THE NEW ATLANTIS
A JOURNAL OF TECHNOLOGY & SOCIETY

Getting Space Exploration Right

Robert Zubrin

In early 2004, President George W. Bush delivered a major policy speech charting a new course for the National Aeronautics and Space Administration (NASA). Instead of focusing on perfecting flight to and operations in low Earth orbit, the space agency would henceforth set its sights on a return to the Moon and then "human missions to Mars and to worlds beyond." The president's move was a direct response to concerted criticism of the nation's space policy following the shuttle *Columbia* accident of February 2003. Numerous members of Congress had decried the fact that the U.S. manned space program had gone adrift, spending huge amounts of money and putting lives at risk without any discernable objective. Accordingly, in a reversal of previous administration pronouncements, the new "Vision for Space Exploration" was created to pose grand goals for America in space.

There is no doubt that a radical policy shift was in order. During the first dozen years of its existence, NASA took the nation from having no space capability to landing humans on the Moon, but since then, the manned space program has been stuck in low Earth orbit. Clearly, three decades of stagnation are enough. The question is whether the new policy is adequate to remedy the problems that have mired the space program in confusion and impotence, or whether it will amount to nothing. What needs to be done to make the Bush vision real?

To answer this question, we need to examine NASA's fundamental mode of operation, and see how the new policy bears on the organization's pathology. Then, to assess how the proposed cure is working, we need to examine the developments that have occurred since the president's announcement. While there are many hopeful signs, there remain large causes for concern, and radical changes in both the policy itself and its method of implementation will be required for the president's vision to succeed. Finally, we need to understand the deeper significance of this endeavor for both America and the human future. We need to ask: Why should human beings explore space at all, and why us?

***Robert Zubrin**, an astronautical engineer, is president of Pioneer Astronautics, a research and development firm, and president of the Mars Society, a space advocacy group. He is the author of the nonfiction books* The Case for Mars *(1996),* Entering Space *(1999), and* Mars on Earth *(2003), and the science fiction political satire* The Holy Land *(2003).*

But first things first. Before we can present the cure, we need to understand the disease.

Why Has NASA Been Failing?

Over the course of its history, NASA has employed two distinct modes of operation. The first prevailed during the period from 1961 to 1973, and may be called the Apollo Mode. The second has prevailed since 1974, and may be called the Shuttle Mode.

In the Apollo Mode, business is (or was) conducted as follows: First, a destination for human spaceflight is chosen. Then a plan is developed to achieve this objective. Following this, technologies and designs are developed to implement that plan. These designs are then built and the missions are flown.

The Shuttle Mode operates entirely differently. In this mode, technologies and hardware elements are developed in accord with the wishes of various technical communities. These projects are then justified by arguments that they *might* prove useful at some time in the future when grand flight projects are initiated.

Contrasting these two approaches, we see that the Apollo Mode is *destination-driven*, while the Shuttle Mode pretends to be technology-driven, but is actually *constituency-driven.* In the Apollo Mode, technology development is done for mission-directed *reasons.* In the Shuttle Mode, projects are undertaken on behalf of various pressure groups pushing their own favorite technologies and then defended using *rationales.* In the Apollo Mode, the space agency's efforts are *focused and directed.* In the Shuttle Mode, NASA's efforts are *random and entropic.*

To make this distinction completely clear, a mundane metaphor may be useful. Imagine two couples, each planning to build their own house. The first couple decides what kind of house they want, hires an architect to design it in detail, and then acquires the appropriate materials to build it. That is the Apollo Mode. The second couple polls their neighbors each month for different spare house-parts they would like to sell, and buys them all, hoping eventually to accumulate enough stuff to build a house. When their relatives inquire as to why they are accumulating so much junk, they hire an architect to compose a house design that employs all the knick-knacks they have purchased. The house is never built, but an excuse is generated to justify each purchase, thereby avoiding embarrassment. That is the Shuttle Mode.

In today's dollars, NASA's average budget from 1961 to 1973 was about $17 billion per year—only slightly higher than NASA's current budget. To assess the comparative productivity of the Apollo Mode with the Shuttle Mode, it is therefore useful to compare NASA's accomplishments during the years 1961-1973 and 1990-2003, as the space agency's total expenditures over these two periods are roughly the same.

Between 1961 and 1973, NASA flew the Mercury, Gemini, Apollo, Skylab, Ranger, Surveyor, and Mariner missions, and did all the development for the Pioneer, Viking, and Voyager missions as well. In addition, the space agency developed hydrogen oxygen rocket engines, multi-staged heavy-lift launch vehicles, nuclear rocket engines, space nuclear reactors, radioisotope power generators, spacesuits, in-space life support systems, orbital rendezvous techniques, soft landing rocket technologies, interplanetary navigation technology, deep space data transmission techniques, reentry technology, and more. In addition, such valuable institutional infrastructure as the Cape Canaveral launch complex, the Deep Space tracking network, and the Johnson Space Center were all created in more or less their current form.

In contrast, during the period from 1990 to 2003, NASA flew about fourscore shuttle missions, allowing it to launch and repair the Hubble Space Telescope and partially build what is now known as the International Space Station. About half a dozen interplanetary probes were launched (compared to over 40 lunar and planetary probes between 1961 and 1973). Despite innumerable "technology development" programs, no new technologies of any significance were actually developed, and no major operational infrastructure was created.

Comparing these two records, it is difficult to avoid the conclusion that NASA's productivity—both in terms of missions accomplished and technology developed—was vastly greater during its Apollo Mode than during its Shuttle Mode.

The Shuttle Mode is hopelessly inefficient because it involves the expenditure of large sums of money without a clear strategic purpose. It is remarkable that the leader of any technical organization would tolerate such a senile mode of operation, but NASA administrators have come to accept it. Indeed, during his first two years in office, Sean O'Keefe (the NASA administrator from 2001 until early 2005) explicitly endorsed this state of affairs, repeatedly rebutting critics by saying that "NASA should not be destination-driven."

Yet ultimately, the blame for this multi-decade program of waste cannot be placed solely on NASA's leaders, some of whom have attempted to rectify the situation. Rather, the political class must also accept major responsibility for failing to provide any coherent direction for America's space program—and for demanding more than their share of random projects that do not fit together and do not lead anywhere.

Advocates of the Shuttle Mode claim that by avoiding the selection of a destination they are developing the technologies that will allow us to go anywhere, anytime. That claim has proven to be untrue. The Shuttle Mode has not gotten us anywhere, and can never get us anywhere. The Apollo Mode got us to the Moon, and it can get us back, or take us to Mars. But leadership is required—and for the last three decades, there has been almost none.

The New Bush Policy

While a growing chorus of critics has decried overspending and other fiscal inefficiencies at NASA over the years, it was only the *Columbia* accident of February 2003 that provided the impetus for policymakers to examine the fundamental problem of America's manned space program.

In the aftermath of *Columbia*'s destruction, both Congress and the administration initiated inquiries into the affair. These included extensive hearings in both the House and Senate and a special blue-ribbon commission appointed by the president and headed by retired Navy Admiral Harold Gehman, Jr. While much of the attention in these investigations focused on determining the specific causes of the accident itself, both Gehman and many of the congressional and press critics took a broader view, identifying as problems not only the particular management failures that led to the shuttle's loss, but also the overall lack of strategic direction of the space agency.

Columbia was lost on a mission that had no significant scientific objectives, certainly none commensurate with the cost of a shuttle mission, let alone the loss of a multi-billion dollar shuttle and seven crew members. In war, when soldiers are lost attempting a military mission of no value, the fallen are still heroes, but the generals have some explaining to do. The *Columbia* flight program included conducting experiments in mixing paint with urine in zero-gravity, observing ant farms, and other comparable activities—all done at a cost greater than the annual federal budgets for fusion energy research and pancreatic cancer research, combined.

After the Columbia Accident Investigation Board's report was issued in August 2003, this line of criticism became a refrain. In response, the Bush administration initiated an internal deliberative process to try to define strategic goals for the American space program. This process was carried out primarily behind closed doors, although a number of outsiders were invited to present their views. From these discussions and a series of congressional hearings, three distinct factions emerged. First, there were those who supported continuing business as usual at NASA, with appropriate cosmetic adjustments to get past the immediate crisis, but no fundamental changes. Second, there were those who called for making a human return to the Moon the central goal of the manned spaceflight program. And third, there were those who argued for an initiative to get humans to Mars.

