CONCLUDING REMARKS

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In these remarks I shall make no attempt to give a balanced review of the current state of cosmology; nor would I be competent to do so. Instead, I shall merely present some brief subjective first impressions of the main themes of the symposium, apologizing in advance if they seem trite and platitudinous.

Our most direct information about the early Universe comes from the microwave background. Blair brought us up to date on its spectrum. These measurements are all now *consistent* with a ~ 2.7 K thermal spectrum (the millimetre 'excess' previously reported being no longer regarded as a genuine cosmic effect); but the shape of the spectrum is still ill-determined at millimetre wavelengths, so there is no reason to believe that it necessarily follows an exact black body curve. This work – together with the remarkable isotropy on small angular scales reported by Boynton – renders any theories that ascribe this radiation to discrete sources at 'recent' ($z \lesssim 10$) epochs even more *ad hoc* and contrived than they were before, thereby strengthening the conventional view that the microwave background is indeed primordial.

Accepting this, we can infer that the Universe was accurately Robertson-Walker back to the last scattering surface. Furthermore, as has been shown by Sunyaev and his collaborators, the lack of observed distortion in the spectrum tells us that the Universe must have been fairly smooth right back to $z \gtrsim 10^5$. For these reasons, the standard isotropic 'big bang' model has been widely adopted as a basis both for interpreting observations and for theoretical calculations. Many of the speakers have adopted a deductive approach, where they have considered some aspect of the physics of the early Universe, and attempted to deduce some consequences which can be confronted with observations.

An example of this deductive approach which we have heard a great deal about concerns the origin of structures – galaxies, clusters, and (maybe) superclusters – in the Universe. It was shown in Lifshitz's classic 1946 paper that 'small' initial perturbations of a Friedmann model can eventually develop into bound systems. But it would plainly be unsatisfactory if one had to feed into the initial 'genetic code' (as it were) *all* the properties one wished to account for. To do this would amount merely to saying 'things are as they are because they were as they were', and would really not explain anything. So the aim of the game is to invoke some smooth spectrum of perturbations, specified by as few free parameters as possible; and hope to show how selective viscous damping, non-linear interactions between different scales, etc. can gradually impress characteristic features on the spectrum, and cause the eventual condensation of bound systems with certain preferred masses. Hopefully, these should correspond to the scales actually observed; and one might also hope to predict mass-density and mass-angular momentum relations.

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We have heard several reports of theoretical progress along these lines. Zel'dovich, Sunyaev and their collaborators have shown how galaxies and clusters might form from 'curvature fluctuations' with amplitudes 10^{-4} on *all* scales, the amplitude being measured when each particular scale first comes within the particle horizon. The key phenomenon here – first calculated by Silk – is the viscous damping of oscillations on scales $\lesssim 10^{12}\ M_\odot$ before recombination. The Zel'dovich group have considered the behaviour of *non*-spherical perturbations. After recombination, they collapse to form sheets (or 'caustic surfaces') where the density is enhanced by a large factor. Radiative cooling prevents the gas from rebounding elastically, and then sheets develop into galaxies.

An alternative hypothesis concerning the initial fluctuations is that they primarily involve vorticity, the accompanying density inhomogeneities being of second order. At this meeting, some consequences of this assumption were described by Ozernoi. Similar ideas have been developed in the 'West' by Harrison, Jones, Stein, Ames, Silk and others. If the initial perturbations have large enough amplitude, then interactions between eddys on different scales will establish a Kolmogorov spectrum. The random velocities then, after recombination generate density inhomogeneities, whose spectrum, being determined by the properties of incompressible turbulence, is more or less independent of the detailed character of the initial perturbations. The only important adjustable parameter in the primordial turbulence picture is the amplitude. However the required initial perturbations are perhaps somewhat less general in form than the 'curvature fluctuations' invoked in the other approach.

None of the workers in this field would really claim to have 'manufactured' a galaxy, although some success has been achieved in accounting for the characteristic masses of galaxies and clusters. It is still unclear what happens between recombination ($z = 10^3$) and say $z = 10$. It should in principle be possible to discriminate between the alternative ideas about the nature of the initial fluctuations, because they predict different mass-density and mass-angular momentum relations; but the data on these relationships are still very sparse. Other relevant observations are the limits on the microwave background isotropy on small angular scales which, in particular, constrains the permissible amplitude of primordial turbulence and evidence on the redshift at which galaxies actually formed.

