Escaping the Space Acquisition Death Spiral

Part Two of a Three Part Series

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Warning Signs of Impending Program Failure

Program managers must be on guard and prepared to detect the distinct *warning signs* of impending program doom, and take immediate action to confront them. They are:

- Failed systems engineering.
- Unrealistic funding realities, including incomplete budgets or volitile program funding.
- Unreasonably pushing the technology envelope, with unstable requirements.
- Overly optimistic planning estimates, with weak program cost and schedule reserves.
- Launch vehicle selection driving program complexity.
- Unreasonable "sunk-cost" arguments.
- Government/customer is not acting and thinking strategically.
- Faltering industrial base.
- Stunts being used as a substitute for mission value.

The manifestation of these systems engineering and process failure warning signs have a tendency to aggregate and compound to create the space program acquisition "death spiral," driving the overall program to failure, as graphically depicted in Figure 1. Program managers, corporate brass, agency heads, and legislative sponsors must therefore work their hardest to prevent their space programs from falling into a "death spiral," refusing to let the factors that manifest the spiral auger in by confronting them head-on.

Failed systems engineering. Successful systems engineering requires robust and proactive risk management. A project team must know the program's requirements, and be able to engineer each spacecraft and its supporting systems to meet mission requirements.

Managing (and owning) risk is critical to systems engineering success. To minimize problems, Tony Spear, in his *NASA FBC (Faster-Better-Cheaper) Task Final Report*, recommends: vigorous system and subsystems engineering to establish standards; continuous, rigorous risk assessments and mitigation throughout development and operations; balanced use of available and advanced technologies; and established metrics for mission risk and technical, cost, and schedule performance.[1] While Spear's systems engineering recommendations are well-considered, performing these tasks on large satellite system programs is often an insurmountable task, given the breadth of the challenge. The necessary risk management is more than just another reporting buzzword. The team must fully comprehend the program's risks. Program managers must be given the authority needed to match their accountability. Unfortunately, their decisions are often second-guessed by a myriad of senior management reviews, many by non-decision makers. As a consequence, managers for important satellite programs often are unable to effect changes on even minor issues without political preparation of senior leaders, lest their decision be questioned by the latter.

Space systems, even quick and dirty small-sized ones are not easily engineered and built. There will always be cost, technical, schedule, and programmatic risk. A program manager steeped in systems engineering practices understands that making changes to a program baseline tends to be very expensive. But it is easy to fall into a trap of making changes to a technical baseline, describing such moves in comforting terms as *improvements*, usually taken in the name of providing *system flexibility*. Such changes have the unfortunate potential to disrupt

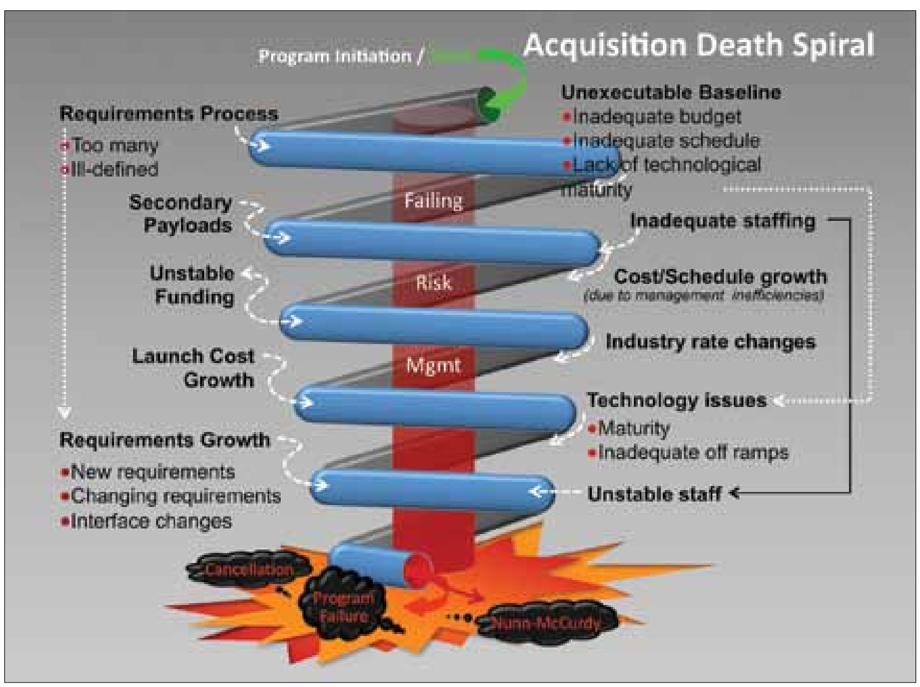

Figure 1. The Space Acquisition Death Spiral.

and cripple large programs by imposing huge cost penalties. Prudent risks must be taken, and program managers must prioritize them and *understand* their impacts on the mission. Once risks are understood, managers must build a plan to mitigate or handle them in event they materialize. They must review early and often the risks and their risk mitigation plan.

