The scientific case for

Many scientists are sceptical about the scientific value of sending people into space. **Ian Crawford** argues that this scepticism is misplaced and that science has been, and will continue to be, a beneficiary of human space flight.

Scientists have been arguing about the benefits of venturing into space since before the space age began. Indeed, the ambivalence, and to some extent the short-sightedness, of the astronomical establishment towards space exploration is well illustrated by its famous dismissal as "utter bilge" by the incoming Astronomer Royal, Richard Woolley, in January 1956 (Woolley 1956).

Although Woolley's off-the-cuff remark is often quoted out of context (it was actually aimed at speculations about interplanetary travel, rather than at the technical feasibility of launching objects into space), it is perhaps revealing of a widespread scepticism among astronomers about the value of space research that, four years later, the Astronomer Royal was still able to doubt the scientific usefulness of even artificial satellites (Woolley 1960). Fortunately for astronomy, the other side of this debate, championed most notably by Lyman Spitzer (e.g. Spitzer 1946, 1960), carried the day, and the enormous scientific benefits of space astronomy are now clear for all to see.

However, while the fundamental scientific contributions of unmanned space probes are now universally acknowledged, arguments continue about the scientific role of people in space. Primarily, these concern the scientific relevance of the International Space Station, and proposals for human missions to Mars. But before discussing these future issues, we should perhaps consider the scientific legacy of the most ambitious human space flight programme to date.

The legacy of Apollo

It is well known that the primary driving forces behind the Apollo project were geopolitical rather than scientific. Indeed, it is naive to believe that anything other than powerful political motives (which at the time were firmly rooted within the context of the Cold War) could have sustained a project which, at its peak, consumed over 4% of the US federal budget. The key question here, however, concerns the extent to which scientific knowledge was increased as a result of the Apollo project, regardless of the political forces behind it.

The fact that Apollo was expensive and not primarily science driven seems to have irritated many in the scientific community, and has even caused some to deny that it had any scientific relevance at all. For example, on the eve of the Apollo 11 landing, the Astronomer Royal, alluding to his remarks over a decade earlier, asserted that "from the point of view of astronomical discovery it [the Moon landing] is not only bilge but a waste of money" (Woolley 1969). Indeed, 25 years after Apollo I overheard a senior astronomer making exactly the same point at a dinner party.

The truth, of course, is that science was an enormous beneficiary of Apollo, primarily because of the 382 kg of lunar rock samples returned to Earth. The analysis of this material has had a huge impact on our understanding, not only of lunar history, but of the early evolution, and indeed the origin, of the solar system as a whole. By permitting an absolute calibration of the impact-cratering rate, the dating of these samples provided strong support for the theory of terrestrial planet formation by planetesimal accretion, as well as our only reliable method of estimating planetary surface ages throughout the solar system.

Moreover, their geochemical analysis, which demonstrated the compositional similarity of the Moon to the Earth's mantle, provided one of the main arguments for the "giant impact" theory of lunar origins (Hartmann and Davis 1975), which further supports models of the merger of planetesimals in the early solar system (Wetherill 1990). The composition of these samples is now being used to calibrate the excellent multispectral images of the Moon recently obtained by the Clementine spacecraft (e.g. Blewett *et al.* 1997). Nor should we forget the geophysical studies carried out during the Apollo project, most notably of the lunar interior by means of active seismology – the Moon is still the only planetary body, apart from the Earth, whose structure has been probed in this way (see Goins *et al.* 1981 for a review).

