Appendix 2 Continuing the Fight for Mars Direct, 2001 -2011

The following articles are a fair sampling of my continued advocacy for Mars Direct over the first decade of the 21st Century. Collectively, they represent a kind of chronicle of the debate within and around the space community concerning human exploration, and the many issues that were encountered and had to be dealt with if the prospects for human Mars exploration were to advance. Parts of them are necessarily repetitious (such as the summary description of the Mars Direct plan that is included in many of them), but a great deal of new material is presented as well. For example, they include extensive discussions of the lunar base initiative proposed by the Bush administration, the fight to save the Hubble Space Telescope, and the controversy surrounding the decision by the Obama administration to redirect NASA back to a non-destination driven mode of operation. Unfortunately, except for the successful efforts to save Hubble and the Mars Science Lab, the story they relate is not a happy one. Indeed, while the science-driven robotic Mars exploration program accomplished much over the period in question, NASA's human space exploration program, operating without (or in willful denial of) any rational plan, is no closer today to sending humans to Mars than it was in 2001 (or, arguably, 1971 for that matter).

Still, there is much to be learned from studying any battle, and perhaps more from a defeat than from a victory. We came close, during the past decade, to getting a humans to Mars program launched, but, at the end of the day, the opportunity was blown. As the German writer Friedrich Schiller once famously said of the French Revolution, "A great moment found a little people." Hopefully we will do better next time, because there will certainly be a next time. For to paraphrase the most celebrated speech of a Frenchman of a more recent time, who may have been many things, but certainly not little, Charles De Gaulle; Mars has lost the battle, but Mars has not lost the war.

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The first decade of the 21st Century has been a period of crisis, which, beginning with the September 11 2001 attack on the Pentagon and the World Trade Center, continues today. I understand that in Chinese, the same word symbol is used to signify “crisis” and “opportunity.” Acting on this wisdom, I attempted to pose a humans to Mars program as a key weapon in the War on Terror. This is not as outlandish an idea as it seems. After all, who would have expected beforehand that the consequence of the failure of the Bay of Pigs invasion would be Americans walking on the Moon 8 years later? The people who conceived and advocated Apollo did so because they believed in the vision of humanity expanding into space, but the people who approved the program did so because they saw it as a way to combat communism – and they were right. I can say that as fact, because I was in Leningrad studying Russian when Apollo 11 landed, and its psychological effect on those behind the Iron Curtain was everything American anti-communist hawks could have asked for – and more. Certainly Apollo did much more to win the Cold War than did our simultaneous military effort in Vietnam, and at much lower cost in blood and treasure. Why not use the same approach to help take down Islamic fundamentalism. In the following article, I make the case.

Victory from Space

Space News, Sept 24, 2001

How can America's space program contribute to victory? There are a number of obvious ways. Our reconnaissance satellites will spot the terrorist encampments, our navigation satellites will guide us to them, and our communication satellites will allow us to coordinate our forces to assure that they prevail in combat. To previous invaders, Afghanistan was a maze of death. Because of our spacecraft, however, we will be able to view the maze from above, rather than within.

These are critical capabilities. They will provide the essential margin needed to eliminate Bid Laden's guerillas before they can strike too many more and deadlier blows.

Unfortunately, however, the enemy is not just a few thousand cultists. It is a cult. To defeat the enemy, we must not only destroy its current forces, we must discredit the ideology that allows it to recruit. We are not at war with a handful of savages. We are at war with an idea.

The September 11 attack on the World Trade Center signaled the beginning of a war by Fundamentalist Islam against the West. Why does Fundamentalist Islam hate the West? It hates the West because of its core beliefs.

The central idea behind western civilization is the radical proposition advanced by the Greek philosopher Socrates that there is an innate faculty of the human mind capable of distinguishing right from wrong, justice from injustice, truth from untruth. This idea was embraced by early Christianity as the basis of the concept of the conscience, which thereupon became the axiomatic foundation of western morality. It is also the basis of our highest notions of law ("We hold these truths to be self evident…") and science, man's search for universal truth through the tools of reason.

Fundamentalist Islam denies all of this. It denies the existence or deserved authority of the conscience. Instead, right and wrong can only be known through the Koran, as interpreted by Fundamentalist mullahs. It denies moral responsibility further because it denies the existence of free will. It denies reasoned investigation of nature completely because it denies the idea of causality. Instead, it argues that the universe is created and destroyed repeatedly in every succeeding instant by the will of Allah. Thus scientific activity is useless, and in fact is proscribed.

It should thus be clear why Fundamentalist Islam is at war with the West. But the West has not been its first target. Its first victim was rationalist Islam.

In the Islamic world, the fundamentalists have not always been on top. In its formative period, Islamic society included a strong rationalist current led by the Mu'tazilites, who believed in the parity of reason and revelation, and produced many profound philosophers such as Al Farabi, Averroes (Ibn Rushd), and Avicenna (Ibn Sina.) A thousand years ago, it was not the West, but Islam, that had the broadest intellectual horizons. Islamic thinkers created algebra and radically advanced astronomy and medicine. At a time when there were no colleges in Europe, the Islamic world had hundreds. At a time when the largest European libraries contained a few hundred volumes, there were Islamic libraries with hundreds of thousands.

But then the fundamentalists took over. The philosophers were made into fugitives. Scientific inquiry was banned. Libraries that were found to contain scientific works were burnt. Printing, which appeared briefly in the Islamic world several hundred years before its advent in Europe, was banned, and did not reappear until its reintroduction by American missionaries in the 1830's.The colleges were turned from centers of inquiry in mental slaughterhouses where generation after generation of the brightest youth were made to rote memorize the Koran.

With the fundamentalist takeover, the most glorious civilization humanity had ever known was turned into a wasteland of misery, mental slavery, degradation, and ignorance. A quarter of the world was turned into a graveyard of the mind, which for the past 700 years has not produced a single significant scientific advance.

It is thus ironic to hear the arguments of the apologists for Fundamentalist terror who claim that the terrorists actions are some kind of counterattack against the immiseration of the Islamic world supposedly caused by the West, or even more absurdly, the minute state of Israel. In fact, it is the internal combustion engines invented and manufactured in the West that have for the better part of the past century supplied the Islamic world with its only significant source of wealth. No, the poverty and degradation of the Islamic world has been caused solely by the fact that those within it who would use reason to advance its condition have been suppressed by fundamentalism.

I believe we need to use science to defeat not only the fundamentalists, but fundamentalism itself. A grand work of reason is not simply an object of utility, but a celebration of the human spirit. This is nowhere more true than when man looks out into space to attempt to comprehend the universe itself. As the Renaissance scientist Johannes Kepler, the discoverer of the laws of planetary motion put it; “Geometry is one and eternal, a reflection out of the mind of God. That mankind shares in it is one reason to call man the image of God.”

There it is. The human mind, because it is the image of God, is able to understand the laws of the universe. It was the forceful demonstration of this proposition by Kepler, Galileo, and others that let loose the scientific revolution in the West.

But works of reason can be more than contemplative, they can be creative. Consider the object of the terrorists rage; the World Trade Center. A triumph of the human mind, the WTC was the most recent of the series of astonishing feats of civil engineering New York City has shown the world over the past 118 years. These architectural marvels have their uses, but their value goes much deeper. The creator of the first of them, Johann Roebling, designer of the Brooklyn Bridge, said it well; “No one will be able to look at it and not feel prouder to be a man.”

Prouder, indeed. Roebling’s bridge doesn’t just have Gothic arches, it’s a Gothic cathedral, whose unprecedented span and poetic form constitute a soaring salute to the power of the human mind.

But some people haven’t gotten the message yet. So I propose we hit harder. Let’s build a gothic cathedral whose significance no one can miss. Kepler et al showed that we could understand the heavens. Let’s drive the point home by using our space program to show that we can navigate them, or better yet, take possession.

There are those who, panic stricken in the current crisis, would gut our space exploration programs. This makes no more sense than a decision to tear down our skyscrapers. In fact, it’s worse, because it would undermine our war effort.

To defeat fundamentalism we need to do more than hunt down its current batch of expendable pawns. We need to utterly humiliate the doctrine itself by demonstrating for all to see the sublime and infinite power of human reason.

So let’s send probes to Europa and humans to Mars. Better yet, let’s settle Mars, and bring the dead planet to life. Let’s show that we can not only understand creation, but continue its process, by transforming barren worlds into new homes for life and civilization.

This is no time for science to retreat; it must attack. Let’s launch an offensive to free forever the minds of all men from fundamentalist tyranny. A universe open to humanity would be a hymn to reason writ large across the firmament. It would be the key to true victory. Because no one will be able to look upon it and not feel prouder to be human.

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Alas, President Bush and his coterie did not take my advice, electing instead for a purely military strategy, with consequences that are well known. However at a conference I attended during late September 2002 at MIT, noted space reporter Leonard David made the observation that if the administration believed that Osama Bin Laden was hiding on Mars, we'd get there in no time. Hearing this, I thought the idea was at least worth a try. So, I submitted an article to that effect to an appropriate publication, one moreover, more likely to have readers among high-level decision makers in the White House than *Space News*. Here it is.

Osama Bin Laden Found, On Mars!

Dan Shepherd (pseudonym for RZ)

Special to the *Weekly World News*

Submitted Oct 8, 2002

According to inside sources within the US government, the secret hideout of America's number one enemy, notorious Islamic terrorist Osama Bin Laden has been found – on the planet Mars.

Bin Laden is widely believed to have been the mastermind and funding source for the September 11 airplane hijackings that destroyed the World Trade Center, damaged the Pentagon, and left over 3,000 people dead. He was almost caught last fall, when US and allied Northern Alliance forces assaulted the Taliban's last-stand stronghold of Tora Bora. However, as a result of what many have termed serious errors of military judgment, he was able to take advantage of the Tora Bora area's difficult mountain terrain to make his escape. A global manhunt to catch the deadly terrorist has been underway ever since.

However, until this week, there were no clues to Bin Laden's whereabouts. As one Central Intelligence Agency official explained the matter during a special confidential press briefing in late September, "The situation is really spooky. We know he is not dead, because if he was, the network [of terrorist radio communications, WWN] would be wild with chatter as they fight to see who succeeds him. But we can't find him anywhere. It's like he's left the planet."

Unknowingly, the CIA man appears to have hit the nail right on the head. The break in the story occurred on October 2, when Malkin Space Imaging (MSI), of Santa Cruz, CA, released a set of photographs that had been taken in May by NASA's Mars Odyssey spacecraft. MSI holds the NASA contract for analysis of photographs taken by the Mars probe, and has displeased many scientists working in the Mars research field by embargoing large numbers of images from public release without giving satisfactory reasons. However, on October 2, under threat of legal action from a consortium of four major universities with an interest in the data, MSI made available a CD containing over 400 high resolution images of the region surrounding Mars' South Pole.

The images set off a firestorm. As Dr. Rick Greenfield, of the Flagstaff Arizona based Mars Research Institute put it; “The pictures were amazing. We all knew that Malkin was hiding something, but we never imagined it could be anything like this.”

Through exclusive sources, the Weekly World News has learned that what the images showed were a complex of buildings, including habitation structures, chemical processing units, a large array of greenhouses, several nuclear reactors, a spaceport with pads for up to six rockets, and a mosque.

CIA officials reached for comment by the Weekly World News played down the significance of the presence of a mosque at the previously secret Mars base. “The fact that there is a mosque there does not necessarily imply the presence of Bin Laden,” one senior official who asked to remain anonymous said. “Any Islamic fundamentalist organization that created a Mars base would almost certainly include a mosque.”

However other observers were quick to note that among Islamic fundamentalists, only Bin Laden has both the resources and the motivation needed to establish a Mars base.

The location of the base near the southern Martian pole also appears to be of strategic significance. According to Dr. Christopher McPhee of NASA’s Jet Propulsion Laboratory, the Mars Odyssey spacecraft has also discovered large quantities of water in the Martian Antarctic. “Water is the key to life,” Dr. McPhee explained. “The amount of water we have found in Mars’ southern polar region is enough to support sufficient greenhouses to feed an army.”

Officers at the Special Operations Division of the Air Force Space Command (SODAFSC) in Colorado Springs commented that McPhee had only seen part of the picture. “There is much more here than meets the eye,” explained one veteran of SODAFSC’s reconnaissance office. “You will note that the number of reactors, chemical plants, and greenhouses is greatly in excess of that needed to support the limited number of personnel who could be housed in these habitation structures. This base is being used to supply food and fuel for other bases, possibly as many as a dozen other bases."

Lieutenant Colonel Frank Browning from the SODAFSC tactical wing concurred. "With the water, power, and chemical industrial facilities we see at that base, you could make enough rocket fuel to power a fleet of spacecraft," he said [If split by electricity, water can be turned into hydrogen and oxygen, the best rocket propellant mixture known – WWN]. There is no doubt about it. Osama's got his boys in the asteroid belt, and is supplying them from Mars."

We asked Lt. Col Browning what the terrorists might be doing in the asteroid belt. "Let me put it to you this way," he explained. "The asteroid belt is a huge collection of giant rocks floating around in space just beyond Mars. You get a spaceship with enough fuel, and push one onto a collision course with Earth. Then when it hits, you'll get an exploration bigger than a million hydrogen bombs. If you want to destroy human civilization, you couldn't find a better weapon."

"We've got to take that base out," Lt. Col Browning added. "The NASA folks say they can get humans to Mars in ten years. But I'm not sure we have ten years."

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Unfortunately, the above excellent piece was declined by the editor, on technical grounds, as he did not believe that humans could fly from Earth to Mars is less than two years, and nothing I could say would convince him otherwise. Osama Bin Laden remains at large.

While not beginning a major human space exploration initiative of any type, the administration did, however, create a group, called the NASA Exploration Team (NExT), to plan for the next such program. Unfortunately the NExT team had to report to President Bush's first

NASA administrator, the technically illiterate bureaucrat Sean O'Keefe, whose dictum was, incredibly, "NASA should not be destination-driven." This absurdity was fully adhered to in the NExT group's proposed "space architecture," which was a hyper-complex plan for going nowhere for a long time at great expense. In the following article I dissected the flaws of such thinking, and argued for a simpler and more effective approach.

NASA NExT Program Needs a Destination

Space News Oct 2002

At the recent World Space Congress the NASA Exploration Team (NExT) program revealed its new strategy for planning human operations in the inner solar system. The NExT plan contains many useful and important recommendations, such as calling for the development of space nuclear power and research into artificial gravity. However the mission architecture proposed by the NExT group is not supportable, and needs to be reworked if America is to have a viable plan for moving forward in space.

The core of the problem with the NExT mission architecture is the proclaimed notion that it should "not be destination-driven." Indeed, it is positively destination-adverse. The claim is made that freedom from destinational constraints allows the program to be "science-driven" instead, which surely would be admirable. But this is not what has occurred. A science-driven program would drive mission planners to send human explorers to those locations where they offer unique scientific capabilities, which is to say the surface of planetary objects, with the greatest priority being Mars. Instead, the lack of a chosen destination has exposed the NExT program to the entropic demands of various constituencies within the space agency, with the result being the proposal for a profusion of technological and infrastructure projects that are far off the line of direct approach to any actual scientific goals.

Let us now critique the NExT mission architecture to highlight these flaws and offer alternative, more cost effective solutions.

At the centerpiece of the NExT plan is a proposal to build a manned space station at the Earth-Moon L1 point. It is claimed that this outpost would serve as a “staging point” for further human space exploration operations. A more accurate term would be “stagnation point,” as the decades of time and tens of billions of dollars worth of cost required to build and maintain such an installation would almost certainly prevent any actual human planetary exploration from occurring within the working lifetime of anyone currently employed in the space business.

The NExT team claims that this station would be useful to service space telescopes that would supposedly shuttled back and forth to it from their operational locations at the Earth-Sun L-2 point, about a million kilometers further out. This is theoretically possible, but if you offered the space astronomy community the ~30 billion dollars it would take to construct the L1 station and the other associated infrastructure needed to implement such a scheme, and gave them the freedom to spend it any way they desired, the amount they would choose to expend building such infrastructure would be zero.

The claim is made that an L1 space station would be useful for Moon bases, because from L1 one can readily access any point on the Moon. The second half of this sentence is true, but you don’t need to have a space station at the L1 point in order to pass through it.

The NExT architecture’s L1 space station would be supported by a solar-electric space tug. Given a power level of 1,000 kilowatts, this object would have a mass on the order of 40 metric tonnes and feature solar arrays twice the size of a football field. While this unit could have

10 times the specific impulse of a chemical rocket, it would weigh and cost much more, and because of its low-thrust propulsion system, would have to employ a trajectory from LEO to L1 requiring more than twice the delta-V needed by a chemical rocket. If the mission is to deliver a payload of 40 metric tonnes from LEO to L1, a compact chemical stage using an off-the-shelf hydrogen/oxygen engine could do it in 3 days with an initial mass in LEO of the combined payload/propulsion system of 90 tonnes. If sent one way to L1, the combined initial mass in LEO of the solar electric tug system and payload would be 93 tonnes, assuming a specific impulse of 5,000 seconds. But of course we would not want to expend the huge and expensive solar-electric tug, so we would need to bring it back, which would increase the initial mission mass to 100 tonnes, with a roundtrip engine-burn time of 13 months.

If the solar electric tug were fully reusable the next 40 tonnes could be delivered to L1 with a launch mass of 60 tonnes. This would appear to offer some advantage over the recurring launch mass of 90 tonnes required by chemical propulsion, but it is questionable how many times the solar electric tug could cycle through the Earth's radiation belts without having its panels degraded, and the lifetime of the electric thrusters themselves is limited as well. Furthermore, because of the large area of its solar panels (or radiological inventory if the decision is made to make the tug nuclear) the electric tug would have to be parked in high orbits when it returned to Earth, thereby degrading the payload delivery capabilities of whatever launch vehicles are used to lift payloads to it. The net result is that the proposed fantastical Earth-Moon solar electric transportation system offers no advantages over chemical propulsion systems that are readily available today.

The NExT strategy for Mars missions is even more difficult to justify. It is proposed that Mars spacecraft be assembled at the L1 space station, and then delivered to Mars with a large nuclear-electric interplanetary space tug. This recommendation is truly baffling, because the delta-V needed to go from L1 to Mars via a low-thrust trajectory is 7,000 m/s, while the high-thrust trajectory path via a powered low-perigee Earth swingby is only 800 m/s. Thus if you were at L1, it would be crazy to use a multi-billion dollar futuristic nuclear-electric spaceship to go to Mars, because you could get there with considerably lower mass by employing an RL-10 propulsion system which costs 3 orders of magnitude less, is far more reliable, and which actually exists.

The NExT Mars plan further degrades the mission's cost-effectiveness by not having the crew land on Mars. Instead they teleoperate robots from space. In subsequent missions they do land, but only briefly. Both strategies are inappropriate to a science-driven program. In order to do science effectively, humans need to go to the surface and interact with the environment directly in a sustained program of field exploration that requires substantial periods of time.

The right way to do human space exploration is not to maximize technological and infrastructural complexity, but to minimize it. Human missions to the Moon and Mars can be readily accomplished by taking a Shuttle launch stack and replacing the orbiter with a hydrogen/oxygen upper stage. Such a vehicle would have the capability of lifting ~120 tonnes to LEO or sending payloads in the 40 tonne class on direct trajectories to the Moon or Mars. Single launches could enable lunar missions or the delivery of manned lunar base modules. Dual launches could enable human expeditions to Mars, using 19th century chemical engineering techniques to process return propellant from the Martian atmosphere. When it comes time for technological evolution, the best way to do it is by supplementing such boosters with a third stage using nuclear thermal propulsion. This would double the booster's interplanetary throw capability while still avoiding the need for the overhead of ruinously-costly in-space infrastructure or gigantic electric-propulsion megaspacecraft.

Methods of employing such practical, direct launch techniques to enable manned Moon and Mars exploration are explained at length in my book “The Case for Mars.” I strongly recommend that the members of the NExT group read it.

In 1962, NASA began the program to develop the Saturn V moon rocket. It flew 4 years later. In 1996, NASA began a program called X-33 to develop space launch technology, canceling that program without significant achievement 5 years later. If, in 1996, Dan Goldin had decided to develop a launch vehicle, instead of play around with imaginative launch vehicle technology, we would have a Shuttle-derived heavy-lift launch vehicle today. We would have in our hands the fundamental tool needed to send humans to the Moon, Mars, or Earth-Sun Lagrange points for that matter, not to mention support the nation’s commercial and defense needs in space as well.

But the Saturn V was not created by an accident of managerial genius. It was produced by the fact that in the 1960’s, America’s space program had a destination, which required the creation of such practical, capable space transportation systems.

Visionary goals can be achieved, but only by practical means. The NExT program needs a visionary destination to force it to become practical. Humans to Mars.

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Even a stopped clock is right twice a day, and O’Keefe was right about one thing – the importance of developing space nuclear power. In January 2003, he revived NASA’s program top develop space nuclear power, which had been dismantled in accord with the wishes of

President Clinton's most influential space policy maker, Vice President Al Gore. Notwithstanding my disagreements with O'Keefe on other issues, he was 100 percent right about this. I offered him my full support.

Forward with Space Nuclear Power

Space News, Feb 3, 2003

The decision by NASA to revive its space nuclear development program is a very positive step that will greatly enhance the prospects for human exploration and settlement of the solar system.

Energy is the ability to do work. Nuclear systems pack a million times the amount of energy per unit mass as chemical reactants. They therefore offer extraordinary advantages for the conduct of space activities.

Near-term nuclear thermal rocket (NTR) technologies can provide high-thrust engines with twice the exhaust velocity of the best possible chemical engines. Using such systems, the payload delivered from low Earth orbit to geosynchronous orbit, lunar orbit, or trans-Mars injection can be increased by 70 percent. Payloads delivered on direct orbits to Jupiter or beyond can be more than doubled.

Nuclear electric power reactors in the 40 to 100 kilowatt range are enabling technology for a human Mars base, where they could provide the power for reliable life support, ultra-high data rate communications, and the in-situ production of local mobility and ascent and return propellants, thereby increasing mission science return and cutting launch costs dramatically.

Beyond Mars, unmanned probes using nuclear power for high specific impulse electric propulsion, active sensing, and high data rate communication could increase mission science return enormously compared to that possible with today's technology.

Consider the following: The amount of data a spacecraft can transmit is directly proportional to its power. An outer solar system probe with a 60 kilowatt nuclear reactor will therefore be able to transmit 200 times the data as one equipped with today's standard 300 W radioisotope thermoelectric generators (RTGs). A major outer solar system probe like Galileo might cost about a billion dollars, which is roughly the expected cost of the space nuclear development effort. Yet by employing the nuclear system, a single probe can return the data that would otherwise require two hundred conventional spacecraft. *The space nuclear power development program would thus justify its cost a hundred times over, and that's just on the first mission*. If we add to this the fact that the nuclear spacecraft offers qualitatively new capabilities, such as high-powered active sensing and nuclear electric propulsion which can enable extensive maneuvering within the destination planet's system of satellites, the case for going nuclear becomes even m ore overwhelming.

The best space nuclear system would be one that combines the capabilities of high thrust nuclear thermal rockets with power generation. Conservative designs for such "dual mode" NTR systems were first put forth by John Beveridge in 1971, and have been advanced an elaborated by others, notably NASA Glenn's Dr. Stan Borowski, in the period since. Employing small NTR engines with about 15,000 lbf thrust capability, and generating about 50 kilowatts of electric power, such practical units could serve the space program as a general workhorse, enabling missions as diverse as transport of humans to Mars, providing power for lunar or Mars bases, and delivery of revolutionary high-powered spacecraft to orbits ranging from GEO to Pluto.

While public concern about the risks involved in launching nuclear power sources is appropriate, it is clear that nuclear power can be launched and operated in space without posing a safety or environmental risk to Earth. Launch of a fission reactor which has not yet been started presents no radiation hazard to Earth because, prior to reactor start, no radionuclides have been produced and so radioactivity is negligible.

There are those who have expressed concern that the nuclear initiative could lead to the "militarization of outer space." Such rhetoric is off the mark. Earth orbital space has been a critical area of practical human endeavor for the past four decades, supporting key military and commercial activities of communication, navigation, reconnaissance, and arms control verification. All useful innovations, ranging from public education to medical cures to warmer socks, invariably have military and economic applications, and space nuclear power is no exception. Space nuclear systems will therefore, no doubt, find utility in enhancing important military and commercial activities in space. This is consistent with NASA's stated mission, which, since the agency's founding, has always included contributing technology of value to the economy and national defense. However space nuclear power and propulsion systems are not weapons; they are not means of inflicting destruction, but of revealing and conveying truth. They should therefore be welcomed.

Administrator O'Keefe's decision to develop space nuclear power is a wise move that will greatly expand our space capabilities and make them far more cost-effective. The name "Project Prometheus" is well taken. Prometheus gave fire to man, giving us the power needed to create civilization on Earth. NASA's Project Prometheus will give us the power we need to extend human civilization to the heavens.

The Mars Society endorses this program and promises to support it energetically in every way we can. We urge all others interested in furthering humanity's prospects in space to do likewise.

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On February 1, 2003, NASA faced a new crisis when the Space Shuttle *Columbia* broke up during reentry with the tragic loss of all aboard. This disaster called the entire NASA human spaceflight program into question. Here is a piece I wrote to help defend it.

No Time to Cut and Run

NASA has gone 30 years without a big dream. In disaster's wake, the time has come to aim higher.

By ROBERT ZUBRIN

© St. Petersburg Times

published February 9, 2003

{note to Free Press editors from RZ. We need to get permission to use this one, as they paid me for it}

Last week, the lives of seven brave astronauts were lost when the space shuttle *Columbia* broke upon reentry. This has left the nation asking many questions that go well beyond the technical causes of the accident sought by NASA's investigators. Questions like: For what did they die? Was it worth the risk? And perhaps most important, where do we go from here?

For what did they die? There are some who say, not much; the scientific experiments carried aboard the *Columbia* on her last flight were unremarkable and not worth the loss of any life. While criticism of the science program of STS 107 is valid, this argument is false at its core. STS 107 was not a flight taken in isolation, but as part of an overall space program, and needs to be understood that way. We could have won World War II without taking any particular hill or village one might care to name. Does that mean that the men who fell in those actions died for nothing? Hardly; Joe did not die to liberate Hill 423, but to liberate Europe.

Did *Columbia* have a purpose of comparable worth? Yes she did. Columbia's cause was the human future.

The Earth is not the only world. There are hundreds of other planetary objects in our own solar system, millions in nearby interstellar space, and hundreds of billions in the galaxy at large. The challenges involved in reaching and settling these new worlds are large, but not beyond human capacity. Should we succeed in becoming spacefarers, we will open up a prospect for a human future that is vast in time and space, and rich in experience and potential to an extent that exceeds the imagination of anyone alive today. When we open the space frontier, we will open the door to the creation of numerous new branches of human civilization, replete with new cultures, new knowledge and epic histories that will add immeasurably to the human story.

This, then, is the cause for which the *Columbia* crew gave their lives, and its value cannot be doubted. It is thus appropriate that political leaders from across the spectrum have rallied to declare that the recent disaster will not deflect us from our course, and that America will persevere in space.

Stagnation is not an option

However it is not enough to continue the quest. We must win it. The American space program, begun so brilliantly in the era of Apollo, has spent the past 30 years without remotely comparable levels of achievement. Indeed, in looking at the space program of the 1960's from the point of view of today, one frequently feels oneself in the same position as a eight-century Italian gazing upon the ruins of imperial Rome and saying to himself in amazement, "We once built that?"

Why was the space program of the Apollo era so more productive than that of today? Was it because of vastly superior funding? In point of fact it was not. NASA's average budget during the period 1961 to 1973, when it built up from near-zero space capability to storm heaven with the Mercury, Gemini, Ranger, Surveyor, Mariner, NERVA, Apollo, and Skylab programs , was $16-billion in 2000 dollars. That is only slightly more than NASA's current $15.5-billion budget. The problem is not lack of money but lack of focus and direction. For the past three decades the U.S. space program has floundered without any central motivating goal. As a result, funds have been spent at a rate comparable to that of the 1960's without producing anything approaching commensurate results.

We need a defining goal to drive our space program forward. At this point of history, that focus can only be the human exploration and settlement of Mars.

Why Mars? Because of all the planetary destinations currently within reach, Mars offers the most, both scientifically, socially, and in terms of what it portends for the future of humankind.

In scientific terms, Mars is critical, because it is the Rosetta Stone for letting 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 natural phenomenon, emergent from chemical complexification wherever there is liquid water, a temperate climate, sufficient minerals, and time, then life should have appeared on Mars. If we can 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 ground water where Martian life may yet persist, we will be able to examine it, and by so doing determine whether life as we know it 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 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 space program itself needs challenge. Consider: between 1961 and 1973, under the impetus of its drive toward the moon, NASA produced a hundred times the rate of technological innovation it has shown since, for essentially the same budget. Why? Because it had a goal that made its reach exceed its grasp. You don't need to develop anything new if you are not doing anything new. Far from being a waste of money, forcing NASA to take on the challenge of Mars is the key to giving the nation a real technological return for its space dollar.

The challenge of a humans-to-Mars program would also be an invitation to adventure to every youth in the country, sending out the powerful clarion call: "Learn your science and you can become part of pioneering a new world." There will be over 100-million kids in our nation's schools over the next 10 years. If a Mars program were to inspire just an extra 1 percent of them to scientific educations, the net result would be 1-million more scientists, engineers, inventors, medical researchers and doctors, making technological innovations that create new industries, finding new medical cures, strengthening national defense, advancing the human condition, and generally increasing 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 for 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 Earth's moon, Mars possesses oceans of water frozen into its soil as 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 important, rocket fuel. Additionally, 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 geothermal 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. Mars can be settled. For our generation and many that will follow, Mars is the New World. In establishing our first foothold on Mars, we will begin humanity's career as a multiplanet species.

How Do We Get There?

Humans to Mars may seem like a wildly bold goal to proclaim in the wake of disaster, yet such a program is entirely achievable. 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 were there eight years later. Given the will, we could have our first teams on Mars within a decade.

How can this be done? Let us start with the present, with the space program flat on its back. This is what we must do:

First, the shuttle must be restored to flight. NASA must investigate the accident, determine the cause, and eliminate it, along with other possible sources of vulnerability identified in the course of the investigation. This can be done in less than a year, during which time the space station can be supported by Russian Soyuz crew transfer vehicles and Progress supply modules.

Once the shuttle is flying again, its operations should be confined for the foreseeable future to space station orbit, where the crew would have a safe haven, and where Russian capabilities are available for rescue.

There is thus no need to collapse NASA's present program. However, that said, the present program is entirely inadequate to get us anywhere. While we must restore the shuttle to flight as soon as possible because it is all we have, we must replace it as soon as possible because it is obsolete.

The shuttle is obsolete, not simply because it is based on 1970's technology, or because its highly stressed components are becoming worn out with repeated use, but because it is the wrong launch vehicle to support the needs of a visionary space program. In truth, the shuttle is not a space lift vehicle at all; rather, it is a self-launching space station. It is not a truck with a heavy hauling capability, it is a Winnebago whose primary function is to move itself. The shuttle at lift off has the same thrust as a Saturn V moon rocket, yet it has only 15 percent of the payload, because 85 percent of the mass it delivers to orbit is that of the orbiter itself. This is why it is the least efficient payload delivery system ever flown.

It is true that at a time when we had no place to stay on orbit, having a self-launching temporary space habitat made some sense. But now that we have a space station, using the massive shuttle as a means of transferring crew to and from it is wildly suboptimal. We don't need a giant Winnebago to travel to our country home; all we need is a small car. Specifically, what we need is a small crew transfer vehicle, either of the Apollo capsule variety or a mini-shuttle like the proposed Orbital Space Plane, which at a mass 10 percent of the orbiter would be light enough to launch on top of a Delta or Atlas launch vehicle. These expendable launch vehicles cost one-tenth as much as a shuttle launch, and would be safer to ride to orbit as well, since they are modern, brand new every time they are flown, and positioned beneath the payload they are lifting, rather than to its side. Thus if something goes wrong with the booster, (as in the *Challenger* incident) the crew capsule can get away, and if something should fall from it (as in *Columbia*), the crew vehicle will not be hit.

However this done, we do not abandon the shuttle launch infrastructure. Rather, by freeing the shuttle launch stack of the orbiter, and giving it a hydrogen/oxygen upper stage instead, we reconfigure it into a true heavy lift launch vehicle capable of duplicating the performance of the Saturn V. With such a system, we could deliver 120 metric tons to low Earth orbit (in place of the current shuttle's 20), or send payloads in the 50-ton class on direct trajectories to the moon or Mars.

Using such a system together with appropriate payload elements which could be readily developed over the next five years, human Mars exploration could begin before this decade is out.

Here's how it could be done: In 2009 we launch a single one of these shuttle-derived heavy lift boosters off the Cape, and use it to throw to Mars an unfueled and unmanned Earth Return Vehicle (ERV) After landing on Mars, the ERV runs a pump to suck in the Martian air -- mostly carbon dioxide -- and reacts this with a small amount of hydrogen brought from Earth to produce a large supply of methane/oxygen rocket propellant. Then, in 2011, another booster is used to shoot the crew out to Mars. Because their return ride is waiting for them on the planet's surface, the crew does not need to fly to Mars in a giant futuristic spaceship. Instead, a basic habitation module would do. The crew lands their hab on Mars in the vicinity of the ERV and use as their house for a year and a half while they explore the Red Planet. At the end of that time they get in the ERV and fly home, leaving the hab behind on Mars. Thus, as one mission follows another, more habs are added to the base, in the process building up mankind's first foothold on a new world.

No great impossible breakthroughs, science fiction futurism or gargantuan technologies are needed to do this. Just some good brass tacks engineering, some 19th century industrial chemistry, and a little bit of moxie. We don't need to spend the next 30 years with a space program mired in impotence, spending large sums of money and taking occasional causalities while the same missions to nowhere are flown over and over again and professional technologists dawdle endlessly in their sand boxes without producing any new flight hardware.

We simply need to choose our 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 there.

If done in a well-managed program, the total development effort cost before the first flight could be kept in neighborhood of $20-billion. After that, each mission by the copy would cost around $2-billion. That's a sum that this country can easily afford. It's small price to pay for a new world. It's a pittance for delivering the birth of a new age in human history.

A Proper Memorial

The *Columbia* seven are heroes, and the tears of noble men and women will water their graves for many years to come. In the United States, public schools and university engineering buildings will be named after each of the crew members. In Israel, no doubt, Col. Ramon will be remembered, among other ways, by trees planted in his memory.

It is a good custom, I think, the Israeli way of tree-planting. It remembers life by creating life. I believe in this instance, though, we should take it further. To truly honor the Columbia crew, let us resolve not to bend in our efforts until seven trees in their honor can be planted on Mars.

From death let forth life; from tragic loss, victory.