Systems engineering, if accomplished properly, can detect technical problems early, and provide insight to more easily resolve them. General Bernard Schriever and his engineering management teams demonstrated these points during the earliest days of the development of US military space. Solving a problem early saves the significant cost and reduces the complexity of extricating oneself from a problem that has endured and spread. However, in the days of NASA's recent FBC experiments, engineering funding was an easy target, and engineering development expertise eroded throughout programs of that era, sometimes with catastrophic results. Problems never get better with time, and failing to find and highlight problems early has become a challenge of re-capturing the vigorous and robust systems engineering of the past.[2]

Despite the seriousness of technical challenges, systems engineering efforts are often not valued by management, even though performing such a function is nearly always vital to program success, and despite the fact that systems of system engineering is a new and growing specialty. It turns out that it is all too easy for managers to just send their systems engineers to program meetings to take action items rather than lead challenges to every requirement, assumption, constraint, ground rule, and the like, or provide real tradeoff and cost/benefit analyses. Systems engineers must be allowed to perform these essential functions. Unfortunately, many acquisition managers are not chosen for their systems engineering acumen—master degrees in business administration, accountants, lawyers, advertising executives, and other non-engineers and non-scientists are invited to take up vital management positions as they wait their turn to enter the corporate boardroom. Instead, these same managers have learned that their most valued functions are often to *win the deal, show corporate leadership*, or just manage the customer, and often are not wise enough to engage quality systems engineers as a foundation to the program's engineering.

Failure to properly engage software engineers early in the program can lead to serious problems, even when the hardware approaches are working out rather smoothly. Since the final software system cannot be delivered until the hardware is in place, the software is usually the last thing delivered. And when poorly resourced and not involved early in the program, problems inevitably occur. As the problems occur, software system resources are tapped as a funding source. Eventually, this bill must be paid. Finally, as the program's hardware suffers performance issues during development, subtle increases in the software requirements and complexity may be needed to cover these issues, further complicating software program delivery. Such late software problems can derail hardware engineering successes, and many such failures have occurred over the decades. In the 1980s, the Inertial Upper Stage Program was disrupted by late-blooming software problems after hardware solutions for the solid rocket motor system were set. And, even today, after a decade and a half of work, space based infrared system (SBIRS) struggles with nagging and complex software problems.

Recognizing the need for effective systems engineering, a variety of Federally Funded Research and Development Centers (FFRDCs) have been engaged to assist the US Government as it acquires space systems, arguably providing important historical expertise and depth to help contain costs, achieve savings, and perform smart technical decision making. But, with so many large national security space programs spinning out of control, one wonders whether the taxpayer is getting his or her money's worth for these expenditures. The taxpayer is not. RAND highlighted this problem at the Air Force's Space and Missile Systems Center (SMC) when it offered the following critique of its FFRDC support: "The cost-estimating process was organizationally too closely associated with bureaucratic interests that held advocacy positions, making independent, disinterested cost analysis more difficult." RAND Corporation concluded that "[i]nadequate cost-estimation and risk-assessment methods and models were used (on SBIRS and GPS)."[3] Confirming the authors' own observations and their discussions with senior leaders in government and industry, RAND described SMC's disappointing FFRDC cost estimation support as being held hostage by bureaucracies and organizational funding politics:

> The ability to generate realistic and credible cost estimates … appears to have been negatively affected by the dominant influence exercised by program offices over the cost-estimating process and by the failure of senior Air Force leadership to insist on objective cost and technology risk assessments because of budgetary or other institutional reasons.
>
> Many of the institutional challenges that we identified as having apparently influenced SMC cost analysts … are closely linked to budgetary politics and the competition among program offices and among the services for scarce acquisition dollars. Program offices competing for funding tend to lose objectivity and become advocates for specific systems, as do the services when encouraged by Department of Defense to form joint programs. As one interviewee summed up the problem in a rather oversimplified but dramatic manner, "program managers generally will not allow realistic cost estimates of their programs because the result could be loss of budget or program termination."[4]

In a related US Government Accountability Office (GAO) analysis, space program cost estimates were found to be unreliable, largely because requirements are not being fully defined and programs start with too many unknowns about technologies.[5] Similarly, RAND observes significant cost estimating problems have arisen out of programs that have become more sophisticated and complex:

> The databases used in traditional space cost-estimating models became increasingly obsolete in this environment, and acquisition reform measures eliminated much of the routine collection of new cost and technical data that had been common in the past. The increased technological complexity of proposed new military space systems challenged the technical and system engineering knowledge of the government acquisition officials

tasked with the job of assessing proposals and contractor progress—at a time when the acquisition workforce was experiencing significant downsizing."[6]

Late science and technology (S&T) spending to address technical challenges generates considerable risk. Satellite acquirers often start programs much too early, before needed technologies are ready to deploy on a spacecraft or in its support systems. Managers cheat their programs by working S&T as a programmatic "risk reduction" activity, rather than waiting for a needed technology to mature. If these technologies were developed with on-ramps into the program with an achievable, but maybe not as capable, baseline, a manager could design his or her program to field working systems, on-cost and schedule, and provide for upgrades in later blocks. However, the current practice is to select these technologies as a baseline without planning for off-ramps in event they fail. In addition, cost and schedule reserves to work around inevitable issues that arise with developing new technologies are not put in place.

As demonstrated by recent program failures, allowing technology development to carry over into system development dramatically increases the risk that significant problems will be discovered late. Solving problems at that juncture often requires significant time, money, and effort. Furthermore, where there are critical technology unknowns, a program cannot reliably estimate the resources needed to complete a program.[7] The negative consequences of all this "just-in-time" thinking has become all too real as space program after program has busted its budget due to one technology issue or another.

