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Towards an Integrated Scientific and Social Case for Human Space Exploration

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Abstract. I will argue that an ambitious programme of human space exploration, involving a return to the Moon, and eventually human missions to Mars, will add greatly to human knowledge. Gathering such knowledge is the primary aim of science, but science's compartmentalisation into isolated academic disciplines tends to obscure the overall strength of the scientific case. Any consideration of the scientific arguments for human space exploration must therefore take a holistic view, and integrate the potential benefits over the entire spectrum of human knowledge. Moreover, science is only one thread in a much larger overall case for human space exploration. Other threads include economic, industrial, educational, geopolitical and cultural benefits. Any responsibly formulated public space policy must weigh *all* of these factors before deciding whether or not an investment in human space activities is scientifically and socially desirable.

Keywords: Human spaceflight, space exploration, Moon, Mars, international cooperation in space

1. Introduction

Human space exploration is expensive, and the tragic loss of the space shuttle *Columbia* in February 2003 reminds us that it is sometimes costly in human, as well as in merely monetary, terms. It follows that, as for any expensive and potentially dangerous activity, we need to be sure of our reasons for engaging in it. As discussed in the Introduction to this volume, the present debate over the future of human spaceflight takes place in the context of the new US Vision for Space Exploration (NASA, 2004) and the European Space Agency's *Aurora* programme (ESA, 2003). Both these programmes aim ultimately to land people on Mars, although it seems certain that the Moon will be an earlier target. It is also likely that, in practice, both will be merged, along with contributions from other spacefaring nations, into a wider international human spaceflight programme. In this paper I will argue that such a programme has the potential to confer a wide range of scientific, technological, economic, political, and cultural benefits. Only when all of these factors are taken into account is it possible to appreciate the full strength of the scientific and social case for public investment in human space activities.

2. The Scientific Case

The scientific case for human space exploration covers the whole gamut of human knowledge, and cannot be properly judged from the viewpoint of any single scientific discipline. This was brought home to me some years ago when a senior astronomer, now deceased, confidently informed me that “science got nothing out of the Apollo programme.” At the time I was taken aback, but I realise now that what he meant was that *ultraviolet astronomy* got nothing out of the Apollo programme, which, excepting the results of the Apollo 16 ultraviolet telescope (Carruthers and Page, 1977), is essentially true. But, *science*, considered as a whole, benefited tremendously from Apollo (for reviews see Heiken *et al*., 1991; Wilhelms, 1993; Taylor 1994; Spudis 1996; Canup *et al*., 2000), it’s just that the major benefits were in areas of lunar geology and planetary science with which ultraviolet astronomers are seldom familiar. This tendency for scientists to equate ‘science’ with their own particular sub-discipline is all too common, and it bedevils attempts to forge a consensus on the overall scientific merits of inherently multidisciplinary activities. In the particular case of human space exploration, a proper assessment of the totality of scientific case requires careful consideration of at least three separate research fields: microgravity, space astronomy, and planetary exploration.

2.1. RESEARCH IN MICROGRAVITY

The microgravity environment of low Earth orbit provides unique opportunities for research in the life sciences (including human physiology and medicine), materials science, and fundamental physics (see Seibert *et al.,* 2001, for a comprehensive review). While much progress has been made in these areas as a result of earlier Earth orbital missions (i.e. Skylab, Salyut, Spacelab, and, especially, Mir), further progress will rely on the capabilities of the International Space Station (ISS). Although the UK has so far opted out of microgravity research on the ISS, the potential scientific benefits are well documented (e.g. Seibert *et al.*, 2001; Minster *et al.*, 2001; see also the contribution by B. Hufenbach and G. Seibert in this volume). Even in the UK, the matter has been investigated by the independent Microgravity Review Panel, which concluded that the potential scientific benefits do in fact justify the UK's participation in the space station utilisation programme (Wakeham *et al.*, 2003), although the UK government has so far failed to act on this advice.

In the field of materials science, areas where our understanding stands to be advanced through research in microgravity include: the thermophysical properties of fluids (e.g. Grassi & Legros, 2001); the crystallization of metals, alloys, and other inorganic materials (e.g. Benz *et al.*, 2001); the crystallization of organic macromolecules (e.g. Garcia-Ruiz *et al.*, 2001); the physics of combustion (e.g. Eigenbrod, 2001); and the properties of complex plasmas (i.e. plasmas containing charged particulates in addition to ionised gasses; Morfill & Thomas, 2001). Many of these areas have clear industrial applications (reviewed by Sprenger, 2001) and, as noted by the UK Microgravity Review Panel, "it would be surprising if there were no practical applications from any of these investigations in the long term" (Wakeham *et al.*, 2003).

Probably the most important scientific benefits of microgravity research will accrue to the life sciences. Space life science research embraces the whole range of studies from molecular and cellular biology to whole-organism physiology (Freeman, 2000; Seibert *et al.*, 2001; Fong 2001, 2004). In the important area of human physiology and medicine, research in the space environment has demonstrated the potential to provide unique insights into such areas as gene expression (e.g. Cogoli and Cogoli-Greuter, 1997), immunological function (e.g. Bouillon *et al.*, 2001; Sonnenfeld and Shearer, 2002), bone physiology (e.g. Turner, 2000; Cancedda, 2001; see also the contributions by M. Rennie, *et al.* in this volume), and neurovestibular and cardiovascular function (e.g. Clement, 2001; Kirsch & Gunga, 2001; see also Fong, 2004, and references therein). These areas are important for understanding a range of terrestrial disease processes (e.g. osteoporosis, muscle atrophy, cardiac impairment, and balance and co-ordination defects), and as such have potential medical applications here on Earth. Moreover, research in space physiology provides a stimulus for the development of innovative medical technology, much of which is directly applicable to terrestrial medicine (see the contribution by K. Fong elsewhere in this volume).

