THE SCIENTIFIC RATIONALE FOR RENEWED HUMAN EXPLORATION OF THE MOON

A White Paper submitted to the Planetary Sciences Decadal Survey 2013-2022

by

Dr Ian A. Crawford
Reader in Planetary Science and Astrobiology
Department of Earth and Planetary Sciences, Birkbeck College London
Malet Street, London, WC1E 7HX, UK
Email: i.crawford@ucl.ac.uk

Co-Authors (listed alphabetically)

Dr Mahesh Anand, Department of Earth and Environmental Sciences, Open University, UK
Professor Mark Burchell, School of Physical Sciences, University of Kent, UK
Dr James Carpenter, Directorate of Human Spaceflight, ESA- ESTEC, The Netherlands
Dr. Barbara Cohen, NASA Marshall Space Flight Center, USA
Leon Croukamp, Council for Geoscience, South Africa
Andrew Daga, Department of Space Studies, University of North Dakota, USA
Professor Hilary Downes, Department of Earth and Planetary Sciences, Birkbeck College, UK
Dr Sarah Fagents, Hawai'i Institute of Geophysics and Planetology, University of Hawai'i, USA
Dr Terence Hackwill, Department of Earth Sciences, University College London, UK
James N Head, Raytheon Company, USA
Dr Essam Heggy, Institut de Physique du Globe de Paris, France
Dr Adrian Jones, Department of Earth Sciences, University College London, UK
Dr Katherine Joy, Department of Earth and Planetary Sciences, Birkbeck College London, UK
Professor Christian Koeberl, Center for Earth Sciences, University of Vienna, Austria
Dr Philippe Lognonné, Institut de Physique du Globe de Paris, France
Dr Clive Neal, Dept. Civil Engineering & Geological Sciences, University of Notre Dame, USA
Dr Noah Petro, NASA Goddard Space Flight Center, USA
Professor Sara Russell, Natural History Museum, London, UK
Joshua Snape, Department of Earth Sciences, University College London, UK
Professor Larry Taylor, Dept. Earth & Planetary Sciences, University of Tennessee, USA
Dr Allan Treiman, Lunar and Planetary Institute, Houston, USA
Shoshana Weider, Department of Earth and Planetary Sciences, Birkbeck College London, UK
Dr Mark Wieczorek, Institut de Physique du Globe de Paris, France
Professor Lionel Wilson, Planetary Science Research Group, Lancaster University, UK

1. Background

The primary scientific importance of the Moon arises from the fact that it has an extremely ancient surface (mostly older than 3.5 billion years, with some areas extending almost all the way back to the origin of the Moon 4.5 billion years ago). It therefore preserves a record of the early geological evolution of a terrestrial planet, which more complicated planets (such as Earth, Venus and Mars) have long lost. Moreover, the Moon's outer layers also preserve a record of the environment in the inner Solar System (e.g. meteorite flux, interplanetary dust density, solar wind flux and composition, galactic cosmic ray flux) from billions of years ago [1-3]. In addition to its astronomical and planetary science importance, the lunar geological record is also of astrobiological significance, as it provides clues to conditions on the early Earth under which life first became established on our planet, and may even preserve samples of the Earth's early crust and atmosphere not otherwise obtainable (refs. [4-6]; see also the White Paper submitted by Anbar et al.).

The top level scientific rationale for continued lunar exploration is set out most recently and authoritatively in the National Research Council's 2007 "Report on the Scientific Context for Exploration of the Moon" ([1], hereinafter the 'NRC Report'). It is our opinion that this key document should form the cornerstone of the Decadal Survey's considerations of how lunar science fits into an overall strategy for Solar System exploration in the decade 2013-2022. Given the comprehensive nature of the NRC Report, there seems to be little point in 'reinventing the wheel' when it comes to defining lunar science objectives. Rather, mindful of the fact that US (and indeed wider international) space exploration policies are currently being reviewed, and that future policy decisions may look to the Decadal Survey for scientific support, here we wish to reiterate the specific scientific benefits of returning humans to the lunar surface. By implication, many of these benefits would also extend to the human exploration of Mars, and perhaps elsewhere, although we do not address those wider issues here.

We note that this Decadal Survey is specifically charged with considering the added scientific value of human space activities under Section C(5) of its Statement of Task (see http://sites.nationalacademies.org/SSB/CurrentProjects/ssb_052412).