President Bush announced the new policy on January 14, 2004, in a speech at NASA headquarters. As articulated in that speech and an accompanying National Security Presidential Directive, the new policy, dubbed the "Vision for Space Exploration," included something for each faction. The vision calls for:

- Implementing a sustained and affordable human and robotic program to explore the solar system and beyond;
- Extending a human presence across the solar system, starting with a human return to the Moon by the year 2020, in preparation for human exploration of Mars and other destinations;
- Developing the innovative technologies, knowledge, and infrastructures both to explore and to support decisions about the destinations for human exploration; and
- Promoting international and commercial participation to further U.S. scientific, security, and economic interests.

The directive then lists a series of actions and activities to achieve these stated goals. These include returning the space shuttle fleet to flight, using it to complete construction of the International Space Station, and then retiring the shuttle and moving beyond it by "the end of this decade." The directive also states that NASA should develop "a new crew exploration vehicle to provide crew transportation for missions beyond low Earth orbit," and should conduct "the initial test flight before the end of this decade in order to provide an operational capability to support human exploration missions no later than 2014." It also says that NASA shall "acquire crew transportation to and from the International Space Station, as required, after the space shuttle is retired from service."

Beyond low Earth orbit, the policy instructs NASA to "undertake lunar exploration activities to enable sustained human and robotic exploration of Mars and more distant destinations in the solar system." By 2008, NASA should begin a series of lunar robotic missions intended to "prepare for and support future human exploration activities." The first human mission is supposed to commence between 2015 and 2020. And unlike the short, three-day stay on the Moon that is the previous record (set by Apollo 17 in 1972), this would be an "*extended* human expedition."

In addition to studying the Moon itself, these lunar activities are meant to "develop and test new approaches, technologies, and systems … to support sustained human space exploration to Mars and other destinations." The plan calls for robotic exploration of the solar system—Mars, asteroids, Jupiter's moons—as well as a search for habitable planets outside our solar system. The knowledge gathered from the robotic exploration of Mars, along with the lessons learned from long-term stays on the Moon, along with new technologies for "power generation, propulsion, life support, and other key capabilities," are aimed at making possible "human expeditions to Mars" at some unspecified date.

The most obvious problem with the Bush plan is its long, slow timeline. The only activities that the Vision for Space Exploration actually mandates before the end of the Bush administration's second term are the return of the shuttle to flight, the use of the shuttle to complete the International Space Station, the flight of one lunar robotic probe, and the initiation of a development program for the Crew Exploration Vehicle. The ten-year schedule for the development of the Crew Exploration Vehicle is especially absurd. Technically, it makes no sense: starting from a much lower technology base, it only took five years to develop the Apollo command module, which served the same functions. Politically, it is unwise: the delay makes the development of the Crew Exploration Vehicle reversible by the next administration. And fiscally, it is foolish: the long timeline only serves to gratify the major aerospace industry contractors, which desire a new long-term, high-cost activity to replace the recently cancelled Orbital Space Plane. Stranger still is the decision to set the next manned Moon landing as late as sixteen years into the future—twice as long as it took the United States to reach the Moon back in the 1960s—and to place the Mars mission at some nebulous time in the future. Such a drawn-out timeline is unlikely to serve as a driving force on the activities of this slow-moving bureaucracy.

Still, there are aspects of the new policy that make it a positive step forward. By declaring that Moon-Mars would be the next order of business after the completion of the space station, the Bush vision precludes starting alternative initiatives that would get in the way. More importantly, by declaring that human exploration of the Moon and Mars is the goal of NASA, the new policy makes it legitimate for the space agency to allocate funds for technology development to support this objective. This is very important, since such spending previously could not be justified unless it could be defended as a necessary part of other programs, such as the space station or the robotic planetary exploration program. The mere designation of the Moon-Mars objective broke a formidable dam against the agency's progress, and the administration rapidly showed its bona fides by requesting several hundred million dollars to support such newly permissible research and development. In addition, it was made clear that funds would be available to demonstrate some of these new technologies using subscale units on robotic missions to the Moon and Mars, starting around the end of this decade. But even this positive news must be viewed with caution. For in the absence of an actual Moon-Mars program—one that develops an efficient mission plan that designates the program's technology needs—broad R&D expenditures can be quite inefficient.

Relative to the decisive form of leadership that drove the success of the Apollo program, the Bush policy set forth a large vision without the sense of urgency to make it real. But an uncertain trumpet is still better than none at all. Before President Bush's announcement, the idea of an American program to pioneer the space frontier seemed to many like the stuff of science fiction writers, wistful dreamers, and marginal visionaries. Suddenly, it was a mainstream political idea, and significant social forces began to rally both for and against the plan.

The Hubble Blunder

The new Bush space policy received mixed reviews in the press. But it was nearly derailed two days after its release when Administrator O'Keefe announced his decision to cancel the planned shuttle mission to maintain and upgrade the Hubble Space Telescope, thereby dooming the instrument to destruction. Lacking any scientific or technical background, O'Keefe might be forgiven for not understanding Hubble's value to astronomy. Yet, as an experienced bureaucrat, he should have had some appreciation of the significance of abandoning several billions of dollars of the American taxpayers' property. Apparently, however, he did not, and the affair that ensued produced one of the worst public relations disasters in NASA's history.

Built, launched, repaired, and successively upgraded at a total cost of some $4 billion, the Hubble Space Telescope has made numerous important discoveries about the nature and structure of the universe. It is the most powerful instrument in the history of astronomy, and far and away the most productive spacecraft that NASA has ever launched. Because it orbits above the atmosphere, which both smears light and blocks out major portions of the spectrum, Hubble can see things that no ground-based telescope will ever see. It took decades of hard work by very dedicated people to create Hubble, and an equivalent space-based replacement remains decades away. In contrast to the general run of meaningless shuttle missions carrying silly science fair experiments, the shuttle flights to Hubble stand as epochal achievements. If one considers the moral significance of the scientific enterprise to our society and culture, Hubble stands out not just as NASA's finest work, but as one of the highest expressions of the human creative spirit in the twentieth century.

At a cost of $167 million, two new instruments, the Wide Field Camera 3 and the Cosmic Origins Spectrometer, had been developed and built which, once installed on Hubble, would together triple the instrument's sensitivity. Accordingly, NASA had scheduled a shuttle mission to the telescope for 2006, both to add these capabilities and to perform certain other maintenance tasks that would extend the life of Hubble through at least 2010. Under the new Bush space policy, the shuttles were scheduled to remain operational through 2010, permitting a final shuttle mission to Hubble to occur toward the end of the decade. This would allow one last replacement of the telescope's batteries and gyros and a reboost of its orbit, thereby making it functional beyond 2015. If no missions to Hubble were flown, however, the space observatory's aging gyroscopes would put it out of commission by 2007.

Incredibly, on January 16, 2004, O'Keefe announced that he had decided to allow that to happen. He justified his decision by claiming that shuttle missions to Hubble were unsafe since they offer no alternative safe haven to the crew, in contrast to missions to the International Space Station (under the president's policy, about 25 more such shuttle missions would be flown). This argument was basically nonsense, since there are numerous features of space station missions that make them more dangerous than Hubble flights. For example, Hubble missions depart Cape Canaveral flying east-southeast, which means that in the event of an abort, the crew can ditch in tropical waters where their survival chances would be much better than in the frigid North Atlantic and Arctic oceans overflown by the northeast-flying ISS missions. Hubble missions also take off much more lightly laden than ISS missions, which makes them safer, as less performance is required of the engines to make it to orbit. Moreover, the danger from micrometeorite and orbital debris is estimated by NASA to be about 60 percent greater at the space station's altitude than at Hubble's.

So NASA's own risk analysis did not support O'Keefe's argument that Hubble missions posed too high a risk, and while the administrator declined to include such information in his briefings to congressional committees, outraged NASA personnel quickly leaked the relevant data to the press. O'Keefe countered by ordering high-level NASA officials who were known to be ardent supporters of Hubble to take public stands supporting his decision. The disgusting spectacle of bureaucratic self-humiliation that followed only excited derision in the press.

Mr. O'Keefe then argued that regardless of the actual risk, the recommendations of Admiral Gehman's Columbia Accident Investigation Board precluded a shuttle flight to Hubble. But this claim was rejected by Gehman himself, in a letter to Senator Barbara Mikulski (D.-Md.), a strong Hubble supporter. Almost all the risk in any shuttle mission occurs during the ascent and descent; "where one goes on orbit makes little difference" to overall safety, Gehman wrote. "Only a deep and rich study of the entire gain/risk equation can answer the question of whether an extension of the life of the wonderful Hubble telescope is worth the risks involved."

Admiral Gehman's response provided Mr. O'Keefe an exit opportunity from his policy blunder, but the NASA Administrator chose not to take it. Not only that, but when Senator Mikulski and Senator Sam Brownback (R.-Kans.) ordered a review from the National Research Council, Mr. O'Keefe responded by saying that while he welcomed a review from such a prestigious body, he would not change his decision regardless of anything they said.

