Re-Thinking Mars Sample Return

DRAFT Louis Friedman, 25 August 2014 **DRAFT**

Mars Sample Return (MSR) has been the holy grail of the planetary science program for at least 40 years. During that time there have been many mission studies, many scientific recommendations and many start and stop programs. The scientific rationale for MSR rests primarily on two factors:

1. Earth-based instruments will always be better than anything we can send to Mars – both because their technology will surpass the fixed, time-constrained decision we must make for spacecraft and because they will not be subject to severe mass, volume and power constraints.
2. There has been no known way to do precise age-dating of Martian samples on board a spacecraft.

The United States has never conducted a robotic planetary surface sample return mission. Russia did – from the Moon. But the U.S. bypassed the need for a robotic lunar sample return when it achieved the Apollo missions which brought back nearly 400 kg of rocks from six different missions to the lunar surface. The only other surface sample mission has been the Japanese Hayabusa mission to a very low gravity, non-planetary Near-Earth Asteroid. The U.S. is currently developing OSIRIS-Rex to a Near-Earth Asteroid for sample return. Sample return is a big job – and in the case of Mars which has the compounding engineering difficulties of coping with an atmosphere and much higher gravity than the Moon, the job gets even bigger. All credible proposals suggest multiple vehicles and launches will be required and the cost will be several billions of dollars.

One question that has not yet been addressed in any depth yet is whether the motivation for robotic MSR would decrease if a human Mars mission was likely in the near future. A human Mars mission will not be driven initially by science objectives, but it undoubtedly will return many more and larger samples from the Martian surface than a robotic mission, and (at least in this author's opinion) will have a much cleverer sample selection system. That is, of course because the human will be cleverer than any telerobotic or artificial intelligence system. We recognize that there is a possible downside – the human will be a contaminant, like much more so than a sterilized robotic system. My view is that this risk will be mitigated by the necessary robotic aides that the humans will have on the surface of Mars and the elaborate protocols they will be following (not unlike dealing with biohazards on Earth). Human mission planning has not been a major question yet because the time scales for planning robotic missions are usually 10-15 years from conception whereas for human Mars mission it is more like 25-35 years, at least.

But things are changing. In the past decade there have been three robotic surface rovers, one fixed lander and several orbiter missions gathering data at and on Mars. Many in-situ measurements have been made some of which have even led to the first absolute age dating of rocks on Mars. Habitable conditions are being investigated with higher and higher precision and the diversity of the Red Planet has raised great uncertainty about the old maxim that "give me one piece of Mars and I will tell you its whole history." The recent summary of the 8th International Mars Conference [reference] included this pithy conclusion, "much more data, much less understanding." It is likely that Mars characterization will be like that of Earth: from many places with many investigations and with many different missions. In-situ capabilities will be increasing significantly in the next two decades and we can expect to creep up on capabilities to do age dating, chemical and possibly biologic analysis approaching that which can now do only on Earth. We'll of course not surpass Earth based instruments in space – but the increasing in-situ capability coupled with increasing benefits of geographically diverse and widely spaced investigations are two factors that suggest re-thinking robotic Mars sample return.

Another factor is that human Mars exploration might actually be getting closer to realization. It's true that the wish might be fostering the thought, but in the 1990s and first decade of the 2000s it was anathema to discuss the human Mars goal at NASA (for a while it was even legislatively prohibited). The proposed lunar goal did not catch on and led to an unsustainable program. Now, it seems that the human to Mars goal is finally driving NASA's human space flight program. The President has explicitly endorsed it, and the NASA Administrator has publicly spoken of it as the purpose and direction of the human program. The goal was also recently endorsed by the National Research Council. It is true that there is no real (funded) commitment to it. Being wary of long-term budget commitments precludes that. It is not politically possible to explicitly begin a human to Mars program, but neither is it politically possible for the U.S. to quit human space flight. It has to continue and many, at least, now want it to have a destination goal. It is of course possible that human spaceflight will continue only in low Earth orbit as it has for the past 40 years, or that it will focus on doing over the Apollo Moon landing goal instead of pushing into deep space, but public and policy interest seems finally to recognize that Mars is next human space goal and that the rationale for human space flight depends more and more on investigating habitability on another planet. The questions of possible life – past, present or future, strongly dominate public interest in space exploration – extending from Mars, past the moons of the Jupiter and Saturn, and on even to planets around other stars. But of all those targets, only Mars is accessible to humans.

