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The astrobiological case for renewed robotic and human exploration of the Moon

I.A. Crawford

School of Earth Sciences, Birkbeck College, Malet Street, London WC1E 7HX
e-mail: i.crawford@ucl.ac.uk

Abstract: An ambitious programme of lunar exploration will reveal much of astrobiological interest. Examples include: (i) better characterization of the impact cratering rate in the Earth–Moon system, with implications for understanding the possible 'impact frustration' of the origin of life; (ii) preservation of ancient meteorites blasted off Earth, Mars and Venus, which may preserve evidence of the early surface environments of these planets, as well as constraining models of lithopanspermia; (iii) preservation of samples of the Earth's early atmosphere not otherwise available; (iv) preservation of cometary volatiles and organics in permanently shadowed polar craters, which would help elucidate the importance of these sources in 'seeding' the terrestrial planets with pre-biotic materials; and (v) possible preservation of extraterrestrial artefacts on the lunar surface, which may permit limits to be placed on the prevalence of technological civilizations in the Galaxy. Much of this valuable information is likely to be buried below the present surface (e.g. in palaeoregolith deposits) and will require a considerable amount of geological fieldwork to retrieve. This would be greatly facilitated by a renewed human presence on the Moon, and may be wholly impractical otherwise. In the longer term, such lunar operations would pave the way for the human exploration of Mars, which may also be expected to yield astrobiological discoveries not otherwise obtainable.

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Introduction

Lunar exploration is undergoing something of a renaissance at present, driven in part by the declared intention by the United States to resume human exploration of the Moon (NASA 2004). Although the main scientific reasons for exploring the Moon relate to fundamental planetary science questions not directly related to astrobiology (e.g. its origin, internal structure and early geological evolution), it will be argued here that an ambitious programme of lunar exploration also promises to reveal much of astrobiological interest.

In order to develop this argument it is first necessary to examine what we mean by astrobiology. By any reckoning, astrobiology includes many different scientific disciplines, and as such is liable to a range of interpretations and definitions. The author's preferred definition, which has been adopted for an introductory university course in astrobiology, and which is offered here as a suggestion to the wider community, is as follows:

> '*Astrobiology is the study of the astronomical and planetary context within which life on Earth has evolved, and the implications of this context for the nature and prevalence of life elsewhere in the Universe*.'

This definition emphasizes the cosmic context within which life has evolved on Earth, reflecting the fact that Earth is the only planet in the entire Universe on which we actually know that life has taken root. It follows that, when and if they are discovered, the detailed scientific study of extraterrestrial organisms will not fall within the purview of astrobiology as here defined, but will form the subject matter of a new sub-discipline of biology (exobiology?), where, by definition, the study of living things properly belongs (e.g. Mayr 1998). Rather, the new science of astrobiology is concerned with understanding the cosmic context of the origin and evolution of life. It follows that astrobiology must contain a strong exploratory streak, for we shall never arrive at a proper understanding of the cosmic context of anything by staying put on the Earth. Knoll (2003; p. 235) has put it well: '*an astrobiology that truly meets the challenge of its name cannot remain Earthbound. It requires exploration, and the entire universe beckons ….*'

It is in this context that lunar exploration may prove to be of great value for astrobiology, because the Moon's ancient surface preserves a record of conditions in the Earth–Moon system during the first billion years of Solar System history that Earth itself has largely lost. In addition, the outer layers of the lunar crust may have continued to record astrobiologically important information right up to the present. The remainder of this paper will outline some specific examples of the importance of the lunar record for astrobiology. These range from the relatively pedestrian, but scientifically hugely important, calibration of the impact cratering rate, to the highly speculative search for extra-terrestrial artefacts.

Characterization of the impact cratering rate in the Earth–Moon system

Impact cratering is a fundamental planetary process, an understanding of which is essential for our knowledge of planetary evolution in general, and the role of impacts in Earth history in particular. From an astrobiology perspective, characterization of the impact rate in the Earth–Moon system between 4.5 and 3.5 Gyr ago (the period of 'heavy bombardment') is especially important because it defines the impact regime under which life on Earth became established. A better understanding of this regime is desirable for at least three reasons:

- it will enable us to quantify the ferocity and duration of the heavy bombardment period, and thus the extent to which the origin of life may have been 'frustrated' by giant impacts (e.g. Maher & Stevenson 1988; Sleep *et al.* 1989; Ryder 2003);
- it will enable us to assess the rates of delivery of cometary and meteoritic volatiles and organics to early Earth (e.g. Chyba & Sagan 1992; Pierazzo & Chyba 1999); and
- it will enable us to better constrain fluxes of meteorites exchanged between terrestrial planets in the early Solar System, which is required for models of lithopanspermia (e.g. Mileikowsky *et al.* 2000; Burchell 2004).