One cannot discuss the 'reasonableness' of the postulated primordial irregularities without facing the basic problem of initial conditions – or, at least, conditions at very early epochs when the particle horizon encompassed far less than a galactic mass. It is important to recall that, since radiation can be thermalised at sufficiently early epochs ($z \gtrsim 10^5$ or $z \gtrsim 10^8$, depending on assumptions), any entropy injected before then would have established thermal equilibrium. The thermal character of the microwave background spectrum thus tells us nothing about whether the universe was 'Friedmannian' in the first year of its life, nor about the adiabat along which it was evolving at that stage. However Wagoner emphasised that if the bulk of cosmic He^4 is primordial, the expansion timescale cannot have differed by even a factor ~ 2 from that given by the standard isotropic model. Further, the assumption that deuterium is primordial constrains the adiabat so as to imply a low density universe with $\Omega \lesssim 0.1$, and also means that the entropy must have already been present at $t \simeq 10$ s ($z \simeq 10^9$). Even though there are plausible ways of making deuterium, and conceivable ways of making the cosmic helium, these results give us at least some confidence in extrapolating the standard 'hot big bang' model right back to that time. It is hard to imagine how helium could be turned back into hydrogen by ordinary astrophysical processes, so there are very cogent objections to a cosmology which predicts much *more* than 25% primordial helium.

Extrapolating back still further, one naturally becomes more dubious about the applicability or completeness of 'known' physics. But one has to venture into these deeper waters in order to tackle the two most basic puzzles raised by the 'hot big bang' model.

The first of these concerns the origin of the entropy: why are there $\sim 10^8$ photons per baryon? Puget has described an ambitious attempt to answer this question which he is pursuing with Omnes and other colleagues. In this work, the net baryon number of the Universe is zero. A complex separation mechanism is invoked to explain why the baryons do not *all* annihilate; and the present ratio of particles to photons then represents the fraction of baryon-antibaryon pairs that have escaped annihilation and survived. There are two stages in the separation mechanism: first, a 'phase transition' occurring when $t \simeq 10^{-5}$ s, which separates matter and antimatter on a scale of $\sim 10^{-3}$ gm; and then a 'coalescence effect', which enlarges the aggregations until they attain galactic masses. This concept – while its details remain controversial and its conclusions in a state of flux – is exceedingly appealing because (if correct) it offers the prospect of deducing the entropy-per-baryon, and accounting for the existence and properties of galaxies, starting from a strictly homogeneous Friedmann model containing pure radiation, with no adjustable parameters whatsoever. The predicted primordial helium abundance is zero in this model, but this is certainly not a fatal defect because we cannot exclude substantial helium production in 'little bangs' early in the history of the Galaxy. This is surely an attractive enough goal to justify and motivate further development of these ideas.

If the Universe *does* have a net baryon number, one might alternatively assume that it started off 'cold' (or at least on a lower entropy adiabat) and that the microwave background was generated via dissipative processes. Several possibilities have recently been suggested in the literature. At this symposium, Zel'dovich and Novikov have discussed, in particular, the possibility of pair creation in regions of extreme space curvature.

Such dissipative processes – occurring either at the 'Planck time' $\sim 10^{-43}$ s or much later – would smooth out at least some kinds of initial inhomogeneity and anisotropy; and they thus relate to the second conceptual problem of the 'big bang': why is the Universe, in the large, as isotropic and homogeneous as Boynton and Partridge have told us it must be? No mechanism yet proposed seems capable of 'isotropising' a universe which starts off with the most general kind of anisotropy. This difficulty was discussed by Hawking, and led him to suggest that the only available answer to the question 'Why is the Universe isotropic'? might be that gravitational instability, galaxy formation, and therefore (?) cosmologists could not occur in any other kind of universe! Whatever the best solution to this problem may be, it is important to bear in mind that in (decelerating) Friedmann models the mass within a particle horizon shrinks to zero as one extrapolates back to $t=0$, implying that at early times there could have been no causal connection between the bits of matter which now belong to different galaxies. Consequently it is the Universe's overall large-scale uniformity which poses the major mystery, rather than the occurrence of galactic-scale primordial irregularities. The microwave background isotropy also constrains any overall rotation which the Universe might possess – a result of special import to adherents of some variant of 'Mach's principle'. One would however, like to have some idea of why the irregularities seem (according to the Zel'dovich group) to have had amplitudes of only $\sim 10^{-4}$, but further work along the lines outlined by Misner and Penrose is probably a prerequisite for this. Clearer ideas on the equation of state at temperatures $\gtrsim 300$ MeV ($t \lesssim 10^{-5}$ s) are also desirable, because this will affect the dissipation of small-scale irregularities at the earliest times (and also – as Wagoner pointed out – might affect the helium abundance if most of the baryons are contained in slowly-decaying 'super-baryons').

