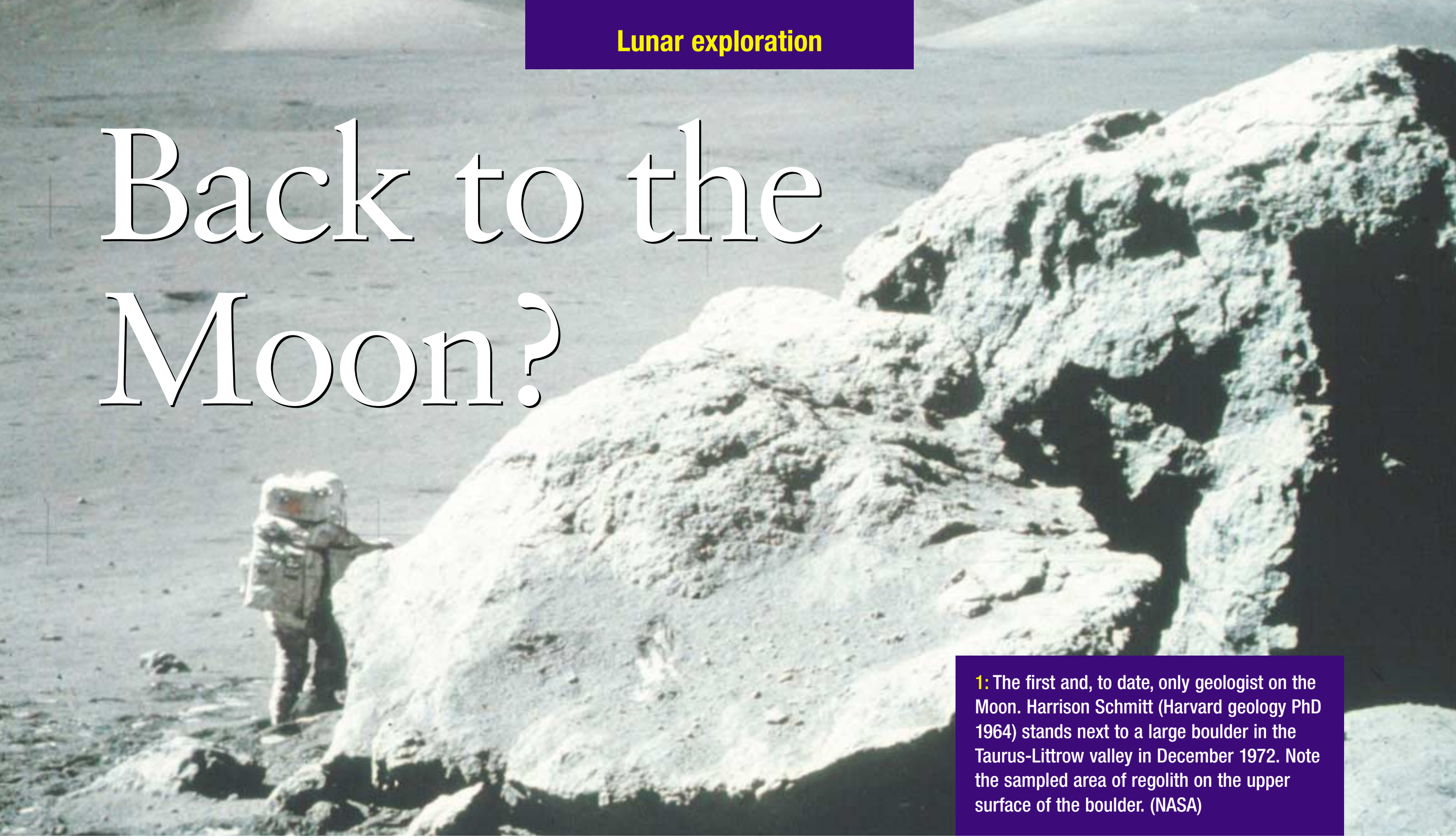
# Back to the Moon?



1: The first and, to date, only geologist on the Moon. Harrison Schmitt (Harvard geology PhD 1964) stands next to a large boulder in the Taurus-Littrow valley in December 1972. Note the sampled area of regolith on the upper surface of the boulder. (NASA)

**Ian A Crawford** makes the case for a return to the Moon, where an archive of information from the early history of the terrestrial planets demands the attention of human observers and explorers on the spot.

In the aftermath of the Columbia tragedy there has been much debate over the wisdom of sending people into space. And rightly so, for human spaceflight is both dangerous and expensive, and if we choose to pursue it we need to be sure of our reasons for doing so. In earlier articles I have argued that a human presence in space is desirable for a range of scientific, political and cultural reasons (e.g. Crawford 2001, 2003), although I realize that many of my colleagues take a different view. Here, I lay out the scientific case for sending people back to the Moon, 30 years since the last astronauts left its surface.