As a final dodge, Mr. O'Keefe then announced that he sincerely wanted to save Hubble, but could not bring himself to risk human life to do so. Accordingly, he would request $1.9 billion in new funds to develop robots capable of performing the mission. This proposal was thoroughly disingenuous. A Hubble upgrade mission requires the coordinated efforts of seven highly trained and superbly skilled astronauts using a spacecraft and other equipment that has been specifically designed and extensively tested as suitable for this purpose. In contrast, there isn't a robot on this planet that can change an overhead kitchen lighting fixture. What's more, the robots touted by O'Keefe as candidates for repairing Hubble ranked much too low on the agency's standard system of "technology readiness levels," meaning that to use them would be a complete abandonment of NASA mission planning discipline.

In December 2004, the National Research Council panel reported back, rejecting the robotic repair—such a robotic mission "would require an unprecedented improvement" in technology in the next few months, the panel concluded—and calling for a manned shuttle mission "as early as possible." A few days later, Mr. O'Keefe announced his resignation, but before departing he submitted a NASA budget containing no funds for either a manned or robotic mission to repair Hubble. Instead, he requested $300 million to develop a special spacecraft to deorbit Hubble—that is, to crash it into the ocean in a controlled fashion. Even aside from the rest of the Hubble controversy, this proposal is remarkable for its irrationality. NASA calculates that if Hubble were to re-enter Earth's atmosphere without direction, there is a 1 in 10,000 chance that the resulting debris would strike someone. If saving lives is the goal, that $300 million could do a lot more good spent on tsunami relief, body armor for the troops, highway safety barriers, childhood vaccinations, swimming lessons—take your pick.

The fate of Hubble remains undecided at this writing, but the damage done to the new initiative has been substantial, and threatens to become much worse if Mr. O'Keefe's decision is allowed to stand. Effectively, by choosing the most valuable part of the old space program and selecting it for destruction as collateral damage of implementing the new, the former administrator has branded the President's vision with the mark of Cain. Opponents of the new policy, such as the *New York Times*, have blamed the loss of the space telescope on the Moon-Mars initiative, and indeed, it is difficult to take seriously the claims of scientific purpose of an agency which chooses to abandon its capabilities so flippantly. Why should NASA receive more funds to build new space telescopes when, like a spoiled child bored with a two-hour old toy, it willfully throws away the one it already has? And how can anyone believe that an agency too scared to launch astronauts to Hubble will ever be ready to send humans to Mars? Congress has spent billions of taxpayer dollars to create the hardware needed to implement the Hubble program and the supporting shuttle infrastructure, only to be confronted with a NASA administrator who refuses to use it. If O'Keefe's decision to desert Hubble is not reversed, how can Congress know that after it spends further tens of billions for human flight systems to the Moon and Mars, that the agency leadership won't get cold feet again?

The Aldridge Commission

In order to give the new space policy some blue-ribbon certification—and also to drum up some public support for the plan—the Bush administration launched the President's Commission on Implementation of the United States Exploration Policy. Chaired by former Air Force Secretary Edward "Pete" Aldridge, Jr., the commission was charged with making recommendations for the scientific agenda, technological approach, and organization strategy for the new space initiative. In addition to Aldridge, the commission included two high-level corporate executives, a retired four-star general, a former congressman, three geologists, and an astrophysicist-cum-planetarium director. Some of these people were quite eminent in their chosen fields, but the absence of any astronautical engineer (or indeed anyone who had ever worked as an engineer in any field) or any astrobiologist was striking. The commission thus lacked credentials in two central areas of its charge. Of the commission members, only one, lunar geologist Paul Spudis, had ever participated in studies of human planetary exploration before, and his scientific interests are so narrowly focused on the Moon that he has been known to make extravagant claims in support of his research agenda (such as maintaining that lunar geology is the key to understanding mass extinction processes on Earth).

Between February and May 2004, the commission held hearings in ten American cities. About a hundred witnesses were invited to testify, but it rapidly became clear that the commission was not actually interested in ideas that diverged from a predetermined mantra. This was partially forgivable, since much of the testimony the commission chose to entertain was quite absurd, like the presentation from one crankish invitee arguing that the best place to look for Martian fossils was on the Moon, by searching for ejected Mars rocks landed there. (This idea was strange, to say the least, since there are many more Martian rocks on Earth than on the Moon—and, of course, there are significantly more on Mars itself.) But while the commission was hard-headed enough to set such nonsense aside, it was also impervious to necessary ideas. A very sad example of this was exhibited at the San Francisco hearings, when noted science fiction author Ray Bradbury testified. Bradbury gave an impassioned and eloquent speech in which he said that the American people could be inspired to support the new space policy if it were presented as the first step in the growth of humanity into a multi-planet spacefaring species. After he concluded, Aldridge replied with a question about how we "sell this to the

American taxpayer." With great patience and poetic clarity, Bradbury explained his point again. Spudis then responded, saying it would be easier to just tell the American people that space is "a source of virtually unlimited wealth." One has to wonder how a group of people who don't actually believe in a great enterprise can hope to lead it.

On June 4, 2004, the commission finally released its report. Remarkably, the group managed to get the answers completely wrong in the three central areas of its responsibility: the scientific goals, the technical strategy, and the reform of NASA.

First, the scientific goals. The commission proposed a sixteen-point science agenda that ranged from discovering the origin of the universe to assessing global climate change. Many of these points represented important fields of scientific research, but fourteen of the sixteen had very little to do with human exploration of the Moon and Mars. Rather, the list seemed to be something that had been cut and pasted from prior National Research Council reports on generic scientific priorities in space. Of the two items on the agenda that did have a clear relationship with human exploration, both dealt with planetary geology. While one of these latter points did include "identification and characterization of *environments* potentially suitable" (emphasis added) for past or present biogenic activity as a goal, absent from the list was any search for past or present *life itself.* This is remarkable because the search for life was clearly central to President Bush's new vision for NASA, and because surely the search for life—especially on Mars—is key to understanding the prevalence and diversity of life in the universe. Even as the commission was doing its work, NASA's Spirit and Opportunity rovers were making headlines identifying the coastal deposits of ancient Martian oceans, and high-level NASA officials were saying things like, "If you have an interest in searching for fossils on Mars, this is the first place you want to go." Astrobiological research conducted on the Martian surface by human explorers provides the most compelling scientific rationale for the new space policy; it is the one really important form of extraterrestrial research that only astronauts can do adequately. Yet the commission did not include it on the agenda. By failing to do so, the commission deprived the human exploration initiative of its strongest rational basis.

Second, the commission identified a list of seventeen technologies that it said need to be developed to enable the new initiative. According to the commission, funds should be spent to create these technologies, after which they should be integrated into the exploration architecture. This is exactly the opposite of the correct way to proceed. Instead of arbitrarily choosing a list of technologies to develop, and then forcing them into the mission plan, NASA should design the mission plan, identify the technologies it requires, and then develop them. To do otherwise is to dissipate resources in random spending. Only about four of the seventeen technologies the commission cited are strictly necessary for human Moon-Mars exploration. Of the rest, about half are generally useful but not necessary mission enhancements, while most of the others are only plausibly useful under certain mission scenarios. Finally, one of the cited technologies is clearly not needed under any circumstances, and one technology that failed to make their list is critically needed. The point is, if you want a system of parts to fit and work together, you design the system first, and then you make the parts. In contrast, the commission approach involves acquiring a bunch of well-marketed items, and then trying to fit them together to make a system—a repeat of the Shuttle Mode approach to spending that has been the primary cause of the past three decades of stagnation.

Third, the commission correctly observed that there is a need for organizational reform in NASA if the new space initiative is to be implemented successfully. It noted that the most effective of the NASA field centers is the Jet Propulsion Laboratory (JPL), and that JPL is not a civil service institution like the other NASA centers but a Federally Funded Research and Development Center (FFRDC). Employee merit can thus be rewarded at JPL with higher pay, or lack of performance punished with dismissal, in a way that is simply not possible in a civil service organization. Linking these two findings, the commission ascribed JPL's superior performance to its FFRDC form of organization, and therefore recommended converting all of the NASA field centers to FFRDCs as the cure for the agency's internal ills.

The commission is arguably correct that JPL is the most productive NASA field center, but the question must be asked if the FFRDC organizational form is truly the cause. The Department of Energy's research labs are all FFRDCs as well, and their productivity today is much lower. So what other factors might account for JPL's success? How about the fact that all of its leaders are technically excellent? From Theodore von Kármán during World War II to Charles Elachi today, all of JPL's directors have been superb scientists or engineers, and the same is true of nearly all its upper managers, middle managers, and senior engineers, right down the line. That is not generally the case at other NASA centers, and it is most certainly not the case at NASA headquarters. In running a space program, it helps if you know what you are talking about.

