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Heavy Lift: A Pillar of American Space Flight

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In mid-April, President Obama spoke at length about the future of the American space exploration effort. He clearly stated his commitment – "And so, as President, I believe that space exploration is not a luxury, it's not an afterthought in America's quest for a brighter future — it is an essential part of that quest." But, he rightly noted, "The challenges facing our space program are different, and our imperatives for this program are different, than in decades past." He is right. There are serious questions about the sustainability of U.S. interest, and subsequently investment, in space exploration. The Augustine Commission identified the basic problem in its 2009 report: "The U.S. human spaceflight program appears to be on an unsustainable trajectory. It is perpetuating the perilous practice of pursuing goals that do not match allocated resources."

Among the many questions that have arisen as the nation considers the future of the exploration is — should the U.S. invest in propulsion capabilities to travel beyond low earth orbit now or later?

On this question, the President identified his administration's priorities – "Next, we will invest more than $3 billion to conduct research on an advanced "heavy lift rocket" — a vehicle to efficiently send into orbit the crew capsules, propulsion systems, and large quantities of supplies needed to reach deep space. In developing this new vehicle, we will not only look at revising or modifying older models; we want to look at new designs, new materials, new technologies that will transform not just where we can go but what we can do when we get there. And we will finalize a rocket design no later than 2015 and then begin to build it. And I want everybody to understand: That's at least two years earlier than previously planned — and that's conservative, given that the previous program was behind schedule and over budget."

A fact sheet from the White House Office of Science and Technology Policy (OSTP) offers additional details:

> This new rocket would eventually lift future deep-space spacecraft to enable humans to expand our reach toward Mars and the rest of the Solar System. This new rocket would take advantage of the new technology investments proposed in the budget — primarily a $3.1 billion investment over five years on heavy-lift R&D. This propulsion R&D effort will include development of a U.S. first-stage hydrocarbon engine for potential use in future heavy lift (and other) launch systems, as well as basic research in areas such as new propellants, advanced propulsion materials manufacturing techniques, combustion processes, and engine health monitoring, all of which are expected to shorten the development time for any future heavy-lift rocket. The new rocket also will benefit from the budget's proposed R&D on other breakthrough technologies in our new strategy for human exploration (such as in-space refueling), which should make possible a more cost-effective and optimized heavy lift capability as part of future exploration architectures.

The approach is reasonable enough — Invest in the future in hopes of accomplishing the heavy-lift task more effectively and efficiently. Substantial investments in space-related research and development (R&D) are desirable and the administration's emphasis on inciting innovation is commendable. But, it is not without questions and, according to some critics, lacks focus. What basic research will mature in five years time to be suitable for use in a deployable rocket? How will this research transfer into development programs? Is basic research the area of greatest need? Are any of the planned investments sufficiently radical to justify the delay in building a new heavy lift capability? What entities, organizations, laboratories, companies, or universities will perform this research? Who will decide which projects are funded, which are not, and when a project is terminated for failing to progress satisfactorily, simply failing, or because higher priorities have emerged? What happens to the industrial base and the workforce needed to build these systems during the five years? What are the implications of delaying the development of new capabilities by five years?

In the best case, in 2016 (NASA states that it will begin a new program in 2015 so actual work could begin the following year after appropriations for the new effort are approved by the Congress), a new heavy lift program actually will begin. At worst, NASA will have defined what vehicle it wishes to design and build, but still need to define the parameters of a new acquisition program which will add additional time to the launch of a proposed new program. Either alternative will have to incorporate a host of new technologies, materials, and manufacturing processes, if the proposed R&D plan produces to hope and expectation, which, in turn, requires testing and analysis to ensure safety and reliability. In basic terms, we'll have to see if the stuff works before it can be put to work.

Those validation efforts take time and effort, typically measured in decades. In the meantime, time inexorably ticks away. If NASA is planning a mission to an asteroid in 2025, while delaying a decision on the heavy lift vehicle that will carry astronauts or robots or whatever there until 2015, only a decade is left to complete development, engineer, build, and test the vehicle. When most major acquisition programs run on schedules of 10-20 years, this timeline is unrealistic (without accompanying acquisition and procurement reforms).

And, as with any technical endeavor, technology development efforts may succeed or they may fail. If five years goes by and the technical investments prove less promising than hoped, the capabilities and techniques for heavy lift will look much the same as they do today.