On 14 December 1972 Gene Cernan and Harrison Schmitt blasted off from the Taurus Littrow valley, on the south-eastern shore of Mare Serenitatis, at the end of the highly successful Apollo 17 mission. Apart from a few robotic Russian landers, the last of which (Luna 24) landed in August 1976, the surface of the Moon has since been left in peace. And although in recent years there has been something of a renaissance in lunar exploration from orbit (e.g. the Clementine and Lunar Prospector missions), there are no plans to revisit the surface. It seems to me that this long hiatus in surface exploration has been to the detriment of lunar and planetary science, and that the time has come to establish a permanent human presence on the Moon.

## Abstract

The Apollo missions left an immense legacy to the scientific world, in the scientific and technological achievements of the programme and, especially, in the samples of the Moon that they returned to Earth. This material is the basis for our understanding of the geological history of the Moon and of the early history of the Earth and the solar system. Yet this complex history hangs on samples from just six landing sites; the rest of the Moon will offer a rich archive of information from the evolution of the Moon, the early Earth and perhaps even the other planets, should we ever explore it.

### Lessons from Apollo

While the Apollo project was, notoriously, undertaken for geopolitical rather than scientific reasons, during the later missions (especially Apollos 15, 16 and 17) scientific exploration became a major component of the programme (e.g. Wilhelms 1993, Taylor 1994 and Harland 1999). This resulted in an enormously rich scientific legacy of which we are the beneficiaries. Yet, in the wake of the Columbia accident, it is poignant to reflect that an earlier tragedy, the Apollo 1 fire of 26 January 1967 which killed astronauts Gus Grissom, Roger Chaffee and Ed White, could easily have put an end to the Apollo programme.

Some weeks before the fire, Grissom had himself contemplated the risks: “We’re in a risky business… and we hope if anything happens to us it will not delay the programme. The conquest of space is worth the risk of life,” (quoted by Allday 2000 p158). It is not, of course, for any of us to judge if the scientific legacy of Apollo was worth this sacrifice, but all planetary scientists can, and should, be grateful for the willingness of the Apollo astronauts to accept the risks. For we are indebted to them for much of our current knowledge of Moon, and of the origin and evolution of the solar system. Indeed, I shudder to think what the textbooks would now have to say about the early history of the solar system had Apollo been cancelled in 1967 – even today, one can scarcely attend a scientific meeting on the subject without seeing geochemical and isotopic analyses of Apollo samples presented in one context or another. By analogy, we might like to reflect on how many future scientific discoveries may never be made if, as advocated by some, the Columbia accident is allowed to put a stop to human space exploration.

There is, I know, a widely held view that the Apollo science could have been achieved much more cheaply using robots, and that people are not required in the exploration of space. Indeed, at a recent meeting organized by the Royal Society (optimistically entitled "To boldly go", and held in London on 9 October 2002) I heard David Scott, the commander of Apollo 15, say that even *he* thought that small, kilogramme-mass "microrovers" could now achieve most of the science that he and his colleagues conducted on the lunar surface a generation ago. However, for reasons given below, I think he was doing himself a grave injustice, and that this just goes to show how deeply the myths surrounding the capabilities of robotic exploration have been allowed to penetrate – even in the minds of those whose personal experiences would seem to indicate otherwise.

I suppose microrovers would be relatively cheap to land on the Moon, which is doubtless an attraction, but how effective would they be? For one thing, how could they possibly collect, *and return to Earth*, something like 10 times their own mass in rock and soil samples (the Apollo 15 haul alone was 77 kg, and the overall Apollo total was 382 kg)? How would they be able to drill cores to a depth of over 2 m and return these intact? And what about the heat flow measurements? The gravimeter traverses? The magnetometer readings? The seismic experiments? The solar wind collection? All of which, and more, were actually conducted at some or all of the Apollo landing sites – Apollo 16 even deployed an ultraviolet telescope for astronomical observations. (See Heiken *et al.* 1991 for a review of the geological and geophysical work, and Carruthers and Page 1977 for the astronomical.) And on top of all of this, we have to ask whether any kind of robot, micro or otherwise, would be able to make the fine distinctions in the field between what it is important to collect or record for later analysis and what it is not, or make serendipitous discoveries not anticipated by its designers back on Earth.