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In March 2003, Dr. Cary Zeitlin, the principal investigator of the MARIE instrument aboard the *Mars Odyssey* spacecraft, which had been launched in April 2001 and been in Mars orbit since October 2001, reported his results. MARIE was designed to measure radiation, and so it did, with the levels measured being, according to Zeitlin, “manageable” by humans. The Associated Press, however, chose to report that Zeitlin had discovered levels of radiation that would severely endanger any astronauts who ventured to go to Mars. This misreportage clearly had to be countered, as its acceptance by the political class would rule out any possibility of a humans to Mars program, possibly forever. Here is my reply.

AP Falsely Reports Mars Radiation Data

Mars Society internet bulletin

March 14, 2003

The Associated Press yesterday issued a wire article claiming that "the radiation on the surface of Mars is so intense that it could endanger astronauts sent to explore the Red Planet." The AP claimed that these were the findings of the MARIE instrument currently operating on the *Mars Odyssey* spacecraft, and ascribed the view that such radiation doses were too high to allow human explorers to Dr. Cary Zeitlin of the National Space Biomedical Institute in Houston. Dr. Zeitlin is the Principal Investigator for the MARIE radiation detection instrument.

In fact, however, the MARIE data, which is publicly available at the MARIE website at marie.jsc.nasa.gov/Results.html, show exactly the opposite. Currently posted data for January 2003 show radiation levels in low Mars orbit of 25 millirads/day, or 9 rads/year. While this level is slightly less than twice the regulatory dose for persons employed in the nuclear industry, it represents no significant threat. According the conservative "linear hypothesis" for dealing with low doses accepted in the radiation health physics community, a dose of 13 rads delivered over a 1.5 year Mars mission surface stay would represent a statistical increase in likelihood of cancer (at some point later in life) of about one quarter of one percent. In contrast, the average American smoker receives a 20 percent increase in cancer risk. The Mars radiation risk is thus only about 1/100th as dangerous as smoking.

The MARIE radiation measurements were taken in Mars orbit. Doses on the surface would be even lower.

Thus far from proving that radiation is a showstopper for human Mars missions, the MARIE data show that radiation is NOT a major obstacle to human exploration.

The AP misreportage of the MARIE results is particularly disturbing because it directly contradicts the points that Dr. Zeitlin made at the *Mars Odyssey* press conference. Subsequent to the publication of the AP article, Dr. Zeitlin sent the following email to Mars Society president Robert Zubrin to set the record straight:

> Bob,
>
> Saw your quote in a version of the AP article that's
> making the rounds tonight about radiation risks on
> a Mars mission. Unfortunately your quote is set up
> as if it were in opposition to my statements, when
> in fact we are in agreement: the radiation is not
> a show-stopper. I said this quite explicitly in the
> press conference and in fact you can see in another
> (more soberly-written) article that I called the risk
> "manageable." I am not sure whether Mr. Bridges
> didn't understand what I was saying or chose to
> sensationalize it; I prefer to give him the benefit
> of the doubt and assume he misunderstood. However,
> not everyone did, as you can see in this article:

www.space.com/missionlaunches/odyssey_radiation_030313.html

Cary

The AP misreportage of the MARIE results is a major disservice to the American public and space program. The Mars Society calls on the Associated Press to issue a retraction and correction of its erroneous article.

Full and accurate discussion of the *Mars Odyssey* results will be presented at the Sixth International Mars Society convention, which will be held at the Hilton Hotel in Eugene Oregon, August 14-17, 2003. Registration is now open at www.marssociety.org.

To find out more about the Mars Society, visit our website at www.marssociety.org. Or contact info@marssociety org.

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By the fall of 2003, many in congress had come around to the position expressed by Admiral Gehman, the chairman of the Columbia accident review board, that “if we are to accept the costs and risks of human spaceflight, we need to have goals that are worthy of those costs and risks.” As a result, Senator John McCain, (R-AZ) decided to hold hearings to gain insight for Congress to enable it to decide intelligently what those goals should be. I was invited to testify. Here is what I said.

Testimony of Dr. Robert Zubrin at Senate Commerce, Science, and Transportation Committee Hearings: "Future of NASA"

Wednesday, October 29, 2003

Senator McCain, members of the Commerce Committee, I would like to thank you for inviting me to testify here today on the future of the US space program. Since many of you may be unfamiliar with me, I hope you will forgive me if I take a few seconds to establish my credentials. I am an engineer with a Masters degree in Aeronautics and Astronautics, a doctorate in Nuclear Engineering, and fifteen years aerospace industry experience. I currently lead my own company, Pioneer Astronautics, which has five NASA and military R&D contracts at this time. I am the author or co-author of over 100 papers, three patents, and five books related to the field, and am the head of an international non-profit organization known as the Mars Society which has built and run a human Mars exploration operations research station on Devon Island, 900 miles from the North Pole.

My remarks today will address four areas. First, I will discuss why NASA is failing, and what fundamental change in method of operation needs to be undertaken if the space agency is to be made effective again, and in particular, explain why an overarching goal must be adopted if that is to occur. Second, I will explain what that goal should be. Third, I will present a plan for a pioneering space program that would allow NASA fulfill its promise and achieve that goal within ten years. Finally, I will make specific recommendations as to what Congress and the Executive branch need to do this year in order to put the space program on the right track.

1. Why is NASA Failing?

In the recent *Columbia* hearings, numerous members of congress continually decried the fact that the US space program is "stuck in Low Earth Orbit." This is certainly a serious problem. If it is to be addressed adequately, however, America's political leadership needs to reexamine NASA's fundamental mode of operation.

Over the course of its history, NASA has employed two distinct modes of operation. The first, prevailed during the period from 1961-1973, and may therefore be called the Apollo Mode. The second, prevailing since 1974, may usefully be called the Shuttle Era Mode, or Shuttle Mode, for short.

In the Apollo Mode, business is 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, after which the mission is 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 internal and external technical community pressure groups 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.

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, then acquires the appropriative 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 to eventually 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 adequate excuse is generated to justify each purchase, thereby avoiding embarrassment. That is the Shuttle Mode.

In today's dollars, NASA average budget from 1961-1973 was about $17 billion per year. This is only 10% more 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 between 1961-1973 and 1990-2003, as the space agency's total expenditures over these two periods were equal.

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, Johnson Space Center, and JPL were all created in more or less their current form.

In contrast, during the period from 1990-2003, NASA flew about three score Shuttle missions allowing it to launch and repair the Hubble Space Telescope and partially build a space station. About half a dozen interplanetary probes were launched (compared to over 30 lunar and planetary probes between 1961-73). Despite innumerable "technology development" programs, no new technologies of any significance were actually developed, and no major space program operational infrastructure was created.

Comparing these two records, it is difficult to avoid the conclusion that that NASA's productivity in both missions accomplished and technology development during its Apollo Mode was at least ten times greater than under the current Shuttle Mode. The Shuttle Mode is the expenditure of large sums of money without direction by strategic purpose. That is why it is hopelessly inefficient. But the blame for this waste cannot be placed on NASA leaders alone, some of whom have attempted to rectify the situation. Rather, the political class must also accept major responsibility.

Consider the following. During the same week in September that House members were roasting Administrator O'Keefe for his unfortunate advocacy of a destination-free NASA, a Senate committee issued a report saying that a top priority for the space agency was to develop a replacement Space Shuttle system. Did any of the Senators who supported this report explain why? Why do we need another Shuttle system? To keep doing what we are doing now? But is that what we actually want to do?

Congress and the Executive branch need to get together and open a discussion as to what the nation actually wants to accomplish in space. Hearings should be held, and the options for a strategic objective examined in public. Is our primary aim to keep sending astronauts on joyrides in low Earth orbit? In that case, a second generation Shuttle might be worth building. But if we want to send humans to the Moon or Mars, we need make that decision, and then design and build a hardware set that is appropriate to actually accomplish those goals.

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 just isn't true.

The Shuttle Mode will never get us anywhere at all. The Apollo Mode got us to the Moon, and it can get us back, or take us to Mars. But leadership is required.

In the beginning, there was the Word.

2. What Should our Goal Be?

In order to accomplish anything in space we need to set a goal. What should that goal 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, both 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 letting 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 phenomenon, emergent from chemical complexification wherever there is liquid water, a temperate climate, sufficient minerals, and time, then life should have appeared on Mars. If we can 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 ground water where Martian life may yet persist, we will be able to examine it, and by so doing determine whether life as we know it 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 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 also be an invitation to adventure to every youth in the country, sending out the powerful clarion call: "Learn your science and you can become part of pioneering a new world." There will be over 100 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 scientific educations, the net result would be 1 million more scientists, engineers, inventors, medical researchers and doctors, making technological innovations that create new industries, finding new medical cures, strengthening national defense, and generally increasing 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 for 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 Earth's Moon, Mars possesses oceans of water frozen into its soil as 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 geothermal 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. Mars can be settled. For our generation and many that will follow, Mars is the New World. 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.

3. How Do We Get There?

Humans to Mars may seem like a wildly bold goal to proclaim in the wake of disaster, yet such a program is entirely achievable. 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 were there eight years later. Given the will, we could have our first teams on Mars within a decade.

The key to success come from rejecting the policy of continued stagnation represented by senile Shuttle Mode thinking, and returning to the destination-driven Apollo Mode method 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 from 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 2009, 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. (Such a booster could be readily created by converting the Shuttle launch stack, deleting the Orbiter and replacing it with a payload fairing containing a hydrogen/oxygen rocket stage.) Arriving at Mars eight months later, the spacecraft uses friction between its aeroshield and Mars' atmosphere to brake itself into orbit around the planet, and then lands with the help of a parachute. This payload 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 cargo, a 100 kilowatt nuclear reactor mounted in the back of a methane/oxygen driven light truck, a small set of compressors and 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 deployed to provide power to the compressors and chemical processing unit. The hydrogen brought from Earth can be quickly reacted with the Martian atmosphere, which is 95 percent carbon dioxide gas (CO2), to produce methane and water, thus eliminating the need for long-term storage of cryogenic hydrogen on the planet's surface. The methane so produced is liquefied and stored, while the water is electrolyzed to produce oxygen, which is stored, and hydrogen, which is recycled through the methanator. Ultimately, these two reactions (methanation and water electrolysis) produce 24 tonnes of methane and 48 tonnes of oxygen. Since this is not enough oxygen to burn the methane at its optimal mixture ratio, an additional 36 tonnes of oxygen is produced via direct dissociation of Martian CO_2. The entire process takes ten months, at the conclusion of which a total of 108 tonnes of methane/oxygen bipropellant will have been generated. This represents a leverage of 18:1 of Martian propellant produced compared to the hydrogen brought from Earth needed to create it. Ninety-six tonnes of the bipropellant will be used to fuel the ERV, while 12 tonnes are 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.

The propellant production having been successfully completed, in 2011 two more boosters lift off the Cape and throw their 40-tonne payloads towards Mars. One of the payloads is an unmanned fuel-factory/ERV just like the one launched in 2009, the other is a habitation module carrying a crew of four, a mixture of whole food and dehydrated provisions sufficient for three years, and a pressurized methane/oxygen powered ground rover. On the way out to Mars, artificial gravity can be provided to the crew by extending a tether between the habitat and the burnt out booster upper stage, and spinning the assembly.

Upon arrival, the manned craft drops the tether, aerobrakes, and lands at the 2009 landing site where a fully fueled ERV and fully characterized 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.

However, 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 2013 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. launch 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 as 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, an investigation key to revealing whether life is a phenomenon unique to Earth or general throughout the universe. 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 driver for a quick return to Earth that plagues alternative 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. Indeed, 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.

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 and taking occasional casualties while the same missions to nowhere are flown over and over again and professional technologists dawdle endlessly in their sand boxes without producing any new flight hardware. We simply need to choose our 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 there. We can establish our first small outpost on Mars within a decade. We and not some future generation can have the eternal honor of being the first pioneers of this new world for humanity. All that's needed is present day technology, some 19th century industrial chemistry, a solid dose of common sense, and a little bit of moxie.

4. What Congress Needs to Do Now

The US civilian space program is presently in a crisis. It is now apparent that the Shuttle Orbiter cannot be used much longer as a system for transporting crews to Earth orbit. The Columbia disaster has made it clear that the antiquated Orbiters are becoming increasingly unsafe. Moreover, even if the Orbiter could be flown safely, it is clear that using a launch vehicle with a takeoff thrust matching that of a Saturn V to transport half a dozen people to the Space Station makes about as much sense as using an aircraft carrier to tow water skiers. The Shuttle was designed as a self-launching space station. Absent a permanent space station on-orbit, such a vehicle had some justification. But with the establishment of the ISS, the rationale for using a flying Winnebago as a space taxi is no longer sustainable.

NASA has already begun to respond to this reality by starting the Orbital Space Plane (OSP) program, which will move the human taxi-to-orbit function from the Shuttle to a small capsule or mini-orbiter that can be launched on top of an Atlas or Delta. If constrained to the objective of producing a simple reliable capsule instead of a complex mini shuttle, such a program could make a great deal of sense. A simple capsule will be much safer than a more complex system, will have a much lower development cost, and can be made available for flight much sooner, thereby cutting short the risks and costs associated with prolonged Shuttle operations. Launched aloft a medium lift expendable launch vehicle, it could assume the Shuttle's crew transfer function at less than 1/5th the cost.

As rational as such an approach might be, however, it poses a direct threat to the jobs of hundreds of thousands of people associated with the existing Shuttle program, and to the bottom line of several major and many minor aerospace companies. For this reason, some people have been lobbying for making the OSP a complex mini shuttle program that would take many years to complete, and cost, at most recent estimate, some $17 billion. This is the wrong approach, and is emblematic of the pathology associated with what we have termed NASA's Shuttle Mode of operation. The raid upon the treasury it involves would sap funding for any other space initiatives, and the delay it would entail in Shuttle replacement would expose our astronauts to serious unnecessary risk. Furthermore, despite patently false claims to the contrary, the wing-and-landing gear ballasted mini-Shuttle is wildly suboptimal for use in any missions beyond low Earth orbit.

As presently constituted, Congress should not fund this program. Making a gold-plated mini shuttle the centerpiece of NASA's development efforts for the next ten years would prevent any human exploration operations for a generation, at the end of which we would be no better prepared to commence piloted planetary exploration than we are today. In fact, we would be worse off, since by simply downsizing from the Orbiter to the OSP mini-Shuttle as a means of transporting humans to orbit at lower recurring cost, we would end up discarding the ten-billion dollar asset represented by the STS launch stack. This would be a disaster, since in the context of a well-planned human exploration initiative, the STS stack would almost certainly be converted into a heavy lift vehicle, rather than scrapped. Such would be the consequences of adopting the piecemeal, reactive approach to dealing with the Shuttle/OSP problem.

Rather than appropriate $17 billion for an OSP program that will not take us anywhere, Congress should appropriate $60 million to fund two six-month $30 million studies to develop end-to-end plans for human exploration of Mars. One of these $30 million studies should be conducted at NASA Johnson Space Center. The other $30 million should go to fund a competing interagency team led by someone from one of the non-NASA government space agencies. Each of these teams should be charged with the task of developing a complete space architecture and mission plan that enables humans to Mars within ten years of program start, with lunar missions enabled by a modified subset of the Mars mission hardware. Constraints should be placed on the plans such as a total development cost limit of $30 billion or less, with a recurring Mars mission cost no greater than $3 billion.

Upon completion of the study, each of the plans should be submitted to a blue-ribbon panel appointed by Congress for evaluation on merit of cost, technical feasibility, and exploration capability. Based on that assessment, the team deemed superior should be selected to lead the human exploration program, and the hardware elements required to implement its plan should be funded and built in accordance with a multi-year schedule laid down in the plan, and then flown. Once again, Congress should not fund the construction of things. It should fund the implementation of a plan.

Directing funding in this focused way does not preclude engaging in exploratory research. What it does mean, however, is that the technologies chosen for research and development are those necessary to enable or enhance the plan, rather than those needed to maintain or enhance the funding of established research and development constituencies.

The recommendation to fund two competing program design teams may seem surprising to some. However the experience of the past several decades has made it clear that, absent the spur of competition, efficient plans will not be generated. The nation does not need a Mars program plan that is bloated with funding for a plethora of unnecessary technology and infrastructure developments. Yet the incentive of as bureaucracy is to use the Mars mission as a kind of Christmas tree upon which to hang various desired technology programs as ornaments. This is the problem that caused NASA to respond to the elder president Bush's call for a Space Exploration Initiative with a hopelessly bloated and overpriced plan in 1989, and is the root pathology that drove the generation of a hyper-complex gargantuan space program design by the NASA Headquarters NExT group during the more recent period.

Mark Twain once said that nothing so focuses the mind as the knowledge that you are going to be shot in the morning. Only the certain knowledge that the cost increases associated with insertion of unnecessary elements in the mission plan threatens the complete loss of programmatic control will force either NASA or an alternative government organization to put parochial interests aside and design the best and most streamlined program possible.

5. Conclusion

Senator McCain, distinguished members of the Commerce Committee. Humanity today stands at the brink of a liberating development which will be remembered far into future ages, when nearly all the other events of our time are long forgotten. That development is the initiation of the human career as a spacefaring species.

The Earth is not the only world. There are numerous other planetary objects in our own solar system, millions in nearby interstellar space, and hundreds of billions in the galaxy at large. The challenges involved in reaching and settling these new worlds are large, but not beyond humanity's ultimate capacity. Were we to become spacefarers, we will open up a prospect for a human future that is vast in time and space, and rich in experience and potential to an extent that exceeds the imagination of anyone alive today. When we open the space frontier, we will open the door to the creation of innumerable new branches of human civilization, replete with new languages, new cultures, new literatures, new forms of social organization, new knowledge, technological contributions, and epic histories that will add immeasurably to the human story.

We were once a small collection of tribes living in the east African rift valley. Had we stayed in our native habitat, that is all we would be today. Instead, we ventured forth, took on the challenges of the inhospitable ice age environments to the north, and then elsewhere, and in consequence, transformed ourselves into a global civilization. When we go into space, the expansion of our possibilities will be equally dramatic. As a result, the human experience a few thousand years from now will be as rich in comparison to ours, as our global society is in comparison to tribal culture of the Kenyan rift valley at the time of our species' origin.

Therefore, I believe that we here today sitting in this historic chamber are gathered not at the end of history, but at the beginning of history. That our nation shall be remembered not so much for the great deeds our predecessors have already done, but for the still greater accomplishments they have prepared us, and those who will follow us, to do. Let us therefore embrace our role as humanity's vanguard, as pioneers of the future. Let us honor the true American tradition by continuing it, and bravely take on the untamed space frontier to open new worlds for our posterity, as our courageous predecessors did for us.

Ladies and gentlemen of the Senate, I ask that you embrace the challenge of Mars, and act forcefully to put NASA on a track that will deliver real results. The American people want and deserve a space program that is actually going somewhere. For that to occur, it needs be given a goal, from that goal a produce a plan, and from that plan, action. It is within your power to make this happen. It is within your power to initiate a program of exploration that will lead in time to the greatest flowering of human potential, knowledge, progress, and freedom that history has ever known. I ask that you do so.

Thank you for your attention.

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In response to pressure from McCain and others in Congress and out demanding that a valid goal be given to NASA so that the agency would indeed become, as it was during Apollo, destination driven," the Bush administration hurriedly formed a working group to determine what that goal should be. As might be expected, they were assailed with people of various persuasions, but most notably from Lagrange point advocates, Moon advocates, Mars advocates, and "pretend to have a goal but really stick with business as usual advocates." Here are a couple of the articles I wrote to try to get Mars on the agenda.

Mars is Our Goal

Letter to the New York Times

To the Editor:

Re "Fly Me to L 1," by Buzz Aldrin (Op-Ed, Dec. 5):

A project to build a space port in a region of space called L 1 would be a costly diversion.

Mars holds the key to knowledge of the diversity of life in the universe, and is thus the true goal for our manned space program.

NASA needs to carry out plans, not build things. America reached the Moon in the 1960's because the space agency had its eye firmly focused on a real mission with a presidential deadline. Under those circumstances, NASA was forced to develop an efficient plan to achieve that mission, and then driven to build a coherent set of hardware elements to carry out that plan.

If President Bush is willing to provide that kind of direction, we can have humans on Mars within a decade.

ROBERT ZUBRIN

President, Mars Society

Indian Hills, Colo., Dec. 7, 2003

The Choice for Kitty Hawk

The Washington Times, Dec 15, 2003

Tomorrow will be the 100th anniversary of the Wright brothers historic first flight at Kitty Hawk and falls within the 200th anniversary of the Lewis and Clark expedition. Such a portentous occasion cannot go by unmarked, and the word is out that President Bush will travel to the scene of the aviation pioneers' triumph to make a statement reaffirming America's commitment to exploring new frontiers, which now lie in space.

The question is, what will the vision be? For the past 30 years, since the conclusion of the Apollo Moon landings, humans to Mars has been the challenge staring the space program in the face. Because it once had abundant flowing liquid water, Mars could have been, and may yet be, a home for life. The Red Planet thus is the Rosetta stone that holds the key to our enlightenment on the issue of the prevalence and diversity of life in the universe. Uniquely among all the worlds within our reach, it possesses all the other resources needed for not only life, but technological civilization. Mars is also the critical test that will determine whether humankind can transcend its limits and become a multi-planet species.

In 1969, NASA had plans for human Mars exploration to commence by 1981. Unfortunately, the program was aborted by the Nixon administration, and American astronauts have been confined to low Earth orbit ever since. Tomorrow, will the president call for our space program to shake off its three decades of stagnation and reach for the prize?

Not if the current agency bureaucracy can help it. According to several reports, NASA headquarters has forwarded a timid plan calling for a return to the moon by the end of the next decade.

How low have we fallen? Manned moon landings in 17 years? Starting with virtually no space technology base, the America of slide rules and rotary phones did it in eight. For the president to stand at Kitty Hawk and proclaim this goal as a bold new vision for the American space program would be farcical. Rather than representing a reaffirmation of the tradition of the Wright Brothers and Lewis and Clark, it would be a denial. Furthermore, by setting a timeline for an initiative that requires no real action within his administration or the next, such an announcement would really serve simply as visionary camouflage for yet another decade of continued NASA random activity, waste and stagnation.

We can do much better. Future-fantasy spacecraft are not needed to send humans to Mars. The primary real requirement is a heavy lift booster with a capability similar to the Saturn V launch vehicle employed in the 1960s. Such a booster could be readily created today by stripping the shuttle launch stack of the Orbiter, replacing it with a payload fairing containing a chemical rocket stage.

The mission could then be accomplished wit two launches. The first would send an unfueled and unmanned Earth Return Vehicle (ERV) to Mars. After landing, this vehicle would manufacture its own methane/oxygen return propellant by combining a small amount of hydrogen imported from Earth with a large supply of carbon dioxide acquired from the Martian atmosphere. The chemistry required to perform this operation has been widely practiced on Earth since the gaslight era.

Once the propellant is manufactured, the crew is sent to Mars in a habitation module launched by the second booster. The hab module is landed near the ERV and used for a year and a half as the crew's base for exploring the Martian surface, after which the crew enters the return vehicle and flies home. The hab module is left behind on Mars, so each time a mission is flown, another habitation is added to the base. There is nothing required by such a plan that is beyond our technology.

The issue is not money. The issue is leadership. NASA's average Apollo-era (1961-73) budget, adjusted for inflation, was about $17 billion a year in today's dollars, only 10 percent more than the agency's current budget. Yet, the NASA of the '60s accomplished a hundred times more because it had a mission with a deadline, and was forced to develop an efficient plan to achieve that mission, and then constrained to build a coherent set of hardware elements to achieve that plan. If Mr. Bush is willing to provide that kind of direction, we can have humans on Mars within a decade. If he is not, we will be left with a space program that continues to spend vast sums on a random set of projects that do not fit together and do not lead anywhere; not to Mars or to the moon, not in 20 years, or in 50.

The American people want and deserve a space program that really explores new worlds. On Dec. 17, the ghosts of the Wrights and Lewis and Clark will cry out to Mr. Bush to give it to them. I hope he will listen.

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As it happened, President Bush did not announce his decision at the Wright brothers 100th anniversary of flight celebration December 16, 2003, but in a room inside NASA headquarters on January 14, 2004. According to Bush, his new Vision for Space Exploration (VSE) was mission to "the Moon, Mars, and Beyond." According to the VSE plan, the period until 2010 would be devoted to flying the Shuttle to complete the ISS. That done, the Shuttle programs would be shut down, and its funds diverted to support a focused drive towards the Moon, with the first return landing occurring in 2020. The Moon program would develop and test in operation technologies useful to support Mars missions, thus allowing the Red planet to be reached by 2030 or 2035. Thus the VSE plan was a compromise, offering something for each of the contending factions (although it must be said that the "pretend to have a goal but really stick with business as usual" crowd got more than the others, since the Bush plan gave them their way until after he would be out of office (even assuming a second term), after which nothing he said in 2004 would matter anyway.

Still, the VSE was a plan to go somewhere, and many in the space community, including me, thought it was a step in the right direction that might evolve into a true breakout, given time and nourishment. However just two days after Bush's speech, NASA administrator O'Keefe issued a statement that he was cancelling the long planned Shuttle mission, SM4, to repair and upgrade the Hubble Space Telescope. According to O'Keefe, Shuttle missions to Hubble were just too risky for NASA to contemplate. This crazy move threatened not only to destroy the world-historic telescope, but to kill the VSE in its cradle, since if NASA was too scared to fly to Hubble, which is located just a few hundred miles up in low Earth orbit, it certainly would never be willing to accept the risks involved in sending astronauts to the Moon, let alone Mars. Given that, Congress would be fully justified in refusing all yellow-feather adorned NASA requests for funds to develop technology for Moon and Mars missions, as granting such requests would be like buying expensive Everest-class mountain-climbing equipment for a child who refuses to go camping. Indeed, coming at a time when hundreds of thousands of young Americans were risking their lives in combat in Iraq and Afghanistan, such a display by NASA of unwillingness to accept the risks required to perform its mission threatened to brand the agency with indelible disgrace. Many NASA employees were aware of this, and rightly outraged that O'Keefe should betray both Hubble, and the agency itself, in such manner. Consequently, several were delighted to leak NASA data to me, which proved that O'Keefe's arguments were completely unfounded. Here is the *Space News* article in which I published some of it. An abridged version of this article then appeared in *The Washington Times* February 11, 2004, followed by *The Rocky Mountain News* and a number of other regional newspapers. Shortly thereafter, the *Wall Street Journal's* Sharon Begley picked up on the story, giving the facts on the matter broad circulation.

Don't Desert Hubble

Space News, February 9, 2004

On January 16th, NASA Administrator Sean O'Keefe announced that he had decided to cancel all future Space Shuttle missions to the Hubble Space Telescope, including SM4, the nearly-ready-to-go flight that would have installed the new Cosmic Origins Spectrograph and Wide Field Camera 3 instruments. This decision came atop an overall policy shift by the Bush administration to phase out the Shuttle and International Space Station (ISS) commitments by 2010, thereby clearing the way to redeploy their budgets towards supporting human exploration of the Moon and Mars. While the general redirection of NASA's human spaceflight program from Earth orbital activities towards planetary exploration was a valuable and long-overdue step, canceling the Hubble upgrade mission was a huge mistake.

The Hubble Space Telescope has been the most scientifically productive spacecraft in history. Through Hubble, we have observed directly the planetary cometary impacts that drive the evolution of life, witnessed the birth of stars that make all life possible, and measured the size and age of the universe itself. Because of Hubble, we now know that ordinary matter is a very small part of the universe and that the expansion of the universe is speeding up, not slowing down as previously thought – thereby revealing a new and unexpected force of nature. The astronaut missions that have made this possible stand as epic achievements in the chronicles of humanity's search for truth.

Now we have a chance to push further. The Cosmic Origins Spectrograph and Wide Field Camera 3 designed to bring the Hubble to its full potential have already been built and tested at a cost of $167 million, and promise an enormous scientific return upon delivery to orbit. With the help of these instruments, Hubble would be able probe deeper into space and time, helping to reveal the processes that governed the origin of the universe and that will determine its ultimate fate. How can the decision abort such a program possibly be justified?

Certainly not on the basis of cost. If the Bush plan were to stand down the Shuttle immediately, and save the $24 billion required to operate it through 2010 so as to initiate the Moon/Mars program this year with substantial funding, that would be one thing. But given the decision to return the Shuttle to flight, canceling the Hubble upgrade would only save a pittance. It takes about $4 billion per year to maintain the standing army of engineers and technicians that support the Shuttle program, but it only costs an additional $100 million or so to fly five Shuttles in a given year instead of four. Thus the additional cost to the taxpayer to fly both SM4 and a subsequent flight a few years later to replace the Hubble's batteries and gyros and reboost it to a higher orbit where it could be functional well into the next decade would only be about $200 million, or less than one percent of the Shuttle program's budget over its remaining life. From a financial point of view, the decision to abandon the Hubble upgrade while continuing Shuttle flights amounts to throwing out the baby while keeping the bathwater.

Safety arguments won't wash either; if the Shuttle is safe enough to fly to the ISS, it's safe enough to go to Hubble. It is true then when flying to the ISS, the crew has a safe haven, so that if they should discover damage to the Shuttle's thermal protection tile system, they could retire to the space station and survive for a short time while they wait for retrieval by a Russian Soyuz capsule.

In this scenario, ISS missions would possess a safety features that Hubble missions lack. But tile damage during launch is not the only source of Shuttle flight risk. According to most analysis, the greatest source of flight risk stems from the possibility fatal impacts by micro meteor or orbital debris (MMOD). ISS orbits are much more hazardous in this respect than Hubble orbits. For example, on STS-113, the last Shuttle station flight, the calculated probability of loss of vehicle and crew by MMOD was 1/250. In contrast, the last Hubble servicing mission (STS-109) had a much lower calculated MMOD probability of 1/414.

After MMOD, it is believed that the greatest risk faced by Shuttle flights stems from the possibility of engine failure during launch. Because Hubble missions lift off with a much lighter payload than most ISS missions, they are can deal with this danger much more effectively. For example, in order to be able to abort to orbit on an ISS mission such as STS-113 (Endeavor), all three Shuttle main engines must fire for a full 282 seconds before one cuts out. In contrast, on Hubble missions such as STS-103 (Discovery), only 188 s of full three-engine operation is required. This lower full-power time requirement for Hubble missions is a critical safety advantage, because the maximum time that either ISS or Hubble missions can attempt a Return to Launch Site (RTLS) abort is about 232 s. Thus Hubble missions have a 50 second overlap during which either a RTLS or orbital abort is possible, whereas ISS missions have a 50 s gap in which neither is possible.

If the Shuttle cannot perform either an RTLS or orbital orbit, it might be able to reach a transoceanic landing site, but in all probability will have to splash down in the ocean. When they depart the Cape, Hubble missions fly east-southeast, and they thus have the possibility to ditch in warm tropical waters. In contrast, ISS flights leave the Cape traveling northeast, and their crews face the prospect of aborts into the frigid waters of the North Atlantic, where their chances for survival would be much less. Thus, while no true quantitative engineering analysis has been done to establish whether and to what extent individual Shuttle flights to ISS are more or less risky than individual Hubble missions, there is good reason to believe that it is Hubble flights that offer greater safety.

However, if we include the consideration that only two Shuttle flights would be needed to make Hubble operational through 2015, while at least 20 missions will be needed to complete the ISS, it becomes apparent that the risk associated with the latter program is at least an order of magnitude greater.

A comparison of mission risk associated with Shuttle flights to ISS and Hubble is presented in the table below.

Table: Comparison of Shuttle Hubble and ISS Mission Risk

Feature	ISS	Hubble	Safer Option
Haven on Orbit?	Yes	No	ISS
Micro meteor Danger(MMOD)	1/250(STS-113)	1/414(STS-109)	Hubble

Ocean Abort Site	North Atlantic	Equatorial Waters	Hubble
No Return (RTLS) time	232 s (STS-113)	231 s (STS-103)	Equal
Abort to Orbit time	282 s (STS-113)	188 s (STS-103)	Hubble
Press to MECO(1 engine out)	391s(STS-113	265 s (STS-103)	Hubble
Press to MECO(2 engine out)	425s(STS-113)	380 s (STS-103)	Hubble
# of Program Flights Needed	>20	2	Hubble

(Press to MECO means time required at full three-engine power before the planned orbit can be achieved.)

Furthermore, consider this: Under the new space policy, the President intends to ask Congress to spend billions of dollars to develop technology to enable human Moon and Mars missions. Yet Congress has just spent $167 million to develop the instruments for SM4, only to be told by the NASA Administrator that he is now afraid to fly the Shuttle to deliver them. If such behavior is accepted, what guarantee can lawmakers have that after they spend billions to develop manned

Moon or Mars exploration hardware, a future NASA administrator might not also get cold feet? It is difficult to understand how an agency which is too risk-adverse to undertake a Shuttle mission to Hubble could possibly be serious in considering a piloted mission to the Moon or Mars.

The decision to cancel the Hubble mission thus completely undermines the President's call for human planetary exploration. Unless we are willing to accept risks equal to, and in fact significantly greater, than those required to upgrade the space telescope, human explorers are not going to the Moon, Mars, or anywhere else. And if we are not going to engage in human interplanetary travel, then the primary rationale for the Space Station program – learning about the effects of long-duration spaceflight on human physiology – must be brought into question as well.

The point is not that we should be blasé about risk. The point is that there are certain things that require accepting risk to achieve, and are worth the price that such a course will entail. The search for truth, carried forward by necessarily perilous human activities in space – whether at Hubble, or on Mars – is one of them. Nothing great has ever been accomplished without courage. If we abandon courage, we turn our back on all that has made our civilization one worth celebrating.

In the face of massive public outrage about his decision, Administrator O'Keefe has agreed to allow it to be reviewed by Columbia Accident Investigation Board Chairman Admiral Hal Gehman. Hopefully Gehman will rectify the situation. But if he does not, then Congress will have to act. They will have to take action, because ultimately the question of whether we do what it takes to keep our eyes open upon the heavens is not one of the technicalities of Shuttle flight safety, but of societal values.

The desertion of Hubble is an offense against science and civilization. It represents a departure from the pioneer spirit, and its ratification as policy would preclude any possibility of a human future in space. It is an inexcusable decision, and it needs to be reversed.

Hubble Honorable Discharge?

Letter to the *Washington Times*

Feb. 24, 2004

In a letter entitled "Hubble honorably discharged" printed in the Washington Times Feb. 21, [NASA Associate Administrator for Human Spaceflight] William Readdy and [NASA Associate Administrator for Space Science] Edward Weiler defended the decision of NASA Administrator Sean O'Keefe to abandon the Hubble Space Telescope.

Linking the term "honorable discharge" to the Hubble desertion decision provokes interesting thoughts. In recommending the abandonment of Hubble, Mr. O'Keefe is allowing the destruction of a $4 billion piece of property paid for and owned by the American taxpayers. Four billion dollars, as former Navy Secretary O'Keefe should know, is also the acquisition cost of a nuclear aircraft carrier.

Let us therefore consider the hypothetical case of an aircraft carrier captain who decided to treat his command in the same manner as Mr. O'Keefe is treating Hubble. Allowing his vessel to sink, he would then offer the following excuse in his report: "The ship developed a leak. I could have saved her by ordering seven men to go below and patch her up, but the odds in their favor were only fifty to one. So I decided the safest course was simply to give up the ship."

