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The Scientific Case for Renewed Human Activities on the Moon

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Abstract

It is over thirty years since the last human being stood on the lunar surface, and I will argue that this long hiatus in human exploration has been to the detriment of lunar and planetary science. The primary scientific importance of the Moon lies in the record it preserves 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. This record may not be preserved anywhere else, and I will argue that gaining proper access to it will require a human presence. Moreover, while this will primarily be a task for the geosciences, I will argue that the astronomical and biological sciences would also benefit from a renewed human presence on the Moon, and especially from the establishment of a permanently occupied scientific outpost.

1. Introduction

It is now over thirty years since the last human being stood on the lunar surface. While the primary motivations for Apollo were of course geopolitical, the scientific legacy of the programme was enormous [1,2]. Indeed, it would probably be no exaggeration to claim that much of contemporary planetary science is built on the Apollo legacy – even today one can scarcely attend a scientific meeting on planetary science without seeing geochemical and isotopic analyses of Apollo samples presented in one context or another. Yet Apollo, quite literally, only scraped the surface of the Moon, and there is undoubtedly a great deal more to learn.

While science is only one of several drivers for human space exploration, and the social, industrial and political benefits are often at least as important as the scientific [3,4], there is little doubt that a return to the Moon would vastly enhance our knowledge of the Universe and our place within it. In what follows, I summarise the main scientific areas that would benefit from a return to the Moon, and especially the establishment of a permanently occupied scientific outpost. Several earlier studies [e.g. 5-9] have also considered this issue, and interested readers might like to refer to them.

2. Lunar Geoscience

The primary scientific importance of the Moon arises from the fact that it has an extremely ancient surface (mostly older than 3 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) billions of years ago. With the possible exception of the much less accessible surface of Mercury, this record has probably not been preserved anywhere else in the Solar System.

I will argue here that accessing this potentially huge scientific archive will require extended human activities on the lunar surface. Key scientific objectives to be addressed are:

2.1 Identification and sampling of palaeoregoliths

One of the many new pieces of information resulting from the Apollo missions was that solar wind, and galactic cosmic ray particles, are efficiently implanted in the lunar regolith [10]. A regolith is formed when a fresh surface is exposed for millions of years to the flux of micrometeorites which constantly impinges on the lunar surface. Apollo also taught us that mare basaltic volcanism continued on the Moon from about 4.2 billion years ago (and perhaps earlier) to at least as recently as 3.1 billion years ago. Thus we may expect to find layers of *palaeoregoliths*, sandwiched between basalt flows of a range of different, but very ancient, ages [11]. These may therefore be expected to contain records of the solar wind from billions of years ago, providing a unique test of models of solar evolution. The potential value of this record was stressed in the conclusions of Wieler *et al*. [10]:

> *"Our results reinforce the unique importance of the lunar regolith for solar physics; not only does it enable us to analyse solar species that are too rare to be detected in situ with present-day instruments, but it also conserves a record of the ancient Sun not otherwise available."*

A similar statement could of course be made regarding the preservation of galactic cosmic rays, and the history of galactic evolution [11].

Moreover, it has recently been suggested [12] that the Moon may have collected meteorites blasted off other terrestrial planets (including the early-Earth and the pre-greenhouse Venus). If such material has been preserved on the Moon, it is more likely to be preserved in layers of palaeoregolith than exposed on the present surface. The recovery of such material would provide a hugely important window into the history and early evolution of the Solar System which could not be obtained in any other way.

However, identifying palaeoregoliths, which may only rarely, if at all, be exposed on the surface, will require considerable geological fieldwork. This may require local seismic profiling, and the ability to extract core samples from depths of hundreds of metres. Such complex geological exploration is better suited to human specialists in the field than to robotic exploration, and may be wholly impractical otherwise. It would be ideally suited to geologists operating from a permanently occupied lunar base, without the strict time constraints which so curtailed the possibilities of geological field exploration during the Apollo project.