We may also note that, while there is a growing body of knowledge of the biological and physiological consequences of microgravity, and further advances are to be expected when the ISS becomes fully operational, the biological effects of prolonged exposure to low, 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 physiological consequences occur. There is particular interest in the long-term effects of reduced gravity on the human body. Of special importance is to establish potential gravity thresholds for different body functions, in particular with regard to loss of muscle and bone mass, reduced cardiovascular capacity, functioning of the central nervous system, and immune system deficiencies.

Long term studies in a reduced gravitational environment will be required to quantify these effects. In the context of future objectives for human space exploration, it may be noted that a permanently occupied lunar base would be ideally suited for these studies. Moreover, the unique radiation environment of the Moon would also provide many opportunities for fundamental research in the field of radiation biology that are not possible in low Earth orbit (ESA, 1992; see also contribution by B. Hufenbach and G. Seibert in this volume). 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, but also to support future human space operations. In particular, there are many strong scientific reasons for wanting to send astronauts to Mars (see below), and yet the basic research into the long-term health of a human crew operating under reduced gravity, and after a long period in microgravity, has not yet been performed. A lunar base, perhaps in combination with microgravity research on the ISS, is probably the only location where such research could be safely conducted.

2.2. SPACE ASTRONOMY

Ever since the space age began, astronomers have been among some of the fiercest critics of human space exploration. The scepticism, and, it has to be said, the short-sightedness, of some in the astronomical establishment towards space exploration is well illustrated by its famous dismissal as “utter bilge” by the UK’s incoming Astronomer Royal in January 1956 (Woolley, 1956), a sentiment he reiterated 13 years later on the eve of the Apollo 11 moon landing (Woolley, 1969). Recent criticisms of ESA’s Aurora programme by prominent astronomers (e.g. Heavens, 2005), and my own experience recounted in the anecdote above, indicates that the same basic negativity towards human spaceflight still prevails in influential quarters of the astronomical community.

There have, of course, been notable exceptions to this generalisation – a decade before Sputnik, Lyman Spitzer was arguing for the construction of a large space telescope (Spitzer, 1946) and later, when plans for such an instrument were well advanced, he drew special attention to the importance of “regular visits by trained astronauts who could maintain and update the instrument” (Spitzer, 1974). Subsequent experience with the Hubble Space Telescope (HST) has demonstrated the essential correctness of this view. It is especially important to recall that the first HST servicing mission (STS 61 in 1993; Fig. 1) did more than just correct for the spherical aberration of the faulty primary mirror – it also replaced the failing solar arrays and faulty gyroscopes, and installed the Wide Field/Planetary Camera (WF/PC2). Thus, even if the HST had been launched in 1990 with a perfect mirror, without human intervention it would still have failed over a decade ago and astronomy would have been the poorer as a consequence. Three subsequent servicing missions have installed several powerful new instruments (notably STIS and NICMOS in 1997, and the Advanced Camera for Surveys in 2002), and made additional repairs to the solar arrays, gyroscopes, and on-board computer equipment, all of which have greatly enhanced the scientific capability of the instrument (NRC, 2005).

The truth is that, without access to a supporting human spaceflight infrastructure, the HST would have been a much shorter lived, and much less scientifically versatile, instrument than it has in fact turned out to be. The telescope designer Roger Angel summed this up succinctly when, testifying before the US Congress in November 2003, he pointed out that the HST “has the huge, proven advantage of astronaut access” (Angel, 2003). But probably the most striking vindication of this argument is the strength of feeling in the astronomical community itself in favour of a further manned servicing mission to prolong the life of the HST once the space shuttle returns to flight (NRC, 2005).

Clearly there are important lessons here for the future of space astronomy. Despite recent advances in adaptive optics, and ambitious plans for very large (30 to 100m) ground-based telescopes, it seems certain that space-based telescopes will generally be preferable from a strictly scientific viewpoint (for all the reasons identified by Spitzer, 1947, who even then was envisioning space telescopes up to 15 m in diameter). Indeed, several ambitious future space telescopes are planned, ranging from the 6.5m James Webb Space Telescope, due for launch in