2. Benefits of human exploration

Although some of the top-level lunar science objectives identified by the NRC Report can undoubtedly be met by suitably implemented robotic missions (for example the emplacement of geophysical networks to probe the interior; [7], see also the White Paper submitted by Neal et al.), most would be greatly facilitated by a human presence, and some may be wholly impractical otherwise. In our view, renewed human lunar exploration would have the following scientific advantages over robotic missions:

(1) More intelligent and efficient collection of samples from a more diverse range of localities, and over wider geographical areas, than is practical robotically.

Increased sampling, and subsequent geochemical analysis, is central to most of the top-level science objectives identified by the NRC Report (e.g. 'Science Concepts' #1, #3, #4, #5, #6 and #7; see Table 3.1 of [1]). The Apollo experience (especially when compared with the *Luna* robotic sample return missions and the *in situ* analyses performed by the Mars Exploration Rovers) indicates that astronauts, when suitably equipped with the means of surface mobility, are very efficient at this task. Indeed, one of the major, but often unspoken, benefits of human planetary exploration is that, because the astronauts have to return to Earth anyway, a large quantity of geological samples can be returned with them. For this reason alone, it may be doubted whether sufficient *in situ* analyses and/or sample return capacity to achieve all the objectives identified by the NRC Report will be realised in the absence of a human return to the Moon.

(2) Facilitation of landing, operating, and maintaining more massive and complex geophysical and other scientific equipment than is likely to be feasible robotically. Examples include, but are not limited to: (a) long-range surface rovers and drilling equipment in support of Item (1) above; (b) next generation (i.e. post-ILN) geophysical instruments (e.g. cryogenic geophysical sensors, long-baseline laser strain meters, and/or local geophysical networks); and (c) equipment for manipulating and characterising the geotechnical properties of the lunar regolith (of possible importance for long-term human habitation and future economic utilisation of the Moon).

There are two key supporting points to make under this heading:

(i) Because human missions, by definition, have to land a lot of mass on planetary surfaces, the additional marginal cost of landing massive or bulky scientific equipment is relatively modest (as the range of equipment deployed by the Apollo missions clearly demonstrated [2]); and

(ii) Human beings are uniquely capable of maintaining and 'troubleshooting' problems with complex equipment at risk of failure (of which the repair and upgrade missions to the Hubble Space Telescope furnish perhaps the best examples [8]).

(3) Facilitation of large-scale exploratory activities such as may be required to locate and sample important but rare and/or buried geological materials. Examples include, but are not limited to, possible mantle outcrops, buried lava flows, impact melt sheets, ancient regolith layers (see below), and possible 'exotic' materials derived from the Earth and other terrestrial planets.

(4) Increased opportunities for serendipitous discoveries – human beings are unique in their ability to recognize new observations or phenomena to be of importance, even if not anticipated in advance.

(5) Facilitation of a number of other, non-planetary, science activities on the Moon such as (i) life sciences investigations under reduced gravity and enhanced radiation conditions [9], and (ii) maintenance and upgrading of astronomical instruments placed on the lunar surface [see the White Paper by Burns et al.].

(6) Gaining operational experience on a planetary surface that will be of value for later exploration of Mars (from which similar scientific benefits as those outlined above may ultimately be expected).

Rather than offer an expanded justification of all these arguments here (which may be found in the published literature [e.g. 10-12]; see also the White Paper on this topic submitted by Harrison Schmitt), in what follows we illustrate the potential scientific advantages of renewed human lunar exploration with two specific examples (in the knowledge that other examples could easily be found).

3. Examples of valuable lunar science facilitated by a human return to the Moon

3.1 Exploring the basaltic lava flows of Oceanus Procellarum

Northern Oceanus Procellarum consists of a patchwork of discrete lava flows with estimated individual ages ranging from about 3.5 to 1.2 Gyr. [13,14]. This is a far greater range of ages than basalt samples collected by the Apollo missions (which occupy the narrow age range 3.8 to 3.1 Gyr). Collecting samples from a number of these different lava flows, and returning them to Earth for radiometric dating and geochemical analysis, would address the following three scientific objectives (see reference [15] for further discussion):