Of course, some argue space acquisition efforts should be leading edge activities, pushing the proverbial "envelope." These optimists argue that technology readiness levels (TRL) can be lower, and the latest and very best just-in-time technologies should be used. Unfortunately, planning acquisitions with overly ambitious TRLs or imagined technologies without adequate on- or off-ramps to add or remove planned technologies to the program, often devastate the engineering of complex space systems, as demonstrated by the SBIRS and doomed future imagery architecture (FIA) efforts. Current spending problems are directly "attributable to programs starting before they have assurance that capabilities being pursued can be achieved within available resources and time constraints."[8] Technology weaknesses are thus causing problems to a whole host of DoD acquisition efforts. The GAO linked the deficiencies to failed contract management weaknesses as follows: "[U]nstable requirements and immature technologies are the most significant contributors to cost and schedule increases, and not just for space acquisitions but all DoD weapons acquisitions."[9]

DoD cost estimates for space systems are consistently optimistic. These problems are rooted in "the failure to match the customer's needs with the developer's resources—technical knowledge, timing, and funding—when starting product development. In other words, commitments were made to achieving certain capabilities without knowing whether technologies and/or designs being pursued could really work as intended. Time and costs were consistently underestimated."[10]

Initially, it may be less exciting to develop a program based on a known path with modest technology on-ramps arranged for technology insertion as it matures and with conservative cost estimates. But such programs are more likely to achieve success than planning for one that selects a high-risk path technology solution to solve its budgeting problems.

The GAO also reports the technical and cost problems have been exacerbated by political wrangling:

> [S]pace systems may suffer from more requirements pressures than other weapon systems because there is usually a very broad constituency—contractors, military services, civilian users, administrations, and Congress—behind each satellite program. This creates challenges in making tough tradeoff decisions.[11]

Unrealistic funding realities. In the current competitive environment, where programs are considered do-or-die for a company, program managers and business executives are under extreme pressure. Large flagship spacecraft programs are not easily funded, and competitions are not easily won. Some might "underestimate costs and over-promise capability, and [create] a host of negative incentives and pressures" in order to win.[12] A space program manager' success often is measured in terms of his or her ability to obtain continued funding and team employment, not in achieving mission success.

The pressures to win are longstanding. Acquisition efforts can extend for decades. The space community covets these funding streams. Once started, it also is extremely difficult to cancel a program—often due to significant political pressures among government agencies, legislators, industry, and other interested parties who serve as patrons for each project. For example, the Defense Support Program and a number of its proposed follow-on programs, such as SBIRS, have been around since the 1960s this stream of funding has nursed a generation of steady engineering and program jobs and large numbers of space professionals.[13]

These realities encourage US government agencies to start more programs than they can afford. Why? Programs and their funding beget stability and success for an agency, contractors, and their political sponsors. Managers often try to leverage separate interests by inserting new requirements into related, large program baselines to secure funding for them. As a result, demands or suggestions to change requirements on large systems tend to grow over the life of a long-duration program. The requirements levied for these associated programs have been substantial and difficult to satisfy.

Several negative consequences arise from all of this:

- Because programs are funded annually and priorities have not been established, competition for funding continues over time, forcing programs to view success as the ability to secure the next installment rather than the end goal of delivering capabilities when and as promised.
- Concurrently, when faced with lower budgets, senior executives within the Office of the Secretary of Defense and the Air Force would rather make across-the-board cuts to

all space programs than hard decisions as to which ones to keep and which ones to cancel or cut back.
- Having to continually "sell" a program creates incentives to suppress bad news about a program's status and avoid activities that uncover bad news.
- When combined with the high cost of launching demonstrators into space, the competition for funding often encourages programs to avoid testing technologies in space before acquisition programs are started.[14]

The funding and political challenges within the US government that affect efforts to acquire space systems can be seen in how NASA has chosen to deal with Mars Science Laboratory (MSL) expenditures. NASA's leadership has chosen to "do what it takes to fund the rover. Many NASA science projects … have taken a small hit to pay for the overruns."[15] The agency now tries to spread the pain, where all funding is fair game to pay for the problems.[16] Acquisitions that enter the confluence of these funding and political pressures and their masters have been cancelled, then reinstated after NASA received significant backlash for imposing the cuts.[17]

Unreasonably pushing the technology envelope with unstable requirements. Competitions to start a new program are intense. They foster strong incentives for competitors to make a system stand out compared to existing or alternative systems. Funding constraints place a high priority on making a program appear affordable. Program sponsors are encouraged, therefore, to submit cost estimates that will fit within funding constraints. These limitations generate incredible pressures, forcing program managers to propose using exotic leap-ahead technologies as a solution. This, then, places incredible strains on programs. "Instead of forcing trade-offs, challenging performance requirements—when coupled with other constraints, such as cost or the weight of the satellite—can drive product developers to pursue exotic solutions and technologies that, in theory, can do it all."[18] Such was the case with the FIA acquisition.

Echoing this point, the GAO has offered the following observations:

> Space acquisition programs have historically attempted to satisfy all requirements in a single step, regardless of the design challenge or the maturity of technologies to achieve the full capability. There is a variety of reasons for this, including a desire to include the most advanced technologies onboard satellites, particularly in view of the length of time it takes to develop space systems. However, this approach invariably increases the technology challenges facing programs, and thus, the risk that costly problems will be encountered.[19]

The need and desire to stand apart from the crowd afflicted the MSL program. The NASA Mars Exploration Program advertised MSL as "the most challenging planetary mission that's ever been flown… pushing the envelope in a number of areas, and it just kind of built up."[20] Indeed, the NASA Mars rovers, Spirit and Opportunity, were designed to look only for water. In contrast, the MSL program team has undertaken a Herculean task—developing a system that can search for the molecules considered to be precursors to life and for evidence of microbes at work. These mission requirements demand a large machine that relies on nuclear power, rather than what the first rovers used—solar panels. In addition, the MSL will carry a full chemistry workshop and a robotic drill arm for gathering rock samples.[21] All of this is terribly challenging and has created intractable frustrations. Even NASA concedes the point, that it "underestimated what it was going to take." To do MSL right, the agency inevitably will "need more funding."[22]

