

## POSTER DISCUSSION

C. NORMAN, the session chairman, invited J. Bardeen, P. Hut, R. Larson, D. Lynden-Bell, B. Paczynski, M. Rees and E. Salpeter to introduce subjects covered in the posters. Each of their comments was followed by a brief general discussion.

HUT: One of the most important problems which we face in this meeting is the question: What is the composition of the most abundant form of matter in the Universe? We have heard from many speakers that the observed types of matter contribute only a small fraction of the gravitationally inferred average matter density. Whether the main type of matter consists of exotic elementary particles or plain old baryons, we do not know.

The dark matter problem is most dramatic on the scale of clusters of galaxies, where the ratio of unseen to seen matter is largest. Some inferences can be made from the distribution of galaxies on these and larger scales. For example, we heard that the observed distribution can be reproduced much more easily with "cold" elementary particles than with "hot" particles such as neutrinos.

However, there are advantages to starting closer to home. Here the observations and theoretical inferences are more direct, even though the dark matter problem is less dramatic. Near the Sun, we have dark matter in the galactic disk with a density comparable to that of the observed matter. I would like to discuss three poster papers which contain hints about the composition of this local dark matter.

Recently an upper limit has been put on the average mass  $M$  of the constituents of the local dark matter,  $M < 2 M_{\odot}$  (Bahcall *et al.* 1985, *Ap. J.*, **290**, 15). Because of the large population of wide and therefore fragile binaries, heavy black holes are excluded. Neutron stars are barely allowable on dynamical grounds (the upper limit quoted is rather generous), but pose severe problems for the metallicity evolution of the Galaxy. Elementary particles which are not dissipative would not have gathered in the galactic disk and are therefore also excluded.

Of the remaining candidates the only promising ones are white dwarfs and brown dwarfs. However, straightforward extrapolations of local observations of either type of star give densities far below what is needed. We have to investigate where we can reasonably deviate from these extrapolations.

One poster paper is clearly in favor of the first solution. In his paper on bimodal star formation, Larson suggests a deviation from a simple power law for the initial mass function (IMF) of star formation. He shows how a bimodal IMF can explain several problems in the chemical evolution of galaxies, and at the same time naturally produce enough white dwarfs to provide the local dark matter. His approach is interesting in that it is not entirely *ad hoc*; it is plausible in the wider context of observations of galactic evolution. The problem, of course, is the question of why we have not seen these many white dwarfs. Perhaps they cool fast enough to have escaped detection? If so, Larson's model has an extra advantage, since it produces most of the white dwarfs early in the history of the Galaxy.

Two poster papers contain arguments restricting the viability of the second solution. Boeshaar, Tyson and Seitzer provide upper limits on the

density of the brighter type of brown dwarfs in the solar neighborhood. Mathieu shows that observations of open star clusters are compatible with the absence of a significant population of brown dwarfs, which might suggest that they do not play a dynamically important role anywhere in the disk. But neither paper contains strong constraints, as the authors point out.

If we combine the arguments of these papers, it seems that white dwarfs are favored as candidates for the local dark matter, but brown dwarfs are not ruled out. So we have to resign ourselves to the fact that we still do not know the nature of half of the matter even in our own galactic backyard.

ALCOCK: A comment on the white dwarf situation. It is beginning to look as though the absence of very low-luminosity white dwarfs means that the observations are inconsistent with a constant rate of white dwarf formation. So in order to have a lot of unseen matter in degenerate stars, we could add an initial burst of white dwarf formation around seven billion years ago.

J. BAHCALL (to Richard Larson): Do you think that there is still a mystery in the observed numbers of white dwarfs?

LARSON: I suggested several ways around it. (1) The scale height of the oldest white dwarfs may be larger than we think. (2) White dwarf cooling theory may not be quite right. (3) As Alcock just pointed out, if all of the star formation occurs early enough, the white dwarfs are now all older than a cooling time. (4) If you allow me more parameters in my initial mass function, I can arrange to make the typical white dwarf mass be  $\geq 1 M_{\odot}$ , in which case the cooling time is almost certainly shorter than  $10^{10}$  years.

PEEBLES: Could the formation of the disk have compressed the halo material enough to account for the missing mass in the disk?

HUT: I don't think so, because of the high velocities in the halo. How would you trap it?

PEEBLES: Since you are deep in the potential well of the disk, you trap the low-velocity tail of the distribution of high-velocity objects.