I'm not sure exactly how the Navy brass would deal with such an officer, but I don't believe an honorable discharge would figure prominently in their list of options.

Robert Zubrin

President, Mars Society

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Within a few months of its announcement, bureaucratic constituencies within NASA started the pull the design of the program in a variety of illogical directions. Here is an article I wrote to try to combat this entropic trend.

Tighten the Exploration Initiative

Space News, April 2004

Question: How much rope does it take to connect two posts separated by a distance of ten meters? The answer varies. If you let the rope be slack or diverted along detours, any amount can be used. But if the rope is pulled straight and tight, the job can be done with about ten meters. The choice of which approach is preferable depends upon whether your goal is to connect the two posts — or if you're trying to sell rope.

The same is true of President Bush's new space exploration initiative. How much will it cost to get humans to Mars? Opponents claim that it could cost a politically fatal half-trillion or more, and while it need not, it could, unless the rope is pulled tight. Unfortunately, what we are seeing is a binge of rope-selling that threatens to repeat the death-by-sticker-shock that killed a similar initiative by the President's father a decade and a half ago.

Three major examples of current large-scale rope sales include the emphasis on the International Space Station, the plans for creating a "Lunar Cape Canaveral," and the push for high-powered nuclear electric propulsion. Each of these is a distraction, wasting time and money.

Let's start at the beginning. What is, or should be, the goal of the new manned spaceflight initiative? The answer can only be to send human explorers to Mars. The recent findings of the Mars rovers have shown with certainty that the Martian surface once hosted standing bodies of liquid water — habitats that could have hosted the development of life. Also, in recent weeks, three different groups of investigators using four different instruments have announced the detection of methane in the Martian atmosphere at levels far above what would make sense if the planet were lifeless. These methane traces must be seen as a probable signature of subsurface microbial life. If human explorers could go to Mars and set up drilling rigs capable of reaching the underground refuges of these microbes, we could sample them, culture them, image them, and subject them to a battery of biochemical tests that would reveal whether Martian life is created in accord with the same plan that underlies all Earth life, or whether it is constructed in another way entirely. Put another way, by going to Mars we have a chance to find out whether life as we know it on Earth is the pattern for all life everywhere, or whether we are just one particular example of a much vaster and more interesting tapestry. This is fundamental science that bears on the nature of life itself, and it can only be done by human explorers on the surface of Mars. It is a rational, program, a search for truth that is worth the billions of dollars of expenditure and the risk of human life necessary to implement it.

So, having chosen the right goal, the question then becomes: What do we need to do to pull it off?

The International Space Station doesn't help reach that goal. While the ISS provides some useful data for Mars mission designers, no one with a budget of $50 billion and the task of getting humans to Mars would choose to spend $30 billion conducting zero-gravity experiments on human subjects in a station orbiting Earth. Not only is it a disproportionate share of the program budget, but the negative effects of zero gravity can be avoided by rotating the Mars-bound spacecraft to provide artificial gravity.

President Bush's planned lunar base could also be a detour from the main goal. The limited research that can be done on the Moon — dating impact craters and other geological work aimed at resolving questions of the Moon's origin — is much less important than the investigation of the nature of life that can be done on Mars. Lunar science is historical, while Martian science is fundamental. The lunar base must therefore seek justification in what it can do to further the enterprise of exploring Mars.

Thus, we now hear proposals for the creation of a "Lunar Cape Canaveral." According to the advocates of this concept, a Moon base will enable Mars exploration because launching from the Moon is much easier than launching from Earth. While it is true that it should be possible to generate liquid oxygen, the majority component of chemical rocket propellant, on the surface of the Moon, and the low lunar gravity certainly makes Moon launch much easier than Earth launch, the fact remains that before the Marsbound spacecraft launches from the Moon it needs to reach the Moon, which means it must be launched from Earth in any case. Furthermore, because the Moon has no atmosphere to enable aerobraking or parachute assisted descent, the amount of rocket propulsion needed to go from low Earth orbit to the surface of the Moon is substantially greater than that needed to go from low Earth orbit to the surface of Mars. What this means is that *even if a Moon base existed right now*, and had large reservoirs not only of liquid oxygen but also of fuel to burn with it, sitting in propellant tanks and available for free, it would make no sense to use it to support Mars expeditions, because it would cost more to get there than it would to go directly to Mars.

A lunar base could serve as a training ground for Mars missions, but that same objective could be accomplished at a thousandth of the cost by establishing prototype Mars stations in the Arctic. Far from making a Mars mission easier, the Moon base would just be a gold-plated lunar tollbooth, wasting tens of billions to build and adding massively to the expense of every Mars mission forced to use it.

Another oft-mentioned diversion from the main goal is high-powered nuclear electric propulsion (NEP). According to the high-power NEP rope-sellers, manned Mars exploration won't be possible using today's rocket technology, because the six-month transit to Mars would expose the crews to lethal doses of radiation. Accordingly, they claim, enormous hundred-megawatt class nuclear electric propulsion systems will be needed, since these would allow the ship to reach Mars in two months.

In fact, nothing could be further from the truth. In order to enable a two month transit from Earth to Mars, the NEP system would need to achieve a power density of 3000 W/kg. In contrast, the actual NEP systems now on NASA's drawing board for the Jupiter Icy Moon Orbiter (JIMO) mission will have a power density of 16 W/kg. If the JIMO spacecraft were sent from Earth to Mars, it would require 48 months to do the trip, each way. In reality, there is no prospect of being able to develop NEP systems with one-third the trip time of current chemical systems, or the same time, or three times the time for that matter.

Fortunately, however, such faster trips are not necessary. The radiation dose received over a 2.5 year period on a roundtrip Mars mission involving two six month transits and an 18 month stay would have no visible effects, and be expected to increase each crew member's lifetime risk of cancer by about one percent (in contrast, the average American smoker increases his cancer risk by twenty percent). Of the half dozen astronauts and cosmonauts who have already received cosmic ray doses comparable to those that would be experienced on a Mars mission, none has experienced any radiation-induced health effects.

It may also be noted that the NEP megasystems described above utilize xenon as propellant, and have no use for the liquid oxygen that might be manufactured at the Lunar Cape

Canaveral. So, while each of these two boondoggle projects lacks merit on its own terms, taken together, they are doubly nonsensical, as neither fits together with the other.

We need to break with this kind of thinking. Unless the rope is pulled tight to define a critical path program, we will be left with a tangled mess of incoherent and useless projects which will never lead to Mars and which ultimately will fail even in their desired objective of rope-selling as their pointlessness becomes apparent.

The missing ingredient is leadership. NASA's average Apollo-era (1961-73) budget, adjusted for inflation, was about $17 billion/year in today's dollars, only six percent more than the agency's current budget. Yet the NASA of the sixties accomplished a hundred times more because it had a mission with a deadline, and was forced to develop an efficient plan to achieve that mission, and then constrained to build a coherent set of hardware elements to achieve that plan.

If the new space exploration program is to succeed, it must proceed in the same way today. To be defensible, it must be rational, which means it must actually commit itself to its true goal, and define a minimum cost, minimum schedule, plan to reach that goal. In the absence of rigorous leadership from NASA headquarters, Congress should take the initiative and instruct the space agency to report back in one year on its options for humans to Mars by 2016, with a total program budget of $50 billion or less.

The rope must be pulled tight.

***   ***   ***

It may be noted that in the above article, I take issue with the idea of designing Mars missions around "NEP Megasystems." This is because in contrast with realistic 100 kilowatt NEP systems that are possible in the near future, and which would be quite useful (I believe nearly essential) for surface power on human Mars mission, the ultralightweight 100,000 kilowatt NEP systems advocated by O'Keefe (or currently by Obama's Science advisor John Holdren) for propulsion to Mars are neither possible nor necessary, but are simply an excuse to delay the mission indefinitely.

In any case, NEP Megasystems were not ion anyone's plans for Moon missions, that Admiral Craig Steidle, the Associate Administrator for Human Exploration, was currently designing in earnest. Acting under direction from O'Keefe, Steidle's group had designed a Moon base mission architecture that did not involve the use of a heavy lift launch vehicle (HLV), as O'Keefe did not care to spend the funds required to develop such a craft. Steidle asked me to perform a formal review of this plan, which is presented in abridged form below. This is somewhat technical, so that those readers not well-acquainted with space program terminology might want to just skip it, as the "How to Build a Lunar Base" articles which follow it contain much of its most interesting content written in more popular form. However those with appropriate technical preparation should take the time to read it, despite its inevitable use of NASA bureaucratic jargon (just think of it as like being on a tour of an extraterrestrial planet ruled by Stalinoid creatures who speak a weird dialect of putative English), as its severe criticisms are equally applicable to all HLV-avoiding Moon and Mars mission architectures, including those being pushed by the Obama administration today.

Review of NASA Lunar Program Requirements Documents

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Oct. 18, 2004

The following review was performed in response to a request from Admiral Craig Steidle and the NASA Exploration Systems Mission Directorate (ESMD). It is based upon examination of documents ESMD-RQ-0010 "Exploration Super-System Requirements Document," ESMD-RQ-0019 "Crew Exploration Vehicle Concept of Operations," ESMD-RQ-0011 "Exploration Crew Transportation System Level 1 Requirements Document (Spiral #1)," ESMD-RQ-0012 "Exploration Crew Transportation System Level 1 Requirements Document (Spiral #2)," ESMD-RQ-0013 "Exploration Crew Transportation System Level 1 Requirements Document (Spiral #3)," and ESMD-RQ-0014, "Robotic Lunar Exploration Program (RLEP) Requirements." All of these documents are dated September 1, 2004, and all are marked "Preliminary."

In accord with the expressed wishes of Dr. Michael Lembeck, Director of the Requirements Formulation Division of the ESMD, this review has been done entirely independently, without any consultation with anyone either inside or outside of NASA. The purpose of this methodology is to provide NASA with a review that is truly independent, and uninfluenced by the views of either NASA or other outside reviewers. This seems to me to be a valid and important first step to take in the review process. However, it should be noted as a result of this procedure, that the ESMD authors of the cited documents have had no opportunity to explain or defend their design choices in a way that might mitigate criticism, nor have potentially critical reviewers had a chance to compare notes to strengthen counter arguments. As the issues of program design embodied in the cited documents are quite important, it is strongly recommended that presentation of this and other independent reviews be followed by extensive direct discussion and, if necessary, debate, between the ESMD and the reviewers to resolve the concerns raised.

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I. OVERVIEW

The cited documents provide the outline of a program whose stated goal is to fulfill the "Vision for Space Exploration" presented by President George W. Bush in a speech at NASA Headquarters on January 14, 2004 and enunciated in greater detail by a National Security Directive entitled "Renewed Spirit of Discovery" released by the White House at the same time.

As stated in the President's January 14, 2004 National Security Directive, the goals of the program are to:

- Implement a sustained and affordable human and robotic program to explore the solar system and beyond. [goal 1]
- Extend 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; [goal 2]
- Develop the innovative technologies, knowledge, and infrastructures both to explore and to support decisions about the destinations for human exploration; and [goal 3]
- Promote international and commercial participation in exploration to further U.S. scientific, security, and economic interests [goal 4].

(Note; The goal numbering, above i.e. “goal 1,” etc. is my own and does not appear in the original directive. I have also used a similar system, i.e. “Action B2” to identify principal bulleted subsections of the action items listed by the directive.)

The directive then goes on to list a series of actions and activities to achieve the stated goals. These include returning the Space Shuttle to flight and using it to complete construction of the ISS, but then retiring it. The directive notes that this date (completion of the ISS and STS retirement) is “planned for the end of this decade.” [Actions A1, A2, A3]

The directive states that NASA should develop “a new crew exploration vehicle to provide crew transportation for missions beyond low-Earth orbit,” conducting “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.” [Action C1] It also says that NASA should “acquire crew transportation to and from the International Space Station, as required, after the Space Shuttle is retired from service.” [within Action C2]

We note that the directive does not say that the crew exploration vehicle (CEV) needs to be used to supply crew transportation to the ISS, but if it is not, then NASA needs to "acquire" such transportation using another system.

For purposes of this review, the directives section concerning the Moon (the first four bullets within Section B.) is highly relevant. We therefore quote it in full.

The Moon

- Undertake Lunar exploration activities to enable sustained human and robotic exploration of Mars and more distant destinations in the solar system; [Action B1]

- Starting no later than 2008, initiate a series of robotic missions to the Moon to prepare for and support future human exploration activities; [Action B2]

- Conduct the first extended human expedition to the Lunar surface as early as 2015, but no later than the year 2020; and [Action B3]

- Use Lunar exploration activities to further science, and to develop and test new approaches, technologies, and systems, including the use of Lunar and other space resources, to support sustained human space exploration to Mars and other destinations. [Action B4]

To implement this directive, the cited ESMD documents outline a program consisting of five primary phases, or "spirals." In summary form, these spirals can be described as follows:

Spiral 1: Develop the CEV and its launch system and operate the CEV in low Earth orbit.

Spiral 2: Develop a Lunar Surface Ascent Module (LSAM) a cargo launch system capable of launching it, and an Earth Departure Stage (EDS) capable of delivering either the CEV or the LSAM separately from LEO to low Lunar orbit (LLO). The CEV performs a rendezvous with the LSAM in LLO, after which the crew transfers to the LSAM for an excursion to the Lunar surface of 4 to 14 days. The crew then ascends in the LSAM to rendezvous with the CEV in LLO. The crew transfers to the CEV which performs trans Earth injection and direct entry and landing at Earth. Using this hardware set, a series of 4-14 day surface-stay missions are conducted to the Moon, including its polar region.

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 (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 undefined.

In parallel with the above, a set of robotic missions are flown to the Moon and Mars to prepare or support human exploration objectives, as required. The Mars robotic precursor missions are undefined. A set of Lunar robotic precursor missions are defined in some detail in ESMD-RQ-0014.

II. SUMMARY CRITIQUE OF ESMD PROGRAM

There are numerous problems with the ESMD program as defined by the cited documents. These problems occur at three levels of the human exploration plan; specifically the programmatic, mission architecture, and vehicle design requirements levels. In addition, there are also problems with the Robotic Lunar Exploration Program (RLEP).

The problem areas are as follows:

A. Programmatic

1. There are too many spirals
2. There is inadequate traceability between spirals
3. The program as defined is not responsive to the presidential directive.

B. Mission Architecture

1. The Lunar mission architecture (spirals 2 and 3) is *severely defective* as a system for supporting either exploration of the Moon or development of a Lunar base. It:
   a) Is excessively complex
   b) Requires unrealistic launch rates

c) Has a higher recurring cost than readily apparent alternatives

d) Imposes much more mission risk than readily apparent alternatives

e) Entails greater risk to crew than readily apparent alternatives

f) Creates less exploration capability than readily apparent alternatives

g) Fails to take proper advantage of Lunar resources.

h) Is ill-adapted to taking advantage of technological advances

2. The Lunar mission architecture *is severely defective* as system for preparing human Mars exploration because almost none of Lunar hardware set is useful for Mars missions.

a) This will greatly increase overall Moon/Mars program schedule, cost, and risk

b) This undermines the presidential directive's stated rationale for the Lunar base

C. Vehicle Design Requirements

Given the proposed severely defective mission architecture, the vehicle design requirements presented in the cited ESMD documents are a mixed bag. Most are good, but some are confused, some are bad, some are nonsensical, some are fantastical, and some necessary ones are missing.

D. Robotic Lunar Exploration Program (RLEP)

Some of the requirements for the RLEP are excellent . However it is apparent that the RLEP missions as defined in the documents are not being designed to meet the needs of preparing and supporting human exploration, but of gratifying the research interests of a subset of the science community with access to requirements development group . Thus while some of the proposed RLEP activities could be quite useful to human explorers, others are not, and a number of important precursor activities that could be done are not considered.

We now address each of the above cited problem areas in more detail.

III. EXPANDED CRITIQUE OF ESMD PROGRAM

A. Programmatic

1. There are too many spirals

There program as designed entails five spirals. There should be three, as follows:

Spiral A: Equivalent in function to the present Spiral 1

Spiral B: Equivalent in function to the present Spirals 2 and 3.

Spiral C: Equivalent in function to the present Spirals 4 and 5.

Or, put another way, the present spirals 2 and 4 should be abolished as independent spirals.

The issue here is not one of terminology, but of program design. The desire is to achieve maximum science return at minimum overall program cost and risk. Spiral 2 Lunar missions accomplish much less than Spiral 3 missions, but entail comparable cost and risk. Spiral 4 Mars missions may entail risk and cost a factor of 2 less then Spiral 5 Mars missions, but the latter offer several orders of magnitude greater scientific return.

Thus Spiral 2 and 4 missions are not cost-effective, and not risk-effective (i.e. offer uncompetitive risk/effectiveness ratios), and therefore should be minimized or eliminated from the program.

Let us consider first 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 can stay on the surface much longer than spiral 2 missions, which must live in the limited habitation offered by the LSAM. Now it is obvious that a mission that operates on the surface for 40 days will accomplish much more exploration than one that stays for 4 days. This advantage for the Spiral 3 mission is amplified much further by the fact that the habitation module will have lab facilities, allowing astronauts to perform preliminary analysis of large numbers field samples while they are on the Moon, selecting only the most interesting subset to return to Earth for further study. Thus Lunar exploration in the Spiral 3 mode will vastly more effective than that possible in the Spiral 2 mode.

Of course, Spiral 3 requires a hab module and its power supply, which is an additional development and delivery cost. But the program is committed to that cost in any case, since a Lunar program that stopped at Spiral 2 would not be responsive to the president's directive. The additional cost of a Spiral 3 mission compared to a Spiral 2 flight is thus the cost of delivery of added consumables required for an extended surface stay.

Assuming that the hab has a 90% efficient oxygen and water recycling system, and neglecting (for the moment) the potential use of Lunar oxygen, then each crew member will need about 1 kg of food, 3 kg of water (includes washing water) , and 0.1 kg of oxygen per day. With

4 crew members and 36 extra days (40 day stay instead of 4 day stay), this comes down to 580 kg. If an LSAM cabin and ascent stage have an assumed dry mass of 5000 kg, the requirement to perform a 2 km/s ΔV to ascend to orbit with a space storable (such as LOX/CH4) propellant with an Isp of 370 s implies a wet mass of about 8700 kg. If we add to this a modest requirement to transport 300 kg of additional equipment one-way, we find that an landing stage must deliver 9000 kg to the surface of the Moon to carry out a Spiral 2 mission. But if we take the same system, and use it instead to deliver 9000 kg of consumables one way to the habitation module, we will have delivered enough to support **Fifteen** 40-day stay Spiral 3 missions.

So if we consider two programs of 16 LSAM lander flights to the Lunar surface, we obtain the following comparison

Table 1. Comparison of Spiral 2 and Spiral B Mission Modes

Spiral 2 mode	Spiral B Mode
16 piloted flights	1 hab flight, 1 cargo flight, 14 piloted flights
Average surface duration ~8 days	Average surface duration ~ 40 days
Up to 16 sites visited	1 site extensively explored
No lab facilities on surface	Lab facility on surface
No backup hab on surface	Backup hab on surface
128 days on surface total	560 days on surface total

We can see that while the two programs have about the same operational costs, the "Spiral B" option provides five times as much useful exploration time, and multiplies the value of this added time by the provision of superior scientific facilities.

Against this we note that the Spiral 2 expeditionary mode allows the visiting of numerous sites separated by long distances across the Moon. This disadvantage of the Spiral B hab module-centered mission mode can be mitigated, however, by several means. These include the use of robotic missions to remote sites of secondary interest, the use of piloted ballistic sortie vehicles operating out of the base (we will discuss this option further below), or simply by mixing in Spiral 2 type expeditions to the Spiral B program. For example, we could decide to send 9 of the 14 piloted flights in the Spiral B program to the primary base, but 5 others to diverse other sites of secondary interest. We would thus visit 6 sites, with one of them studied in great depth.

So a Spiral B base-centered program includes the option to launch short duration Spiral-2 type expeditions to remote sites, as required. But such sorties should be deferred until later in the program, because flights to a location where a hab module has been propositioned **are safer** than flights to an undeveloped site. A crew traveling to an undeveloped site has no safe haven other than the LSAM. A crew traveling to a propositioned hab module has both the LSAM and the hab module. The recent CAIB report strongly recommended that where possible, piloted expeditions travel to destinations where safe haven facilities are available.

Thus from the point of view of both safety and mission cost effectiveness, the correct program strategy is to develop and deploy a habitation module to the Moon **before** any human expeditions. Deferring the deployment of the hab module until after a series of Spiral 2 expeditions will waste money and expose astronauts to unnecessary risk. Thus Spiral 2 needs to be abolished.

This is not to say that the first Spiral B mission should necessarily stay for 40 days. Selecting shorter durations for initial missions is a reasonable strategy. But, for the sake of both science and safety, the hab module should be delivered first, with surface stay duration expanding as rapidly as mission experience shows to be prudent

The hab module is also the lab module. It provides the crew with the endurance and the equipment they need to do effective exploration. *We should not send explorers to the Moon without the primary tool they need to do their job.*

In the case of the issue of condensing Spirals 4 and 5 into a single Spiral C, the issue is even more clear. Mars flyby missions entail significant cost and risk, but accomplish no meaningful scientific goals. Their only valid function is to flight test hardware. (They also test human endurance, but such tests could be accomplished much more cheaply and safely in geocentric space). Thus there is neither need nor purpose to develop a separate hardware set to conduct Mars flyby missions. It might appear to make sense to develop a flight system capable of conducting actual human Mars missions (i.e. that go to the surface and conduct exploration), and flight test part of it in a trans-Mars flyby with a human crew aboard. But having flown the crew all the way to Mars, they will have absorbed that part of the risk and expense of a real Mars mission, and having done so, it would be irrational to abort the mission without cause. Therefore, it is equally irrational to design a mission to be aborted without cause from its conception.

The risk of piloted Mars missions can be reduced at no incremental cost by using the Mars mission hardware to perform useful necessary roles such as delivery of Lunar missions or of unmanned Mars cargo missions that preposition useful infrastructure for the program in orbit or on the Martian surface. At significant cost, the risk can also be potentially reduced by test flying the flight elements to the Martian surface and back without crew. However, flying an abort-by-design mission followed by an actual mission (which may or may not be aborted depending upon events) increases overall program risk and cost compared to simply flying an actual mission. For this reason Spiral 4 should be abolished.

2. There is inadequate traceability between spirals

Spiral 2 may be fairly said to be based on Spiral 1, since it makes full use the CEV and its launch system. Similarly, Spiral 3 is clearly based on Spiral 2. But neither Spiral 4 or 5 are in any serious way based on Spirals 2 or 3. That is, except for the CEV developed during Spiral 1, almost none of the hardware developed during the previous spirals is appropriate for Mars missions. With a better designed mission architecture, the Spiral 3 (or Spiral B) hardware *could be* directly useful for Mars missions (Spiral 5 or C). But that is not the case here.

3. The program as defined is **not responsive** to the presidential directive.

Because the Lunar hardware is being designed to support Lunar missions only, with no regard for Mars requirements, the program as defined is really a Moon-only program which fails to fulfill the presidents directive as specified in Goals 1 and 2 and Action B1 and Action B4, listed above. These goals and action items clearly state that the purpose of the Lunar program is to enable sustained human exploration of Mars. However, rather than enable human Mars exploration, the program as defined will be a massive and costly detour which will delay such missions beyond the working lifetime of anyone in NASA or the aerospace industry today.

We repeat the president's marching orders:

- Undertake Lunar exploration activities **to enable sustained human and robotic exploration of Mars** and more distant destinations in the solar system; [Action B1]

- Use Lunar exploration activities to further science, and **to develop and test new approaches, technologies, and systems, including the use of Lunar and other space resources, to support sustained human space exploration to Mars** and other destinations. [Action B4]

These orders have been ignored by the designers of the program as defined.

B. Mission Architecture

1.The Lunar mission architecture (spirals 2 and 3) is *severely defective* as a system for supporting either exploration of the Moon or development of a Lunar base. It:

a) Is excessively complex

b)-Requires unrealistic launch rates

c)- Has a higher recurring cost than readily apparent alternatives.

d) Imposes much more mission risk than readily apparent alternatives

e) Entails greater risk to crew than readily apparent alternatives

f) Creates less exploration capability than readily apparent alternatives

g) Fails to take proper advantage of Lunar resources.

h) Is ill-adopted to take advantage of technological advances

In order to establish the above points, we will need to do some mission analysis. In performing this analysis, the following assumptions have been used.

Table 2: Assumptions Used in Lunar Mission Analysis

CEV inert Mass	9000 kg

LSAM inert Mass	5000 kg	
LOX/H2	450 s Isp	stage dry fraction =0.13, except TLI/LOC =0.12
LOX/CH4	370 s Isp	stage dry fraction = 0.1
NTR (15 klbf)	900 s Isp	stage dry fraction = 0.18,+4000 kg for NTR &shield
Delta V	TLI =3200 m/s, LOC or TEI =1000 m/s, LLO to surface=1900 m/s	

In the above, stage dry fraction is the inert mass of a propulsive stage as a fraction of the propellant it contains. So for example a LOX/H2 stage with a dry faction of 0.13 containing 10 tonnes of propellant would have an inert mass of 1.3 tonnes.

In the baseline Spiral 2 mission plan, A CEV with a propulsive capability for TEI (1 km/s) is launched to orbit where is rendezvous with an EDS capable of delivering it to LLO (i.e. perform TLI + LOC = 4.2 km/s). Separately from this, an LSAM is launched to orbit either together or separately with an EDS, which then delivers to LSAM to LLO. The CEV performs a rendezvous with the LSAM in LLO, after which the crew transfers to the LSAM for an excursion to the Lunar surface of 4 to 14 days (1.9 km/s each way). The crew then ascends in the LSAM to rendezvous with the CEV in LLO. The crew transfers to the CEV which performs trans Earth injection (1 km/s) and direct entry and landing at Earth.

If we choose as our mission baseline LOX/H2 propulsion for the TLI/LOC stage (i.e. the EDS), and the LSAM landing stage, and space storable LOX/CH4 for the TEI stage and LSAM ascent stage, we obtain the following we masses for the primary mission components.

Table 3 Wet Masses for Primary components of Baseline Spiral 2 Mission

CEV (including TEI stage)	12.25 tonnes

LSAM (including ascent and descent stages)	15.0 tonnes
EDS for CEV	27.05 tonnes
EDS for LSAM	33.0 tonnes
Total	87.3 tonnes

In the Baseline mission, the CEV is called out as to be launched separately, in apparent conformity to the presidential directive [Action C2] dictate to "separate to the maximum practical extent crew from cargo transportation." Whether this is the case, we will discuss further below. For now, we will accept it as a given. The architecture is open as to how the remaining mass will be launched. However if the plan were to launch all the rest together in a single 76 tonne to LEO launch, there seems little point in splitting up the CEV and its EDS from the rest, sending it to the Moon separately for a subsequent mission-critical LOR with the LSAM. Instead we would simply have a single 60 tonne TLI EDS launched with the LSAM, and rendezvous this set with the CEV and go to the Moon as a single flight. (This "two-launch" plan would be a much better plan than the baseline, but it is not the baseline, so we set it to one side for now.)

Rather, the clear intent of the baseline plan is to launch the non-CEV elements in several launches, so as to eliminate the need to develop a heavy lift launch vehicle (HLV). Examining the masses in Table 3, we see than a medium lift vehicle with a capacity of 33 tonnes to the LEO staging orbit would be able to launch the mission in 3 flights, provided that the CEV were launched together with the LSAM. This would a good idea, but it ground ruled out, so instead we are left with four launches, two of a 33 tonne to LEO booster and two of a 15 tonne to LEO booster. Such medium lift vehicles (MLVs) could be created by enhancement of the current line of EELVs fielded by Lockheed or Boeing. However we note that:

i. The total lift capacity represented by the 4 MLVs required would be 96 tonnes to LEO, despite the nominal 87 tonne mission LEO mass.

ii. Packaging concerns have been ignored, and it is not clear that the small launch fairing of a 15 tonne to LEO MLV would be sufficient for the LSAM, so a bigger MLV may be required

iii. Four MLV launches are required per mission.

iv. The above four launches must be done quickly, since the EDS and LSAM vehicles are carrying cryogenic LOX/H2 hydrogen, and the piloted CEV is launched last.

v. Four mission critical rendezvous operations are required per mission

vi. The crew flies to the Moon without the LSAM.

Points i., ii, and iii, above speak to costs of the program. Instead of paying for launching 87 tonnes to orbit per mission, we are paying for launching 96 tonnes , and more importantly doing it by the non-cost effective means of using multiple MLVs to launch an HLV payload. It is a well known feature of launch vehicle economics that larger boosters are more economic on a cost/kg basis than smaller boosters. We illustrate this with a listing a sample cases (source for all but last entry: S. Isakowitz, et al "International Reference Guide to Space Launch Systems, third edition, AIAA, Reston Va, 1999. Where Isakowitz has given a range, such as $90-100 million, we cite the mean – $95 million. Source for last entry: Lockheed Martin HLV briefing)

Table 4. How Economics of Launch Vehicles Improves with Size

Vehicle	LEO Delivery (kg)	Cost ($M)	kg/$M	$/kg
Pegasus XL	443	13.5	32.8	30,488
Taurus	1,320	19.0	69.5	14,388
Delta IV Medium	8,600	82.5	104.2	9,597
Delta IV Medium plus	13,600	97.5	139.5	7,168
Delta IV Heavy	25,800	155.0	166.5	6,006
Atlas IIAS	8,618	97.5	88.4	11,312
Atlas IIIB	10,718	97.5	109.9	9,099
Atlas V 400	12,500	82.5	151.5	6,607
Atlas V 500	20,050	97.5	205.6	4,864
Lockheed HLV	150,000	300	500	2,000

Examining Table 4 we see a very clear trend, the larger the launch vehicle, the cheaper the launch per kg. The Atlas V heavy is more than twice as economical a launch system as the Atlas IIAS, and Lockheed cost projections for their Atlas derived HLV are more than twice as economical as those for the Atlas V 500. These data are shown again in Fig. 1, where they are also compared to a scaling law: cost/kg ~ $L^{-0.5}$, where L is the booster's lift capability.

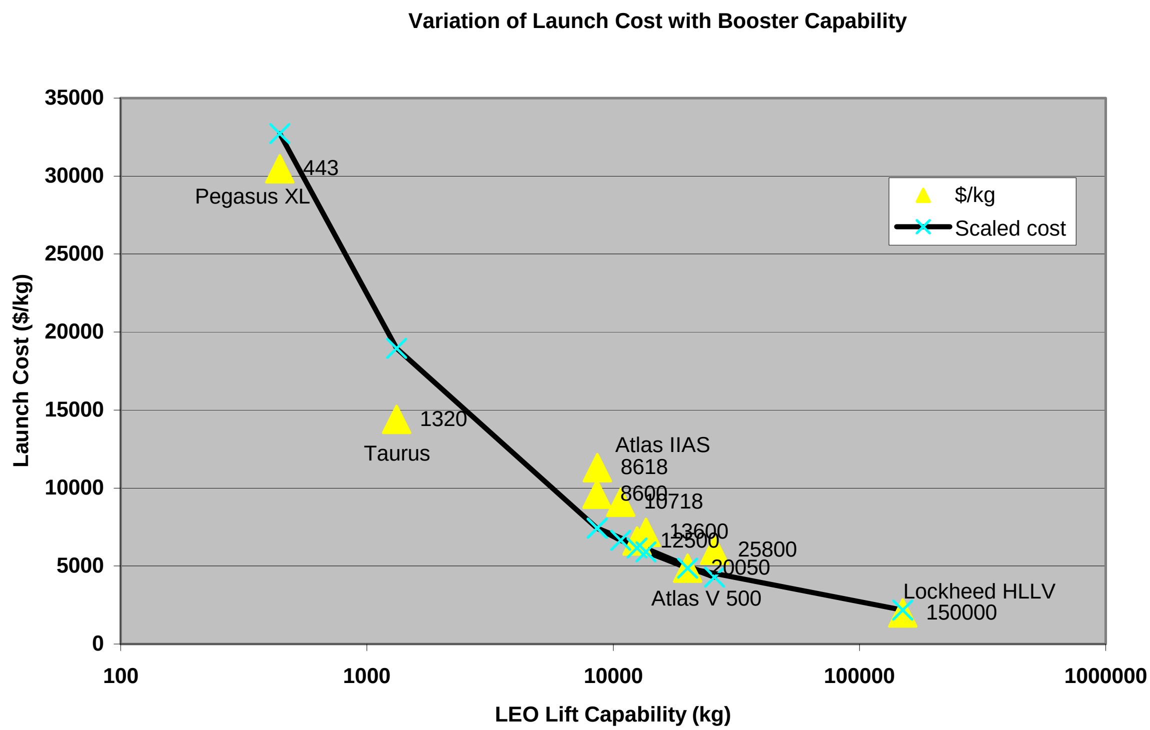


Fig. 1. Launch Cost as a function of Booster Size. Cost/kg scales as $L^{-1/2}$.

It can thus be seen that by adopting a strategy of multiple MLV launches, the baseline ESMD plan will maximize program launch costs.

Points iii and iv speak to feasibility. The program requires four MLV launches within a very short period. According to ESMD the requirements documents, the required period is about two weeks *for four MLV launches*, three of which involve cryogenic upper stages, and the fourth involving a piloted vehicle, all from the Cape. 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 wild, unsupported optimism.

It may be remarked that unlike certain other unproven technologies that might be proposed for a Lunar mission program, the failure to develop such an ultra-high readiness launch vehicle has not been due to lack of interest or funds. On the contrary, this is a problem that a great deal of effort and attention has been expended on for the past four decades, with a long track record of non-accomplishment. Therefore, a decision to base the Lunar program on the availability of such technology is unsound.

Point iii and v. also speak to complexity and mission risk. In contrast to the Apollo mission plan, which only required one launch and a single rendezvous, or the two-launch plan mentioned in passing above, which required two rendezvous (one EOR, one LOR), the baseline plan requires four mission-critical rendezvous and four launches to all occur successfully. That's eight big chances per mission (in addition to Lunar landing and ascent) for an operational failure that would cause loss of mission. Now this mission architecture is supposed to support not a single Lunar mission, but routine, repeated access to the Moon, including, in Spiral 3, logistical support of a Lunar base. Inserting so much complexity and vulnerability into such a transportation system makes no sense. It is an open invitation to program failure.

Let us consider for a moment the magnitude of the risk that the ESMD quadruple launch, quadruple rendezvous strategy would introduce into the program. Launch vehicle development program officials like to throw around claims that their vehicles are 0.999+ reliable, but the actual record of experience is much different. Table 5 shows the actual success record of the Delta, Atlas, and Titan lines through 1999 (data from Isakowitz, 3rd edition)

Table 5. Success Record of US Medium Lift Vehicles (through 1999)

Vehicle Family	Number of Launches	Number of Successes	Success Rate
Delta	271	253	93.3 %
Atlas	305	265	86.9%

Titan	203	184	90.6 %
Total	779	702	90.1%

Now it is true that launch vehicle reliability tends to improve with experience, but it is sobering to note that despite this trend, the actual success record of the Atlas between its 201st and 250th launch of was only 88%.

