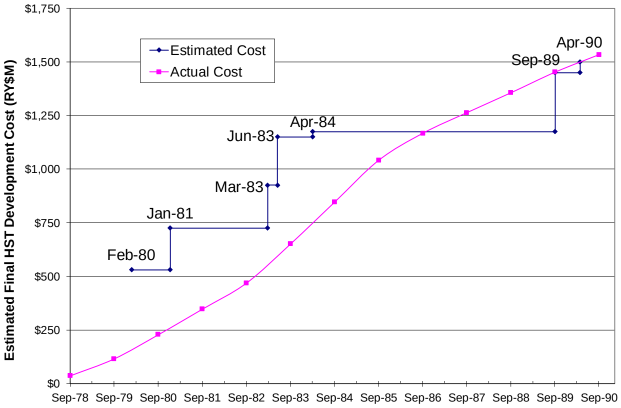


Figure 3. HST Cost Estimates vs. Actual Cost

The National Academies of Sciences identified the difficulty in assessing Flagship missions in Decadal Survey studies and developed the Cost and Technical Evaluation (CATE) process, in conjunction with The Aerospace Corporation, in an effort to estimate large, strategic missions given the best available knowledge. [9] The CATE process includes elements to anticipate cost growth such as design growth threats, launch vehicle threats and schedule threat in order to anticipate how the technical and programmatic baseline may evolve over time. The CATE process was implemented in the 2010 Astrophysics Decadal Survey as a result of the significant underestimation of several missions in the 2001 Astrophysics Decadal Survey.

4. FLAGSHIP COST GROWTH

Large projects are difficult, regardless of how much many times something similar has been undertaken. A recent study looked at over 60 different construction projects in the mining, infrastructure, and oil and gas industries. The study determined that the average cost overrun was 80% and the projects were delivered 2 years later than promised. [10]

In the transportation sector, “Megaprojects” is the term used to discuss the type of projects that has some key defining factors: funding requirements are large (on the order of hundreds of millions or billions of dollars); human resource demands are commensurately large; the projects have high complexity, with technology development requirements; and such projects have the potential to greatly impact their environment. [11] The “Big Dig,” which was a construction project to develop a tunnel under the Boston metropolitan area, ended up costing $14.6 billion relative to the original project cost estimate of $2.6 billion and was originally scheduled to take 4 years vs. 10 years it took to complete. [12] Although the United States Navy has been building aircraft carriers since prior to World War II, the latest Gerald R. Ford aircraft carrier, a literal Flagship commissioned in 2015, cost $13B and took 10 years to develop, which was 3 years behind schedule and $2.4B over its initial cost. [13]

A more complex project that is representative of the unprecedented nature of a NASA Flagship mission is the Large Hadron Collider (LHC), which was developed to find the Higgs boson constant. The initial cost of the LHC plus experiments was supposed to be on the order of 2.8B Swiss francs and ended up being around 5.8B. It was supposed to be built in 7 years and took 10 years to finish, as well as needing another year to become operational after a magnet quench incident. [14] Similarly, the U.S. version of the LHC, the Superconducting Super Collider (SSC), was originally supposed to cost $4.4B and was estimated to be $11B when it was cancelled after 6 years of development and over $2B spent. [15]

A study that was conducted in 2013 identified that Flagship missions stand out from other NASA missions in terms of complexity and visibility. The Flagship Assessment team identified several common issues affecting major mission performance: [16]

- “A low cost and schedule estimate, sometimes referred to as buy-in, submitted by a Program or project based on a number of beliefs, including optimism that the new mission can be done better (i.e., faster, cheaper) than previous missions, the notion that new techniques will improve cost and schedule performance, the desire on the part of those external to NASA for the Agency to find ways to do more work at a lower cost, experience that says changes will happen regardless of the robustness of the project’s plan, or a desire to win a competitive bid for the next new mission;
- Inadequate funding for concept studies, concept, and technology development;
- Changes in requirements, funding profiles, workforce, and partner contributions throughout development, even after the Agency has committed to mission content, cost, and schedule;
- Technical challenges, mission complexity, or the number of new technologies needed for the mission to succeed;
- Disconnects with the external budget environment or changes in the political environment; and
- Differences between Agency and stakeholder priorities where NASA prioritizes mission success and other stakeholders set delivering a mission on cost and schedule as an equal priority.”

Case Studies Overview

To illustrate the challenge with developing NASA Flagship missions, a series of case studies was investigated. These included the Mars Science Laboratory (MSL), the Space Interferometry Mission (SIM) and the James Webb Space Telescope (JWST).

