THE SCIENTIFIC CASE FOR A HUMAN SPACEFLIGHT INFRASTRUCTURE

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Abstract. I argue that science stands to benefit from the infrastructure developed to support a human space programme. By infrastructure I mean all those facilities and capabilities which purely scientific budgets could never afford to develop, but which nevertheless act to *facilitate* scientific research which would not otherwise take place. For example, the human presence on the Moon during the Apollo Project resulted in the acquisition of scientific data which would not have been obtained otherwise, and the same is likely to hold true for future human missions to both the Moon and Mars (and indeed elsewhere). In the more distant future, an important scientific application of a well-developed human spaceflight infrastructure may be the construction of interstellar space probes for the exploration of planets around other nearby stars.

Keywords: Human spaceflight, space exploration, space infrastructure

1. Introduction

Scientists have been arguing about the benefits, or otherwise, of sending people into space since the earliest days of the space age. Indeed, on the very eve of the Apollo 11 Moon landing the then Astronomer Royal, Sir Richard Woolley, gave a newspaper interview (Woolley, 1969) in which he bluntly asserted that “from the point of view of astronomical discovery it [the Moon landing] is not only bilge but a waste of money”. Moreover, the whole tenor of Woolley’s opposition to Apollo is similar to that of many leading scientists today when it comes to discussing the scientific role of the International Space Station (ISS) or future manned missions to Mars (e.g., Van Allen, 1986; Sleep, 1997; Coates, 2001; see also the contribution by A. Coates in this volume).

On the other hand, all scientists would agree that it is the job of science to reach as full an understanding of our Universe as is humanly possible. Consider the following quotation from a book by the eminent astronomer Roger Tayler (1991, p. 13):

> Astronomers have a very ambitious aim. They wish to understand the structure of the entire Universe.

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In this paper I will argue, perhaps controversially, that if we are really serious about this objective, then, in addition to building ever larger ground-based telescopes, and ever more sophisticated robot spacecraft, we will also have to invest in the infrastructure of human spaceflight.

2. The Legacy of Apollo

Given that the Apollo Project was by far the most ambitious human spaceflight project yet attempted, it is important for the present discussion to determine whether the negative contemporary assessment of Woolley and other critics has turned out to be justified. A brief summary of the actual scientific legacy of Apollo will therefore provide a perspective for discussion of future human spaceflight activities.

As is well known, the Apollo project was initiated, and financed, for geopolitical rather than scientific reasons. Nevertheless, by the time of the last three Apollo missions (the so-called J missions: 15, 16 and 17), scientific exploration had in fact become a major component of the programme (Wilhelms, 1993). The scientific legacy of Apollo has been summarised by (Taylor, 1994) and, very briefly, consists of:

1. 382 kg of lunar rock and soil samples, comprising over 2000 discrete samples and including 24 drill cores (to a maximum depth of almost three meters). The analysis, and especially the dating, of this material has had a major impact on our understanding, not only of lunar history, but of the origin and evolution of the Solar System as a whole.
2. Information on the lunar interior obtained by the Apollo seismology experiments. It is noteworthy that the Moon is still the only planetary body other than the Earth whose interior has been probed in this way.
3. Information gleaned from *in situ* measurements of the magnetic, gravity, heat flow, charged particle environment, and "atmospheric" composition at some or all of the six landing sites.