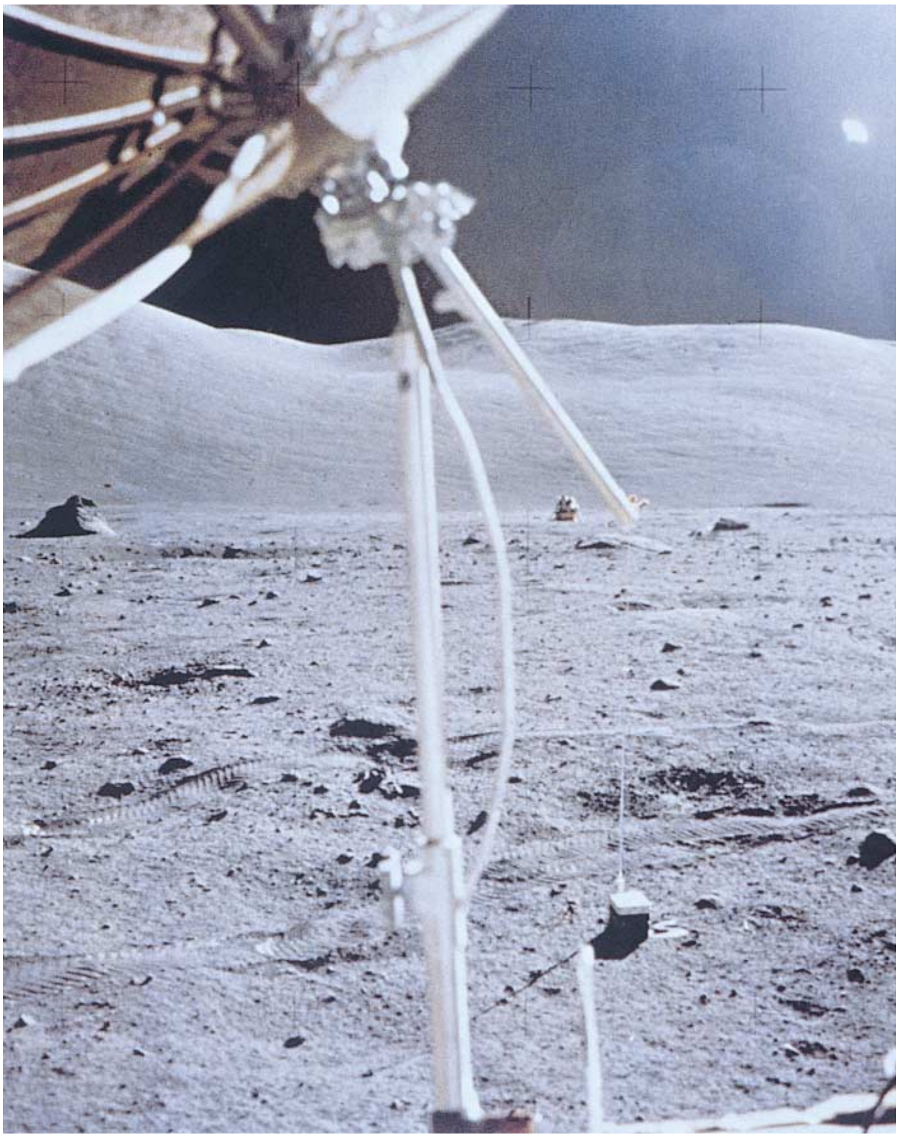
The opponents of human space flight will argue that all this could have been achieved much more cheaply with robotic missions. However, I think this is a mistake. While it is true that much of the Apollo science could, in principle, have been performed robotically, there must be considerable doubt as to how much would actually have happened had the manned landings not taken place. For example, although it is true that three unmanned Soviet space probes (Lunas 16, 20 and 24) successfully collected 321 g of lunar material in the 1970s, it is notable that this was less than 0.1% of the amount returned by the Apollo missions. Moreover, the Apollo material consisted of more than 2000 individual samples, intelligently collected from many locations around each landing site, while the Luna material consisted of a single core from each site. No practical, or (within a purely scientific budget) affordable, robotic programme could have returned anywhere near the quantity, or the diversity, of the Apollo lunar samples.

It is, of course, quite obvious why the Apollo missions were able to carry a large quantity of scientific equipment to the Moon, and to return with hundreds of kilograms of rock samples. As each flight had to transport three men and all their life-support equipment to the Moon anyway (in order to satisfy the political objectives of the programme), the marginal cost of carrying bulky scientific equipment (such as the seismic arrays and their explosive charges), and of bringing back the rock samples, was a negligible fraction of the total cost. This illustrates an important scientific advantage of human space flight: any space mission that has to transport people will, by its very nature, be able to carry a significant scientific pay-load, *even if science is not the primary driver for the mission.*

The Space Station

The International Space Station (ISS) is another major human space project which is not primarily science driven. Predictably, therefore, it has again raised the ire of those in the scientific community who confuse an absence of overriding scientific purpose with scientific worthlessness. The ISS, like Apollo before it, is being built primarily for political reasons (many of which, like the encouragement of international co-operation, are good reasons) but this does not mean that science will not be a beneficiary (see Lewis 1998 for a review). It may be true that the proposed scientific uses of the ISS, such as microgravity and life science research, could never justify the construction costs of the ISS on their own, but they are nevertheless important scientific disciplines which stand to benefit substantially from it. Even astronomy is likely to benefit, with the recent proposal to

human space flight



One of the eight explosive packages deployed at the Apollo 17 landing site as part of the active seismic profiling experiment. This view is from the lunar roving vehicle (LRV) towards the Sculptured Hills (which border Mare Serenitatis) 4 km to the east. The Apollo 17 lunar module, where the geophone array was set up, is visible in the distance.

place an all-sky X-ray monitor on board (Matsuoka *et al.* 1997), and other astronomical applications are likely to follow.