Case Study – Mars Science Laboratory (MSL)

NASA's MSL mission was responsible for landing the largest Mars rover ever developed, Curiosity, which was the size of a small car. Its major objective was to find evidence of a past environment that could support microbial life. Curiosity carried the most advanced payload of scientific instruments ever used on Mars' surface with a payload more than 10 times heavier than earlier Mars rovers. More than 400 scientists from around the world participate in the science operations. [17]

To illustrate the relative size of the MSL rover, Figure 4 shows the comparison of Curiosity at 900 kg to previous NASA Mars landers and rovers including the Mars Exploration Rover (MER) at 174 kg, the Phoenix lander at 410 kg, and the Mars Pathfinder Sojourner rover at 11.5 kg. [18] MSL also required several critical technologies to be developed including: Entry, Descent, and Landing System, Mars Lander Engine, Long-Life, Extreme Environment Actuators, Sample Acquisition/Sample Processing and Handling System, Advanced Rover Technologies, Mobility Technologies, and Integrated Simulation Tools. [19]

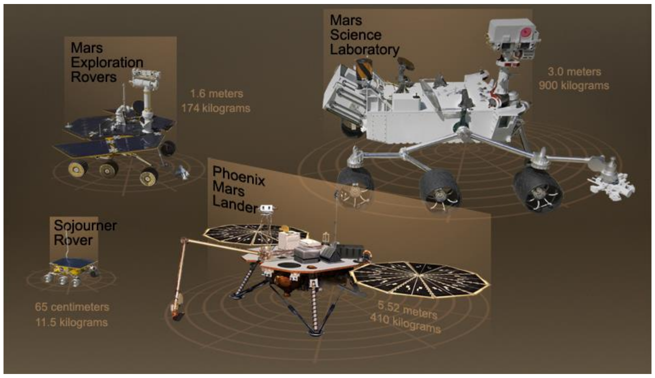


Figure 4. MSL, MER, Phoenix, Pathfinder Comparison[18]

The initial development schedule for MSL was aggressive given the unique first-of-a-kind capabilities that needed to be developed such as the Sky Crane, propulsive lander development, the sample acquisition, processing and handling systems, as well as the complex Sample Analysis at Mars (SAM) instrument which conducted the primary surface sample analysis. As the project progressed, it became evident that MSL could not develop the components required and successfully integrate and test the complete system prior to the original 2009 launch date. It was decided in December 2008 that the launch date should be moved to the next available launch date in 2011. [20]

MSL was chosen as part the 2003 National Academy of Sciences Planetary Decadal Survey estimate was estimated to be a "medium" class mission, which was defined as being less than $650M. [21] This characterization as a medium mission was problematic given that MSL requirements were not fully determined and there was not a design of MSL at the time of the Decadal Survey. The baseline cost for MSL set after the Preliminary Design Review (PDR) in August 2006 was $1.6B. [22] Due to the assumptions of the initial aggressive schedule, the required technology developments, and the subsequent 26-month delay for the next available launch period, the additional effort required resulted in a final cost for MSL of $2.5B. [23] Lessons learned from MSL, and a substantial amount of design heritage, are being incorporated into the next Mars rover mission, Mars 2020, which conducts Mars surface sample analysis and will collect samples for a future Mars Sample Return mission which has been a priority in previous decadal surveys. [24]

Case Study – Space Interferometry Mission (SIM)

SIM's science goals were primarily in the area of ultra-precise astrometry, the measurement of the minute motion of stars, and other astronomical sources. The goal was to provide a two orders of magnitude improvement in astrometric precision relative to the European Space Association Hipparcos mission. [25] Figure 5 illustrates the astrometric precision required by SIM as opposed to the Hipparcos and HST. [26] Technological challenges included nanometer-level control and stabilization of optical elements on a lightweight flexible structure, sub-nanometer-level sensing of optical element relative positions over meters of separation distance, and overall instrument complexity and the implications for interferometer integration and test and autonomous on-orbit operations. [27]

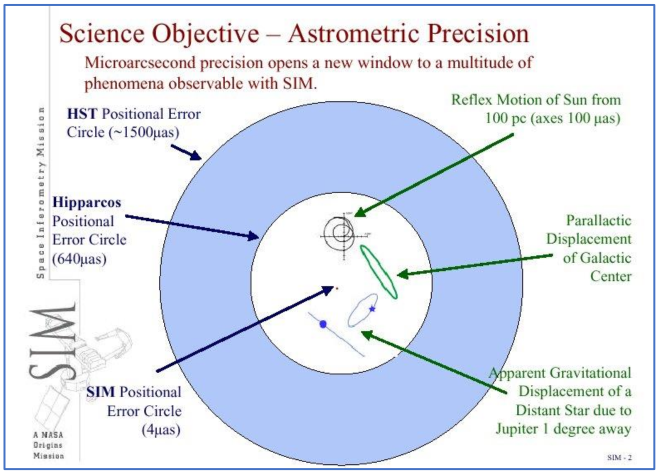


Figure 5. SIM Capability Relative to Other Telescopes

The 1991 Astrophysics Decadal Survey identified SIM, which was then known as the Astrometric Interferometry Mission (AIM), as a high priority mission which would use small telescopes in space separated by up to 100 meters to measure the positions of stars with 3-to 30-millionths of an arcsecond precision. It was considered a "moderate" mission at the cost of $250M. [28] SIM was also stated as high priority in the 2001 Astrophysics Decadal Survey as the committee reaffirmed the recommendations made regarding SIM in the previous Decadal Survey. In particular, the committee recognized that AIM had evolved to the more capable SIM mission, which would "enable the discovery of planets much more similar to Earth in mass and orbit than those detectable now, and it should permit astronomers to survey the Milky Way Galaxy 1,000 times more accurately than is possible now.” [29]

It was recognized, however, that the technology development required for SIM to be implemented should be fully developed prior to the start of system implementation. As such, a comprehensive technology development program was developed where eight specific technical milestones were identified before SIM could proceed to construction. By 2006, all of the eight technology milestones had been demonstrated. [30] Additionally, the concept for SIM continued to evolve, starting with a 10-meter baseline interferometer in 2000, to a 9-meter baseline in 2006, to the final 6-meter baseline, now referred to as SIMLite, in 2009, in an effort to simplify the design to reduce cost.

