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Study Week on:

ASTROPHYSICAL COSMOLOGY

September 28 - October 2, 1981

CONCLUSIONS

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PRESENTATION

The Pontifical Academy of Sciences held a Study Week from September 28 through October 2, 1981, on the theme: "Astrophysical Cosmology".

We present here the Conclusions of all the papers, drawn up by Prof. M.S. Longair.

At the end of this publication are listed the names of the Participants in the Study Week with the titles of their communications.

The complete Proceedings are contained in a Volume published by the Pontifical Academy of Sciences and edited by Prof. H.A. Brück, Pontifical Academician, Rev. Father George Coyne, Director of the Vatican Observatory, and Prof. M.S. Longair, Royal Astronomer for Scotland.

COSMOLOGY AND FUNDAMENTAL PHYSICS
CONCLUDING REMARKS

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1 - INTRODUCTION

It is an impossible task to do justice to the level of scientific discussion which we have heard during the last five days. All the lectures have contained so much new and exciting material that the best I can do is to recommend to readers that they read each article with the closest attention and then they will have some idea of the wealth of physical phenomena which we now believe are important in understanding the large-scale structure of the universe. I will therefore restrict my attention to some personal remarks about what I found particularly impressive and how I believe we may be able to make real progress in elucidating many of the fundamental problems discussed.

Before discussing these aspects, let me make a few general observations about what has happened this week. First of all, it must be stated that not everything which could be said about cosmology and fundamental physics has been said. With only twenty-two participants it is natural that some fields have only been lightly covered and others completely omitted. In the latter category we must include all non-conventional interpretations of cosmological data. Although regrettable, this omission was intentional on the part of the organisers. Our intention was to discuss how far conventional physics can account for the known facts of our observable universe. We must accept that there is a finite chance that this approach is not correct and that the non-conventional picture will prove to be right. In this case, this volume will go down in history as an interesting sidelight on intellectual history. However, if one were to ask the average astronomer or physicist what approach was most likely to lead to a better understanding of the universe, I am confident that the overwhelming majority would adopt that contained in this volume.

A second striking aspect was the degree of unanimity of the participants on virtually all topics. This was not at all accidental, since it is obvious that there is a very strong Cambridge-Princeton-CalTech atmosphere about the meeting. According to my calculations, all the participants have either worked at these locations for substantial periods or have been regular visitors. Thus, the apparent unanimity is almost certainly deceptive and one must ask what the rest of the world would have said about the above lectures. In certain quarters, I know, there would be little sympathy for some of the ideas but, on the whole, I suspect there would be agreement about the basic ideas.

A third point which struck me was the way in which observational cosmology has become simplified. It is a credo among many cosmologists that the basic facts of cosmology should be simple. I am very reluctant to believe that the way in which the large scale structure of the universe came about depended upon a large number of different complicated physical phenomena. As an example of this simplifying trend, I would cite the picture of galaxies which is emerging. For virtually all purposes, galaxies were simply objects consisting of 10% visible matter and 90% invisible. These are the building blocks which define the large scale structure of the universe. Individual galaxies look very different and of bewildering complexity but there was an underlying trend in the discussions that much of this may well be of secondary importance. I believe Jerry Ostriker's comment this morning was the culmination of this trend when he remarked that all you really need to know about a galaxy is its mass and all the rest is irrelevant detail. Thus, to a first approximation, all the cosmologist need explain is how objects of different masses formed with 10% visible matter and 90% dark. To anyone but a cosmologist it may be a depressing thought that everything we know and love in the universe is simply referred to as "dissipation".

I will do three things. First, I will try to assess what facts have been established about the universe. I do this with some trepidation since I recall vividly a previous conference in which I tried to define the facts on another topic and was accused by Jerry Ostriker of being a Mr. Gradgrind. He was the character in Dickens' "Hard Times" whose children were only taught facts, facts, facts. They never saw horses dancing in firelight; they saw only the combustion of carbon and oxygen and so on.

Fortunately, my facts are very exciting and form the basis for the proper scientific study of cosmology. Second, I will try to answer Martin Rees' seven questions and, finally, I will look at future prospects.

2 - MODERN FACTS OF COSMOLOGY

It is not trivial to define precisely what one means by a fact, since they evolve with time. Being realistic, I will take a fact to be what everyone in this room would agree to be correct with, say, 95% confidence at 4.30 p.m. on 2 October 1981.