If we accept that, at all epochs accessible to direct observation, the Universe is indeed highly isotropic, this lends added interest to the 'classical' cosmological problem of determining which particular Friedmann model best describes the Universe. Tammann described the latest estimate of H_0, which yield a value ~ 55 km s^{-1} Mpc^{-1}, but other participants voiced scepticism about the precision of this determination. The value of H_0 is actually not of special cosmological interest, provided that the Hubble time is long enough to avoid conflict with age determinations, etc. The value of the deceleration parameter q (whose measurement involves a disjoint set of problems from those entailed in determining H_0) is still so uncertain that we cannot tell whether the Universe is closed or open (and the opinion poll conducted by Prof. Wheeler showed gratifyingly, and perhaps surprisingly, that most of us are prepared to wait for solid evidence before pronouncing on this question!). Even though Oke gave the exciting news that galactic redshifts may soon be measured out to $z \simeq 0.6$, most cosmologists seem resigned to the view that reliable estimates of q still lie a long way ahead. This is because of the uncertain – but possibly substantial – corrections needed to take account of evolution of the galaxies, gravitation effects arising from clumpiness of matter along the line of sight (an effect first pointed out by Dashevskii and Zel'dovich), obscuration, and selection effects (e.g. the 'Scott effect').

Another line of attack on the problem of whether the Universe is closed or open involves attempting to determine the density parameter Ω ($\Omega = 2q_0$ if $P/\varrho \ll c^2$ and $\Lambda = 0$) by searching for 'missing mass'. X-ray observations have provided evidence for diffuse gas, and as described by Field the amount of gas could be enough to yield $\Omega \simeq 1$ even if the gas were concentrated in clusters and groups, and the mean emission per unit mass enhanced accordingly. This is still, however, an upper limit rather than a firm result. On the other hand, there are many other forms (e.g. collapsed objects) which missing mass may take. Tammann mentioned another argument, based on the observation that the supercluster is expanding more or less in accordance with the Hubble law, which suggests $\Omega \ll 1$. The supercluster represents a volume where the density of galaxies is at least twice as high as the average. If the mean density of the Universe corresponded to $\Omega = 1$, a region with more than about twice the mean density would not be expanding at all, contrary to observation. This is a suggestive argument, but it would not apply if the 'missing mass' were predominantly in some weakly-interacting relativistic form, because the distribution of such material would be much more uniform than that of galaxies.

The radio source counts and the distribution of quasars (reviewed by Longair and Pauliny-Toth) still provide no evidence at all on the deceleration parameter, because the drastic dependence of average source properties on cosmic epoch is not theoretically understood and cannot be corrected for. The main interest of these studies lies in the clues they may provide to the astrophysical nature of the objects themselves. Searches for possible anisotropies in the radio source distribution may, in conjunction with optical work, reveal possible inhomogeneities in the distribution of matter on scales $\gg 30$ Mpc.

All the work I have alluded to so far has been performed and interpreted within the framework of a 'standard' picture of the Universe. But, as we all know, some astronomers seem convinced that the data already reveal contradictions which require us to abandon the 'established' world picture, or at least modify it drastically. Such views are held by a minority – a minority represented at this meeting only by Arp – but it would be wrong to deny this radical viewpoint serious consideration; and would indeed be especially inappropriate to do so at a symposium linked to the memory of Copernicus. I won't attempt to review Arp's arguments here; still less will I try to summarise the multifarious arguments adduced by others over the last few years in support of 'non-cosmological' redshifts. But the following brief comments may be apposite. It is all too easy to perceive patterns in random data – patterns which may (when their 'statistical significance' is tested a posteriori) be 'improbable' at the 1% level. Indeed it ought to be superfluous to emphasise the methodological dangers of this procedure when one has not formulated a well-defined hypothesis in advance. As more data accumulate, it is inevitable that *more and more* surprising effects will be discovered. Unless, however, these can *nearly all* be incorporated into a single theory which is as specific and clearly defined as the cosmological hypothesis (and we must not forget the great body of data that *is* consistent with the latter), these effects cannot be claimed as adding cumulative weight to an unorthodox viewpoint. Certainly nobody has devised even the outlines of a model which could account for the various peculiarities Arp has claimed: the alleged correlation between quasars and nearby galaxies, the redshift-angular size relation for QSS's, 'superlight velocities', redshift 'periodicities', etc. Another methodological weakness is that the relevant objects have often been singled out for study only because (for example) there *was* a quasar nearby. There is also of course the possibility that superposed isophotes can give rise to apparent 'bridges', which can have a variety of shapes if the superposed objects are both extended. One wonders also how many 'normal' galaxies would reveal genuine excrescences or a 'disturbed' appearance if subjected to comparably intensive scrutiny. The issues involved are of such fundamental importance that one fervently hopes these studies will indeed be pursued in an increasingly systematic fashion. At the moment, however, most astronomers will probably prefer to suspend judgement on the significance of bridges between objects of different redshift, and on the other alleged evidence for non-cosmological redshift; and to adopt the conventional picture as a working hypothesis unless and until some really blatant contradiction emerges, or a more attractive and comprehensive specific alternative viewpoint is proposed.