2.2 Calibration of the lunar cratering rate

The vast majority of lunar terrains have never been sampled, and their estimated ages are based on the observed density of impact craters. The current calibration of the cratering rate, used to covert crater densities to absolute ages, is based on the Apollo sample collection. However, it is neither as complete nor as reliable as it is often made out to be. For example, consider the age assigned to Copernicus, a prominent nearside impact crater which defines a key stratigraphic horizon in lunar geology. The age of Copernicus is usually put at 810 million years [13]. However, no Apollo mission actually visited Copernicus, and the age comes from a light grey layer found just below the surface at the Apollo 12 landing site (over 300 km to the south), and interpreted as a ray of Copernicus ejecta. Several assumptions underlie this interpretation: the deposit may not actually be from Copernicus at all, and, even if it is, the age obtained from it may not represent that of the Copernicus impact. Clearly, this is an unsatisfactory basis for dating a key event in lunar history.

The age of Copernicus, however, is only symptomatic of the task before us. Many other lunar surfaces and features are also lacking accurate dates, and Copernicus ranks as only seventh in the priority list compiled by Wilhelms [13]. Indeed, there is still uncertainty over whether the lunar 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 characterised by an unusually high rate of impacts.

A better calibration of the cratering rate would be of great value for planetary science for the following reasons:

(i) It would provide better estimates for the ages of unsampled regions of the lunar surface;
(ii) It would yield a more reliable estimate of the impact history of the inner Solar System, especially that of the Earth at a time when life was evolving on our planet; and
(iii) The lunar impact rate is used, with various assumptions, to date the surfaces of other planets for which samples have not been obtained – to the extent that the lunar rate remains unreliable, so do the age estimates of surfaces on the other terrestrial planets.

The truth is that the collection, and radiometric dating, of a much greater range of samples, taken from areas with a wide range of crater densities, will be required to arrive at a truly reliable lunar impact cratering rate. It seems clear that this activity would require a considerable amount of geological fieldwork, which would benefit greatly from the infrastructural support offered by a permanently occupied lunar scientific outpost.

2.3 Sampling a representative range of lunar lithologies

Essentially our whole knowledge of lunar petrogenesis, and thus the origin and evolution of the lunar crust, has come from the geochemical and mineralogical examination of the Apollo samples. However, it is now recognized that the Apollo samples are not representative of the lunar crust as a whole, being heavily biased by the peculiar lithologies (the so-called Procellarum KREEP Terrain) which surround the Imbrium Basin on the west-central nearside [14]. Study of lunar meteorites, which have come from random, but unknown, locations on the lunar surface, and spacecraft remote sensing data, further reinforce the conclusion that Apollo did not sample anything approaching the full range of lunar rock types.

Additional samples are urgently required from the polar regions (especially the floor of the giant South-Pole Aitken basin, which may have penetrated the lunar mantle [14]), and from the unsampled lunar farside. Only once such samples are collected will it be possible to arrive at a consistent model of the evolution of the lunar crust, which can then inform models for the early evolution of other, more complex, terrestrial planets.

It is especially important to obtain samples from undisturbed lunar bedrock, rather than from samples which happen to be lying around in the upper surface of the regolith. No Apollo samples were obtained from bedrock units, and, of necessity given Apollo's operational constraints, the vast majority were collected from the uppermost surface of the regolith and thus lack a known geological context. Some samples, collected from the blocky ejecta of small impact craters, and from the rim of Hadley Rille at the Apollo 15 site, can probably be assigned to particular mapped geological units, but this is not true of most of the collection.

2.4 Enhanced understanding of impact cratering mechanics

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. Yet 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 [15]. The Moon provides a unique record of essentially pristine impact craters of all sizes (from micron-sized pits up to the 900 km diameter Orientale Basin). 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 pristine lunar craters of a range of sizes would greatly aid in our understanding of the impact cratering process. Infrastructural support for such, necessarily time-intensive, field work could most naturally be provided by a permanently occupied scientific outpost.

2.5 Establishing a comprehensive lunar seismic network

Seismology is our most powerful geophysical technique for studying the deep interiors of terrestrial planets. The Apollo seismometers remained active for up to eight years, and did provide useful information on the structure of the lunar crust and upper mantle (see [16] for a review). However, the deep interior of the Moon was only very loosely constrained by the Apollo seismology – even the existence, never mind the physical state and composition, of a lunar core is uncertain.