It is conventional to begin cosmology courses with the basic facts that: (a) the sky is dark at night; (b) the universe of galaxies is expanding more or less uniformly; (c) the universe exhibits a very high degree of isotropy on the largest scales as evidenced by the microwave background radiation. None of these facts are seriously questioned. To these, I suggest we add the following.

(i) *The hot Big-Bang.* There was no serious challenge to the basic hot Big-Bang picture of the history of our universe. The pieces of evidence which provide the strongest support for this picture are the coincidences between the predictions of light element synthesis, ^{3}He, ^{4}He, D, ^{7}Li, and the observed abundances which were discussed in detail by Jean Audouze. This coincidence is so striking that it seems inconceivable that this could occur by chance, although we know of other cases in astronomy in which such arguments can be misleading. It is intriguing that this type of evidence appears to contradict McCrea's law of evolutionary cosmologies that you always have sufficient freedom in evolving cosmologies to patch up anything which appears difficult to explain by reasonable or hypothetical astrophysical processes at various cosmological epochs. Therefore, you can never prove that evolutionary cosmologies are correct. To put it another way, if the abundances of the light elements had turned out to be totally irreconcilable with the predictions of the hot Big-Bang model, we would have tried to patch it up by appeal to subsequent astrophysical processes. Undoubtedly, this would have been a severe blow to the hot Big-Bang model and would have weakened our confidence that the universe passed through a phase when the temperature exceeded 10^{10} K. I would argue that this coincidence is so striking that it substantiates convincingly the inference from the properties of the microwave background radiation that the universe went through a hot phase. It would however be unwise to believe that the question of the details of the basic hot Big-Bang are settled. There remains the question of how much additional mass might be present in massive neutrinos.

Nonetheless, the evidence is sufficiently encouraging that cosmologists and physicists can now treat seriously the three basic problems of the hot Big-Bang, namely: (a) the origin of the baryon asymmetry in the universe; (b) the origin of the isotropy of the universe; (c) the origin of the fluctuations from which the fine-scale structure of the universe evolved. Until recently, cosmologists supposed that these properties had to be ascribed to the initial conditions from which the universe evolved — a totally unsatisfactory situation. We now begin to have the glimmering of an understanding of how these features might come about in evolutionary cosmologies at very early epochs. The papers by Steven Weinberg and Stephen Hawking indicated the directions in which progress may be expected in the not-too-distant future in understanding these very fundamental cosmological questions. It would not be appropriate to comment on these very exciting ideas at this stage. We must await the process of natural selection in cosmology to find out whether or not these ideas will survive further advances in elementary particle physics and quantum gravitation.

(ii) *The dark or hidden matter.* It was probably always foolish to hope that all the matter in the universe would make itself readily visible to astronomers. At least we have powerful dynamical methods of measuring how much mass is present in bound or almost bound systems and the general consensus seems to be that from the scales of Sc galaxies to groups and rich clusters of galaxies and probably beyond, most of the matter is unobservable at present other than by its gravitational influence. The discrepancy between the visible and invisible matter is agreed to be about a factor of 10 and possibly greater on the largest scale. Nobody is certain, however, about what form it is likely to take. Martin Rees illustrated this vividly by suggesting that at present the uncertainty in the form of the missing mass is about a factor of 10^{70} in mass, the possibilities ranging from massive neutrinos to massive black holes.

We must treat all these possibilities seriously. At the low mass end the possibility of massive neutrinos has many astrophysical attractions. Specifically, it may well go a long way to resolve the rather serious problems which have arisen in understanding how fluctuations in the mass distribution in the universe leave little trace of their presence at the epoch of recombination and in reconciling dynamical estimates of Ω with those derived from primordial nucleosynthesis. My own view on this possibility is that it is not really up to the astronomers to provide the answer. It is up to the nuclear physicists to tell us whether the neutrino does indeed have mass of ~ 10 to 100 eV and, if it does, that may settle the question.

If the neutrino does not have the appropriate rest mass, we have to use purely astronomical means to find the nature of the missing mass and this will not be easy. For example, it might consist of ultra-low mass stars or Jupiters. These may be detectable by their infrared emission. If it is in the form of black holes, certain masses may be detectable observationally by accretion phenomena or as gravitational lenses. That the latter might prove to be an effective approach is suggested by the argument that the excess of quasars around bright galaxies can be explained by gravitational lensing of background objects by discrete compact masses in and around galaxies. Incidentally, if this picture of gravitational enhancement of quasar images by stars in nearby galaxies turns out to be correct, it will be a classical example of a supposedly non-conventional phenomenon, advocated by Arp for years, becoming completely respectable and indeed conventional. It will be a remarkable vindication of Arp's approach to observational astronomy.