HUT: I haven't looked at it, but you would have to increase the halo density by a factor of ten to account for the local disk density.

BARNES: I have done N-body models in which I start with a spheroid or cloud which represents the halo and then impose the disk field. You don't flatten the halo very much; you can't flatten it enough to get a local dark-matter density equal to that of the luminous matter.

BINNEY: I don't agree. How much you flatten the halo depends on how radial a velocity distribution you are willing to have in the initial configuration. The halo becomes squishable as you take away its tangential velocity dispersion. I think you could find a model; whether or not you would believe it is another matter.

SCHECHTER: You hang a lot on the fact that the wide binaries seem to

survive. There was a question the other day (and I wasn't quite satisfied with the answer) about whether the wide binaries could be stabilized by a third unseen companion.

HUT: It doesn't work. If the companion is far away from both visible stars, the binary is broken up anyway. If it is close to one of them, you will notice the very different radial velocities of the visible stars.

GUNN: There is a range of separations that would allow you to hide a lot of mass in a wide binary. If you were seeing two planets that orbit at  $\gtrsim 0.01$  pc around something dark that weighs  $100 M_{\odot}$ , they would be very hard to break up and you would be none the wiser from the dynamics.

J. BAHCALL: That's right, but the frequency of wide binaries is such that you can't afford to have a very massive black hole for every wide binary.

LARSON: Several posters address the question of whether the ratio of dark to luminous matter is the same in all galaxies. The situation is not simple or clear. On the one hand there are at least two posters, by Carignan and by Casertano and Bahcall, which show that when you fit rotation curves with models, the similarity in the rotation curves demands a ratio of disk mass to halo mass which is remarkably close to one for all cases. This suggests that the disk and halo matter are closely related, perhaps even made of the same kind of material (as Casertano and Bahcall suggest). On the other hand, a poster by Kent finds more variability in results of this kind.

Several posters address the question of how  $M/L$  ratios correlate with galaxy properties. One by Vader studies the dependence of  $M/L$  on color for both visual and IR colors. She confirms a conclusion obtained by Tinsley several years ago that the variation of  $M/L$  with color disagrees with the predictions of "standard" models in the sense that the bluer galaxies have much more mass than those models would predict. This suggests that the luminous and dark matter don't vary together, at least as a function of color. Then there is a poster by Athanassoula, Bosma and Papaioannou, in which they determine  $M/L$  ratios by fitting multiple-component disk and halo models. They show a plot of  $M/L$  versus color which to my eye looks almost like a scatter diagram. There does appear to be a trend, and they plot the Larson-Tinsley models, but there is more than an order of magnitude of scatter. There is also a poster by Bosma, Athanassoula and van der Hulst about disk galaxies with very low surface brightnesses. Dynamically these are giant spirals, but in terms of light they are very dim. Again something funny is going on with the mass-to-light ratios.

This gives me a chance to make a comment on mass-to-light ratios and the Larson-Tinsley models. The predictions of models are critically dependent on the initial mass function. These and nearly all other models assume for the initial mass function a power law or something similar that resembles the original Salpeter function. I would contend that the Salpeter power law is remarkably poorly constrained by existing observations except at the high-mass end. The relationship between the high- and low-mass ends, which is crucial here, is hardly constrained at all. The conventional continuity constraint is nearly always assumed. If you drop it, all hell breaks loose.

You can make models with mass-to-light ratios ranging from a few tenths to infinity for a population of stars and their remnants. So looking at the light tells you almost nothing about the mass of the associated stellar population except that it is not zero. It could be essentially infinity for possible assumptions about the initial mass function and the time dependence of the star formation rate. Because of this, I would suggest that the concept of luminous mass is a myth; the term should either be dropped or defined very carefully.

BURSTEIN: I would like to emphasize a point that has been made before. I am impressed with the lack of constraint placed on dark matter in spirals by comparing luminosity profiles and rotation curves. For each galaxy observed, there is a wide range of ratios of luminous to dark matter that produces acceptable fits. The arbitrariness in the luminosity-dependent approach is consistent with what Vera Rubin and I find for the systematics of rotation curves: they *do not* reflect the Hubble type of the galaxy.

SANDERS: I want to make a similar point. Everyone who plays the game of using rotation curves to make models basically assumes that the mass-to-light ratio of every component in a galaxy is independent of radius. Can you think of any reason why that should be true?