For the 2010 Astrophysics Decadal Survey, SIMLite was one of the many concepts evaluated. At this point, the National Academy of Sciences CATE process was in place and estimated the total cost of SIMLite to be $1.9B from October 2010 onward. Although SIMLite was technically mature and would provide a substantial, important new capability in interferometry, it was not included in the recommended program for the decade, following the committee’s consideration of “the strengths of competing compelling scientific opportunities and the highly constrained budget scenarios described in this report.” [31]

SIM experience showed that the process worked, i.e., that technology development was required before mission development began, and that design evolution could occur to satisfy both science advancements and budgetary constraints before, unfortunately, other missions took priority over SIM. Although almost $610M were spent on SIM technology development from 1999 to 2008, this funding was not wasted. [32] The money spent on technology development, however, was useful to reduce the risk and increase the robustness of the technical baseline for future missions, such as the Habitable Exoplanet (HabEx) Observatory mission, which will be submitted to the upcoming Astrophysics Decadal Survey. [33]

Case Study – James Webb Space Telescope (JWST)

JWST was initially identified as the Next Generation Space Telescope (NGST) as a follow-on to HST. JWST is the most complex space-based observatory ever developed requiring significant technology development to operate at cryogenic temperatures to take the measurement needed to meet its science requirements. JWST has a 6.5-meter aperture consisting of 18x1.32-meter beryllium mirrors which are unfolded to be able to fit within a launch vehicle’s fairing and are controlled individually to fine-tune the telescopes overall performance. Figure 6 illustrates the size of the fully deployed JWST mirrors vs. that of HST’s monolithic 2.4-meter mirror. [34] As noted previously, HST was the size of a school bus, while JWST, when fully deployed, is the size of a tennis court.

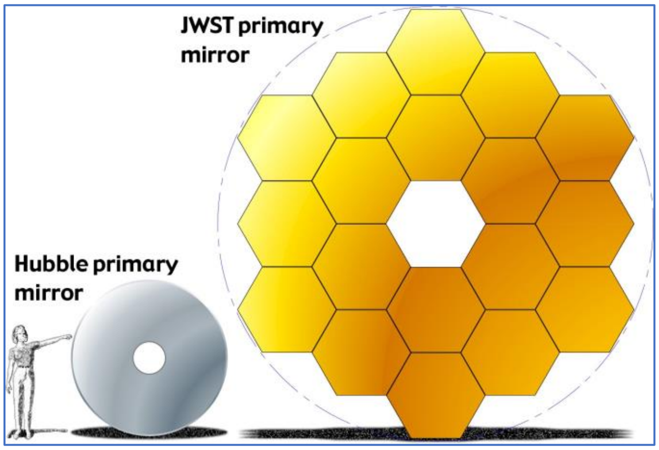


Figure 6. Relative Size of JWST Mirror to HST[34]

NGST was initiated during the “faster, better, cheaper” era in the mid-1990s by then NASA Administrator Daniel Goldin. Goldin challenged NASA to build an HST follow-on that was larger and cheaper than HST. This challenge included operating the 8-meter telescope at 40 degrees above absolute zero (40 Kelvin), while requiring both the telescope and a super-thin, lightweight thermal sunshield to be folded to fit within available launch vehicle fairings. [35]

Prior to the 2001 Astrophysics Decadal Survey, a study was conducted in 1997 to assess the feasibility of an 8-meter NGST. NASA and its industry and academic partners studied three approaches which included a TRW deployable 8-meter segmented primary mirror telescope and erectable sunshield deployed at Lagrange point L2 orbit, a Lockheed Martin monolithic 6-meter thin shell primary mirror telescope and fixed sunshade in an interplanetary orbit beyond that of Mars, and a Goddard Space Flight Center (GSFC) developed deployable 8-meter segmented primary mirror telescope and inflatable sunshield, also deployed at L2. [36]

All three teams determined that NASA could launch NGST by 2005 and confirmed, because of advanced technology and the requirement that the observatory have one-fourth the mass of HST, the Agency would be able to build NGST for significantly less than the $2 billion (1990 dollars) it had invested in HST. Each of the studies assumed, however, that NASA would receive at least $175 million (1996 dollars) for mission definition and technology development and another $500 million for construction. The estimates for each of the concepts, each of which is below $600M, is shown in Figure 7, for the three telescopes studies as well an alternative Lockheed-Martin 6-meter monolithic telescope. [36]