- Better calibration of the lunar cratering rate for the last 3 billion years, with clear benefits to the dating of planetary surfaces throughout the Solar System [NRC Report Science Goals 1c, 1d, and 1e; see also White Paper submitted by Bottke et al.].
- Better understanding of the geochemical evolution of the lunar mantle to more recent times than possible using the Apollo samples [NRC Report Science Goals 2b, 3b, 5a, 5b, and 5d] ; and
- A search for ancient buried regolith ('palaeoregolith') deposits, sandwiched between stratigraphically distinct lava flows within the age range 3.5 to 1.2 Gyr. Such buried regoliths may contain a record of the solar wind, galactic cosmic rays, and, more speculatively, samples of the Earth's atmosphere and crust, from these early but (given a sample return capability) easily dateable times [NRC Science Goals 7a, 7c and 7d; see also reference [15] for a more detailed discussion].

Taken together, this would be a very rich scientific harvest, but it does have certain implications for the exploration architecture. It may be doubted whether a project such as this is practical robotically – just to sample the different lava flows would require multiple landings and sample return from several (perhaps half a dozen) sites several hundred km apart (or, perhaps less practically, a long range robotic rover with sample caching capabilities, a central sample depository, and a robotic lunar ascent stage with the capacity for returning samples collected from many different localities). Moreover, when it comes to identifying and accessing palaeoregolith layers trapped between lava flows, which may require a ~tens of metres drilling capability (especially if undisturbed regolith is to be recovered along with samples of the under- and over-lying lava flows), the feasibility of a robotic implementation looks even less plausible.

On the other hand, such a project would lend itself to a human sortie-class expedition, such as would be facilitated by an exploration programme on the scale originally envisaged for the Vision for Space Exploration. In order to support scientific investigations such as this the exploration architecture would have to support:

- Adequate provision for sample collection and return capacity (roughly estimated at several hundred kg per sortie).
- Provision for surface mobility – in the specific case of the Procellarum basalt flows mapped by [14] a range of order 250 km would permit access to a number of different units with a wide range of ages. This implies use of a pressurized rover.
- Provision of the means to detect and sample palaeoregolith deposits. For detection, ground penetrating radar would be a suitable technique (see discussion in [16,17]). For access, unless suitable outcrops can be found at the boundaries between flows, provision of a drilling capability (perhaps to ~100m depths) may be required. This in turn implies the need for storage and transport of the drill cores.

3.2 The exploration of the South Pole-Aitken Basin

The South Pole-Aitken (SPA) Basin is the largest known impact structure in the Solar System and is a high priority for lunar exploration (see the White Paper by Petro & Jolliff). There are several reasons for this:

(i) Because SPA is the oldest identified lunar impact basin, obtaining an accurate age for it is crucial to constraining the bombardment history of the Moon [NRC Report Science Goal 1(b); see also White Paper by Bottke et al.];

(ii) The great depth of the Basin (up to ~ 12 km), and the inferred much greater depth of the SPA transient cavity, means that the basin floor may expose lower crustal, or even lunar mantle, outcrops not otherwise accessible [NRC Report Science Goal 3(c)];

(iii) As the largest known basin in the Solar System, an improved knowledge of its structure (including the thickness and differentiation of its impact melt deposits) is important for improving our knowledge of impact processes at the largest scale [NRC Report Science Goals 6(a) and 6(b)];

(iv) The SPA Basin includes the lunar south pole and adjacent high southern latitudes which, as noted by the NRC Report [Science Concept #4] “are special environments that may bear witness to the volatile flux [in the inner Solar System] over the latter part of Solar System history”; and

(v) Finally, the SPA Basin also contains many younger basins, small maria, cryptomaria, and pyroclastic deposits of great interest in themselves.

All these reasons make the SPA a compelling target for exploration. However, the large size of the basin (diameter ~2500 km), together with its great age (which implies that many key geological materials will be buried beneath and/or mixed with the ejecta of later impacts), means that it will be difficult, and perhaps impossible, to adequately address all these questions with one (or even several) small-scale robotic missions. For example, determining the age of the SPA will require the unambiguous identification of samples of SPA impact melt, and their return to Earth for radiometric dating. Widespread sampling (informed by orbital remote sensing data) would help distinguish these samples from impact melts produced by younger basins within the SPA, the individual ages of which will also be of interest. Moreover, localities selected for sampling impact melt deposits will not be the same, and may be several hundred km distant from, those required for other studies (e.g, putative mantle outcrops, polar ice deposits, and the structural geology of the basin).