Although the Earth itself has lost any record of the impact flux to which it was subjected early in its history, this record is still preserved on the Moon. Our current knowledge of the lunar cratering rate is based on the calibration of crater density with surface age made possible by the radiometric dating of Apollo samples. However, while this calibration is indeed one of the major scientific legacies of the Apollo programme, it is neither as complete nor as reliable as it is often made out to be. Indeed, there is still uncertainty over whether the cratering rate has declined monotonically since the formation of the Moon, or whether there was a bombardment 'cataclysm' between about 3.8 and 4.0 billion years ago characterized by an unusually high rate of impacts (Hartmann *et al.* 2000). The answer to this question will profoundly influence our conception of the impact regime under which life appeared on our planet, and the kinds of early terrestrial environments where it may have taken root.

It is certainly possible to arrive at a definitive knowledge of the cratering rate in the Earth–Moon system during its first thousand million years, but this will require the collection, and radiometric dating, of a much greater range of lunar samples, taken from areas with a wider range of surface ages, than was achieved by the Apollo missions. In principle, this could be achieved with a number of robotic sample return missions (such as the Russian Luna 16, 20 and 24 missions) sent to well-chosen localities, but it would be greatly facilitated by renewed human operations on the lunar surface (e.g. Crawford 2004a; Stern 2005).

Enhanced understanding of the biological consequences of hypervelocity impacts

The apparent correspondence between the sharp decline in the impact rate 3.8 billion years ago and the emergence of life on Earth was highlighted by Maher & Stevenson (1988), who considered the degree to which impacts may have 'frustrated' the development of life. It seems safe to assume that liquid water must have been present on Earth for life to have evolved (or to have gained a foothold here if it arrived from somewhere else), and recent evidence from detrital zircon grains indicates that this requirement may have been met as early as 4.4 Gyr ago (Wilde *et al.* 2001). A key part of the impact frustration argument concerns the stability of oceans during the heavy bombardment, given that a basin-forming impact has sufficient energy to boil away at least the uppermost, 'photic', zones of such oceans (Sleep *et al.* 1989). However, it is now recognized that chemoautotrophic organisms can survive several kilometres below the Earth's surface (e.g. Stevens & McKinley 1995; Fisk *et al.* 2003), and the 'impact frustration' argument needs to be extended to assess the degree to which such crustal environments may have been protected from giant impacts (Willis *et al.* 2005). Only then will we have a clear idea of how early in Solar System history terrestrial planets may have become habitable. This in turn will require a better understanding of the volume of planetary crusts that were subjected to impact-generated shock pressures greater than the survival threshold of any micro-organisms which may have been present, and the typical time between successive passages of such shocks at different depths within the crust.

In addition, to understanding the impact environment on the early Earth, the case of Mars is also of interest. If evidence for life is found on Mars, it will be important to determine whether or not it is indigenous, or whether it arrived there from elsewhere. Several studies (e.g. Mileikowsky *et al.* 2000) have shown that bacterial spores could in principle have been transferred between Earth and Mars by meteorites in the early Solar System. As a smaller planet, likely to have accreted earlier, and not having experienced a large late-stage impact such as that thought to be responsible for our Moon, it is possible that Mars may have possessed a habitable near-surface environment several hundred million years before the Earth did (indeed, the existence of a thick, stable, crust on Mars as early as 4.5 Gyr ago is demonstrated by the existence of ALH 84001, an igneous Martian meteorite of this age; Jagoutz *et al.* (1994)). We therefore have to be open to the possibility that Mars may have seeded Earth, and not the other way around (e.g. Davies 2003). However, if the heavy bombardment had the same duration throughout the inner Solar System, the sterilization of the Martian crust by impact-generated shock waves may have nullified this temporal advantage. Ultimately, assessing the timing and consequences of the heavy bombardment on Mars will require geological field studies on Mars itself. However, this is not an option for the early Earth, and in any case there are strong reasons for thinking that the basic understanding of impact cratering processes underpinning such work would benefit greatly from lunar exploration.

