THE ASTROPHYSICS OF THE FUTURE

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1. Apologia

It is with some trepidation that I set down these thoughts. The history of physics and astronomy is littered with pontifications about the future, most of which simply end up embarrassing their authors. There are many projects which can be regarded as very safe bets but these might not be the ones which totally transform the nature of the discipline. The situation is analogous to that in the early 1950s when extragalactic astronomy simply meant optical astronomy since there was no other way of carrying out such studies – few would regard that as an adequate position nowadays. Similarly, it is difficult nowadays to imagine cosmology without the Microwave Background Radiation. Thus, the problem for the prognosticator is to tread the narrow line between science fiction and a simple extrapolation of what we do now with our facilities. It is in the spirit of this meeting to concentrate upon space observatories but I believe that it is instructive to look at the whole of astronomy, both from space and from the ground.

2. Theorems for the Established Astronomies

Ask any astronomer from any waveband what they would like and it would be very surprising if they did not all answer in the same vein – they would all say that they need larger telescopes (i.e. more collecting power), higher angular and spectral resolution, larger detector arrays and many more telescopes which can provide all of these. Let me comment on these requirements in a slightly provocative way by enunciating a number of plausible theorems about how these facilities will be promoted and used.

Theorem 1. The scientific goals of all future large facilities are the same. The reason for this is very simple – in order for any very large facility to be funded nowadays, it must be a facility which can be used by a large fraction of the astronomical user constituency and this means that it must attempt to be all things to all astronomers. For example, the Hubble Space Telescope, the VLBI array and the AXAF Observatory are very different projects but important parts of their scientific rationales are, for example, the determination of Hubble's constant and the deceleration parameter, the origin of quasars, the evolution of extragalactic populations with cosmic epoch, the formation of stars. Even the case for the Gravitational Wave Observatory includes as part of its future programme the determination of Hubble's constant and the deceleration parameter as well as the study of quasars and black holes.

Y. Kondo (ed.), Observatories in Earth Orbit and Beyond, 421–426.

There is a very positive side to this situation. The similarity of the scientific cases means that very different disciplines have much to contribute to each astrophysical area. It is a great tribute to instrumental advances that already sensitivities are such that most classes of object can be observed in all wavebands, with some obvious notable exceptions. The negative side of the situation, which has already been noted by the politicians, is that the astronomers seem to be asking for more and more expensive instruments to tackle the same set of problems for which the last major facility was approved. They ask "When is it all going to stop?", to which the answer is, "It isn't". The scientists will continue to press for larger facilities because that is the very nature of the scientific enterprise. The resource is not, however, unlimited – even in an ideal world, no pure science project is likely to cost more than a small fraction of the gross national product of even the richest nations, although I understand that Tycho Brahe achieved this in building his Observatories at Hven in the 1570s. Perhaps we should learn something from him. In the current climate when pure science may not necessarily have the top priority for funding, I believe we have to be very careful how we sell the large projects. Their apparent similarity to the non-astronomer is not necessarily a good thing.

On a previous occasion, I made provocative remarks to the effect that it might be beneficial if the astronomers concentrated upon the scientific areas which are best matched to the technological capabilities of each waveband. This was not a popular suggestion because everyone is used to designing facilities which cater for very wide communities and we have grown used to the concept that, if you have to put up a large satellite for one purpose, you might as well add on other instruments and broaden the range of the science which can be undertaken at relatively little cost compared to those of the construction and launching of the observatory itself. My concern is that this tendency results in a dilution of the scientific programmes which are best matched to the waveband. This leads to Theorem 2.

Theorem 2. The most important programmes for astrophysics require many years of dedicated effort by groups of astronomers tackling specific major astrophysical programmes. One of the major problems with many of the most important programmes in astronomy is that they require large data sets obtained in a systematic way with well-defined selection criteria. In a survey lecture in 1982, I gave a list of what I considered to have been major observational programmes which had put the subject of astrophysical cosmology on a secure foundation. This included topics such as the definition of the large scale structure of the Universe, systematic surveys of quasars and radio sources, the determination of the distances to nearby galaxies, and so on. It is the characteristic of these programmes that they require an enormous amount of effort to obtain a convincing result. There is no way that these important programmes can be achieved without substantial effort. The great discoveries in astronomy are what one remembers most vividly but what converts a discovery into real astrophysics is the systematic study of the properties of whole classes of object.