Based on the historical record, then, the actual expected reliability of a US MLV is only about 0.90. But for the sake of discussion, let us suspend disbelief and generously concede a reliability of 0.98 to the MLVs used on the ESMD program. This represents a failure rate *five times* lower than the track record. At this level of reliability, the probability that a four-launch Lunar mission could avoid outright failure due to launch vehicle failure would be 0.922. The mission would also, fail, however, if a launch delay caused any of the three launches after the first to stall too long for cryogenic propellant onboard orbiting payload #1 to last until TLI, or if any of the four orbiting payloads were to take an orbital debris hit while waiting in LEO for TLI, or if any of the four spacecraft should malfunction, or if either of the two TLI or two LOC burns should fail, or if any of the four orbital rendezvous operations should fail, to name just a few additional sources of mission failure that multiply in proportion the number of flight elements and critical operations. If we assign a reliability of 0.99 for each of the four spacecraft and each of the four rendezvous operations and TLI and LOC burns, and a probability of on-time launch of 0.98 to each of the three follow-up launches required per mission and for the one Lunar landing operation, and disregarding all other sources of potential mission failure, we obtain a total mission reliability of:

Mission Reliability = (0.98^4 for 4 successful launches)(0.99^12 for 4 rendezvous, 4 S/C, 2 TLI & 2 LOC)(0.98^4 for 3 on-time launches & 1 Lunar landing) = (0.9227)(0.886)(0.9227) = **0.754**

Or, put another way, using this plan roughly one out of every four missions could be expected to fail.

The program requirements state that the minimum rate of missions will be one per year, with a maximum of 4. If we assume the mean of these two figures for an average of 2.5 missions flown per year, then using this plan, we could expect a mission failure every 1.6 years. If we assume a typical suspension of operations of two years after each mission failure, we find that the program could be expected to be shut down for failure investigation roughly 55% of the time.

This is not a good way to design a program.

Point vi speaks to risk to crew. Apollo traveled to the Moon with the LEM attached to the Command Module. Availability of the LEM during transit proved essential to saving the lives of the Apollo 13 crew. Has the ESMD baseline plan been employed by the Apollo program, the crew of Apollo 13 would all be dead.

It will be observed that the “two-launch” alternative cited above also travels to the Moon with the LSAM, which offers comparable resources to the LEM. Thus if this “two-launch” plan were adopted, an important crew safety feature of Apollo missions could be preserved. As noted above, because it uses only two launches instead of four, and two rendezvous instead of four, the two-launch mission plan will also be lower cost and risk than the four-launch quadruple rendezvous ESMD baseline

The question now arises; why not take this logic one step further and integrate the CEV on the same launch stack as the LSAM and EDS, and launch the whole affair in one-launch, with only one rendezvous per mission, just as was done during Apollo? Is the only answer "because the presidential directive [Action C2] instructs us to "separate to the maximum practical extent crew from cargo transportation?" This is not a sensible answer. The directive is quite clear; it says "separate to the maximum <u>practical</u> extent." The decision to separate the CEV launch from the rest of the mission stack must therefore be justified as being practical. But it is not. Separating the CEV launch from the rest of the launch stack adds an MLV launch and a mission-critical rendezvous to the mission with no countervailing benefits. On the contrary, it adds risk to both hardware and crew.

With respect to the issue of risk to hardware, there is a misunderstanding in some quarters to the effect that launching a mission in separate pieces reduces hardware risk because only a subset of the hardware is placed in jeopardy at any one time. In fact, the opposite is true. To see this clearly, consider the case of the proposed two-launch mission and compare it to a one-launch scenario. In the two-launch mission, we launch both EDS units (or a single larger EDS unit) and the LSAM on the first launch, and the CEV on the next. So in the first launch, the two EDS units and the LSAM are each exposed to one launch's worth of risk. This is also true of the one-launch plan. But in the two-launch plan, the three units launched first are subjected to a *second* unit of launch risk when the CEV is launched, because if the CEV launch fails, the program will stall and the cryogenic EDS stages and LSAM will certainly suffer disabling boiloff long before another CEV can be launched to use them.

So while in the one-launch scenario, each of the payload elements need only survive one unit of launch risk, in the two-launch scenario the CEV must also survive one launch, *but the other payload elements must <u>each</u> survive <u>two</u> launches*!

The situation is even worse in the four-launch ESMD baseline, as in that case, the first payload launched must survive four launches, the second payload must survive three launches, the third must survive two launches, and only the final payload element need only survive a single launch.

Thus we find, that for a mission involving four payload elements:

One-launch requires 4 elements risked once => 4 units of payload-launch risk

Two-launch requires 3 elements risked twice and one once =>7 units of payload-launch risk

Four-launch requires 1 element risked 4 times, 1 thrice, 1 twice, one once=>10 units of p-l risk.

A rational mind must therefore enquire, why should we subject most of the very expensive payload elements used in our Lunar missions to extra rolls of the launch dice? The only possible justification would be that by doing so, we are adding materially to crew safety.

However there is no particular reason to believe that a CEV launched on top of an HLV stack will be any less safe than one launched on top of an MLV. On the contrary, by launching the crew after the LSAM/EDS HLV payload, we are adding significantly to not only hardware and mission risk, but crew risk, because the pre-launched EDS and LSAM will be placed on orbit exposed to micrometeorites and orbital debris for an extended period prior to use. If launch delays stall the launch of the crew, this would result in the EDS stage(s) and LSAM becoming increasingly unreliable and unsafe to use. It could be very difficult to determine when this point is reached. If one errs on the conservative side, billions of dollars of hardware would be needlessly lost. If one errs on the optimistic side, the crew will be lost.

The safest transportation system for the crew to use is that which has spent the minimum of time in Earth orbit prior to employment by the crew. *That means that the safest transportation system is one that launches with the crew.*

The wisdom of launching crew separately from cargo is legitimate in the case of arguing against using self-launching space stations like the Shuttle as a cargo delivery system. Such systems are inefficient as cargo launchers because of the large inert mass of the habitable Orbiter. One does not use a Winnebago to transport cross-country freight. But if you are going on a trip in a Winnebago, it is a much better idea to include the Winnebago's engine in the vehicle at the start of the journey, instead of arranging a series of rendezvous with strangers to tow you from town to town installing various parts along the way.

The Choice of Mission Mode

Having thus dispensed with the four-launch, quadruple-rendezvous ESMD baseline, and even the improved two-launch, double rendezvous alternative in favor of a single launch system, it is now time to discuss whether the Lunar orbit rendezvous (LOR) mission mode assumed as necessary by the ESMD plan is optimal for a Lunar base.

The Apollo program utilized the Lunar orbit rendezvous (LOR) plan to reduce overall mission mass. However it should be observed that Apollo employed low-performance hypergols for all propulsion maneuvers after TLI, a design choice which increases the value of staging propellant in low Lunar orbit. Furthermore, Apollo's mission requirement was simply to transport a person to the Moon and back- the amount of useful activity done on the Moon was not a major consideration. Thus the fact that one third of the Apollo crew was not participating usefully in exploration operations on the Lunar surface did not matter, nor did it matter that they were risking three people on the mission to accomplish a two-person sortie to the Moon. If the criterion had been maximizing the number of person-days spent *on the Moon* for a given launch mass, a Direct Return (DR) scenario(launching directly back to Earth from the Lunar surface without orbital rendezvous would have been quite competitive with LOR, becoming more advantageous as higher specific impulse propellants are used for post TLI propulsion.

Moreover, if the objective is to establish a permanent Lunar base, and not just perform sorties to the Moon, then the production of Lunar oxygen is feasible, and because of the numerous advantages it offers, must perforce be an early base priority. Once Lunar oxygen is available, DR defeats LOR as a mission mode on a mass basis. This can be seen to be true since LOX/Methane burns at 3.5:1 mixture ratio, which means that a craft which brings its methane fuel to the Moon and adds the oxygen from local sources obtains a total propellant mass 4.5 times greater than it transported. If we multiply this leverage of 4.5 times the physical specific impulse of 370 s of the LOx/CH_4 engine, we find that such a system is operating with an effective specific impulse of 1665 s – comparable to electric propulsion, but at high thrust! If the ERV ascent propulsion were LOX/ H_2, (which we might not baseline because of the more difficult long term cryogenic storage issues associated with liquid hydrogen), then the effective specific impulse of the direct return system rises to over 3100 s (7.0 leverage times 450 s = 3150 s). Such high levels of effective specific impulse overwhelm the advantages offered by orbital propellant staging, and the advantage is further increased by the elimination of the dry mass (and development and production cost) of the orbital spacecraft and second crewed vehicle as well.

In addition to these mass advantages, substituting DR for LOR enhances overall program safety. In the DR mode, the launch window back to Earth is always open, no waiting for phasing is required. The risk of a failed LOR maneuver (which would cause loss of crew) is also eliminated. Lunar orbits are unstable, and if the Lunar orbiting craft is not used within its allotted time, it will be lost. The LOR mission plan is also faced with unattractive choices regarding the maintenance of the orbital craft. It could be decided to of keep someone aboard the orbital craft watching over it, in which case a person is being subject to flight risk, zero gravity health impacts, and radiation dose with no matching addition of surface exploration capability. Alternatively, the orbital craft could be left unmanned, in which case it could be lost if a problem needing correction should develop while it is alone (thus stranding the base crew on the Moon), or, if the defect is not detected, the returning crew could be lost when they discover they have met up with a non functional return system.

Thus, for a Moonbase program, the DR mission mode is clearly preferable. In Tables 6 we show the results of trade studies exploring the mass implications of employing either the DR or LOR one-launch mission plans using various technological options. Mission assumptions are those presented in Table 2, above. In cases involving In-situ propellant production, only production of Lunar oxygen is assumed. In all cases the fuel required to burn with the LLOX (CH4 or H2) is assumed to be transported from Earth.

Table 6 Initial Mass of Lunar Missions in LEO (tonnes)

Mode	TLI-LOC Stage	Post TLI Fuel	No ISPP	ISPP
LOR	H2/O2	CH4	87.3	72.1
LOR	H2/O2	H2	81.3	68.1

DR	H2/O2	CH4	121.0	69.2
DR	H2/O2	H2	104.7	61.5
LOR	NTR	CH4	56.5	47.9
LOR	NTR	H2	53.1	45.7
DR	NTR	CH4	75.5	46.3
DR	NTR	H2	66.4	41.9

It can be seen in Table 6 that before ISPP is employed LOR edges DR in absolute mass terms, but only succeeds in doing so on a person-days-on Moon/tonnes-in-LEO basis ("mass-effectiveness") if it takes the extra risk to leave its CEV unmanned in Lunar orbit during the excursion. If the LOR mission planners decide to reduce risk by keeping a pilot in the CEV during the surface excursion, then the non-ISPP LOR (3 person on surface) and DR (4 person on surface) missions stand equal on a mass-effectiveness basis. However once ISPP is introduced, then the DR mission defeats LOR in absolute mass terms, and enjoys a major advantage in mass-effectiveness unless the LOR mission is willing to accept the extra risk of abandoning its CEV during the surface excursion.

In Table 7, we show the impact of technological improvements on evolving DR and LOR Lunar base transportation systems. The baseline case for each is assumed to employ H2/O2 for TLI, LOC, and lunar landing, CH4/O2 for all post landing burns (i.e. lunar ascent and TEI), and no ISPP. We then present the mass reduction factor (factor by which initial mass in LEO is reduced) if NTR, ISPP, or post landing H2/O2 propulsion ("cryo") are introduced either separately or in combination.

Table 7 Mass Reduction Factor for Lunar Transportation Technologies

Technology	LOR	Direct Return
Cryo post landing	1.07	1.16
NTR for TLI-LOC	1.55	1.60
ISPP	1.21	1.75
Cryo & NTR	1.64	1.82
Cryo & ISPP	1.28	1.97
NTR & ISPP	1.82	2.61
Cryo & NTR & ISPP	1.91	2.89

It can be seen that while both LOR and DR missions can be improved by these three technological advances, the DR mission gains more. This is strikingly true for ISPP.

It will also be observed that the effect of the three technologies is synergistic, with the benefit of any two in combination close to the product of the two taken separately.

System Evolution Using NTR

We observe that NTR benefits both LOR and DR missions strongly. We also note that since the combined ΔV of TLI and LOC is 4.2 km/s , identical to the ΔV required for Trans-Mars Injection (TMI) of a human Mars mission leaving Earth on 2-year free-return trajectory (- and thus the safest option, with a ~6-month outbound transit to Mars), the NTR stage used for this purpose to support Lunar missions could serve an identical function in support of Mars exploration.

Let us assume that we start the Lunar program using a H2/O2 TLO+LOC stage, but then introduce an NTR TLI+LOC (or TMI) stage in a later "Block 2" spiral. In Table 8, we show how the capability of the system evolves.

Table 8 Evolution of Lunar-Mars Systems using NTR Stage

Mode	Post Landing Propul.	IMLEO	Block 1 H2/O2		Block 2 NTR	
			TMI	L-surface	TMI	L-surface
LOR	CH4/O2	87.3	27.2	9.1	42.2	18.1
LOR	H2/O2	81.3	25.4	8.3	38.9	16.5
DR	CH4/O2	121.0	37.8	22.8	60.5	36.5
DR	H2/O2	104.7	32.7	19.8	51.3	31.1

In Table 8, we see under IMLEO the launch mass of each mission option, with zero margin (These results are trade studies for comparative purposes, not mission designs. If these scenarios are to be used as mission designs, mass margin should be added.). If these missions are to be launched in a single launch, then a booster with this lift capability, plus margin, needs to be developed. The next column, labeled "Block 1 H2/O2," is the amount than an H2/O2 stage could then deliver to TMI (which is also the amount it could deliver to TLI+LOC) or the Lunar surface (L-surface). Examining this column, we see another important advantage of the DR architecture, that is, the much larger payload that the DR missions deliver to the Lunar surface. For example, if we consider the baseline mission, the DR option has an IMLEO 1.39 times that of the LOR mission, *but it delivers 2.5 times as much payload to the Lunar surface*. So, while for either option such Block 1 systems could implement piloted Lunar missions under the scenario indicated, if the DR mode is used, the potential for Lunar base development is far better.

No consider the columns under "Block 2 NTR." These show the amount that the same booster could deliver to TMI (and thus TLI+LOC) or the Lunar surface (L-surface) if we were to switch out the H2/O2 TLI/LOC stage and replace it with an NTR stage. It can be seen that the Lunar surface delivery capability of the DR options becomes truly muscular, (*quadruple* the capability of the Block 1 LOR options!), but not only that, the system now has the capability to throw 50 to 60 tonnes on direct Trans-Mars Injection. This is sufficient to undertake human Mars missions without on-orbit assembly using either a two-launch Mars Direct or three-launch Mars Semi-Direct mission plan (references: R. Zubrin and D. Weaver "Practical Methods for Near-Term Piloted Mars Missions," AIAA 93-2089, republished in JBIS July 1995, and R. Zubrin "The Case for Mars," The Free Press, Simon and Schuster, New York, 1996.).

Thus, the use of a single-launch Direct Return Lunar mission architecture provides a simple, robust, and powerful method for effectively creating, supporting, and expanding a Lunar base, and, with block 2 NTR augmentation for TLI+LOC, provides a direct evolution to a transportation system capable of enabling human Mars exploration.

Finally, we note in passing that while either (LOR or DR) of these single-launch Lunar mission architectures receives very strong benefit by the subsequent introduction of NTR technology, the same is less true for the ESMD 4-launch scenario, since in that case two (expensive) NTR TLI/LOC stages will need to be expended per mission, instead of one with these single launch options. The use of a single 15 klbf NTR to send a ~100 tonne spacecraft on TLI or TMI is entirely feasible, as documented by studies done in the early 1990's which showed that it could done without significant gravity losses by dividing the injection maneuver into three perigee kicks, with the engine firing in a thrust arc +-45 degrees around periapsis.

System Evolution Using ISPP

The Moon's surface is about 50% oxygen by weight, with the oxygen occurring in the form of oxides of various metals, as shown in Table 9.

Table 9 Average Lunar Soil Analyses.

Constituent	Average of Apollo 16 Soils	Average of Selected Apollo 14 Soils	Average of Apollo 17 Soils	Average of Apollo 15 Soils	Average of Selected Apollo 12 Soils	Sample 10002 Apollo 11 Soil	Average of Luna 24 Soil
	Descartes Crater (Highlands)	Fra Mauro Crater (Highlands)	Serentitatis Basin (Highland-Mare Boundary)	Hadley-Apennine (Highlands & Mare)	Oceanus Procellarum (Mare basalt)	Mare Tranquillitatis (High Ti basalt)	Mare Crisium
SiO_2	45.0	48.1	43.2	46.8	46.3	42.2	43.9
Al_2O_3	27.3	17.4	17.1	14.6	12.9	13.6	12.5
FeO	5.1	10.4	12.2	14.3	15.1	15.3	19.8
TiO_2	0.5	1.7	4.2	1.4	3.0	7.8	1.3
MgO	5.7	9.4	10.4	11.5	9.3	7.8	9.4
CaO	15.7	10.7	11.8	10.8	10.7	11.9	12.3
K_2O	0.17	0.55	0.13	0.21	0.31	0.16	0.04
Na_2O	0.46	0.70	0.40	0.39	0.54	0.47	0.11
MnO	0.30	0.14	0.17	0.19	0.22	0.20	0.25
Cr2O3	0.33	0.23	0.33	0.36	0.34	0.30	0.32
P_2O_3	0.11	0.51	0.12	0.18	0.40	0.05	0.11
S	0.07	---	0.09	0.06	---	---	0.14
TOTAL	100.8	99.8	100.1	100.8	99.1	99.8	100.2

It is important to note that the iron concentrations in Table 9 range from a low of about 5 percent to a high of about 20 percent (as FeO). Typical values are in the 10 to 15 percent range, which is not strikingly different from the averages shown for Mars. While Lunar iron oxide is reported in Table 9 (and conventionally elsewhere) as "FeO," it frequently occurs in a variety of mineralogical forms in association with other metal oxides, such as ilmenite, which is FeO-TiO_2, or $FeTiO_3$. Nevertheless, it is the iron oxide part of these minerals which is most important, because it is the easiest common metal oxide to reduce with chemical reagents such as hydrogen or carbon monoxide.

Thus for example we can readily reduce FeO at temperatures as low as 600 C using reactions such as:

$FeO + H_2 \Rightarrow Fe + H_2O$ (1)

Or

$FeO + CO \Rightarrow Fe + CO_2$ (2)

The use of reaction (1) for Lunar oxygen production was demonstrated by Carbotek during the 1990s. Reaction (2) has been used in iron production for several thousand years. The water produced by reaction (1) can then be broken down by electrolysis to produce oxygen product and hydrogen, which can be recycled to reduce more FeO. The CO_2 produced by reaction (2) can by reduced back to CO via :

$CO_2 + H_2 \Rightarrow H_2O + CO$ (3)

Reaction (3) is the reverse water gas shift, which has been known to chemistry since the 19th century. It has a moderately unfavorable equilibrium constant (~0.1) at temperatures of interest, but in 1997 Pioneer Astronautics showed that it could be driven to completion using an air separation membrane and a condenser to eliminate the products from the effluent stream and then recycle. It is also a reaction of great interest for use on Mars, where it can be used to strip oxygen from the prevalent CO_2 atmosphere.

Thus, reduction of Lunar iron oxides to produce iron and oxygen is well understood. A important difficulty, however, is that the iron oxide is generally only about 12% of the regolith. Thus Lunar oxygen processes that only reduce the iron must process a great deal of material, and waste a lot of power heating up oxides of other metals which remain inert.

In order to deal with this problem, an alternative process known as carbothermal reduction has been investigated. The first to study this process was Sanders Rosenberg, who conducted experiments at Aerojet in the 1960s showing that by using methane as a reductant at somewhat higher temperatures (~1400 C), he could reduce not only the iron, but also the silicon oxide (which is typically ~45% of the regolith) as well as several minor constituents. The problem with Rosenberg's process, however, is that the methane tended to coke out in the system in an uneven way, causing carbon deposits to form that could not be recovered for further reaction without interventions such as scraping.

In 2002, Pioneer Astronautics found a solution to this problem by using carbon monoxide reductant instead. In the Pioneer process, CO was used to reduce the FeO present in the sample using reaction (2). However, Pioneer continued to add CO after all the FeO was reduced. This caused the CO to react with itself within the sample via;

$$2CO \Rightarrow CO_2 + C \quad (4)$$

The carbon so produced would deposit completely homogenously throughout the sample. Once this had occurred, the temperature was raised to 1400 C, during which process the C would react with the oxides of silicon, phosphorus, manganese, sodium, potassium, and part of the titanium to strip them of their oxygen. This would leave only the oxides of the much more refractory magnesium, aluminum, and calcium unreduced (collectively about 40% of the regolith). Thus, using this process, it becomes possible to access the oxygen present in about 60% of typical Lunar surface material. This cuts the power required for Lunar oxygen production by about a factor of four and also frees the program from the need to select an iron oxide-rich location for the base.

Alternative potential processes for reducing Lunar regolith include systems based on fluorine chemistry, sulfuric acid dissolution and selective precipitation of iron oxide, and direct ultra-high temperature pyrolysis. There is also some evidence that water ice or permafrost could exist in permanently shadowed locations near the Moon’s poles. If this is true, then oxygen (and hydrogen) could be made at such locations using permafrost mining and water electrolysis.

The point however is that there are methods available for producing oxygen nearly anywhere on the surface on the Moon. By combining Lunar oxygen with fuels transported (if necessary) from Earth, base support costs can be greatly reduced while scientific capacity can be radically expanded.

Long Range Mobility on the Moon

While a smaller world than the Earth, the Moon is still a very big place, and it cannot be explored on foot. Many of the important impact craters and other geological features are hundreds of kilometers in size, and proper investigation requires systems that can traverse such distances. Another science objective of great interest is the establishment of optical interferometers. These are arrays of optical telescopes whose data are correlated by computer in a manner analogous to the Very Large Array of radiotelescopes in Socorro, New Mexico. Ideally, such an array would span distances comparable to the diameter of the Moon, as this could allow it to produce images with a resolution approximating that of a telescope with such an aperture. Such a phenomenal instrument would be able to image geographic features on planets at interstellar distances and perform other astounding feats which, taken together, provide much of the scientific justification for a Lunar base. While individual telescopes comprising elements of this array would probably be landed directly at their designated locations by delivery systems launched from Earth, it would be most convenient and cost effective to be able to perform necessary maintenance and upgrade visits by sorties from the Moon base. Thus the needs of both Lunar geology and astronomy generate a strong desirement, and possibly a requirement, for Lunar mobility systems capable of traversing continental distances.

While methanol/oxygen fuel cell-driven pressurized rovers could enable regional mobility, the difficulty of much of the Lunar terrain combined with the time demands of travel dictate that true long distance mobility on the Moon can best be achieved using flight vehicles. Since the Moon has no atmosphere, such flight vehicles must be ballistic hoppers utilizing rocket propulsion.

The range of a ballistic projectile on a spherical planet with no atmosphere is given approximately by:

$$R = r/[(V_o/V)^2 - 1] \qquad (5)$$

Where R is the projectile's range, V_o is the low orbital velocity, V is the takeoff or muzzle velocity, and r is the radius of the planet. Of course, in the case of a Lunar ballistic hopper, a given flight sortie will involve four rocket ΔV (2 takeoffs, 2 landings) each equal at least to the factor V in equation (5), and further inflated by gravity losses during ascent and descent. If we assume such gravity losses are equal to 25% of the ideal ΔV in question, then the total ΔV required by the hopper to perform a flight sortie and return over a given range R will be five times the value of V as given by equation (5).

The propellant requirements for long distance flight with such a vehicle are presented in Table 10. In Table 10 it is assumed that the hoppers in question have a dry mass of 5 tonnes. The LOx/H_2 hopper is assumed to have a specific impulse of 450 s and burns at a mixture ratio of 6:1, while the LOx/CH_4 hopper has a specific impulse of 375 s and burns at a mixture ratio of 3.5:1.

Table 10. Propellant Requirements for Long Distance Lunar Ballistic Hopper

Sortie ΔV	Range (km)	LOx/H_2 Hopper		LOX/CH_4 Hopper	
		Propellant	Fuel	Propellant	Fuel
1.0 km/s	25	1.27 tonnes	0.18 tonnes	1.56 tonnes	0.35 tonnes
2.0 km/s	104	2.87	0.41	3.62	0.80
3.0 km/s	253	4.87	0.67	6.31	1.40
4.0 km/s	508	7.38	1.05	9.85	2.19
5.0 km/s	949	10.5	1.51	14.5	3.22
6.0 km/s	1799	14.5	2.07	20.6	4.57
7.0 km/s	3911	19.5	2.78	28.6	6.35

Let us examine Table 10, and consider as a typical example the ballistic hop with a round trip ΔV of 5 km/s. This has a sortie range of 949 km, which means that a Moon base at the center of a series of such sorties could access an entire hemisphere of the Moon. We observe that a 5 tonne LOx/H_2 hopper would need 10.5 tonnes of propellant to perform such a mission, but only 1.51 tonnes of this would be hydrogen. So if we wanted to visit a number of sites on the Moon, instead of flying individual mission to each from Earth with our complete HLV/DR flight system, we could fly a single cargo lander to the Moonbase loaded with hydrogen. Returning to Table 8, we see that our DR system architecture using H2/O2 for post TLI propulsion can deliver 19.8 tonnes to the Lunar surface. Let us assume that if this is a hydrogen shipment, 4 tonnes is tankage while 16 is liquid hydrogen. Since we require only 1.51 tonnes per hopper sortie, this shipment would support 10.6 sorties, a radical increase in number of sites visited per HLV launch from Earth. If the hopper system uses LOx/CH_4 to obtain greater storability, then 3.22 tonnes of fuel are needed for each sortie, which still allows 6.2 sorties to distant sites for each HLV-launched 22.8 tonne cargo lander (20 tonnes CH4, 3 tonnes tankage) fuel delivery from Earth.

Thus we see that the use of Lunar oxygen leads to an order of magnitude improvement in overall Moonbase cost-effectiveness. There is no other technology that has anything approaching this dramatic effect. For this reason, the development and rapid implementation of Lunar oxygen production technology should be a top priority for the Moonbase program.

But as we have seen, once ISPP is available, DR is more mass efficient than LOR. Therefore, since ISPP's ability to multiply the cost-effectiveness of Lunar exploration by an order of magnitude makes the case for developing such technology totally compelling, DR should be chosen over LOR, as it is simpler, safer, more capable, more mass-effective, and requires development of fewer spacecraft.

[We note that since DR does not use an LSAM, it lacks the Apollo-13 LEM-lifeboat safety feature whose absence we criticized in the ESMD architecture. However the point there is that if you have an LSAM, you are voluntarily giving up a safety feature if you choose to send the crew to the Moon without it. The DR mission lacks an LSAM, and thus cannot use it as a backup. But by the same token, it never needs to depend on an LSAM. The LOR mission requires <u>both</u> an LSAM <u>AND</u> the CEV to work properly if the crew is to get home alive. The DR mission only requires a CEV.]

Thus, by employing a DR architecture together with ISPP, we can create a Lunar transportation system where a single launch allows human explorers to visit 10 sites on the Moon. This compares quite handsomely to the ESMD plan, where 4 launches are required to visit one site.

The Question of HLV Development

The single launch Lunar mission architectures recommended in this review require development of a heavy lift vehicle (HLV), whereas the ESMD plan does not. Indeed, it is quite clear that the desire to avoid the development of an HLV is the prime motivator of the ESMD mission architecture. That may be, however, as we have shown, the ESMD decision to avoid the expense of HLV development leads it to an unsound and untenable quadruple-launch, quadruple rendezvous mission architecture that will expose the Lunar base program to massive cost, risk, and almost certain failure. Furthermore, the Lunar base building capabilities that the resulting hardware set possesses are marginal, at best, and there is no substantial traceability of the hardware to that required to enable human Mars missions. By refusing to develop an HLV, the ESMD plan is refusing to do what is necessary to perform its mission .

The HLV required to enable single launch Lunar missions and direct launch (with two-three launches) Mars missions, is, depending upon scenario and technology assumptions, somewhere in the 90 to 130 tonne to LEO range. HLVs of this class (Energia, Saturn V) have already been developed in the past, and so there is no technical risk whatsoever associated with the assumption that such systems can be developed in the future.

Cost estimates for HLV development range from ~$2 billion for Shuttle-derived vehicles to ~$4 billion for clean-sheet systems like the proposed Lockheed 150 tonne to LEO HLV. The trade between these systems involves comparing lower development costs (for Shuttle-derived) to lower recurring costs (for clean-sheet.) However either of these approaches will have much lower recurring costs than the quadruple MLV launch approach, and since the Moon-Mars program is an open-ended initiative, at the end of the day, recurring costs will dominate. Furthermore, even if an ultra-high readiness MLV could be developed capable of being launched four times in two weeks, no one can guarantee the weather two weeks in advance, so the quadruple-launch Lunar mission architecture is impractical in any case.

There is no alternative that allows for mission success. A competent Moon-Mars initiative must develop an HLV.

Correct Design Decisions made by the ESMD Team

While this review has been critical of many aspects of the ESMD mission design, there are a number of potentially controversial points where we find that the ESMD team has made correct decisions. Since some may question these decisions, it is useful to put our concurrence on record.

Important correct choices made by the ESMD team include its decisions to avoid reusability, avoid L1 staging, avoid the development of L1 infrastructure, avoid the development of LEO infrastructure, and avoiding the use of electric propulsion. We briefly discuss each of these in turn.

* No Reusability: Reusability of expensive space hardware sounds attractive, but there is a mass penalty associated with reusability, and due to the nature of the rocket equation, this becomes increasingly damaging as one moves up the launch stack. Furthermore, in order to support reusability in space, one needs infrastructure in space, which requires technological development, launch, and logistic support itself. During the 90-Day Report Study associated with the 1989 Space Exploration Initiative, NASA management ordained reusability as a system priority, with the result that the 90 Day Report included proposals to spend billions developing orbital support facilities whose use, if successful, would have allowed for the preservation of hardware worth tens of millions. At that rate, it would have taken hundreds of Lunar missions to break even, and the situation was made even worse by the fact that the imperative to reuse the Lunar transportation system (or Space Transfer Vehicle, STV, as it was then called) increased launch mass, and thus launch cost, to the point where there was no recurring saving at all. The ESMD was wise to avoid this costly mess.

* No L1 staging: As the ESMD supporting team documented at length in their document ESMD-RQ-0006 “Lunar Architecture Broad Trade Study Final Report,” L1 staging increases (~doubles) Lunar mission flight transit time, mission mass (+~25%), and ΔV, with no

countervailing benefits. It should therefore be avoided in favor of either Lunar orbit staging or direct return.

* No L1 infrastructure: Since L1 staging is bad, spending vast sums of money to create infrastructural liabilities in L1 to support such staging is even worse. Until recently, a team at NASA HQ had been pushing an L1 space station as a “gateway to everywhere.” In fact, such a station would have been a sinkhole that added no capabilities to the transportation system. Furthermore, the effort to justify such a flying toll-booth by forcing missions to use it as their rendezvous node would have placed a useless burden on the missions. The ESMD is to be commended for having dispensed with this crazy diversion.

* No LEO infrastructure: LEO infrastructure to support Lunar missions is unnecessary. Injecting it into the plan therefore adds cost and liability, with no benefit. ESMD was right to shun such creations.

* No Electric propulsion: Supporting electric propulsion is fashionable at NASA these days, and some recent plans have tried to enhance its prestige by giving it a starring role in human Lunar transportation systems. This is a mistake. Electric propulsion would increase transit time to the Moon by two orders of magnitude, so obviously it is unsuitable to the crewed portion of the flight system. That reduces its role to transporting cargo, such as the LSAM, from LEO to LLO. The ΔV to do this with electric propulsion is about 8 km/s, compared to 4.2 km/s with high thrust propulsion. The EP system might still produce a recurring mass saving, however, since its exhaust velocity is ~ten time s that of a chemical system. However we note that due to its long

transit time, it would probably be necessary for an EP-transported LSAM to use space storable propulsion for its landing stage, instead of H2/O2, and this would eat into its mass savings. Further reduction is mass-effectiveness is caused by the mass of the EP system itself.

Lets say the goal is to land a 20 tonne payload on the Moon. We could do this with a H2/O2 TLI+LOC stage followed by an H2/O2 landing stage, an NTR TLI+LOC stage followed by an H2/O2 landing stage, or by an EP TLI+LOC system followed by a CH4/O2 landing stage. Assuming 5000 s Isp of the EP system, with an mass/power ratio ("alpha," or α) of 30 kg/kW (less than half the weight of the system being designed for JIMO, whose α =65 kg/kW), a thruster efficiency of 0.7, and a tank dry fraction of 0.1, results are shown in Table 11.

Table 11. Comparison of EP to High Thrust Lunar Cargo Missions (20 tonnes to L-Surface)

TLI+LOC System	IMLEO (t)	Flight Time Out	EP Power Level	
H2/O2	106.0	3 days	0	
NTR	67.2	3 days	0	3 t NTR expended
EP	65.1	324 days	600 kW	18 t EP expended
EP	86.6	215 days	1200 kW	36 t EP expended
EP	129.8	162 days	2400 kW	72 t EP expended
EP	69.3	345 days	600 kW	18 t EP reused
EP	95.2	237 days	1200 kW	36 t EP reused
EP	146.9	183 days	2400 kW	72 t EP reused

Observing Table 11, we see that EP can be mass-competitive with NTR only by reducing power levels to the point where transit times are 100 times greater. Even here, the amount of high technology nuclear flight hardware that must be expended to perform the mission is 6 times greater than NTR. This could be mitigated by reusing the EP hardware, but doing so would require extensive orbital infrastructure and technology development. It will also be observed that the payloads so delivered by the EP system will spend many months transiting through the Earth's radiation belts, an experience which will threaten their electronics and thus mission safety. Furthermore, since the 600 kW option (the only mass attractive one) has a round trip time of close to two years, supporting 4 missions per year with this technology would require at least 8 such vehicles constantly in operation between the Earth and the Moon. That is very unlikely to be cheap or even manageable. In short, EP is not a good choice for Lunar missions. The ESMD team was right to avoid it.