Currently, our knowledge of impact processes is based on a combination of theoretical modelling, small-scale laboratory hyper-velocity impact experiments and field geological studies of generally poorly-preserved terrestrial impact craters (Melosh 1989). The Moon provides a unique record of essentially pristine impact structures of all sizes (from micrometre-sized pits up to basins thousands of kilometres wide). Field studies, combining sample collection (including drill cores) and *in situ* geophysical studies (e.g. active seismic profiling) of the ejecta blankets and sub-floor structures of lunar craters, and of the shattered outer layers of the lunar crust itself (the so-called 'megaregolith'), would greatly aid in our understanding of the habitability of planetary crusts during the heavy bombardment. Even more than the calibration of the impact rate discussed above, however, these more sophisticated field studies would be greatly facilitated by a human presence.

Preservation of materials from early Earth, Mars and Venus

In an important paper, Armstrong *et al.* (2002) suggested that the Moon might have collected meteorites that were blasted off the other terrestrial planets in the first billion years of the Solar System's history. Such samples may preserve evidence of the early surface environments of those planets that, at least in the case of the early Earth and Venus, may not be preserved anywhere else. The recovery of such material would provide a hugely important window into the early history of the Solar System, including possible information on the nature and prevalence of early life that is unlikely to be obtained in any other way. As Armstrong *et al.* (2002; p. 184) put it:

> '*Mars is presently the focus of attention with regard to the search for early signs of life outside Earth. Ironically, the Moon may be the better place to search for the remains of both early martian and early terran life.*'

This may overstate the case somewhat with respect to Mars, as the best place to study ancient Mars must surely be Mars itself. The high crater densities on the Noachian terrains of the southern hemisphere, and the existence of ALH 84001, indicate that the planet still retains significant quantities of its earliest rocks – if we want to study ancient Mars we will have to go to Mars. However, with regards to ancient Earth (i.e. before about 3.8 Gyr ago), and early (and possibly pre-greenhouse) Venus, Armstrong *et al.* (2002) may well be correct in identifying the Moon as the last best hope. Moreover, by its very nature, finding this material on the Moon would provide a direct record of the rate at which material as been transferred between the terrestrial planets throughout Solar System history, and thus greatly help in constraining models of lithopanspermia (e.g. Mileikowsky *et al.* 2000).

In order to assess the potential value of the lunar record in this respect it is necessary to estimate the quantity of terrestrial (and other terrestrial planet) material expected to have landed on the Moon, demonstrate that some fraction of it will have survived the impact and consider how we might identify such exotic materials on or below the lunar surface. Armstrong *et al.* (2002) estimate that between 10^7 and several times 10^8 kg km^{-2} of terrestrial materials must have landed on the Moon during the heavy bombardment between 3.8 and 3.9 Gyr ago, corresponding to an equivalent layer of between several millimetres and several centimetres thick. As noted by Armstrong *et al.* (2002), most of this material will be buried, probably to depths of several hundred metres, but the continual gardening of the regolith by meteorite impacts will ensure that some fraction of it is exposed at the lunar surface at any given time. For a well-mixed regolith, Armstrong *et al.* (2002) estimate that 200 kg km^{-2} of terrestrial material, mostly dating from 3.8 to 3.9 Gyr, should be exposed at the surface at the present time. The corresponding figures for martian and venusian material are 1.8 and at most 0.3 kg km^{-2}, respectively.

As the Moon has no atmosphere to decelerate impacting meteorites it is necessary to consider how much, if any, of this material will survive impact with the lunar surface in any recognizable form. Armstrong *et al.* (2002) estimate a maximum impact velocity of about 5 km s^{-1}; the minimum impact velocity would be 2.4 km s^{-1} (i.e. the Moon's escape velocity). In addition to its dependence on the impact velocity, the severity of the shock to which an impacting meteorite is exposed depends on the angle of impact, with oblique impactors being less severely shocked than those which strike the surface at high angles (Pierazzo & Melosh 2000). In addition, the unconsolidated nature of the lunar regolith may further enhance the survivability of impacting meteorites. Based on these considerations, Armstrong *et al.* (2002) concluded that '*the likelihood of terran ejecta surviving in some large aggregate sample is quite high*', and our own work using the AUTODYN hydrodynamic modelling code supports this conclusion (some background to our work with AUTODYN is given by Baldwin *et al.* (2005); the lunar impact results will be presented elsewhere). As noted by Armstrong *et al.* (2002), with regard to the survival of biomarkers in this material we may expect to find a range from those likely to be preserved in any intact meteorite (e.g. isotope ratios and organic carbon) to those which may only be preserved in the least shocked, and thus rarest, specimens (e.g. complex molecular fossils and/or actual microfossils). It is hoped to address these issues of biomarker survivability in future work.