The main problem was that the Apollo seismometers were deployed in a geographically limited triangular network (between Apollos 12/14, 15 and 16) on the nearside. As a consequence, seismic waves capable of probing the deep interior had to originate close to the centre of the farside. Indeed, the tentative seismic evidence for a lunar core arises from the analysis of just one farside meteorite impact that was sufficiently strong to be detected by more than one nearside Apollo seismic station in eight years of operation.

This is clearly an unsatisfactory state of affairs, and there is a pressing need for a much more widely-spaced network of lunar seismic stations, including stations at high latitudes and on the farside. There is also a case for more aggressive active seismology, with the detonation of artificial explosions sufficiently powerful to probe the deep interior, rather than having to rely on rare and geographically random meteorite impacts, and on very weak natural moonquakes.

2.6 Other geophysical investigations

There are many other geological and geophysical investigations waiting to be performed on the Moon. These would all aid in the characterisation of lunar structure and evolution, and would be greatly facilitated by a permanent human presence. They include:

- Accurate determination of the lunar heat flow. The two successful Apollo measurements (Apollos 15 and 17) were both on the nearside and in mare (as opposed to highland) geological units. Moreover, they were based on measurements within the top two meters of the regolith, rather than in solid bedrock. There is a pressing need to extend these measurements to constrain models of lunar thermal evolution.
- Local seismic profiling and gravity measurements to constrain the thickness of the basin-filling mare flows. A successful demonstration of the value of these techniques was performed at the Apollo 17 site [17]. They now need to be extended into the interiors of the maria in order to arrive at an accurate estimate of the total volume of mare basalt erupted onto the surface (which then feeds back into models of the evolution of the lunar mantle).
- Deep (several km) drilling of boreholes into the lunar crust to: (i) determine the number and ages of mare flows currently buried beneath the surface; (ii) identify palaeoregolith layers (see Section 2.1); (iii) search for buried ‘cryptomaria’ in the highlands; (iv) search for, date, and chemically characterise basin impact melt deposits, and any pre-mare volcanic materials, buried by later mare deposits; and (v) provide calibration and ‘ground truth’ for active seismic profiling surveys.
- Perform geomagnetic studies of lunar rocks to understand the origin of the surface remanent magnetisation discovered by Apollo (i.e. was there an early core dynamo, or have these fields been induced by impact?). It is especially important to perform measurements on easily dated, *in situ*, mare basalt bedrock (rather than, as for Apollo, blocks excavated by impact craters) as these will preserve the *orientation* of the palaeomagnetic field.
- Perform *in situ* geophysical investigations of the mysterious lunar magnetic anomalies such as Reiner Gamma [18] in order to understand their origin.

There are many more such examples, but the above list is sufficient to demonstrate the strength of the geological case.

2.7 The geological 'bottom line'

From the above it will be clear that studies of lunar geology and evolution would benefit greatly from a renewed human presence on the Moon. Some of this work, such as the identification and characterisation of palaeoregolith layers, and the drilling of km-deep boreholes, probably absolutely requires a human presence. Some of the other tasks, such as sample collection, and the deployment of seismometers and magnetometers, could in principle be performed robotically. Even so, there are grounds for thinking that humans would be desirable even for these seemingly simpler tasks. Collecting samples for dating and geochemical analysis is seldom achieved by collecting rocks at random – it is necessary to discriminate between the material of interest and other materials (e.g. ejecta from distant impacts) which may be littering a landing site, and this makes the activity better suited to an experienced human field geologist than a robot probe [7].

We must also consider the sheer quantity of material which will need collecting and analysing – probably scores, perhaps hundreds, of sites will have to be visited. Just working out the geology of an area as complicated as the South-Pole Aitken basin, with its many superimposed craters, basins, and small maria, will require many individual sample collection sites. It must be doubted whether an undertaking on this scale could be performed using robot vehicles alone.

It is therefore clear that lunar science would be a major beneficiary of a renewed human presence on the Moon, and that this would *facilitate* scientific studies which would not otherwise occur. Once the infrastructural support provided by a lunar base is in place, opportunities for the wide-ranging collection of rock samples would arise naturally, as would opportunities for the deployment of scientific instruments (seismometers, magnetometers, gravimeters….) that may not occur otherwise. Moreover, human specialists are more likely to make serendipitous discoveries not anticipated in advance. As a final point, we may note that, given the presence of qualified personnel and their equipment actually on the Moon, only a fraction of the intrinsically heavy rock samples may need to be transported to Earth for analysis.