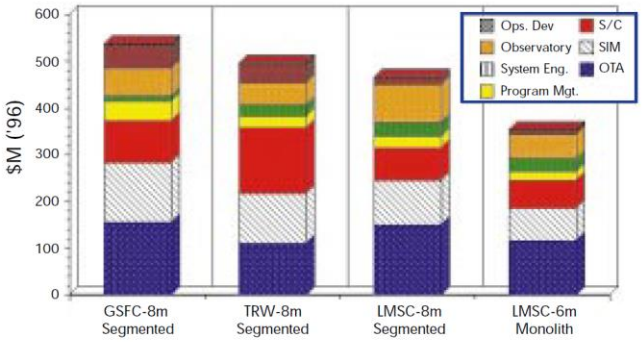
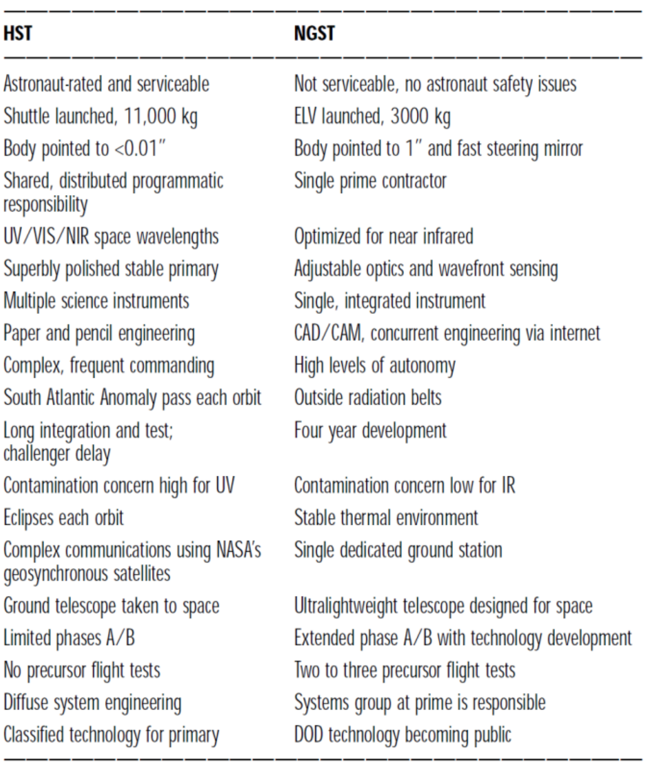


Figure 7. Comparison of Initial Estimates for NGST

The NGST study team rationalized that the cost of NGST could be lower than HST believing that HST was more complex since NGST was not serviceable, was much lighter and had reduced pointing requirements, would only be operated in near infrared, had innovative new computer-aided design and manufacturing (CAD/CAM) tools to reduce the cost of design, and was based on ground-based segmented telescopes so the primary segmented-mirror technology was already proven. A full list of this comparison is shown in Table 1 as taken from the NGST study report. [36]

Table 1. Comparison of HST to NGST from 1997 Study



HST	NGST
Astronaut-rated and serviceable	Not serviceable, no astronaut safety issues
Shuttle launched, 11,000 kg	ELV launched, 3000 kg
Body pointed to <0.01″	Body pointed to 1″ and fast steering mirror
Shared, distributed programmatic responsibility	Single prime contractor
UV/VIS/NIR space wavelengths	Optimized for near infrared
Superbly polished stable primary	Adjustable optics and wavefront sensing
Multiple science instruments	Single, integrated instrument
Paper and pencil engineering	CAD/CAM, concurrent engineering via internet
Complex, frequent commanding	High levels of autonomy
South Atlantic Anomaly pass each orbit	Outside radiation belts
Long integration and test; challenger delay	Four year development
Contamination concern high for UV	Contamination concern low for IR
Eclipses each orbit	Stable thermal environment
Complex communications using NASA's geosynchronous satellites	Single dedicated ground station
Ground telescope taken to space	Ultralightweight telescope designed for space
Limited phases A/B	Extended phase A/B with technology development
No precursor flight tests	Two to three precursor flight tests
Diffuse system engineering	Systems group at prime is responsible
Classified technology for primary	DOD technology becoming public

It was quickly realized that the initial estimate was underestimated such that the 2001 Astrophysics Decadal Survey provided an estimate of $1.2B, in FY 2000 dollars, for the development of the NGST. [37] By January 2003, after the selection of the primary contractor, the cost of the now-named JWST, 7-meter aperture telescope mission would be on the order of $1.6B in FY 2002 dollars. [38] The estimated cost of JWST increased to $3.5B by 2004 and $4.5B by 2006 and had evolved to its current design of a 6.5-meter telescope. [39]

Knowing that JWST needed to develop 10 technologies to be able to develop the system, the JWST project took the unusual step of having a Technology Non-Advocate Review (TNAR) to confirm that technology was mature. The TNAR consisted of non-advocate industry experts to review each of the 10 technologies to ensure that they reached Technology Readiness Level (TRL) 6 one-year prior to the JWST PDR. Specifically, the 10 technologies that needed to be developed included: [40]

1. Near Infrared (NIR) Detectors
2. Sidecar ASIC (application-specific integrated circuit)
3. Mid Infrared (MIR) Detectors
4. MIRI Cryocooler
5. Microshutters
6. Heat Switch
7. Sunshield Membrane
8. Wavefront Sensing & Control (WFS&C)
9. Primary Mirror
10. Cryogenic Stable Structures

At the time of PDR in 2008, the cost of JWST was $4.5B and all technologies were TRL 6 or above. The funding profile, however, did support the continued development of JWST. Historically peak funding for a project occurs after the Critical Design Review (CDR) as the design is being finalized and components are being delivered. As shown in Figure 8, the JWST funding profile peaked at PDR and then decreased significantly during the CDR and integration and test (I&T) phase. [41] As can be seen, spacecraft development had yet to begin while sunshade development and the full I&T of the most complex spacecraft system that has ever been developed, was still ahead. The funding profile resulted in work being deferred to the future given that annual funding constraints required immediate problems to be resolved.