A beautiful recent example is the Harvard-Center for Astrophysics galaxy survey. In that project, over 20,000 redshifts for galaxies in a carefully selected portion of the Zwicky Catalogue have already been observed, the complete sample consisting of about 30,000 galaxies. These have been used to produce the most complete three-dimensional map of the distribution of galaxies yet available. This programme was only possible because a telescope was dedicated to this single task. This map is one of the most important probes of the large scale structure of the Universe. In addition, because of the systematic way in which the data have been collected, we are obtaining a completely new view of the properties of the galaxies themselves. In my view, it is projects like this which advance our understanding of the Universe in a substantive way.

Let me give another example in which I believe an opportunity may have been missed. One of the most exciting results concerning active galactic nuclei has been the spectroscopic monitoring of variations in the continuum and broad-line regions of Seyfert-1 galaxies. The discovery of correlated variations between the continuum and line intensity variations with a time-lag of several days is a beautiful example of what was possible with a concerted effort by European astronomers using IUE observations over a period of several years. An enormous amount of information about the properties of the broad-line regions and consequently about the innards of the active nucleus itself can be learned from these studies. It had seemed to me that this was an ideal programme of observations to be undertaken by the Hubble Space Telescope as a key project. One can imagine a systematic campaign of observations of a dozen active galactic nuclei. This programme was not selected as a key project, I believe, because it can at least partially be carried out using optical observations made with ground-based telescopes. I only hope this is the case. This is the type of programme which would have benefitted enormously from a systematic programme of observations under the guaranteed ideal conditions from space.

I believe that as scientists, we should be prepared to make these difficult decisions about which programmes we are going to do properly. The consequence of theorem 1 is that we try to do everything with our telescopes. This is naturally a very exciting way to carry out astronomy and is the way that important discoveries are made. I would argue that it is essential, however, that we make time available in our planning to do a selection of the most important problems properly, which means spending a lot of observing time on them. It also has the consequence that astronomers have to work in larger teams and this certainly runs against one of the major attractions of the field in that, although the facilities for astronomy are large, the science done with them is often small science carried out by an astronomer and a graduate student. This absolutely must be preserved but I would hope that the balance could shift in the direction of the large programmes.

Another problem with the large facilties is exposed in Theorem 3.

Theorem 3. Although the size of the telescope increases, the number of observations remains constant per unit observing time. With some very obvious exceptions, as one increases the power of the telescope, the number of objects observed does not increase. This is because the most interesting objects are always at the very limit of observation. Thus, while we struggle to take the optical spectra of 22nd magnitude galaxies now, we will struggle to measure the spectra of the same number of 24th magnitude galaxies with Very Large Telescopes. This theorem has important consequences for the design of observing programmes. Obviously, if one were to stick to programmes on objects of the same brightness as are observed with smaller telescopes, these could be observed in much larger numbers much more rapidly. However, this is not how the psychology of time allocation panels tend to work. If the programme can be done with a smaller telescope, even if it takes a long time, it is not as attractive as a programme which uses the larger facility to its very limits. My own personal preference would be to ensure that adequate time is allocated to programmes which require large statistics on moderately faint objects as well as the limiting observations which require lots of time per object.

I believe these theorems are important for making the case for future large facilities and for determining the strategies to be adopted in utilising properly the next generation of large space facilities.

3. Future Facilities

I will say very little about the astrophysics because I have already presented a broad survey of the whole of astrophysics (Longair 1989). Those interested in my views on the most important problems of contemporary astrophysics should look there. I will simply highlight some of the major problems and the future space facilities which will be of special importance for these.

Solar and Stellar Seismology. This is a very new field and has already revolutionised the way we study the internal structure of the Sun. We need to be able to undertake the same types of studies for stars as well as the Sun. These studies are complementary to the neutrino astronomy of the Sun and stars.

Brown dwarf astrophysics. The astrophysics of baryonic dark matter is in its infancy. The first tentative identifications of brown dwarfs have been made but we need to convert this into a genuine scientific discipline. Very large space infrared facilities are needed for this task.

Stellar mass black holes. The three reasonably convincing cases for stellar mass black holes in binary X-ray sources need to be consolidated one way or another. The observational understanding of the behaviour of matter about black holes of stellar mass is crucial for fundamental physics. We need to have facilities which enable us to study these types of object in nearby galaxies. Large X-ray astronomy observatories are essential.

The formation of stars and interstellar chemistry. We have yet to catch any star in the process of formation, i.e. actually collapsing to form a main sequence star. The understanding of the processes of star formation in detail is the biggest gap in our understanding of stellar evolution. It is closely tied up with molecular astrophysics and the chemical processes inside the extremely dusty regions in which stars form. These studies have consequences for the whole of the physics of galaxies and astrophysical cosmology. Large infrared, millimetre and sub-millimetre observatories are essential for this problem.