Summary of Mission Architecture Trades

In Table 12 we present a summary of the results of the preceding discussion comparing the ESMD quadruple-launch quadruple-rendezvous mission architecture with simpler alternatives. More complex alternatives rightly rejected by ESMD are not considered. Mission mass assumptions are those presented in Table 2. Mission reliability calculations are based on the assumptions of 0.98 for each successful launch, of 0.99 for each spacecraft and each rendezvous operation and TLI and LOC burns, and a probability of on-time launch of 0.98 to each follow-up launches required per mission and for the one Lunar landing operation. Launch vehicle recurring cost is based on the scaling law presented in Fig 1 [recurring cost/launch = M$300(L/150)$^{1/2}$, where L is the booster lift capacity in tonnes], launch vehicle development cost is assumed to scale in parallel fashion, but somewhat more strongly with mass[development cost/vehicle = G$4(L/150)$^{3/4}$,] spacecraft development cost is assumed to be $0.5 billion for human-rated unmanned vehicles (such as independently maneuvering TLI stage S/C), $0.3 billion for man- rated stages that are not autonomous, and $5 billion for manned vehicles. The recurring cost of manned spacecraft is set at $500 million. The recurring cost of autonomous unmanned spacecraft is set at $100 million, and $50 million for non-independent stages. We do not include the development cost of the MLV used for CEV launch, since that is developed in Spiral 1, but we do including its recurring cost. Surface habitat development cost, including production of the one required unit is assumed to be $5 billion for a 20 tonne habitat, with cost scaling as the habitat mass$^{3/4}$. Non transportation miscellaneous system development costs (reactors, spacesuits, rovers, etc.) are assumed to be $5 billion, and recurring costs $100 million. Mission operations are assumed to cost $100/mission. Diagrams showing the mission plans are presented in Fig. 2. Wire diagrams showing the series of risk events required for mission success in each mission scenario are presented in Fig. 3

It can be seen that the one-launch DR option has booth the lowest development costs and the lowest recurring costs, since the elimination of the need to develop and produce the LSAM more than counterbalances the need to develop and produce a somewhat larger HLV. The one-launch DR option also has the highest reliability, the highest Lunar base development capability (more than double the surface cargo delivery of the other options), the best evolvability with both NTR and ISPP technologies, and the best evolvability to Mars. It should be noted that the DR options ability to deliver a much more massive hab to the Lunar surface not only will greatly increase program scientific return through provision of adequate scientific facilities in-situ, but also improves program safety since it will be possible to provide the surface hab with a much more robust set of backup systems. The one-launch DR plan is therefore the clear winner. The ESMD architecture has comparable development costs to the other LOR plans, but the worst recurring cost and overall program cost, low Lunar base capability, low ISPP and NTR evolvability, no significant Mars traceability, and absolutely unacceptable mission risk. It is the clear loser.

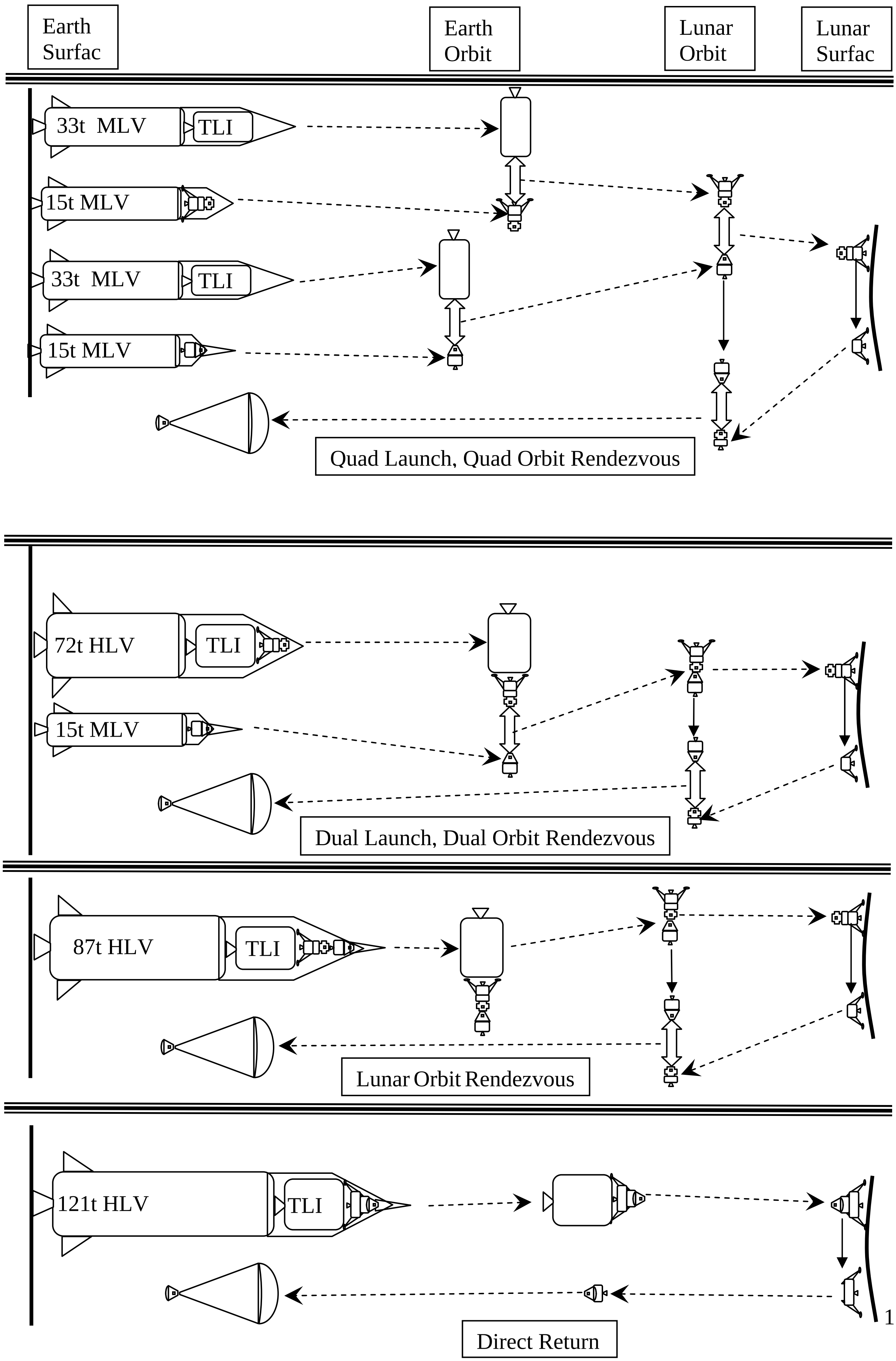
One can challenge the specific choice of numbers used for these calculations, but for any reasonable set, the same trends will always hold. The one-launch DR option will always have the lowest program cost, lowest mission risk, best Lunar base building capability, best ISPP and NTR evolvability, and best Mars traceability. The ESMD plan will always be the worst in all of these categories.

Table 12. Summary of Mission Trades

Feature	ESMD	Two-launch-LOR	One-Launch LOR	One Launch DR
# Launches	4	2	1	1
# Time Critical Launches	3	1	0	0
# Rendezvous	4	2	1	0
# Spacecraft/mission	4	2	2	1
MLV Development ?	yes	no	no	no
HLV Development ?	no	yes	yes	yes
LV Readiness Requirement	Ultra High	High	Low	Low
IMLEO without ISPP (t)	87.3	87.3	87.3	120
IMLEO with ISPP (t)	72.1	72.1	72.1	69.2
L-surface cargo capability (t)	9.1	9.1	9.1	22.8
LV Development Cost G$	1.3	2.3	2.7	3.4
Manned S/C Devel. Cost G$	10	10	10	5

Auto S/C Devel Cost G$	1	0.5	0	0
Non-auto stage Devel cost	0.3	0.6	0.9	0.9
Surface Hab Devel. Cost	2.8	2.8	2.8	5.5
Non-transport Misc Devel	5	5	5	5
Total Development Cost G$	**20.4**	**21.2**	**21.4**	**19.8**
LV Recurring Cost G$	0.471	0.303	0.228	0.269
Manned S/C Recur Cost G$	1.0	1.0	1.0	0.5
Auto S/C Recur Cost	0.3	0.1	0	0
Non-auto stage recur cost	0.05	0.1	0.15	0.15
Non-transport misc. recur	0.1	0.1	0.1	0.1
Mission operations	0.1	0.1	0.1	0.1
Total Recurring Cost G$	**2.021**	**1.703**	**1.578**	**1.119**
Total Cost Dev+20 mission	**60.82**	**55.26**	**52.96**	**42.18**
Launch Success Reliability	0.922	0.9602	0.98	0.98
Launch On-time Reliability	0.941	0.98	1.0	1.0
TLI+LOC Reliability	0.9606	0.98	0.98	0.98
Rendezvous Reliability	0.9606	0.98	0.99	1.0
S/C Reliability	0.9606	0.98	0.98	0.99
Lunar Landing Reliability	0.98	0.98	0.98	0.98
Total Reliability	**0.755**	**0.868**	**0.913**	**0.932**
#Missions between Failures	**4.08**	**7.58**	**11.49**	**14.71**
Lunar Base Capability	Very Poor	Poor	Fair	Excellent
ISPP Evolvability	Fair	Fair	Fair	Excellent

NTR Evolvability	Fair	Good	Good	Excellent
Mars Evolvability	Very Poor	Fair	Good	Excellent
Overall Grade	**D**	**C**	**B**	**A**


Earth Surfac
Earth Orbit
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33t MLV
TLI
15t MLV
33t MLV
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15t MLV
Quad Launch, Quad Orbit Rendezvous
72t HLV
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Dual Launch, Dual Orbit Rendezvous
87t HLV
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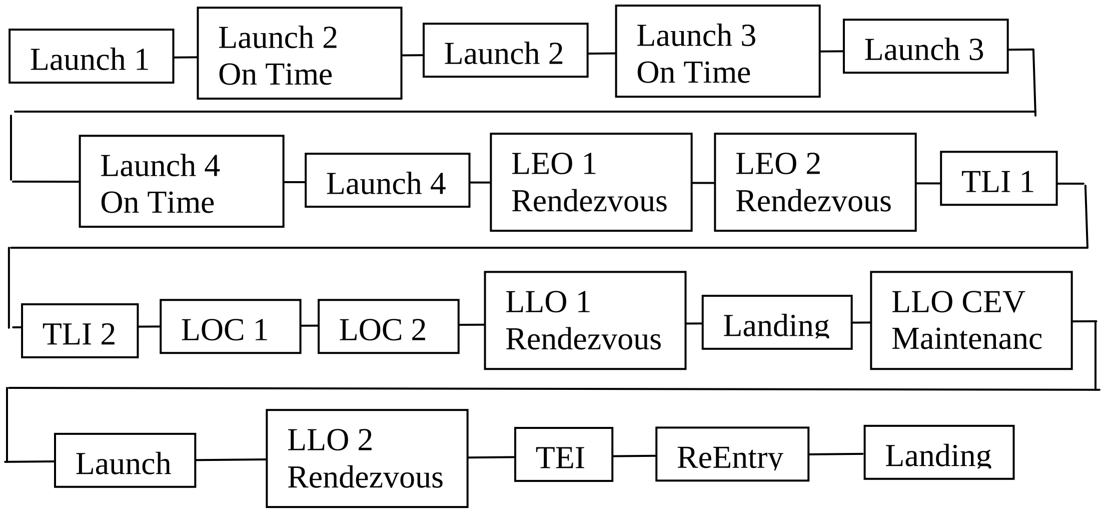
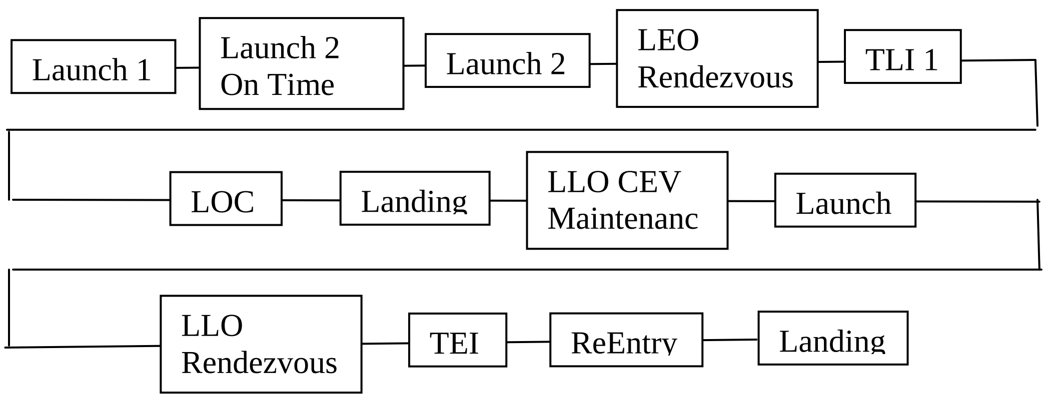
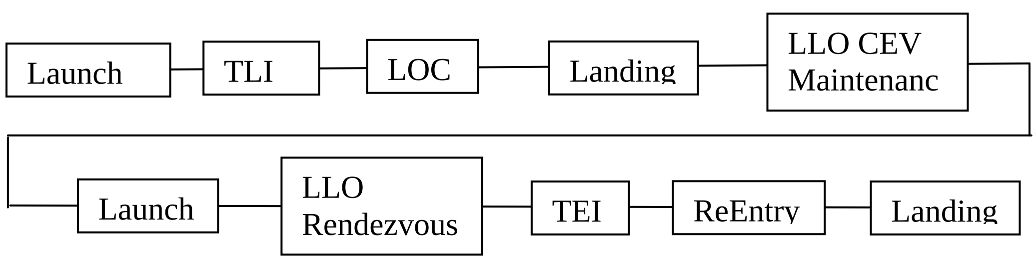
Lunar Orbit Rendezvous
121t HLV
TLI
Direct Return



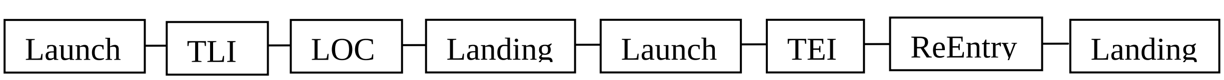
Baseline Mission Success Events



Dual Launch, Dual Orbit Rendezvous mission Success Events



Lunar Orbit Rendezvous Mission Success Events



Direct Return Mission Success Events

Fig. 3. Risk Events for Each Mission Scenario. The ESMD 4-launch scenario is too complex.

2.*The Lunar mission architecture is severely defective as system for preparing human Mars exploration because almost none of Lunar hardware set is useful for Mars missions.*

b) This will greatly increase overall Moon/Mars program schedule, cost, and risk.

In the preceding section, we considered the development and recurring cost and risk of various Lunar mission architectures strictly from the point of view of the Lunar base program itself, and found that the ESMD plan was inferior to other options employing HLVs. These conclusions, however, become even more forceful when one considers that the President's directive calls for a Moon-Mars program, not just a Moon program. The ESMD plan accepts a plethora of additional development and recurring costs and mission risks for the sole purpose of avoiding the development cost of an HLV. Yet, since the goal of the program is to get humans to Mars, an HLV and a potent TMI stage will need to be developed anyway. So on a cost basis the ESMD plan will lose twice over, since there will be much more hardware to develop for Spiral 4-5 (or Spiral C). Furthermore, in addition to imposing the greatest mission risk for Lunar explorers through its own excessive complexity, the ESMD plan will also increase the risk to Marts explorers relative to the other plans, because the ESMD Lunar plan will not test the Mars mission hardware. In contrast, using the other HLV-large TLI/LOC stage based options, hardware that is directly applicable to human Mars exploration will be developed and extensively exercised in advance in the course of the Lunar program. This will greatly reduce the cost and risk of human Mars exploration, and shorten the schedule required to transit from Spiral 3 to 4-5 (or B to C).

c) This undermines the presidential directive's stated rationale for the Lunar base.

The purpose of the Lunar base, as stated in the president's directive, is to prepare the way for human Mars exploration. The Lunar program as defined in the ESMD architecture does not do this, and thus will open the program up to the valid criticism that it is not, in fact, delivering the goods that it is promising. Human Mars exploration offers the potential to resolve fundamental issues concerning the prevalence and diversity of life in the universe. These are questions of deep scientific, philosophical, and public interest, and the commitment to the search for truth to answer them provides a strong and solidly rational intellectual foundation for the program. A properly designed Lunar transportation system could support this goal directly, and by doing so, make the Lunar program much more defensible in the scientific, public, and political arenas. This is critical for program success, because the extended schedule of the program requires that it survive through many changes of political fortune. By designing a Lunar transportation system that is useless for Mars, the ESMD plan threatens to fatally weaken the program.

C. Vehicle Design Requirements [Deleted for brevity]

D. Robotic Lunar Exploration Program [Deleted for Brevity]

IV CONCLUSION

The ESMD program plan as reflected in the documents submitted for review represents an advance over other recent NASA thinking in that it rejected a number of recently fashionable but highly impractical ideas for Lunar mission support such as the L1 gateway, reusable spacecraft, and electrical propulsion. However the program remains deeply suboptimal for the reasons listed below and documented at length in this review.

Programmatic

1. There are too many spirals

2. There is inadequate traceability between spirals

3. The program as defined is not responsive to the presidential directive.

Mission Architecture

1. The Lunar mission architecture (spirals 2 and 3) is *severely defective* as a system for supporting either exploration of the Moon or development of a Lunar base. It:

a. Is excessively complex

b. Requires unrealistic launch rates

c. Has a higher recurring cost than readily apparent alternatives

d. Imposes much more mission risk than readily apparent alternatives

e. Entails greater risk to crew than readily apparent alternatives

f. Creates less exploration capability than readily apparent alternatives

g. Fails to take proper advantage of Lunar resources.

h. Is ill-adapted to taking advantage of technological advances

2. The Lunar mission architecture *is severely defective* as system for preparing human Mars exploration because almost none of Lunar hardware set is useful for Mars missions.

a. This will greatly increase overall Moon/Mars program schedule, cost, and risk

b. This undermines the presidential directive's stated rationale for the Lunar base

Vehicle Design Requirements

Given the proposed severely defective mission architecture, the vehicle design requirements presented in the cited ESMD documents are a mixed bag. Most are good, but some are confused, some are bad, some are nonsensical, some are fantastical, and some necessary ones are missing.

Robotic Lunar Exploration Program (RLEP)

Some of the requirements for the RLEP are excellent. However it is apparent that the RLEP missions as defined in the documents are not being designed to meet the needs of preparing and supporting human exploration, but of gratifying the research interests of a subset of the science community with access to requirements development group . Thus while some of the proposed RLEP activities could be quite useful to human explorers, others are not, and a number of important precursor activities that could be done are not considered.

Concluding Remark

As a final remark, we observe that the central cause of the severe defects in the ESMD's proposed human Lunar exploration mission architecture is the failure of the ESMD to embrace development of the heavy lift vehicle (HLV) necessary for a successful program. As a result, an extremely high risk, high cost, and low Lunar mission capability program has been proposed, with no effective traceability to Mars. In this review we have documented the superiority of the one-launch, one-rendezvous Apollo mode over the quadruple-launch, quadruple-rendezvous ESMD plan, and argued for the still greater superiority of a one-launch, zero rendezvous Direct Return (DR) mission mode as a means of implementing a very capable Lunar base transportation system that also opens the way to Mars. Indeed, if one compares the ESMD plan to the DR option, we see that the only benefit the ESMD plan obtains in exchange for its huge number of mission risks associated with its multitude of launches, spacecraft, and in-space operations is its ability to forgo to cost of HLV development. The DR approach, on the other hand, has much fewer risks, and is able to forgo the development of the LSAM and several other technologies. The development costs of the crewed LSAM and the HLV may be comparable, but the LSAM is useful only for Lunar missions, and adds to their recurring cost, while the HLV reduces Lunar mission recurring cost and can also be used to enable human Mars exploration, other NASA missions, and a wide spectrum of commercial and military applications as well.

We therefore recommend that the ESMD Lunar mission architecture be redesigned, with the one-launch LOR and one launch DR mission plans considered as the primary options. In trading these two, the ESMD needs to consider development cost, recurring cost, mission risk, Lunar base development capability, technical evolvabilty, and traceability to Mars. Once this is done, a set of vehicle requirements that reflects the optimum mission architecture can be defined.

*** *** ***

In other words, just as in the case of Hubble rescue, the O'Keefe regime's approach to human exploration of the Moon and Mars was totally FUBAR. This could not go on forever. On December 13, 2004, in the face of heavy criticism, he resigned. A full year had been wasted, without accomplishing anything towards implementing the Vision. Several months then went by, without a successor being named. I took the time to publish the key observations drawn from the Steidle study and elsewhere, so as to hopefully help NASA do better the next time. The first set of articles, "How to Build a Lunar Base," which were serialized in the industry weekly *Space News*, presents in more popular form most of the central technical recommendations made to Steidle. Taken together, it amounts to a handbook on the proper approach for a Lunar Base program. The second piece, "Getting Space Exploration Right," run in *The New Atlantis*, a Washington DC think tank publication devoted to issues of science and technology, was written to educate the political class overseeing the American space program at its highest levels.

How To Build a Lunar Base

Part 1: The Launch Issue

Space News, February 21, 2005

First in a Series of 3 Articles

President Bush has called on NASA to implement a human lunar exploration program with the objective of both supporting operations on the Moon and developing the technologies to enable piloted Mars missions. The question is: how should this be done? Three central issues that need to be addressed are launch strategy, lunar mission mode, and method of evolution from Moon to Mars exploration capabilities.

With respect to the launch issue, the key question is whether or not we need a heavy-lift launch vehicle.

Currently, those opposed to such development have advanced an argument for a quadruple-launch, quadruple-rendezvous mission architecture employing medium-lift launch vehicles. As the success or failure of the program depends upon the practicality of its launch strategy, this concept needs to be carefully scrutinized.

Quadruple Launch, Rendezvous

In the quadruple rendezvous mission plan, a Crew Excursion Vehicle (CEV) is launched into orbit where it rendezvous with an Earth departure stage capable of delivering it to low lunar orbit.

Separately, a lunar surface and ascent module and another Earth departure stage are launched. They also rendezvous and head for low lunar orbit.

Once in lunar orbit, the CEV rendezvous with the lunar surface ascent module so the crew can take it down to the lunar surface. Once their excursion on the Moon is over, they use their ascent module to rendezvous with the CEV in low lunar orbit. They can then take the CEV all the way back to Earth's surface.

If we choose liquid oxygen/hydrogen propulsion for the Earth departure stage and the lunar surface ascent module descent stage and then use a space-storable propellant of liquid oxygen/methane for the trans-Earth injection and lunar ascent stage, the mass that has to launch from Earth is 12 metric tons for the CEV (including the trans-Earth injection stage), 15 metric tons for the lunar module (including ascent and descent stages), 27 metric tons for the Earth departure stage used by the CEV and 33 metric tons for the departure stage used for the lunar module.

So, quadruple mission could indeed be launched by two medium-sized launch vehicles capable of lifting 30 metric tons to low Earth orbit and two medium launch vehicles capable of launching 15 metric tons to low Earth orbit.

However, packaging concerns have been ignored in this scenario, and it is not clear that the small-launch fairing of a 15 ton to low Earth orbit medium-launch vehicle would be sufficient for the lunar module, so it is possible a bigger medium-launch vehicle might be required. But four medium-launch vehicles are required for each mission.

The above four launches must be done quickly, since the Earth departure and lunar surface ascent vehicles are carrying cryogenic liquid oxygen/H2 hydrogen, and the piloted CEV is launched last. In the quadruple scenario, the crew also flies to the Moon apart from the lunar module. These features are all causes for great concern.

Using multiple medium-launch vehicles to launch a heavy-lift payload is not cost effective.

It is a well-known feature of launch-vehicle economics that larger boosters are more economic than smaller boosters. Thus, by dividing the launch mass into four parts, the overall launch costs per mission roughly doubles.

The quadruple scenario requires four launches of medium launch vehicles within a very short period of just a few weeks. Three of those launches involve cryogenic upper stages and the fourth involves a piloted vehicle, all launched from Cape Canaveral. Such a launch rate has never been accomplished by medium launch vehicles with any payload, and to assume that it can be done repeatedly with payloads of this complexity is wildly optimistic.

In contrast to the Apollo mission plan, which only required one launch and a single rendezvous, the quadruple plan requires four mission-critical rendezvous and four launches to all occur successfully. That's eight big chances per mission (in addition to lunar landing and ascent) for an operational failure that would cause loss of mission.

The mission would also fail if a launch delay caused any of the three launches after the first launch to stall too long for the cryogenic propellant aboard the initial orbiting payload to last until trans-lunar injection, or if any of the four orbiting payloads were to take an orbital debris hit while waiting in low Earth orbit for trans-lunar injection.

The mission also fails if any of the four spacecraft malfunction, or if either of the two trans-lunar injection or two lunar orbit capture burns should fail, or if any of the four orbital rendezvous operations should fail, to name just a few additional sources of potential mission failure that multiply in proportion the number of flight elements and critical operations.

This mission architecture is supposed to support not a single 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 program failure.

In fact, an elementary calculation using very optimistic assumptions (presented in detail at www.spacenews.com) shows that, at best, the quadruple plan using medium lift vehicles might obtain a mission reliability of about 0.75. 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.

This is not a good way to design a program.

Apollo traveled to the Moon with the lunar excursion module attached to the command module. The availability of the lunar excursion module during transit proved essential to saving the lives of the Apollo 13 crew. The quadruple plan lacks this important safety feature.

The reason the quadruple mission scenario 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. The way to solve this problem is simple: develop a heavy-lift vehicle that allows the entire mission to be launched with a single booster, just as was done during Apollo.

This will cut program launch costs in half, and reduce the risk of mission failure by a factor of four. It also creates and exercises a system that is directly useful to enable human Mars exploration, which is the primary purpose of the lunar program as stated in the president's directive.

Some people within the aerospace establishment understand that the development of a heavy-lift vehicle is essential for a successful lunar program, but wish to postpone consideration of the issue for political reasons.

This is very unfortunate. One of the cheapest options to create a heavy-lift launch vehicle is by converting the shuttle. The shuttle launch stack has the same takeoff thrust as a Saturn 5, and if we delete the orbiter and add a hydrogen/oxygen upper stage, we can create a launch vehicle with similar capability.

However, under NASA's current plans, only about 25 more shuttle launches are contemplated, and absent a plan for shuttle conversion to a heavy-lift launch vehicle, much of the industrial infrastructure for manufacturing key shuttle-system components (such as external tanks) will soon be dismantled. Recreating such capabilities after they have been lost will cost the taxpayers billions of dollars.

If such massive waste is to be avoided, NASA needs to make the case for heavy lift immediately.

Next article: The Question of Mission Mode

How to Build a Lunar Base:

Part 2: The Mission Plan

Space News, February 28, 2005

Second in a Series of 3 Articles

Week One: The best way to get people and payload to the Moon is a heavy lift launch vehicle.

In our previous article we discussed the question of launch strategy for a Lunar program and showed that the Quadruple Launch Quadruple Rendezvous (QQ) mission scenario currently being discussed in some quarters is technically unsound and needs to be replaced by a single launch strategy employing a heavy lift vehicle (HLV). It is now time to discuss whether the Lunar orbit rendezvous (LOR) mission mode assumed as necessary by many is optimal for a Lunar base.

The Apollo program utilized the Lunar orbit rendezvous (LOR) plan to reduce overall mission mass. However it should be observed that Apollo employed low-performance hypergols for all propulsion maneuvers after Trans Lunar Injection (TLI), a design choice which increases the value of staging propellant in low Lunar orbit. These advantages diminish sharply if higher performing propellants such as LOx/hydrogen or LOx/methane are used for post TLI propulsion.

Moreover, if the objective is to establish a permanent Lunar base, and not just perform sorties to the Moon, then the production of Lunar oxygen is feasible, and because of the numerous advantages it offers, must perforce be an early base priority. Once Lunar oxygen is available, DR defeats LOR as a mission mode on a mass basis, as we shall show below.

In addition to these mass advantages, substituting DR for LOR enhances overall program safety. In the DR mode, the launch window back to Earth is always open, no waiting for phasing is required. The risk of a failed LOR maneuver (which would cause loss of crew) is also eliminated. Lunar orbits are unstable, and if the Lunar orbiting craft is not used within its allotted time, it will be lost. The LOR mission plan is also faced with unattractive choices regarding the maintenance of the orbital craft. It could be decided to of keep someone aboard the orbital craft watching over it, in which case a person is being subject to flight risk, zero gravity health impacts, and radiation dose with no matching addition of surface exploration capability. Alternatively, the orbital craft could be left unmanned, in which case it could be lost if a problem needing correction should develop while it is alone.

Thus, for a Moonbase program, the DR mission mode is clearly preferable. In Table 1 we show the masses either the DR or LOR one-launch mission plans using various technological options, assuming a CEV with a dry mass of 9 tonnes and an Lunar Surface and Ascent Module (LSAM) cab with a mass of 5 tonnes. In cases involving In-situ propellant production (ISPP), only production of Lunar oxygen is assumed, while the fuel required to burn with the oxygen (CH_4 or H_2) is assumed to be transported from Earth.

Table 1 Required Initial Mass of Lunar Missions in LEO (tonnes)

Mode	Post Landing Fuel	No ISPP	ISPP
LOR	CH_4	87.3	72.1
LOR	H_2	81.3	68.1
DR	CH_4	121.0	69.2

DR	H2	104.7	61.5

It can be seen in Table 1 that before ISPP is employed LOR edges DR in absolute mass terms, but that once ISPP is introduced, then the DR mission requires less mass than LOR, and enjoys a further major advantage in mass-effectiveness (person-days on the Moon/mission mass) unless the LOR mission is willing to accept the extra risk of abandoning its CEV during the surface excursion.

Developing a Lunar base will also require delivering substantial cargo one-way to the Lunar surface. Here the DR scenario has a large advantage over LOR, since it can use the same large standard lander to deliver substantial habitat modules or other large cargo elements to the surface as it employs to deliver the fully-fueled CEV. Thus, starting from the very first mission, substantial facilities can be available to the crew on the Moon, enhancing their scientific capabilities, safety, and enabling longer, and thus more cost-effective surface stays. In contrast, the LOR system must use the Lunar Surface and Ascent Module as its lander, and its cargo capability is much less. Once ISPP is introduced, both plans can also deliver significant cargo on every piloted flight (neither can before ISPP becomes available), but here again, the delivery capability of the DR plan is triple that of the LOR hardware.

Table 2 Lunar-Cargo Delivery Capability (tonnes)

Mode	Post Landing Fuel	LEO mass	Cargo w/o Crew	Cargo with Crew
LOR	CH4	87.3	9.1	3.0
LOR	H2	81.3	8.3	2.6
DR	CH4	121.0	22.8	9.9

DR	H2	104.7	19.8	8.3

Achieving Long Rang Mobility

Because it has no roads or atmosphere, true long distance mobility on the Moon can only be achieved using rocket powered ballistic flight vehicles. Assuming a methane/oxygen ballistic hopper with a dry mass of 5 tonnes, it would take about 15 tonnes of propellant to send it on a flight of 1000 km, land, takeoff again, and return. But of the 15 tonnes, 12.7 would be oxygen, which can be made on the Moon, while only 3.3 is methane fuel that needs be transported from Earth. But looking at Table 2, we can see the DR missions' standard lander can deliver 22.8 tonnes of cargo per mission, and if 20 tonnes of this were methane, that would enable six such long range excursions to be conducted from the lunar base for every launch from Earth. If hydrogen/oxygen propulsion were used on the hopper, ten such distant sites could be visited for every cargo flight. This compares quite handsomely to the QQ plan, where 4 launches are required to visit one site.

Thus we see that the use of Lunar oxygen combined with a DR mission architecture leads to an order of magnitude improvement in overall Moonbase cost-effectiveness, enabling well-equipped long-duration surface stays, with many diverse sites visited during each mission.

Furthermore, because it minimizes the number of critical operations, the single-launch DR plan is much lower risk than the QQ plan, and significantly lower than the single launch LOR architecture, both of which provide much less capability. In addition, the DR mission offers lower development cost and lower recurring cost than either alternative plan, because while it must develop a 120 tonne to LEO HLV, the need to develop a complete manned spaceflight system, the LSAM, is avoided, and the need to expend such system on each Lunar mission is eliminated as well.

Trading off the LSAM development costs to create an HLV is a very good deal, because the HLV needs to be developed to enable human Mars missions in any case, and in addition can be used to support any number of other national goals (including a truly muscular Moon base program), while the LSAM is only useful for supporting a severely constrained form of Lunar exploration.

The QQ plan would leave us with a set of inefficient, costly, and failure-ridden missions that lead nowhere, while the single-launch DR option would provide us with a robust and cost-effective Lunar program that develops a significant fraction of the hardware set that takes us to Mars and beyond.

Next Article: Evolution to Mars

How to Build a Lunar Base

Part 3 Evolution to Mars

Space News, March 7, 2005

Third in a series of three Commentaries

Week One: The best way to get people and payload to the Moon is a heavy lift launch vehicle.

Last Week: The best mission scenario for establishing a lunar base.

In the preceding articles, we considered the advantages and disadvantages of lunar mission architectures strictly from the point of view of the lunar base program itself and found that the currently fashionable quadruple launch, quadruple rendezvous plan was inferior to other options employing heavy-lift vehicles (HLVs) to accomplish missions in a single launch.

These conclusions, however, become even more forceful when one considers that the president's directive calls for a Moon-Mars program, not just a Moon program. The quadruple plan accepts a plethora of additional development and recurring costs, and mission risks for the sole purpose of avoiding the development cost of an HLV. Yet, since the goal of the program is to get humans to Mars, an HLV and a potent trans-Mars injection stage will need to be developed anyway. So on a cost basis the quadruple plan will lose twice over, since there will be much more hardware to develop for Mars.

Furthermore, in addition to imposing the greatest mission risk for lunar explorers through its own excessive complexity, the quadruple plan also will increase the risk to Mars explorers, because the quadruple lunar plan will not test the Mars mission hardware. In contrast, using the HLV-large upper-stage-based options, hardware that is directly applicable to human Mars exploration will be developed and extensively exercised in advance in the course of the lunar program. This will reduce the risk of human Mars exploration and shorten the schedule required to transit from lunar activities to Mars exploration.

The quadruple plan also undermines the lunar program rationale. The purpose of the lunar base, as stated in the president's directive, is to prepare the way for human Mars exploration. The lunar program as defined in the quadruple architecture does not do this, and thus would open the program up to the valid criticism that it is not, in fact, delivering the goods that it is promising.

Human Mars exploration offers the potential to resolve fundamental issues concerning the prevalence and diversity of life in the universe. These are questions of deep scientific, philosophical and public interest, and the commitment to the search for truth to answer them provides a strong and solidly rational intellectual foundation for the program.

A properly designed lunar transportation system could support this goal directly, and by doing so, make the lunar program much more defensible in the scientific, public and political arenas. This is critical for program success, because the extended schedule of the program requires that it survive through many changes of political fortune. By designing a lunar transportation system that is useless for Mars, the quadruple plan would threaten to fatally weaken the program.

To see why this is so, consider that, depending upon the mission architecture selected, a human Mars mission will require two to four times the launch mass of a lunar mission. Therefore, a hardware set that requires four launches and four rendezvous to accomplish a mission to the Moon will need eight to 16 launches and a similar number of rendezvous operations.

Thus the quadruple plan hardware set, which provides a foundation for a lunar program that is simply unsound, becomes utterly ludicrous when extended to Mars.

Now consider our recommended alternative: a Direct Return lunar mission architecture accomplished with a single launch of an HLV.

This booster employs a hydrogen/oxygen upper stage, which delivers 38 metric tons of payload from low Earth orbit to low lunar orbit. The delta-V (change in velocity) required to accomplish this maneuver is 4.2 kilometers per second, which is exactly the same as the delta-V needed to send a payload from low Earth orbit to Mars on a six-month trajectory. Moreover, if it should be decided to abort the mission, this particular trajectory will return the payload to Earth precisely two years after the date of departure.

