Chapter 10
Afterword

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Space exploration and the search for a better understanding of life have never been entirely separate from one another. This is not simply a matter of policy, a decision by political administrations to combine the two. Rather, it is a matter of the ways in which both draw upon the same scientific culture and upon overlapping societal influences. Some of the latter are the political influences of particular times and particular places, others are of a far broader nature. Progress in one field has tended to be combined with advances in the other. It is a familiar point that, in the very year that NASA was founded, i.e. 1958, the American molecular biologist Joshua Lederberg won a Nobel Prize for his discovery that bacteria can exchange genetic material (a process now known as 'bacterial conjugation'). This, in turn, was only six years after the classic Miller-Urey experiment to replicate the production of some of the chemical precursors of life. And the Miller-Urey experiment, in turn, overlapped in time with the work of Watson, Crick and Rosalind Franklin in England, on the structure of DNA. Major breakthroughs came in both fields (activity in space and research into life) within the same time-frame, and drew upon at least some of the same background influences and interests. When Lederberg went on to work with NASA on the early programs to look for life on Mars, the progression was, in an everyday sense, natural. Interest in space and interest in life went together. They have always tended to do so.

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What motivates both, and justifies a good deal of scientific expenditure at a state and international level, is an overlapping set of human concerns: about where we come from, where we might go and whether or not we can assume that life exists only here. The familiar formal definition of astrobiology, the definition used in the White Paper above ('the scientific study of the origins, evolution, and distribution of life') reflects these concerns and provides a more precise framework for research and for collaborative activities. How quickly this research, and these activities, might proceed is a different matter. In order to be effective over more than the short term, proposals for institutional initiatives which are geared to significantly advance research must be not only 'good in principle', but also timely. Good ideas at inconvenient or impractical moments are not necessarily a solid foundation for research structures. Timeliness matters.

The recent history of our human activities in space can help to illustrate the point. It is littered with initiatives that have produced a good deal of debate about precisely this issue of when actions ought to be carried out. Premature moves do not always end well. Notoriously, Bernard Lovell, the long-term leader of the Jodrell Bank telescope team, described the US Air Force Westford program as 'ethically wrong' for having sent 350 million copper needles into space in October 1961. The intention was that they might act as dipole antennae, in the hope of improving military communications. More needles followed in 1963. In retrospect, it is difficult to disagree with Lovell's assessment. If we had known then what we know now about space debris, it is highly unlikely that this would have been done even once. Moving too quickly, and then repeating the exercise without due caution, resulted in a counterproductive outcome.

Even the Apollo program has been subject to questions along these same lines. The questioning is not so much about the many accomplishments of the program, or even the validity of its core goals, but about its pace and the problem of sustainability. The early US space program was, after all, driven in part by a transitory Cold War, as well as by human imperatives to explore. While it was broadly welcomed, and rightly celebrated for its remarkable achievements, even at the time some public figures such as J. G. Ballard questioned the wisdom of trying to press too far too quickly, for what might ultimately turn out to be a premature space age. Whether or not more would have been done, or could have been done, in the 1970s and 1980s, if the pace of the space program expansion in the earlier 1960s had been more *even* remains a matter of debate. These are issues for the historians, issues that are likely to continue to be debated and will not be settled here. What is done is done. However, the background assumptions, that sustainability matters and timeliness cannot be ignored, are harder to set aside. There is a strong case for accepting that major institutional proposals do require justification, not simply in terms of the validity of their goals but also in terms of both of these key factors: the sustainability and timeliness of the proposals, their prospects for a positive longer term contribution to research outcomes.

The White Paper has made a case for the timeliness of a European Astrobiology Institute. This would be a major move, contributing to cohesion within the European Research Area. In this particular case, the justifications do seem to be strong. First, the proposal comes at a point in time when our human activities in space are significantly expanding and when further and rapid growth in the near future is reasonably anticipated, driven in part by the emerging commercial space sector and its ambitions. Development here is not simply a matter of increasing scale, but also broadening involvement, with both state and private sectors taking joint and separate initiatives, and with Europe as well as the US and Asia gearing up for major steps forward. (These three seem particularly important for the next phases of development.) In the European case, at the time of writing, the European Space Agency in collaboration with the Russian space agency, Roscosmos, is gearing up to send the ExoMars rover to the red planet in 2021 in order to look for atmospheric gasses that might be linked to active geological or biological processes. The rover will be the first to drill down into the surface of the planet, down as far as 2 m. (With all of the hopes for new information and issues of planetary protection that raises.) The move involves an ambition to emulate successes by the US, but also to go further and deeper.