It is hard to be optimistic about our ability to determine the nature of the dark mass in view of the vast range of possibilities. For example, if the matter were in the form of mini-black holes, which form during phase transitions in the early universe as suggested by Stephen Hawking, they would be very difficult to detect.

In my optimistic moods I believe that the hidden mass is probably more or less distributed like the galaxies. I say this on the basis that the hidden mass ratio does not seem to vary greatly as one goes up in scale from Sc galaxies to the scale of superclusters. This probably makes its detection easier than if it were, say, concentrated in the space between galaxies. It is very improper for cosmologists to seek easy ways out of problems like this but it would be very convenient if the neutrino did turn out to have mass of ~ 30 eV. The results of the forthcoming experiments to attempt to measure the neutrino mass are of the greatest interest cosmologically.

(iii) *The large scale structure of the universe — the great holes.* We now have a much greater appreciation of the way in which matter is distributed in the universe on the largest scale. Professor Oort and Marc

Davis showed convincing evidence that there exist large regions of enhanced galaxy density — superclusters — between which there are large holes which are almost completely void of galaxies. The reality of these holes and the filamentary supercluster structures are of the greatest interest for understanding the sequence of events which took place when galaxies first formed. Jim Peebles described his numerical experiments which try to mimic the filamentary structure seen in his magnificent map of the distribution of galaxies in the Shane-Wirtanen counts of galaxies. His model assumed a smooth covariance function without holes. You will recall his pleasant turn of phrase to the effect that, when he compared his simulations with the real universe, his attempts were like that of the rude apprentice compared with the work of the old master. The apprentice's picture was not as sharp as that of the old master. It was blurred and lacked the fine brush strokes of the original. He suggested that the general success of his simulations indicated that his smooth covariance function was a good model for the distribution of galaxies. My own personal view is that the old master knows his trade better than Jim. If Professor Oort will excuse my referring to him as the old master, I believe he is correct in identifying the filamentary structures, the cells and holes in the distribution of galaxies as real features which are telling us something important about how the largest scale structures formed.

It is, of course, rather difficult to prove the reality of these structures statistically. However, I take reassurance from the fact that it took Jim Peebles a long time and a great deal of hard work to establish the clustering of clusters of galaxies despite the evidence of the eye that they are clustered. I believe that if Jim works even harder on his statistics he will also be able to demonstrate the reality of filaments of galaxies and prove Professor Oort absolutely correct.

The reason for wanting to have an agreed answer to this question is that it is one of the few tests suggested to discriminate between those theories in which the largest scale structures form first and then form superclusters and filaments by collapsing into pancakes and those theories in which the galaxies form first and then develop into higher order clustering systems by hierarchical clustering. I am personally very impressed by the numerical simulations of Zeldovich, Doroshkevich and their colleagues which show convincingly how cell-like and filamentary structures can form in the first of these scenarios, commonly known as the pancake theory. It is not clear how these filamentary structures form in the hierarchical picture in which, almost by definition, fine scale structure tends to get washed out as the clustering hierarchy grows to larger and larger scales. It is important to establish how critical a test this is of the way in which the large scale structure of the universe forms. If indeed it supports the pancake picture, the implications are profound for observational cosmology. The epoch of galaxy formation must have been relatively recent, $z \leqslant 5$ to 10, and then we may realistically hope to be able to observe directly phenomena associated with the formation of galaxies.

A further footnote to this "fact" is to note how rich Marc Davis is astronomically. His pictures of the three dimensional structure of galaxies in the universe are quite remarkable and suggest many marvellous tests which can be made on the clustering environment of different classes of objects. As yet the statistics are not very large, although definitely impressive, and one can look forward to obtaining answers to questions such as:

(a) Do spirals, ellipticals and S0 galaxies exhibit the same large scale structure or are their clustering properties different?

(b) What is the clustering environment about different classes of active galaxy — for example, Seyfert galaxies, pecular galaxies, radio galaxies, etc?

(c) What is the correlation between gas content and clustering environment?