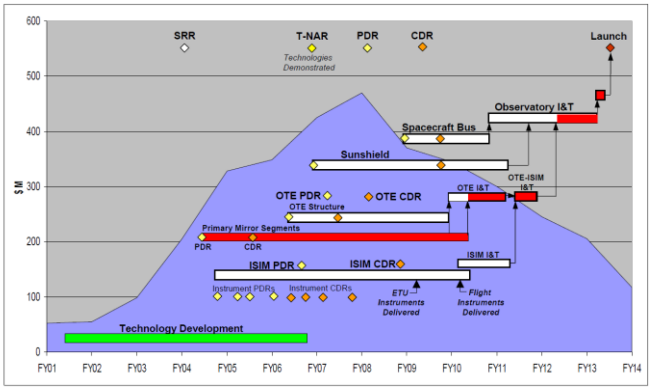


Figure 8. JWST Funding Profile as of May 2006

After JWST passed its CDR in 2010, there was a growing concern that deferred work would cause the launch schedule to slip and the cost of JWST to increase. The Independent Comprehensive Review Panel (ICRP) was formed and recommended that the earliest possible launch date was in late 2015 and that the development cost of JWST should be between $6.2B and $6.8B. [42]

As an exercise to understand the impact of the funding profile on the potential cost of JWST, The Aerospace Corporation ran an analysis that estimated an "ideal" funding profile vs. the actual funding profile made available to JWST based on historical funding profiles. [43] Figure 9 shows the results of the analysis and shows the significant decrease in funding after PDR that JWST experienced compared to the ideal funding profile that increases through CDR and into the first part of I&T. Based on other analysis conducted, the penalty associated with this reduced funding profile during time of need is on the order of $1B. [44]

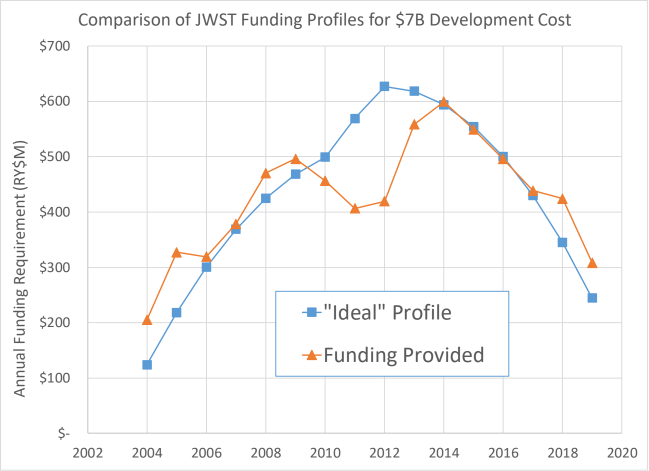


Figure 9. Comparison of Actual vs. Ideal Funding Profile

JWST, when launched, will be the most complex system ever sent into space and will provide new discoveries for the next decade. Further, the success of JWST will pave the way for next generation space telescopes such as the Large UV/Optical/IR Surveyor (LUVOIR) [45] and the Origins Space Telescope (OST). [46]

5. COULD COSTS HAVE BEEN ANTICIPATED?

The unique challenges presented by Flagship missions make it extremely difficult to fully predict cost and schedule given that the technical advances needed to meet performance requirements are unprecedented. Flagships are typically complex and demanding in terms of scale, teaming arrangements, priority, and novel technology. Often they involve technological advances or new applications of technologies, new processing, and unique manufacturing. [47] Although the projects are planned to initial estimates, delays cause the cost to meet schedule to escalate, and therefore require more funding on an annual basis than was requested. Limiting the funding in the early years can cause further growth in later years while work is deferred and the schedule is stretched even more. This developmental difficulty is consistent with other government agencies as noted by the General Accounting Office (GAO) which has identified similar issues with Department of Energy (DoE) large-scale program implementation. [48]

For the case studies identified in this paper, estimating the cost and schedule for these missions would have been extremely challenging. Due to the unprecedented nature of Flagship missions, there is no comparable cost to use as an analogous estimate. The development schedule for technology development is also uncertain, leading to a large uncertainty in the final launch schedule. In addition, early in their lifetime, the design of Flagships is not fully known so it is difficult to assess the cost of a "moving target" as the design evolves. The design trades and options are numerous and indeterminate through early concept development and preliminary designs as technologies mature. The final cost of Flagships missions really cannot be fully baselined until after the technology development is complete and the design has fully matured which is typically after CDR.