LARSON: No.

BARDEEN: I want to raise some questions about the dynamics of the formation of halos and elliptical galaxies. There are quite a few poster papers on this subject. We have seen suggestions that luminous ellipticals form mainly from mergers while spirals and small ellipticals form from more isolated density perturbations. This raises the question: To what extent can we make more-or-less spherical accretion models *versus* something more lumpy and complicated? Also, some recent studies, e. g., by Primack, Blumenthal and Faber, of the violent relaxation of collapsing collisionless systems suggest that it is difficult to get an extended flat rotation curve. On the other hand, the simulations by Frenk suggest that an isolated lump which is not formed by merging gets a rotation curve that is flat out to quite a large distance. And the final question is: What is the time of galaxy formation? In the case of accretion, this is not a particularly well-defined concept; in the cold dark matter scenarios, galaxy formation is fairly recent, and in the numerical simulations by Frenk and collaborators, the galaxies are still forming now. To what extent might these predictions be in conflict with attempts to find young galaxies? These are the questions I'd like to open up for discussion.

PRIMACK: You suggest that the simulations by Blumenthal *et al.* of halo formation are in conflict with those of Frenk. But actually, we are only trying to simulate the inner parts; our outer boundary conditions are not realistic. And Frenk *et al.* are simulating the outer parts; their resolution is not adequate to study the inner regions. My impression is that the models fit together rather nicely. The characteristic features of rotation curves are that they rise rapidly and within a couple of exponential scale

lengths are essentially at their asymptotic value. This is very hard to understand without having the baryons modify the dark matter distribution. This happens automatically, it is a dynamical fact of life. It is shown by several simulations, e. g. by Barnes and by Ryden and Gunn. And it is rather nice that we automatically get the kinds of spectra that are predicted by the cold dark matter model. So we get rotation curves which are fairly flat even beyond where you can measure them in real galaxies.

KAISER: Isn't it true, though, that if we could measure rotation curves for rich clusters, we would find that they also are flat? But the slope of the spectrum on cluster scales is rather different from the slope on galaxy scales. So it is not clear that flat rotation curves are the result of a "proper" choice of spectrum.

BINNEY: A few years ago a paper by Simon White demonstrated that merger remnants always have  $r^{-3}$  density profiles, in agreement with the Hubble-Reynolds formula. At this meeting, on the other hand, White told us that in simulations of the clustering of cold dark matter, galaxies predominantly form through mergers and have flat rotation curves. What has changed?

DEKEL: That's a very important question. Those simulations showed that, with almost any initial condition, a finite system which collapses and violently relaxes ends up with a de Vaucouleurs-law profile. Now the question is: What happens when you allow secondary infall after the center has relaxed? The results turn out to be very similar to the predictions of the self-similarity solutions of Goldreich and Fillmore, of Bertschinger, and of Gunn and Ryden. If the fluctuation spectrum is steeper than  $n = -1$ , you don't have enough secondary infall to change the original de Vaucouleurs profile. But if you have a lot of power on small scales, e. g., if on galactic scales the cold dark matter spectrum has  $n = -2$ , then you have enough secondary infall to produce the  $r^{-2}$  profile. So the theoretical solution says that if  $n \leq -1$  you end up with an  $r^{-2}$  density profile.

WHITE: Let me make a comment in response to James Binney. People who read other people's papers should be careful. Like salesmen of any kind, the people who do N-body work always present their results in the way that best makes the point. If you want to show that your model has an  $r^{-3}$  luminosity profile like in an elliptical galaxy, you plot the logarithm of density versus the logarithm of radius and find a slope of -3. If you want to demonstrate that you have a flat rotation curve, you take the mass within a radius, divide it by  $r$  and plot the result as a function of radius. Now, if the models are well defined over only a factor of four or five in radius, you can very well have something whose density looks like  $r^{-3}$  but whose rotation curve looks flat. (laughter, catcalls, uproar)

FRENK: There are two generic types of rotation curves that arise in our high-resolution N-body simulations of a universe dominated by cold dark matter. Those clumps which remain relatively isolated for long periods of time (and 8 of our 10 largest clumps do so) develop rotation curves which are essentially flat from at least 10 kpc outward. In contrast, clumps which form by merging of similar-size sub-clumps have rotation curves which

rise slowly out to  $\sim 50 - 80$  kpc and only then become flat. These are the objects that may be identified with the halos of ellipticals. It is not clear to what extent flat rotation curves are specific to the cold dark matter model. I suspect that the substantial large-scale power in this model is responsible and may explain the difference between our results and those obtained earlier from studies of the collapse of an isolated system or the merger of two isolated ones.