This is only a short list of the many exciting things which can come out as by-products of Davis' massive survey. It is very important to pursue these studies with much larger bodies of data.

(iv) *Direct evidence for evolution of astrophysical objects over cosmological time-scales.* The evidence for evolution over cosmological time-scales has been convincing for some time for active systems such as quasars and strong radio sources. The presentations of Maarten Schmidt and Harry van der Laan indicated how these studies are now getting down to the details of how various classes of objects evolve with cosmic epoch. The only problem with these studies is that they concern the evolution of rather exotic objects whose astrophysics is not properly understood.

I found particularly intriguing the evidence which is now accumulating for the evolution of the ordinary stellar component of massive galaxies with cosmic epoch. Jim Gunn showed evidence for evolution of the properties of the brightest galaxies in clusters which may be accountable for a combination of cannibalism and stellar evolution. Harry van der Laan showed evidence that radio galaxies which are probably at redshifts between 0.5 and 1 are much bluer than nearby radio galaxies. In my own work with Simon Lilly, we have shown that the optical-infrared colours of radio galaxies show strong evolution of the stellar population out to redshifts exceeding 1. I regard all these studies as our first glimpse of what is now possible when the full power of modern instruments and techniques is applied to the study of galaxies at epochs significantly earlier than the present. There was *a priori* no reason why such evolutionary effects should be observable; it just happens that they have now been observed and they are bound to give us much deeper insight into the sequence of events which took place between the epochs when galaxies first formed and the present day.

One intriguing item concerns the question of the epoch at which galaxies formed and whether it is related to the apparent cut-off in the distribution of quasars described by Maarten Schmidt. It should be noted that we have known for a long time that the evolution of the comoving space density of quasars and radio galaxies must converge at redshifts $z \sim 3$ to 4 because otherwise the source counts would not decrease as dramatically as they do at low flux densities. However, it has not been possible to say whether the sources are absent beyond this redshift or whether the comoving density of sources simply increases much more slowly into the past.

It may be useful to look at the results described by Maarten Schmidt in a way which is independent of modelling the evolution of the quasar population. The question concerns the interpretation of Osmer's result on the absence of quasars in the redshift range $3.5 < z < 4.7$. Malcolm Smith has presented the result in the following way. There are 7 quasars in the Hoag and Smith survey in the redshift range $2.5<z<3.5$ which should be detectable in the Osmer deep survey with $3.5 < z < 4.7$ if such objects indeed exist in that redshift range. If it is assumed that the comoving space density of quasars remains constant between redshifts $z = 2.5$ and $z = 5$, i.e. no evolution over this redshift range, 7 to 9 quasars should be observed in the range $3.5 < z < 4.7$ whereas none are found. Thus, even on this very conservative basis, the decrease in comoving density of quasars at these redshifts must be taken seriously. My own impression is that the radio data are probably trying to tell us the same thing but the evidence is much less direct.

It has been remarked by a number of authors that such a cut-off might be associated with the epoch when the bulk of the galaxies in the universe first formed and this would be entirely consistent with the pancake picture, in which it is the largest scale structures which collapse first at relatively small redshifts, $z \sim 5$.

3 - TENTATIVE ANSWERS TO SOME BASIC QUESTIONS

In this opening lecture, Martin Rees listed seven questions which he hoped would at least be addressed, if not answered, during the last week. I will be so bold as to give tentative answers to these.

Question 1. *What is the value of Hubble's constant to a precision of 25%?*

I was rather surprised when I looked through my notes to find the degree of unanimity among the various estimates. I have summarised my understanding of what was said in Table 1.

Interpreted literally, these estimates tend to favour a value of the Hubble constant in the region of 50 to 60 km s^{-1} Mpc^{-1}. This is a case where I am sure the agreement is only apparent and due to the absence of certain views at this meeting. In particular, the infrared Fisher-Tully method was not properly discussed although Gustav Tammann did express some concerns about the technique. It is only fair to say that probably proponents of higher values of H_0 would have reservations about the way in which the low value was derived. I fear it is only prudent to reserve judgement on the answer to this question.

