

SPACE ASTRONOMY - THE NEXT THIRTY YEARS

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1. Introduction

I have a strong feeling that I am not the right person to give this lecture. Many of you will have noticed that recently the National Academy of Sciences has published a report entitled "Space Science in the 21st Century - Imperatives for the Decades 1995 - 2015". This assessment was requested by NASA and was headed by the Space Science Board of the National Academy of Sciences. This is a very large and ambitious report and is essential reading for those who wish to have a broad vision of what might become possible in the coming decades. As a result, this brief review will make no attempt to be complete but will simply raise some points which may contribute to the discussion of future directions of space astronomy. I would emphasise that these are personal views.

In looking at the next thirty years of space astronomy, it is salutary to look at the state of astrophysics thirty years ago. In Table 1 I show a selected list of discoveries and achievements which occurred over that period along with a selection of space missions which have had a major impact upon astrophysics. It is salutary to remember that the space age itself is only just thirty years old. There is no question but that the last thirty years have been the golden age of astrophysics - there has never been a period when so many new disciplines were opened up or so many key discoveries made about the ways in which matter behaves in the extreme physical conditions found in the Universe. Space science has contributed in a central way to many of these achievements.

Looking to the future, it is interesting to look at these successful space astronomy missions and likely future space missions in terms of their size and cost. I display this information in Table 2 in which the missions are arbitrarily divided into small, medium and large programmes and presented in terms of the coverage of the electromagnetic spectrum. Missions already in orbit are shown in bold, roman letters, approved future programmes in underlined, bold letters and proposed but unapproved future missions in italics.

There are a several well known features of Table 2. The first is the fact that the exploratory phases of most of the wavebands which can only be observed from space have now been carried out by small to medium sized missions. There are still some regions of the spectrum which have yet to be explored. I would include among these the extreme ultraviolet waveband, the difficult γ -ray waveband about 1 MeV and higher energies and possibly the sub-millimetre wavebands where there may well still be surprises waiting for us. You would have to ask Martin Harwit

TABLE 1 - Selected Astronomical Discoveries and Achievements of the Last Thirty Years and Selected Space Missions

	Radio Galaxies	
1960	-----	Sputnik
	Quasars	
	Microwave Background radiation	
	Pulsars	
1970	-----	SAS-2
	γ -rays from the Galaxy	UHURU
	Binary X-ray sources	
	X-ray emission from Clusters	
	Infrared "protostars"	IUE
		COS-B
	Binary pulsar	
1980	-----	Einstein
	Superluminal radio sources	
	Gravitational lenses	
1988	-----	IRAS
	Galaxies with redshifts > 1	EXOSAT
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TABLE 2 - Selected Past, present and future space missions

Waveband	Radio	Infrared	Optical	UV	X-ray	γ -ray
Size of Mission						
Large		<i>LDR</i> <i>SIRTF</i> <i>FIRST</i>		<i>HST</i>	<i>AXAF</i> <i>XMM</i>	
Medium	<i>Quasat</i> <i>Radioastron</i>	<i>ISO</i> <i>IRAS</i>	<i>Hipparcos</i>	<i>Lyman</i> <i>IUE</i>	<i>Einstein</i> <i>EXOSAT</i> <i>XTE</i>	<i>Advanced</i> <i>GRO</i> <i>GRO</i>
Small		<i>COBE</i>			<i>UHURU</i>	<i>SAS-2</i> <i>COS-B</i>

about the number of qualitatively new phenomena we might uncover in these wavebands but I would be surprised if there were none.

The next natural step in all the wavebands is the construction of larger scale missions aimed at developing real astrophysical understanding. It is true of all great discoveries in astronomy that, whilst the discovery phase has a unique excitement and intellectual stimulation for astrophysicists, it is the systematic follow-up phase by dedicated observatories which sets the new science in its real physical and astrophysical context. There is therefore a trend towards astrophysical observatories with a wide range of scientific capabilities. It is gratifying to see that what are referred to as the "great observatories" are planned for all the astronomical wavebands only accessible from space but it can also be seen that the tendency is for the missions to migrate towards the upper part of the Table, i.e. towards large programmes, to order of magnitude on the scale of the Hubble Space Telescope project. Indeed, there are already some very ambitious programmes in this Table, the Large Deployable Reflector (LDR) probably being the most ambitious of all these missions and I would not be surprised if it slipped right off the top of my diagram.