There are however two huge engineering barriers to overcome before we have the capability to even plan to send humans to Mars: long duration (multi-year) life support for humans and safe entry, descent and landing for a human crewed spaceship on Mars. These are not the only engineering issues, but they are surely the dominant ones. The first of the challenges requires bigger rockets and a large crew habitat for multi-year flights. The current SLS/Orion program has requirements for these in their future stage of development in the 2020s. So, too, is it the goal of SpaceX with its development of the Falcon 9 Heavy and Dragon capsule. It won't be until perhaps 2030 that we will have the necessary capabilities – but that is o.k., because we won't need them before then. Human space flight and Mars entry technology will also take that long to develop.

Mars entry technology is going to require bigger entry vehicles and new techniques to assure astronauts' safe landing and efficient delivery of large surface infrastructure to support them. Elon Musk has recognized this with his explicit requirement to have SpaceX's Dragon capsule capable of delivering robotic payloads to the surface). Looking at this requirement (for Dragon or for future other human Mars landing precursors) we realize that there will be opportunities for large science payloads on such precursors. In fact, it is probable that human Mars mission development will require a robotic Mars sample return precursor – not for science, but to test all the landing, ascent and in-situ infrastructure systems necessary for the human mission(s). This provides a third reason to reconsider the role of robotic Mars sample return in the science program of NASA, viz. that the human program will drive the engineering need, as well as the eventual science return.

This brings us full cycle to the most noteworthy aspect of all previous MSR studies in NASA: they have all been rejected as costing more than could be afforded in the science program. Some believe this reason will be overcome (in putative, but unlikely, better days) – but long and painful experience and evidence has been built up to suggest that cost constraint is intrinsic to any science proposed MSR mission. Consider that:

1. Robotic MSR is designed to meet budget expectations in whatever is the current NASA program, but then suffers increased cost demands of planetary protection (at Mars and then on Earth and other sample curatorial requirements. These add hundreds of millions, if not billions of dollars (depending on facility assumptions) the mission design estimate.
2. Simple "grab sample" objectives have been rejected as the understanding of Mars' diversity has grown and the need to avoid lander contamination has been understood.
3. Similarly no single sample return mission is deemed sufficient, that planning for MSR engenders a requirement for a program of MSR missions – a very

expensive single focus in the planetary program. Such a focus runs into resistance from programs competing for the same dollars, viz.:

a. Other scientific communities: physics, astronomy, Earth science,…
b. Other planetary communities: outer planets, small bodies…
c. Other Mars scientists: geophysics, atmosphere, polar region and other hazardous terrain investigators …

It is hard to imagine sufficient support generated solely within the science program for a MSR focus – it has never happened and current trends (including budgetary and policy resistance to so-called Flagship missions) point away from such a dominant goal – the James Webb Space Telescope is a counterexample, but it currently being cited in a negative context about future large, dominant projects.

Conversely, we have made the case above that growing development of the human Mars goal will support a robotic Mars sample return precursor – without a scientific requirement! This is a much lower cost MSR because it is the scientific requirements and the sample handling that drive up the cost. Presumably at this point (as was the case with Apollo) the scientific community will be happy with a number of in-situ Mars missions and the prospect of the human mission in the (then) near future. The robotic Mars science community,, e.g. the Mars Exploration Program Assessment Group, has, in the past few years recognized the tie between their goals and the eventual human mission.
This combination of increased in-situ capability, a demand for more diverse site investigation, unlikely budget support strictly within the science program for MSR and progress toward the human Mars landing goal leads to the suggestion that robotic Mars sample return may no longer be the holy grail and that the science program should focus on geographically diverse investigations leading to human exploration. This will also support the characterization of good candidate sites for human missions.

Some say that humans to Mars will always be rejected either by the fear of contaminating the pristine planet (or at least one that we cannot prove *a priori* is not pristine) or by the fear of contaminating the Earth with some pathological unknown Mars bug (the Andromeda strain). History suggests that the desire to explore will overcome these fears, but if not – they may be equal inhibitions to robotic sample return. Either way, it is time to rethink robotic MSR in the science program – perhaps leaving it for the humans. If so, we can start by re-planning Mars 2020, which Has a lot of resources and energy going into sample caching – caching of samples unlikely to ever be returned to Earth.