6. A POTENTIAL NEW APPROACH

A proposed approach to developing Flagship missions should help eliminate some of the historical issues in Flagship development. This approach would ensure that a programmatic baseline is established after both the technology and design have matured such that an accurate estimate could be developed. It has been shown that a policy that sets the programmatic baseline after a mature design had been developed, provides the ability to manage the program to cost. [49]

The proposed process for developing future Flagship missions is shown in Figure 10 and consists of the following:

- Step 1: Conduct a science assessment and concept feasibility study to determine the value of the science and define technology challenges.
- Step 2: Fund technologies to TRL 6 with defined pass/fail gates for each technology where the phase is open ended with a consistent level of technology funding until technologies pass the required TRL gate.
- Step 3: Begin an open-ended Phase B to mature the whole system concept to TRL 6 by PDR, include prototyping of manufacturing and test activities.
- Step 4: Agree to a not-to-exceed annual funding level that continues until a prototype is complete (Step 6).
- Step 5: After the technology development phase is complete, develop a prototype of the system to work out implementation issues to know the scope of work going forward.

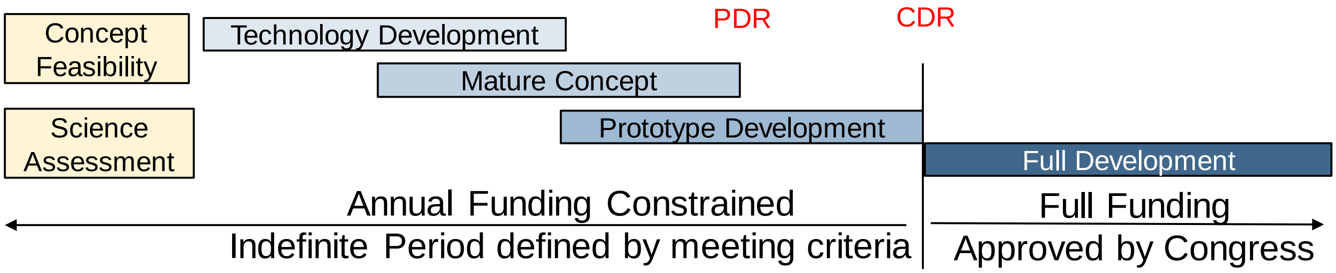


Figure 10. Proposed Approach to Developing and Funding Flagship Missions

- Step 6: As prototype development is nearing completion, provide a realistic estimate of the scope of work ahead using CDR as the gate for continuation.
- Step 7: Get Congressional approval for all remaining development funds which is similar to working capital funds for the U.S. Navy for aircraft carrier procurement.
- Step 8: Conduct Phase C/D as typical, holding the Systems Integration Review (SIR), Pre-Environmental Review (PER), Pre-Ship Review (PSR), etc., with lower level peer reviews as needed.

Step 7 requires a unique approach where Congress approves the full funding for the remaining cost of the Flagship mission once the technology development is complete, the concept is mature and a prototype is developed. This approach helps to avoid the restriction of annual funding changes during the final part of development and is analogous to the approach that the U.S. Navy acquires aircraft carriers in terms of no year funding. [50] No year funding provides for a one-time allocation of funding approved by Congress that can be used as needed over several fiscal years. Similar funding mechanisms are implemented by the U.S. Air Force, U.S. Army, and other agencies. [51] This approach is also consistent with GAO's recommendation that DoE utilize a similar mechanism for its large-scale projects. [52] Consistent with GAO's recommendations for DoE, a capital account or no year funding appropriation could lower the uncertainty of future funding inherent in the incremental funding process for future NASA large scale, strategic mission developments. Such a mechanism would also enable these missions to be more appropriately managed to the programmatic baseline total life cycle cost, and not be subject to variations in annual budgets. Per the Office of Management and Budget's (OMB) Circular A-11, full funding means that appropriations are, "enacted that are sufficient in total to complete a useful segment of a capital project before any obligation may be incurred for that segment." [53] This approach is not unprecedented in NASA as Congress authorized full funding for the Space Shuttle orbiter Endeavour (OV-105) in August 1987 as a replacement for the Space Shuttle orbiter Challenger. [54]

The steps outlined should allow a mature system to be developed before a programmatic baseline is established, while fully funding the mission allows for maximum flexibility with limited interruptions due to annual funding constraints.

To test this theory, the authors used The Aerospace Corporation developed Sand Chart Tool (SCT). SCT is a probabilistic simulation of budgets and costs that simulates a program's strategic response to internal or external events that cause cost and schedule to grow and was developed to assess the effect of potential overruns on a portfolio of missions. [55] It includes a series of penalties, based on historical data, when projects need to be shifted, stretched, or have funding reprogrammed given restrictions in annual budgets. It has been used in several cases including the assessment of a new, instrument first, science mission acquisition approach and to assess the appropriate budgeting and funding confidence level for different types of program portfolios. [56, 57] SCT can also be run for a single mission to assess how these penalties can affect cost overruns due to standing army cost and the inefficiencies of schedule stretches due to a non-optimal profile.

SCT was used for a fictitious Flagship mission to identify if the proposed different acquisition strategy could be more effective. For the analysis conducted, two cases were developed: Case #1, which represents the traditional acquisition approach, and Case #2, which represents the new Flagship acquisition approach identified above. Each case initiates a Flagship mission in the year 2020, with Case #1 baselining a $4B FY20$ mission that will take 10 years to develop. Unfortunately, this estimate is premature, given all the unknowns in the requirements, design, and technology such that the baseline funding is underestimated. Figure 11 shows the average of the simulation runs for the 1) original planned funding, 2) adjusted, unpenalized funding that would be needed if the budget was unconstrained, and 3) observed, penalized funding profile given the annual budget limit imposed by the project.