Of course, locating terrestrial samples on the lunar surface will be challenging, but they are likely to have unique spectral properties that would cause them to stand out from the surrounding lunar materials. For example, hydrated silicates exhibit a strong absorption band at ~3 μm, and weaker bands at 1.4 and 1.9 µm, due to O—H stretching and H—O—H bending modes (e.g. Gaffey *et al.* 1993; Bibring *et al.* 2005). Similarly, carbonates exhibit a characteristic series of sharp, narrow absorption features in the spectral region between 2 and 3 µm (Gaffey *et al.* 1993). Lunar rocks are wholly devoid of water and carbonates (e.g. Papike *et al.* 1998), so any rocks found on the Moon with these spectral signatures would be good candidates for terrestrial meteorites. In this respect we may draw a parallel with the now routine collection of meteorites in Antarctica – just as there are places in Antarctica where any rock found on the surface is likely to be a meteorite, we can imagine scanning the lunar surface with infrared eyes sensitive to hydrated silicates such that every spot detected stands a good chance of being a terrestrial (or martian) meteorite and thus deserving of more detailed analysis. It is certainly possible to design a suitable infrared imaging system that could survey hundreds of square kilometres quite quickly, and thus efficiently identify candidates despite their expected rarity.

However, as already alluded to, the bulk of terrestrial (and other terrestrial planet) material on the Moon is likely to be buried, making location and access much more difficult. In this respect, locating layers of *palaeoregoliths*, possibly tens or hundreds of metres below the present surface, are of particular interest. A regolith will be formed when a fresh basaltic surface is exposed for millions of years to the flux of micrometeorites that constantly impinge on the lunar surface. Mare basaltic volcanism continued from at least as early as 4.2 Gyr ago to perhaps as recently as 1.1 Gyr (e.g. Wilhelms 1987; Hiesinger *et al.* 2003), and we may therefore expect palaeoregolith layers to be sandwiched between lava flows within this age range. It is within just such palaeoregolith layers that the best preserved (and most easily dated) terrestrial meteorites are likely to be found, although identifying them is likely to entail extensive geological fieldwork (Crawford 2004a). Indeed, the desirability of having human explorers back on the Moon to locate and process ancient terrestrial materials was explicitly recognized by Armstrong *et al.* (2002; p. 194):

> '*For a complete analysis of this material we suggest the best way to conduct these studies is on-site measurements by human observers – in essence, a return to the Moon.*'

We will return to this point later in this paper.

Preservation of samples of the Earth's early atmosphere

Ozima *et al.* (2005) have reported the presence of nitrogen and noble gases implanted in lunar soils, which they interpret as having been stripped off the Earth's atmosphere by the Solar wind at times when the geomagnetic field was weak or absent. If this discovery is confirmed, it raises the possibility that the (nearside) lunar regolith may preserve samples of Earth's very early atmosphere and a record of its changing composition (e.g. the build up of biogenic oxygen) through geological time. It is also possible that this record will provide information on the evolution of the Earth's magnetic field, which is also of astrobiological relevance. As with the terrestrial meteorites discussed above, any such record is likely to be best preserved in palaeoregolith layers below the present surface, and will thus require a drilling programme to gain access to it. The value of any such deposits as a record of Earth history will be greatly enhanced by the fact that their ages will be well constrained by the radiometric dating of basaltic lava flows above and below the layers of palaeoregolith.

Astrobiological implications of polar ices

As is well known, the *Lunar Prospector* neutron spectrometer found evidence for enhanced concentrations of hydrogen at the lunar poles, which has been widely interpreted as indicating the presence of water ice in the floors of permanently shadowed polar craters (Feldman *et al.* 1998). This potentially very important result is still awaiting confirmation, but if water ice is present it is most likely derived from the impacts of comets with the lunar surface. Of course, the water in impacting comets will be vapourized by the impact itself, but water molecules finding themselves in a shadowed crater at temperatures of ~90 K will re-condense and freeze. Thus, over millions of years, substantial deposits of water ice might accumulate, presumably mixed in with the uppermost few metres of regolith. While the original cometary volatiles will have been considerably reworked by impact vaporization, migration to the poles and subsequent condensation, it remains possible that some information concerning the importance of comets in 'seeding' the terrestrial planets with volatiles and pre-biotic organic materials (e.g. Chyba & Sagan 1992; Pierazzo & Chyba 1999) may be preserved. Clearly, a proper assessment of the astrobiological relevance of these cometary volatile deposits will require a thorough *in situ* study, followed by returning any ice samples that may be found to Earth for more detailed analysis.