Overly optimistic planning estimates. Nearly all space professoinals have worked on one or more programs that have habitually slipped their schedules. The authors worked together on one program that was four years from launch when they started. Yet, when they left the program, years later, launch was scheduled to occur more than four years later. Acceptance of delay and problems on programs such as SBIRS, Advanced Extremely High Frequency Satellite, James Webb Space Telescope (JWST), or MSL, however, is a luxury the space community is fast losing. Schedule slips are driving cost growth, and these trends must be reversed.

Successful space acquisition efforts need adequate budgets with program reserves and executable schedules with margin. However, there is a recent maddening tendency to drastically limit programmatic reserves and margin, handcuffing a program manager from a program's start. In addition the budget process tends to preclude long-term and multi-year funding baselines. Further complicating the challenges, large space programs face new and dramatically increased scrutiny and oversight, especially given recent troubling cost and program cancellation trends. Adding insult to injury, the increased scrutiny invites more bureaucratic oversight and overhead expense that programs can ill afford.

As a counterintuitive consequence of optimistic planning, spacecraft and constellations developed under these pressures are often out-of-date even before their first launch. As noted by Paul Brooks, Surrey Satellite Technology Limited's vice president of sales:

> When a satellite is being designed the owners look for ways to extend its mission. The designers then put more payloads on the spacecraft to deliver more value, but then the cost goes up… This creates more financial risk which then requires greater assurance that everything will work as planned. The greater assurance lengthens the lead time. You ultimately end up with very large missions and by the time the payload is launched, it is out of date. We noticed that this pattern repeated itself in the satellite industry and, unlike other technology-driven markets, there weren't huge increases in performance and large decreases in cost. We believe that Moore's Law should apply to spacecraft as well.[23]

Launch vehicle selection drives program complexity and launch rates drive launch vehicle economics. Spacelift is very expensive, and must be, despite the dreams and longing of program managers and science fiction buffs. That's too bad, but all too real; the reality must not be ignored. The costs of sustaining a standing army and fixed infrastructure at launch sites are substantial. There are absolute costs associated with purchasing propellants and other expendables needed to safely lift systems to orbit. These always will be substantial given the chemistry involved with rocket propellants. Fabrication of launch vehicles also is a complex task, where even small errors can cause catastrophic failure. This demands rigorous engineering discipline and time. Fabrication should only be performed by highly skilled and trained space professionals. Mixing, pouring and forming a solid-rocket motor is still an art, one mastered only by accomplished virtuosos of chemistry and engineering.

Some argue these expenses could be amortized over a large number of space launches, to achieve economies of scales. But that only can happen, if at all, when there is a need for many launches. Even though the false prophets for regular and ready access to space would wish differently, there is no such need.

This is not a new issue. After the Mercury, Gemini, and Apollo program successes during the 1960s and early 1970s, NASA leadership desperately huddled to identify and develop missions to continue and expand what has turned out to be its half-century Holy Grail—the manned spaceflight mission. As a result, NASA moved aggressively to develop the reusable, manned space transportation system or space shuttle during the 1970s to transport crew members and payloads into low Earth orbits (LEO). The system originally was billed as one that would launch spacecraft once a week and reap low launch costs through amortization of a fixed launch infrastructure, standardized launch processes, and reusable space vehicles and solid-rocket boosters. As one would expect, the shuttle's development costs were advertised as being recouped through frequent access to space. Plans, concepts of operations, and procedures detailed probable attainment of these launch rates. The ambitious claims of efficiency and technical prowess were made in a conscious effort to secure funding from the US Congress.[24]

Of course, NASA over-promised the shuttle economies of scale.

Originally described as operating somewhat like a commercial airliner, with a big-rig, tractor-trailer shaped cargo bay, NASA boasted that after landing, the orbiter would be checked out, and then "mated" to the rest of the launch stack (the external tank and solid rocket boosters), and stand ready for launch within only a few weeks. This ground processing turned out to take months. Attempting to fuse a reusable vehicle with 1970s technology drove a number of design compromises. Complexity was inevitable. This added weight, which in turn reduced payload capacity and constrained launch and de-orbit options. Ceramic tiles were selected to shield the orbiter from heat on reentry and save weight. This engineering innovation generated a logistics nightmare, because the tiles turned out to be very expensive and time-consuming to repair and replace. Furthermore, the shuttle system had to be man-rated, certified as safe for astronauts to launch, work on-orbit, and return safely to Earth. Each aspect of the shuttle's complex hardware systems had to function perfectly, because there were no survivable abort modes in many of the failure scenarios. This all generated the requirement that the system be carefully inspected before each flight, which conflicted with any goal to launch payloads cheaply and quickly.

