SUMMARY

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This conference has been marked by our willingness to entertain grand schemes of synthesis of theoretical ideas and the observational evidence on how galaxies and large-scale structure might have formed. At IAU Symposium 104 on this subject held in Crete just 5 years ago there was little discussion of how all the pieces of the puzzle might fit together. Now we have at least three candidate grand syntheses that have been worked out in some detail and have been discussed here: scale-invariant cold dark matter, by Frenk; massive cosmic strings, by Turok; and exploding magnetized superconducting cosmic strings, by Ostriker. I have accordingly placed each of the topics I want to review under the heading of the grand scheme for which it seems most embarrassing. This negative approach is a little unfair, but I think we can take it as given that we would not be considering a scheme that did not have many good points, and that the real interest is the probing of weak points by which we hope to learn which schemes might be strengthened, which might safely be abandoned.

1. INFLATION

The inflation scenario suggests a beautiful resolution to the vexatious problem of initial conditions for the classical Big Bang cosmology. We discussed two possible points of contact with observation, the mean mass density and the initial mass density fluctuations out of which large-scale structure might have grown.

We have two observations that suggest the mean mass density may be as high as the critical Einstein-de Sitter value that most naturally fits inflation: the Loh-Spillar application of the classical cosmological tests and the IRAS galaxy dynamical test discussed by Davis and Yahil. The Loh-Spillar method will I think prove to be of lasting importance. The main criticism I have heard is that the Loh-Spillar ansatz for evolution of the galaxy luminosity function is too crude; the challenge now is to come up with more realistic models that can be fitted to the joint distribution of galaxy redshifts and

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apparent magnitudes. The criticism we heard of the IRAS result is that these galaxies avoid the neighborhood of the Coma cluster where we know there is a considerable mass concentration. This suggests that the IRAS galaxies are more smoothly distributed than is the mass, so the test may overestimate the mean mass density. Indeed, Davis found that a rough correction for the bias yields the familiar result from optical dynamical tests, a density about 30% of Einstein-de Sitter.

Turner discussed candidates and experiments that might detect dark matter. The experiments are beautiful, and it would be hard to overstate the importance of a positive outcome. It behooves us all to lend our support to this effort.

If the mean mass density were high we would have to explain why, according to the dynamical tests, galaxies detected in the optical are more strongly clustered than is mass. As discussed by Rees, proposed explanations can be classified as "astrophysical biasing" or "natural biasing," a term introduced by Davis, Frenk, Efstathiou and White. Examples of natural biasing would include galaxy formation in clusters in a smoother mass background, as would be the natural outcome of the massive cosmic string scheme, in which converison of baryons to stars may be quite inhomogeneous, or an explosion that piles up the baryons in ridges and leaves the dark matter behind.

Astrophysical biasing assumes that the seeds of galaxies were distributed like the mass but that seeds in the voids were prevented from developing into recognizable objects by an environmental effect, perhaps due to the galaxies that formed earlier in groups and clusters in regions of unusually high ambient density. A seed might fail to develop into a galaxy either because the stellar IMF has been altered to a shape unfavorable for the accumulation of 0.1 to 1 solar mass stars or because the baryon space distribution has become too compact or too broad for easy detection. Rees noted that the resulting "unborn galaxies" might serve to bind the hydrogen clouds in the Lyman α forest discussed by Sargent. There are however serious observational problems with this astrophysical biasing concept. First, we see the effect of environment in morphology segregation, but to my mind the striking point is the similarity of diameters, luminosities and mass-to-light ratios of the inner parts of the early and late type galaxies found in such exceedingly different environments, which we would not have expected if luminosity or surface brightness could be strongly affected by environment. Second, if galaxy structure and/or luminosity were capable of modification by environment we surely would not have expected that observable galaxies would show a tight correlation between luminosity and a structure-dependent quantity, the velocity dispersion or 21 cm line width. How could astrophysical biasing be reconciled with the fact that the Faber-Jackson and Tully-Fisher relations both are remarkably tight, which means they cannot be sensitive to environment? Third, we might have expected to have seen an appreciable abundance of dwarf galaxies among "almost unborn galaxies" in the voids. Einasto and Tully emphasized that this is not observed for gas rich dwarfs. The limits are poorer for dwarf ellipticals and spheroidals, but it would seem perverse if the remnants of frustrated galaxies were gas free, given that gas rich dwarfs can exist near giant galaxies.