3. Observational astronomy

The Moon is a potentially valuable site for astronomical observation [6,19]. The lunar farside, in particular, is probably the best site in the inner Solar System for radio astronomy, as it is continuously shielded from the Earth. The lunar surface also lends itself well to cosmic ray astronomy (as it lies outside the Earth's magnetosphere) and other astronomies requiring large, bulky detectors (e.g. gamma-ray astronomy). For optical and infrared astronomy there is an argument that the second Sun-Earth Lagrange point (L2) offers a better location than the Moon. However, while L2 may indeed by required for some specialised instruments, we should not forget that the Moon remains a very good astronomical site for all wavelengths, certainly better than the surface of the Earth, or even Earth orbit.

In this context, it is interesting to note that, in his recent testimony to the US senate hearing on lunar exploration (6 November 2003), the leading telescope designer Roger Angel noted that the lunar environment, and especially the lunar south pole, offers significant advantages for certain types of large optical telescopes. To quote:

> *"In conclusion, based on astronomical goals and telescope engineering constraints, the lunar [south] pole deserves to be taken seriously as an observatory site for large cryogenic telescopes….."* [20]

Thus, even if the scientific case for a return to the Moon is primarily based on geo- and life-science objectives which can only be done on the Moon, we may still expect astronomy to benefit.

In particular, the maintenance and upgrading of astronomical instruments will benefit from proximity to a human infrastructure. This, after all, has been one of the major lessons of our experience operating the Hubble Space Telescope which, as Angel put it "has the huge, proven advantage of astronaut access" [20]. Something similar can now be seen with the International Space Station (ISS) – although many astronomers opposed construction of the ISS, now that it exists as a piece of infrastructure they are beginning to suggest astronomical uses for it [21]. Thus, once a lunar base is established, the Moon may become a *more* attractive astronomical location than either LEO or L2, precisely because a human-tended infrastructure will exist to transport, service, and upgrade the instruments.

4 Life Sciences

There exists a wide range of life sciences research which would benefit from the establishment of a permanently occupied scientific outpost on the Moon:

4.1 Fundamental biological research

Space life science research embraces the whole spectrum of studies from molecular biology to whole-body physiology [22]. Indeed, it is now realised that many of the physiological responses of organisms to the space environment are modulated at the cellular and sub-cellular levels. Gravity, in particular, appears able to affect cellular function at a molecular level, influencing such fundamental cellular processes as signal transduction and gene expression [22, 23].

While there is a growing body of knowledge of these processes in microgravity, the biological effects of reduced, but non-zero, gravity are largely unknown. For example, it is not known whether reduced gravity causes the same biological changes as zero gravity, only more slowly, or whether some, or all, such processes have gravity thresholds which must be passed before they occur. Long term studies in a reduced gravitational environment are required to quantify these effects, and a permanently occupied lunar base would be ideally suited to this task. Moreover, the unique radiation environment of the Moon would also provide many opportunities for fundamental research in the field of radiation biology [6].

4.2 Human physiology and medicine

There is particular interest in the long-term effects of reduced gravity on the human body. It is of special importance is to establish potential gravity thresholds for different body functions. This research is needed partly to enhance our understanding of fundamental biological processes, with potential feedback into the design of medical therapies for use on Earth [22], but also to support of future human space operations. In particular, before it will be possible to safely send astronauts to Mars (see below) much research into the long-term health of a human crew operating under reduced gravity, and after a long period in microgravity, will be required. A lunar base, perhaps in combination with microgravity research on the ISS, is probably the only location where such research could be safely conducted.

4.3 Artificial ecosystems

Long-term future human space operations will require increased reliance on ecologically closed life support systems. They will be particularly important for long-duration spaceflights, and as means of reducing reliance of orbital, lunar and martian outposts on supplies from Earth. Moreover, construction and operation of such closed ecosystems may help inform our understanding of the operation of the terrestrial biosphere.