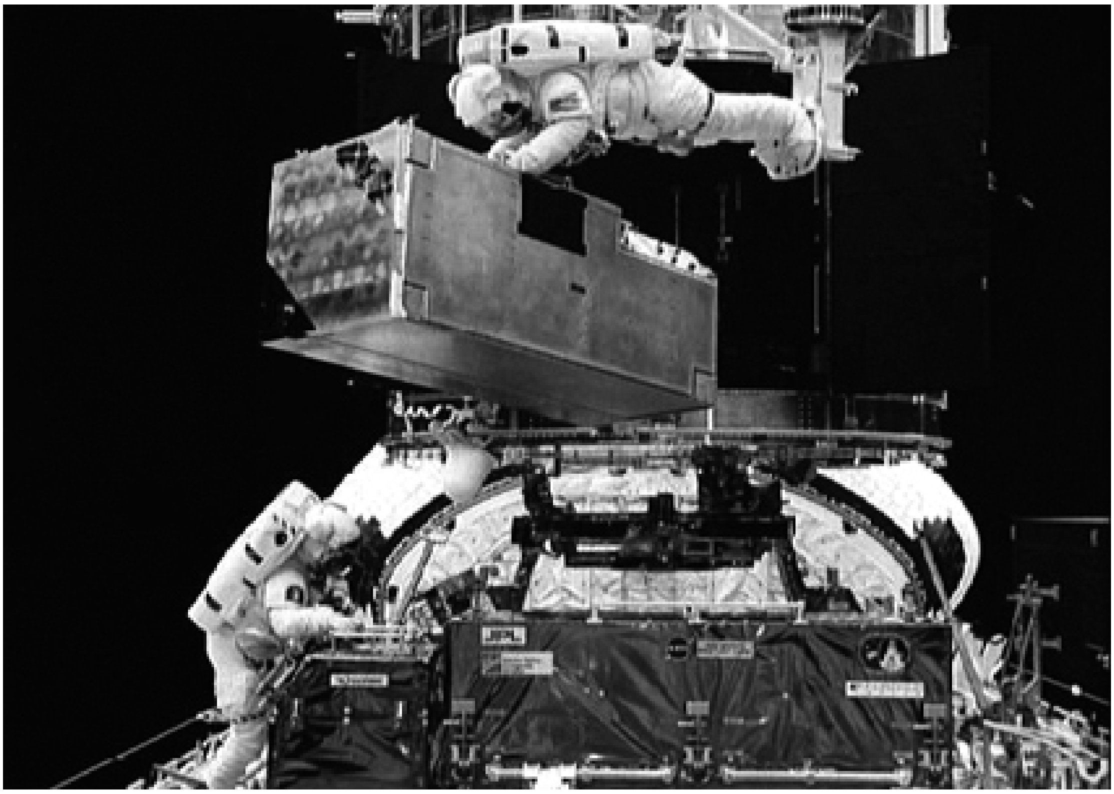

Figure 1. NASA astronaut Kathryn Thornton with the corrective optics (COSTAR) unit during the first HST servicing mission in December 1993. This and subsequent HST servicing missions demonstrated the value of astronauts in maintaining and upgrading astronomical instruments in space (NASA).

Figure 2. Astronauts of STS 113 install the 13.7m long P1 truss on the International Space Station in November 2002. In the future, this kind of large-scale construction experience will pave the way for the building of large astronomical facilities, and other pieces of scientific infrastructure, in space (NASA).

2011, to conceptual designs for large space-based interferometers such as *Darwin* and *Terrestrial Planet Finder* (TPF) which are intended to search for Earth-like planets around other stars. It is currently intended to locate many of these instruments at the second Sun-Earth Lagrange point (L2), 1.5 million km from the Earth, which is in many ways an ideal location for space-based optical and infrared telescopes (Lester *et al*., 2004). The HST experience teaches us that the operational lifetime, and scientific productivity, of these instruments are likely to be enhanced if a human spaceflight infrastructure exists which is able to maintain and upgrade them. However, this will require a significant enhancement of present human spaceflight capabilities (e.g. Huntress *et al*., 2004).

An alternative location for some future space telescopes might be the lunar surface. Although perhaps not quite as good as L2 for some astronomical instruments (Lester *et al*., 2004), the Moon nevertheless remains a very good astronomical site (much better than the surface of the Earth, or even low Earth orbit), and the lunar far-side is probably uniquely suited to long-wavelength radio astronomy (e.g. Burns et al., 1990; Benaroya, 1995). Moreover, the lunar environment, and especially the lunar southpole, may actually confer positive advantages for large, cryogenic, infrared telescopes; to quote Angel (2003):

> “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…..”

Lunar telescopes might become especially attractive from an operational point of view if a human-tended infrastructure were to be developed on the Moon in support of lunar exploration (see below). Something similar can now be seen with the ISS – although many astronomers opposed construction of the ISS, now that it exists as a piece of infrastructure, astronomical uses for it are beginning to be suggested (e.g. Parmar, 2001). Thus, if a lunar base is ever established, the Moon may actually 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. Of course, lunar and space-based observatories are not mutually exclusive, and both may have important roles in the future of observational astronomy, but as noted by Lester *et al*. (2004):

> “whether on the Moon, or in free space, it is likely that the largest and most ambitious [space] observatory facilities will require hands-on attention from astronauts.”

We should also bear in mind the possible role of a human spaceflight infrastructure in assembling astronomical instruments in space that are larger and more complicated than anything that could be launched and deployed automatically. I think it is too-little appreciated how much experience in space construction is being gained through the assembly of the ISS. Some sense of it may be gained from Figure 2, which shows the crew of STS 113 attaching the 13.7m long P1 truss to the ISS in 2002. Once such experience has been gained on the ISS it is potentially available for the construction of large space-based telescopes and interferometers (either at L2 or on the Moon), and for the construction of other large items of space infrastructure likely to facilitate the exploration of the universe. Examples of the latter might include vehicles for transporting people and supplies around the inner solar system, and the construction of scientific outposts on planetary surfaces, the scientific case for which we will consider next.

2.3. PLANETARY EXPLORATION

The Apollo programme clearly demonstrated the scientific value of astronauts as explorers of planetary surfaces (e.g. Wilhelms,1993; Harland, 1999). The rich scientific legacy of Apollo to planetary science has already been alluded to, and summarised by Heiken *et al*. (1991), Wilhelms (1993), Taylor (1994) and Spudis (1996), and compelling scientific reasons for a human return to the Moon can be identified (e.g. Taylor, 1985; ESA, 1992, Spudis, 2001, Crawford, 2004a). These include the recovery of ancient galactic and solar wind particles (Spudis, 1996; Wieler *et al.,* 1996), and meteorites blasted of the surfaces of early Earth, Mars and Venus (Armstrong *et al*., 2002), from buried palaeoregolith layers. They also include a better calibration of the lunar (and hence terrestrial) impact cratering rate, a better understanding of impact cratering processes, and a range of geological and geophysical investigations (see discussion by Crawford, 2004a). Many of the arguments for human exploration of the Moon also apply, with some modifications, to the future exploration of Mars (e.g. Spudis, 1992; Crawford, 2004b), but with the added dimension of searching for evidence of past or present life on the planet (e.g. Hiscox, 1999; 2001).