The second possible contact between inflation and observation is the production of the initial mass density fluctuations out of which large-scale structure might have grown. We learned from Kofman that inflation is capable of producing a great variety of initial fluctuations. This is good news, in the sense that inflation can be adjusted to fit the observations; but bad news, in the sense that we have lost one of the few predictions of the scenario.

2. SCALE-INVARIANT COLD DARK MATTER

As Frenk noted, this is one of the first schemes one would want to try: the assumptions are simple and consistent with inflation, the parameters are few, and the dynamics can be modeled in a fairly clean way. I continue to be impressed and surprised at how good the model results are on intermediate mass scales. There are two crises: the predicted mass autocorrelation function is negative on large scales where people see evidence of structure in configuration and velocity, and mass fluctuations on small scales are small so galaxies form uncomfortably late in the theory.

Large-scale structure is one of the main subjects of this conference; we heard results from Huchra, da Costa, Giovanelli; Shvartsman, Karatchensev, Bahcall, Tully, Shaver and Koo on surveys of angular and redshift distributions. My impression is that the clustering pattern on scales less than about 20 h^{-1} Mpc ($H = 100\ h$ km sec^{-1} Mpc^{-1}) is pretty well measured and not inconsistent with what would be expected in the cold dark matter scheme. There is considerable evidence of structure on scales $\gtrsim 50\ h^{-1}$ Mpc, but I think it is fair to repeat the old questions: could this be an artifact of errors in the catalogues? Could the eye be picking patterns out of noise? If the answers were definitely "no" it would be very damaging for scale invariant cold dark matter. We all will be following the debate with great interest.

Several other probes of large-scale structure should be mentioned. Meszaros showed that structure is usefully constrained by the anisotropy of the X-ray background. (It is frustrating that we still do not have an estimate of the autocorrelation function of the X-ray sky surface brightness.) Pariski and Kellermann remarked that it probably is time to take another look at radio source clustering, to complement Shaver's discovery of quasar clustering evolution. Kaiser and Bond concluded that present limits on the microwave background anisotropy on scales $\lesssim 1°$ are not threatening. The larger scale result described by Lasenby is not expected in cold dark matter (and is about consistent with the baryonic isocurvature model) but this still is a preliminary result. The still larger scale limits from RELICT reported by Strukov are important as a check that the observed anisotropy really is dipole and thus reasonably interpreted as the result of our motion, and as remarkably tight bounds on large-scale mass clustering, but are not immediately threatening to scale-invariant cold dark matter (which was designed to suppress the quadrupole moment, the price being suppression of large-scale structure).

Let us consider next the large-scale velocity field. The dipole anisotropy of the microwave background is most reasonably interpreted as the result of the peculiar motion of the Local Group, about 600 km sec^{-1}. Faber and Burstein argued that an appreciable part of this velocity is in a component that varies more or less smoothly to a maximum some 40 h^{-1} Mpc distance from us, and this picture received dramatic supporting evidence from Mould and Rubin. But Davis and Yahil arrived at a very different picture. They concluded from the IRAS galaxy redshift survey that the peculiar motion of the Local Group is within ~ 20° of the direction to be expected if it were the result of the gravitational attraction of the mass in and around the Local Supercluster(say, within 10 h^{-1} Mpc distance of the Virgo Cluster). This would suggest a relatively small coherence length for the peculiar velocity field, which would be consistent with the cold dark matter scheme. If the Faber _et al_. picture were right the velocity of the Local Group would have a substantial contribution from a "great attractor" well beyond the Local Supercluster, so the IRAS result would have to be accidental, which does not seem all that likely. It is hard to see how the large coherence length suggested by the old Rubin-Ford effect and by the new results of Faber _et al_. could be reconciled with the scale-invariant cold dark matter scheme. Again, the debate on which is right will be followed with considerable interest!