BARDEEN: I think it is fair to say that in these models you expect a rapid burst of star formation at some fairly early time, like at a redshift of five or six, and then a more gradual star formation after that. This might not violate the limits set by searches for primeval galaxies.

LYNDEN-BELL: None of us knows what dark matter is. It is now so fashionable to think that it is some exotic and unknown type of elementary particle that many people refer to the observed matter as the "baryonic density". However, there are perfectly good baryonic candidates for dark matter, such as giant planets.

We now have rather good evidence that around a number of giant elliptical galaxies baryonic matter is disappearing from hot, X-ray-emitting gas. The place where it disappears is right for the making of dark halos; the rate of its disappearance would build a halo in  $10^{10}$  years. If we want to believe the observations rather than our prejudices, we should take as our best bet that dark halos are baryonic and made from cooling flows. In a subject where observations are few, theorists have great freedom to build and are loath to abandon their castles in the sky. But if the disappearing hot gas does not make dark matter, how else can we get rid of it? When exotic neutral particles have been found in laboratories, I shall be happy to postulate them in the cosmos, but until then, let us use the observations and not the prejudices.

DRESSLER: The talks on cooling flows suggest a scenario for making both relatively high-mass ( $\sim 2 M_{\odot}$ ) stars in the centers of galaxies and very low-mass stars which cannot be seen forming in their halos. Has anyone thought about what might control the mass function in these two regimes?

FABIAN: The initial mass function is probably pressure-dependent. If high pressures give low-mass stars, then most of the matter in cooling flows turns into such stars. But it is possible to produce transient, low-pressure regions from large blobs cooling in the flows, giving rise to  $H\alpha$  filaments. If there are stars forming in those regions, they will form at pressures similar to those in the disk of our own Galaxy, and so will have higher masses. But I agree entirely that we need more work on star formation.

J. JONES: You don't need to form as many Jupiters as you might believe. In the models of elliptical galaxy evolution I have been making, I find that, provided you are considering mostly low- to intermediate-mass stars with lifetimes of  $\sim 10^9$  to a few  $\times 10^{10}$  years, these stars eject enough gas when they die that, if it falls into the center, it will provide a means of

condensing the galaxy in a way that agrees with the observations. You don't actually need to put the gas into extremely low-mass stars, provided you are not making ultra high-mass stars, i. e., stars with masses  $> 10 M_{\odot}$ .

STEIGMAN: There is a worse problem than understanding star formation that makes only low-mass stars. You have to turn *all* of the gas into stars. This is not observed in the Galaxy: molecular clouds certainly don't turn completely into stars.

LYNDEN-BELL: We don't know that no gas is left over. We see gas in lots of elliptical galaxies, for example in the form of filaments.

SALPETER: A poster by Trinchieri and Fabbiano describes the extensive X-ray emission from elliptical galaxies which are not in cluster cores. I want to discuss a controversy raised by such data--cooling flows *versus* galactic winds -- and a "diplomatic compromise" -- galactic fountains. The data in the poster refer directly only to isolated galaxies, but they also have an indirect bearing on rampressure stripping for elliptical galaxies in a cluster environment.

(a) *X-Ray Halos and Galactic Fountains*: The X-ray emission from large, isolated elliptical galaxies has two properties which suggest thermal emission from hot gas that was ejected in star deaths and is now in a cooling flow: (i) The total X-ray luminosity  $L_x$  of a galaxy with optical luminosity  $L_{opt}$  scales approximately as  $L_x \propto L_{opt}^{1.6}$ . The rate of gas mass loss by stars scales as  $L_{opt}$  and the kinetic energy per particle scales as the square of the velocity dispersion,  $\sigma^2 \propto L_{opt}^{2/3}$ . The observed  $L_x$  thus scales correctly as the rate of (thermal plus gravitational) energy release. (ii) For a given galaxy, the observed X-ray emissivity  $f_x(r)$ , as a function of distance  $r$  from the galaxy center, is approximately proportional to the star density  $\rho_s(r)$ . The emissivity, proportional to the square of gas density  $\rho_g(r)$ , scales correctly with gas release rate but the density ratio  $\rho_g(r)/\rho_s(r)$  increases with increasing  $r$ , roughly as  $\rho_s^{-1/2}$ . Thus, most of the total gas mass resides in the outer parts of the halo where the X-ray data are poorest. Furthermore, the empirical relation (ii) breaks down in the outer parts of the better-studied elliptical galaxies: (iii) The average  $\rho_g(r)$  decreases even more slowly with increasing  $r$  at large  $r$ , giving the appearance of a "broken ring" or an outer halo of gas.