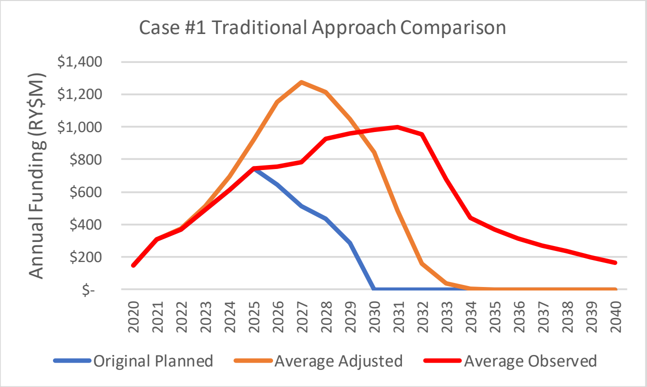


Figure 11. Case #1 Traditional Funding Comparison

Case #2, however, has a similar funding profile in the first 2 years but levels off at a fixed annual funding level, as described in Step 4 above, until the first six steps are executed and the project faces a CDR after a prototype is built. Given that the there is a much greater understanding of the baseline and all technology development and manufacturing challenges have been addressed, the baseline estimate of $7B over 14 years is more mature, and there is less uncertainty in the estimate. In addition, the full funding approach allows for the funding to be provided in a timely fashion. Figure 12 shows the results of the new approach and how the greater certainty in the estimate leads to fewer deviations from the plan and less cost and schedule growth due to inefficiencies imposed by annual funding constraints.

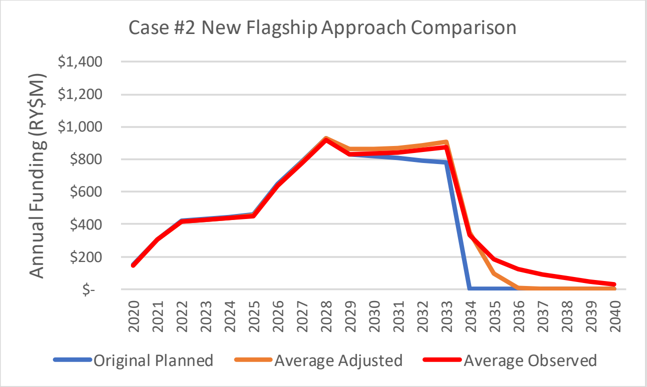


Figure 12. Case #2 New Approach Funding Comparison

Figure 13 shows a comparison of the average observed funding from the simulation between the traditional, Case #1 funding and the new, Case #2 funding. Notice that the Case #2 funding hits an agreed upon plateau, approximately $400M per year, then remains at that level until a prototype is built. Case #1 shows that additional funding is spent, although not as productively, as the initially under-scoped activities are stretched to fit annual funding constraints. These penalties compound and are paid off over the life of the project resulting in a greater cost and a longer development schedule.

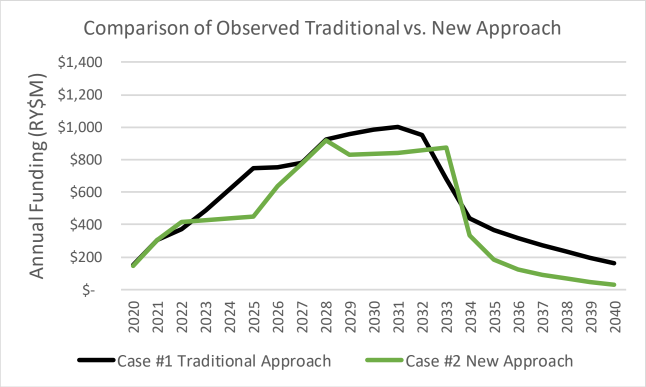


Figure 13. Case #1 and #2 Observed Funding Comparison

A comparison of the planned vs. average observed launch date and average total cost for each case provides additional insight. Tables 2 and 3 provide a summary of the planned vs. observed results for the two cases. Notice that the traditional Case #1 in Table 2 has significant overruns in both cost and schedule, which are consistent with a traditional, Flagship-like acquisition, whereas the new, Case #2 approach shows a minimal delay and minimal cost overrun. In addition, the absolute cost of Case #2 is less, and the launch date is earlier, than the Case #1 traditional approach. The simulation results show that having a more robust estimate with less uncertainty obtained by maturing the design and building a prototype prior to making a full funding commitment can lead to a less costly mission with an earlier launch date than the traditional approach.

Table 2. Planned vs. Observed Comparison for Case #1

Case #1 Traditional	Original Planned	Simulation Observed
Launch Date	March 2029	May 2035
Cost (FY$20)	$4.0B	$9.5B

Table 3. Planned vs. Observed Comparison for Case #2

Case #2 New Approach	Original Planned	Simulation Observed
Launch Date	March 2033	January 2034
Cost (FY$20)	$7.0B	$7.7B

The key to the new approach is to convince all stakeholders to approve funding upfront, $2B in the case of the example shown, for a mission that may be too expensive for the country to afford. This approach does, however, provide a mechanism for all stakeholders to consciously approve or cancel a mission based on a final cost that is much more known than has been traditionally. Although the cancellation risks may be high with this new approach, there is also benefit in terms of the more robust cost-benefit trade that can be conducted by industry, academia, OMB, Congress and the public given the cost certainty. What the project risks in cancellation, it gains later in advocacy when a firm, rationale decision is made based on the perceived value of the mission and its true cost and schedule.