The second major crisis for the scheme is that it tends to assemble galaxies rather late. This is seen in the numerical N-body model results of Frenk _et al_. The mass distribution in the model at the present epoch has distinct concentrations that the authors have identified as halos of galaxies on the very reasonable grounds that they have about the right abundance, clustering properties and gravitational potential well depths. But these mass concentrations traced back to redshift $z = 1$ typically break up into smaller clumps spread over hundreds of kiloparsecs, which certainly is not right. The sharp debate is over whether it is a reasonable step in the right direction.

On the observational side, we need estimates or lower bounds on two epochs, the redshift $z_\star$ at which the bulk of star formation had been completed, and the redshift z_g at which the bulk of the visible parts of galaxies had been assembled. Dressler gave ample evidence that star formation is an ongoing process, but noted also that the spectra and infrared luminosities of many galaxies seen at redshifts approaching unity look like a predominantly old stellar population, passively evolving from formation with a standard IMF a few billions of years earlier. On the face of it, this is evidence that z_* is well above unity. For z_g we note that the majority of bright galaxies have thin discs that seem to be delicate and so presumably have not been disturbed since the disc stars formed. (Mass could be added after disc formation as a gentle rain of gas; but accretion of dwarf galaxies of the sort expected in hierarchical galaxy formation seems to be ruled out.) The disc of our galaxy contains white dwarfs whose age Liebert et al. put at about 9 billion years. Since our disc seems to be typical, I conclude that $z_g > 1$.

There is some evidence for still earlier bounds. If the Wolfe HI clouds are discs of galaxies, or the CIV absorption systems arise in galaxy halos, as Sargent argued, then $z_* \gtrsim 3$. The same follows if quasars formed in typical galaxies. Wilkinson instructed us that the 700 μ anomaly found by the Berkeley-Japan cosmic background measurement should be treated with caution because the measurement is difficult, but should be seriously considered because the measurement was well executed. Two explanations for the anomaly were discussed. Comptonization would require a considerable energy source at high redshift, which could not be arranged in the cold dark matter scheme, but, Ostriker remarked, would be expected from dissipation of magnetized superconducting cosmic strings. Thermal emission from dusty young galaxies peaks up at $\lambda \sim 50$ μ. Bond noted this would produce the anomaly if $z_* \sim 10$.

The present challenge for cold dark matter is to find a prescription for galaxy formation that satisfies the minimal constraints $z_* > 1$, $z_g > 1$, and that at $z < 1$ holds the merging rate for the majority of galaxies (the spirals) to a low level. The danger is that if this were accomplished it would threaten biasing, for there are in the low density regions of the model many concentrations massive enough to make star clusters of the sort that are so conspicuously absent from nearby voids. The same would apply if to increase z_* it were assumed that stars formed before galaxies. And a final worry: if the Frenk et al. model were adjusted to make $z_g > 1$ it would seem reasonable to suspect that that would spoil the successful prediction of galaxy clustering and abundance.

We had stimulating exchanges on two lessons to be drawn from detailed models of formation of galaxies. First, Freeman argued that the systematics of spiral galaxies, about which a good deal is known, could be understood if these objects were assembled out of gas rich star clusters. Ostriker disagreed, noting the problem of understanding the correlation of bulge metallicity with luminosity if the bulge were assembled from pre-existing stars. The point is important because in any simple variant of the cold dark matter scheme galaxy formation would proceed in a hierarchy commencing from gas clouds at the Jeans mass, $\sim 10^6$ M_0. Second, if galaxies formed at $z_g \sim 1$ they would have to collapse by a considerable factor. White described a model in which a protogalaxy at $z \sim 3$ would have been a collection of star forming gas clouds with a small overall mean density contrast. To reach the present typical mean density within the Holmberg radius this system would have to collapse by some four orders of magnitude in density. As Gunn observed, one might wonder whether this is reasonable in a star forming system. Ostriker notes that if the collapse factor were achieved it might make bulges of spirals rotate unacceptably fast.

Theory also is constrained by the evolution of the clustering pattern. In the cold dark matter scheme the large-scale clustering pattern would expand with the general expansion of the universe because it is a pattern on a nearly smooth mass background. I presume that what happens in the model on small scales depends on how galaxies are identified. Shanks and Loh concluded that the galaxy two-point correlation function is evolving in a way roughly consistent with stable galaxy clustering in an evolving backgound density. This might be a problem for biasing. As Shanks noted, the existence and clustering of CIV lines at $z \sim 2$ (as discussed by Sargent) is a serious challenge to the cold dark matter scheme. On the other hand, clustering of the Lyman α clouds is considerably smaller than the extrapolated stable model for galaxy clustering, and is most naturally interpreted in Rees' biasing model, where the gas is confined by frustrated galaxies.