It also helps if you know what you are trying to accomplish. JPL is mission-driven, and the missions it selects are science-driven. It develops the technologies that are necessary to enable those mission designs. The system isn't perfect; human weakness enters in, mistakes are sometimes made, and biases sometimes get into play, but overall the operation is rational and purposeful—precisely because it does not operate in the mode that the Aldridge Commission recommended for NASA. The FFRDC may be a superior organizational form to the civil service, but it isn't the decisive factor. During the Apollo period, civil service NASA centers such as Johnson Space Center and Marshall Space Flight Center had records of accomplishment at least as impressive as JPL's. But their technical leadership at that time was also superb, and they were mission-driven, too. Today, much of NASA fails to meet these two basic criteria for success.

Technical Competence and Political Convenience

The central importance of technically qualified leadership at NASA is sometimes countered by the example of James Webb, who served as the space agency's highly successful administrator during the Kennedy-Johnson years. It is true that Webb lacked a technical background, but that is only part of the story. Webb's Oklahoma country boy persona was an act used to hustle the gullible. In fact, Webb was a highly educated and incisive intellect. As one of the authors within the Kennedy administration of the Apollo program, he was passionately committed to its success, and he made it his business to learn everything necessary to understand what was going on and lead the program to victory. He could be very forceful when dealing with competing bureaucratic powers, but he never tried to dictate technical reality to engineers. Rather, he gathered together some of the top technical talent of all time, and he listened to it.

By contrast, the consequences of NASA leadership lacking in technical competence or even respect for scientific or technical considerations are amply demonstrated by the events of the O'Keefe years. In addition to the Hubble debacle, discussed above, the gross managerial failures during this period included the Orbital Space Plane program, the Jupiter Icy Moon Orbiter program, and the loss of the space shuttle *Columbia.*

First, the Orbital Space Plane. During the Clinton administration, NASA's Johnson Space Center in Houston, Texas had begun a program called X-38 to develop a crew capsule that could launch astronauts to orbit atop a medium lift launch vehicle, thereby allowing space station crews to be rotated at much lower cost than is required for a shuttle flight. Since the Johnson Space Center is the primary NASA center with expertise in crewed flight systems, it made sense for the project to be assigned there. But apparently for political reasons, Mr. O'Keefe decided to move the program to the Marshall Space Flight Center in Huntsville, Alabama. Claiming the X-38's estimated price tag of $1.6 billion was too high, he cancelled that program in midstream and set up the Orbital Space Plane program in Alabama in its place. The actual expertise of the Marshall Space Flight Center is in launch vehicles, however, and without the necessary experience, costs rapidly escalated out of control, with the estimated program budget growing to over $15 billion by the fall of 2003. Congress balked at funding this boondoggle, and the program collapsed with nothing accomplished and close to a billion dollars of the taxpayer's money down the drain.

Next, the Jupiter Icy Moon Orbiter (JIMO) intended to use advanced technology to study the frozen moons of Jupiter. This program was begun by O'Keefe himself, and could have been his greatest accomplishment—it would have been a significant scientific achievement and it would have made the essential capability of space nuclear power into a reality. The merit of this proposal lay in the fact that replacing today's radioisotope generators with nuclear power would allow a probe sent to the outer solar system to employ active sensing instruments and to transmit back vastly greater amounts of scientific data. Using nuclear power would also enable electric propulsion ("ion drive"), allowing the spacecraft to engage in extensive, highly efficient maneuvers among Jupiter's moons.

So far, so good. However, in order to get more funding, the electric propulsion community managed to insert a requirement into the program that the flight from Earth to Jupiter be accomplished using electric propulsion, and that the trip to Jupiter not use any planetary gravity assists ("the slingshot effect"). Suddenly, under these new rules, the power needed to propel JIMO grew to 150 kilowatts in order to reach Jupiter in nine years. This is not only absurd (in the 1970s, Voyager made the trip in less than three years; in the 1990s, Galileo did it in five) but disastrous, since the nuclear reactor cannot be rated in advance for nine years of operation. In other words, JIMO would almost certainly fail before it reached the planet. Furthermore, as a result of the weight and the huge mass of the 150 kilowatt reactor and xenon propellant, the spacecraft couldn't be launched into space on any existing rocket. In contrast, had these rules not been adopted, the reactor could have been scaled down to 20 kilowatts, all the interplanetary transfer xenon propellant been eliminated, and the spacecraft thus made light enough to be put on top of an existing rocket and thrown toward Venus for the first in a series of gravity assists. These maneuvers would have allowed the spacecraft to reach Jupiter in five years on a Galileo-like trajectory, without needing to start burning the reactor until operations within the Jupiter system began. In other words, JIMO done the easy way could have been accomplished with one-seventh the power, one-quarter the mass, half the flight time, and a much greater success probability as JIMO done the hard way. Administrator O'Keefe apparently did not understand any of these issues. Instead, the former Secretary of the Navy wrongly equated nuclear electric propulsion for spacecraft to nuclear power for submarines, allowing them to transcend the limits of chemical propulsion and "go anywhere, anytime," without the need for such old-fashioned tricks as gravity assists. Because of his naïveté on such matters, O'Keefe failed to see this bunk for what it was, and in fact promoted it as a programmatic mantra. As a result, the program's cost ballooned to over $9 billion, and the White House declined to ask for further funding for Fiscal Year 2006. In the meantime, more money was spent studying JIMO than was spent designing, building, flying, and analyzing the data from the highly successful Mars Global Surveyor mission, from start to finish.

Finally, the loss of the space shuttle *Columbia* can also be traced to managerial disrespect for technical advice. No information has come to light directly linking Mr. O'Keefe to the specific decisions that led to the accident, but the accident does clearly illustrate the consequences of arrogantly insisting that technical reality conform to the management line. NASA engineers informed the agency's management that they had data showing that there could be a serious problem with *Columbia*'s thermal protection system. The managers had the means to investigate the engineers' suspicions, either by asking the Air Force to shoot high-resolution photographs of the shuttle, or by having the shuttle astronauts conduct a direct inspection themselves. Had management undertaken either course, the damage to the thermal protection tiles would have been discovered. That being the case, the crew could have attempted an ad hoc repair. It might have worked, or it might not. It is untrue that the situation was necessarily hopeless. *Columbia* actually made it most of the way back, and perhaps a crude repair might have done the trick—or if the pilot had been informed of the problem, he might have been able to fly the craft in such a way as to favor the weaker wing enough to survive. We'll never know. But certainly the managers who decided to stick with the "position" of the agency and not check the problem didn't know either. In consequence, the crew members were not even given a chance to fight for their lives.

The Aldridge Commission report did not speak to these kinds of serious shortcomings. All in all, it was a dull read, and had limited impact. Since it basically endorsed the status quo of a non-driven NASA, there was little positive damage it could do. But an opportunity to force necessary changes had clearly been lost. As a result, the key questions remained unsettled—including the need to set rational scientific goals, to ensure qualified leadership, and to decide whether program engineering will be driven by technical judgment or political convenience. The drift continued, and the Bush vision still lacked a real-life plan adequate to the boldness of its goals.

The New Space Budget

Even without a plan, the president's vision needed funding, and the members of the diverse American aerospace community lined up to show their support. This community includes a few large and many small aerospace companies; numerous government and university participants; and an array of industrial associations, technical and professional societies, and advocacy groups. These organizations differ in their prioritization of scientific, commercial, and military goals in space; in their preference for a government-led space program or a free-enterprise space industry; and in their nationalist or internationalist orientation.

Nevertheless, with virtually complete unanimity, this assemblage responded to the Vision for Space Exploration with a strong endorsement. Two organizations were formed, the industry-led Coalition for Space Exploration and the advocacy group-led Space Exploration Alliance, and nearly every outfit in the field, either through one of these leagues or on its own, commenced lobbying for the president's new policy. The unprecedented unity of the aerospace community sent a strong message to Congress that a new focus for the American space program was truly needed, and that the Moon-Mars initiative was a long-overdue step in the right direction.

While lacking in merit as a technical decision-maker, NASA Administrator O'Keefe was extremely adroit in working the congressional funding process. That fact, combined with the very clear support from the aerospace community, sufficed to reap initial funding for the Vision for

Space Exploration for Fiscal Year 2005. Only about $150 million requested actually represented new funding, but preexisting programs were amalgamated to create a new Exploration Systems Mission Directorate (ESMD) with a fairly serious initial budget on the order of a billion dollars. Retired Navy Rear Admiral Craig Steidle, the former head of the Joint Strike Fighter development program, was brought in to lead the new directorate.