As discussed by J. Garvin and C. Cockell in their contributions to this volume, humans bring speed, agility, versatility and intelligence to exploration in a way that robots cannot. Although it is true that humans will face many dangers and obstacles operating on other planets, mostly due to their physiological limitations when compared to robots, the potential scientific returns (resulting from rapid sample acquisition, the ability to integrate widely disparate data and past experience into a coherent picture, and the on-the-spot ability to recognise observations to be of importance even if they relate to phenomena not anticipated in advance) is more than sufficient to justify employing astronauts as field scientists on other planets.

Rather than reiterate all these arguments here, I will instead illustrate the value of astronauts as field geologists with a single example from the Apollo missions. Figure 3 shows the landing site of Apollo 17 in the Taurus-Littrow Valley on the south-east shore of Mare Serenitatis. The valley lies between two large mountain blocks (the North and South Massifs), and is approximately 8 km wide. The Apollo 17 Lunar Module (LM) landed close to the centre valley, near a prominent cluster of small craters. During their three days on the lunar surface, the two astronauts (Gene Cernan and Harrison Schmitt) conducted three traverses with the lunar roving vehicle (LRV) as indicated in Fig. 3. Each EVA was just over 7 hours long, resulting in a total time spent outside the LM of 22.1 hours (the first traverse covered a shorter distance than the others owing to the time required to deploy the LRV, lay out the surface science experiments, and obtain a 3m deep drill core). The total distance traversed was 35 km, and a total of 110 kg of rock and soil samples were collected and returned to Earth (e.g. Heiken *et al.*, 1991; Wilhelms, 1993).

As had been anticipated in advance, the Taurus-Littrow valley turned out to be a geologically diverse locality (Fig. 3). The valley floor consists of a basaltic fill, which flooded the Serenitatis Basin approximately 3.75 billion years (Gyr) ago. The North and South Massifs are anorthositic highland blocks that were uplifted by the Serenitatis impact, approximately 3.9 Gyr ago. Samples were obtained from both these units. In addition, a number of interesting serendipitous geological discoveries were made, two of which are highlighted in Fig. 3. Firstly, there is the famous ‘orange soil’, discovered close to base of the South Massif, and which turned out to be a deposit of 3.6 Gyr pyroclastic glass. The second example is the course-grained troctolite sample (Apollo sample 76535) found close to the base of the North Massif, which is often considered to be one of the most interesting Apollo rock samples – it represents material from a very ancient (*c*.4.3 Gyr old) magnesium-rich

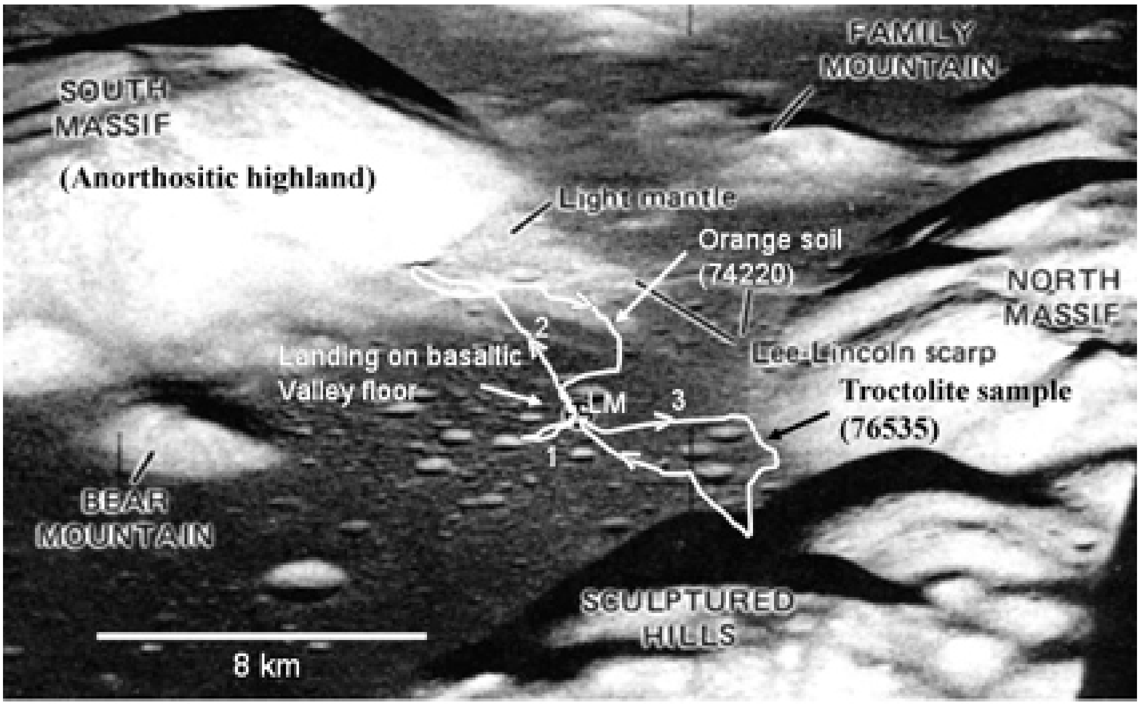

Figure 3. The Apollo 17 landing site in the Taurus-Littrow valley. The three traverses with the LRV are indicated, as are some of the locations from which geologically important material was recovered (Apollo sample numbers in brackets). Note that whereas the crew of Apollo 17 conducted these traverses in three days, in a whole year of operation on Mars *Spirit* covered a distance of about 4 km, or only half the width of this valley (background image courtesy of NASA).