Even NASA's famed can-do bureaucracy became part of the shuttle's problems and led to the system's 1986 *Challenger* launch and 2003 *Columbia* reentry catastrophes:

> While the technical details of the accidents are quite different, the organizational problems show remarkable similarities. In both cases events happened which were not planned for or anticipated. In both cases, engineers were greatly concerned about possible problems but these concerns were not properly communicated to or understood by senior NASA managers. In both cases the vehicle gave ample warning beforehand of abnormal problems. A heavily layered, procedure-oriented bureaucratic structure inhibited necessary communication and action. In both cases a mind set among senior managers developed that concerns had to be objectively proven rather than simply suspected.[25]

Some proponents of manned space flight optimistically claimed the system would fail only once in 10,000 launches, at a time when the best US launch system was failing during four percent of its launches. The first major failure of the shuttle system occurred on its 25th launch, resulting in a 4 percent failure rate. NASA worked hard to resolve the shuttle's bureaucratic, logistical, and engineering nightmares; but the concept of weekly, even monthly, missions proved nearly impossible. When flights were halted pending lengthy safety reviews after each mission loss or significant technical problem, launch campaigns further slowed.[26] The design problems and resulting catastrophes induced NASA managers to restructure system operations and sustainment practices. This, in turn, imposed a high labor cost on NASA's operations, requiring tens of thousands of workers to continue operations; labor costs spiraled upwards totaling about $1 billion per year.[27] Unfortunately, even though the shuttle and its heroic crews have proven able to achieve all the originally identified experiment, lift, and on-orbit repair objectives, the system just was too expensive and complex.

Justification for funding the manned space shuttle space transportation system, the evolving expendable launch vehicle (EELV), even more recent spacelift systems, have been rooted in themes now shouted out by present-day operationally responsive space (ORS) carnival barkers—enhanced flexibility and significant savings in systems costs and time. Unfortunately, cost savings and responsiveness for expendable ORS systems have not been achieved, and the sought-after combination of flexibility and savings has not been found. Even relatively small launchers have their cost, reliability, and schedule issues. As noted by industry analyst Jeff Foust, "While Pegasus and Taurus were designed for relatively high launch rates and launches on short notice; in practice these attributes have not been utilized."[28]

"Orbital [Sciences Corporation] designed the Pegasus launch vehicle to carry out one to two launches a month…with a surge rate of one a week. The Taurus, meanwhile, was designed to launch on a plat pad on just eight days' notice."[29] But, as of May 2011, the Pegasus has only flown 40 times, 37 successfully, since a first flight in 1990.[30] Taurus has conducted nine launches, six successfully, since 1994, and failures in three of the last four launches.[31] Fairing separation problems have been cited as reasons for failures on two of the launches which involved two Earth monitoring missions: the 2009 $273 million Orbiting Carbon Observatory and the 2011 $424 million Glory climate change monitoring satellite missions.[32] The economies of scale needed by the Pegasus and Taurus systems for flexibility and savings can never be achieved with a relatively modest number of launches needed to sustain planned constellations. A study performed in 2001 by the Center for Strategic and Budgetary Assessments showed the Taurus pound-for-pound was the most expensive system to place a satellite in LEO or geosynchronous Earth orbit orbits among the 16 LEO-capable expendable launch vehicles and 10 geosynchronous transfer orbit boosters[33]. Still, "[n]o matter who is the service provider, space launch costs are staggeringly high. Driving down the cost of launch has been the subject of a host of studies and government panels, and the buzzwords of many a corporate business presentation to meetings and seminars."[34]

Reducing costs was also the idea behind the 1990s X-33 and X-34 reusable space launch technology development initiatives. They were cancelled because of technology failures and the associated outlays.

The cost issue for heavy-lift vehicles drove the Air Force from the Titan IV launch system to the EELV.[35] Both the Lockheed/Martin Atlas V and Boeing Delta IV EELV systems have completed successful launches. The two product lines are tremendous engineering marvels, and they have delivered lower total launch costs per pound than did the Titan IV system they replaced. The original business plan for both systems, however, depended unrealistically on a thriving commercial demand for launch services. Low launch rates adversely affect costs, especially since subcontractors supporting the EELV systems produce scarce components on an annual base. And, there are tremendous inefficiencies in carrying production lines, people and facilities capable of handling much higher launch rates. The EELV programs did meet some of their overarching cost-reduction goals by simplifying launch processing and payload interfaces, but the two vendors did not capture enough of the commercial market to meet projected reductions in life-cycle costs. Consequently, the Air Force and industry shareholders were induced to invest billions to develop two new families of launch vehicles that will largely be used for just US government national security and civil space payloads. This has resulted in higher overhead costs to the DoD, and the US has lost an opportunity to maximize the competitive posture of its domestic industry. Eventually, given the ongoing cost challenges, the Atlas and Delta lines were consolidated into a joint venture: the United Launch Alliance.

"Sunk-cost" arguments - "We are already in too deep to stop now." When the International Space Station (ISS) is completed in 2011, total investment in the system will exceed $125 billion. The Bush administration's Vision for Space Exploration had outlined using the ISS for five years after its completion as a test-bed for exploration and then allowing the program to come to an end. However, the Augustine Committee has now made the argument that such an investment in ISS should not be wasted. Partner nations in the ISS are endorsing the idea of continuing the program. If the recommendations are fully implemented, operations for the ISS will continue for at least 10 more years, until 2020 and beyond, instead of the five years originally planned. The problem is that to maintain and operate the ISS costs the US approximately $3 billion per year. Over 10 years, that will amount to $30 billion out of the NASA budget.

In evaluating the argument for continuing the program, one must concede the point that the money used to assemble the ISS already has been spent. Since NASA's budget will no doubt be relatively flat for the foreseeable future, this means $3 billion a year has to come at the expense of other priorities like new Earth Science missions. The questions that NASA and the White House need to answer are: Is the $30 billion best spent on the ISS, which to date has not delivered on any of its technical promises? Or, is the $30 billion better spent on something else, perhaps on getting humans out of LEO for other missions? Money can be saved, costs avoided, and funding better targeted on needs and wants if these questions are confronted realistically.