But if, as I have argued, science was a beneficiary of having had people on the Moon 30 years ago, this begs some important questions: how much more would we have learned had Apollo not been terminated when it was, just as the first scientifically trained astronaut reached the lunar surface (figure 1)? And how much more do we stand to learn by sending people back there in the future? The remainder of this article will consider some specific answers to these questions. However, in order to gain a general sense of perspective, it will be helpful to reflect that while the Moon has a surface area 25% larger than the continent of Africa, the entire, hugely influential, Apollo data set was obtained from just six landing sites (all at low to mid latitudes on the near side), within a maximum distance from a landing site of approximately 7.5 km (on the second Apollo 17 traverse), and a total time spent outside a lunar module deploying equipment and/or exploring the surface of just three-and-a-third days (summed over all six missions; see Harland 1999). It should be obvious that there is still a huge amount of work waiting to be done.

**2:** David Scott stands next to the Apollo 15 lunar rover, parked near the crest of Hadley Rille on the eastern side of Mare Imbrium. The rille here is about 1.5 km wide and 350 m deep, and outcrops of layered basaltic lava flows were observed on the far wall, as shown in figure 3. (NASA)

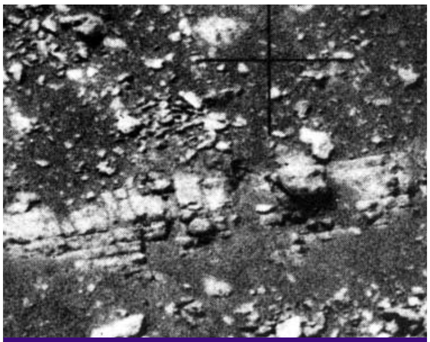
## Lunar dating and the cratering rate

Consider, for example, one of the most important scientific contributions of Apollo: determining the lunar impact cratering rate. Knowledge of this is absolutely fundamental to our understanding of solar system history, and what we have we owe to Apollo, but it is not quite as reliable as we might wish. For example, Copernicus is a prominent near-side impact crater whose ejecta forms a key stratigraphic horizon on the Moon (the boundary between the Eratosthenian and Copernican eras), usually dated at 810 million years ago (Wilhelms 1987). However, no Apollo mission actually visited Copernicus, and the age comes from a light grey layer in the regolith found just below the surface at the Apollo 12 landing site (340 km to the south), and interpreted as a ray of Copernicus ejecta. A number of assumptions underlie this interpretation – the deposit may not be from Copernicus at all and, even if it is, the dates obtained for it may not represent that of the impact itself. Needless-to-say, this is an unsatisfactory basis for dating a key event in lunar history.

The only secure way to obtain the age of a meteorite impact is to date material melted by it, which must first be identified in the field and then collected. Ironically, had Apollo not been terminated when it was, it is likely that one of the cancelled missions (18, 19 or 20) would have visited Copernicus (Wilhelms 1993) and we would now have a reliable age. As it is, dating Copernicus remains a key scientific objective for lunar studies, necessitating the sampling of impact melt deposits in its vicinity and returning these to Earth. However, dating Copernicus is only indicative of the task before us, and indeed only ranks seventh in the priority list compiled by Wilhelms (1987). There are many major events in lunar history that were not dated at all, or not reliably dated, by Apollo. Anything approaching a full understanding of lunar history will ultimately require sample collection and analysis on a far larger scale. Conceivably, some of this could be achieved by a well-targeted set of robotic sample-return missions, although great care will have to be taken to distinguish the particular deposits of interest from other materials littering the immediate vicinity of the landing site – which makes this kind of intelligent sampling far better suited to an experienced human field geologist than to a robotic probe (e.g. Spudis 1992, 2001).

## Treasures in the regolith

The lunar surface environment may contain other, more subtle, scientific treasures requiring a human presence for their extraction. To gain a sense of what may be waiting for us, recall that the Moon has preserved a record of the environment of the inner solar system, and especially the near-Earth environment, from a time shortly after its formation 4.6 billion years ago. The vast majority of dated Apollo samples are older than 3 billion years, with many occupying the range 4.5 to 3.8 billion years which is hardly represented at all by the extant terrestrial geological record. This was a crucial period in Earth history, which saw the origin (or, if it originated elsewhere, at least the establishment) of life on our planet, yet the Earth itself has retained no record of it. It also covers an interesting period in the evolution of the Sun, immediately after its arrival on the main-sequence and the final



3: Discrete basaltic layers exposed in the wall of Hadley Rille, photographed by David Scott using a 500 mm telephoto lens. This outcrop lies about 25 m below the crest of the rille (P D Spudis, personal communication), and the individual layers are of the order of a metre thick. (This image is an enlarged region of NASA image AS15-89-12104, reproduced from Taylor and Spudis 1990)

dispersal of its protoplanetary disk.