The key technologies involved will, of necessity, be biological, and an understanding of how the component biological systems function in reduced gravity, and under different radiation environments, will be required. It will be especially important to have the opportunity to *gradually* reduce reliance of the artificial ecosystem on external supply, and to have the capacity to intervene safely if and as necessary. All these considerations suggest the Moon as an appropriate location to initiate experiments in closed ecosystem design.

5 The Moon as a test-bed for Mars exploration

There are grounds for believing that a full exploration of the planet Mars, and in particular obtaining of a meaningful answer to the question of whether life ever evolved there, will ultimately require astronauts operating on its surface [7, 24]. This case has been made most eloquently by Mike Malin and Ken Edgett, principal investigator and lead geologist, respectively, for the Mars Orbital Camera on board the Mars Global Surveyor spacecraft [25]:

> "*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…. For about two years now [we] have been absolutely convinced that we're going to have to send people there.*"

However, given our current state of knowledge and expertise, sending people to Mars is a hugely ambitious objective. Much will have to be learned about human adaptability to the space environment, and the long-term operation of human outposts on hostile planetary surfaces, before we will be in a position to safely send a human expedition to Mars. To some extent, the necessary research on human physiology and psychology can be conducted on the ISS, and this is indeed one of its major long-term scientific benefits. However, learning to construct and operate an International Moon Base would help pioneer the technical and operational expertise that will be required for eventual human operations on Mars. Long-term experience of human physiology in a reduced, but non-zero, gravitational environment will be particularly important (see above).

6 Economic exploitation of the Moon

It is at least possible that the Moon contains natural resources of potential economic value to human civilisation. In principle, such resources could be of value to the terrestrial world economy, or to future space operations, or both.

Much previous work on lunar resource exploitation has centred on the possible use ^{3}He in the lunar regolith as a potential fuel for future nuclear fusion reactors [26,27]. However, the concentration of ^{3}He in the regolith samples returned by Apollo is very low (about 4 parts per billion [28]) and it is far from clear whether significant exploitation could ever be economic. On the other hand, we currently have very little information on ^{3}He concentration with depth (the deepest Apollo regolith core, at the Apollo 17 site, was only 3 meters [1]), and for all we know greater concentrations might occur in currently unsampled areas. Thus, there *is* a good case for obtaining a better inventory of lunar ^{3}He, and for implementing a pilot ^{3}He extraction scheme on the Moon to assess its possible long-term value. Moreover, any scheme designed to extract ^{3}He from the lunar regolith would also yield many other solar wind-implanted volatiles of possible economic benefit [28].

The extent to which other economically exploitable mineral deposits may exist on the Moon is currently unknown. As the Moon is apparently wholly lacking in water (apart from possible non-endogenous polar ice deposits), the hydrothermal concentration of economically important minerals, which is important for ore formation on Earth, cannot have occurred. However, there has been a lot of molten rock on the Moon in the past (e.g. an original 'magma ocean', and several later episodes of partial melting to produce a range of intrusive and extrusive igneous rocks [1]. Gravitational settling of crystals within melts can in principle concentrate economically important minerals, and the very low viscosity of lunar basaltic melts is expected to enhance the efficiency of this process [29,30]. To quote from Papike *et al*. [29]:

> "*It is therefore possible that layered ore deposits similar to or even larger than those on Earth may occur on the Moon.*"

Like so much else of lunar geology, we won't have an answer to this until we have conducted much more thorough geological surveys than anything attempted to date. Thus, there seems to be a strong case for establishing a human presence on the Moon so that its long-term economic potential can at least be properly assessed.

7 Conclusions

The 30 year hiatus in lunar surface exploration since Apollo has been to the detriment of lunar and planetary science. Very considerable scientific advantages would follow from a return, and especially from the construction of a permanently occupied scientific outpost. While planetary science may be expected to be the major beneficiary, significant advantages can also be identified for the life and astronomical sciences. Moreover, moon base operations would naturally support longer term aspirations to send human beings to Mars later in the century.

The economic potential of the Moon is less easily quantified at this stage, but it seems clear that we will never know if the Moon is an economically important asset for human society unless we go back and explore it in greater detail than hitherto. Such exploration would be ideally suited to a permanently occupied lunar base.

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