The government is not acting and thinking strategically. Space programs now experience exasperating material failures (parts, subsystems and subassemblies, batteries, solar panels, out-gassing materials, etc.). This is due, in part, to a collapse of the US space industrial base and funding instability. It also results from marginal accountability for poor performance by providers of specific materials.

Furthermore, there is too much oversight on tangential issues, as demonstrated by the 243 "shalls" in the NASA Authorization Bill only 15 of which have to do with money and funding.[36] The Vision for Space Exploration was planned to accomplish an important national mission, but it now is fast evolving into an initiative designed to just protect "jobs." All of these examples are symptomatic of a government failing to act and think strategically.

On FIA, the US government was so desperate to stand up a competitor to Lockheed Martin that it ignored warning signs and obvious flaws in Boeing's proposal and selected it. FIA is a bell-weather of a gathering acquisition "perfect storm." Managers now are asked to architect programs that consciously are non-executable; and cost-plus contracts have not discouraged buy-in bids by industry competitors. Indeed, US space industry competitors legitimately know that if they lose, they are essentially out of business; this forces "death bids" where a contractor finds it is better to be on contract and under bid than on the street.[37] The US government fostered this outcome through its acquisition practices.

Sometimes, US government strategy is to select a qualified, but not the best qualified, bidder in order to preserve the contractor industrial base. During the JWST competition, for example, some believe Northrop Grumman signaled they wanted to get out of the civilian space market. NASA, however, desperately wanted to keep a third large prime in its contractor mix and a serious competitor to Lockheed Martin in large optical telescopes. In the authors' opinion, there was a strong US government bias to select Northrop Grumman. As it turned out, the TRL for the

Northrop Grumman technical solution was too low when bid.

Faltering industrial base. Engineering, science and industrial innovation and excellence have secured the success won by the space industry and its cutting-edge global leadership over most of the past half-century. This success also is linked directly to a vibrant and flexible work force, and to their facilities and programs. Now, as a consequence of the long-term failure of government, industry and academia to act strategically, the US space industrial base suffers from the following effects:

- Shrinking workforce.
- Reductions in new programs.
- Elongation of the development cycles.
- Inadequate societal math/science education and preparation all pose considerable challenges and the dangerous potential to derail future successes.
- A decades-long decline in S&T investment.

In short, we are losing the "bench-strength" needed for program success. While there has been hand-wringing in public forums, these long-term issues are not perceived and have not been treated by industry executives and their corporate money changers as important. Their attention is directed instead to short-term gains and profits; understandably, they focus on keeping a company's stock performing at its best.

Misguided priorities have forced program managers to integrate S&T spending as an integral part of their program plans in an attempt to deploy technologies needed to win a bid or program approval. As noted by the GAO and directly observed by the authors over the last three decades:

> [T]here is a tendency among space system acquisition programs to take on technology development that should occur within the S&T environment. Reasons for this include the greater ability to secure funding for costly technology development within an acquisition program versus a science and technology program, a belief among the acquisition community that labs in charge of developing space technologies do not adequately understand their needs, as well as communication gaps between the S&T and acquisition communities."[38]

The 1990's experiment in Total System Performance Responsibility (TSPR) has compounded the space community's industrial base woes, even though it was, specifically to offset the effect of the DoD's dramatic reduction in force and the devastating loss of its engineering and acquisition talent. The thinking behind TSPR was, as the name implies, to give total system performance responsibility to a contractor. It was hoped this would achieve savings and efficiencies by reducing intrusive government monitoring and letting contractors work to achieve programmatic success. It was argued that TSPR also would give the contractor the flexibility to simplify the integration of all aspects of a program, determine the best resources to get the job done, and reduce costs by eliminating redundant management systems.[39] Unfortunately, some space contractors did not effectively deploy TSPR on several very expensive and important space acquisition programs. They also received the increasing TSPR contractor responsibilities while the DoD was cutting program budgets. As a result, the programs suffered, and valuable experience in working through significant systems engineering challenges was lost by a generation of government program managers and engineers.

The US government, industry, and academia have allowed the US industrial base to languish in other ways. For example, "smart buyers" are hard to find in government; many space acquirers lack significant management or engineering skills and experience. Emblematic of inexperience and lost expertise in its space acquisition, the Air Force teaches the SBIRS debacle as part of its space professional National Security Space Institute and points to management problems as the cause. Unfortunately, very few of its instructors have the experience or expertise to synthesize what to tell students about better management strategies or engineering solutions that could or should have been employed to avoid the program's failures.[40]

Stunts being used as a substitute for mission value. Demonstrations have value. They can provide visual evidence of what can be done or should not be done. For example, the Mercury and Gemini programs demonstrated and validated technologies and operations necessary for success in the Apollo Lunar program.

