2235 Talia Ave.
Santa Clara, CA 95050
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Dear Committee Members:

In the interest of full disclosure it should be noted that I work as a contractor at a NASA research center, assisting lunar scientists in their work. But I must be clear that I am making these comments as a private citizen and a concerned taxpayer, and not in any other capacity.

I strongly believe in the purpose and utility of the American space agency, and I remain convinced that human exploration of the Moon, Mars, and beyond is an endeavor that will bring back great riches in scientific knowledge, technical advancement, and an economic return on investment many times in excess of its cost.

However, the Constellation program as envisioned by the previous administration, and currently being executed at NASA suffers from some very serious flaws. In 2004 President Bush introduced his vision of a return to the Moon as a "stepping stone" to Mars (and beyond). However the plan currently in place can only be considered a rushed and improperly planned and budgeted return to the Moon with more thought given to impending deadlines than to how such a mission would benefit future exploration of Mars and beyond. Allow me to give a small number of concrete examples.

<u>The Moon: Testing astronauts' ability to live and work on low gravity surfaces</u>

The current plan is for a series of sortie missions to the Moon, followed by long series of short missions to build up a lunar base. This does not maximize the return on investment for a trip to Mars or beyond. Allow me to explain:

Current plans for a mission to Mars are based on assumptions about what effects Mars' 1/3 gravity will have on the human body. We know what effect Earth gravity has, and thanks to experiments on Skylab, Shuttle, Mir, and Space Station we know what effects there are in microgravity. Current mission planning is based on an interpolation of these results, and a few necessarily limited experiments done on Earth to estimate the effects on the human body of living in 1/3 gravity. But the truth is we have no reason to assume that our models for interpolation are at all based in reality, and experiments on Earth can only take us so far. Living and working in low (but not zero) gravity environments for extended periods of time may have unexpected consequences on the human body. We simply don't know. But this is where the Moon comes into the picture:

even though the Moon's gravity is half that of Mars, it will give us a chance to test and refine our models for interpolating the effects of gravity on the human body. This in turn will allow us to predict what will happen on a trip to the surface of Mars or beyond with a confidence that we are currently lacking.

Unfortunately the current mission plan, with an extended series of sortie and base-building flights, is not setup to provide the data described. In terms of exploration objectives, the sortie missions are planned to enable the selection of a base site. The large number of base-building missions is for the construction of a large (perhaps ISS-sized) permanent base. Although an opportunity for great engineering, this plan would neither give maximum scientific return on investment nor properly prepare us for a trip to Mars. If instead we refocus on a trip to Mars as our goal, while doing as much science as we can on the side, a different vision becomes clear.

The sortie missions can for the most part be done away with (except for one or two short-term technology demonstrators). If our interest is indeed in preparation for a trip to Mars, then there is really only one part of the Moon that is of interest: the polar regions. The poles have stable temperature regimes similar to Mars, abundant resources, interesting geography and points of scientific interest (especially the south polar region), and other areas of the lunar surface are simply too inhospitable to support anything more than short human missions in the near term. As originally planned the sortie missions had the stated purpose of investigating multiple regions to enable the selection of a permanent base site. But as of 2009, there is no question that the south polar region is the only region of interest, and with advances in robotic exploration (particularly LRO), we will have a short list of possible base sites in the next few years, before project Constellation even gets off the ground. One or two short sortie missions may still be required, as technology demonstrators, to verify our observations from orbit, and to investigate an exact landing site within the region of interest for longer duration missions. But we should certainly not expect the 6 sortie missions prior to base building, as is currently planned.

And secondly, the idea of a monumental base is not in line with the long-term goals of Mars and beyond. A large, permanent base will not be built on Mars anytime in the foreseeable future. A large, permanent base on the Moon will provide no science that couldn't also be done by a smaller, more mobile team of astronauts operating from a single habitat/laboratory unit and a pressurized long-distance lunar rover. Rather than a long series of short duration base-building flights, NASA should instead aim for multiple long duration science and exploration missions with maximal payoff. The lunar missions should resemble Martian missions: long duration stays on the surface (perhaps 3 months to a year) by a highly mobile team, with a new habitat/laboratory unit and pressurized vehicle sent as part of each mission, as needed. This will bring down costs, increase science, and better prepare us for a trip to Mars. If the habitat modules are landed in close proximity to each other, pressurized vehicles could be reused and in time a full permanent lunar base could still be developed. This latter model for lunar

exploration is purposefully quite similar to the current Mars design reference mission, and would therefore have the added benefit of testing our mission planning for eventual Mars missions.

In-Situ Resource Utilization: Moon/Mars on the cheap

The development of on-orbit liquid oxygen/methane propulsion (LOX/CH4) systems must take place to enable economical expansion of a Moon base, or exploration of Mars and beyond. This is due to the unique chemistry of LOX/CH4, which makes it relatively easy to extract components of one or both propellants from resources readily available on the Moon and Mars. On a long-term mission beyond Earth orbit, controlling mission weight is of primary importance. Not being required to take all that propellant with you is what frees up room for food and other consumables necessary to support long-duration human missions to the Moon, Mars, and beyond. If our long-term goal is truly to move beyond the Earth-Moon system, development of on-orbit LOX/CH4 rocket technology is an absolute requirement.