This is the fastest free-return option that is physically possible for any Mars mission, and is therefore to be preferred to higher energy trajectories, which in addition to imposing much greater propulsion and mass penalties on the mission, are actually less safe, as they make free-return survival nearly impossible.

Thus an HLV with an upper stage optimized for delivery of substantial payloads from low Earth orbit to low lunar orbit is in fact the best system for sending humans to Mars.

Using such a system, it is possible to undertake human Mars missions without on-orbit assembly using either a two-launch Mars Direct or three-launch Mars Semi-Direct mission plan.

For example in the Mars Direct plan, the first HLV delivers an unfueled Earth Return Vehicle to Mars, which then manufactures its return propellant by reacting a small amount of onboard hydrogen with carbon dioxide from the Martian atmosphere, which is 95 percent CO2.

After this is done, a second HLV launch sends the four-person crew to Mars in a large habitat module that is closely based upon the hab employed in the Direct Return lunar plan.

The crew lands their hab near the Earth Return Vehicle, and then for the next 18 months, conducts an intensive program of field exploration using the hab as their home and laboratory. At the end of 18 months, the launch window to Earth opens, and the crew transfers to the Earth Return Vehicle for a six-month flight home. The hab module is left behind on Mars, so that each time a mission is flown, another hab is added to the base, or alternatively, a string a mini-bases can be set up supporting field exploration on an extended geographic scale.

Alternatively, if a six-person crew is desired, the three-launch Semi-Direct plan can be employed. If necessary, mission mass margins could be greatly expanded by replacing the hydrogen-oxygen upper stage with a small (15,000 pounds thrust) expendable nuclear thermal rocket (NTR) stage.

NTR units of this size (and considerably larger) were ground tested in the United States in the 1960s, and their feasibility and performance is not in doubt.

Highly versatile, small expendable NTRs also could be used with medium-lift launch vehicles to enable potent outer solar system robotic missions with flight times to Jupiter of 2.7 years. Using such near-term technology nuclear stages, the trans-Mars throw capability of our heavy-lift booster would grow from 38 metric tons to 60 metric tons.

We do not need giant futuristic space ships to go to Mars. We can do it with multiple launches of the same system we use to go to the Moon – provided we choose our lunar mission system design correctly.

Finally, there is the issue of program phasing. If we attempt our lunar program with a Moon-only hardware set such as that employed by the quadruple launch plan, then cost considerations will require abandoning the lunar base in order to shift the resources required to create and operate new flight systems for Mars.

However, if we design a hardware set that is appropriate for both the Moon and Mars, then lunar and Mars programs can proceed in parallel. Thus, for example, if the single-launch Direct Return lunar architecture with in-situ propellant production recommended in the previous articles is implemented, only two launches per year will be required to support continuous activities that visit 12 locations per year.

This would leave plenty of launch capability free to support human missions to Mars and the near-Earth asteroids as well. In contrast the quadruple plan would require four launches to support an anemic lunar program that visits only one site per year, making scrapping the Moon base a precondition for any further exploration.

If we wish to go to the Moon to stay, we must do it with flight systems that support Mars exploration as well. Such flight systems are also best for supporting the lunar base considered in isolation.

The right way to design a Moon-to-Mars program is not to create a Moon-only transportation system, and then drop it and build something else when it is time to go to Mars. Rather, the right way is to start by designing a minimum-cost Mars mission, and then define a lunar flight system using a modular subset of the Mars mission hardware.

Done this way, only one hardware set will need to be developed instead of two, Moon missions will validate Mars hardware directly, and lunar activities can continue after Mars exploration begins. Proceeding this way, the program will save tens of billions of dollars, decades of time and dozens of lives. Going this way, we can make our gains in space permanent.

It is the intelligent way to go.

Getting Space Exploration Right

The New Atlantis,

Spring 2005

In early 2004, President George W. Bush delivered a major policy speech charting a new course for 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?

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.

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On March 11, 2005, president Bush announced that the next NASA administrator would be Dr. Mike Griffin. Virtually everyone who wanted the VSE to move forward cheered this decision. In place of the anti-intellectual uninformed, uncommitted and timid career bureaucrat Sean O'Keefe, Griffin was a bold, brilliant, hard driving PhD aerospace engineer, with a life-long passionate devotion to the space program. It is true that Griffin could be difficult to deal with, as he is famously blunt and tactless – but that is because he has no compunction against telling it as he sees it, and those who know him accept it as an expression of his frankness, rather than rudeness. Within days after taking office in April, Griffin threw O'Keefe's Lunar mission plan in the trash, and dismissed some 30 "Roadmapping" (really roadblocking) committees charted by O'Keefe to draw up lists of experiments, technology development programs, flight tests, precursor missions, and so forth supposedly necessary before we send humans to Mars. Moving quickly, he replaced Steidle as Associate Administrator for Exploration with veteran astronaut Scott ("Doc") Horowitz, an excellent choice, as Horowitz was no mere flyboy, but a PhD engineer with a long track record of commitment to the goal of humans to Mars. As an added benefit, Horowitz was also very committed to the goal of saving Hubble, and as he had flown several missions to it in the course of his astronaut career, he could speak with absolute authority as to the feasibility of such a mission.

A very hard-nosed and conservative engineer, Griffin had no time for fantastical schemes involving doing Lunar missions with 8 rendezvous per mission. Instead he chose to base his mission architecture on the way it had been done during in the 1960's, using a HLV and a single Lunar orbit rendezvous on the return leg – "Apollo on steroids, as he called it." This, in my view, is not the best plan for a Lunar base, but it clearly workable. The bigger problem, however, was that Griffin chose to regard himself not as the "leader" of the space program, but as its "administrator," charged with implementing the president's program, rather than defining it, and said so explicitly many times. Those he refrained from taking a stand against the problematical compromise at the core of the VSE – maintaining the Shuttle and ISS as the primary thrust of the human spaceflight program through 2010. As a result, the funds available to him to develop the Moon-Mars flight hardware remained quite limited.

I met with Griffin in his office in June 2005, and urged him to depart from the VSE plan as written. Instead of flying the shuttle through 2010, I said, he should use it for just one more mission, Hubble repair. Then retiring the Shuttle, he could pour its $5 billion per year into developing heavy lift and the rest of the Moon-Mars system. While this would involve deferring continued expansion of the space station for a few years, once developed, the HLV could be used to complete the ISS in just one or two flights, getting the job done faster in the end than by using the Shuttle for another 19 missions, at much lower cost and risk. More to the point, I put it to Griffin that he could only expect to remain at his post until January 20, 2009, at most, after which a new President would take power and everything on the planning charts for the future would be moot. On that day, on his way out, he needed to able to show the incoming administration that he had already created most of the hardware needed to get to the Moon by the end of its first term in 2012, and Mars by 2016, so that aborting the program would be manifest folly, or all bets would be off. The only way this could be done would be by escaping from the financial burden of the Shuttle program, and minimizing that of the Shuttle's proposed replacement, the Crew Exploration Vehicle (CEV, subsequently renamed "Orion.") Griffin responded, "I know all that. But you don't understand the constraints I'm operating under."

No doubt, I didn't. But constraints set by people can be changed by people. So I decided to make the case for such a course change public. Here are some articles I wrote towards that end shortly after my meeting with Griffin.

The Case for a Small CEV

Space News, July 4, 2005

There is a strong case to be made for downsizing the Crew Exploration Vehicle (CEV) into a much smaller, cheaper, and lighter vehicle than the Orbital Space Plane (OSP) derivatives currently under widespread discussion.

The OSP was conceived of as a means of servicing the crew rotations of the International Space Station at lower cost and lower risk than the Space Shuttle. It was thus specified that it be able to carry crew of at least five, to approach the Shuttle's crew ferrying capability. To meet this goal, vehicle masses on the order of 12 tonnes or more were considered acceptable, since the OSP was only going to orbit, and launch capabilities to deliver such mass to LEO are readily obtainable.

However, now NASA's mission has changed, and instead of perpetual flights to orbit we are reaching for the Moon and Mars, and the question must be asked whether such a large crew carrying vehicle really is optimal to support these new goals. In fact, it is not.

The simplest, safest, least expensive, and most capable Lunar base transportation system is one based upon direct launch to the Lunar surface, and direct return with no Lunar Orbit Rendezvous (LOR), using a single launch vehicle. This is so because the direct return architecture requires the least number of vehicle elements to develop, expends the fewest hardware elements per flight, has the fewest necessary operations per mission, avoids the need for untended mission critical liabilities in Lunar orbit, always has its return launch window to Earth open, and also has the lowest recurring mission launch mass once lunar oxygen production commences at the base. Doing each mission with one launch is also extremely important, because a multiple launch mission architecture not only costs more, it greatly increases mission risk. Indeed, a multi-launch Lunar mission will fail not only if any one of its several launches is lost, but also if weather or other reasons should cause any launch after the first to be delayed beyond the boiloff endurance of any of the cryogenic flight elements launched earlier.

This being the case, there is a direct relationship between the capability of the Heavy Lift Vehicle (HLV) NASA chooses for development and the allowable mass of the CEV. The fastest route to creating a HLV at this point is by reconfiguring the hardware of the Space Shuttle stack, deleting the Orbiter and replacing it with a fairing and an upper stage. A variety of such Shuttle derived HLVs are possible, with LEO delivery capabilities ranging from 70 to 130 tonnes, with the more capable versions costing more to develop.

Indications are that NASA has decided to develop such a vehicle, with the preferred variant in the mid range, offering roughly 100 tonnes to LEO lift capability. This would be a very reasonable choice.

If that is the decision made, then the math that determines acceptable CEV mass follows directly. Using a hydrogen/oxygen stage for Trans-Lunar Injection (TLI) and Lunar Orbit

Capture, and an hydrogen/oxygen propelled lander, a system that launches 100 tonnes to LEO would also be able to deliver 20 tones of payload to the Lunar surface. If direct return is to be used, this 20 tonnes must include the CEV plus its ascent stage for flight back to Earth. Using hydrogen/oxygen propulsion for the ascent stage, an 8.6 tonne CEV could be thus delivered round trip to the Moon. If instead, for superior long-term storability, methane/oxygen propulsion is chosen for ascent, then the CEV capsule would have to be limited to 7.4 tonnes.

Such lightweight CEV capsules are certainly possible. For example, the Apollo capsule, which transported three people to Lunar orbit and back, had a mass of about 6 tonnes.

Thus a lightweight, Apollo capsule derived 3-4 person CEV would allow a direct return lunar mission with a single launch, but a heavy 5-6 person OSP clone would not. If the heavy OSP clone is chosen, then development of a Lunar transportation system would require either development of a second generation super heavy lift booster, an entire lunar excursion module manned spacecraft system, or implementation of a costly, complex, and failure prone multi-launch mission architecture.

In short, developing a CEV that is too heavy for the HLV to launch to the Moon and direct return back would be a huge mistake. If the CEV matches the direct return mission capability of the HLV, then the only additional hardware elements needed to begin lunar exploration are the TLI/LOC stage and the lander. The same lander used to deliver the CEV and its ascent stage could also deliver heavy cargo such as a 20 tonne habitation module (ISS modules weigh 20 tonnes), making long duration lunar surface stays possible right from the start of the program.

But the small CEV not only cheapens and accelerates the Lunar program, it cheapens and accelerates the CEV program itself. The funds saved by reducing the size and cost of the CEV could be used to start HLV development immediately, which would save further funds, since early deployment of the HLV would allow space station construction to be completed sooner, allowing early retirement of the $4 billion per year Space Shuttle.

By reducing the size of the CEV to close derivative of the Apollo capsule, the CEV program could be turned from an extended developmental contractor banquet into a production procurement. With development minimized, NASA could compete a contract of the following form: "The winner of this contract will be paid $300 million each for five CEVs if they are delivered in 2008, plus $200 million each for five CEVs delivered in 2009, and $100 million each for five CEVs delivered per year starting in 2010 through 2015." Such a contract form would provide a strong incentive for early delivery of the CEV, thereby allowing early retirement of the Space Shuttle without any discontinuity of US human spaceflight capability. Furthermore, it would eliminate nearly all NASA expenditure on the CEV program during 2006 and 2007, allowing these funds to be reprogrammed for immediate development of the HLV. Together with other savings obtained by canceling useless programs such as the Hubble deorbit module, these funds should be sufficient to pay for the entire HLV development.

So to summarize, the choice of small CEV enables an optimum single-launch, direct-return, Lunar mission architecture. It also enables a reduced cost, accelerated commercial procurement of the CEV itself. The savings in the CEV program thus obtained can be used to launch the HLV program immediately, and together the CEV and HLV would allow early retirement of the Space Shuttle, with massive savings to the taxpayer resulting.

Furthermore, with a CEV matched to an HLV for direct lunar missions in hand, and STS retired or nearly so, outgoing NASA Administrator Griffin would be able to say to the President elect in January 2009: "We have 80% of the hardware needed for human lunar missions already developed, and have freed the funds required to develop the rest. If you choose to go forward with flat funding, we can have humans on the Moon by 2012, and Mars by 2016, by the end of your second term. The choice is yours."

It's a winning pitch.

Where is NASA Going?

Robert Zubrin

Space News, September 26, 2005

On September 19, 2005, NASA Administrator Mike Griffin revealed the agency's new plan for implementing the President's Vision for Space Exploration.

The plan has significant positive and negative features.

On the positive side, it recognizes the need for the development of a true heavy lift launch vehicle (HLV), and takes concrete steps the preserve the Shuttle industrial infrastructure necessary to produce such a vehicle by initiating development of a medium lift launch vehicle using Shuttle technology. The importance of this cannot be overemphasized. An HLV is absolutely necessary to enable human exploration of the Moon or Mars, and it was a measure of former NASA Administrator Sean O'Keefe's unfitness for his position that he was willing to promote a clearly unworkable quadruple launch/quadruple rendezvous lunar architecture for the purpose of justifying the abandonment of that capability. Dr. Griffin has reversed that position, and backed his policy with action, and that is excellent.

Another strong feature of the plan is its decision to develop and employ methane/oxygen rocket engine for Lunar ascent. Methane/oxygen is far more storable propellant combination than hydrogen/oxygen and offers much better performance than conventional hypergols, making it a much better choice than either for lunar ascent and return propulsion. More importantly, however, methane/oxygen is the easiest propellant combination to synthesize out of the Martian atmosphere, and some could be made out of lunar base waste products as well. The choice of this propellant therefore shows good system engineering sense, with thoughtful consideration of how to select Lunar mission technologies that will be most useful in enabling human exploration of Mars as well.

On the more problematic side is the decision to develop such a large Crew Exploration Vehicle (CEV). While a large CEV certainly enables larger crews and greater comfort, it will cost more to develop, produce, and launch than a smaller capsule. Furthermore, because of its excessive mass, the large CEV makes direct return lunar missions impossible, thus mandating a lunar orbit rendezvous mission architecture. This, in turn, will require the costly development and production of lunar excursion modules, and impose return rendezvous phasing complications that could hamstring the operations of a lunar base, especially if surface stays greater than two weeks are desired.

Another cause for concern is the decision to launch the CEV after the HLV that delivers the rest of the mission components to orbit. The HLV's cargo will include stages employing cryogenic liquid hydrogen/oxygen propellant, and this propellant will start to boil away immediately after launch. Thus for the mission to succeed, the CEV must be launched on time, within a few weeks at most of the prior flight, without fail. Otherwise, the billion dollar class HLV launch and cargo will have to be written off. This situation will put great pressure on managers to launch despite warnings, thereby putting crews at risk. Moreover, NASA's record of achieving on-time crewed launch to date is very poor. Unless it is radically improved, this aspect of the plan will have to be abandoned.

That said, the plan is an enormous improvement over its predecessors. One has only to compare it to the psychedelic NASA mission architecture of 2002, which called for supporting Lunar exploration from a LaGrange point space station supplied by giant cycling nuclear electric spaceships, or the nonsensical O'Keefe quadruple-launch/quadruple rendezvous lunar mission plan of 2004, is order to breathe a huge sigh of relief. The previous NASA plans were pure bullshit. This one is real engineering. Finally, we have a plan that could actually work.

There is, however, a deeper problem with the plan than the engineering concerns noted above. That is, that while preserving the HLV infrastructure, the plan relegates the development of an HLV to a subsequent administration. In consequence, for the next 13 years, NASA will continue to send crew after crew up and down to low Earth orbit, at a cost of some $70 billion, for no justifiable purpose whatsoever.

Both Admiral Gehman and Dr. Griffin have made the point that if we are to accept the costs and risks of human spaceflight, we should be undertaking missions that are worthy of those costs and risks. But for the next 13 years, we will continue not do so.

To paraphrase St. Augustine, NASA is now saying "Lord, make me a destination-driven space agency, but not yet."

In saying this, NASA is, in fact, acting in accord with the Bush "Vision for Space Exploration," as enunciated in January 2004. That policy however, was formulated by a White House which lacked a competent NASA administrator to advise it. Now, however, that we have a qualified NASA administrator, this policy needs to be revisited and reformulated.

Let us review the consequences of blindly following the mediocre "Vision" scriptural document of January 2004. That document was a compromise between those who wanted a destination driven space program, and those who did not. Therefore, in accord with the bargain reached, NASA would be allowed to continue to fly Shuttle missions for the rest of the decade, after which the destination driven program could begin.

But does this make any sense? The only really time-critical Shuttle mission is Hubble repair. This is indeed a truly important mission, and it should be flown with dispatch, as it is without question worthy of the 2% risk to crew that any Shuttle mission must entail. But the rest of the Shuttle manifest is devoted to Space Station construction, and these cargos could be delivered much more expeditiously by the HLV NASA needs to develop to reach the Moon anyway.

Griffin's HLV design will be able to deliver 125 metric tons to low Earth orbit. The Shuttle can only deliver 20 tons. With a single launch then, the HLV will be able to deliver as much payload as the Shuttle program can during a year. That's during a good year. Compared to current Shuttle launch rates, which will have managed only one flight between February 2003 and February 2006, (at a cost of $15 billion), the HLV will be able to launch in an afternoon everything the Shuttle program would be able to launch for the next 18 years.

Operating the Shuttle program for the next five or six years to deliver a few space station payloads early will cost us $30 billion. All that money could be saved simply by shutting the Shuttle down after Hubble repair, and shifting the Shuttle program funds over to immediate development of the HLV and the other Lunar exploration hardware elements. We could then use the HLV to complete the space station and reach the Moon by 2012 instead of 2018.

In the wake of Hurricane Katrina and the financial burdens it will impose on the nation, gratuitously wasting $30 billion of the taxpayers' money in order to dogmatically fulfill an old scriptural document is unacceptable. The new NASA architecture is a good plan for implementing a flawed policy. We need a good policy. We have real talent at NASA now, and we should make use of it to revise the policy itself.

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By 2006, another problem had emerged. In the course of implementing the Vision for Space Exploration, NASA had lost sight of the vision. What had once been a Moon-preparing-the way-to-Mars program, was devolving into a Moon-only program. This became increasingly evident as previous implied requirements that systems chosen for development for use in the Moon effort be relevant to Mars exploration were dropped or ignored. In this article I tried to fight the trend.

The Vision at Risk

Space News, March 27, 2006

NASA's recent announcement that methane-oxygen propulsion would no longer be a requirement for the Crew Exploration Vehicle (CEV) has created great concern in the space community that the agency's commitment to the human exploration of Mars might be waning.

Because methane-oxygen can be readily manufactured on the surface of Mars out of local materials, it is the ideal propellant combination for Mars ascent propulsion. Its earlier prescribed development as part of the CEV program was therefore widely seen as evidence that the CEV was being pursued not merely as a thing in itself, but as part of a broader vision that would take America all the way to Mars. Its abandonment has therefore been interpreted as indicating the collapse of that vision.

In some respects, this dark view is overdrawn. NASA's exploration office remains committed to the development of a heavy-lift launch vehicle, which is the primary hardware element needed for a human Mars mission, and as far as methane-oxygen propulsion is concerned, two contracts were recently awarded by NASA supporting its development outside of the CEV program, and should that technology be employed for lunar ascent but not CEV, that would still be timely enough to prepare it for Mars application.

Yet it must be said that the dropping of methane-oxygen from CEV, while not a fatal blow in itself, illustrates a dangerous trend that could well destroy the human exploration program. It is always easier to conduct any technology development program with a view towards meeting only immediate mission requirements, while ignoring those needed for evolutionary application.

This, in fact, is why methane-oxygen was dropped from CEV. Methane-oxygen offers superior performance to conventional storables on CEV itself, and becomes increasingly advantageous as applications for first lunar ascent and then Mars ascent are brought into play. However in order to reduce immediate costs, its development has been deferred.

Now let us consider the lunar program that is supposed to follow CEV. It will, perforce, be cheaper in the short term to design human lunar exploration systems without regard for potential application to Mars. Thus a NASA adopting the view that it is best to solve one problem at a time will be driven in precisely that direction. The net result will be a Moon program that is just a Moon program, and not, as President George W. Bush specified in his national security document authorizing the Vision for Space Exploration, a Moon-Mars program in which lunar activities are conducted in order to "develop and test new approaches, technologies and systems to support sustained human exploration to Mars and other destinations.”

The consequences of allowing the vision to be degraded in this way would be grave. This is made nowhere more clear than in an op-ed article by Paul Spudis, senior research scientist at Johns Hopkins University's Applied Physics Lab, that was published in the Dec. 27 edition of the Washington Post advocating precisely such a course. A noted lunar geologist and space policy insider, Spudis' article is of great clinical interest because he is the most eloquent and informed advocate of a Moon-only vision for NASA. He argues that such a program could be justified on three grounds:

* First, that studying lunar cratering will allow us to understand the processes of mass extinctions on Earth;

* Second, that Lunar activities will provide us with practice for exploration of "other worlds;" and:

* Third, the Moon base will provide an economic return by enabling the development of Lunar solar power stations that will beam electricity back to Earth.

However, these programmatic foundations have no basis. Argument one is false because the Moon's lower gravity gives it a lower impact rate than the Earth, and its lack of an atmosphere or biosphere makes impossible any studies of the relevant post-impact terrestrial phenomenon that cause and shape mass extinction. Argument two is false because while we can practice for operating on other worlds on the Moon, we can do much more in that line at 1/1000 the cost in the Arctic.

Argument three is false because a photovoltaic panel only receives twice the solar flux on the Moon as it does in Arizona, and all of its increased output would be lost in the inefficiencies of the transmission system. Thus the useful output of a photovoltaic power station on the Moon would only be equal to one on Earth, while logistics costs to support it would be 100,000 times as great.

Furthermore, the station would be blacked out two weeks at a time, and require three receiving rectenna and power distribution systems on Earth as well, each of which would be blacked out two-thirds of the day during the half of the month that the station produced any power at all.

In short, the programmatic justifications offered by the ablest advocate of a Moon-only vision have no valid basis at all. Under favorable political conditions, NASA might get by for a while by having its supporters chant such nonsense to entertain Congress, in the same manner as it used similar unsound "rationales" to justify the shuttle and space station programs. However, at the end of the day little of real value will have been accomplished at great expense.

The shuttle and station programs initially were proposed as bridges to an expansive evolutionary future. Yet because of design compromises to save costs on the programs themselves, without regard to how they would really serve a useful role supporting human exploration beyond low Earth orbit, neither have any such utility, and NASA's primary current concern with these programs is how to escape from them so it can get on with its mission.

Again, it was precisely because the design of the shuttle and station had been effectively detached from the need to play a useful role in the achievement of worthwhile goals beyond themselves, that NASA felt the need to grossly exaggerate their potential return as stand-alones. That pathology threatens to repeat itself.

We need to do better. Instead of organizing NASA's activities around projects conceived largely to give the agency and its contractors something to do, and then justifying those programs with whatever excuses someone can dream up, NASA needs to set a rational objective for its human spaceflight program and devote its efforts and expenditures towards that end.

That goal can only be humans to Mars.

In contrast to a Lunar return program supported by promises of electricity from Moon beams, human Mars exploration has a real rational purpose: the search to determine whether life is a general phenomenon in the universe and whether life as we know it on Earth is the pattern for all life everywhere, or whether we are just a particular example drawn from a much more diverse tapestry.

This is true, fundamental, science of a sort that bears on questions that thinking men and women have debated passionately for millennia. It is a goal that can be truthfully and forcefully defended as worth risking life and treasure for. It is a search that can only be accomplished by human explorers on Mars, because of the complexity of operations required to find, culture and characterize Martian life are far beyond the capacities of robotic devices.

Furthermore, since, unlike the carbon, nitrogen and water impoverished Moon, Mars possesses all the resources needed to support life and human settlement, if the objective of our space program is to extend human civilization into space, our goal needs to be to send humans to Mars. There is really no way around this.

There are legitimate reasons to send astronauts to the Moon, but just as was the case for the space station, these are in fact of insufficient worth to justify the huge cost and multidecade delay in the achievement of more important objectives that a stand-alone program must entail.

Therefore, since lunar activities can most rationally be supported as intermediate milestones in an effort to get humans to Mars, it should be clear that their hardware design requirements should be driven by the real goal. If we fail to take that approach, we will spend further tens of billions of dollars developing hardware, as exemplified by the shuttle and space station, that serves little useful purpose towards getting us where we want to go, and which will have to be set aside in order to accomplish anything real.

If we launch a lunar program with a hardware set designed for a Moon-only effort, the hardware will prove useless for Mars, and we will have to abandon the Moon while we spend many billions more and waste further decades to develop a second hardware set that can take us to the red planet. But if we intelligently design our hardware set for Mars, we can use a subset of that to reach the Moon.

By adopting such a rational approach, based upon real goals courageously embraced, NASA will be able be to achieve truly valuable accomplishments with its manned spaceflight program, and do it at much lower cost, risk and time than would be possible otherwise.

It will cut cost because only one hardware set will need to be developed instead of two. It will cut schedule, radically, for the same reason. It will reduce risk because the lunar missions will be used to exercise the Mars flight hardware directly. It also will strengthen the rationale for the lunar program itself, because in this case it would really pave the way to Mars, and because, with a common hardware approach, the Moon would not have to be abandoned for the Mars program to begin.

However, if instead the agency allows itself to devolve into an irrational Moon-in-itself project, then it will end up repeating the wasteful folly of shuttle and the international space station, and create yet another tollbooth blocking America's progress in space. Upon that choice hangs the fate of the vision.

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At the end of October 2006, Mike Griffin announced his decision; the Hubble repair mission would proceed. Space News asked me to write an article discussing the significance of this victory. Here it is.

Hubble Decision a Victory for Reason

Space News, November 2006

NASA Administrator Mike Griffin announced Oct. 31 that NASA would mount a Shuttle mission to save and upgrade the Hubble Space Telescope, thereby ending a dark episode in the history of the American space agency.

The crisis had begun on January 16, 2004, when then-NASA Administrator Sean O'Keefe, just two days after President George W. Bush's declaration of the new Vision for Space Exploration chose to announce his decision to desert the Hubble Space Telescope.

According to Mr. O'Keefe, Hubble needed to be abandoned because – despite four successful previous shuttle missions to the Hubble – NASA now realized that after the Columbia disaster that missions to Hubble were too dangerous to risk. Instead, the space agency would limit its future shuttle flight plans to a supposedly much safer program of 29 missions to the International Space Station (ISS).

O'Keefe's statement was categorically absurd. While ISS missions which manage to reach their target have a potential safe haven at the space station, which Hubble missions lack, Hubble flights have much better abort options than those to the ISS. When they depart the Cape Canaveral Fla., Hubble missions fly east-southeast, and thus have the possibility to ditch in warm tropical waters. In contrast, ISS flights leave the Cape traveling northeast, and their crews face the bleak prospect of aborts into the frigid waters of the North Atlantic, where their chances for survival would be much less.

Furthermore, Hubble missions, because they take off more lightly laden than ISS flights, can abort to orbit with engine out much earlier. For example, in order to be able to abort to orbit on an ISS mission such as STS-113 (Endeavor), all three shuttle main engines must fire for a full 282 seconds before one cuts out. In contrast, on Hubble missions such as STS-103 (Discovery), only 188 seconds of full three-engine operation is required. This lower full-power time requirement for Hubble missions is a critical safety advantage, because the maximum time that either ISS or Hubble missions can attempt a Return to Launch Site (RTLS) abort is about 232 seconds. Thus Hubble missions have a 50 second overlap during which either a RTLS or orbital abort is possible, whereas ISS missions have a 50 s gap in which neither is possible.

Finally, the Hubble orbit has a much lower micrometeorite and orbital debris hazard than that of the ISS. So, in short, there is no reason to believe that a Hubble mission is more dangerous than one flight to the ISS, let alone 29. Yet Mr. O'Keefe chose to blandly ignore the data and proceed to abandon Hubble – the most productive scientific instrument in human history, and by far the most important accomplishment of NASA's manned spaceflight program since Apollo.

But faced with powerful political opposition from Senators whose opinions he could not dictate, Mr. O'Keefe attempted a diversionary tactic by ordering a study of a the feasibility of a robotic rescue mission using unproven – indeed nonexistent – technology as an alternative. The acceptance of this nonsense would have represented a complete abandonment of NASA's engineering discipline, which requires that mission-critical technology be mature before it is used.

This is the reason why, notwithstanding the VSE announcement which we strongly favored, the Mars Society responded to Mr. O'Keefe's decision to desert Hubble with an immediate denunciation and a forceful and sustained counter-campaign. It does not matter what the space agency's nominal goals are if it does not have integrity. It is not just that a NASA too timid to return to Hubble would never be able to reach for the Moon or Mars – although that is certainly true. Rather, it is the case that it is impossible for any engineering organization to operate competently for any purpose if it becomes accepted practice that management has the right to suppress facts, order concurrence, and deny technical reality on the basis of political convenience or arbitrary whim. Engineering needs to be done on the basis of truth.

So now the decision has been made to save the space telescope. It's the right decision, even though the Hubble flight will be risky. Yes, risky – no more so than an ISS mission, but risky nevertheless. It could fail. Despite all the hard work done by the shuttle team since the *Columbia* accident to improve safety, the best bet is that the risk of loss of future Shuttle missions is about the same as past ones – 2 percent. The Shuttle is a very complex system, with thousands of potential failure modes; we've eliminated two.

Yet it is the right decision, because it averts a historic crime against science. It is the right decision, because it represents the victory of reason. It is the right decision because it reasserts NASA's commitment to its mission. It is the right decision, because it saves the honor, and the soul, of NASA.

Note: Griffin's decision held, although it was not realized until a few months after he left office.. On May 11th, 2009 the Space Shuttle Atlantis took off and performed a flawless mission to repair and upgrade Hubble. Here's the photo I took of the launch. It was a sight to see.

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The robotic Mars exploration program had performed admirably from 1996 through 2007 by following Clinton-era NASA administrator Dan Goldin's strategy of using many small and relatively cheap spacecraft, launched frequently (one or two every two years), rather than a few large and very expensive launched at a rate of one or two per decade. Sean O'Keefe had begun to diverge from this mode by initiating the Mars Science Lab (later renamed *Curiosity*), which was significantly larger and more complex that its predecessors *Spirit* and *Opportunity*. This was not necessarily a bad decision, since despite its larger size and cost, the *Curiosity* program did not require so much money as to preclude carrying out the MER, MRO, and *Phoenix* missions, all of which were done during Curiosity's development period. In 2007 however, Associate Administrator for Space Science Alan Stern began to push for a Mars Sample Return (MSR) mission, which would be several times more expensive than *Curiosity*. He proposed to pay for this by eliminating all other robotic Mars missions until 2020, when the MSR mission would presumably fly ("I'd be happy to give you a hamburger in 2020 for a hamburger today."). I thought this was a very bad idea, and resolved to campaign against it. Here is what I wrote.

Don't Wreck the Mars Program

Space News, Aug 1, 2007

Reports are circulating that NASA Associate Administrator for Space Science Alan Stern is currently considering a plan to reorganize the robotic Mars exploration program, with the said reorganization consisting of canceling the entire existing robotic Mars exploration program after the 2009 Mar Science Lab flight, including the Scout program, and replacing the lot with a Mars Sample Return mission scheduled for 2020. Such a reorganization would be a very bad idea.

Since its origin a decade ago, the existing fly-every-opportunity robotic Mars program has proven to be a brilliant success, producing an unparalleled and ever increasing science return, putting a robust communications infrastructure in place at Mars, and creating an ever more proficient team competent to carry out ever more complex Mars missions. While many of former Administrator Dan Goldin's initiatives may have been questionable, there is no doubt that the concept of a sustained exploration program involving frequent launches of medium sized spacecraft has proven to be far superior to the previous mode of attempting to explore a planet with one grand spacecraft every decade or so. The reorganization plan would thus abandon success – indeed, it would abandon the greatest success that NASA has to show for its efforts for at least the past decade.

Furthermore, in its stated objectives, the reorganization plan is much less supportive of the VSE goal of enabling human Mars exploration than a program of the existing type. While, strictly speaking, no robotic precursor is *required* to enable human Mars exploration (as we now know considerably more about Mars than we knew about the Moon at the time of the Apollo landing), the question remains: How can our robotic exploration capabilities be best used in advance of human exploration to enhance the capabilities of those missions? The answer to this is to perform a sustained reconnaissance to identify the sites richest in:

(a) science, and

(b) resources

for subsequent direct human investigation and exploitation, respectively.

Mars is important for humanity's quest for truth because it is a critical testbed for the hypothesis that life originates from chemistry with high probability wherever appropriate physical and chemical conditions pertain for a sufficiently long period of time. However, we now know that Mars had standing bodies of water on it at a time when there was already plentiful microbial life on Earth, and that there is and has always been natural transfer of unsterilized (portions of AH84001 never exceeded 40 C during the rock's entire travel career) material between the planets. Therefore discovery of microfossils on the planet's surface, while very interesting, would not in itself constitute proof of a second origin for life, since the lifeforms in question could well have come from Earth. Rather, to settle the question, we need extant organisms, whose biochemistry can be examined. These, if they exist, can best be found in groundwater. Thus the most important goal of the robotic program – if it is to be used to enable human explorers to achieve fundamental scientific discoveries of world-historic importance, - is to identify sites where bodies of liquid water can be found within practical drilling distance of the surface. This can best be done not with an MSR mission, but with a comprehensive scouting program involving orbiters, rovers, drillers, and possibly aircraft or balloons carrying ground penetrating radar.

If we consider the question of prospecting for resources for supporting a human Mars base, once again, this can be done best not by a couple of MSR missions that samples a site or two, but by a comprehensive reconnaissance program of the type described above. Such a program might identify not only sources of accessible water, but also mineral ore. Mars has had a sufficiently complex geologic history to make the discovery of mineral ores of comparable quality to terrestrial commercial varieties a real possibility. While regolith that contains 15% iron oxide can be found anywhere on Mars, those who wish to make useable iron or steel would be much better off if we can locate deposits of material that is 90% (Duluth-quality) or better. Similarly, while regolith that is ~40% silicon dioxide can be found anywhere on Mars (as it can on the Moon), if you want to make glass, let alone solar panels, you are far better off if you can find silicon dioxide deposits or purity comparable to terrestrial quartz or sand. If our goal really is to extend human civilization to other worlds, these and other useful minerals can and should be prospected for by the robotic program in advance of human Mars missions, so that our base site can be chosen appropriately.