The "orthodox" theoretical view (e. g., Forman, Jones and Tucker 1985, *Ap. J.*, 293, 102) invokes supernovae only for mild stirring of the gas; the X-ray emission leads to inward cooling flows and is powered by the gravitational energy released in the flow. According to the orthodox view, a complete galactic wind, i. e., immediate ejection of gas from the galaxy by supernova explosion, would give X-ray emission many orders of magnitude less than the slow cooling flows because of the very disparate flow times. Ostriker, on the other hand, was arguing that galactic winds are necessary to inject the heavy elements into the intergalactic gas. This sounds like a controversy. But I want to argue that both sides are right, as follows.

The radiative cooling that feeds the inward flow is not steady. Small density fluctuations are magnified into strong cooling instabilities. This must lead to an irregularly shaped gas surface even in the absence of

supernovae. Sometimes supernovae will erupt close to a "dipping part" of the gas surface; with little overburden of gas, these supernova will give a *temporary* and *local* galactic wind carrying heavy elements outward. While this local wind produces negligible X-ray emission itself, it does *not* decrease the emission from the other inward-flowing regions appreciably: the inward mass flow rate is decreased only slightly, and even this is offset partly by compression and heating from inward-directed parts of supernova explosions. Furthermore, in this more complex picture (coupled with an assumed dark halo which increases the escape velocity  $V_{esc}$  slightly) one should also get a third phenomenon, namely galactic fountains: The cooling instability and the increased  $V_{esc}$  lead to blobs cooling and condensing far out, falling inward, clashing with upwelling gas, etc. This process will puff up the gas surface on the average, but will be highly irregular since it is highly dynamic. Although these fountains have been invoked mainly for spiral galaxies (e. g., Bregman 1980, *Ap. J.*, 236, 577), I consider the broken rings of gas surrounding ellipticals (point iii, above) as the most direct visualization of the fountain predictions.

(b) *Rampressure Stripping of a Puffed Up Galaxy*: The above internal effects for an isolated elliptical galaxy must also interact with rampressure effects in a cluster environment. There are two kinds of interplay. The simpler of the two has just been calculated in a Cornell Ph. D. thesis by Terry Gaetz. He calculates rampressure heating and stripping by external gas flowing with velocity  $V_f$  relative to a spherical elliptical galaxy, with inward radiative cooling flows and central star formation included. His results differ from previous, simpler calculations in several ways. The increased cross section of the "puffed up" galactic gas increases the efficiency of rampressure stripping. Also, the stripping efficiency depends on a larger power of the ratio  $(V_f/V_{esc})$  than the previously assumed  $(V_f/V_{esc})^2$ , because heating effects are as important as momentum effects.

A second kind of interplay has not been calculated yet, but the external flow must also affect the internal gas dynamics. Consider a very massive elliptical galaxy which, when isolated, has galactic fountains but very little galactic wind. The same galaxy in a dense cluster has its outermost internal gas stripped by the external rampressure. The decreased density far out could then convert a galactic fountain into a partial galactic wind, resulting in an even larger mass loss. I therefore conjecture that a galaxy's environment can have a strong influence on the mass inflow rate in a cooling flow and on the resulting central star formation rate. However, the effect on the X-ray luminosity is weaker and more complicated, because the decreased mass flow is partially compensated by the increased heating from the external flow.

TUCKER: Do you predict that X-ray halos should become dimmer as you move closer to the core of a cluster?

SALPETER: For a galaxy with low mass and large velocity (large  $V_f/V_{esc}$ ), the X-ray halo should shrink in size with little change in central surface brightness. However, for massive galaxies moving slowly in the cluster core ( $V_f/V_{esc} \lesssim 1$ ), the X-ray halos should brighten instead of dimming (accretion heating instead of stripping).