For these reasons, although there is a strong argument for a preliminary robotic precursor mission (which would help refine objectives for later missions), a full exploration of the SPA Basin will require *in situ* analyses and sample return from multiple localities. It is possible to imagine this being achieved with a sufficiently large-scale robotic exploration programme involving multiple landers, but not by a single robotic sample return mission to just one particular locality within the basin. On the other hand, and as for the exploration of the Procellarum basalts discussed in Section 3.1, many of these objectives could be addressed simultaneously by a human sortie class expedition equipped with the means for surface mobility (with a range of several hundred km), sub-surface geophysical sensing instruments, sub-surface drilling capability, and adequate sample return capacity (which, to reiterate, will always be greater for a human mission than for a robotic one). Some of these scientific objectives of human exploration within the SPA Basin would benefit from the establishment of a permanent human outpost at or close to the lunar south pole, as recently elaborated by Clark et al. [18].

4. Conclusions

Science is not, and is never likely to be, the sole motivation for human space activities. Nevertheless, as argued here, planetary science stands to be a major *beneficiary* of human space exploration, especially as regards the geological exploration of the Moon and Mars. Given that the time frame covered by this Decadal Survey (2013-2022) is likely to include major decisions and investments in the human spaceflight area, both in the US and internationally [19], it is important that the Survey's deliberations take the *scientific* benefits of human space exploration into account when recommending a balanced future programme of Solar System exploration.

References

[1] National Research Council (2007), *The Scientific Context for Exploration of the Moon*.

[2] Heiken, G.H., et al., eds. (1991), *The Lunar Sourcebook*, Cambridge University Press.

[3] Jolliff, B.L., et al., eds. (2006), New Views of the Moon, *Rev. Min. Geochem*., 60, 1-704.

[4] Armstrong, J.C., et al. (2002), Rummaging through Earth's attic for remains of ancient life, *Icarus*, 160, 183-196.

[5] Crawford, I.A. (2006), The astrobiological case for renewed human and robotic exploration of the Moon, *Internat. J. Astrobiol.*, 5, 191-197.

[6] Ozima, M., et al. (2008), Toward understanding early Earth evolution: Prescription for approach from terrestrial noble gas and light element records in lunar soils, *PNAS*, 105, 17654-17658.

[7] Neal, C.R., **(**2006), On the lookout for quakes on the Moon, *Geotimes*, 51 (July)**,** 30-35.

[8] National Research Council (2005), *Assessment of Options for Extending the Life of the Hubble Space Telescope*.

[9] Gronstal, A., et al., (2007), Lunar astrobiology: a review and suggested laboratory equipment, Astrobiology, 7, 767-782.

[10] Spudis, P.D. (2001), The case for renewed human exploration of the Moon, *Earth, Moon & Planets*, 87, 159-171.

[11] Crawford, I.A., (2001), The scientific case for a human spaceflight infrastructure, *Earth, Moon & Planets*, 87, 221-231.

[12] Crawford, I.A. (2004), The scientific case for renewed human activities on the Moon, *Space Policy* 20, 91-97.

[13] Wilhelms, D.E. (1987), *The Geologic History of the Moon*, USGS Prof. Paper 1348.

[14] Hiesinger, H., et al. (2003), Ages and stratigraphy of mare basalts in Oceanus Procellarum, Mare Nubium, Mare Cognitum, and Mare Insularum, *JGR*, 108, E7, 1-27.

[15] Crawford, I.A., Fagents, S.A., Joy, K.H. (2007), Full moon exploration: valuable (non-polar) lunar science facilitated by a return to the Moon, *Astron. Geophys*., 48, 3.18-3.21.

[16] Heggy, E., et al. (2009), Potential of probing the lunar regolith using rover-mounted ground penetrating radar: Moses Lake dune field analog study, 40^{th} Lunar and Planetary Science Conference, Abstract #2183

[17] Ono, T., et al. (2009), Lunar radar sounder observations of subsurface layers under the nearside maria of the Moon**,** *Science*, 323, 900-905.

[18] Clark, P.E., et al. (2009), Lunar field exploration scenarios for a south pole outpost, 40^{th} Lunar and Planetary Science Conference, Abstract #1135.

[19] Worms, J.-C., et al. (2009), ESSC-ESF position paper on a science-driven scenario for space exploration: Report from the European Space Sciences Committee, *Astrobiology*, 9, 23-41.