TABLE 1 - *Data relevant to the Hubble Constant*

Type I supernovae	Sandage and Tammann	$H_0 = 52 \pm 6$ kms^{-1} Mpc^{-1}
Globular cluster ages	Faber	$T = 16 \pm 3 \times 10^9$ years
	Sandage and Tammann	$T = 17 \pm 2 \times 10^9$ years
Age of the Local Group	Lynden-Bell	$T = 16 \times 10^9$ years
Rhenium/Osmium ages	Audouze	$T = 20 \pm 10 \times 10^9$ years

If $H_0 = 50$ kms^{-1} Mpc^{-1}, $H_0 = 20 \times 10^9$ years $\geqslant T_{\text{cosmological}}$.

While on the subject of cosmological parameters, it is as well to discuss the value of the deceleration parameter, q_0, and the density parameter, Ω. On the surface, it looks as if one has a strong result from the preferred value of Ω of about 0.03 to 0.1, which comes from the abundances of the light elements: D, ^{3}He, ^{4}He and ^{7}Li. None of the estimates of Ω was greater than 1. However, the trend of the dynamical estimates of Ω was to values in the range 0.1 to 0.7. These came from estimates of the ratios of dark to visible matter in the universe, from various versions of the cosmic virial theorem by Jim Peebles and from the Local Supercluster test described by Marc Davis. Although Gustav Tammann argued for a low value of Ω from the latter tests, there was some feeling that a literal interpretation of the data suggested values closer to $\Omega \sim 0.3$ than to 0.03. Among the ways of resolving this discrepancy is the possibility that the neutrinos have finite rest mass. If they have the canonical value of about 30 eV, they are relativistic when light element production takes place and do not affect the abundances of the elements, provided the baryon density remains low. However, they do influence the dynamics of the universe and it is an intriguing question whether or not it is possible to devise a consistent picture, so that the light elements can be synthesised in their correct proportions and there be sufficient mass in the neutrinos to give consistency with the dynamical estimates of Ω. My impression was that it is indeed possible and it makes the question of the laboratory value of the mass of the neutrino all the more urgent.

Question 2 (i). *Are there preferred scales in the distribution of galaxies?*

A tentative yes can be given to this question in the sense that the covariance function of galaxies appears to become zero or negative on scales $\sim$ 15 to 20 Mpc. In any case there must be a break in the covariance function from the value found on a scale of $1 \leqslant 8$ Mpc. Jim Peebles demonstrated how significant quadrupole anisotropies in the microwave background would become, if indeed there remains little or no correlation between structures on scales greater than about 20 Mpc. Nonetheless, we probably do not know the covariance function with any reliability on scales between 20 to 100 Mpc and this should be the subject of study on large scale deep plates of typical regions of the universe.

Question 2 (ii). *Is there evidence of dissipative non-gravitational effects?*

In my view, this is inevitable in all models of galaxy formation. I was particularly impressed by Sandra Faber's diagram which showed that all known classes of galaxies now lie within the region of density and temperature space in which dissipation of energy by radiative processes must have taken place. I do not believe this actually helps discriminate between models of the early evolution of large scale structures, because it must eventually occur in all of them so that galaxies with the properties we know can form. On the scale of clusters and superclusters, however, the question of dissipation was controversial and the arguments which suggest it is important are strongly model dependent. It would be very exciting if we could identify large scale systems in the process of dissipating energy during formation but to my knowledge none of these systems has yet been identified.

Question 3. *On what scales are galaxies tracers of the hidden mass?*

A provisional answer can be given to this question. The fact that the ratio of dark to visible matter is about 15 on all scales from Sc galaxies to giant clusters suggests that the dark matter is associated with visible matter. If the density parameter is low, $\Omega \approx 0.1$, then probably all the dark matter could be simply associated with the distribution of visible matter. If, however, the density parameter were closer to unity, the dark matter would have to be elsewhere, almost certainly in the spaces between clusters where the theoretician is free to endow it with a wide range of exotic properties, many of which would be essentially impossible to exclude by observation. This is just another aspect of the problem of resolving the discrepancy between the dynamical estimates of Ω and the amount of mass in known systems.

Question 4. *What is the relation between quasar and galaxy evolution?*

The basic problem here is that there is no theory of quasars and radio sources which is sufficiently secure that it can be used to predict what the relation between the galaxy and its active nucleus should be. We can all hazard guesses as to how it comes about. The central black hole is fed by gas and, possibly stars, from the surrounding galaxy and grows until it becomes a quasar-like object when its mass is $\sim 10^8 M_\odot$ and there is a ready supply of fuel. But it is not at all clear how ideas like these can be put into a form which provides quantitative relations between galaxy and quasar evolution. We need more empirical understanding of quasar, radio galaxy and galactic evolution to be able to put flesh on this question. We have seen how these studies are now possible out to redshifts $z \simeq 1$ and I am optimistic that we will gain great insights from studies of these objects in this redshift range, rather than by studying those at the largest redshifts.