FABIAN: We find that, in order to fit the data for (say) NGC 4472, the gas must cool out of the flow as fast as it appears. If you strip the outer layers of gas, you stop star formation in the outskirts of the galaxy. But you don't strip the center, so you don't stop star formation there. The central star formation shouldn't depend very much on stripping.

SALPETER: My opinions are half way between yours and Jerry Ostriker's. I think the supernova rate is always enough to lift the gas produced by planetary nebulae to about twice its initial radius, while Jerry says the factor is infinity.

SHAPIRO: The success of a galactic fountain usually depends on the dimensionless ratio of the cooling time for the hot gas to the dynamical flow time, and is thus a reasonably sensitive function of metallicity. I therefore suspect that there is an additional correlation between the star formation rate, which affects the metallicity, and the ability to strip the galaxy *versus* the production of a galactic fountain.

EKERS: I'd like to show a slide and describe an observation of a galaxy we've heard quite a lot about, NGC 4472. The slide shows NGC 4472 and the X-ray contours. Also shown are HI contours from observations by Sancisi, Carignan and myself. There is a small HI cloud between NGC 4472 and its dwarf companion, containing about as much HI as you'd expect for a dwarf of that brightness. But the HI is no longer in the dwarf galaxy. So the suggestion is that we are actually seeing the ram-pressure stripping of HI from the dwarf by NGC 4472's X-ray halo. This observation is also of interest because we seem to have HI right inside a very hot corona, and we have to worry about how it survives.

PACZYNSKI: The beautiful maps of the six known gravitational lenses were shown by Ed Turner. These maps convinced me that in two cases (Huchra's lens and O957+561AB) there is an obvious galaxy that does most or all of the lensing. In the other four cases there is no galaxy which looks important. In fact, in some cases the faint candidates are not within, but outside of the group of observed images. The image splittings are too large for galaxies and too small for the cores of clusters of galaxies. I think we should very seriously contemplate the possibility that most of these lenses are due to unknown dark objects.

SCHECHTER: Can you put limits on the surface density of the lensing object?

PACZYNSKI: It must be higher than some critical value which depends on the distance. In typical cosmological models, the central density of a rich cluster of galaxies is not quite sufficient, although not by a large factor. At the optimum distance, the center of a large Abell cluster has  $\sim 60 - 70\%$  of the critical density. So some clusters might be able to do the overfocussing. In such cases you would expect to see an additional image at some large distance of the order of a few arcminutes or greater.

EKERS (to Ed Turner): What is the evidence that the galaxies found in the vicinity of gravitational lenses are anything but chance coincidences?

E. TURNER: The sizes of the lens systems are typically only a few arcsec, around galaxies whose typical magnitudes are  $\gtrsim 20$ . The density of galaxies is not high enough for such a coincidence to happen easily. But that's not the way to look at it. The whole phenomenon is due to a chance coincidence. We see that the galaxy is there, it is at a smaller distance than the quasar, and it has a gravitational field. So it must be involved in the lensing.

PACZYNSKI: It might be involved, but it is not necessarily making the largest contribution.

E. TURNER: I agree.

TYSON: In cases of "missing" lens galaxies: As an alternative to over-focussing, the multiplicity observed (even instead of odd) would also be consistent with a dark, compact lensing object (an idea originally due to Press and Gunn).

PACZYNSKI: I would expect a galaxy to produce an even number of images, because nuclei are very compact. Whenever the central density in a lensing galaxy is very high, the image formed near the center is very faint. One can show that the intensity for a typical magnification goes like (surface density)<sup>-2</sup>. You would expect that the central image should be demagnified by a factor of 100 for a typical galaxy having a core radius of a few kpc.

BURKE: I think it is important to use all of the available information. Unfortunately, only 0957+561 has a relative wealth of information available. In this case, one should note that the VLBI jet shows the parity reversal expected for the pair of images. Any of these other suggestions should be able to demonstrate the proper parity pair.

REES: I would like to focus on a topic that has attracted surprisingly few poster papers, i. e., the implications of background radiation measurements and upper limits in various wavebands. Most of the baryons in the Universe could be uniformly spread in a diffuse intercluster gas. It is well known that such gas, if ionized, yields a UV and X-ray background. About ten years ago Field and Perrenod, Boldt and others considered whether the hard X-ray background could be due to such gas at temperatures  $\sim 40(1+z)$  keV. Recent work by Guilbert and Fabian renders this idea implausible on energetic and other grounds. Nevertheless, the constraints on gas at more modest temperatures ( $10^4 - 10^7$  K) aren't very strong. The background from such a gas with a density sufficient to contribute  $\Omega = 1$  would not be detectable. The best constraints on such gas come from considering the pressure confinement of the clouds that cause QSO absorption lines, but these are rather model-dependent. It may be possible to reconcile the data even with  $\Omega = 1$  with a carefully contrived thermal history and a low Hubble constant.