Demonstrations must be distinguished from *stunts*, tests that look spectacular but prove no usable or scientific point. Stunts often indicate the last gasps of a failing program. One of the authors helped fight a proposed stunt on an Strategic Defense Initiative Office program. The need for a demonstration of the program's technology was questionable. The program also was facing huge cost overruns. That was when some proposed modification of the demonstration to make the overall program appear more thrilling and dramatic. They excitedly proposed putting detectors on a missile linked to a system that would cause the missile to self-destruct when it detected laser illumination. It was hoped the proposed demonstration would look spectacular on video, thereby garnering even more support for a space-based laser system. But, video of the *stunt* would have created a false impression that the technology being demonstrated could somehow be used for missile defense purposes, and the false impression that the illuminator laser actually had destroyed the missile. Clued through side channels to its potential problems, SDIO management wisely deferred, and ultimately declined to endorse and fund the stunt. The SDIO director then moved to curtail the program and effectively cancelled it, saving US taxpayers several hundred million dollars on an effort that was going nowhere fast. These bureaucratic machinations pre-dated and nearly echoed the 1985 M247 Sergeant York DIVAD (Division Air Defense) scandal, where it was alleged system tests had been misrepresented, and that program also was ultimately cancelled.[41]

Similarly, some argue the recent NASA Lunar Crater Observation and Sensing Satellite (LCROSS) mission was a just a public relations stunt. The $79 million mission supposedly was conceived to find evidence of water on the Moon. The mission involved vectoring a two-ton Centaur rocket to hit the floor of a crater near the Lunar south pole. NASA scientists hoped that dust and vapor kicked up by the impact would climb high enough to catch sunlight, allowing a satellite trailing behind the rocket to hunt for traces of Lunar water in the ejected debris. In addition, the Hubble Space Telescope and many Earth-based observers also were recruited to watch for a plume of debris rising from the impact site.[42]

Figure 2. NASA Lunar Crater Observation and Sensing Satellite.

But, the mission was not chosen through NASA's scientific, peer-reviewed selection process that follows the National Research Council's Decadal Surveys. Apparently, it was selected by the NASA Exploration Office only as a means to generate excitement for Lunar exploration. Indeed, during the weeks before the LCROSS mission crashed into the moon, scientists involved with the mission were predicting very little, if anything, would be seen from the impact–despite a well-publicized observation campaign. They argued the mission could not deliver a meaningful result, even if it managed to find evidence for water on the Moon.[43]

Critics contend that the mission generated too little scientific value to justify its price tag and created additional cost and complexity to the more important Lunar Reconnaissance Orbiter (LRO) mission. LCROSS was launched with the LRO system. Others questioned whether the LCROSS mission made any sense at all. According to Paul Spudis of the Lunar and Planetary Institute, "LCROSS was not a sound strategy to pursue if your objective was to answer the question, is there water ice on the moon? And if so, where is it and what is its state?"[44] Spudis contended the mission would not say anything definitive about the moon because it could simply mean that scientists were unlucky in hitting a dry patch. On the other hand, a positive detection of water would not provide any information about the extent or distribution of ice on the Lunar surface, which Spudis said was the point of looking in the first place. "That tells me the fundamental rationale behind the mission was flawed."[45] Spudis argued that a better, albeit more expensive, alternative would be a series of missions culminating in a rover. The rover could move from sunlit to dark regions near the poles to compare Lunar environments and characterize any ice found. "Instead, [NASA] came up with a PR stunt, and it kind of backfired."[46]

Notes:

1 Tony Spear, *NASA FBC Task Final Report*, 31 March 2000, 13, http://klabs.org/richcontent/Reports/fbctask.pdf.

2 Thomas D. Taverney and James D. Rendleman, "Ten Rules for Common Sense Space Acquisition," *High Frontier* 6, no. 1 (November 2009), 62.

3 Obaid Younossi, Mark A. Lorell, et al., *Improving the Cost Estimation of Space Systems: Past Lessons and Future Recommendations*, RAND Corporation, Santa Monica, California: 2008, 72.

4 Ibid., 82.

5 *Space Acquisitions: Stronger Development Practices and Investment Planning Needed to Address Continuing Problems*, GAO-05-891T, 12 July 2005, 8.

6 Younossi et al, *Improving the Cost Estimation of Space Systems*, 96.

7 *Space Acquisitions: Stronger Development Practices…,* GAO-05-891T, 8-9.

8 Ibid., 8.

9 Ibid.

10 *NASA: Projects Need More Discipline Oversight and Management to Address Key Challenges*, GAO-09-436T, 5 March 2009, 5.

11 *Space Acquisitions: Stronger Development Practices…*, GAO-05-891T.

12 Ibid.

13 Follow-on programs to the DSP include the Boost Surveillance and Tracking System (BSTS), Space Surveillance and Tracking System (SSTS), SBIRS-High, SBIRS-Low, Advanced Warning System (AWS), Follow-on Early Warning System (FEWS), and Alert, Locate, and Report Missiles (ALARM).

14 *Space Acquisitions: Stronger Development Practices…*, GAO-05-891T , 9.

15 Traci Watson, "Troubles parallel ambitions in NASA Mars project," *USA Today*, 14 April 2008.

16 According to Edward Weiler: "I'm trying to spread the pain … Everything is fair game." Traci Watson, "Troubles parallel ambitions in NASA Mars project," *USA Today*, 14 April 2008, citing Edward Weiler, acting head of NASA's science division, http://www.usatoday.com/tech/science/space/2008-04-13-mars_N.htm.

17 Dawn's mission to visit a pair of asteroids was reinstated following "a mission review" intended to "help ensure open debate and thorough evaluation of major decisions regarding space exploration and agency operations…" Tariq Malik, "NASA Reinstates Cancelled Asteroid Mission," *Space.com*, 27 March 2006, http://www.space.com/news/060327_dawn_mission.html. "NASA Reinstates Dawn Asteroid Mission," *Space Daily*, 27 March 2006, http://www.spacedaily.com/reports/NASA_Reinstates_Dawn_Asteroid_Mission.html.