7. SUMMARY

NASA Flagship missions are unique in terms of their consistent attempt to push the boundaries of scientific discoveries by orders of magnitude above previous missions. As such, they provide substantial benefits to the science community as well as to the prestige of NASA. This challenge typically requires technology and engineering developments that are often first-of-a-kind such that predicting the cost and schedule of these missions is difficult. Because of these unique circumstances, the approach to developing NASA Flagship missions should be unique. The paper proposed a way in which annual funding is provided in the early stages of development, to cover feasibility studies, technology developments and prototype development, before fully funding the Flagship mission for the remaining development. The proposed approach should allow for a full assessment of the benefits of a given Flagship mission while having a firm grasp on the cost prior to fully committing to the mission.

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ACRONYMS

AIM	Astrometric Interferometry Mission
ASIC	Application-Specific Integrated Circuit
CAD/CAM	Computer-Aided Design & Manufacturing
CATE	Cost and Technical Evaluation
CCD	Charge Coupled Device
CDR	Critical Design Review
DoE	Department of Energy
FY	Fiscal Year
HabEx	Habitable Exoplanet
HST	Hubble Space Telescope
ICRP	Independent Comprehensive Review Panel
GAO	General Accounting Office
I&T	Integration and Test
JWST	James Webb Space Telescope
LHC	Large Hadron Collider
LUVOIR	Large Ultraviolet/Optical/Infra-Red
MER	Mars Exploration Rover
MIR	Mid-Infrared
MIRI	Mid-Infrared Instrument
MSL	Mars Science Laboratory
NASA	National Aeronautics and Space Administration
NGST	Next Generation Space Telescope
NIR	Near Infrared
OMB	Office of Management and Budget
OST	Origins Space Telescope
PDR	Preliminary Design Review
PER	Pre-Environmental Review
PSR	Pre-Ship Review
RY	Real Year
SAM	Sample Analysis Mars
SCT	Sand Chart Tool
SIM	Space Interferometry Mission
SSC	Superconducting Super Collider
SIR	System Integration Review
TNAR	Technology Non-Advocate Review
TRL	Technology Readiness Level
WFS&C	Wavefront Sensing & Control

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BIOGRAPHY

Robert Bitten *is a principal engineer at The Aerospace Corporation and has conducted independent cost estimates for NASA proposal evaluations and independent assessments for a variety of different NASA missions and organizations. He is a winner of The Aerospace Corporation's President's Award for his effort in assessing the cost effectiveness of different alternatives in the Hubble Space Telescope Remote Servicing Module Analysis of Alternatives. He also won the 2007 NASA Cost Estimating Support Contractor of the Year Award that is awarded to recognize an individual who has provided "outstanding contractor support to the NASA cost estimating community and significantly contributed to the field of cost estimating." He has a Bachelor's in Industrial and Systems Engineering (B.I.E) from the Georgia Institute of Technology and a M.B.A. from Pepperdine University.*

Steve Shinn *is the Chief Financial Officer for NASA's Goddard Space Flight Center. In this role, he is responsible for overseeing the entire range of budget planning and execution, financial management, and financial system activities at the Center. He manages GSFC's budget and financial operations, directs the preparation and submission of annual financial and budgetary reports, and coordinates agency financial management activities with other federal agencies. Before accepting this position in 2016, Mr. Shinn served as deputy director for planning & business management for GSFC's Flight Projects Directorate, a position he held beginning May 2011. He managed all matters related to business, PP&C, resource management, organizational staffing, workforce development, diversity and equal opportunity, and physical assets. He is also an instructor in PP&C at The Johns Hopkins University Whiting School of Engineering. He was awarded NASA's Agency Honor Award and Robert H. Goddard Award for Leadership for* *his efforts in leading business change. He was also awarded NASA's Cost Estimating Award for Leadership and has received two team awards for Quality and Process Improvement for initiatives he championed and led for business change and risk analysis.*

***Dr. Debra Emmons** is the General Manager of the Communication Technologies and Engineering Division at The Aerospace Corporation and is an assistant general manager at The Aerospace Corporation where she is responsible for program development and formulation for NASA and other Civil customers. Dr. Emmons has supported numerous science and exploration NASA project and program studies. Dr. Emmons was also one of the architects for a programmatic assessment tool which allows the evaluation of interdependencies of budget and schedule for NASA Headquarters. Dr. Emmons is a two-time winner of the President's Achievement Award, which she shared in 2006 and 2010, for her efforts on the Hubble Space Telescope Remote Servicing Module Analysis of Alternatives, and the Review of Human Space Flight Committee support (aka Augustine Panel), respectively. Prior to joining The Aerospace Corporation, Dr. Emmons worked at Hughes Space and Communications Company where she was a systems engineering project manager on a commercial telecommunications satellite development. Dr. Emmons has B.S.E.E. and M.S.E.E. degrees from Cornell University, an M.B.A. degree from the Imperial College of London, and a Ph.D. in Systems Engineering from George Washington University.*