The opponents of human space exploration will argue that all this could have been achieved much more cheaply using robots. However I think this view is mistaken. While it is true that much of the Apollo science could *in principle* have been obtained robotically, there must be considerable doubt as to how much would actually have been accomplished had the manned landings not taken place. For example, although three unmanned Soviet probes (Lunas 16, 20 and 24) successfully collected a total of 321 g of lunar material in the 1970s (Heiken et al., 1991) it is notable that this was less than 0.1% of the quantity returned by Apollo. Moreover, the Apollo material consisted of many individual samples, intelligently collected from a range of locations around each landing site, while the Luna material consisted of a single sample from each site. It seems unlikely that any 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 important to realise that Apollo was able to carry a large quantity of scientific equipment to the Moon, and return with hundreds of kilograms of rock samples, because each mission had to transport three men and all their life support equipment to the Moon and back anyway, in order to satisfy the other (mainly political) objectives of the programme. Thus, the marginal cost of transporting scientific equipment and the rock samples was a negligible fraction of the total, even though purely scientific budgets would never have been able to afford it on their own. I think it is fair to say that had the Apollo missions not taken place then even now, thirty years on, we would still have obtained only a fraction of this immensely valuable scientific data. Thus, with hindsight, we can see that had the view of Woolley, and other scientific critics at the time, prevailed then our present knowledge of the Moon, and therefore of the early history and evolution of the Solar System, would be more impoverished than it actually is. This should give pause for thought to those who advocate a purely robotic exploration of other planets, and it illustrates an important truth: any space mission which has to transport people will, by its very nature, also be able to carry a significant scientific payload, *even if science is not the primary driving force behind it.*

3. The International Space Station

Today the debate has moved on, and mainly concerns the scientific role of the International Space Station (ISS), now under construction in Earth-orbit, and plans for future human missions to the Moon and Mars in the coming decades. Like Apollo before it, the ISS is being built largely for political reasons (some of which, like the encouragement of international co-operation, are *good* reasons; Logsdon and Millar, 2001), but this does not mean that science will not be a beneficiary. The short-term scientific applications of the ISS focus on materials and life science research in the microgravity environment (Freeman, 2000; see also the contributions by K. Fong and O. Minster elsewhere in this volume). It has also become apparent that the exterior structure of the ISS will provide an excellent site for certain types of astronomical observation, and several instruments designed to exploit this are already planned (see the contribution by A. Parmar in this volume). It may be true, as the critics of the ISS point out, that these research areas could never have justified the construction cost of the ISS solely on the basis of their anticipated scientific returns – although it remains true that they are important scientific disciplines which stand to benefit considerably from it.

However, it seems to me that there are three longer-term reasons why science has a vested interest in the successful construction and operation of the ISS.

1. The experience that is being gained in the construction of large structures in space. With a final mass exceeding 450 tonnes, and requiring more than 900

hours or extra-vehicular activity for its construction (Freeman, 2000), the ISS will be by far the largest and most complex structure yet assembled in space. Once developed for the ISS, this experience will potentially be available for the construction of future large space facilities, some of which (e.g., astronomical instruments and lunar or planetary outposts) are themselves likely to have a considerable scientific impact.

2. Studies of the long-term physiological effects of the space environment, which will be required before humans can venture further afield in the Solar System. In particular, I shall argue below that significant scientific benefits will follow from the human exploration of Mars. However, it is quite clear that our understanding of the long-term effects of the space environment (especially the radiation, microgravity and psychological aspects) is still not sufficient for us confidently to embark on such a venture. Although the critics of human spaceflight point to the vunerability of the human body in the space environment as a potential "show stopper" for human planetary exploration (see the contribution by A. Coates elsewhere in this volume), it seems just as likely that intensive research, which can realistically only be performed on the ISS, will result in the development of effective countermeasures (e.g., Freeman, 2000; White and Averner, 2001; see also the contribution by K. Fong in this volume).
3. Experience gained in the development of new *institutional* arrangements for the management of complex international space projects (which, again, are themselves likely to yield considerable long-term scientific benefits). The ISS is by far the largest international collaborative space project yet attempted, and if this experience helps lay the foundations for a future world space programme, perhaps along the lines suggested by Crawford (1992), that alone will be one of its most important legacies.

