Human exploration of the Moon and Mars: *implications for Aurora*

Ian Crawford puts the case for sending people as well as robots to explore the Moon and Mars.

Abstract

In the near future, Europe will have to decide how to respond to the new US plans for human space exploration, and how far its existing Aurora programme is consistent with them. The UK will shortly have to make a decision on whether, and to what extent, to participate in these exciting developments. Here I argue that there is a strong scientific case for the human exploration of planetary surfaces, and that the robotic exploration of Mars, as currently envisaged by Aurora, should be pursued in parallel with the development of a human spaceflight infrastructure on the Moon. Such a strategy would pave the way for eventual human missions to Mars by the middle of the century. ESA (and within ESA, the UK) should aspire to be a major participant in such a programme.

These are exciting times for space exploration. We are currently witnessing a veritable renaissance in the robotic exploration of the solar system, as exemplified by the present flurry of activity on and around Mars, the fast approaching rendezvous of Cassini/Huygens with Saturn and Titan, the successful launches of Rosetta and SMART1, and the forthcoming missions to long-neglected Mercury. At the same time, there has been a re-examination of the exploratory potential of human spaceflight and, for the first time since Apollo, human missions beyond Earth orbit are being actively considered. To its great credit, the European Space Agency (ESA) led the way in November 2001 with the formal adoption of the Aurora programme, aimed at the robotic and human exploration of the solar system, with the ultimate aim of landing people on Mars by 2033 (ESA 2004). Then, in January 2004, the US administration announced a re-direction of NASA's human spaceflight activities away from Earth orbit and towards the Moon and Mars, with a manned return to the Moon, possibly as early as 2015.

In the midst of all this, the UK has to decide whether, and to what extent, to participate in these endeavours. As a consequence, an extensive, if not wholly transparent, decision-making process is underway. To my mind, there are two top-level strategic decisions that urgently need to be addressed as part of this exercise:

- To what extent is Aurora's current emphasis on Mars a sufficient foundation for a well-rounded programme of solar system exploration, and in particular to what extent should lunar exploration have a greater emphasis?
- To what extent is human spaceflight essential to the exploratory aspirations of Aurora, and as such deserves to be supported by all Aurora participants, including the UK?

The Apollo missions demonstrated that there are three primary scientific benefits of having astronauts operating on a planetary surface. First comes human versatility, especially the ability to make on-the-spot decisions and take advantage of serendipitous discoveries not foreseen in advance (e.g. Spudis 1992). Second, the opportunity to collect, and return to Earth more, and more diverse, rock and soil samples than is feasible with robotic probes (the Apollo haul was 382 kg, comprising more than 2000 discrete samples; nothing comparable has been, or is likely to be, achieved robotically). Third, the ability to carry a wider range, and a larger mass, of scientific equipment (e.g. active seismic experiments, heatflow instruments, magnetometers, gravimeters and, crucially, drilling equipment) to a planetary surface than is likely to be practical with robotic probes alone (e.g. Crawford 2003).

There are thus strong grounds for believing that the exploration of both the Moon and Mars would benefit from a human presence, and that the human component of Aurora can indeed be justified in terms of the overall exploratory goals of the programme. It follows as a corollary that life sciences research into the effects of the space environment on human physiology, necessary to underpin long-term human operations in space (e.g. White and Averner 2001), can also be justified by the goals of Aurora (while noting that these are also likely to yield additional benefits in terms of fundamental biological knowledge and practical medical applications here on Earth; Fong 2001).

Moon or Mars?

It is important to realize that the scientific cases for exploring the Moon and Mars are both very strong, but very different. The primary scientific importance of the Moon arises from its extremely ancient surface, which preserves a record of the early evolution of a terrestrial planet, and of the near-Earth cosmic environment in the first billion years or so of solar system history (Spudis 1996). This record is not likely to be preserved elsewhere and, from a fundamental planetary science perspective, this arguably makes the Moon a *more* important target than Mars. However, the strong scientific arguments for renewed human exploration of the Moon have been reviewed extensively elsewhere (e.g. ESA 1992; Spudis 1996, 2001; Crawford 2003, 2004), so I concentrate here on the scientific case for the human exploration of Mars, and examine how this might be linked to an earlier phase of activity of the Moon.

Broadly, the scientific case for the human exploration of Mars can be divided into two main, although not distinct, categories: the search for life, and geological/geophysical investigation of the martian environment. The strategies adopted in the "search for life" will further depend on whether such life is extant or extinct.

Life on Mars

If life presently exists near the surface of Mars, it is possible that chemical signatures of active metabolism could be detected by suitably instrumented robot spacecraft (e.g. Hiscox 2001, Bada 2001). The proposed Pasteur payload on Aurora's EXOMARS rover shows the kind of experiments that might be attempted (ESA 2003). On the other hand, the near surface environment of Mars is extremely hostile to life as we know it: very cold, highly oxidized, and exposed to solar ultraviolet radiation. For these reasons, if life does exist on Mars today it is most likely to be found underground, at depths of one or two kilometres, where geothermal heat should melt the base of a probable planet-wide cryosphere. Evidence of such a cryosphere is a target of the MARSIS instrument on Mars Express. This environment would include liquid water within the pore spaces of the rock, would be much warmer than the surface, and be completely protected from solar UV. We know that chemoautotrophic organisms can survive in similar environments on Earth – for example the SLiMEs (subsurface lithoautotrophic microbial ecosystems) found over a kilometre underground in the Columbia River Basalts (Stevens and McKinley 1995, Fredrickson and Onstott 1996). These organisms use H_2 (released by water reacting with iron-bearing minerals) as an electron donor, and dissolved CO_2 as a carbon source. They are independent of the surface and similar organisms could live in the martian crust.