In 2020 or so, the Space Station will no longer be viable, we are told. What will remain of the American space program at that point — the emerging commercial launch industry servicing low earth orbit, and robotic and space science missions. The risk with the current approach is that the U.S. will be left without a viable program for deep space exploration in the latter years of this decade and the early 2020s.

Are there technologies worth waiting for? A breakthrough technology that could radically change the cost or efficiency of space travel might be worth the wait. The Augustine Commission identified solar and nuclear propulsion technologies as promising. On-orbit refueling stations are another concept frequently mentioned. That capability changes the size and mass of the lift vehicle (because it will not need to carry as much fuel into space), but the technical characteristics of the vehicle itself may change very little. Certainly, a breakthrough propulsion system has the potential to revolutionize space travel, but the probability of such breakthroughs emerging in a five-year R&D program is low; a view validated by informal discussions with space experts over recent weeks. At a minimum, it appears safe to say that it is equally likely that there will be no breakthrough in propulsion that will require a reconfiguration of the basic approaches to

heavy lift in the timeframe established by the President.

Others suggest the delay in developing a new launch vehicle is justifiable because there is no mission for which such a capability is required. Developing a launch vehicle without knowing what it will carry and to where is problematic. Such an effort would lack focus and is potentially wasteful if the mission never materializes. These concerns have validity, but they speak to a broader issue — what does the United States expect from its human space exploration program in the decades to come?

President Obama outlined the goal of traveling to an asteroid in the mid-to-late 2020s and a mid-2030s goal of sending astronauts to orbit Mars. If that is the earliest one requires heavy lift capabilities, then delaying work on a new system until the early 2020s may seem reasonable, but that decision is not without known costs. In 15-20 years much will change. Some of the large and small firms that work in this field will have exited or switched focus. Skilled workers will have retired, moved on to other interests, or simply forgotten the tacit knowledge accumulated over time. Certainly the companies that plan to compete for the launch market to low earth orbit will remain, but the marketplace will look much different than it does today.

Furthermore, space policy is clearly in a period of transition. The end of the Shuttle era and the near-end of the Space Station leaves the public and policy makers wondering what is next for U.S. space. While public support for space exploration is widespread, there is a lack of agreement about priorities and objectives. President Bush thought a return to the Moon would inspire support. President Obama sees missions to asteroids and Mars as providing the necessary inspiration of the public and policymakers. Neither administration nor successive Congresses provided the funds needed to make these visions reality. The Augustine Commission declared human exploration beyond low earth orbit "not viable" under the FY 2010 budget, basically signaling the end of President Bush's Vision for Space Exploration. A recent analysis by staff at the House Science Committee declared "the budget for the administration's proposed plan through 2025 [the date of the asteroid mission] is $47 billion lower than the amount the Augustine committee determined would be needed to make any of its expansion options viable over the same period." Clearly, future space exploration efforts face serious budget challenges. At the timelines for space missions now being discussed, the next President, whether they take office in 2013 or 2017, will have as much, if not more, to say on where the U.S is going and how it is going to get there.

This period of uncertainty still leaves the stark choice — should the U.S. pause the construction of a new heavy lift launch vehicle for the foreseeable future? The balance of the evidence suggests "no" is the appropriate answer.

Answering no does not mean the R&D initiative recommended by the President should be put aside either. The two are not mutually exclusive. In fact, their goals are mutually reinforcing. An active development programs offers a ready home for the maturation and use of the research and innovation that emerges from the R&D investments. Similarly, an ongoing development program offers focus and priority to the near-term elements of the R&D program.

Maintaining an active program obviously sustains the workforce and industrial base, and ensures the preservation of critical experiential knowledge in government and industry. It also precludes the atrophy of infrastructure and facilities.

Any heavy lift program will have to deal with obvious constraints. Available funds will be limited. President Obama is set to grow the NASA budget, but his budget does not include this kind of initiative. Cuts to existing programs, reallocations from new initiatives, or new funds will have to be found to accommodate it. Absent a reversal of priorities by the administration or robust support from the

Congress, limited resources will be available. Similarly, without a destination, any program will have to exhibit flexibility and adaptability, and capable of operating efficiently under low launch rate conditions. Those characteristics will place a premium on design efficiency as well as cost effectiveness.

Those are serious risks, but the end result offers multiple options for the space program. On the one hand, it maintains a focus on the next generation by preserving the investments in new knowledge and innovative capacity. On the other, it preserves the practical skills and capability needed to engineer, build, and test operable launch vehicles and spacecraft.