The real significance of the ISS, however, is that it will help lay the foundations for future space programmes with vastly greater scientific potential. There are three aspects to this. Firstly, the ISS will provide considerable experience in space engineering; although many scientists are sceptical of the suggested scientific applications of the ISS itself, a moment's reflection will show that considerable scientific advantages are likely to follow from the ability to construct large structures (e.g. telescopes and interferometers) in space.

Secondly, studies of the physiological effects of weightlessness to be conducted on the ISS will be essential before human beings are able to undertake long journeys to other planets in the solar system. Notwithstanding the objections of the critics of human space flight, I shall argue below that the scientific returns of such missions are likely to be considerable.

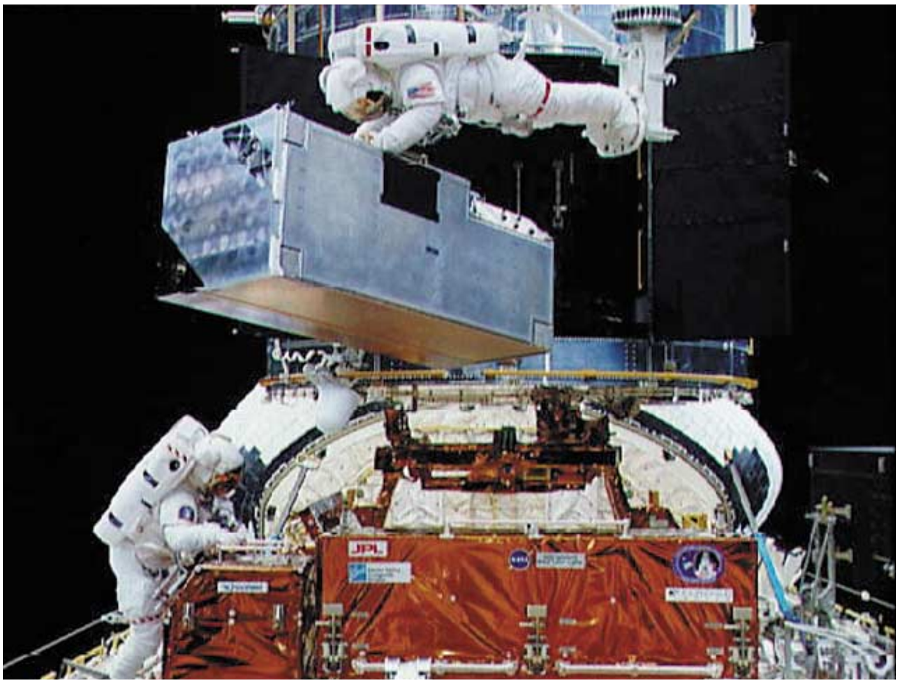
The third point concerns the development of new institutional arrangements for the management of complex international space projects. Indeed, one space analyst has already expressed the view that "in effect, an international space agency has been created for the station" (Logsdon 1998). This may be overstating things at present, but there are strong reasons for believing that, if humanity is to have a significant future in space, something along these lines will be both necessary and desirable (Crawford 1992). If experience in building and operating the ISS helps to develop the institutional foundations for a future world space programme, that alone will be one of its most important legacies.

Let us now consider the scientific opportunities of human space flight in the post-ISS era.

A return to the Moon

There are broadly three scientifically important reasons for humans to return to the Moon:

- *Science on the Moon.* The Moon is an important object of scientific study in its own right, and one that is likely to continue to provide major insights into the origin and evolution of the solar system. However, a moment's reflection will reveal that we have not yet achieved anything like a complete understanding of its structure, environment, or history. This is especially obvious when we consider that all our lunar samples and *in situ* measurements have come from low to mid latitudes on the nearside only. Thus the scientific case for renewed lunar exploration is extremely strong, and, as for Apollo, I suggest that more exploration will be carried out as part of a manned programme than if we rely exclusively on robotic means.
- *Science from the Moon.* The potential advantages of the Moon as a platform for astronomical observations have been reviewed extensively elsewhere (e.g. Burns and Mendell 1988, Burns *et al.* 1990), and I will not repeat them all here. Briefly, they arise from the stability of the lunar surface (possibly an advantage for the construction of long-baseline optical/IR interferometers); the slow rotation period of the Moon (permitting very long integration times on a single object); the extreme cold (<100 K) in shadowed areas (a significant advantage for infrared instruments); and the extreme radio-quietness of the lunar farside (probably the best site for radio astronomy anywhere in the solar system). It may be that some of these applications could, in principle, be performed from unmanned space observatories. However, the point here is that a human return to the Moon, undertaken for whatever reason, is likely to provide astronomy with great opportunities which might not otherwise be practical or affordable.
- *Experience gained on the Moon.* Finally, a