If we decided from all this that the standard scale-invariant cold dark matter scheme is observationally untenable we could of course turn to variants. Increasing the amplitude of the density fluctuations would help make galaxies form earlier, but there is the danger mentioned above that this would suppress biasing and would tend to spoil the successful prediction of galaxy clustering and abundance. Kofman showed that the inflation scenario can be adjusted to change the shape of the mass fluctuation power spectrum to add power on large scales, which would ease the problem with large-scale structure. Primack remarked that the same could be accomplished by lowering the density parameter (though that does vitiate the argument for exotic matter). Umemura discussed models with two types of dark matter. We were also invited to reconsider the old pancaking hot dark matter model. The problem with the original version is that it produces galaxies after the collapse of protoclusters, while we are in an old galaxy in an apparently young part of the Local Supercluster. Doroshkevich and Sato remarked that the youth of the Local Supercluster could be only apparent if it formed out of dark matter that decayed and allowed the cluster to expand nearly freely after the initial gravitational collapse. I cannot resist taking note of the slight air of unreality of these discussions of hierarchies of hypotheses.

No matter how this debate is concluded the scale-invariant cold dark matter scheme will be lastingly remembered for providing us with a workable test problem: trace the evolution of pressureless dissipationless matter starting from a spectrum of density fluctuations that conveniently concentrates the epochs of collapse of a wide range of length scales to a relatively narrow range of time. Frenk and his colleagues have provided a beautiful solution, but still problems remain and were raised by Bouchet, Starobinsky and others. Here is an example. Frenk et al. build initial conditions from a Fourier series in $\delta\rho/\rho$ that is dominated by a relatively small number of components. Each component has a Gaussian distribution in real and imaginary parts. Those components that happen to be assigned unusually large upward fluctuations in amplitude would be even fewer in number and, if few enough, would produce artificial planar structures in $\delta\rho/\rho$. Could this account for the striking linear features seen in some models?

It was clear from the discussion at this meeting that there is considerable interest in research in the numerical method of modelling the evolution of cold dark matter, and that there is a pressing need for complementary progress on the theoretical side following the way shown by Shandarin. Equally pressing is the need to find a sensible way to treat the hydrogen. The physics is a good deal harder but, as Ostriker stressed, we have the great advantage of observational guidance.

3. MASSIVE COSMIC STRINGS

Cosmic strings with the wanted mass per unit length do not fit very naturally into the standard inflation scenario, but given the observational situation for inflation I do not see that that is a serious problem. As Turok showed, a very attractive feature of the cosmic string picture is that it offers a natural way to account for large-scale structure, including a quantitative prediction of the abundance of rich clusters of galaxies. Perhaps the weakest point is the galaxy mass function, as follows.

It will be recalled that the circular velocity, v_c, at 5 h^{-1} kpc from the center of a large spiral galaxy typically is $v_c \sim 250$ km/sec. The value of v_c for giant ellipticals (here v_c is the speed of a star in approximately circular orbit) tends to be a little higher. The record for any galaxy is $v_c \sim 500$ km sec^{-1}. Rubin has offered a prize of a \$100 U.S. bill for the discovery of a galaxy with $v_c > 600$ km sec^{-1}; she has received no claims. We see that nature is quite adept at placing a mass of 1×10^{11} h^{-1} $M_\odot$ in a sphere of radius 5 h^{-1} kpc, but supresses if not forbids the placing of four times that amount in the same volume! In the cosmic string scheme galaxies grow by accretion of matter onto loops. The loop mass, m_ℓ, has a power law frequency distribution, so the seed mass perturbations provide no reason for this remarkable property of the galaxy mass function. Thus the challenge is to find some cause from secondary effects such as loop motions or explosions of the accreted material.