The suggestion that records of these early times may still exist on the Moon has been advanced by Spudis (1996), and rests on the range of ages (roughly 4.2 to 3.1 billion years; Heiken *et al.* 1991) of the lava flows of the lunar maria. In general, older flows must underlie younger ones, and layered deposits seemingly indicative of this were observed in the wall of Hadley Rille (a lava channel some 1.5 km wide and 350 m deep, close to the eastern edge of Mare Imbrium) by the crew of Apollo 15 (figures 2 and 3). A lava flow exposed on the lunar surface for millions of years will develop a surface layer of regolith due to micrometeorite bombardment, which will be buried (and thus preserved) by later lava flows deposited on top of it. The process of regolith formation will then begin again at the upper surface of the new flow, until this is buried in its turn. We may thus expect to find layers of such paleoregoliths sandwiched between lava flows of progressively younger age, providing snapshots of the lunar surface environment at particular epochs billions of years ago. Such layers will provide information on the flux and composition of interplanetary dust particles in the early solar system, and successive layers will provide information on how these have evolved with time.

More importantly, because solar wind ions and cosmic-ray particles are efficiently trapped in lunar regolith (e.g. Heiken *et al.* 1991, Wieler *et al.* 1996), paleoregolith layers may provide a unique record of the charged particle environment of the inner solar system in its early history. Of particular interest will be the strength and composition of the ancient solar wind, as this will provide a test of solar evolution models not obtainable in any other way. To quote the conclusions of Wieler *et al.* (1996): “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.” Certainly, such studies should easily be able to test the controversial hypothesis (advanced to explain apparently warm temperatures and liquid water on ancient Mars) that the Sun was originally some 5% more massive than it is today, losing this extra mass through a powerful solar wind over its first billion years (Whitmire *et al.* 1995). Conceivably, paleoregoliths may also record variations in the galactic cosmic-ray flux billions of years ago, including records of high-energy galactic events (e.g. nearby supernovae or gamma-ray bursts) of significance for biological evolution.

Furthermore, Armstrong *et al.* (2002) have recently suggested that meteorites blasted off other terrestrial planets by giant impacts in the early history of the solar system may be preserved on the Moon. These could include samples from the early Earth, relevant to the origin and evolution of life, and samples of the pre-greenhouse Venus not otherwise available. Of course, such materials will be difficult to identify, especially as they are more likely to be preserved in layers of paleoregolith dating from the time of their delivery, rather than lying around on the present surface. But if they could be located and recovered, it is clear that they would provide yet another valuable window into the early history of the solar system.

## Accessing the lunar archive

All this information, and perhaps much more, may presently be archived in the top few kilometres of the lunar surface – an archive of conditions that prevailed at a key period in solar system history, but which (with the possible exception of the much less accessible surface of Mercury) will exist nowhere else. The question is how best to access it? It will be clear from the above that this is unlikely to be amenable to the kind of small-scale robotic rovers and sample-return missions sometimes advocated as an alternative to human exploration. Merely identifying paleoregolith layers is likely to require a considerable amount of fieldwork, very likely involving seismic profiling and the ability to extract core samples hundreds of metres deep. It seems to me that this kind of complex geological exploration would be much better conducted by human specialists in the field, and may be wholly impractical otherwise. Furthermore, it is not only lunar studies that would benefit from the establishment of a human outpost on the Moon. In particular, the Moon has many advantages as a platform for astronomical observation (e.g. Burns *et al.* 1990) and, once the basic infrastructure of a lunar base is established, we may expect astronomers to begin exploiting it for their instruments (just as they are now beginning to exploit the exterior structure of the International Space Station, ISS, for the same purpose; e.g. Parmar 2001).

## Look to the future

Fairly soon now, a decision will be required concerning the direction of human spaceflight activities in the post-ISS era. While Mars will have its advocates, and would indeed benefit from a programme of human exploration (for reasons given briefly by Crawford 2001; see also Spudis 1992, Hiscox 2001), consideration of the limitations imposed by our current knowledge and capabilities suggests that such a move might be premature. Sending people to Mars will be orders of magnitude more challenging and expensive than sending them back to the Moon, and there is a strong case for learning to operate successfully on the latter before attempting this greater challenge. Establishing a human presence on the Moon would both help pioneer the necessary technical expertise (Eckart 1999) and offer the significant scientific advantages outlined above. Moreover, an *international* Moon base would naturally build on the experience gained in managing and operating the ISS – the largest international collaborative space project yet attempted. Rather than allowing the incipient world space programme represented by the ISS to dissipate once that project is completed, we should aim to build on it in order to develop an international human spaceflight infrastructure from which science can only benefit. I believe that an international Moon base would be the obvious next step in this process. ●

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