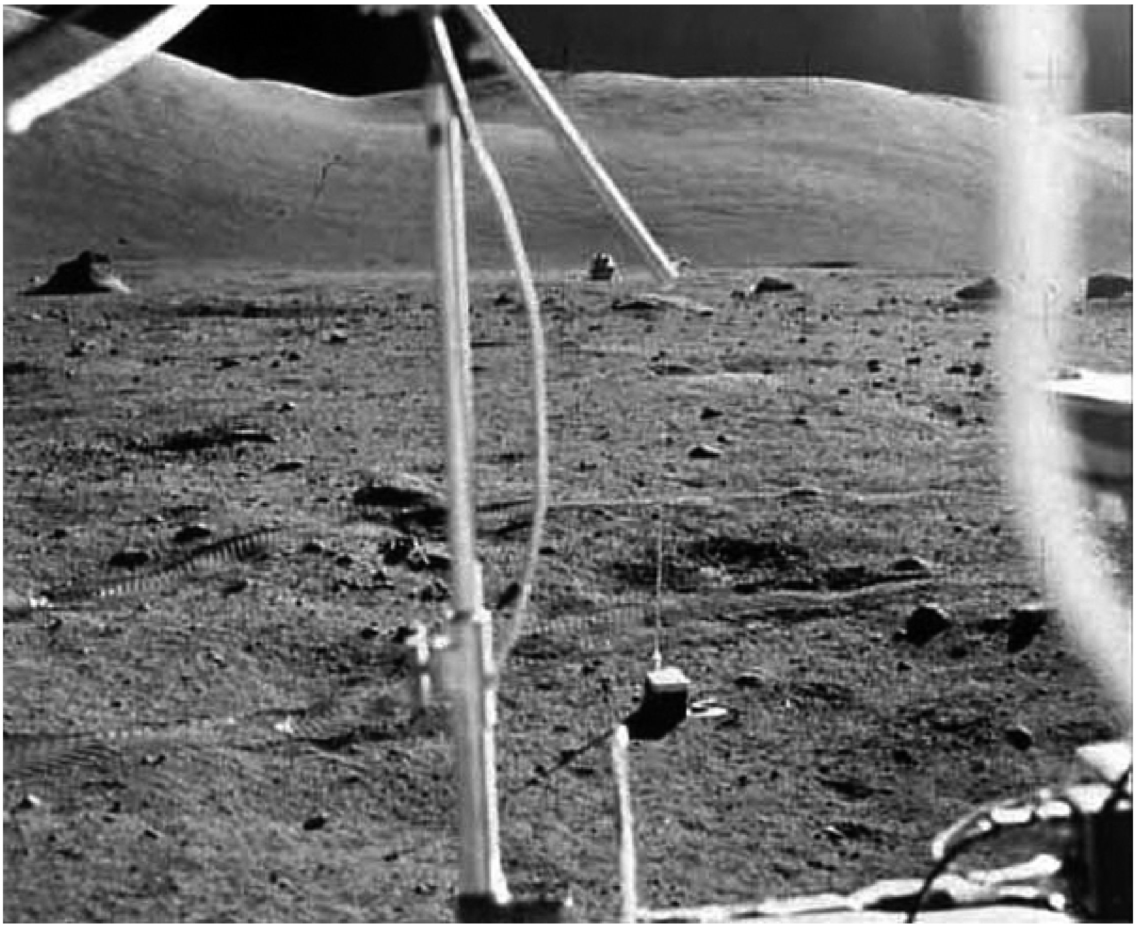

Figure 4. One of 8 explosive packages (black box with a white lid and aerial in the foreground) deployed in the Taurus-Littrow valley as part of the Apollo 17 active seismic profiling experiment. This view is from the LRV towards the Sculptured Hills (see Fig. 3). The lunar module, where the seismic array was set up, is visible in the distance (NASA).

igneous intrusion into the original anorthositic lunar crust. All these samples, together with those obtained at the other Apollo landing sites, have added greatly to our knowledge of the origin, and subsequent geological evolution, of the Moon (e.g. Heiken *et al*., 1991; Wilhelms, 1987, 1993; Canup *et al*., 2000).

It is instructive to compare the speed, and relative thoroughness, of the Apollo 17 exploration of the Taurus-Littrow valley with what could have been achieved using small-scale robotic rovers of the *Spirit* and *Opportunity* type. During its first 330 days of operation on Mars, *Spirit* traversed a total distance of just 3.9 km, which may be compared with the 35 km covered in three days by the Apollo 17 crew. As is apparent from Fig. 2, a *Spirit*-type rover deposited in the middle of the Taurus-Littrow valley would not have moved off the basaltic valley floor in a whole year's worth of operation, and would therefore not have approached the interesting geological localities around the Massifs. Moreover, while in one year *Spirit* remotely determined the approximate major element geochemistry of perhaps a dozen rocks, the Apollo crew was able to collect, and return to Earth for more detailed analysis, 471 discrete samples having a total mass of 110 kg. In addition, in their 22 hours on the surface, the Apollo 17 astronauts obtained a 3m deep core sample of the regolith, measured the lunar heat flow by sinking thermocouples about 2 m below the surface, deployed 8 explosive packages around the Taurus-Littrow valley as part of an active seismic profiling experiment (Fig. 4), measured the local gravity field using a traverse gravimeter, measured the mechanical and electrical properties of the lunar regolith, and performed a number of additional surface experiments (Heiken *et al.*, 1991; Wilhelms, 1993) -- all in just three days of field work!

Comparing the three days spent exploring the 8 km-wide Taurus-Littrow Valley with the 330 days spent by *Spirit* exploring just 3.9 km of the floor of Gusev crater, there can be no doubt that human exploration is not only orders of magnitude more efficient than robotic exploration, but that astronauts can accomplish exploration goals that are just not possible using robots. Given the tremendous technical success of *Spirit* and *Opportunity* it seems harsh to point out their limitations, but the truth is that had human crews landed at those sites on Mars they could have accomplished all that rovers have done in a year in a single afternoon. Moreover, many of the scientifically most interesting localities on Mars (such as scarps at the edge of the polar ice deposits, and the floors and walls of outflow channels) are characterised by steep slopes and rugged terrain that are not readily accessible to robotic exploration. It is at just such locations where the versatility and experience of human explorers will come into their own. As pointed out by NASA’s Chief Scientist, Dr Jim Garvin (see his contribution to this volume), these considerations imply that human exploration may actually be *less* expensive than robotic exploration. For example, *Spirit* and *Opportunity* cost approximately $1bn, whereas a human mission to Mars might cost $100 bn. However, if human exploration can be shown to be more than a hundred times as capable and efficient, then its cost ‘per discovery’ will be less – and some discoveries may be impossible using robots anyway.