It will be noted that a satisfactory explanation must take account of the transition cases. That is, the mechanism that suppresses accretion around dangerously massive loops presumably has an effect that varies with m_ℓ in a continuous way, so there is some range of values of m_ℓ for which the effect is strong enough to disturb the galaxy but not strong enough to make it unrecognizable as a galaxy (or masquerade as the descendant of a low mass loop). For example, if accretion were suppressed by loop motion, the loop speed increasing with increasing m_ℓ, then the transition case would be massive galaxies with low central concentrations (which I think are not observed).

I have been advertising this mass function problem for the past year and have seen no promising response, and so am beginning to suspect that the theory fails the test. Following Rubin, I hereby offer a prize of a $100 Canadian bill and a citation suitable for framing for the first explanation of the galaxy mass cutoff that is based on the standard elements of the massive string scenario and that is reasonable in terms of astronomy and physics, reasonableness of entries to be judged by the members of the Canadian Institute for Theoretical Astrophysics.

4. EXPLOSIONS

This picture, a natural extension of what is observed to happen in the interstellar medium, predicts that galaxies tended to form on sheets, which agrees with recent observations, which is a considerable triumph. The potentially most damaging observation is the one discussed by Faber and Burstein: galaxy peculiar velocities may have a coherence length larger than the typical bubble size. It is hard to imagine how an explosion could produce motion uniform over large scales either directly by non-gravitational forces or indirectly by the gravitational field produced by the large-scale rearrangement of mass by explosions, while leaving undisturbed the frothy galaxy distribution and quiet Hubble flow observed on smaller scales. The large-scale velocity field is still controversial, however, so we must suspend judgment on this critical point.

There is also a problem with the local velocity field. The galaxies within 10 Mpc are concentrated in the thin plane of the Local Supercluster (LSC), but there are many galaxies well off the plane, in Tully's spurs. Following Ostriker, let us suppose that the LSC is a remnant of a shell around a hole blown by an explosion. The spurs would be parts of other intersecting shells, or else debris from the explosion that produced the ridge of the LSC. Now with the standard prescription for the conversion from heliocentric velocity to velocity of the Local Group (LG), the velocity of the LG relative to the microwave background and normal to the plane of the LSC is 350 km $\sec^{-1}$. Since the LG is in the plane of the LSC the natural presumption is that the plane is moving with the LG, and in fact this is a reasonable value for the rate of expansion of a hole produced by an explosion of the wanted size. The problem is that the redshift-distance relations for nearby galaxies on and off the plane are indistinguishable, while one would have looked for a systematic difference in streaming motions if the spurs were other shells passing by. Aaronson and his colleagues have provided us with an exceedingly valuable probe of the local velocity field, a catalogue of infrared magnitudes and 21 cm line widths for nearby galaxies and a calibration to the infrared Tully-Fisher relation (IRTF) based on more distant clusters. At IRTF distances cz ~ 700 km/sec the rms scatter in redshift corrected to the Local Group is about 200 km/sec, on and off the plane of the LSC (which is consistent with the uncertainty in cz). If galaxies were produced on moving ridges how can we understand why the difference in velocity on and off the plane of the LSC is so small?

Yet another challenge to the explosion picture might be mentioned: the small scatter in the redshift - IRTF distance relation means that the luminosity - Δv_{21} relation is quite similar for nearby galaxies on and off the plane of the LSC. My guess is that this will be hard to explain in a scheme where galaxies on and off the plane were produced in different explosions or in different phases of the same explosion.

5. A LIST OF ISSUES

It might be useful to list the general issues involved in our present search for an understanding of the structure of the universe. I have ordered my list under three questions: what is the main sequence of events, what are the principal actors, and what are we supposed to explain? The broad issue behine the third question is the role of observation in the invention of theory. One extreme point of view is that cosmology is such a deep problem and the observational evidence so indirect that we will have to be content with the guidance of general principles and broadly conceived analogies. The extreme opposing view would be that we ought to deduce the theory from the observations. I don't think the latter is any more viable here than in any other physical science. The former approach has worked - that is how we got the successful Big Bang cosmology - but we have a considerably denser web of evidence now and I suspect we might profit by consulting the observational entrails a little more closely than is the present standard practice.