4. A Return to the Moon?

In the post-ISS era, two possibilities suggest themselves as further opportunities for human space exploration: a return to the Moon, and the human exploration of Mars. The scientific arguments for a renewal of human lunar exploration fall into three categories:

1. Studies of the Moon itself. The Moon is an important object of scientific study in its own right, and one that is likely to continue to yield major insights into the origin and evolution of the Solar System. The scientific case for renewed human exploration has been powerfully enunciated by Spudis (1992; see also his contribution in this volume). Although the scientific benefits of Apollo were considerable, it remains true that we have not yet achieved anything like a complete understanding of the Moon's structure and evolution. This will be especially obvious when we consider that all our lunar samples, and *in situ* measurements of any kind, have come from mid to low latitudes on

the nearside only. Thus, the scientific case for renewed lunar exploration is extremely strong, and, as demonstrated by Apollo, I argue that more will be achieved as part of a human-centered initiative (e.g., involving construction of a lunar base) than could be achieved by purely robotic means. In this context it is especially important to draw the distinction made by Spudis (1992) between the initial reconnaissance of planetary environments, to which robots are well-suited, and the detailed follow-up by specialists in the field which will be required if scientific knowledge is to be maximised.

2. Astronomical observations from the Moon. The potential advantages of the Moon as a platform for astronomical observations have been reviewed extensively elsewhere (Burns and Mendell, 1988; Burns et al., 1990; Benaroya, 1995). Briefly, these arise from the stability of the lunar surface (an advantage for the construction of long-baseline optical/infra-red interferometers); the slow rotation period of the Moon (permitting very long integration times for a given astronomical object); the extreme cold in shadowed areas (a significant advantage for infra-red instruments); and the extreme radio-quietness of the lunar farside (probably the best site for radio astronomy anywhere in the Solar System).
3. Experience gained in living and working on hostile planetary surfaces. Gaining such experience is likely to be particularly important when it comes to constructing human outposts further from home, and in particular on the planet Mars.

5. The Case for Mars

The ultimate aim of planetary science must be to understand the other planets to the same extent as we understand our own. This will eventually require a comparable degree of scientific *access* to these other planetary environments, and there are strong grounds for believing that this cannot be achieved using robotic vehicles alone. The planet Mars has a surface area approximately equal to the land area of Earth, and by all accounts has had a highly complicated geological, climatological and (possibly) biological history. The quest for anything like a full understanding of this environment will require such a range and complexity of scientific equipment (e.g., for magnetic, gravity and seismic surveys, and for drilling km-deep boreholes), and the analysis of such a large quantity of Martian materials (probably thousands tonnes), that it will far outstrip the capabilities of purely robotic exploration. Certainly this is not the way we explore our own planet, so why should we aspire to less when it comes to exploring others?

Consider the search for ancient life on Mars, probably the single most important scientific question to be addressed on the planet (see the contribution by J. Hiscox elsewhere in this volume). This will require techniques similar to those used to find the oldest microfossils on Earth (Schopf, 1993), and involve the microscopic analysis of such a large quantity of material, from so many different sites, that only *in situ* studies by human specialists are likely to be practical. And if evidence for past life *is* found, that will mark the beginning, not the end, of the new field of Martian palaeontology (Gould, 1994). The subsequent demand for additional samples, and supporting geological and environmental studies, will be considerable, and may again outstrip the capabilities of robotic exploration (just how many tonnes of material can realistically be collected robotically and sent to Earth for analysis?)

In order to gain a sense of perspective, imagine two alternative futures – both a century hence. In one, our exploration of Mars has proceeded by means of a few dozen robotic spacecraft, and has culminated in the robotic return to Earth of a few tens of kilograms of Martian samples. In the other, a permanently occupied scientific outpost has been established, staffed by dozens of human specialists (geologists, geophysicists, climatologists, biologists, ...), with their own laboratory facilities and local transportation infrastructure (e.g., rovers, and perhaps aircraft). In which of these alternative futures would we have learned the most about Mars, and after which would we be best placed to learn more? I think the answer to these questions is obvious – the human option by a long way. As Spudis (1992) has put it: “[a]lthough robots could play a significant role in gathering data, conducting science in space will require scientists”.