Moving in Spirals

Over the spring and summer of 2004, the ESMD proceeded to develop a program strategy to carry out the new space policy and created a mission architecture to implement the lunar portion of the plan. Completed in outline by the fall of 2004, this first-draft (or "Point of Departure") strategy consisted of five primary phases, or "spirals."

Spiral 1: Develop the Crew Exploration Vehicle (CEV) and its launch system and operate the CEV in low Earth orbit.

Spiral 2: Begin short duration lunar missions. To achieve this objective, the plan proposes the following design for a transportation system. First, NASA must develop a Lunar Surface Ascent Module (LSAM) to carry astronauts to and from the Moon's surface, a medium lift vehicle (MLV) capable of launching it, and an Earth Departure Stage (EDS) capable of delivering either the CEV or the LSAM separately from low Earth orbit to low lunar orbit. Carrying out a mission would require *four separate launches*—one MLV for the CEV, one for the LSAM, and one for each of two EDS vehicles. These four components would all be put into low Earth orbit. The manned CEV would then rendezvous with one EDS, and the empty LSAM would rendezvous with the other EDS, and each would be driven separately from the Earth's orbit to lunar orbit. The CEV would then rendezvous with the LSAM in low lunar orbit, after which the crew would transfer to the LSAM for an excursion to the Lunar surface of 4 to 14 days. The crew would then ascend in the LSAM to rendezvous with the CEV in lunar orbit, transfer back to the CEV, and come back to Earth. (If this all sounds terribly complex, that's because it is. More on the implications of that complexity in a moment.)

Spiral 3: The hardware set developed for Spiral 2 is augmented by a cargo lander and a variety of surface systems, including a habitation module. Using the habitation module and associated systems, lunar surface sorties are extended to 42 days, with 90 days as a goal.

Spiral 4: A set of hardware (as yet undefined) is developed and used to perform Mars flyby missions.

Spiral 5: The Spiral 4 hardware set is expanded to enable human exploration missions to the Martian surface. The nature and duration of these missions is as yet undefined.

According to the plan, the development effort for Spiral 1 would begin immediately, with piloted CEV flight operations in low Earth orbit commencing in 2014. Spiral 2 flight operations would begin in 2020. No dates have been set for Spirals 3, 4, or 5. At the same time, starting with Spiral 1, a set of robotic missions would be flown to the Moon and Mars to prepare for or support human exploration objectives.

This ESMD plan contains many flaws that deserve severe criticism. In fairness, it should be said that most of these problems stem from weaknesses in the original presidential directive, or to arbitrary interference in the engineering design process by Mr. O'Keefe or other non-technically educated individuals. But because of these flaws, the current plan jeopardizes the success of the vision, and actually makes it possible that we will *lose* space capabilities. Put simply, the ESMD plan has too many spirals; the spirals don't logically build upon one another; the plan isn't responsive to the president's vision; and the overall mission architecture is technically unsound. Each of these four deficiencies needs to be examined in detail.

First, the point that there are *too many spirals.* As presently designed, the plan entails five spirals. There should be only three:

Spiral A: Equivalent to the present Spiral 1, but done much quicker.
Spiral B: Equivalent to the present Spirals 2 and 3.
Spiral C: Equivalent in function to the present Spirals 4 and 5.

That is, Spiral 1 should be abbreviated, while Spirals 2 and 4 should be abolished entirely as independent spirals.

Spiral 1 needs to be dramatically shortened, because the ten year timeline to develop the CEV is a dangerous stall. The decision to delay piloted CEV flights until 2014 comes directly from the original White House policy directive, which defers supplying substantial funds to the new initiative until the shuttle and space station programs can be wound down at the end of the decade. That decision was thus above the pay grade of Admiral Steidle and the ESMD mission planners to dispute. But it is a decision with unfortunate consequences. The CEV is essentially the functional equivalent of the Apollo command module which, as previously mentioned, was developed in just five years in the 1960s starting from a much lower technology base. By artificially stretching out the CEV program, the cost will be greatly increased. Furthermore, with shuttle operations scheduled to end in 2010, putting off the completion of the CEV until 2014 will leave the United States with no human spaceflight capability for four years. During this period, the taxpayers will be paying for a human spaceflight program that is not actually doing anything. This is a serious problem.

Meanwhile, Spirals 2 and 4 are unnecessary in a program seeking to achieve maximum scientific return with minimum cost and risk. Spiral 2 lunar missions accomplish much less than Spiral 3 missions, but entail comparable cost and risk. And while Spiral 4 Mars missions require less cost and risk than Spiral 5 Mars missions, the latter offer several orders of magnitude greater scientific return. Thus Spiral 2 and 4 missions are neither cost-effective nor risk-effective, and should be minimized or eliminated from the program.

This is a critical point, so let us consider it in greater detail, looking specifically at the relationship between Spirals 2 and 3. The primary distinction between these two spirals is that Spiral 3 missions have a habitation module on the lunar surface, and therefore crews can stay on the surface much longer than in Spiral 2 missions, which would offer only the limited living space of the lunar module (as in the Apollo missions). Now it is obvious that a mission that operates on the surface for forty days will accomplish much more exploration than one that stays for four days. This advantage of the longer Spiral 3 missions is amplified much further by the fact that the habitation module will have lab facilities, allowing astronauts to perform preliminary analysis of large numbers of field samples while they are on the Moon, selecting only the most interesting samples to return to Earth for further study. Thus lunar exploration during Spiral 3 will be vastly more effective than in Spiral 2.

To be sure, there are plausible objections to eliminating Spiral 2. For instance, one might argue that Spiral 3 requires a habitation module and its power supply, which is an additional development and delivery cost. But the program is committed to that cost in any case, so why not aim to use these technologies from the beginning? Another objection might be that each expedition during Spiral 2 can land at a new site on the Moon, while explorers during Spiral 3 are limited to a radius around a single lunar base. This is true, although Spiral 3 missions compensate for that loss of novelty by allowing a more thorough exploration of each site, and by being less risky because the crew will have two safe havens (the lunar module and the habitation module). And since the habitation module is also the lab module, it provides them with both the endurance and the equipment they need to do effective exploration. It makes no sense to send explorers to the Moon without the primary tool they need to do their job. As a matter of cost-effectiveness, scientific sense, and crew safety, the correct strategy is to develop and deploy a habitation module to the Moon *before* any human expeditions. The first missions don't need to be 40 days long; selecting shorter durations for initial missions is a reasonable strategy. But, for the sake of both science and safety, the habitation module should be delivered first, with crew surface duration expanding as rapidly as mission experience shows to be prudent. Deferring the deployment of the habitation module until after a series of Spiral 2 expeditions will waste money and expose astronauts to unnecessary risk.

The issue is even more clear in the case of condensing Spirals 4 and 5 into a single "Spiral C." Mars flyby missions entail significant cost and risk, but accomplish no meaningful scientific goals. Their only valid function is to test hardware. (They also test human endurance, but such tests could be accomplished much more cheaply and safely near Earth.) There is no need to develop a separate hardware set, as Spiral 4 calls for, just to conduct Mars flyby missions. It makes far more sense to just build and test the hardware for real Mars missions. This hardware can most affordably be tested by having it perform necessary work like delivering missions to the Moon or pre-positioning useful infrastructure on Mars; it can even be tested, albeit at great cost, by flying an unmanned mission to the Martian surface and back. But it is irrational to send manned flyby missions to Mars. Having flown the crew all the way to Mars, they will have absorbed a large part of the risk and expense of a real Mars mission, and having done so, it makes no sense to end the mission without actually going to the surface. Flying such an abort-by-design mission before any actual missions only increases the overall program risk and cost. For this reason, Spiral 4 should be abolished.

The second major problem with the ESMD plan is that the spirals *don't sufficiently build upon one another*. The concept of "spiral development" in an engineering program involves introducing a hardware set that creates an initial capability, then improving it in subsequent phases or "spirals" by the addition of further technology. Rightly understood, therefore, spiral development involves enhancing or expanding the hardware set employed in an early phase to enable a later, more aggressive, set of objectives.

But the ESMD plan calls for designing a program that creates and then *abandons* a series of hardware sets to accomplish a progression of new goals. This is unnecessarily wasteful. Spiral 2 may be fairly said to be based on Spiral 1, since it makes full use of the CEV and its launch system. Similarly, Spiral 3 is clearly based on Spiral 2. But because the LSAM, the EDS, and the MLVs employed in the plan are all useless for Mars missions, Spirals 4 and 5 are not in any serious way based on Spirals 2 and 3. That is to say, except for the CEV developed during Spiral 1, almost none of the hardware developed during the previous spirals is appropriate for Mars missions. By contrast, with a better designed mission architecture, the Spiral 3 hardware could be directly useful for Mars missions. But that is not the case here.