18 *Space Acquisitions: Stronger Development Practices…*, GAO-05-891T.

19 Ibid., 8.

20 Traci Watson, "Troubles parallel ambitions…," citing Doug McCuistion, head of NASA's Mars Exploration Program (MEP).

21 Ibid.

22 Ibid., citing project manager, Richard Cook, NASA's Jet Propulsion Laboratory.

23 Greg Berlocher, "Small Satellite Technology: Gains Open Space to More Player," *Via Satellite*, 1 August 2008, http://www.viasatellite.com/via/features/Small-Satellite-Technology-Gains-Open-Space-to-More-Players_23881.html. According to *Webopedia*, the observation made in 1965 by Gordon Moore, co-founder of Intel that the number of transistors per square inch on integrated circuits had doubled every year since the integrated circuit was invented. "Moore predicted this trend would continue for the foreseeable future. In subsequent years, the pace slowed down a bit, but data density has doubled approximately every 18 months, and this is

the current definition of Moore's Law, which Moore himself has blessed. Most experts, including Moore himself, expect Moore's Law to hold for at least another two decades." See "Moore's Law," http://www.webopedia.com/TERM/M/Moores_Law.html.

[24] Mark Lardas, Ian Palmer., *Space Shuttle Launch System 1972-2004*, Osprey Publishing, 7.

[25] See "Space Shuttle history - Shuttle operations," *Century of Flight*, http://www.century-of-flight.net/Aviation%20history/space/Space%20Shuttle%20history.htm.

[26] "Space Shuttle," *The Internet Encyclopedia of Science*, http://www.daviddarling.info/encyclopedia/S/Space_Shuttle.html.

[27] Ibid.

[28] Jeff Foust, "Operationally Responsive Spacelift: A solution seeking a problem," *The Space Review*, 2, http://www.thespacereview.com/article/52/2.

[29] Ibid., citing Lou Amoriosi, vice president of Orbital Sciences Corporation.

[30] "Pegasus Mission History," Orbital Sciences Corporation, http://www.orbital.com/SpaceLaunch/Pegasus/pegasus_history.shtml.

[31] "Taurus Mission History," Orbital Sciences Corporation, http://www.orbital.com/SpaceLaunch/Taurus/taurus_history.shtml.

[32] William Harwood, "NASA science satellite lost in Taurus launch failure," *Spaceflight Now*, 4 March 2011, http://www.spaceflightnow.com/taurus/glory/failure.html.

[33] Frank Sietzen, Jr, "The Myth of the $10,000 Per Pound Launch Cost?," *SpaceRef.com*, 18 March 2001, http://www.spaceref.com/news/viewnews.html?id=301.

[34] Ibid.

[35] Ibid.

[36] Eric Sterner, The Marshall Institute, *Symposium on Aligning Policies and Interests*, Space Policy Institute, 2 June 2009.

[37] Al Munson, Deputy Director, National Intelligence for Future Capabilities, *Symposium on Aligning Policies and Interests*, Space Policy Institute, 2 June 2009.

[38] *Space Acquisitions: Stronger Development Practices…,* GAO-05-891T, 8.

[39] Henry P. Pandes, "Total System Performance Responsibility," *Contract Management*, March 2002, 24-29.

[40] This can be attributed to staffing the program with government and contractor instructors who have much less space engineering and management experience than some of the students being taught. The institute's deputy commandant has no space experience, and its strategic-level space course is taught primarily by Air Force and Marine pilots, mortar and riflemen, intelligence officers, and missileers, with only marginal space engineering and acquisition experience.

[41] According to *Time Magazine* description of the DIVAD tests: "The Army's videotape is spectacular. As unmanned planes sweep into view, the high-tech antiaircraft gun on the ground swivels and blows them out of the sky. It looks like a brilliant performance by one of the Pentagon's most controversial new weapons, the Sergeant York division air-defense gun, known as the DIVAD. In a test last year, the gun's laser-and-radar guidance system could not even hit a stationary helicopter, one of many embarrassments for the problem-plagued system. This time, claimed the contractor, Ford Aerospace, the weapon destroyed "six of seven high-performance aircraft."

Not so, said Republican Congressman Denny Smith of Oregon, a veteran pilot who flew 189 missions over Vietnam. Smith pointed out that the unmanned planes used in the $54 million test came in higher and slower than they would in a battle. Worse, when he investigated further, he learned that the aircraft were in fact exploded by remote ground control within seconds of each firing from Sergeant York. Smith believes that the gun never actually hit the drone planes. The army says that the rapid-fire shots came close enough to destroy the aircraft and that the remote-controlled blasts were used to keep them from flying out of control. Still, John Krings, the Pentagon's director of testing, conceded that "the limitations [of the test] were and still are significant." "Gunning for Sergeant York," *Time*, 18 April 1985, http://205.188.238.109/time/magazine/article/0,9171,1050466,00.html.

[42] Ivan Semeniuk, "Was moon-smashing mission doomed from the start?," *New Scientist*, 15 October 2009, http://www.newscientist.com/article/dn17991-was-moonsmashing-mission-doomed-from-the-start.html.

[43] Ibid.

[44] Ibid.

[45] Ibid.

[46] Ibid.

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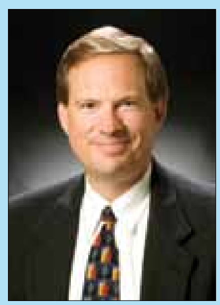

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