2. The New Projects

It has been remarked many times that, for the large space astronomy missions, there is a very long time between the initial concept and the successful deployment of the spacecraft in orbit. The Hubble Space Telescope is an example of a mission which needed about ten years to reach the final approval phase and then more than ten years to construct the telescope and place it in orbit. The logic of this experience is that we already know most of the possibilities for the first fifteen years of our 30-year forward look period. If a large project has not reached a certain degree of maturity by now, we know that it will be beyond 2000 before it will fly. This makes half of my task easy – we already know the likely programme of large missions for the next 15 years. Indeed, one might take the position that, if the programme laid out in Table 2 were achieved by 2000, that would indeed be a considerable achievement.

The remarks in the last paragraph refer to the large Observatory-class missions. The small and medium-class missions have, however, a key role in the future development of the space astronomy programme. There is a continuing need to undertake these classes of mission in order to carry out qualitatively new types of astronomy, where the exploratory phases have yet to be undertaken, and also to exploit new techniques and instruments for astronomical observations. There are many such possibilities. Some examples which spring to mind in the first category might include:

- **Studies of the Microwave Background Radiation** There will be a continuing need to define its intensity and fluctuation spectrum with greater and greater precision. As shown by the RELIKT experiments, a great deal can be achieved with small experiments.
- **"Simple" optical and infrared interferometry from space** Here I am not proposing an enormous experiment but one carried out with small mirrors but taking

full advantage of the lack of phase fluctuations in the wavefronts of the signals as observed from space.

- "Solar probe" type experiments in which a heavily protected space vehicle passes close to the surface of the Sun.
- The "Interstellar probe" in which a small space vehicle is sent beyond the Solar System to sample the nearby interstellar medium.

This list is simply illustrative, designed to make the point that important new classes of experiments should be undertaken which need not fall in the Observatory-class category and which are likely to open up new ways of doing astronomy.

As an illustration of the second point, there is an excellent case to be made for a small to medium sized mission in γ -ray astronomy, what I have called *Advanced GRO* in Table 2, in which new technologies are exploited to make very large increases in scientific capability possible. To a certain extent, the same argument can be applied to the types of X-ray instruments to be flown on the USSR *Spectrum-X* mission. There are many other examples in other space astronomy disciplines which could be listed.

3. The Large and Very Large Projects

The big question concerns the strategy for the large and very large missions which could be achieved in the future if the resources were made available. The natural tendency is to think in terms of large and very large observatory-type missions, the goals being the need to increase angular resolution, sensitivity, field of view and the precision of astronomical observations. The scientific case for such projects has to be outstanding and the proposed missions must exceed by at least one order of magnitude, and preferably by many, what has been achieved before.

I would make two points about these missions. First of all, I believe that it is not at all difficult to make a very convincing scientific case for constructing such facilities. The enormous scientific and technical advances made over the last 20 years and the need to understand the many new facets of physics and astrophysics opened up by them makes the writing of the proposals remarkably straightforward. Even in the case of optical astronomy where it might be thought that the prime initiatives might lie with the ground-based astronomers, the case for very large facilities in space can be made very compelling. To give a simple example of this in the case of cosmology, we can now undertake real astrophysical studies of the very brightest galaxies at redshifts greater than 1. However, we all know that what we really need is to study the common stuff of the Universe as it was when the Universe was very much younger than it is now. We would aim to read off directly by observation the evolutionary history of our Universe. I have no doubt but that this could be dressed up as a very compelling case for a 16-metre optical telescope in space but the essence of the case would in the end not be very different from these few words.

The second point concerns advanced technology. Unlike a few years ago, I believe that, although there are unquestionably technological problems to be solved in producing enormous space astronomy facilities, progress in the design of telescopes

and their instrumentation in all wavebands has been so rapid that, provided the resources are made available to accomplish the programmes, the big future programmes are unlikely to be impossible on technological grounds. An excellent example is the 8 or 16-metre space telescope described by Dr. Illingworth. There are plainly many technical problems to be solved but I would be amazed if, by the year 2015, it were not entirely feasible to put together the segments of a mirror in space to produce a huge telescope which could be maintained in essentially perfect alignment. There is a clear requirement to be able to fabricate large facilities in orbit but one would imagine that this should be exactly the motivation for the construction of space stations. The natural extension of this single mirror concept is to arrays of optical-infrared interferometers which would have been preceded by the small types of mission discussed above. Again, I would be amazed if there were any real technical show-stoppers in this area by the year 2015. Similar arguments can be made for the other astronomical wavebands.