It seems clear that discovering life in such deep environments will not be readily amenable to the kind of small-scale robotic vehicles currently envisaged for the search for life on Mars. Given that an operation capable of drilling to depths of over a kilometre beneath the surface will be required – which is how the terrestrial SLiMES have been discovered – this is the kind of large-scale exploratory activity which would, at the very least, be facilitated by a human presence, and which may be wholly impractical otherwise.

Even if there is no life on Mars today, there are good grounds for believing that it may have done 3.5–4.0 billion years ago when the surface seems to have been both warmer and wetter (e.g. Hiscox 2001, de Duve 1995). If such life is now extinct, as is perhaps most likely, the task will involve searching for fossil evidence, probably fossilized bacteria (Gould 1994). As fossils will have long since ceased to metabolize, they will not leave the kinds of chemical biosignatures that might reveal the presence of extant life, and this may make them very hard to find. Past life may have left a record in stable isotope ratios, especially $^{13}C/^{12}C$, which might be detected robotically if suitable carbon-bearing organic material exists in the immediate vicinity of a landing site. However, the controversy surrounding the interpretation of such ratios in ancient rocks on Earth (e.g. van Zuilen *et al.* 2002) means that any such detection is unlikely to be definitive. The oldest (recently controversial) microfossils on Earth are 3.5 billion years old, and have been isolated from rocks of that age found in Western Australia (Schopf 1993). However, these specimens were not, and could not have been, identified by parachuting a robotic vehicle into Western Australia. Rather, it relied on decades of careful geological fieldwork, and the patient sifting through large quantities of carefully collected material with microscopes.

It is likely that the search for microfossils on Mars will have to proceed in a similar way, which is not readily amenable to robotic exploration (Hiscox 2001). Rather, the search will involve the microscopic analysis of such a large quantity of material, from so many different sites, that only studies by human specialists may be practical. The recent controversies that have sprung up concerning the oldest terrestrial microfossils (Brasier *et al.* 2002) illustrate how difficult it would be to interpret data obtained robotically. Or, to put it another way, if, after a few years of searching near-surface rocks at a handful of discrete locations, rovers such as EXOMARS fail to find convincing evidence for fossil life on Mars, how convinced will we be that it's not there to be found?

Moreover, if evidence for past life is found, that will mark the beginning, not the end, of the new field of martian palaeontology (Gould 1994). The subsequent demand for samples, and supporting geological and environmental studies, may outstrip the capabilities of robotic exploration (just how many tonnes of material can realistically be collected robotically and sent to Earth for analysis?).

Martian geology

While the search for past or present life is probably the most important scientific question to be addressed on Mars, the geological study of the planet has its own intrinsic scientific interest (e.g. Kallenbach *et al.* 2001). Many of the detailed scientific arguments for using humans as field geologists on the Moon (e.g. Spudis 1992, Crawford 2004) apply equally to Mars. Indeed, to the extent that martian geological history has been much more complicated than that of the Moon, we might expect human exploration to be even more desirable. To reinforce this point, consider the statement by Mike Malin and Ken Edgett, principal investigators for the Mars Orbital Camera on the Mars Global Surveyor spacecraft (quoted by Sawyer 2001):

"We are constantly aggravated by the fact that all the questions we have about Mars could be answered… if we could just walk around on the planet for a few days… It's unusual to hear people like us argue for manned space exploration. But for about two years now [we] have been absolutely convinced that we're going to have to send people there."

Given that two of the world's leading practitioners in the robotic exploration of Mars have been driven to this conclusion, I'm prepared to rest the geological case.

Moon, Mars and Aurora

Given the strong scientific case for a human return to the Moon, and the equally strong, but different, scientific reasons for wanting to send people to Mars, it makes sense to combine the two in some self-consistent strategy for solar system exploration. Given that the Moon will be easier and cheaper to get to, my own view is that ESA would be better concentrating its human spaceflight activities over the next 25 years on the Moon rather than on Mars, presumably as a partner in an international programme arising from the renewed US focus on lunar exploration. This would help pave the way for future human Mars missions as envisaged by Aurora, and of course the robotic exploration of Mars could, and should, continue in parallel with the development of a human spaceflight infrastructure on the Moon.

Apart from anything else, without learning a great deal more about the response of human physiology to long-term exposure to reduced gravitational, and enhanced radiation, environments, we will not be in a position responsibly to send people to Mars, despite the scientific benefits outlined above. In addition, there is still a great deal to learn about the martian environment before we could commit ourselves to such a project. Not least is whether, despite all the odds against, the near surface of the planet actually contains an indigenous biosphere; if it does, this would radically alter the terms of the discussion, scientifically and ethically! There are thus probably several decades of worthwhile robotic exploration ahead before sending people to Mars is likely to be necessary.

By first building up a human spaceflight infrastructure on the Moon, and pursuing a robotic programme of Mars exploration in parallel, there may be a realistic chance that, sometime before mid-century, the former will have developed the human spaceflight expertise, and the latter the detailed knowledge of the martian environment, to make human missions to Mars both scientifically worthwhile and technically feasible. ●

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