The nature of the dark matter in clusters of galaxies. We need observational tools to enable us to determine the distribution of the dark matter which we now know must be present in large-scale structures such as clusters of galaxies and larger scale systems. In many ways, understanding the spatial distribution of the dark matter is as important as its nature which may end up being the province of the particle physicist. We need large optical facilities to be able to probe the gravitational potential distribution in the Universe, i.e. the ability to measure the three-dimensional distribution of galaxies and their peculiar velocities.

The physics of quasars and active galactic nuclei. Not only are quasars and active galactic nuclei the most powerful sources of energy we know of in the Universe, they display a remarkable range of physical phenomena which have little counterpart in smaller scale phenomena. While the identification of the ultimate energy source as a supermassive black hole is convincing, the details of how we get from that concept to what we see is very far from being firmly established. The needs span the whole of the electromagnetic spectrum from ultra-high resolution radio studies, through optical and ultraviolet spectroscopy to X- and γ-ray studies. In all cases, the prime requirement is for more sensitivity and resolution.

The determination of cosmological parameters. A central goal of cosmology is the determination of a number of basic parameter of the Universe – its rate of expansion (Hubble's constant), its kinematics (the deceleration parameter), its mass density (the density parameter) and the cosmological constant. These are all interrelated through the physics of the large scale dynamics of the Universe. All wavebands have significant contributions to make to these problems.

The origin of the large scale structure of the Universe. There is no more challenging problem than the understanding of how the large scale properties of our Universe came about. Many aspects have become the plaything of the particle theorists and many exciting ideas have been developed. In the end, however, these ideas have to be tested against the real Universe as we observe it. We are only now entering the decades when we have a real chance of studying the astrophysics of galaxies, quasars and other large scale systems at epochs significantly earlier than the present. To convert these studies into the real astrophysics of the early Universe needs sensitivities a order of magnitude and more greater than those which are available from the currently proposed projects.

In addition to the observing facilities necessary for carrying out the astrophysics of the future, complementary facilities are essential for theory and data reduction.

We are now entering the epoch of the **Great Observatories**. Optical, infrared, ultraviolet, X-ray and γ-ray astronomies need the Hubble Space Telescope, AXAF and GRO to make significant advances in the exciting astrophysics in these wavebands. The same can be said of those equally important missions which do not have the accolade "Great" but which are just as important for astrophysics – for example, ROSAT, EUVE, Lyman-FUSE, SOHO-Cluster, XMM, etc. Looking beyond the next decade, it is already evident that we know how to build observatories an order of magnitude larger than these. The availability of Soviet launchers which are capable of launching payloads of 100 tonnes removes many of the constraints which have so far limited the ambitions of the astronomers. Thus, as has been emphasised by Garth Illingworth, it is no longer ridiculous to think about very large optical telescopes in space. Granted the strength of the case for large ground-based telescopes, the case for doing the same in space is very strong.

What I have discussed above is the "dull man's" approach to future space missions – simply put up bigger telescopes to advance all aspects of sensitivity by an order of magnitude or more. In addition, we need the more adventurous missions. I would place the **Hipparcos** mission of ESA in this category as well as the various proposals for VLBI from space. Further studies of the Microwave Background Radiation are essential – we need a "Super-COBE" to increase sensitivities by at least an order of magnitude. Such an experiment must discover fluctuations in the background radiation and start a qualitatively new discipline. I am attracted by simple "probe" experiments, for example, the Solar probe and the interstellar probe, which attempt to sample the material and conditions in these regions directly. Continuing in this vein, I am convinced that optical, infrared and ultraviolet interferometry from space has outstanding potential. The gains in phase stability in the absence of the atmosphere make these programmes orders of magnitude easier from space than from the ground and also orders of magnitude more efficient. This would have the potential of opening up completely new areas of parameter space which are not available now. I have a gut feeling that these types of project have real potential to come up with the qualitatively unexpected as well as a core of outstanding science.

Once one begins this type of extensive thinking, there is literally no end to the possibilities. My own view is that even the most exotic possibilities are now feasible in principle with the availability of heavy launchers and the enormous advances in telescope design and instrumentation. The only problem is that the programmes have to be sold to our funding agencies. Let us make sure that we do full justice to the enormous scientific potential of these facilities by leaving behind major real contributions to our understanding of the Universe and not just a sample of what might have been.

References

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