Europe is also a major area for the production of cubesats and is in the final stages of the completion of Galileo, Europe's own global navigation satellite system, comparable to GPS and GLONASS (the US and Russian navigation equivalents). When completed, in or around 2020, it should consist of 24 operational satellites and a further 6 spare satellites positioned in three Medium Earth Orbit planes at 23,222 km above the Earth. Again, the project is ambitious. Similarly, while Europe already has a spaceport in French Guiana, the prospect of future space tourism has led to discussions about the modest development of spaceports within Europe itself. A change that will be hard to avoid once the industry has moved from basic infrastructure and logistics to regular operations.

By comparison, during the Apollo program in the 1960s, although many individual European scientists and technicians were involved, Western Europe was largely an institutional bystander and the European Space Agency had yet to be formed. (Activities were still coordinated by the precursor body, the European Space Research Organisation.) While US space activities still remain significantly larger, Europe is now a major player within an emerging range of commercial and state space activities, with the prospect of playing a full part in the next phase of human activities in space. Seen in the light of this, the proposal for the formation of a European Astrobiology Institute is geared to allow the development of astrobiology in Europe to keep pace with other developments across various space sectors within Europe and elsewhere.

Second, astrobiology now has a strong international research community, and a particularly strong research community within Europe. This is an area of special and sustained European strength. Research does not depend upon a small number of scholars, but upon a sustainable international research community. By comparison with some of the classic disciplines of science, astrobiology is a new research field. However, it is no longer in its infancy. We have come a very long way from Darwin's speculations about life originating in a 'warm little pond', and from the conjectures about life's origins by the European scholars Oparin and Haldane in the 1920s. We also know a good deal about how quickly a new field of scientific research can expand, when it is driven by social considerations as well as scientific curiosity and the search for knowledge. Genetics is an obvious example and one which is, again, linked to the goal of arriving at a better understanding of life as such. The thought that Europe should not play ‘catch up’ when it can be at the forefront of this research, plays an important justifying role. Given an expanding research field, the questions of ‘How integral can work across the European Research Area be?’ and ‘How integral *should* it be?’ are important policy issues. The White Paper has argued that Europe can and should play a major role, that it should be at the center of this research because it has something distinctive to add. It has a distinctive voice, exemplified in the extensive collaboration involved in the production of the White Paper itself. It can speak for a greater culture of co-operation between the sciences and relevant discipline areas within the humanities. More importantly, it can show how such co-operation may proceed.

Finally, in addition to the point that human activities in space are rapidly increasing (and Europe is already part of this expansion), and the point that the European contribution to astrobiology is already significant and distinct, it should be recognized that our knowledge of where to look for life, and where it is unlikely to be found, is far greater now than in even the recent past. As an example, and one which few people are aware of, NASA no longer comprehensively sterilizes landing craft for the surface of Mars. Instead, rovers are constructed in clean rooms with partial sterilization for the simple reason that landing on the surface of Mars, with the exposure to ultraviolet radiation that this involves, is itself an efficient means of sterilization for at least some purposes. (Matters change when, as with ExoMars, there is any prospect of drilling into the surface. Planetary protection then requires further precautions.)

The whole project of exploring the conditions for life, and the possibility of finding traces or life signatures, is vastly more sophisticated than it was during the pioneering days of lander-based exploration. When the Viking landers touched down upon the surface of Mars in 1976, the first extremophile organisms had only recently been discovered here on Earth. Learning where to look and how best to look for *traces* of life was a new challenge. The modelling for the experiments, guided by what was known at the time, was rudimentary, resulting in a false positive and subsequent controversy. By contrast, if there is life elsewhere in the Solar System, we are now far better equipped (both technologically, and in terms of our overall knowledge about life) to find it. It is even tempting to think of ours as an era of ‘preparing for discovery’ rather than merely one of searching hopefully for life.