Furthermore, even if one concedes considerable importance to the MSR mission, it is doubtful whether the programmatic path being considered in the reorganization plan is the right way to get there. If we do as it recommends and ground the Mars program for a decade, all the best people will leave the team, to be replaced by those who enjoy drawing charts and schedules. In addition, the MSR mission will be poorly prepared for technically, scientifically, and politically.

It will be poorly prepared for technically, because instead of a live flight program which proves out key components (for example larger aerobrakes for larger landing systems) and generates valuable experience over the decade preceding flight, the entire flight system will have to be designed on the basis of analysis by a team composed of a mixture of green and rusty personnel. It will be poorly prepared scientifically, because an ongoing program of increasingly potent orbital and rover reconnaissance missions that could have contributed much to identifying the best site for sampling would not have occurred. It will be poorly prepared politically, because instead of being integrated into an ongoing, funded, and demonstrably competent robotic Mars exploration program, those who ask for funds to initiate Stern’s proposed MSR mission program circa 2013 (assuming that they actually do so) will be starting from square zero. Furthermore, they will have to continue getting funding for their project across the span of a decade during which no new discoveries on Mars are being made to maintain interest in the program.

Let us consider an alternative scenario. Rather than wrecking the current Mars program and hoping for the best, let us build on it. Let's fly the orbiter in 2011, and then the Astrobiology Field Lab in 2014. Then perhaps in 2016 fly something like an Advanced Science Orbiter, with instrumentation, data, handling and com capabilities exceeding those of MRO by all that a decade's worth of technical progress can supply. With the discoveries of the AFL and the ASO building justified excitement, NASA will be in a position – indeed it will have earned the right – to ask for a plus-up in funding to increase the programmatic level of effort so as to add an MSR mission into the queue. Then, while this is getting ready, rovers and drillers are sent to the most promising sites identified by the AFL and ASO to confirm their suspicions and gather and cache samples whose return will really matter. Then, with a well-practiced team using well-practiced equipment running the show, the MSR mission is flown and returns with samples providing ground truth to the data indicating the past or present presence of life on Mars. In addition to a truly worthwhile science return, such a mission would provide enormous and well-justified excitement of a sort that might well give NASA the boost it needs to actually get a humans to Mars program off the ground.

There are those who are envious of the funding of the Mars exploration program that would prefer to take it for their own purposes. One would therefore be naïve to believe that a plan to abandon the existing Mars program has as its true purpose the goal of enabling a future MSR mission in 2020, rather than that of funding some alternative project outside of the Mars program during our own time. Yet, one must ask, is this what is best for the nation? Is wrecking NASA's finest program to gratify the esoteric research interests of some jealous people with other agendas really in the best interests of the space agency? Is this the way to support the vision of human exploration of other worlds? If our goal is to put our stamp upon the future by expanding civilization into space, is this really the way we should proceed?

The robotic Mars exploration program has proven itself to be a jewel not only in the crown of NASA, but of America; indeed it represents one of the great cultural accomplishments of contemporary human civilization. It should not be discarded lightly. Rather than breaking from it, we should build on it. That is the way to Mars.

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In the fall of 2008, it became apparent that the *Curiosity* mission would significantly overrun its budget. With $1.8 billion already spent, the program managers asked for an additional $400 million to complete the development of the spacecraft. In response, Associate Administrator Stern said he preferred to cancel the mission. He then publicly justified this decision in a New York Times op ed, claiming that "NASA's managers and masters must all make cost performance just as important as mission successes." Here is my reply.

To the Stars! (But Stay on Budget?)

Letter to the New York Times

To the Editor:

Alan Stern's argument that "NASA's managers and masters must all make cost performance just as important as mission successes" is absurd. Successful missions with cost overruns can still have great value. Failed missions performed within their budget have none.

The space program's greatest accomplishments, including Apollo, Viking, Voyager, Hubble and Cassini, ended up costing more than NASA thought they would going in. So did the Panama Canal, the transcontinental railroad and almost everything else this country has ever done that was hard to do.

Encountering unanticipated difficulties while developing a new kind of space probe is not a reason to quit. If we had taken that attitude as our guideline in the past, this nation would have never accomplished anything.

If we are going to continue to be a nation of explorers, we need to do what it takes to make our missions succeed.

Robert Zubrin

Lakewood, Colo., Nov. 25, 2008

Note: In December 2008, NASA Administrator Mike Griffin overruled Stern, who then resigned. The Curiosity mission work continued, and the spacecraft is currently scheduled for launch in between Nov 25 and December 18 2011.

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As predicted, Griffin left NASA in January 2009, leaving the agency essentially leaderless. Not to worry, direction was soon provided from another source.

On May 7, 2009, Dr. John Holdren, President Obama’s Science advisor, initiated a review of NASA’s human spaceflight program. Operating through a group led by former

Lockheed Martin Chairman Norm Augustine and reporting to Holdren, the Committee on Human Spaceflight then held a series of public hearings for the alleged purpose of gaining knowledge to inform its recommendations. I was invited to testify, and did so, at the hearings held on Washington DC August 17. While I was grateful to be included in the discussion, it was apparent that me that the Committee members were not really interested in anything that the witnesses had to say, but rather were going through the motions of open public discourse in order to provide legitimacy to a policy that had already been decided upon.

The nature of that policy was made clear later that month, when the Augustine Committee started leaking what its recommendations would be. The result: Cancel the Bush push to the Moon, but put nothing comparable in its place. Instead, for the next ten years, at least, NASA's human spaceflight program would revert to the destination-free (and accomplishment-free) mode of operation previously championed by Sean O'Keefe. According to the Augustine committee, such a shift was required because the Bush administration's goal of sending humans to the Moon by 2020 was impossible. To back up this claim, they provided wildly inflated cost estimates for development of the hardware needed for the lunar effort – such as $36 billion to develop an HLV, in spite of the fact that SpaceX chairman Elon Musk had testified to them that he would do it for $2.5 billion, and Augustine's own Lockheed Martin company's cost estimates for the job was $4 billion. Thus, for the foreseeable future, NASA would have to devote itself, not to reaching any goal, but to develop supposedly key technologies that might enable the agency to go somewhere someday.

This "flexible path" without goals of schedules was unacceptable. Here is my reply

Augustine's Pathway to Nowhere

Space News, August 24, 2009

According to the Committee on Human Spaceflight headed by former Lockheed Martin Chief Executive Norman Augustine, it would involve too much risk for NASA to aim to reach Mars by the end of the next decade. Indeed, according to Mr. Augustine and his fellow worthies, it is beyond the capability of the United States to return astronauts to the Moon half a century after they first went there.

A more responsible option, say the eminences of the committee, would be to continue to fly the space shuttle until 2015, and then initiate a program to deorbit the international space station by 2020. In other words, rather than embracing the risk of attempting Mars, the Augustine Committee believes that it is fully responsible for NASA's human spaceflight program to plan to spend $100 billion of the taxpayers' money over the next 11 years in order to accomplish absolutely nothing.

Of course, if we fly the aging shuttle fleet (with its demonstrated 2 percent risk of loss per flight) till 2015, there is plenty of chance we will lose a vehicle and crew. However, this risk could be mitigated by reducing the launch rate to one per year, or perhaps by just funding the STS program, without actually flying any shuttles at all.

But wait, there's more. The Augustine panel believes that NASA's human spaceflight program should have its funding increased. Given what they might have said in this regard, that's a relief. But in conjunction with this recommendation, I am reminded of an employee I had in my company a few years ago. He was doing seriously substandard work, and I told him so. He replied that the reason why his work was so poor was because I paid him too little; if I wanted to get him to do a good job, I would have to give him a raise. I leave it to the reader to guess what happened next.

Interviewed on PBS TV Aug. 14 about his committee's conclusions, Mr. Augustine said the reason why NASA cannot be expected to achieve comparable feats to those of the Apollo era, is because at that time, the space agency commanded 4 percent of the federal budget, while today it gets less than 1 percent. These figures seem compelling but are actually misleading.

Put in today's dollars, NASA received a total of $230 billion between 1961 and 1973, for an average of $18 billion per year. NASA's budget this year is $18 billion, and the political establishment seems willing to provide about this level of support for the foreseeable future. Between 1961 and 1973, with this same funding rate, NASA built and flew the Mercury, Gemini, Apollo, Skylab, Ranger, Surveyor, Mariner and Pioneer programs, and performed all the development needed for the Viking and Voyager missions as well.

In addition, over the same span, the agency developed hydrogen/oxygen rocket engines, multistage heavy-lift launch vehicles, in-space life support, spacesuits, deep space navigation and communication, nuclear rocket engines, space nuclear reactors and radioisotope thermoelectric generators, lunar rovers, soft planetary landing techniques, re-entry technology, orbital rendezvous technology — indeed the entire bag of tricks we have used ever since to do everything we do in space — and built the Cape Canaveral launch complex, the Deep Space tracking network, the Jet Propulsion Laboratory, the Johnson Space Center, and all of the rest of the ground infrastructure supporting the space program as well.

And again, all of this was done on the same average budget as NASA has today. NASA's current budget is a smaller percent of the total federal budget than it was in the 1960s because the country was much poorer then, and had less money to spend overall. But the relative poverty of the nation in the past was hardly an advantage for the Apollo era. No, what the space program had then which it lacks today is not money, but bold, competent and responsible leadership. Unfortunately, it was this that the Augustine Committee proved unwilling or unable to provide.

That America's space leaders of the 1960s were bolder and more competent than those since is beyond dispute; but, with all the daring risks they took, were they truly more responsible? Yes, they were. They were more responsible because they understood that from those to whom much is given, much is expected. Reasonable people may differ on whether it is more worthwhile to spend $100 billion to open new worlds to humanity in space or to meet human needs on Earth. But it is hardly tenable to defend $100 billion in zero-accomplishment space agency expenditures in the face of social needs elsewhere.

To take one example, every $3 million spent on highway repairs in this country, will, on average, save one life. NASA's yearly $18 billion budget therefore, comes at a cost of 6,000 lives. In the face of this harsh reality, is it therefore responsible to recommend, as the Augustine Committee does, spending a billion dollars or so (330 highway deaths) to deorbit a space station built at the cost of over $60 billion (20,000 highway deaths) to avert a 0.1 percent chance of someone being harmed should the ISS, abandoned by NASA, eventually re-enter in its own good time?

Or more to the point, is it responsible to waste hundreds of billions of dollars avoiding taking on the challenge of Mars for decades, in order to marginally reduce the risk exposure few volunteers when, or if, the mission is ever flown?

To those to whom much is given, much is expected. The Augustine Committee received plenty of testimony making it crystal clear that Mars — not low Earth orbit, not the Moon, not the Lagrange points, not the near Earth asteroids, but the Martian surface — is the place in space where human explorers are of truly critical value.

So Mars is the mission, and, as they say in the Army, the mission needs to come first. The Augustine Committee wants more money for NASA, but refuses to propose the mission that would make more money worthwhile. Instead they prefer to accept a course that can only result in further decades of stagnation, endured at tremendous cost, accomplishing nothing, until perhaps the patience of the American taxpayer is exhausted and our human spaceflight program is consigned to oblivion.

This is indeed tragic. One can only pray that an administration elected on the promise of hope, and change, and the fierce urgency of now rejects such spiritless advice, and elects instead to provide the decisive leadership necessary to give the American people what they want and truly deserve, which is a space program that is actually going somewhere.

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Worse however, was to come. Supposedly acting on the basis of the recommendation of the blue-ribbon Augustine Committee (which they had created, and whose conclusions Holdren had almost certainly dictated in advance - you will note from the above article, by the way, that Augustine announced the Committee's conclusions on August 14, three days *before* its Washington DC public hearings) the Obama administration presented its new space policy to Congress the following February. Sure enough, all the bad ideas of the Augustine Committee, were there, and more, in that the specific list of putative "gamed changing" technologies that it claimed had to be developed before we could reach for Mars had no relationship to any real exploration program requirements, and some of them verged on outright fantasy. The following articles lay out what I had to say about it.

NASA Needs a Destination

Space News, February 22, 2010

On Feb. 2, the administration of U.S. President Barack Obama announced a new space policy incorporating three key decisions:

* NASA's subsidization of the development of private launch systems for delivering astronauts to the international space station.

* Cancellation of the Constellation program devoted to developing a hardware set for enabling piloted missions to the Moon.

* Abandonment of the concept of setting a specific mission goal for NASA's human spaceflight program in favor of an approach based on funding technology research for the purported purpose of better enabling some mission that might be selected later.

The first of these is a positive decision that is long overdue. The second is harmful, but could be made good if something better were proposed in Constellation's place. The third, however, is a horrible mistake that, if accepted, would guarantee zero accomplishment for the U.S. human spaceflight program for the foreseeable future.

Over the course of its history, NASA has employed two distinct modes of operation. The first, which has always been the method of the robotic exploration effort, but which prevailed in the human spaceflight program only during the period from 1961 to 1973, may therefore be called the Apollo Mode. The second, prevailing within the human spaceflight effort since 1974, may be called the Shuttle Mode.

In the Apollo Mode, business is conducted as follows: First, a mission goal is chosen.

Next, a plan is developed to achieve this objective. Following this, hardware designs are developed to implement that plan, and if necessary, technologies are created to enable such hardware. The hardware set is then built, after which the mission is 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 internal and external technical community pressure groups 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.

Imagine two couples, each planning to build their own house. The first couple decide what kind of house they want, hire an architect to design it in detail, then acquire the appropriative materials to build it. That is the Apollo Mode. The second couple poll their neighbors each month for different spare house parts the neighbors would like to sell, and buy them all, hoping to eventually 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 adequate 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 $18 billion per year. That is about the same as 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 from 1961 to 1973 and from 1998 to 2010, as the space agency's total expenditures over these two periods were nearly equal.

Contrasting the brilliant record of achievement of NASA's human spaceflight program during the Apollo period with that of the past decade speaks for itself. In technology development, the Apollo-era NASA was also far superior, creating hydrogen oxygen rocket engines, multistage 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 contrast, during the past 13 years, no new technologies of major significance were developed.

The only area in which the achievements of the current NASA compare with those of its Apollo period is robotic planetary exploration. But this is precisely because the Jet Propulsion Laboratory-led robotic planetary exploration effort continues to use an Apollo-style mission-driven approach. In the JPL program, missions are selected based on rational, science-driven criteria, designs are then drawn up, and if necessary, technology work is undertaken to help implement those designs. If JPL instead chose to spend most of its funds developing random technologies, and then designed its missions around the purpose of employing such toys for their own sake, its productivity would fall to nil as well.

Consider the following: At the same time it announced its new space policy, NASA gave notice that the three key supposedly "game-changing" inventions it would seek to develop as part of the effort would be the Variable Specific Impulse Magnetoplasma Rocket (VaSIMR) propulsion drive, orbital space depots, and heavy-lift technology.

But the VaSIMR thruster, while energetically advocated by its inventor, offers no clear mission benefits over existing ion drive electric propulsion systems, and both remain useless as tools for supporting human exploration missions in any role without the development of multi-megawatt space nuclear reactors to power them, which is not part of the program. Furthermore, even if such huge space nuclear power systems were created, the claim that VaSIMR (or any other electric thruster) would then enable transit to Mars with much shorter flight times than existing chemical propulsion systems, or even equal flight times to those available from existing rockets, simply has no basis in technical reality. So stalling a Mars program while waiting for such magic-based capabilities to materialize is just a prescription for having the human spaceflight program continue to mark time.

As for the orbital propellant depot, this was a favorite hobbyhorse of one of the members of the Augustine committee responsible for recommending the new policy. Its potential utility, however, as a way to enable human Moon, near-Earth asteroid, or Mars missions has never been established. To the contrary, none of NASA's recent designs for Moon or Mars missions has involved refueling spacecraft from orbital propellant stations. To insist that mission architects adopt such a strategy because "this is the technology we are working on" is to force the program to accept a suboptimal system design based on an arbitrary decision to favor one technology.

Finally, it is simply not the case that we need new technologies to create heavy-lift launch systems — we flew our first heavy-lifter, the Saturn 5, in 1967. What is needed to give us a functioning heavy-lift booster is a decision to build it, which will never come until there is mission to employ it.

Thus, without the guidance supplied by a driving mission, under the new Obama space policy, another 10 years and more than a hundred billion dollars will be spent by NASA's human spaceflight program without achieving anything significant. We may take part in another 20 flights to low Earth orbit, but there is no new world there to explore. Together with the Russians, we have already flown there some 300 times over the past half-century. Spending a king's ransom to raise that total to 320 hardly seems worthwhile. Under the Obama plan, we may develop some new technologies, but without a mission plan to guide their selection, they won't be the right technologies, they won't be realized as actual flight systems, they won't fit together, and they won't take us anywhere.

The American people want and deserve a human spaceflight program that really is going somewhere, and not just anywhere, but to a destination that is really worth going to. That destination is Mars. For the past four decades since the end of Apollo, Mars is the challenge that has stared the American space program in the face. A world with varied resources and a past history that includes oceans of liquid water, Mars is the Rosetta stone that will tell whether the development of life from chemistry is a general phenomenon in the universe, and whether life as we know it on Earth is the pattern for all life everywhere, or alternatively that we are simply one esoteric example of a far vaster and more interesting tapestry of possibilities. Moreover, Mars is the closest world that truly has the resources needed for human settlement. For our generation and those that will follow, Mars is the New World. We should not shun its challenge.

But regardless of what destination we choose, what is essential is that there be a destination, which defines a mission plan, which defines a hardware set, which then defines what technologies should be developed and what hardware elements will be procured. If matters are approached this way, there are many methods of procurement of flight systems that can be used, including conventional and entrepreneurial approaches, but they need to be employed coherently to achieve a defined objective.

If this is not done, then 10 years from now, after spending another $100 billion on human spaceflight, we will be no closer to sending astronauts to the Moon or Mars than we are today.

The Obama administration claims that its new space policy enables a “flexible path.” In reality, it is a prescription for yet another wasteful random walk. Four decades of stagnation in space is enough. If any progress is to be made, a course must be set. Leadership is required.

In the beginning, there was the Word.

Obama’s Fake Space Program

NY Daily News , April 16, 2011

In a speech to political allies gathered at Cape Canaveral on April 15, President Obama laid out his vision for America’s space program. Under the Obama plan, NASA will spend $100 billion on human spaceflight over the next ten years in order to accomplish nothing.

Of course, that’s not how Mr. Obama phrased it. But beneath the President’s flowery rhetoric, that’s how things add up.

Here’s the background. In 2004, the Bush administration launched a program called Constellation to develop a set of flight systems, including the Orion crew capsule and the Ares 1 and Ares 5 medium and heavy lift boosters, that together would allow astronauts to return to the Moon by 2020, and subsequently fly to destinations beyond. Under the plan announced by the president, almost all of this will be cancelled. The only thing preserved out of the past 6 years and $9 billion worth of effort will be a version of the Orion capsule – but one so purposely stripped down that it will only be useful as a lifeboat for bringing astronauts down from the space station, not as a craft capable of providing a ride up to orbit. With the Space Shuttle program set to end in the near future, what this means is that the only way Americans will be able to even reach low Earth orbit will be as passengers on Russian launchers, with tickets priced at the Kremlin's discretion.

In other words, instead of NASA developing spacecraft for flying astronauts from the Earth to the Moon, the agency's human spaceflight program will be operated as a vehicle for transporting cash from Washington to Moscow.

In his speech, however, the President choose to represent the abandonment of the Moon program not as a retreat, but as a daring advance. We've been to the Moon before, he declared, and so we have. There's a lot more of space to explore; we should set our sights on points beyond, to the near Earth asteroids, and reach for Mars. Indeed, we can and should. But the President's plan makes no provision for actually doing so. Instead, he proposes to simply stall.

So, for example, as the first milestone in his allegedly daring program of exploration, Obama called for sending a crew to a near Earth asteroid by 2025. Such a flight is certainly achievable. To do an asteroid mission, all that is required is a launch vehicle such as the Ares 5, a crew capsule (such as the Orion), and a habitation module similar to that employed on the space station. Had Obama not cancelled the Ares 5, we could have used it to perform an asteroid mission by 2016 – i.e. within Obama's own prospective second term. But, the President, while calling for such a flight, actually is scrapping the programs that would make it possible.

The same holds true with the question of reaching Mars. From a technical point of view, we are much closer today to being able to send humans to Mars then we were to being able to send men to the moon in 1961 when President Kennedy made his speech committing us to that goal - and we were there 8 years later. Given true, Kennedy-like commitment, we could have astronauts on the Red Planet within a decade. Yet president Obama chose to set that goal for the 2040s, a timeline so distant and hazy as to not require him to actually do anything to realize it.

Thus, the bottom line is this: Under the Obama plan, NASA will be able to send astronauts anywhere it likes, provided that its effort to do so begins after he leaves office.

The president's science advisor, Dr. John Holdren, attempts to justify this expensive ($10 billion per year) stalling game by claiming that the pause in flight programs will allow us to develop more advanced technologies that will make everything much more achievable later. This is false to the core. We know how to build heavy lift boosters – we flew our first, the Saturn 5, in 1967, and despite many NASA launcher technology programs over the past several decades (Shuttle C, NASP, ALS, NLS, X-33, Spacelifter, the Space Launch Initiative), nothing fundamentally better has been produced since. With current in-space propulsion technology, we can do a round trip mission to a near Earth asteroid or a one-way transit to Mars in 6 months – a time no greater than a standard crew shift on the space station. Holdren claims that he wants to develop a new electrically powered space thruster to speed such trips up. But without gigantic space nuclear power reactors to provide them with juice, such thrusters are useless, and the administration has no intention of developing such reactors. So far from enabling quick trips to Mars, the unnecessary futuristic electric thruster simply provides an excuse for not flying anywhere at all.

The American people want and deserve a space program that really is going somewhere. President Obama should give it to them. To do that, he needs to stop the fakery, and put real commitment behind his visionary rhetoric. That means a real program, whose effort will commence not in some future administration, but in his own; one whose goal is not Mars someday, but Mars in our time.

Will Obama Wreck NASA?

Commentary Magazine , June 2010

"We choose to go to the Moon! We choose to go to the Moon in this decade and do the other things, not because they are easy but because they are hard, because that goal will serve to organize and measure the best of our energies and skills, because that challenge is one that we are willing to accept, one we are unwilling to postpone, and one which we intend to win…This is in some measure an act of faith and vision, for we do not know what benefits await us…But space is there and we are going to climb it."

John F. Kennedy, September 1962

On April 15, Barack Obama travelled to Cape Canaveral. Speaking there to a closed audience of political allies, the president laid out his soaring vision for America's space program. Under the Obama plan, NASA will spend $100 billion on human spaceflight over the next ten years in order to accomplish nothing.

It must be said that the President phrased his policy wonderfully, so that -- with the Kennedy Space Center workforce prudently excluded -- the camp followers gathered for the occasion had no difficulty in providing the requisite applause. But beneath Mr. Obama's flowery rhetoric, his message was anything but Kennedy-esque. Translated into the English of mortals, he said:

We choose not to go to the Moon, nor do other things, because they are hard. We do not want a goal that will serve to organize and measure the best of our energies and skills, because that challenge is one that we are unwilling to accept, one we are quite willing to postpone, and one which we will not win….

The background to Obama's speech is as follows. In 2004, the Bush administration launched a program called Constellation to develop a set of flight systems, including the Orion crew capsule and the Ares 1 and Ares 5 medium and heavy lift boosters, that together would allow astronauts to return to the Moon by 2020, and subsequently fly to destinations beyond. Under the plan announced by the president, almost all of this will be cancelled. The only thing preserved out of the past 6 years and $9 billion worth of effort will be a version of the Orion capsule – but one so stripped down that it will only be useful as a lifeboat for bringing astronauts down from the space station, not as a craft capable of providing a ride up to orbit. With the Space Shuttle program set to end in the near future, what this means is that the only way Americans will be able to even reach low Earth orbit will be as passengers on Russian launchers.

In his speech, however, the president chose to represent the abandonment of the Moon program not as a retreat, but as a daring advance. "We've been to the Moon before," he said. "There's a lot more of space to explore." Obama proclaimed it was now time to set our sights on points beyond, to asteroids near the Earth, and to Mars. Indeed, he is correct on all counts. But the president's plan makes no provisions for actually following such a course. Instead, it initiates a long stall.

For example, as the first milestone in his allegedly daring program of exploration, Obama called for sending a crew to a near Earth asteroid by 2025. Such a flight is certainly achievable. All an asteroid mission requires is a launch vehicle such as the Ares 5, a crew capsule (such as the Orion), and a habitation module similar to that employed on the space station. Had Obama not cancelled the Ares 5, we could have used it to perform an asteroid mission by 2016—during Obama's own prospective second term. But, the president, while calling for such a flight, actually is scrapping the programs that would make it possible.

The same holds true for the question of reaching Mars. From a technical point of view, we are much closer today to a manned Mars trip than we were to being able to send men to the Moon in 1961, when President Kennedy made his speech committing us to that goal. Yet even then, we reached our destination in only 8 years. Given true, Kennedy-like commitment, we could have astronauts on the Red Planet within a decade. But president Obama chose to set that goal for the 2040s, a timeline so long and hazy as not to require him to actually do anything to realize it.

Under the Obama plan, NASA will be able to send astronauts anywhere it likes, provided that its effort to do so begins after he leaves office.

In an effort to provide some sex appeal for the new program, the administration also announced with great fanfare that its future budgets would provide some funds to support deliveries to the space station by new launch companies. This is a good idea, and long overdue, but not terribly important for the overall future or character of the space program, since NASA has been buying launches from private space firms for the past half century. A few more launches to low Earth orbit subcontracted out to corporate vendors will change very little.

The man responsible for devising the go-nowhere space policy is the president's top scientific advisor John Holdren, the director of the Office of Science and Technology Policy (OSTP). According to Holdren, the program's expensive ($10 billion per year) stalling game is justified. Eliminating any focused human-mission goals for NASA will supposedly allow the agency to develop more advanced technologies. This, in turn, will make everything much more achievable at some point in the future, when plans to go somewhere are finally drawn up. To the uninitiated, such arguments may appear plausible, but they are false to the core.

Over the course of its history, NASA has employed two distinct modes of operation. The first, which prevailed in the human spaceflight program during the period from 1961 to 1973, may be called Apollo Mode. The second, prevailing within the human spaceflight effort since 1974, may be called Shuttle Mode.

In Apollo Mode, business is conducted as follows: First, a mission goal is chosen. Next, a plan is developed to achieve this objective. Following this, hardware designs are developed to implement that plan, and if necessary, technologies are created to enable such hardware. The hardware set is then built, after which the mission is flown.

Shuttle Mode operates altogether 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 Apollo Mode is destination-driven, while Shuttle Mode pretends to be technology-driven but is actually constituency-driven. In Apollo Mode, technology is developed to support an overall mission, which means the space agency's efforts are focused and directed. In Shuttle Mode, NASA's efforts are random and entropic.

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

In today's dollars, NASA's average budget from 1961 to 1973 was about $19 billion per year. That is the same as NASA's current budget. Yet because it had the focus provided by a definite goal that served to "organize its energies and skills," the NASA of the Apollo period was vastly more effective than the equally well-funded agency is today.

Contrasting the brilliant record of achievement of NASA's human spaceflight program during the Apollo period with that of the past decade speaks for itself. It also, in no way, lets administrations between Kennedy's and Obama's off the hook. In technology development, too, the Apollo-era NASA was far superior, creating hydrogen oxygen rocket engines, multistage heavy-lift launch vehicles, nuclear rocket engines, space nuclear reactors, and beyond—all during a 13-year period. In contrast, during the agency's last quarter century of random research, no new technologies of major significance were developed.

It is this method, of constituency-driven, unfocussed, never completed, and perpetually incoherent research activity Holdren proposes as the basis for NASA's flight into the future.

According to Holdren, the three new “game-changing” technologies that NASA needs to develop before it attempts to design missions to the asteroids or Mars are electrically powered space thrusters, orbiting depots for propellant storage, and advanced heavy-lift boosters.

Given current technology, we can do a round trip mission to a near Earth asteroid or a one-way transit to Mars in 6 months – a time no greater than a standard crew shift on the space station. Holdren claims that we need to develop electrically powered space thrusters to speed such trips up. Thus advised, President Obama argued in his April 15 speech that “Critical to deep space exploration will be the development of breakthrough propulsion systems.” But without gigantic space nuclear power reactors to provide them with juice, such “breakthrough” thrusters are useless, and the administration has no intention of developing such reactors. So far from enabling quick trips to Mars, the research effort on the unnecessary and unpowerable electric thruster simply provides an excuse for not flying anywhere at all.

The orbital propellant depot’s potential utility as a way to enable new manned missions has never been established. To the contrary, none of NASA’s recent designs for Moon or Mars missions has involved refueling spacecraft from orbital propellant stations. To insist that mission architects adopt such a strategy because “this is the technology we are working on” is to force the program to accept a suboptimal system design based on an arbitrary decision to favor one technology.

Finally, it is simply not the case that we need new technologies to create heavy-lift launch systems. We not only know how to build them, we actually flew our first heavy-lifter, the Saturn 5, in 1967, just five years after the Apollo program contract to create it was signed. In the period since, however, instead of missions requiring booster production contracts, NASA funded a series of launcher technology research programs[1]. None of these resulted in the development of any real-flight hardware whatsoever. Under the Constellation program, NASA developed a fully satisfactory design for a Saturn 5 equivalent booster, which it called the Ares 5. Instead of proceeding with its development, however, Holdren has cancelled it, promising to produce a new design, after further research, by 2015. But all that is needed to give us a functioning heavy-lift booster is a decision to build it, which will never happen until there is a suitable mission.

Thus, without the guidance supplied by a driving mission, under the new Obama space policy, another 10 years and more than $100 billion will be spent by NASA's human spaceflight program without achieving anything significant. We may take part in another 20 flights to low Earth orbit, but together with the Russians, we have already flown there some 300 times over the past half-century. Spending a king's ransom to raise that total to 320 hardly seems worthwhile. Under the Obama plan, we may research some interesting technologies, but without a mission plan to guide their selection, they won't be the right technologies, they won't be realized as actual flight systems, they won't fit together, and they won't take us anywhere.

The American people want and deserve a space program that really is going somewhere. President Obama should give it to them. To do that, he needs to put real commitment behind his visionary rhetoric. That means a real program, whose effort will commence not in some future administration, but in his own; one whose goal is not Mars in our dreams, but Mars in our time.

"To the frontier the American intellect owes its striking characteristics. That coarseness of strength combined with acuteness and inquisitiveness; that practical, inventive turn of mind, quick to find expedients; that masterful grasp of material things, lacking in the artistic but

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[1] Shuttle C, NASP, ALS, NLS, X-33, Spacelifter, and the Space Launch Initiative.

powerful to effect great ends; that restless, nervous energy; that dominant individualism, working for good and evil, and withal that buoyancy and exuberance that comes from freedom – these are the traits of the frontier, or traits called out elsewhere because of the existence of the frontier."

- Frederick Jackson Turner, *The Significance of the Frontier in American History*, 1893.

For many years some of those on the political left have opposed the space program on the grounds that its funding would do more good if applied to various social needs on Earth. This is, at least, an arguable position.

Yet, the administration's proposal to paralyze the space program hasn't a thing to do with competition for funding. Quite the contrary; In order to make the plan palatable to NASA and its contractors, the president has offered to *increase* the agency's funding by $6 billion over the next six years. During the mid years of the coming decade, for example, Obama's budget proposes to fund the space station program at a rate of $3 billion per year, even though the nation will neither be conducting any launches to the space station nor building any new modules for it. Billions more will be spent updating the Shuttle launch pads, no matter that the Shuttles themselves will no longer be flying, and no new launchers will be developed to take their place. Still more will be spent on crew capsules that can only fly down, orbital propellant depots for refueling non-existent interplanetary spaceships, and electric rockets without sockets to provide them with power.

So if it's not about the money, why does the Obama administration wish to derail NASA? The answer can only be that objections lie not with what the agency *gets*, but with what is *does*.

There is good reason to believe that the administration doesn't like what NASA, and in particular NASA's human spaceflight program, represents.

NASA may be a government agency with the usual bureaucratic attributes, but it is also something else – it is the epitome of the pioneer spirit. The agency's formative adventure – and in a very real sense the agency itself – was launched by an administration whose slogan was "The New Frontier." It is not without meaning that so many of its craft have names like Liberty, Freedom, Pioneer, Voyager, Discovery, Endeavor, Pathfinder, Opportunity, and so on. Its astronauts are heroes in the most classic Homeric sense of the term, voluntarily risking death to do great deeds and win eternal glory.

The values championed by the Obama administration are comfort, security, protection, and dependence. But the frontier sings to our souls with different ideals; telling stirring tales of courage, risk, initiative, inventiveness, independence, and self-reliance. Considered as a make-work bureaucracy, NASA may be perfectly acceptable to those currently in power. But for mentalities that would criminalize the failure to buy health insurance, the notion of a government agency that celebrates the pioneer ethos by risking its crews on daring voyages of exploration across vast distances to terra incognita can only be repellent.

There is still a more imperative and even transcendent way in which NASA's human spaceflight program plays within our society's war of ideas. This has to do with our general view of the human future, and whether we consider it to be closed or open.

The closed future is one based upon the doctrine of limited resources. Its classic formulation can be found in the early nineteenth-century writings of Thomas Malthus but in its general form, the construct boils down to this: (A) There isn't enough of *x* to go around, where *x* = food supplies, lebensraum, natural resources, carbon use permissions, etc., as fashion dictates.

(B) Therefore human aspirations must be suppressed. (C) Thus, authorities must be empowered to effect such necessary suppression.

The Obama administration has embraced the closed future theory in its latest “global warming-requires-carbon-consumption-constraints” incarnation. For Holdren, however, the closed future is not merely a current fad to incorporate into a political portfolio. It has been central to his ideology and preoccupations throughout his career, going back well before global warming became the emergency-necessitating-tyranny de jour. Holdren has co-authored several books with Paul Ehrlich, the Stanford University insect ecologist and perennial panic merchant who wrote the 1968 best seller *The Population Bomb*. (As a solution to this problem, Ehrlich advocated that the U.S. force sterilization programs upon the third world, and set up a domestic Bureau of Population and Environment to issue childbirth permits to American citizens.) In 1988, Holdren and Ehrlich co-organized and led “The Cassandra Conference,” which put forth a menu of potential threats usable for justifying global regulation. These included overpopulation, industrial resource exhaustion, acid rain, deforestation, food shortages, energy shortages, the arms race, toxic pollution, runaway technology, and global cooling. In the first chapter of the proceedings of this conference, global “triage” advocate Garrett Hardin writes: “The idea of Progress has become a religion for many in our time. As evidence consider a statement made by the astronaut Scott Carpenter…: ‘I know – I am absolutely positive – that anything a man can imagine, he can accomplish.’”