Astronaut Kathryn Thornton with the corrective optics (COSTAR) unit during the first servicing mission to the Hubble Space Telescope in December 1993. Astronaut Tom Akers is at left. The blue skies of Earth are reflected from both the COSTAR unit and the body of the telescope (at top).

human return to the Moon would provide experience in living and working on hostile planetary surfaces. This will be particularly important when it comes to constructing human outposts elsewhere in the solar system, and in particular on the surface of Mars.

The case for Mars

The well-worn arguments against sending people to Mars have been reiterated by Sleep (1997), who asserts that this would be "the most dangerous, costly, inefficient and counterproductive method yet devised for exploring the Red Planet", and that machines could do it all much better. I certainly agree that the robotic exploration of Mars to date has been a tremendous success, and that the collection, early in the next century, of a few kilograms of Mars rock by a robotic sample return mission will be of tremendous scientific importance. However, a moment's reflection will show that a proper exploration of Mars will require a lot more than this.

The ultimate aim of planetary science must be to understand the other planets to the same extent that we understand the Earth, and even that is far from complete. Mars has a surface area approximately equal to the land area of Earth, and by all accounts it has had a highly complicated geological, climatological, and, possibly, biological history. To reach anything like an adequate understanding of Mars will require, as a minimum, the analysis of tonnes (possibly thousands of tonnes) of rocks collected from all over the planet; it will require magnetic, gravity and seismic surveys; and it will require boreholes, probably kilometres deep, drilled in selected regions. The idea that this could be achieved with half a dozen robot landers is frankly ridiculous.

Consider the most important scientific question which needs to be addressed on Mars: did life evolve when, some 3.5 to 4 billion years ago, liquid water flowed on its surface and conditions were similar to those that prevailed on Earth when life evolved here? Recent work on the origin of life (e.g. de Duve 1996) is close to predicting that life ought to have evolved on Mars at that time. It is hugely important for our understanding of the origin of life, and indeed for the whole science of biology, to ascertain whether or not it did so, and, if it did, how similar Martian lifeforms were to terrestrial ones. An answer to this question will require procedures similar to those used to find the oldest microfossils on Earth (e.g. Schopf 1993): it will be necessary to conduct a detailed search for Martian sedimentary rocks of the appropriate age, to determine their geological and palaeo-environmental context, and to painstakingly sift through them with microscopes. It is very difficult to see how such a programme could be conducted satisfactorily with robots alone.

Space infrastructure

The main point I want to make is that science stands to benefit greatly from exploiting the technology, and especially the infrastructure, developed to support a human space flight capability. By infrastructure I mean all the background capabilities (for example, launchers, spaceports, space stations, interplanetary transports, lunar and planetary outposts) which purely scientific budgets could never afford to develop, but which nevertheless act to facilitate scientific research which would not otherwise take place. We have seen how this worked in the case of Apollo, and how the ISS will provide infrastructural support for a wide range of scientific investigations.

The in-orbit repair of the Hubble Space Telescope (HST) in 1993 provides a good example of the usefulness of a human space flight infrastructure. Sleep (1997) has rather disparagingly asserted that this was only to correct a fault of NASA's own making, but this misses the point entirely: without that human intervention in space we would still be stuck with the uncorrected telescope, and astronomy would be greatly impoverished as a consequence. Moreover, the first HST refurbishment mission (STS 61) didn't just install the corrective optics (COSTAR), it also replaced the solar panels, installed new gyros, repaired the GHRS, and installed WF/PC2. A subsequent astronaut-tended upgrade last year (STS 82) installed two new instruments (STIS and NICMOS), and two further deliveries of new instruments are planned. Thus the HST experience clearly illustrates the scientific advantages of being able to call upon the capabilities of a human space flight infrastructure when the need arises (something already foreseen by Spitzer 1974).