2.4. A HOLISTIC VIEW OF THE SCIENTIFIC BENEFITS OF HUMAN SPACEFLIGHT

The above discussion indicates that human space exploration stands to enhance human knowledge across a number of fields simultaneously. Gathering such knowledge is the primary aim of science, but science's compartmentalisation into isolated academic disciplines tends to obscure the overall strength of the scientific case. This is especially evident when individual disciplines attempt to conduct 'cost-benefit' analyses of the issue solely from their own perspective, and relying solely on their own particular expertise. In fact, simplistic ‘cost benefit ‘analyses conducted from the point of view of any single discipline are essentially meaningless, as the same 'costs' (say of establishing a lunar base) may confer simultaneous 'benefits' in areas as diverse as lunar geology, observational astronomy, materials science, cell biology, and human physiology and medicine. Any consideration of the scientific arguments for human space exploration must therefore take a holistic view, and seek to identify potential synergies across the widely disparate research fields identified above.

3. The Social Case

Despite the strength of the scientific arguments for human space exploration outlined above, it is important to realise that science is only one thread in a much larger overall case for human spaceflight. Other threads include the economic (e.g. enhanced employment in key industries, and the resulting positive multiplier effect on the wider economy); the industrial (e.g. the development new skills and innovative technologies likely to have wider applications); the educational (particularly the inspiration of young people into science and engineering); the geopolitical (especially the opportunities for, and encouragement of, peaceful cooperation between nations); and the cultural (i.e. the stimulus to art, literature and philosophy, and a general enrichment of our world view, that inevitably results from expanding the horizons of human experience). Some of these wider issues are explored in more detail below.

3.1. THE ECONOMIC AND INDUSTRIAL CASE

Human spaceflight is technically very demanding, and this is indeed one of the reasons why it is so expensive. However, for this very reason, engaging in human space activities must necessarily act as a stimulus for employment, skill development, and technical innovation in the participating industries. This expansion of technical capabilities is likely to find applications in other areas of the wider economy. Note that this would be true even if the research actually conducted in space did not itself yield economic benefits, whereas in fact some industrially beneficial applications of microgravity research can already be foreseen (Section 2.1; see also the review by Sprenger, 2001).

When considering the potential economic benefits of ambitious space projects, it is also necessary to consider the beneficial multiplier effect on the wider economy resulting from employment in key industries. Human space exploration may be expensive, but the money itself does not leave the Earth. Rather, it stays on the ground where it can help stimulate economic activity. A detailed study of the wider economic effects of space expenditure was performed by Bezdek & Wendling (1992), who traced the influence of NASA's 1987 procurement budget of $8.6 billion dollars on the US economy. They found that this public expenditure generated $17.8 billion in industrial turn-over (i.e. an economic multiplier of 2.1), and created 209,000 private sector jobs. All this economic activity raised $5.6 billion in federal, state and local taxes, so the net public expenditure was actually only $3 billion. An important result of this study was that while the initial beneficiaries of NASA procurement were the large aerospace companies, much of the economic benefits filtered down through layers of sub-contractors to the industrial heartland of America. As noted by Bezdek and Wendling in the conclusion of their study:

> "Many workers, industries and regions benefit substantially, and these benefits are much more widespread …. than has heretofore been realised. We believe our results imply that the economic benefits and costs of space exploration need to be reassessed."

3.2. THE GEOPOLITICAL CASE

Space exploration provides a natural focus for international cooperation, as indicated by the collaboration of some 15 nation states (sadly excluding the UK) in the construction and operation of the ISS. In trying to build a stable geopolitical environment on Earth, it must be desirable to increase the range and depth of such collaborative endeavours. Human space exploration is especially, and perhaps uniquely, well-suited to enhancing a sense of global solidarity owing to its globally high media profile, and to the extraterrestrial perspective on human affairs that naturally follows from it. From this point of view, it is highly desirable that the international collaborative framework developed to build the ISS be maintained and extended beyond the lifetime of that project. International human space missions to the Moon and Mars would perfectly satisfy this requirement, in addition to yielding the scientific and industrial benefits discussed above.

The geopolitical importance of supporting and managing the international aerospace industry should also not be underestimated. These industries are economically and politically important, directly employing well over half a million people in the US (and over 100,000 in the UK), and many more people are employed in supporting industries. No government can afford to see these industries run down. Unfortunately, however, when not engaged in making spacecraft, these industries are usually employed in making high-tech weaponary, much of it for export to unstable parts of the world. For both political and ethical reasons it is desirable to identify non-military activities for these industries, and constructing the hardware for space exploration is an obvious, and perhaps the *only* obvious, candidate. Indeed it was in part to provide such an alternative to the aerospace industry of the former Soviet Union that Russia was invited to join the ISS project in 1993 (e.g. Harland & Catchpole, 2002), and this policy appears to have been at least partially effective (Logsdon & Millar, 2001). A recent report on UK science policy (Langley, 2005) has likewise drawn attention to the extent that UK science and technology is disproportionately focussed on weapons-based research, with 31% of the country’s research and development budgets being spent on military projects. Again, increased involvement in space exploration would provide an alternative, while maintaining employment and innovation in the industries concerned – after all, had the UK not opted out of ESA’s contribution to the ISS, UK companies such as BAE Systems (formerly British Aerospace) could have been devoting more of their business to building space station components and less to selling weapons abroad -- this would appear to be both socially and ethically desirable.