Almost everyone includes among the list of principal actors in cosmology baryons and the blackbody radiation background; the key issue is whether we must take account of more exotic contributions to the stress-energy tensor. Cosmologies based on baryons and radiation alone (the latter including neutrinos with negligible mass) have been unpopular for good and familiar reasons, but they have not been ruled out, and there are even some attractive cases, such as the pure baryon-radiation isocurvature scenario I have been considering lately.

The most popular exotic case is cold dark matter, which merits further intensive study because it is particularly simple, but we should not overlook hot dark matter, in the form of massive neutrinos. Massive neutrinos were generally abandoned a few years ago when analytic and numerical N-body model studies suggested that it would be difficult to arrange that galaxies form at reasonably high redshift without overproducing clusters of galaxies. If we are willing to assume that galaxies formed at low redshifts this objection is weakened and perhaps should be reconsidered. And of course accretion of massive neutrinos in isocurvature perturbation scenarios remains a very interesting possibility. Many other forms of dark matter particles are available for study, if we choose. Each of us must judge the balance between the desire not to overlook a good possibility and the reluctance to compound hypotheses.

Among non-particle exotic objects cosmic strings are particularly attractive because they offer such an unusual and promising variety of effects. A related object from another gauge theory, a primeval magnetic field, has been less popular because no one has been able to think of how the field could be produced in the Big Bang. Years ago Dicke noted that the field could be a remnant of previous phases of an oscillating universe. The appearance of a primeval magnetic field as one ingredient of the theory of Ostriker, Thompson and Witten has lent magnetic fields a certain present psychological credibility. And the stress due to a magnetic field of interesting strength (10^{-9} Gauss at 10^{-6} protons cm^{-3}) would have an interesting effect on the evolution of the large-scale baryon distribution.

The course of evolution of the structure of the universe depends on the stresses. The dominant force on the scale of galaxies and larger at the present epoch is gravity, and gravity usually is taken to dominate the late stages of formation of large-scale structure. The issue is the role of non-gravitational forces at higher redshifts, say, $z \gtrsim 3$. In the adiabatic gravitational instability picture structure grew continuously out of very small inhomogeneities originating at $z \gg 10^{25}$. At the other extreme is the explosion picture in which the large-scale mass distribution is violently rearranged at fairly low redshifts, $z \sim 5$ to 10. And we all can think of theories that would be ranked between these extremes. My guess is that the gravitational instability picture with Gaussian initial fluctuations from homogeneity is unable to account for the curious character of the galaxy distribution on very large scales, where we see rare large departures from homogeneity. It would be easy to remedy this by assuming non-Gaussian fluctuations, less easy to justify. I consider it an attractive feature of the explosion picture that it naturally produces non-Gaussian perturbations. The same is true of cosmic strings and the stresses of a primeval magnetic field, and, I expect, of any process that initiates formation of large-scale structure at fairly low redshifts.

I have already mentioned in the previous sections many of the most pressing observational issues, but it might be good to present a short list here. What is the coherence length of the galaxy peculiar velocity field? What is the largest distance across which fluctuations in the galaxy space distribution are physically related? What is the time evolution of the space distribution? When were galaxies assembled? When did their discs form? When did the bulk of the stars form? What limits can be placed on the variation in space and time of the star initial mass function, particularly the crucial high and low mass ends? How were galaxies assembled? Out of material with high or low mean density? Out of a single gas cloud or a collection of gas clouds or a collection of systems of stars and gas? Is it correct to use Newtonian mechanics to deduce the presence of dark mass in the outer parts of galaxies? If so, what constraints can we put on the nature of the dark mass: low mass stars or planets; black holes or other stellar remnants; exotic matter? What might be lurking in the vast spaces greater than one megaparsec from any bright galaxy? Clouds of baryons? Clouds of antibaryons? Dead or unborn galaxies? Still more exotic objects? What is the mean mass density? What is the mean space curvature? Are we going to have to learn to live with a cosmological constant?

5. CONCLUDING REMARKS

I hope it is understood that the negative tone of my summary signifies a very positive and exciting state of affairs for our subject: we know enough to have reason to hope that an understanding of the nature and origin of large-scale structure is within our grasp, but not so much that the answer is at all apparent. We have several promising theories and can expect to see more and we have a rich and growing web of observations that we can use in thorough critical analyses of the theories. I think it is reasonable to hope that something believable will come of this.

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