Of these three considerations, the first two may turn out to be the more important. The third is, perhaps, more of an ancillary consideration. Important, but it functions largely as a reinforcer of the other two. We do not, ultimately, need any shift to a ‘preparing for discovery’ setting in order for the two main justifications to stand. They hold up in their own right. The validity and timeliness of the proposal for Europe to further coordinate its leading research role, and give greater cohesion to research activities across the European Research Area, does not depend upon the discovery of life elsewhere. Nor does it depend upon how close we might be to such a discovery. Rather, it turns upon more Earthly concerns. Understanding life on Earth is a scientific and social obligation, an ethical imperative and a practical necessity which reaches beyond any institutional proposals and frameworks. A consensus about the importance of understanding life is at the core of the environmental concern now shared across the world by governments, non-governmental agencies and citizens alike. It is also at the core of the major international agreements whose goal it is to safeguard life, and to protect life on Earth from the worst effects of climate change. But understanding life on Earth, and doing so in a sufficiently deep and detailed way, requires us to consider why life has emerged here yet not in various other places, and why some locations elsewhere might turn out to be better candidates for life (or for historic traces of life) than others. Overall, we know vastly more about these matters than we did in the middle of the last century, when the first space programs emerged. And we will know vastly more again in fifty years' time. This is a period of rapid development in our knowledge and in our grasp of the larger story within which humans are situated.

Consequently, the conducting of the relevant research, the protecting of the relevant locations, and the theorizing of results, has a value that is independent of the discovery of any actual second location where life might once have had a foothold. (Fascinating and important though such a discovery would be.) Astrobiology is, in brief, integral to the deepening of our best understanding of human and terrestrial life. Its defined focus, upon 'origins', 'evolution' and 'distribution', directs our attention towards precisely the range of questions that we need to address if life anywhere (including here on Earth) is to be understood in a way that truly deserves to be called deep and sufficiently detailed. The rapid growth of research into extremophiles is an example. Once regarded as something of a special exception, the succession of discoveries of organisms capable of surviving at extremely hot or extremely cold temperatures, or in conditions of high acidity or alkalinity, is now regarded as integral to our grasp of the resilience and variability of life itself. Hydrothermal vents, the sub-ice waters in Antarctica, and the Marianas Trench (the very deepest place on the surface of the Earth) have been found to harbour life. Automatically discounting the possibility of life securing a hold in difficult and remote locations, is no longer plausible. Coming to grips with the extensive presence of extremophiles on Earth has reshaped scientific narratives. Yet, if life exists in such places here, but not in favourable locations elsewhere, this itself poses questions about its origins and evolution as well as distribution.

Extremophiles are, however, only one example of this entanglement of research into terrestrial life and exploration of the possibilities for life elsewhere. Again, it is the *possibilities* for life elsewhere, rather than any act of discovery, that ultimately seems to matter. The most important advances for our understanding of life are not always the most dramatic. Hence, the challenges of science outreach which the White Paper have drawn attention to. While it is true that a discovery of life traces on another planet, or in a meteor, or even on an asteroid, would be a newsworthy story for the ages, it is simply not a required justification for extensive and detailed astrobiology research. In line with this, speculation about whether or not life will ultimately be found elsewhere has remained outwith the bounds of the White Paper discussion. Not because it is uninteresting. It is obviously an interesting question and a great deal can be said about what we should expect the outcome of such a search to be. Rather, it has remained outwith the limits of the White Paper discussion because of a concern to remain within the bounds of what we actually *know* and can predictably work with.

The thought has not been to make policy suggestions based upon conjecture, no matter how interesting that conjecture might happen to be. Instead, the thought has been to work upon the basis of what is already known, what is understood about how science evolves, and what is reasonably anticipated about future space-related research and activities. What is known, understood, and reasonably anticipated is that the research field of astrobiology will continue to grow; the commercial space sector will expand considerably; Europe and Asia will both be significant players alongside the US; and multiple stakeholders are likely to establish a growing presence in space. For most policy makers, these are non-controversial assumptions.

As a final point, with growing human activity in space comes the inevitable set of discussions about regulations, systems of co-ordination, responsibility, legal and ethical duties and entitlements. In brief, all of the problems of establishing a shared legal framework for international activities by multiple agencies from different nations. Such discussions might issue, at some point, in the revision of existing agreements or in a new international agreement comparable to the Outer Space Treaty of 1967. Or, in the light of the considerable difficulties of securing a new agreement to accommodate the interests of so many legitimate stakeholders, there may be a move to align norms and practical working understandings without any new and overarching space treaty. This too is a matter of conjecture. Reasonable opinions among policy analysts may differ, and especially so in light of the fact that regulation is thought of differently in the US, Europe and across various and distinct parts of Asia. As yet, the outcome of such dialogue remains unclear. That it will take place is not, however, conjectural. It has already begun. And a strong European voice in the field of astrobiology, expressing a broad scientific and scholarly consensus, promises to be an important contributor.