The really big problem is that I have not attached a price tag to any of the huge observatories. This is where the crystal-ball gazing really begins. The generation of space observatories after those shown in Table 2 are likely to be orders of magnitude more expensive than the large projects. It is not a scientific question to ask whether or not it is sensible to plan for such possibilities. The astronomers have no problem in comparing the costs of such huge observatories with, say, the cost of a manned flight to Mars or selected defense programmes and then showing that they are really quite modest and should be affordable by the advanced nations. The reality is of course that we are talking about different types of money. An optimist might argue that the great powers should come together to pool their resources for space astronomy and in this way bring the huge projects within the bounds of possibility.

Projects of such enormous scale and cost require major political initiatives and we must continue to press for the next generation of space astronomy missions stressing the great value of international collaboration without which many of the most ambitious and scientifically important projects are unlikely to be affordable. It is only realistic to note that there are real limits to the what can be afforded by agencies such as NASA and ESA. Everyone is aware of the fact that the likely cost of a 16-metre optical telescope in space exceeds by a large factor the typical cost of an ESA cornerstone mission.

In this situation, it is interesting to look at some of the lessons which have come out of the major astronomy missions so far. One striking feature is that they have all been relatively simple missions. For example, IUE was essentially a single purpose UV spectroscopic mission, IRAS was a fixed scanning photometric telescope with limited capability for pointed observations, Einstein contained more complex instruments but the pointing requirements were kept simple. This contrasts with the complexities of the Hubble Space Telescope project where there are six complex scientific instruments, a pointing accuracy of 0.007 arcsec and enormous numbers of modes of operation of the telescope, let alone the facility for on-orbit servicing and replacement of scientific instruments in low Earth orbit. It is an interesting question to ask to what extent the overall costs, and even more the operating costs, have been driven by the complexity factor. On the one hand, there is a certain minimum infrastructure needed to maintain a space vehicle in orbit and the question is really what the incremental cost is for each added layer of complexity in addition

to the basic instrument. I believe the moral which will come out of such a study will be **keep it simple!**

A second concern is the ability of space astronomy to do a complete job in any particular area of astrophysical endeavour. It is interesting to note the time needed to complete any major programme of astronomical observations on ground-based telescopes with a generous allocation of observing time. My own experience is that the major programmes require a great deal of time to reach a successful result as opposed to one-off observations which often produce interesting results but which are difficult to set in their astrophysical context. I consider that it was an enlightened decision of the Space Telescope Project to accept the concept of **Key Projects**, the aim being to ensure that these programmes have adequate observing time to enable good results in major programmes to be obtained. It is important to note how few of these large projects can be accommodated even in a long-lived space observatory such as the Hubble Space Telescope. There is no question about the wealth of new science which will come out of project like the Hubble Space Telescope but it is important to recognise that even with a 15-year lifetime, only a limited number of major programmes can be accomplished.

These concerns lead me to wonder if we might not adopt a somewhat different approach to the large and very large missions which would be rather more astrophysically driven than facility driven. I have invented a game for astronomers in which the participants have to decide what would be the single most important space mission for the solution of a specific astrophysical problem. No more than 2 scientific instruments are permitted aboard the satellite observatory to solve the problem. The participants may throw up their hands and say "But I need information from all wavebands to solve my problem!" That answer is disallowed. Just to get the game going, I would suggest that the following astrophysical problems as candidates for these missions : the formation of planetary systems, the origin of stars, the origin and evolution of galaxies, the physics of black holes, the physics of relativistic and non-relativistic cosmic plasmas, the physics of dark matter and so on. The winner is the person who comes up with the cheapest, realistic space project which is likely to achieve its scientific goals as agreed by the other players. Tactical voting is not allowed.

I am not pretending that this is a solution to the problem of deciding the directions of future very large missions. Astronomers are always wary about putting all their eggs in one basket. However, in many areas, the exploratory phases are over and one needs dedicated facilities to make substantial progress. The only virtue of the above game is to suggest that, with the increasing maturity of the astrophysical sciences which can be carried out in space, we might with advantage plan missions with much more specific astrophysical goals and design the satellites and their scientific payloads specifically to address major physical and astrophysical problems. Of course, given such a facility, an enormous range of other science could be undertaken but I would allocate at least half the time to the solution of a small number of really major problems. The rest of the time would be available for all other types of science. It may well be that after this exercise is carried out, we would find ourselves back where we started with "Great Observatory" class missions but I believe the case for them would then be that much stronger.