Furthermore, as pointed out by Lucey (2000), lunar polar ice deposits may be of considerable astrobiological interest even if they do not preserve any vestigial information concerning their cometary sources. This is because any such ices will have been continuously subject to irradiation by galactic cosmic rays and, as such, may be expected to undergo 'Urey–Miller-like' organic synthesis reactions. Exactly analogous reactions may be important for producing organic molecules in the icy mantles of interstellar dust grains, and on the surfaces of outer Solar System satellites and comets. The lunar poles are much more accessible than any of these localities, causing Lucey to note that:

> '*If organics are present in the lunar poles, they represent an opportunity to field-test models of organic synthesis which are proposed for comets and interstellar clouds which in turn are proposed to have been important in providing organic material to the early Earth. No other location in the Solar System is so (relatively) convenient for this type of study.*'

Possible preservation of extraterrestrial artefacts

Among the most exciting, if somewhat speculative, astrobiological applications of lunar exploration would be the search for extraterrestrial artefacts on the Moon. Such a search, whether successful or not, could help constrain estimates of the prevalence of technological civilizations in the Galaxy (for a discussion of some of the issues involved see Crawford (1997, 2000)). Broadly speaking, there are two possible ways that extraterrestrial artefacts might end up on the Moon – they could have been deliberately placed there (as envisaged by Clarke (1951, 1968)), or they could have been arrived in an uncontrolled way as space debris drifting in from the interstellar medium (as suggested by Arkhipov (1996, 1998)).

With regard to the former possibility, Rose and Wright (2004) have drawn attention to the relative efficiency of 'inscribed matter' as a means of interstellar communication (provided only that speed is not considered essential!). If extraterrestrial civilizations are, or have been, common in the Galaxy, it is not necessarily implausible to assume that some of them would have long ago identified our Solar System as being of interest (perhaps by using *Darwin*-type instruments (e.g. Fridlund 2000) to determine that life existed on the third planet) and chosen to send information-bearing artefacts in our direction. However, it seems certain that the senders would have known that placing them on geologically dead worlds, such as the Moon, would better ensure their long-term survival than sending them to more biologically interesting, but geologically active, planets such as the Earth. This is the principal reason for keeping an open mind to the possibility of finding such artefacts on the Moon, although the probability of success will depend sensitively on how many technological civilizations there are, and how often the Solar System has passed close to one (say to within a few tens of parsecs) during its twenty-odd revolutions around the Galaxy.

The suggestion by Arkhipov (1996, 1998) that microscopic artefacts produced by extraterrestrial civilizations may be found on the Moon is rather different. It does not assume that alien artefacts were ever deliberately directed here, but relates to the fact that any space-faring civilization, even if it never leaves its own planetary system, cannot avoid producing a large amount of space debris. Indeed, after only 50 years in space, and very few operations beyond Earth orbit, we have ourselves already generated copious amounts of debris (UN 1999), and a true space-faring civilization (e.g. one engaged in mining asteroids for raw materials, or building space-based solar power stations) would generate orders of magnitude more. The smaller parts of this debris (e.g. sub-micrometre-sized specks of unusual alloys eroded from the interiors of rocket engines) will eventually be pushed out into interstellar space by radiation pressure from the parent star. Larger objects, such as nuts and bolts, and even abandoned spacecraft, will eventually be expelled through gravitational sling shots past giant planets. Clearly, the more space-faring civilizations that have existed over the history of the Galaxy the greater will be the interstellar density of such debris, and the greater the concentration swept up by ancient, exposed, planetary surfaces such as that of the Moon. Thus, it is conceivable that a careful search for (generally micrometre-sized) fragments of exotic materials in the lunar regolith (and in buried palaeoregoliths) could help constrain the number of technological civilizations in the Galaxy, even if none of them have ever been here.