The third significant flaw in the ESMD plan is that it *fails to respond to the presidential directive*. As currently constituted, the hardware used in Spirals 2 and 3 is designed to support lunar missions only, with no regard for Mars requirements. But the president's policy directive clearly specified that a central purpose of the lunar program is to enable sustained human exploration of Mars. These orders were effectively ignored by the designers of the plan.

The problem here is not merely one of formal disobedience to White House objectives. Rather, it is a matter of serious negative consequences. The ESMD plan requires a plethora of additional recurring costs and mission risks for the sole purpose of avoiding the development cost of a big new rocket—a heavy lift vehicle (HLV). Yet, since one goal of the Vision for Space Exploration is to get humans to Mars, an HLV will need to be developed anyway. So on a cost basis, the ESMD plan will lose twice over, since it requires new hardware for Spirals 2 and 3, and then even more new hardware for Spirals 4 and 5. Furthermore, in addition to imposing maximum mission risk for lunar explorers through its own excessive complexity, the ESMD plan will also increase the risk to Mars explorers, because the ESMD lunar plan will not test the Mars mission hardware. Rather than enable human Mars exploration, the plan as presently defined would be a massive and costly detour; it would delay such missions for many decades. And since the plan would involve two different sets of hardware, it even threatens to create a situation where cost considerations will make it necessary to abandon the Moon when the time comes to proceed to Mars. By contrast, if a common transportation system were designed instead, both destinations could continue to be explored in parallel.

The plan's fourth major flaw is that it is fundamentally technically unsound. It goes to great lengths to avoid the necessity of developing a heavy lift vehicle, employing (as described above) an astonishingly complicated mission architecture involving four rocket launches and four space rendezvous for each lunar mission—what we might call a "quadruple launch, quadruple rendezvous" (QQ) mission architecture.

Using some reasonable estimates based upon the masses of the primary components of the Apollo mission, it can be shown that it is technically possible that a QQ mission could be launched on four medium launch vehicles. But is it technically wise? Note the following factors:

i. Each mission requires four MLV launches.
ii. Those four launches must be done quickly, since the EDS and LSAM vehicles are carrying cryogenic liquid oxygen and hydrogen, and the manned CEV is launched last.
iii. Each mission requires four critical rendezvous operations.
iv. The crew flies to the Moon separate from the lunar module.

Point i speaks to the cost of the program. Using multiple MLVs to launch what could be a single HLV payload is not cost-effective. It is a basic feature of rocket economics that larger boosters are more economic than smaller boosters. The larger the launch vehicle, the less it costs to put each kilogram into orbit. So, for example, the Atlas V 500 is more than twice as economical a launch system as the Atlas IIAS, and cost projections for the next-generation HLV on the drawing boards based on the Atlas series are more than twice as economical as those for the Atlas V 500. The basic lesson here is that by adopting a strategy of multiple MLV launches, the plan will maximize rather than contain the program's launch costs.

Points ii and iii speak to feasibility. The program requires four MLV launches within just a few weeks. Three of those launches would involve cryogenic upper stages, and the fourth would involve a manned vehicle, all launched from Cape Canaveral. Such an MLV launch rate has never been accomplished with any payload and to assume that it can be done repeatedly with payloads of this complexity is wildly optimistic.

Points i, ii, and iii also speak to both complexity and mission risk. In contrast to the old Apollo mission plans, which required only one launch and a single rendezvous, the QQ plan requires four mission-critical launches and four mission-critical rendezvous. Each must be successful. That's eight big chances (in addition to lunar landing and ascent) for an operational failure that would ruin the mission.

In fact, the mission architecture is so complexly interdependent—and therefore so fragile—that a huge number of potential problems could end any given mission. The mission would fail if a mere launch delay caused *any* of the last three launches to stall so long that the propellant aboard the first payload runs out. The mission would fail if *any* of the four orbiting payloads were damaged by orbital debris while waiting in low

Earth orbit. The mission would fail if *any* of the four spacecraft should seriously malfunction. The mission would fail if *any* of the four orbital rendezvous operations failed. The mission would fail if *any* of the four engine burns needed to reach the Moon and get into lunar orbit underperformed. Just think: This mission architecture is supposed to support not just one lunar mission, but routine, repeated access to the Moon. Inserting so much complexity and vulnerability into such a transportation system is an open invitation to failure.

It is even possible to assign some rough figures to this vulnerability. Let's assume that the rockets used for this new space program will each have a 98 percent success rate. (In real life, a study of the historical reliability of the U.S. Delta, Atlas, and Titan medium lift vehicles shows a success rate of only about 90 percent.) And let's assume that that each of the major operations in space—each rendezvous and engine burn—has a 99 percent success rate. And let's generously assume that there is a 98 percent chance that each of the last three rocket launches happens on time, and a 98 percent chance that the lunar landing is successful. Forget all the other potential failure points. Just calculating from those few assumptions, each mission would only have an expected 75 percent success rate. This means that roughly one out of every four missions could be expected to fail. If three missions are flown per year, there would, on average, be mission failure roughly every 1.3 years. Assuming a typical suspension of operations of two years after each mission failure, the program would need to be shut down for failure investigations at least 60 percent of the time.

Point iv speaks to the risks to crew. Apollo traveled to the Moon with the lunar module attached to the command module. This made the lunar module available to each crew as an emergency safe haven—which is precisely what famously saved the lives of the Apollo 13 astronauts. Had the Apollo program used a system similar to that proposed in the QQ plan, the crew of Apollo 13 would have died.

The central reason why the QQ mission architecture has such low reliability is because of the incredible proliferation of critical events that occurs if four launches, four rendezvous, and four spacecraft are required for each mission. Fortunately, the way to solve this problem is simple: Develop a heavy lift vehicle (HLV) that allows the entire mission to be launched with a single booster, just as was done for the Apollo missions. This would greatly reduce program launch costs and reduce the risk of mission failure by a factor of four. It would also create a system directly useful to sending humans to Mars, which is a key requirement of the president's directive.

Regrettably, in designing this mission architecture, the ESMD planners had to act in conformity with the direction of the technically unqualified Mr. O'Keefe, who enunciated a preference that the program be conducted without heavy lift vehicles. Such politically dictated technical decision-making is unacceptable; it is a formula for programmatic catastrophe.

Fortunately, this complicated plan is just a starting point in the design process; the ESMD is not committed to it. But it is imperative that they depart from this plan as rapidly as possible, because vacillation risks missing a tremendous technological opportunity. One of the cheapest ways to create a heavy lift vehicle is by converting the shuttle. The shuttle launch stack has the same takeoff thrust as the powerful Saturn V rocket that put American astronauts on the Moon during the Apollo era. Since the Saturn V was imprudently cancelled decades ago, the United States has had no heavy lift vehicle. But by adapting the shuttle—removing the orbiter and adding an upper stage—we can create a launch vehicle with a capability comparable to the Saturn V.

And this is precisely why delay is so dangerous. Under NASA's current plans, only about twenty-five more shuttle launches are contemplated. Absent a plan for shuttle conversion to a heavy lift vehicle, much of the industrial infrastructure for manufacturing key shuttle components, such as external tanks, will soon be dismantled. We will be repeating the mistake of the Saturn V cancellation. Recreating such capabilities after they have been lost will cost the taxpayers billions.

Like Mr. O'Keefe's fake Hubble robotic rescue proposal, the spurious QQ mission plan merely serves to lull policy makers while critical capabilities are being lost. If such massive waste is to be avoided, NASA needs to make the case for heavy lift vehicles immediately. But it is difficult to justify the development of a heavy lift vehicle if flight operations for that system are not to begin until 2020. Thus we encounter again the fundamental problem with President Bush's policy. By postponing the program's goals until far in the future, important capabilities that could be used to achieve those goals will be lost before the time comes for those goals to be attempted. Under the current plan, Spiral 1 might succeed, at maximum cost, in producing a CEV in ten years. But in the meantime, the heavy lift vehicle components embodied in the shuttle program will have been lost. As a result, in 2014, NASA will actually possess a *smaller* fraction of the hardware needed to send humans to the Moon than it does today. A decade will have gone by, along with some hundred and fifty billion dollars spent on the space program, to achieve *negative* progress overall.

Arbitrarily stretching out the program may appear to be convenient from a political point of view, as it avoids the necessity of asking for large funding increases in any particular year. But from the point of view of anyone attempting to achieve the program's mission, it is the equivalent of an order to conduct a cavalry charge in slow motion: it maximizes the losses.