To reinforce this point, I would like to quote a statement of Mike Malin and Ken Edgett, principal investigators for the Mars Orbital Camera on board the Mars Global Surveyor spacecraft (quoted by Sawyer, 2001, p. 51):

> 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. ... It’s unusual to hear people like us argue for manned space exploration. But for about two years now [we] have been absolutely convinced that we’re going to have to send people there.

Given this considered, and informed, opinion of two of the world leaders in the current *robotic* reconnaissance of Mars, I am prepared to rest my case.

6. Space Infrastructure

The main point I wish to make in this paper is that science stands to benefit very greatly from the *infrastructure* developed to support a human space programme. By infrastructure, I mean all those facilities and capabilities (e.g., launch vehicles, astronauts, space stations, 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. One recent example of the scientific usefulness of a human spaceflight infrastructure is the in-orbit repair of the Hubble Space Telescope in 1993, without which astronomy would have been significantly worse off and key discoveries made subsequently would have been impossible. We have seen how the ISS will provide infrastructural support for a range of scientific investigations, and how a human presence on the Moon during the Apollo Project resulted in the acquisition of scientific data which would not have been obtained otherwise. There is every reason to expect that the same will hold true for future human missions to both the Moon and Mars (and indeed elsewhere), especially if these involve the establishment of permanently occupied scientific facilities.

Perhaps a down-to-earth example will illustrate the unconscious, but nevertheless essential, reliance of the scientific community on infrastructure developed for other purposes. My astronomical research often takes me to the Anglo-Australian Telescope. I typically travel to and from Australia on a Boeing 747 aircraft, costing somewhere in the region of $180 million. The UK astronomy budget could not afford to buy this one aeroplane, never mind the vast fleets of similar aircraft flying around the world, or the global air traffic support infrastructure which exists to service them. And, of course, astronomy budgets would never have been able to finance the development of such a vehicle in the first place. Indeed, it is amusing to imagine the likely response of senior astronomers in the past had any such suggestion been made to them (perhaps something like “From the point of view of astronomical discovery civil aviation is not only bilge but a waste of money . . . ”). And yet here we are, absolutely dependent on this infrastructure for whatever astronomical discoveries we may make. By analogy, we may imagine a geologist in the next century making the long trip to a well-established Mars base for the purpose of six month’s field work. Her research budget could never have afforded either the spaceship on which she travels, or the facility that is her destination, but both will be central to whatever geological discoveries she may make.

I would like to conclude this discussion of a space infrastructure with a leap into the more distant future. I suggest that an important long-term scientific application of a well-developed human spaceflight infrastructure may be the construction of interstellar space probes (capable of travelling at, say, ten to twenty percent of the speed of light), for the exploration of the planets recently discovered around other nearby stars. We already know that we need spacecraft for the exploration of the planets of our own Solar System, so it seems clear that we will eventually require spacecraft to study these other planetary systems also. However, the scale of the undertaking, and the highly energetic (and therefore potentially dangerous) energy sources that will have to be employed (Mallove and Matloff, 1989; Crawford, 1990; Leifer, 1999), means that the construction of even robotic interstellar probes will have to take place in space. The potential long-term scientific benefits of interstellar space probes are nothing short of awe-inspiring, but it is important to realise that such vehicles will only be possible once a significant space infrastructure has been developed.

7. Science Education

Closer to home, we should also reflect on the *educational* value of high-profile human space activities. As noted by the late Carl Sagan (1994):

> Exploratory spaceflight 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.

8. The Cost in Context

It is true that human spaceflight is expensive. However, even this should be seen in context: NASA's annual $14 billion budget is only 5% of the US military budget ($283 billion in 1999; see SIPRI, 2001), and the whole Apollo programme cost only one-seventh as much as Vietnam War (Wilhelms, 1993, p. 345). Globally, civilian space activities (dominated by, but not restricted to, human space projects) consume of the order of 0.1% of the Gross World Product of $29 trillion (World Bank, 2001), compared with the approximately 3% devoted to military procurement. Thus, by any objective standard, human spaceflight activities are strictly affordable. And, of course, all this money is actually spent on the ground, where it has a beneficial multiplier effect on the wider economy (Bezdek and Wendling, 1992).