Question 5. *How can we probe observationally the redshift range $5 < z < 1000$?*

In principle, if there exist discrete objects in this redshift range, we should be able to investigate their properties and physical conditions along the line of sight to them. However, I believe it will be very difficult to find such objects. Clearly, they would have to be highly luminous objects to be observable and the best objects for this purpose, the quasars, show at least a convergence in their distribution, if not a cut-off, at redshifts about 4. In addition, there is the awkward fact that the comoving volume per unit interval of redshift decreases as $z^{-1.5}\,dz$ and so, even if the comoving space density of sources remains constant, the probability of finding large redshift objects decreases.

If we cannot use discrete objects, we have only the integrated properties of these objects to look for, i.e., diffuse emission from a background of sources or emission and absorption features in the background radiation. Distortions of the spectrum of the microwave background might provide some information but at present the evidence suggests that these distortions must be rather small.

I am not at all optimistic about studying this redshift interval on the basis of what we know now. I suspect we need a new discovery to make progress in this area.

Question 6. *How secure are the abundances of the light elements: ^{4}He, ^{3}He, D, ^{7}Li?*

Jean Audouze gave a complete answer to this question. I was particularly impressed by the new results on ^{7}Li and, by and large, the story looks rather convincing. However, there is a real question concerning abundance variations of deuterium. My inspection of his data suggested that many of them are consistent with a single deuterium abundance with one or two points which deviate from this mean value by a significant amount. This is such an important question that the origin of these variations should receive urgent attention.

Question 7. *Are there comprehensible physical processes which are important for the time interval* $10^{-44} < t < 10^{-36}$ *seconds and which can explain simultaneously the overall isotropy of the universe and its finite degree of inhomogeneity on a fine scale?*

It is one of the most remarkable facts of modern cosmology that this question can now be asked with a straight face and that there are now the first tentative steps towards a real physical explanation of those profound cosmological problems concerning the isotropy of the universe, its baryon asymmetry and the origin of the fluctuations from which galaxies form. I leave it to each individual to decide how seriously he wishes to take these extrapolations of the known laws of physics.

4 - THE WAY FORWARD

I find it revealing to look at the observational basis for some of the new "facts" about the universe. The list shown in Table 2 is by no means complete but I hope it illustrates my point.

You will notice that all of these result from large and systematic studies. In other words, rarely does one make a single simple observation

TABLE 2 - *Some studies described during the Vatican Study Week*

The covariance function for galaxies	Peebles
The three dimensional distribution of galaxies	Davis, Oort
The luminosity-velocity dispersion relation	Faber
Evolution of giant elliptical galaxies	Gunn
Distances of nearby galaxies	Tammann
Surveys of radio quiet quasars	Schmidt
Radio galaxy identifications	van der Laan, Longair
X-ray counts for quasars and active galaxies	Setti, Woltjer
D, ^{3}He, ^{4}He, ^{7}Li abundances	Audouze

which results in a quantum leap in our understanding. As soon as such a phenomenon is observed, the immediate question is "How common is it among similar objects of its class?" and true understanding only comes from further systematic studies. Perhaps the most striking exception to this rule is the discovery of the microwave background radiation but then I would argue that there is no other member of its class available for observation in cosmology.

Therefore, I firmly believe that the way forward is through the pursuit of yet more difficult systematic work in all the fields we have heard about this week. It may, of course, be that we are lucky and another single great discovery will be made which at a stroke answers some of the profound problems which we have heard about. Realistically speaking, however, such a discovery is most likely to come out of the systematic studies, as for example, happened in the cases of quasars, pulsars, X-ray binaries, etc.

By and large, my list of top priorities would consist of more of the above. If I had to select those which I think may be particularly profitable, I would suggest: further detailed studies of nearby galaxies, deep surveys of large areas of sky, the velocities and spectra of large samples of galaxies, the relation of active galaxies to the properties of the stellar and gaseous component of their parent galaxies and to their environments, measurement of fluctuations in the microwave background radiation with high precision. In particular, studies of how the properties of galaxies and the large scale distribution of galaxies and diffuse matter change with cosmic epoch are of particular significance.