3.3. THE EDUCATIONAL CASE

Space exploration is inherently exciting, and as such is an obvious vehicle for inspiring the public in general, and young people in particular, to take an increased interest in science and engineering. This was explicitly recognized in the conclusions of the UK Microgravity Review Panel:

> "We have also found considerable public interest in activities in space, particularly those that have human involvement…. This is important in addressing the need for future students to study science and technology subjects and in engaging the public in scientific issues." (Wakeham *et al*., 2003).

Although these arguments have so far fallen on deaf political ears in the UK, they are recognized to be of importance in other industrial economies. As Representative Jim Bacchus put it during a debate on the future of the space station in July 1992:

> "School children need something more than their parent's prodding ways to encourage them to study and learn, to look to the future …. They need to be inspired…. and the space station can be that inspiration…." (Bacchus, 1992).

Such inspiration must be of value to any modern, knowledge-based economy, especially at a time when the number of young people opting for careers in science and engineering is falling. And if the ISS can be inspiring (and I for one believe that it is), consider how much more so will be the really exciting exploratory human space missions to the Moon and Mars that are now within our grasp. Carl Sagan put this most eloquently is his penultimate book, *Pale Blue Dot* (1994):

> "Exploratory spaceflight puts scientific ideas, scientific thinking, and scientific vocabulary in the public eye. It elevates the general level of intellectual inquiry."

We all have the greatest possible interest in encouraging this process.

3.4. THE CULTURAL CASE

Beyond the more utilitarian arguments for human space exploration discussed above, is it also possible to identify less tangible cultural benefits? It seems to me that a sense of purpose and achievement must be important for the well-being of any society, and that a vibrant culture requires continually renewable sources of intellectual stimuli. In a celebrated 1989 essay, followed by a best-selling book, the American political philosopher Francis Fukuyama painted a bleak picture of a ‘post historical’ world lacking new sources of cultural inspiration:

> “… daring, courage, imagination and idealism will be replaced by economic calculation, the endless solving of technical problems, and the satisfaction of sophisticated consumer demands. In the post-historical period there will be neither art nor philosophy, just the perpetual caretaking of the museum of human history” (Fukuyama, 1989).

Although Fukuyama’s analysis was much criticised at the time, some of the trends he identified are all too apparent in our contemporary global civilisation. It may be overstating the case to claim that space exploration is the *only* means of avoiding the kind of cultural stagnation predicted by Fukuyama, but it is surely one way of providing a sense of adventure, and an influx of intellectual and cultural stimuli, for an increasingly constricted and finite world (Crawford, 1993).

Moreover, there are reasons for believing that as a species *Homo sapiens* is genetically predisposed towards exploration and the colonisation of an open frontier (Gamble, 1993). Access to such a frontier, at least vicariously, may be in some sense psychologically necessary for the long-term wellbeing of human societies. It is important to note that this is not merely a western predisposition, but a *human* one – one that led to the human colonisation of the entire planet following our evolution as a species in a geographically restricted corner of Africa (Gamble, 1993). Fukuyama is not the first philosopher to draw attention to the human need for adventure, and similar arguments were advanced by William James (1910) and Bertrand Russell (1952); in the context of space exploration, Sagan (1994) put it well:

> “We are the kind of species that needs a frontier – for biological reasons. Every time humanity stretches itself and turns a new corner, it receives a jolt of productive vitality that can carry it for centuries.”

An ambitious programme of human space exploration may be one of the few options left open us in the twenty-first century that satisfies these psychological and cultural requirements. But regardless of how seriously one takes these arguments, it must be true that our horizons will be broader, and our culture richer, if we engage in the exploration of the universe than if we do not.

4. The Cost in Context

It is sometimes argued that, whatever the real or imagined benefits of human space exploration, the cost is so high as to be prohibitive (e.g. Coates, 2001). This assertion needs to be assessed critically. The current global cost of human spaceflight is approximately $9.0 bn (Euroconsult 2004). This is made up as follows (2003 figures): NASA $7.85 bn (i.e. approximately half of NASA's total budget of $16 bn is devoted to human spaceflight activities, principally the Shuttle and the ISS); ESA $0.69 bn; Japan $0.20 bn; Russia $0.12 bn; and China $0.10 bn (all figures from Euroconsult 2004; those for Russia and China are estimates). There are several points to make:

- A global level of expenditure of $9.0 bn p.a. amounts to approximately 0.025% of the Gross World Product ($36.4 trillion in 2003; World Bank, 2004). Thus, by any objective standard human spaceflight is affordable.

- To reinforce this point, the US contribution to human spaceflight costs American citizens approximately $25 per person per year. In Europe (of course excluding the UK) ESA's human spaceflight activities cost about €2 per person per year – about the price of a cup of coffee!

- As noted in Section 3.1, the money invested in human spaceflight does not itself fly off into space. Rather, it is spent on the ground, where, as demonstrated by Bezdek & Wendling (1992), it circulates in the economy with socially and economically beneficial consequences.

- Compared to many other things that governments spend public money on, the costs of human space exploration are modest. For example, NASA's human spaceflight budget of about $7.8 bn is only 1.8% of the US military budget ($446 bn in 2003; SIPRI,

> 2004). This mismatch between space and military spending is long-standing, and is perhaps most clearly illustrated by the fact that the entire Apollo project only cost one-seventh as much as the, more or less contemporaneous, Vietnam War (Wilhelms, 1993).

Given the multiple scientific, economic and social benefits identified above, the present level of investment in human spaceflight doesn't appear to be excessive, and somewhat higher levels may be politically possible, as well as scientifically and socially desirable.