Nevertheless, the high cost of human space exploration does mean that it will always be undertaken for a complex range of reasons, most of which are social and political in nature, rather than narrowly scientific (e.g., Logsdon, 2001). Indeed, it is precisely because of the existence of these socio-political imperatives that there can be no simple trade-off between resources spent on human spaceflight and those devoted to robotic missions. If NASA was not flying the Shuttles, or leading the construction of the ISS, it is most unlikely that it would still have a $14 billion budget to spend on space science. Coates (2001) has argued that for the cost of the ISS "about 600 unmanned missions to Mars could be built". For all I know this may be true, but it is naive in the extreme to imply that if the ISS were cancelled this would be a likely consequence.* The fact is that the construction of hundreds of robot probes would not satisfy the same political and industrial constituencies as does a major human spaceflight project like the ISS. It follows that any resources freed by its cancellation would more likely be diverted to other sectors of the economy, with military procurement (which *does* satisfy many of the same constituencies)

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[*] And in any case, for the reasons given above, it may be doubted whether even "600 unmanned missions" would tell us as much about Mars as would a permanently occupied Martian outpost, the establishment of which is likely to rely on experience gained with the ISS!

and/or tax cuts perhaps being the most likely politically. It is therefore fortunate that a range of socially beneficial political drivers for human spaceflight in the coming century, ranging from the encouragement of international cooperation to the utilisation of extraterrestrial raw materials, can already be identified (Crawford, 1995).

Regardless of these wider issues, it seems clear that science has advanced, and will continue to advance, as a result of having people in space. Developing a sustainable, and an *international*, human spaceflight infrastructure is a noble goal for the twenty-first century, and one which deserves the full support of the scientific community.

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Discussion

A member of the audience: Could you comment on the role of the private sector in developing space infrastructure?

Dr. Crawford: I agree that there is potentially a private sector opportunity, and that private investment can play a role in the exploration of space. But I do think that it is properly government's job to provide infrastructure. Private capital will not be invested if the perceived risk is too great, so it is government's job to provide infrastructure up to the level that private capital feels it can build on.

Mr. Horace Regnart: Something that should be discussed is the argument that the money would be better spent on problems on Earth, relating to health, poverty, education, and the environment. There is an answer to this argument in five categories. Briefly: (1) human spaceflight can be considered as a cultural endeavour; (2) there are technical and economic benefits; (3) there are fundamental scientific benefits; (4) there is the benefit of long-term insurance for the human race against, for example, bolide impacts; and (5) there are the benefits arising from international cooperation. On the last point, scientists are generally successful at international cooperation, and so an international scientific cooperative project is particularly likely to succeed, and likely to set a good example of what international cooperation can do. If we don't succeed at international cooperation it is unlikely that our species will survive on spaceship Earth.

Dr. Crawford: Well, I agree entirely, and especially with your last point. I certainly think that stimulating international cooperation is the primary *geopolitical* driver for human spaceflight. The previous speaker mentioned that there are more nations collaborating on the Cassini mission than on the Space Station, but if you are interested in forging a spirit of solidarity amongst nations on the Earth it is not quite as simple as adding up the nations you've got on your project – what matters is global visibility. Humans in space have a very high profile, and having astronauts of different nationalities working together in a challenging environment that's constantly in the media eye is a much better way of building a sense of global solidarity. The Space Station is ideal for that, and a human mission to Mars would be even better.

Dr. Julian Osborne (University of Leicester): Is their a political danger from an environmentalist view of talk of the industrialisation of the Moon or Mars?

Dr. Crawford: Personally, I think it depends which environment we are talking about. I am less worried about the Moon than I am about Mars. I certainly agree that we have to be very careful about not contaminating the Martian environment until we have done enough work to ensure that there is no indigenous ecosystem on the planet. Otherwise contamination could be scientifically, and indeed ethically, disastrous.