If Arp and his colleagues were right, we would be somewhat further from delineating the large scale structure of the cosmos than most people now believe. One would not necessarily have to jettison the whole 'hot big bang' scenario because, as Ambardsumian long ago suggested, the anomalous effects may be restricted to a peculiar class of object, or to special regions of space. If these ideas *were* right, however, they would have the exciting corollary that astronomical studies would have revealed some fundamentally new basic physics.

Shortage of time does not allow me even to mention the many other new results reported at this symposium. Before concluding, however, it might be worth mentioning some of the areas where rapid progress seems most likely in the next few years. At the top of my list would definitely be observations of the microwave background on small angular scales. The present upper limits to $\Delta T/T$ are tantalisingly close to the level at which one might expect to see effects resulting from inhomogeneities on the 'last scattering surface', and it would be disappointing if a modest further technical imimprovement did not yield some positive results. The spectrum of the background radiation at millimetre wavelengths should soon be pinned down both by space observations and by studies of interstellar molecules. This would provide constraints on just how 'chaotic' the early Universe could really have been. We already know (from the 24 hr isotropy of the background and from the precision with which Hubble's law is obeyed) that the velocity field is remarkably smooth, but we know remarkably little about the distribution of matter on scales $\gg$ those of superclusters. Such information may come from galaxy counts, supplemented by evidence on the distribution of radio sources. Technical improvements proceed apace in X-ray astronomy, and we can therefore expect firmer evidence on the amount and clumping of intergalactic gas. It should, by 1980, be feasible to detect X-rays from individual clusters out to $z \simeq 3$, and this would be of obvious importance for theories of the thermal history of intergalactic gas, and the evolution of galaxies and clusters. There seems no good reason why optical and radio astronomers should not detect quasars with substantially larger redshifts than those so far measured, and this would have an obvious bearing on theories of galaxy formation. Finally, of course, we can expect continuing progress on the 'classical' problem of determining H_0 and q_0 optically.

The microwave background has brought the physics of the very early Universe within the framework of serious scientific discussion, and we can expect further theoretical work on the equation of state at the earliest epochs, the problems of matter-antimatter separation, primordial element production and the origin of galaxies. There will also be fuller investigations of some even more fundamental questions – the origin of the entropy, the reason for the Universe's overall isotropy, and the nature of the singularity – and one hopes that these ideas will have further consequences that can be confronted with observation.

At the risk of introducing too cynical a note into the symposium proceedings I would like to finish with an extract (which Prof. Zel'dovich kindly showed me) from the autobiography of Will Rogers: "A week or so ago I attended my first thing called a symposium. I didn't know if it was going to be a circus, burlesque show, or a preaching. Well, it was all three... All this exchange of talk is a lot of hooey: it changes nobody or affects no opinions; it's kind of like weather-talk, it does no harm. But a symposium is really pretty good. If one ever travels through your town and plays there, go hear it. It's the old cracker-barrel arguments over again." Well, we have heard much talk here; but there have been many new arguments, and some people may even have changed their opinions. Scientists often seem to be opinionated and dogmatic to an extent that correlates inversely with the number of relevant facts: but cosmological facts, though still limited, are certainly not as sparse as they were a decade ago. I am confident that this trend will continue, so that when the next IAU cosmology symposium is held, we won't just hear all the same arguments again, but that progress in the intervening years will have clarified our ideas on the many fascinating topics aired and discussed here in Cracow during the last three days.