Future potential

Considerable as these advantages have been, however, they pale into insignificance compared to those potentially available in the future. We have already outlined the likely scientific benefits of human outposts on the Moon and Mars, and alluded to the possibilities for building large astronomical instruments in space. Other possibilities include the development, and in-space construction, of interplanetary vehicles capable of taking human crews to both near-Earth and Main Belt asteroids, and to the Galilean satellites of Jupiter. In the case of the asteroids, the primary motivation for human exploration is likely to be economic rather than scientific (e.g. Lewis *et al.* 1993), but it seems clear that our knowledge of these objects, and thus of the early history of the solar system, would be greatly increased as a consequence. As regards the Galilean satellites, the arguments for human exploration closely follow those already advanced for the Moon and Mars. Consider Europa, for example, a world almost as large as our Moon and which is of biological interest owing to the likely presence of an ocean of liquid water below its icy crust. How much of the history, structure and environment of this important object will it be possible to piece together from robotic missions alone?

In the more distant future, we should keep in mind the enormous scientific opportunities that would result from the ability to construct fast ($v > 0.1c$) interstellar space probes (Crawford 1990). However, it is important to understand that the construction of even an unmanned interstellar probe will entail large-scale engineering work in space (see Mallove and Matloff 1989, and Crawford 1990 for reviews), and will only be possible once the necessary infrastructure has been developed.

Wider motives for human space flight

I have argued above that science has been, and will continue to be, a major beneficiary of human space flight, and that the vociferous opposition to it from some quarters of the scientific community is badly misplaced. It seems to me that most of this opposition, from Richard Woolley onwards, stems from two implicit, but erroneous, assumptions: first, that the primary motives for sending people into space are, or at least ought to be, scientific; and second, that the high cost of human space flight is taken from existing scientific budgets.

In fact, ambitious human space projects are undertaken for a variety of reasons, most of which are sociopolitical in nature rather than scientific. In the case of Apollo these arose from the perceived imperatives of the Cold War, and are now thankfully behind us. However, compelling social and political arguments in support of human space flight remain. These range from the economic (where major space initiatives act as high technology "public works" projects, having a significant multiplier effect on the economy as a whole; e.g. Bezdek and Wendling 1992), to the geopolitical (especially the encouragement of co-operation between former Cold War adversaries). In the future, powerful sociopolitical reasons for human space flight are likely to include the demands of the world economy for extraterrestrial raw materials, and the continuing need for high-profile international projects as aids in building a stable geopolitical environment here on Earth (Crawford 1995). Quite frankly, these arguments are sufficiently strong to justify a major human space programme even in the absence of any scientific benefits whatsoever.

As the complex motivations for human space flight are not primarily scientific, it follows that they are not, and indeed cannot be, financed primarily from scientific budgets. Consider the US space programme: NASA currently has an annual budget of approximately $14 billion (which, to put things in perspective, is only about 5% of the US military budget). However, this should not be perceived as a science budget *per se*, because NASA is not primarily a science agency (US Congress 1958). There are those in the scientific community who seem to believe that if only NASA was not operating the Space Shuttle, or contributing to the ISS, then the whole $14 billion would be available for space science. However, as we have seen, the former activities are motivated primarily by politically worthwhile, but generally non-scientific, policy objectives of the US government; if the money was not spent on manned space flight it would more likely be spent on military hardware, welfare payments, or tax cuts than on science.

The International Space Station will provide much-needed experience of building large structures in space.

It is true that there is currently a grey area where the manned and unmanned budgets sometimes have to compete for funds within NASA, and that there has been a history of cost overruns in the former decreasing provision for the latter (Van Allen 1986). However, while this is certainly unfortunate, it is really an argument for reform of the way NASA's budget is allocated by the US Congress rather than for the abandonment of a human space flight capability. Pursuing the latter course would only marginally increase the funds available for space science in the short term, but would prevent the long-term development of a space infrastructure from which science stands to gain so much.

Science education

Nor should we overlook the stimulus to scientific and technical education provided by high-profile human space activities. This extends well beyond stimulating young people to embark on careers in science and engineering, important though that is, but also leads to an increased scientific awareness throughout society. Sagan (1994) put it eloquently: "Exploratory space flight puts scientific ideas, scientific thinking, and scientific vocabulary in the public eye. It elevates the general level of intellectual inquiry." The whole scientific enterprise has the greatest possible interest in encouraging this process.

Conclusion

While recognizing that many of the driving forces behind human space flight are social and political, rather than narrowly scientific, it seems clear that science has been, and will continue to be, a major beneficiary of having people in space. What, after all, is the alternative? We can either stay at home, sending a few robot spacecraft to our neighbouring planets, and continuing to gaze at the more distant universe across light years of empty space, or we can get *ourselves* out among the planets and, eventually, the stars. In which alternative future would we learn the most about this universe and our place within it? ●

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