Turning now to longer wavelengths, the detection of distortions in the microwave background spectrum or in an infrared background would tell us about early galactic history and about the energy production associated with

Population III star formation. Maybe someone who has thought about this would care to comment?

CARR: I would like to comment about the importance of background light constraints. Many processes in the period after decoupling would be expected to produce radiation, including primeval galaxies, population III stars, pregalactic explosions and black hole accretion. Therefore, observational upper limits on the background radiation density place interesting constraints on these processes, especially if the radiation presently resides in the optical and UV, where the observational limits are strong. Probably, however, the radiation will have been reprocessed by dust. Dust absorption may occur near the sources or in the background Universe; dusty galaxies could provide the background absorption if they cover the sky. In these situations one would expect to see a far-infrared background peaking at 200 - 500  $\mu$ , a wavelength which depends only weakly on the grain characteristics and cosmological parameters (Bond, Carr and Hogan, preprint). Such a background and its anisotropies could be detectable with future space experiments and may already have been seen at 100  $\mu$  by IRAS (Rowan-Robinson, preprint). Jonathan McDowell has calculated the constraints which background light limits already impose on astrophysical processes in the early universe.

MCDOWELL: I think the interesting data have not yet arrived. For a significant contribution to  $\Omega$  from pregalactic very massive objects, the models I have made generally predict backgrounds that are close to current observational limits. We should have interesting constraints in both the near and far infrared in the coming decade.

DAVIS: I am surprised that Joe Silk hasn't mentioned the particle background. If they formed the halo of the Galaxy, some candidates for cold dark matter would annihilate there. In a paper with Mark Srednicki, Silk finds that such annihilation can provide a natural explanation for the local low-energy antiproton flux, which is otherwise very difficult to understand. Perhaps Silk would like to talk about this?

SILK: Suppose the dark halo consists of any Majorana-type massive fermions. A particularly attractive candidate is the supersymmetric partner to the photon, the photino. Srednicki and I have shown that annihilations of these particles in the halo produce an observable flux of low-energy cosmic-ray antiprotons. Once the photino mass is specified, the prediction is quite specific, and the photino mass is restricted to a narrow range by the value of  $\Omega$ . The annihilation products include  $p\bar{p}$  pairs of energy several hundred MeV, which accumulate in the halo over a typical leakage time of  $\sim 10^8$  yr. There are essentially no secondary cosmic-ray antiprotons produced in this energy range by the interactions of high-energy primary cosmic rays with the standard grammage of interstellar matter assumed in cosmic-ray confinement models. We concluded that this could provide a unique signature of a not-improbable form of halo dark matter. One experiment has reported detection of low-energy cosmic-ray antiprotons, but this result has to be confirmed before any conclusion can be reached about the antiproton source.

STEIGMAN: If the density in ordinary baryons is, say, 10 % of the critical density, and most of it doesn't turn into galaxies, can you hide it?

REES: Gas can be collisionally ionized if it is hotter than  $10^6$  K and it can be photoionized by an ultraviolet background if it is at  $\sim 10^4$  K. I don't think there is any objection, if  $H_0 = 50 \text{ km s}^{-1} \text{ Mpc}^{-1}$ , to having an  $\Omega$  of at least 0.1 or 0.2 in uniformly distributed baryons in that temperature range.

SHAPIRO: I think you can probably have a hotter gas at a higher density and still hide it, with the possible exception of the constraint imposed by the pressure inferred for the metal-free Ly $\alpha$  clouds. If the gas were between  $5 \times 10^6$  and  $10^7$  K with an  $\Omega$  of 0.3, I doubt we could see it. It would have too low a temperature to be seen as the X-ray background and too low a density to compete with the local soft X-ray contribution of our Galaxy. But it would overconfine the Ly $\alpha$  clouds.

REES: We don't know the pressure of the Ly $\alpha$  clouds unless we think they are spherical and unless we also know the UV background that is ionizing them.

SHAPIRO: Then it is even easier to hide this gas.