As noted by Rose and Wright (2004), these considerations '*suggest that carefully searching our own planetary backyard may be as likely to reveal evidence of extraterrestrial civilizations as studying distant stars through telescopes.*' Although, for reasons given elsewhere (e.g. Crawford (1997, 2000)), the author personally thinks that extraterrestrial technological civilizations are probably very rare in the Galaxy, and the likelihood of finding artefacts in our own Solar System is low, it would be good to keep an open mind to the possibility in the context of future lunar exploration – if we do not look we will never find!

Preparing for Mars

While, as it has been argued here, the Moon has much to teach us that is directly relevant to astrobiology, it seems certain that it has never actually had any indigenous life of its own. In this respect, one could make the case that Mars is a more interesting target for astrobiological investigation. However, the exploration of the Moon and Mars should not be seen as mutually exclusive, but rather as complementary. Not only will lunar exploration yield insights into early Solar System processes that will inform our understanding of Mars (as discussed above), but human operations on the Moon will pave the way for the eventual, and far more challenging, human exploration of Mars. While there undoubtedly remains much essential reconnaissance work to be performed robotically on Mars, in the longer term there are very strong reasons for believing that a human presence will be required if we are ever to obtain a meaningful answer to the question of whether life ever existed, or still exists, on the planet (e.g. Boston 1999; Hiscox 2001; White & Averner 2001; Cockell 2004; Crawford 2004b; Clancy *et al.* 2005). Moreover, if evidence for past or present life *is* found, that will mark the beginning, not the end, of the new field of Martian biology (and/or palaeontology, as appropriate), and the subsequent demand for follow-up investigations is likely to soon outstrip the capabilities of purely robotic exploration.

Despite these scientific advantages, however, there is still much to learn about human physiological and psychological responses to long-term immersion in the space environment (including reduced, but non-zero, gravity fields, enhanced radiation levels and confinement in small spaces) before we will be in a position to send people safely to Mars (e.g. Freeman 2000; White & Averner 2001). Much of this knowledge can be gained relatively safely by working on the Moon, where rapid evacuation to Earth is possible in an emergency. Moreover, many of the operational techniques that will be required for exploring Mars (e.g. field geology using suited astronauts and pressurized and/or unpressurized rovers, and the development of planetary drilling technologies) would most logically be pioneered on the Moon first, where they will be required to address the scientific issues discussed earlier in this paper. There is also undoubtedly still a great deal to learn about Mars (not least whether or not an indigenous biosphere exists at or near the present surface, which would alter the terms of the discussion fundamentally), before we could responsibly commit ourselves to sending people there.

For all these reasons there are probably several decades of robotic Mars exploration ahead of us before sending people to the planet is likely to be either necessary or practical. However, as discussed by Crawford (2004b), by first building up a human spaceflight infrastructure on the Moon, and pursuing a robotic programme of Mars exploration in parallel, there is a realistic chance that, sometime before the mid-century, we will have developed the human spaceflight expertise, and the detailed knowledge of the martian environment, to make human missions to Mars both scientifically worthwhile and technically feasible.

Conclusion

It has been argued that the lunar geological record will preserve a diverse range of information directly relevant to astrobiology. Not only is much of this hugely valuable material unlikely to be preserved anywhere else, but it is relatively accessible to us – only three days away using space technology that we have had for 30 years. Some of this material (e.g. samples required for calibrating the cratering rate or for studies of polar ice deposits) could in principle be obtained by a series of carefully targeted robotic landers and/or sample return missions. However, locating the most exciting material, for example meteorites derived from the early Earth, will require a considerable amount of geological fieldwork on the lunar surface. Moreover, much of the lunar record of interest to astrobiology will be buried below the present surface, for example in palaeoregolith deposits, and drilling (perhaps to $\sim$100 m depths) will be required to gain access to it. This kind of large-scale exploratory activity would be greatly facilitated by a human presence on the Moon, and much of it may be wholly impractical otherwise (e.g. Spudis 1996, 2001; Crawford 2004a; Garvin 2004; Stern 2005). Thus, in addition to the more mainstream areas of lunar geology and planetary science, the new science of astrobiology will also have much to gain from the renewed human exploration of the Moon that is now in prospect. In the longer term, lunar operations will pave the way for the human exploration of Mars, which (as discussed by, e.g., Boston (1999); Hiscox (2001); Cockell (2004)) is also likely to result in astrobiological discoveries not otherwise obtainable.

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