The Right Way to Mars

So far we have discussed the problems that have caused NASA to drift for the past thirty years, how those problems came to the fore in the aftermath of the *Columbia* disaster, and the efforts of the administration to address those endemic problems. As we have seen, the resulting new space policy, while clearly a step in the right direction, includes so many compromises with the old way of doing business that a positive outcome remains in doubt. We must now address the question of how a rational human space exploration initiative should be done.

It is not enough that NASA's human exploration efforts "have a goal." The goal selected needs to be the *right* goal, chosen not because various people are comfortable with it, but because there is a real reason to do it. We don't need a nebulous, futuristic "vision" that can be used to justify random expenditures on various fascinating technologies that might plausibly prove of interest at some time in the future when NASA actually has a plan. Nor do we need strategic plans that are generated for the purpose of making use of such constituency-based technology programs. Rather, the program needs to be organized so that it is the goal that actually drives the efforts of the space agency. In such a destination-driven operation, NASA is forced to develop the most practical plan to reach the objective, and on that basis, select for development those technologies required to implement the plan. Reason chooses the goal. The goal compels the plan. The plan selects the technologies.

So what should the goal of human exploration be? In my view, the answer is straightforward: Humans to Mars within a decade. Why Mars? Because of all the planetary destinations currently within reach, Mars offers the most—scientifically, socially, and in terms of what it portends for the human future.

In scientific terms, Mars is critical, because it is the Rosetta Stone for helping us understand the position of life in the universe. Images of Mars taken from orbit show that the planet had liquid water flowing on its surface for a period of a billion years during its early history, a duration five times as long as it took life to appear on Earth after there was liquid water here. So if the theory is correct that life is a naturally occurring phenomenon, emergent from chemical complexification wherever there is liquid water, a temperate climate, sufficient minerals, and enough time, then life should have appeared on Mars. If we go to Mars and find fossils of past life on its surface, we will have good reason to believe that we are not alone in the universe. If we send human explorers, who can erect drilling rigs which can reach underground water where Martian life may yet persist, we will be able to examine it. By doing so, we can determine whether life on Earth is the pattern for all life everywhere, or alternatively, whether we are simply one esoteric example of a far vaster and more interesting tapestry. These things are truly worth finding out.

In terms of its social value, Mars is the bracing positive challenge that our society needs. Nations, like people, thrive on challenge and decay without it. The challenge of a humans-to-Mars program would be an invitation to adventure to every young person in the country, sending out the powerful clarion call: "Learn your science and you can become part of pioneering a new world." This effect cannot be matched by just returning to the Moon, both because a Moon program offers no comparable potential discoveries and also because today's youth cannot be inspired in anything like the same degree by the challenge to duplicate feats accomplished by their grandparents' generation.

There will be over a hundred million kids in our nation's schools over the next ten years. If a Mars program were to inspire just an extra one percent of them to pursue a scientific education, the net result would be one million more scientists, engineers, inventors, and medical researchers, making technological innovations that create new industries, find new cures, strengthen national defense, and generally increase national income to an extent that utterly dwarfs the expenditures of the Mars program.

But the most important reason to go to Mars is the doorway it opens to the future. Uniquely among the extraterrestrial bodies of the inner solar system, Mars is endowed with all the resources needed to support not only life but the development of a technological civilization. In contrast to the comparative desert of the Moon, Mars possesses oceans of water frozen into its soil as ice and permafrost, as well as vast quantities of carbon, nitrogen, hydrogen, and oxygen, all in forms readily accessible to those clever enough to use them. These four elements are the basic stuff not only of food and water, but of plastics, wood, paper, clothing—and most importantly, rocket fuel.

In addition, Mars has experienced the same sorts of volcanic and hydrologic processes that produced a multitude of mineral ores on Earth. Virtually every element of significant interest to industry is known to exist on the Red Planet. While no liquid water exists on the surface, below ground is a different matter, and there is every reason to believe that underground heat sources could be maintaining hot liquid reservoirs beneath the Martian surface today. Such hydrothermal reservoirs may be refuges in which survivors of ancient Martian life continue to persist; they would also represent oases providing abundant water supplies and geothermal power to future human settlers. With its 24-hour day-night cycle and an atmosphere thick enough to shield its surface against solar flares, Mars is the only extraterrestrial planet that will readily allow large scale greenhouses lit by natural sunlight. In other words: Mars can be settled. In establishing our first foothold on Mars, we will begin humanity's career as a multi-planet species.

Mars is where the science is, Mars is where the challenge is, and Mars is where the future is. That's why Mars must be our goal.

How Do We Get There?

Some may say that human exploration of Mars is too ambitious a feat to select as our near-term goal, but that is the view of the faint of heart. From the technological point of view, we're ready. Despite the greater distance to Mars, we are *much* better prepared today to send humans to Mars than we were to launch humans to the Moon in 1961 when John F. Kennedy challenged the nation to achieve that goal—and we got there eight years later. Given the will, we could have our first teams on Mars within a decade.

The key to success is rejecting the policy of continued stagnation represented by senile Shuttle Mode thinking, and returning to the destination-driven Apollo Mode of planned operation that allowed the space agency to perform so brilliantly during its youth. In addition, we must take a lesson from our own pioneer past and adopt a "travel light and live off the land" mission strategy similar to that which has well-served terrestrial explorers for centuries. The plan to explore the Red Planet in this way is known as Mars Direct. Here's how it could be accomplished.

At an early launch opportunity—for example 2014—a single heavy lift booster with a capability equal to that of the Saturn V used during the

Apollo program is launched off Cape Canaveral and uses its upper stage to throw a 40-tonne unmanned payload onto a trajectory to Mars. (A "tonne" is one metric ton.) Arriving at Mars eight months later, the spacecraft uses friction between its aeroshield and the Martian atmosphere to brake itself into orbit around the planet, and then lands with the help of a parachute. This is the Earth Return Vehicle (ERV). It flies out to Mars with its two methane/oxygen driven rocket propulsion stages unfueled. It also carries six tonnes of liquid hydrogen, a 100-kilowatt nuclear reactor mounted in the back of a methane/oxygen driven light truck, a small set of compressors and an automated chemical processing unit, and a few small scientific rovers.

As soon as the craft lands successfully, the truck is telerobotically driven a few hundred meters away from the site, and the reactor is deployed to provide power to the compressors and chemical processing unit. The ERV will then start a ten-month process of fueling itself by combining the hydrogen brought from Earth with the carbon dioxide in the Martian atmosphere. The end result is a total of 108 tonnes of methane/oxygen rocket propellant. Ninety-six tonnes of the propellant will be used to fuel the ERV, while 12 tonnes will be available to support the use of high-powered, chemically-fueled, long-range ground vehicles. Large additional stockpiles of oxygen can also be produced, both for breathing and for turning into water by combination with hydrogen brought from Earth. Since water is 89 percent oxygen (by weight), and since the larger part of most foodstuffs is water, this greatly reduces the amount of life support consumables that need to be hauled from Earth.

With the propellant production successfully completed, in 2016 two more boosters lift off from Cape Canaveral and throw their 40-tonne payloads towards Mars. One of the payloads is an unmanned fuel-factory/ERV just like the one launched in 2014; the other is a habitation module carrying a small crew, a mixture of whole food and dehydrated provisions sufficient for three years, and a pressurized methane/oxygen-powered ground rover.

Upon arrival, the manned craft lands at the 2014 landing site where a fully fueled ERV and beaconed landing site await it. With the help of such navigational aids, the crew should be able to land right on the spot; but if the landing is off course by tens or even hundreds of kilometers, the crew can still achieve the surface rendezvous by driving over in their rover. If they are off by thousands of kilometers, the second ERV provides a backup.

Assuming the crew lands and rendezvous as planned at site number one, the second ERV will land several hundred kilometers away to start making propellant for the 2018 mission, which in turn will fly out with an additional ERV to open up Mars landing site number three. Thus, every other year two heavy lift boosters are launched, one to land a crew, and the other to prepare a site for the next mission, for an average launch rate of just one booster per year to pursue a continuing program of Mars exploration. Since in a normal year we can launch about six shuttle stacks, this would only represent about 16 percent of the U.S. heavy-lift capability, and would clearly be affordable. In effect, this "live off the land" approach removes the manned Mars mission from the realm of mega-spacecraft fantasy and reduces it in practice to a task of comparable difficulty to that faced in launching the Apollo missions to the Moon.