Many of these programmes can be accomplished from ground based facilities. With the development of CCD cameras and spectrographs, infrared photometers and arrays of high sensitivity and complementary radio and X-ray studies, the redshift range $0 < z < 1$ is already available for detailed study, provided the bodies which award large telescope time recognise that, to make significant advances in these studies, a large amount of dedicated observing time is needed. This is not encouraged in the present climate where all large telescopes are grossly oversubscribed and it is necessary to make a quick killing to ensure that the next application will have a reasonable chance of success. I hope some of the influential people in this room will help ensure that some of these demanding large scale programmes are carried out in an effective manner.

The main problems in pursuing these programmes are technical. There is an urgent need for efficient CCD spectrographs so that the redshifts of normal galaxies as faint as $V = 22$ can be achieved in a sensible observing time. The first effective infrared detector arrays will produce a new picture of the universe, very likely with most of the faint objects being galaxies at large redshifts. It may even be possible to detect those elusive objects, primaeval galaxies, in the near infrared waveband. To complement these infrared pictures, one needs infrared spectrographs. These are only now becoming available but without the sensitivities to undertake faint galaxy work. There is great scope for technological development in this area.

There are already many excellent large ground-based optical and infrared telescopes. The development of optical and infrared telescopes with apertures much greater than 5m is perhaps the most significant future development in ground-based astronomy. Their singular importance is their ability to increase, by a large factor, the number of photons received from faint galaxies and quasars, as compared with the present generation of detectors, and so probe the universe directly to the very faintest magnitudes.

In space astronomy the next major advance in cosmology will be made by the NASA Space Telescope due for launch in early 1985. This will open up the world of galaxies with very high angular resolution out to redshifts of one and greater. The types of astrophysics which we can now undertake at redshifts of 0.1 will be possible for objects at significantly earlier cosmological epochs than the present. The maximum gain will be for point objects and luminous quasars of the type we know could be observed at redshifts as large as 10 or 15, provided that: (a) they emit significant radiation at wavelengths $\lambda = 800/(1 + z)$ nm; (b) we know where to look for them; (c) they exist. I fully expect us to obtain definitive evidence about the detailed evolution of galaxies and clusters between redshifts of 1 and the present epoch. I also expect we will be able to see the effects of gravitational condensation in large scale structures between the same epochs. These are only a few of the many exciting cosmological questions which can be addressed and, I hope, answered with some confidence by the Space Telescope.

Beyond that, we can look forward to other exciting space observatories; for instance, COBE, which will perform detailed studies of the isotropy of the microwave background on scales $\Theta \geqslant 7°$, may be launched in 1987. In the 1990's we may look forward to the AXAF X-ray telescope which may be thought of as the X-ray equivalent of the Space Telescope.

5 - CONCLUDING REMARKS

I repeat my feeling that the threads of the broad picture are beginning to be drawn together into a plausible physical picture for the origin and evolution of the universe. Although many of the issues are complex in their working out, the underlying physical ideas have an attractive simplicity. Not only are the physical ideas basically simple, but the present picture of the observed world of galaxies is becoming simpler.

I emphasised that facts are relative things and that in astronomy they tend to evolve with time. However, at least for a few hours, I have a vision of a comprehensible universe which we can account for by the best of contemporary physics. Our job is to find out whether this is illusory or whether we will build upon this hard-won understanding.

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H. van der Laan and R.A. Windhorst: *The Colours of Faint Radio Galaxies*

S.J. Lilly and M.S. Longair: *Evidence for the Cosmological Evolution of the Stellar Content of Radio Galaxies*

IV. Evolution of Quasars, Radio Galaxies and the X-Ray Background Radiation

M. Schmidt and R.F. Grees: *The Space Distribution of Quasars*

L. Woltjer and G. Setti: *Quasars in the Universe*

G. Setti and L. Woltjer: *The Origin of the X-Ray and γ-Ray Backgrounds*

L.Z. Fang: *The Distribution of Quasar Redshifts*

H. van der Laan and R.A. Windhorst: *Evidence from Deep Radio Surveys for Cosmological Evolution*

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V. Primordial Nucleosynthesis and the Origin of Galaxies

J. Audouze: *Primordial Nucleosynthesis and its Consequences*

J. Silk: *Fundamental Tests of Galaxy Formation Theory*

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