5. The Anomalous Case of the UK

The United Kingdom is the only major industrialised economy that has consistently refused to participate in human spaceflight, and the reasons for this anomalous situation need to be addressed. Present UK government thinking on the subject was spelt out by the Science Minister, Lord Sainsbury, in a speech at the Royal Society on 17 October 2001:

> "We also do not intend actively to participate in manned exploration of the Solar System. This is because we are not convinced that the benefits of human exploration go beyond the political and cultural into the scientific and commercial … We require a solid justification rooted in science or commercial arguments before supporting any human spaceflight programme."

This is an interesting, if rather muddled, justification for present policy. It acknowledges that "political and cultural" benefits of human spaceflight exist (which, as we saw in Section 3, is demonstrably true), but it implies that these are not in themselves sufficient to justify spending money on it. Given how important some of these benefits could prove to be (e.g. the reinvigoration of the UK aerospace industry, and the stimulus to scientific and technical education), and how relatively modest the proposed costs (Section 4), this seems a rather strange conclusion to draw. Instead, Lord Sainsbury's statement attempts to justify UK policy regarding human spaceflight by its alleged lack of "scientific and commercial" justifications. But, as we have seen, there are in fact clear scientific (Section 2) *and* commercial (Section 3.1) benefits of having people in space. Thus the stated reasons for UK government policy are unconvincing, and the policy itself should perhaps be re-examined. As noted by the independent 'Cross-Council Report' on UK participation in ESA's Aurora programme (Holdaway, 2004):

> “[G]iven the potential scientific and social benefits of human spaceflight …. there may be a case for asking the government to review its position in this respect.”

There is, of course, no suggestion that the UK should develop its own, independent, national human spaceflight programme. Rather, as a full member of the European Space Agency, the UK should aim to play a role in ESA’s existing programme commensurate with the size of its economy. The UK has the fourth largest economy in the world (World Bank, 2004), and the second largest among ESA member states, and as such it should be pulling its weight. This would mean the UK contributing something like 17% of ESA’s human space activities – perhaps €120 million p.a., or about €2 per head of population. Moreover, under the ESA principle of ‘juste retour’, most of this money would in any case be invested back in UK, thereby stimulating UK industrial innovation and protecting UK jobs. Given that it would also be inspiring UK school children, advancing UK science on many fronts simultaneously, and making a positive contribution to international cooperation, this level of contribution really ought to be supportable politically – but it will require politicians, and those who advise them, to engage objectively with the totality of argument, which is not happening at present.

6. Conclusion

I have argued that human spaceflight has the potential to advance human knowledge in a number of areas simultaneously. The space life sciences, and especially human physiology and medicine, require people in space because people form the experimental subjects for this research. In addition, astronomy will benefit from access to a human spaceflight infrastructure able to construct, maintain and upgrade large space-based instruments, and the fields of planetary geology and astrobiology stand to benefit from the versatility of human beings working as field scientists on the Moon and Mars. The latter activities will of course rely on former – field geology requires active, healthy people operating on planetary surfaces, which means that the effects of the space environment on human physiology must be fully understood and appropriate countermeasures devised. As noted by Paul Spudis, in his contribution to this volume, science both enables, and is enabled by, human space exploration. As the scientific benefits will accrue to several different scientific fields simultaneously, it is essential that judgements of the scientific usefulness of human space exploration are made for an interdisciplinary perspective. Moreover, the benefits of human space exploration are far from being restricted to science. Rather, as argued in Section 3, a wide array of potential economic, industrial, political, educational, and cultural benefits can also be identified. Any responsibly formulated public space policy must take a holistic view, and weigh the totality of the scientific and non-scientific arguments together, before deciding whether or not an investment in human spaceflight is worthwhile.

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Discussion

Professor Neville Brown (Oxford University): Is there not a danger of microbial infection being brought back from Mars as a result of human missions?

Dr Crawford: This is an important question which needs to be addressed seriously. While the likelihood of back contamination appears small, the consequences of it happening would be so serious that appropriate safeguards will have to be devised in advance. Of course, for both scientific and ethical reasons, we will also want to avoid the biological contamination of Mars, at least until we know whether or not it possesses an indigenous biosphere. All this goes to show, I think, that attempting to send people to Mars in the near future may be premature – there is still much we need to learn about Mars, and also a lot to learn about the physiological and psychological effects of long-duration spaceflight, before we could responsibly send people there. My own view is that we should continue to explore Mars robotically (including robotic sample return) for the next several decades, while at the same time developing, in parallel, a human spaceflight infrastructure focussed initially on lunar exploration. There may then be a chance that, sometime before mid-century, the former will have provided the detailed knowledge of the martian environment (including the presence or absence of near-surface indigenous life), and the latter the human spaceflight expertise (in addition to a lot of knowledge about the Moon in its own right), that will be required to make human missions to Mars technically feasible, scientifically rewarding, and ethically responsible.

Mr Dean Talboys (University of Leicester): Won’t it be more difficult to find meteorites from ancient Earth, Venus and Mars on the Moon compared to the Antarctic ice sheet where meteorites are commonly found?

Dr Crawford: It will certainly be much more difficult to find meteorites in palaeoregoliths on the Moon than in Antarctica, where meteorites from the Moon and Mars are now found routinely. However, it is important to realise that meteorites found in Antarctica, or anywhere on the surface of the Earth today, are not very old. Typically they have resided on the Earth for some tens of thousands of years, and have cosmic ray exposure ages (i.e. the time since they were knocked off their parent planet) of a few tens of millions. Thus, they represent samples from the *contemporary* solar system. The importance of looking for meteorites from other planets on the Moon is that many of these are likely to be *billions* of years old. Thus, in principle we might find samples from the early Earth that pre-date any surviving terrestrial rocks, we might find samples blasted of Mars when that planet was warmer and wetter than it is today, and we might even find samples of the pre-greenhouse Venus. These would be enormously important finds. Of course, finding them will be very difficult, and I think will require the kind of large scale exploratory fieldwork that would best be conducted from a lunar base – I do not believe it will be done using small-scale robotic rovers.