The crew will stay on the surface for 1.5 years, taking advantage of the mobility afforded by the high-powered chemically-driven ground vehicles to accomplish a great deal of surface exploration. With a 12-tonne surface fuel stockpile, they have the capability for over 24,000 kilometers worth of traverse before they leave, giving them the kind of mobility necessary to conduct a serious search for evidence of past or present life on Mars. Since no one has been left in orbit, the entire crew will have available to them the natural gravity and protection against cosmic rays and solar radiation afforded by the Martian environment, and thus there will not be the strong pressure for a quick return to Earth that plagues other Mars mission plans based upon orbiting mother-ships with small landing parties. At the conclusion of their stay, the crew returns to Earth in a direct flight from the Martian surface in the ERV. As the series of missions progresses, a string of small bases is left behind on the Martian surface, opening up broad stretches of territory to human cognizance.

In essence, by taking advantage of the most obvious local resource available on Mars—its atmosphere—the plan allows us to accomplish a manned Mars mission with what amounts to a lunar-class transportation system. By eliminating any requirement to introduce a new order of technology and complexity of operations beyond those needed for lunar transportation to accomplish piloted Mars missions, the plan can reduce costs by an order of magnitude and advance the schedule for the human exploration of Mars by a generation.

The Lunar Architecture

Since a lunar-class transportation system is adequate to reach Mars using this plan, it is rational to consider a milestone mission, perhaps five years into the program, where a subset of the Mars flight hardware is exercised to send astronauts to the Moon.

This can be done as follows: First, a single booster is used to launch an unmanned habitation module which is landed on the Moon. Then, another booster is launched, sending the crew to the lunar surface in a CEV equipped with a methane/oxygen driven ascent stage which is capable of propelling it directly back to Earth. The crew lands near the pre-placed habitation module, which they then use as their house and laboratory on the Moon for an extended duration stay, after which they transfer back to the CEV and return to Earth.

This approach is much preferable to the QQ approach, because only one launch and no orbital rendezvous are required per mission, and a substantial habitat and laboratory are available to the crew starting on the very first mission. This enhances crew safety, and will make missions much more productive scientifically, as they will be able to stay longer and be much better equipped to conduct research while they are there. Furthermore, from the surface of the Moon, the launch window back to Earth is always open, as there are no orbital rendezvous phasing issues, further adding to the safety of the crew.

If the objective is to establish a permanent lunar base and not just to perform sorties to the Moon, then the production of lunar oxygen is feasible (by reducing the oxides of iron that comprise about 10 percent of Moon dirt); because of the numerous advantages it offers, this should be an early priority. If we want to visit multiple lunar sites, the most effective way is not to launch individual missions from Earth, but to employ a small rocket-powered ballistic flight vehicle—a "hopper"—operating out of the lunar base camp. Using the fuel delivered from Earth by a single heavy lift vehicle, such a hopper could make six long-range excursions if it used methane/oxygen propulsion, or ten excursions if it used hydrogen/oxygen propulsion. This compares quite handsomely to the QQ plan, which requires four major launches from Earth to visit just one site.

Thus we see that proper design of a coherent human exploration initiative allows not only Mars missions, but cost-effective lunar activities as well, using a modified subset of the Mars hardware. Approaching the design issue in this way can sharply cut overall program cost, risk, and schedule, because only one fundamental hardware set needs to be developed instead of two, and the lunar activities can be used to validate Mars mission hardware directly. This makes the rationale for the lunar missions clear, and makes it possible to continue lunar activities even after Mars missions begin, as only one transportation system will need to be supported.

The Need for Speed

Clearly, I have suggested some rather near-term dates for the human Mars mission, in significant contrast to various NASA "roadmapping" charts which situate this accomplishment sometime in the middle of the twenty-first century. Yet it should be observed that the first Americans walked on the Moon not after the hundredth anniversary of Sputnik, but before the twelfth. Indeed, it was the speed of the Apollo program that was the central factor in the program's success.

In 1961, President Kennedy committed the nation to reach the Moon before the end of the decade, and we did. But consider what would have happened if instead of choosing 1970 as his deadline, JFK had selected 1990. Had we then proceeded in such a more leisurely way, 1968 would not have seen Apollo 8 ready on the launch pad, but perhaps one of the later Mercury one-man capsule flights. But in 1968, the national mood was totally different from the Camelot era. We were in the Vietnam War, hundreds of thousands of protesters were marching in the streets, and, at the end of the year, a different party won the White House. Under those conditions, the tepid nominal Moon effort almost certainly would have been cancelled—as in fact Nixon did cancel the quite successful Apollo program in real life. Clearly, if Kennedy had set his sights on the Moon in thirty years, we would not have made it there at all.

The issue, however, goes beyond the intrinsic difficulty of maintaining a political consensus in support of a program over multiple decades. There is also the matter of forcing the required technical focus for success. To use an analogy, think of two posts separated by a certain distance, say ten meters. How much rope is needed to connect them? It could take many kilometers, if the rope is allowed to be slack or tangled. Alternatively, it could be done with about ten meters, but only if the rope is pulled tight.

The Apollo era was filled with just as much human weakness as our own time. There were companies and NASA centers that were self-interested, and technologists that were obsessed with their own hobby horses. Early in the program, many fanciful and overly complex ideas were advanced on how to reach the Moon, but very rapidly, the impending deadline forced nearly all of them out. For Apollo, it was the tight schedule that tightened the rope.

It is just the same today. Mention humans-to-Mars within the NASA community, and you will be deluged with proposals for space stations and fuel depots in various intermediate locations, fantastical advanced propulsion technologies, and demands that billions upon billions of dollars be spent on an infinite array of activities which define themselves as necessary mission precursors. Representatives of such interests sit on various committees which write multi-decade planning "roadmaps" and exert every effort to make sure that the "roads," as it were, go through their own hometowns. Under such conditions it takes not kilometers, but light years, of line to connect the posts. If we are actually to make it to Mars, however, the rope needs to be pulled tight, and only a tight schedule will suffice to do that job.

It is unreasonable today to spend ten years to develop a CEV, when in the 1960s we did it in five, or sixteen years to reach the Moon, when two generations ago we did it in eight. Embarking on the program in such a dilatory way will cost us the heavy lift hardware of the shuttle, which is something we can ill-afford. To believe that such slow-paced achievement is the best we can do means believing that we have become less than the people we used to be, and that is something we can afford even less.

Exploring Mars requires no miraculous new technologies, no orbiting spaceports, and no gigantic interplanetary space cruisers. We don't need to spend the next thirty years with a space program mired in impotence, spending large sums of money on random projects and taking occasional casualties while the missions to nowhere are flown over and over again, and while professional technologists dawdle endlessly in their sandboxes without producing the needed flight hardware. We simply need to choose the right destination, and with the same combination of vision, practical thinking, and passionate resolve that served us so well during Apollo, do what is required to get us there.

We can establish our first small outpost on Mars within a decade. We, and not some future generation, can have the honor of being the first pioneers of this new world for humanity. All that is needed is present day technology, some nineteenth-century industrial chemistry, a solid dose of common sense, and a little bit of moxie.

Why Now? Why Us?

So we can do it, and it should be done, but why should we be the ones to do it? Why, at a time like this, with the nation at war, with new menaces threatening to appear in various corners of the globe, and our allies drifting away, should the United States government expend serious resources on such a visionary enterprise? In my view, such considerations simply make the matter all the more urgent.

While I would not deny the necessity of military action in certain circumstances, in the long run civilizations are built by ideas, not swords.

The central idea at the core of Western civilization is that there is an inherent facility in the individual human mind to recognize right from wrong and truth from untruth. This idea is the source of our notions of conscience and science, terms which, not coincidentally, share a common root.

Both our radical fundamentalist and our totalitarian enemies deny these concepts. They deny the validity of the individual conscience, and they deny the necessity of human liberty, and indeed, consider it intolerable. For them, conscience, reason, and free will must be crushed so that humans will submit to arbitrary and cruel authority.

Against this foe, science is our strongest weapon, not simply because it produces useful devices and medical cures, but because it demonstrates the value of a civilization based upon the use of reason. There was a time when we celebrated the divine nature of the human spirit by building Gothic cathedrals. Today we build space telescopes. Science is our society's sacred enterprise; through it we assert the fundamental dignity of man. And because it ventures into the cosmic realm of ultimate truth, space exploration is the very banner of science.

If the United States is to lead the West, it must not only carry its sword, but the banner of its most sacred cause. And that cause is the freedom to explore on the wings of human reason. The French may sneer, with some cause, at our fast food restaurants and TV sitcoms, but the Hubble Space Telescope can inspire nothing but admiration, or even awe, in anyone who is alive above the neck. A human Mars exploration program would be a statement about ourselves, a reaffirmation that we remain a nation of pioneers, the vanguard of humanity, devoted to the deepest values of Western civilization. But even more, it would be a declaration of the power of reason, courage, and freedom writ clear across the heavens.

Now, more than ever, we need to make those statements. Now, more than ever, we need to sign that declaration—in handwriting large enough that no one will need spectacles to read it.