However in comparing LOX/CH4 to other propellants the "Exploration Systems Architecture Study" the LOX/CH4 combination had better characteristics and consistently out-performed the competition. Despite these advantages, it was recognized that development of LOX/CH4 technology might delay the CEV schedule, and it was labeled as a "risk" by the report. For perhaps that reason NASA has yet to decide if the CEV on-orbit propulsion system, and Altair lander will use LOX/CH4. However, to fund trips to Mars and beyond in a sustainable manor LOX/CH4 technology must be an absolute requirement.

If developing LOX/CH4 will delay a return to the Moon by a few years, then so be it. It is better to make that investment now than to sink money into a technology that will not enable us to do anything more than short duration missions within the Earth-Moon system. I understand, as I know this committee does, that the realities of politics and budgeting are such that years from now when that investment is absolutely needed to get us to Mars or beyond cheaply, the money will no longer be available. Development of such enabling technologies should take place now, while we have the time and political will to do so.

I offer LOX/CH4 propulsion systems as an example of necessary enabling technology for exploration of Mars and beyond that NASA has shelved or refused to commit to. I am confident that there are others as well. I urge this committee to recommend that NASA put together a list of such enabling technologies that need to be developed, and commit to moving forward on them. Progress must be made now so as to prevent delays in the future or budget overruns.

Ares: Rockets that should never have been

This is not the 1960's; there is today a fully developed commercial space industry. NASA should no longer be in the business of designing launch vehicles except under circumstances of absolute necessity.

I am referring specifically to the Ares-I rocket. The system is over budget, unsafe, and plagued by design problems. Furthermore, it provides no capability that existing commercial rockets do not. Boeing has informed your committee that they could man-rate the Delta IV for approximately $1 billion. Let me be clear: that's 1/40th the current estimated cost of developing the Ares-I launch vehicle. I'm sure that Lockheed-Martin and SpaceX could provide a similar bid for the Atlas V and Falcon 9.

For the Ares-V, there is no commercially available launch system that provides similar capability. However the Shuttle has all the necessary technologies to develop a non-reusable but cost effective system that is both safe and reliable. Your committee has already heard the proposal of Stephen Metschan of the Shuttle-derived Jupiter Launch Vehicle (DIRECT v3.0) and John Shannon's Shuttle-C proposal. I have nothing more to add except to urge that your committee seriously consider these proposals. They will free up large budgets, cut years of development off the schedule, and maintain much of the large Shuttle workforce and knowledgebase.

That's not to say that our investment (as taxpayers) in Ares should be thrown out. I would recommend that the project be reorganized into a spin-off company, and let the investment market decide if it still has commercial value. At the very least the company would surely be picked up by one of the large aerospace firms for the knowledge and proprietary technology, giving the taxpayers at least a minimal return on investment.

Near-Earth Objects: An opportunity, not a distraction

From the news reports I have read, it would seem that this committee is pushing the possibility of sending missions to near-earth objects (NEOs) as an alternative to a return to the Moon. I applaud the committee for bringing up the NEOs as a potential target for exploration, as they are an overlooked resource and there is much to be gained both scientifically and in preparation for a trip to Mars and beyond from a visit to such an object.

However, I would caution that a NEO trip does not substitute for a return to the Moon. On a trip to Mars there are two very large engineering and human factors unknowns: long duration space flight outside of Earth orbit, and long duration stays on the surface of a low-gravity object. A trip to a NEO would fill our knowledge of and test our capabilities of the first unknown (or more accurately, fill in the gaps of what we learned on ISS), but there is simply no substitute for a return to the Moon to address the second.

I strongly urge this commission to recommend that trips to NEOs be included alongside, but not replace the goal of a return to the moon as a stepping-stone to Mars and beyond. It can be done cheaply (relative to the whole cost of the lunar program), and the payoff is worth the investment, but only if we are committed to the other goals as well.

In terms of the science, there are things to be learnt from a NEO: their structure, and how to defend the Earth potential collisions, for example. But there would be no opportunity to learn some of the big-ticket items that NASA is charged by the public and by Congress with investigating. We will not find life on a near-earth asteroid, like we might on Mars. We will not learn the impact history of Earth--and therefore the early history of life--like we would on the Moon. It will not provide a radio shield allowing for deep exploration of the cosmos, like the far side of the Moon.

Let me be clear: **the "flexible path" is not a viable option for answering the big-ticket questions in science and exploration**. It would be making he worst compromise of Pareto's principle: 80% of the investment for 20% of the return.

I urge this committee to recommend one or more missions to a NEO, but to do so as part of a comprehensive exploration plan, and not in exclusion of other targets.

Conclusion: The vision is right, the implementation is wrong

I am proud to work alongside NASA employees in my day-to-day duties. I believe that this agency has the right vision in its long-term and big-picture goals. My only concern is whether we are doing the best things we can to reach these goals and fulfill the vision. I hope that the committee seriously considers the points that I have raised, and the relevant data that has come from other sources in your investigations. I urge the committee to keep the vision of "return to the Moon [and NEOs] as a stepping stone to Mars and beyond" in place as NASA primary focus for human space flight, but to make recommendations for modifying the current plan for implementing that vision.

We can do it; there's no question about that. But if we're going to do it, we might as well do it right, within budget, and with maximal science and exploration return.

Sincerely,

Mark Friedenbach