Which is exactly the fundamental complaint that the closed-future folks have with the human space exploration program; it makes people think that everything is possible. The issue is not that resources from space might disrupt the would-be regulators rationing schemes. Rather, it is that the *idea* of an open future with unlimited resources undermines the walls of the prison of the mind that the self-appointed wardens of mankind seek to construct.

The closed futurists require us to believe that our possibilities are exhausted. But by beginning the expansion of the human domain to new worlds, the space program proudly relays the opposite message for all to hear; that we are not done, that far from living at the end of history, we are living at the beginning of history.

It is a message of true audacity and hope, and clearly not agreeable to John Holdren. The question is whether it is acceptable to Barack Obama.

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The Obama-Holdren space policy aroused furious opposition in Congress, and it is unclear as of this writing (February 2011) how things will go. A new threat has emerged, however, with the recent awakening of Congress to the problem of America's exploding budget deficit. In 2009 and 2010 it didn't matter to most of the political class that NASA's human spaceflight program had no plans to accomplish anything, as those in power were looking for ways to spend money so in order to stimulate the economy. But now matters have changed. If the goal of the human spaceflight effort is to accomplish nothing, that can be readily achieved for a lot less than the agency is getting now. Unless it gets its act together, NASA could soon face deep cuts.

Lacking any effective direction from either the White House, Congress, or NASA headquarters, the American space program seems adrift for now. There are people in the middle ranks of the space community who have ideas and commitment, but we are not united. How then can any progress be made? If leadership must come from below, what is the big-tent idea that can bring us together, and rally others to our banner? Here is my recommendation.

Opening Space with a 'Transorbital Railroad'

The New Atlantis, Fall 2010

In the history of the American frontier, the opening of the transcontinental railroad was an epochal event. Almost instantly, the trip to the West Coast, which had previously required an arduous multi-month trek and a massive investment for an average family, became a quick and affordable excursion. With the easing of commerce and communication across the continent, economic growth rapidly accelerated, creating new industries, new prosperity, and new communities.

Can we today deliver a similar masterstroke, and open the way to the full and rapid development of the space frontier? Can we open a "transorbital railroad"? Here's how it could be done.

First, we could set up a small transorbital railroad office in NASA, and fund it to buy six heavy-lift launches (100 tonnes to low-Earth orbit) and six medium-lift launches (20 tonnes to low-Earth orbit) per year from the private launch industry, with heavy- and medium-lift launches occurring on alternating months. (A tonne is a metric ton — 1,000 kilograms, or about 2,200 pounds.) The transorbital railroad office would pay the launch companies $500 million for each heavy launch and $100 million for each medium launch, thus requiring a total program expenditure of $3.6 billion per year — roughly 70 percent of the cost of the space shuttle program.

NASA would then sell standardized compartments on these launches to both government and private customers at subsidized rates based on the weight of the cargo being shipped. For example, on the heavy-lift vehicle, the entire 100-tonne-capacity launch could be offered for sale at $10 million, or divided into 10-tonne compartments for $1 million, 1-tonne subcompartments for $100,000, and 100-kilogram slots for $10,000 each. The same kind of pricing could be offered on the medium-lift launcher. While recovering only a tiny fraction of the transorbital railroad's costs, such low fees (levied primarily to discourage spurious use) would make spaceflight readily affordable.

As with a normal railroad here on Earth, the transorbital railroad's launches would occur in accordance with its schedule, regardless of whether or not all of its cargo capacity was subscribed by customers. Unsubscribed space would be filled with containers of water, food, or space-storable propellants. These standardized, pressurizable containers, equipped with tracking beacons, plumbing attachments, hatches, and electrical pass-throughs, would be released for orbital recovery by anyone with the initiative to collect them and put their contents and volumes to use in space. A payload dispenser, provided and loaded by the launch companies as part of their service, would be used to release each payload to go its separate way once orbit was achieved.

As noted above, the budget required to run the transorbital railroad would be 30 percent less than the space shuttle program, but it would accomplish far more. Since its inception in the early 1980s, the space shuttle program has averaged about four launches per year. Given the shuttle's theoretical maximum payload capacity (rarely used in full) of about 25 tonnes, this means that the shuttle program could be expected to deliver no more than 100 tonnes to low-Earth orbit per year. By contrast, the transorbital railroad would launch 720 tonnes per year. The

U.S. government would thus save a great deal of money, since its own departments in NASA, the military, and other agencies could avail themselves of the transorbital railroad's low rates to launch their payloads at trivial cost. Much further savings would occur, however, since with launch costs so reduced, it would no longer be necessary to spend billions to ensure the ultimate degree of spacecraft reliability. Instead, commercial-grade parts could be used, thereby cutting the cost of spacecraft construction by orders of magnitude. While some failures would result, they would be eminently affordable, and moreover, enable a greatly accelerated rate of technological advance in spacecraft design, since unproven, non-space-rated components could be much more rapidly put to the test. With both launch and spacecraft costs so sharply reduced, the financial consequences of any failures could be readily met by the purchase of insurance by the launch companies, which would reimburse both the government and payload owners in the event of a mishap.

With such a huge amount of lift capability available to the public at low cost, both public and private initiatives of every kind could take flight. If NASA desired to send human expeditions to other worlds, all it would have to do would be to buy space on the transorbital railroad for its payloads. But private enterprises or foundations could use the transorbital railroad to launch their own lunar or Mars probes — or settlements — as well. Those who believe in solar-power satellites would have the opportunity to put their business plans into action. Those wishing to operate orbital space hotels would have the launch capacity necessary to make their concepts feasible. Those hoping to offer commercial orbital ferry service to transfer payloads from low-Earth orbit to geostationary orbit or beyond would be able to get their crafts aloft, and have plenty of customers. As such enterprises multiplied, a tax base would be created both on

Earth and in space that would ultimately repay the government many times over for its transorbital railroad program costs.

While the implementation of a cargo-only transorbital railroad would be a great advance over our current situation, we should not exclude using it to transport human beings as well. As John F. Kennedy said at the dawn of the space age, “We go into space because whatever mankind must undertake, free men must fully share.” The transorbital railroad’s compartments should thus be open to receive passenger capsules provided by private vendors, thereby making affordable trips to orbit possible for anyone. Some might say that such open access to human spaceflight would put people at risk. This is true. But bold endeavors have always involved risk, whether personal or financial, and free men and women should be allowed to decide for themselves what risks they are willing to accept in order to achieve their dreams. This would free our space effort from the crippling constraint of excessively risk-averse government bureaucracy.

We don’t have to wait years to implement the transorbital railroad. We already have the capability to begin it right away, with twelve medium-lift launches per year using existing Atlas V, Delta IV, and Falcon 9 rockets. This would cost only $1.2 billion yearly, so if the program were fully budgeted from the beginning, more than $2 billion per year would still remain to support the development of heavy-lift vehicles through two or more fixed-price contracts issued on a competitive basis. Once these heavy-lift launchers became available, the full transorbital railroad service would be enabled. With a guaranteed market, launch vehicle companies would be able to put mass-production techniques into action, thereby causing the costs of their rockets to fall over time. This, in turn, would allow the transorbital railroad to further increase the frequency of its service, from one launch per month to two, three, or more, and would result in a dramatic drop in the cost of launch vehicles bought outside of the transorbital railroad program as well.

Some critics might argue that the implementation of the transorbital railroad would represent an anticompetitive subsidization of the U.S. launch industry. But the federal government has always subsidized transportation, supporting the development of trails, canals, railroads, seaports, bridges, tunnels, subways, highways, aircraft, and airports since the founding of the republic. Creating an affordable transportation infrastructure is one of the fundamental responsibilities of government. Meanwhile, international competitors in Europe or Asia who might be inclined to complain about anticompetitive behavior could create transorbital railroads of their own, thus multiplying even further mankind's capacity to reach into space.

Within a few years, we could be sending not a mere handful of people per year to orbit, but hundreds. Instead of a narrow space program with timid objectives moving forward at the snail's pace of politically constrained bureaucracy, we could have dozens of bold endeavors of every kind, attempting to realize every vision and every dream — reaching out, taking risks, and proving the impossible to be possible. With the aid of the transorbital railroad, the vast realm of the solar system could be truly opened to human hands, human minds, human hearts, and human enterprise: a great new frontier for free men and women to explore and settle, their creativity unbounded, with prospects and possibilities as unlimited as space itself.

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I was five when Sputnik flew. It's the first major world event I can personally remember. While the adults may have been terrified, I was exhilarated, because that little moving star said to me that the science fiction stories I was reading about the space-faring future would someday become reality.

Few people today still go out at night to scan the sky for satellites. I don't. Frankly, my sight isn't good enough to do it anymore. But there is one I can see - if not with my eyes then with my mind. It made its appearance in December 2010, and when it came into view, I felt again a touch of that grand, wonderful, magnificent, wild hope that filled my spirit on that chilly autumn night in 1957. Here's why.

The New Sputnik

Space News, December 13, 2010

On Oct. 4, 1957, Soviet engineers amazed the world by placing Sputnik, the first artificial satellite, into orbit around the Earth. Sputnik was a huge embarrassment for U.S. technological leaders, but in the end, the medicine was good for them. Shocked out of complacency, they got to work, and 12 years later Americans were walking on the Moon.

On Dec. 8, Sputnik flew again. Again the technological establishment was shown up, this time not by uppity Russkies, but by uppity Yanks. With the orbital flight and landing of its Falcon 9-Dragon combination, the Space Technologies Corp. (SpaceX) team accomplished a feat previously reserved for major governments, and did it on a budget one-tenth the size and a schedule one-quarter the length of that assumed as necessary by conventional bureaucratic planners.

The Falcon 9 medium-lift booster (capable of launching 10 tons to orbit) and Dragon capsule (potentially capable of being upgraded to transport up to seven astronauts) were created on a combined budget on the order of $200 million. Last year, SpaceX's Elon Musk told the Augustine commission that he could develop a heavy-lift vehicle for $2.5 billion. The commission chose to ignore him, instead insisting that development of a heavy-lift vehicle would cost $36 billion, and therefore both it, and any human Moon or Mars exploration programs that might require it, are beyond the nation's means for the coming decade. But the Dec. 8 flight put the lie to such counsels of despair.

They say it can't be done. But SpaceX shows that it can. If a 10-ton-to-orbit system can be developed for $200 million, then a 100-ton-to-orbit launcher for $2 billion is definitely in the cards. And if a heavy-lift booster can be developed for a couple of billion dollars, so can each of the other principal hardware elements required for a human exploration program.

The best flight plan to enable a human lunar base is a direct-landing, direct-return approach, as it has the fewest hardware elements and the simplest operational requirements and enables the most robust exploration capability with the greatest safety. If such a flight plan is adopted, a total of just five principal transportation system units would be required:

* A heavy-lift booster.

* An Earth escape rocket stage (for translunar injection and lunar orbit capture).

* A lunar landing stage.

* An Earth return stage (for lunar ascent and Trans Earth Injection).

* A crew capsule.

Using hydrogen/oxygen propulsion for all in-space flight elements, a transportation system based on a 100-ton-to-orbit booster could land 20 tons on the Moon, which is more than enough to deliver a crew capsule to the lunar surface together with a fully fueled Earth return stage capable of propelling it home. Alternatively, the same 20-ton-to-lunar-surface landing system could be used to deliver large cargoes, such as habitation modules, to the Moon, enabling the quick buildup of a substantial lunar base.

Extending such a modular system for flights into deep space is straightforward. Near-Earth asteroid missions could be accomplished using the heavy-lift booster, the Earth escape stage, a habitation module and the crew capsule. Human Mars missions could be accomplished by employing the heavy-lift booster and Earth escape stage to send major payloads directly to the red planet, and adding three units to the asteroid mission set:

* A Mars aerocapture, entry and landing stage.

* A Mars ascent vehicle system.

* A space-storable Earth return rocket stage.

Given the commonality of hardware units among those employed on Moon, Mars, and asteroid missions, a transportation system enabling exploration of all three objectives could be built out of a total of nine elements. At a development cost on the order of $2 billion each, that’s a price tag of about $20 billion to open up the entire inner solar system to human exploration, with key destinations being reached well before the current decade is out.

Such are the implications of SpaceX’s Sputnik, if properly taken to heart. Those embarrassed should take up its challenge and resolve to raise their mettle to meet its test. A new standard has been set. Hear its call. Beep. Beep. We have nothing to lose but our chains, and worlds to win. Beep. Beep. Sputnik flies again.

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In April, 2011, the SpaceX company announced that it was developing a low-end heavy lift booster, called the Falcon 9 Heavy, capability of lifting 53 metric tons to low Earth orbit, with first flight scheduled for 2013.Upon hearing this, I travelled to SpaceX’s Los Angeles facility, met with CEO Elon Musk, toured the facility, and liked what I saw. The place was a beehive of activity, turning out rocket engines, boosters, and crew capsules. This set me to thinking. At 53 tons lift capability, the Falcon-9H was lighter than desirable for supporting human Mars exploration, but in contrast to a more optimal system, it would soon be actually available. Could it be used to get a near-term human Mars exploration program underway? So I began to work on the problem, and the numbers came together, indicating that if the crew were reduced to 2, a human exploration mission utilizing three Falcon-9H launches to implement a scaled down-version of the Mars Semi-Direct mission plan could indeed be feasible. I wrote up the results, and on May 14, 2011 published them in summary form as an op-ed piece in the *Wall Street Journal,* with a backup paper providing additional technical detail posted on the Mars society website. These are presented below.

How We Can Fly to Mars in This Decade – And on the Cheap

The technology now exists and at half the cost of a Space Shuttle flight. All that is lacking is the political will to take more risks.

By Robert Zubrin

Wall Street Journal, May 14, 2011

Spacex, a private firm that develops rockets and spacecraft, recently announced it will field a heavy lift rocket within two years that can deliver more than twice the payload of any booster now flying. This poses a thrilling question: Can we reach Mars in this decade?

It may seem incredible—since conventional presentations of human Mars exploration missions are filled with depictions of gigantic, futuristic, nuclear-powered interplanetary spaceships whose operations are supported by a virtual parallel universe of orbital infrastructure.

There's nothing like that on the horizon. But I believe we could reach Mars with the tools we have today, or will have in short order. Here's how it could be done:

The Spacex's Falcon-9 Heavy rocket will have a launch capacity of 53 metric tons to low Earth orbit. This means that if a conventional hydrogen-oxygen chemical rocket upper stage were added, it would have the capability of sending 17.5 tons on a trajectory to Mars, placing 14 tons in Mars orbit, or landing 11 tons on the Martian surface.

The company has also developed and is in the process of demonstrating a crew capsule, known as the Dragon, which has a mass of about eight tons. While its current intended mission is to ferry up to seven astronauts to the International Space Station, the Dragon's heat shield system is capable of withstanding re-entry from interplanetary trajectories, not just from Earth orbit. It's rather small for an interplanetary spaceship, but it is designed for multiyear life, and it should be spacious enough for a crew of two astronauts who have the right stuff.

Thus a Mars mission could be accomplished utilizing three Falcon-9 Heavy launches. One would deliver to Mars orbit an unmanned Dragon capsule with a kerosene/oxygen chemical rocket stage of sufficient power to drive it back to Earth. This is the Earth Return Vehicle.
A second launch will deliver to the Martian surface an 11-ton payload consisting of a two-ton Mars Ascent Vehicle employing a single methane/oxygen rocket propulsion stage, a small automated chemical reactor system, three tons of surface exploration gear, and a 10-kilowatt power supply, which could be either nuclear or solar.

The Mars Ascent Vehicle would carry 2.6 tons of methane in its propellant tanks, but not the nine tons of liquid oxygen required to burn it. Instead, the oxygen could be made over a 500-day period by using the chemical reactor to break down the carbon dioxide that composes 95% of the Martian atmosphere.

Using technology to generate oxygen rather than transporting it saves a great deal of mass. It also provides copious power and unlimited oxygen to the crew once they arrive.

Once these elements are in place, the third launch would occur, which would send a Dragon capsule with a crew of two astronauts on a direct trajectory to Mars. The capsule would carry 2500 kilograms of consumables—sufficient, if water and oxygen recycling systems are employed, to support the two-person crew for up to three years. Given the available payload capacity, a light ground vehicle and several hundred kilograms of science instruments could be taken along as well.

The crew would reach Mars in six months and land their Dragon capsule near the Mars Ascent Vehicle. They would spend the next year and a half exploring.

Using their ground vehicle for mobility and the Dragon as their home and laboratory, they could search the Martian surface for fossil evidence of past life that may have existed in the past when the Red Planet featured standing bodies of liquid water. They also could set up drilling rigs to bring up samples of subsurface water, within which native microbial life may yet persist to this day. If they find either, it will prove that life is not unique to the Earth, answering a question that thinking men and women have wondered upon for millennia.

At the end of their 18-month surface stay, the crew would transfer to the Mars Ascent Vehicle, take off, and rendezvous with the Earth Return Vehicle in orbit. This craft would then take them on a six-month flight back to Earth, whereupon it would enter the atmosphere and splash down to an ocean landing.

There is nothing in this plan that is beyond our current level of technology. Nor would the costs be excessive. Falcon-9 Heavy launches are priced at about $100 million each, and

Dragons are even cheaper. Adopting such an approach, we could send expeditions to Mars at half the mission cost currently required to launch a Space Shuttle flight.

What is required, however, is a different attitude towards risk than currently pervades the space policy bureaucracy. There is no question that the plan proposed here involves considerable risk. So does any plan that actually involves sending humans to Mars, rather than talking about it indefinitely. True, there are a variety of precursor missions, technology developments, and testing programs that might be recommended as ways or reducing risk. There are an infinite number of such potential missions and programs. If we try to do even a significant fraction of them before committing to the mission we will never get to Mars.

But is it responsible to forgo any expenditure that might reduce somewhat the risk to the crew? I believe so. The purpose of the space program is to explore space, and its expenditures come at the cost of other national priorities. If we want to reduce risk to human life, there are vastly more effective ways of doing so than by spending $10 billion per year for the next two or three decades on a human spaceflight program mired for study purposes in low Earth orbit. We could spend the money on childhood vaccinations, fire escape inspections, highway repairs, better body armor for the troops—take your pick. For NASA managers to demand that the mission be delayed for decades while several hundred billion dollars is spent to marginally reduce the risk to a handful of volunteers, when the same funds spend elsewhere could save the lives of tens of thousands, is narcissistic in the extreme.

The Falcon 9 Heavy is scheduled for its first flight in 2013. All of the other hardware elements described in this plan could be made ready for flight within the next few years as well. NASA’s astronauts have gone nowhere new since 1972, but these four decades of wasteful stagnation need not continue endlessly. If President Obama were to act decisively, and bravely embrace this plan, we could have our first team of human explorers on the Red Planet by 2016. The American people want and deserve a space program that is really going somewhere. It's time they got one. Fortune Favors the Bold. Mr. President, seize the day.

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The Use of Spacex Hardware to Accomplish Near-Term Human Mars Mission

Robert Zubrin

Pioneer Astronautics

May 15, 2011

The recent announcement by the entrepreneurial Spacex company that it intends to field within two years a heavy lift rocket capable of delivering more than twice the payload of any booster now flying poses a thrilling question: Can we reach Mars in this decade?

I believe the answer is yes. In this paper, I will lay out a plan to make use of the soon-to-be-available Spacex systems to accomplish near-term human Mars exploration with minimal technology development. First, I will layout a baseline mission architecture and plan. In the next section, I will discuss various technology alternatives available within the selected mission architecture. Then, in the following section, I will discuss alternative mission architectures. I will then conclude with some overall observations bearing on the question of sustained exploration and settlement of Mars.

It may be noted that the author is not an employee of the Spacex company, and does not have detailed knowledge of the Spacex systems. It will take the hard work and ingenuity of the Spacex engineers to develop configurations and systems that can make these ideas a reality.

Nevertheless, it is apparent that if an approach such as that recommended here is adopted, the requirements and capabilities numbers can be made to converge. We can reach Mars in our time.

1. Baseline Mission Plan

Here's how it could be done. The Spacex Falcon-9 Heavy will have a launch capacity of 53 metric tons to low Earth orbit. This means that if a conventional hydrogen-oxygen chemical rocket upper stage were added, it could have the capability of sending about 17.5 tons on a trajectory to Mars, placing 14 tons in Mars orbit, or landing 11 tons on the Martian surface. The same company has also developed and is in the process of demonstrating a crew capsule, known as the Dragon, which has a mass of about 8 tons. While its current intended mission is to ferry up to 7 astronauts to the International Space Station, the Dragon's heat shield system is overdesigned, and is capable of withstanding rentry not just from Earth orbit, but from interplanetary trajectories. It's rather small for an interplanetary spaceship, but it is designed for multiyear life, and if we cut its crew from 7 to 2, it should be spacious enough for a pair of astronauts who have the right stuff.

Using these basic tools, a Mars mission could be done utilizing three Falcon-9 Heavy launches. One would deliver to Mars orbit an unmanned Dragon capsule with a kerosene/oxygen chemical rocket stage of sufficient power to drive it back to Earth. This is the Earth return vehicle (ERV).

A second launch will deliver to the Martian surface an 11 ton payload consisting of a Mars Ascent Vehicle (MAV) employing a single methane/oxygen rocket propulsion stage, a small automated chemical reactor system, 3 tons of surface exploration gear, and a 10 kilowatt power supply, which could be either nuclear or solar. The MAV would land with its propellant tanks filled with 2.6 tons of methane, but without the 9 tons of liquid oxygen required to burn it. This oxygen could be made over a 500 day period by using the chemical reactor to break down the carbon dioxide that composes 95 percent of the Martian atmosphere. Since together the reactor and the power system together only weigh about 2 tons, using such technology to generate the required oxygen in-situ rather than transporting it saves a great deal of mass, and offers the further benefit of providing copious power and unlimited oxygen to the crew once they arrive. Combined, the 11.6 tons or methane/oxygen propellant is sufficient to deliver a 2 ton crew cabin (equal in dry mass to the lunar ascent vehicle used during the Apollo missions) from the Martian surface to high Mars orbit where it can rendezvous with the ERV.

Once these elements are in place, the third launch would occur, which would send a Dragon capsule with a crew of two astronauts on a direct trajectory or Mars. The capsule would carry 2500 kilograms of consumables, sufficient, if water and oxygen recycling systems are employed, to support the two-person crew for up to three years. Given the available payload capacity, a light ground vehicle and several hundred kilograms of science instruments could be taken along as well.

The crew would take six months to reach Mars, after which they would land their Dragon capsule near the MAV. They would then spend the next year and a half exploring Mars. Using their ground vehicle for mobility and the Dragon as their home and laboratory, they could search the Martian surface for fossil evidence of past life they may have existed in the past when the Red Planet featured standing bodies of liquid water. Going further, they could set up drilling rigs to bring up samples of subsurface water within which native microbial life may yet persist to this day. If they find either, they will prove that life is not unique to the Earth, but is a general phenomenon in the universe, thereby answering a question that thinking men and women have wondered upon for millennia.

At the end of their 18 month surface stay, the crew would transfer to the MAV, take off, and rendezvous with the ERV. This craft would then take them on a six month flight back to Earth, whereupon it would enter the atmosphere and splash down to an ocean landing.

2. Technical Alternatives within the Mission Architecture

a. MAV and associated systems

In the plan described above, methane/oxygen is proposed as the propulsion system for the MAV, with all the methane brought from Earth, and all the oxygen made on Mars from the atmosphere. This method was selected over any involving hydrogen (either as feedstock for propellant manufacture or as propellant itself) as it eliminates the need to transport cryogenic hydrogen from Earth or store it on the Martian surface, or the need to mine Martian soil for water. If terrestrial hydrogen can be transported to make the methane, about 1.9 tons of landed mass could be saved. Transporting methane was chosen over a system using kerosene/oxygen for Mars ascent, with kerosene coming from Earth and oxygen from Mars because methane offers higher performance (Isp 375 s vs Isp 350 s) than kerosene, and its selection makes the system more evolvable, as once Martian water does become available, methane can be readily manufactured on Mars, saving 2.6 tons of landed mass per mission compared to transporting methane, or about 3 tons per mission compared to transporting kerosene. That said, the choice of using kersosene/oxygen for Mars ascent instead of methane oxygen is feasible within the limits of the mass delivery capabilities of the systems under discussion. It thus represents a viable alternative option, reducing development costs, albeit with reduced payload capability and evolvability.

b. ERV and associated systems.

A kerosene/oxygen system is suggested for Trans-Earth injection. A methane/oxygen system would offer increased capability if it were available. The performance improvement is modest, however, as the required delta-V for TEI from a highly elliptical orbit around Mars is only 1.5 km/s. Hydrogen/oxygen is rejected for TEI in order to avoid the need for long duration storage of hydrogen. The 14 ton Mars orbital insertion mass estimate is based on the assumption of the use of an auxiliary aerobrake with a mass of 2 tons to accomplish the bulk of braking delta-V. If the system can be configured so that that Dragon's own aerobrake can play a role in this maneuver, this delivered mass could be increased. If it is decided that the ~1 km/s delta V required for minimal Mars orbit capture needs to be done via rocket propulsion, this mass could be reduced to as little as 12 tons (assuming kerosene/oxygen propulsion). This would still be enough to enable the mission. The orbit employed by the ERV is a loosely bound 250 km by 1 sol orbit. This minimizes the delta V for orbital capture and departure, while maintaining the ERV in a synchronous relationship to the landing site. Habitable volume on the ERV can be greatly expanded by using an auxiliary inflatable cabin, as discussed in the Appendix.

c. The hab craft.

The Dragon is chosen for the primary hab and ERV vehicle because it is available. It is not ideal. Habitation space of the Dragon alone after landing appears to be about 80 square feet, somewhat smaller than the 100 square feet of a small standard Tokyo apartment. Additional habitation space and substantial mission logistics backup could be provided by landing an additional Dragon at the landing site in advance, loaded with extra supplies and equipment. Solar flare protection can be provided on the way out by proper placement of provisions, or by the use of a personal water-filled solar flare protection “sleeping bag.” For concepts for using inflatables to greatly expand living space during flight and/or after landing, see note in Appendix.

3. Alternatives to the Selected Mission Architecture

a. Direct Return

In an ideal world, direct return from the Martian surface using in-situ produced propellants is the way to go. This, of course, is the basis of the Mars Direct plan, which other things being equal, would be my preference. However, under the assumption that this is a near term mission using soon-to-be-available systems with minimal technology development, that is not feasible. For example, direct return of a Dragon capsule from the Martian surface in one stage using hydrogen/oxygen propellant produced from Martian water would require about 50 metric tons of propellant. This would require 50 kilowatts of 24-hour power to produce, which, assuming a nuclear reactor is not available, means a solar array of about 5000 square meters. Such an array would likely weigh at least 10 tons, thereby blowing the mission mass budget, and be difficult to deploy by automated systems as well. In addition, assuming a water concentration of 4% by weight in the soil, obtaining 50 tons of Martian water would require mining 1200 tons of soil, which is a non-starter. Using Martian water in combination with atmospheric CO_2 to produce methane/oxygen instead of hydrogen/oxygen would cut the power requirement by about 40% and the mining requirement by 60%, but the plan still remains unfeasible within the limits of the available systems. Thus the use of a lightweight LEM-type vehicle to perform Mars ascent and rendezvous with a Dragon placed in highly elliptical Mars orbit is necessary if the mission mass requirements and delivery capabilities are to converge.

b. Double rendezvous

An alternative to the plan described here might be to fly the crew to Mars in the same Dragon used for the ERV (i.e. a "mothership"), and fly another Dragon to the Martian surface to provide a surface hab. The crew would then rendezvous with the MAV, and take it down to land near the surface hab, which they would live in for 1.5 years, after which they would ride the MAV back up to the ERV. This architecture is feasible in principle, but inferior to the one selected because it requires two orbital rendezvous per mission instead of one, does not allow the ascent propellant to be made in advance of the launch of the crew, and lands the crew separate from substantial living quarters or extended life support capability, without any countervailing advantages.

4. General Observations

The proper goal of a human Mars mission program should be sustained exploration followed by settlement. This can only be done if costs are kept low. This plan creates sufficiently low cost mission architecture to enable sustained exploration. Falcon-9 Heavy launches are priced at about $100 million each, and Dragons are presumably even cheaper. Adopting such an approach, we could send expeditions to Mars at half the mission cost currently required to launch a Space Shuttle flight. In addition, both Dragons employed in the mission are re-used: one remaining on site to contribute to the growing Mars base, and the other returned to Earth. It will be observed that no orbital infrastructure, advanced orbital operations, advanced propulsion, or even surface nuclear power systems (although the 10 kilowatt Topaz demonstrated by the Soviet program would fit the bill) are required to enable the mission. This, plus the fact that the mission can be done using a booster soon to be available minimizes development cost and time, and moves the potential timeframe of the mission from the indefinite future to the near-present.

For settlement, cheap one-way transportation to Mars is required. In addition, cargos larger in scale both in mass and in dimension need to be delivered. This will require development of a true heavy lift vehicle, with at least an 8 meter and preferably a 10 meter fairing, and launch capabilities of over 100 tonnes to orbit. Furthermore, if costs are to be lowered, reusability is desired. However reusability needs to be placed in perspective. The most important part of a space transportation system to make reusable is the lowest stage, since this is the most massive (therefore offering the greatest reusability savings), and adding mass to it (to make it reusable) does not cause any increase in the mass of the stages above it. On the other hand, making upper stages or interplanetary transfer systems reusable only saves a small amount of hardware, but causes the mass of the stages below them to increase. Thus reusability needs to be implemented in steps from the bottom-up, rather than from the top down (as was unfortunately done in the Shuttle.)

Using the mission architecture described here, and the soon to be available Falcon-9 Heavy and Dragon, the first human missions could be done and an initial outpost could be established on Mars during the present decade. With the advent of a heavy lift vehicle capable of delivering ~9 m diameter hab modules in the 30 ton class one-way to Mars, the subsidized settlement of Mars could begin, with such return flights as remain necessary continuing to be conducted by the F9H/Dragon-derived systems. If the heavy-lift vehicle can evolve to reusability, starting with its lowest stages, costs of one-way transport to Mars could be lowered further, eventually reaching the point where individuals of fairly ordinary means would be able to pay their own way, freely venturing forth to start new lives on a new world.

Appendix: Notes Concerning Various Mission Issues

A.1. Zero Gravity Health Effects

There is no need for zero gravity exposure. Artificial gravity can be provided to the crew by tethering the Dragon off the TMI stage, in the same way as is recommended in the baseline Mars Direct plan.

A. 2. Radiation

Cosmic ray radiation exposure for the crew is precisely THE SAME as that which would be received by those on any other credible Mars mission, all of which would use the 6 month Conjunction class trajectory to Mars, both because that is the point of diminishing returns (the "knee of the curve") where delta-V trades off against trip time, and because it is uniquely the trajectory that provides a 2-year free return orbit after launch from Earth. Assuming the baseline mission, the total cosmic ray dose would be no greater than that already received by a half dozen cosmonauts and astronauts who participated in long duration missions on Mir or ISS, with no radiation induced health effects having been reported. (Cosmic ray dose rates on ISS are 50% those of interplanetary space. The Earth's magnetic field does not shield effectively against cosmic rays. In fact, with a crew of 6, the current planned ISS program will inflict the equivalent of 30 man-years of interplanetary travel GCR doses on its crews over the next decade. This is an order of magnitude more than that which will be received by the crew of the mission proposed here.) There are enough consumables on board to provide shielding against solar flares.

A.3. Aerocapture

The preferred method of Mars capture is aerocapture, rather than direct entry. This means that the Dragon aeroshield, which has some lifting capability, may well be adequate. This is a complex problem, but a back of the envelope calculation indicates that the Dragon's shield size is in the ballpark. Thus, consider a loaded Dragon system with an entry mass of 17000 kg, an effective shield diameter of 4 meters, a drag coefficient of 1, coming in with an entry velocity of 6 km/s at an altitude of 33 km, where the Mars atmospheric density is 0.8 gm/m^3. Setting drag equal to mass times deceleration, It can be seen that that the system would decelerate at a speed of 42 m/s^2, or a little over 4 gs. It could thus perform a 1 km/s deceleration in about 25 seconds, during which time it would travel about 140 km. This deceleration is sufficient to capture the spacecraft from an interplanetary trajectory into a loosely bound highly elliptical orbit around Mars. If the perigee is not raised, the craft will reenter again, and again, progressively lowering the apogee of its orbit, until either a desired apogee for orbital operations is achieved or the craft is committed to entry for purposes of landing. That said, if a larger aerobrake were desired, this could be created by adding either a flex-fabric or inflatable skirt to the Dragon core shield.

A.4. EDL

Using just its aeroshield for deceleration, the Dragon would have a terminal velocity of around 340 m/s on Mars at low altitude (air density 16 gm/m3). So we could either give it a rocket delta-V capability of 600 m/s (a 20% mass hit assuming storable or RP/O2 propulsion, Isp~330 s) to land all propulsive, or we could use a drogue to slow it down (a 20 m diameter chute would slow it to ~70 m/s) and then employ a much smaller rocket delta-V for landing.

A.5.Living Volume

The habitable volume of the Dragon capsule is admittedly lower than optimal. However it should be noted that with 5 cubic meters per crew member, it is 2.5 times higher than the 2 cubic meters per crew member possessed by Apollo crews. Alternative comparisons include 9 cubic meters per crew member on the Space Shuttle, or 8 cubic meters per crew member on a German U-Boat (Type VII, the fleet workhorse) during WWII. This would be uncomfortable, but ultimately, workable by a truly dedicated crew.

However these limits can be transcended. The Dragon has a 14 cubic meter cargo area hold below the aeroshield. Into this we could pack an inflatable hab module, in deflated form, but which if inflated, could be as much as 6 m in diameter and perhaps 8 m long, thereby providing 3 decks, with added volume of 226 cubic meters of useful habitable volume and a total floor space of 85 square meters, equal to 85 percent as much as that in the Mars Society's MDRS or FMARS stations, which have proved adequate in size for crews of 6. After Trans Mars injection, the Dragon would pull away from the cargo section and turn around, then return to mate its docking hatch with one in the inflatable. It would then pull the inflatable out of the cargo hold, much as the Apollo command module pulled out the LEM. The inflatable could then be inflated. The other end of the inflatable would be attached to the tether, which is connected to the TMI stage, for use in creating artificial gravity.

Upon reaching Mars the inflatable could either be expended, along with the tether system and TMI stage, prior to aerocapture. Alternatively, and optimally, the tether and TMI stage alone would be expended, but the inflatable deflated and retained for redeployment as a ground hab after landing.

Extra space could be also be provided on the ground by using a 4th launch to preland another Dragon loaded with supplies, including one or more inflatable modules which could be set up by the crew after they land.

A.6. Overall Risk

The mission architecture is much safer than any based on complex mega systems requiring orbital assembly, since the quality control of orbital assembly does not compare with that which can be accomplished on the ground. It would be better to have a crew of four, but if we are to do it with Falcon 9 Heavy's, a crew of two is all we can do. While such a crew size lacks a degree of redundancy otherwise desirable, it also offers the counter benefit of putting the fewest number of people at risk on the first mission. It's quite true that not flying anywhere at all would be safer, but if you want to get to Mars, you have to go to Mars.