GENERAL DISCUSSION (CHAIRMEN J. PEEBLES AND S. TREMAINE)

PEEBLES: The first part of the general discussion focusses on four
particularly active and important topics that can be associated with four
of the speakers at this conference. To provoke discussion, I will ask:

(1) *Local K_z determination:* Does anyone not believe John Bahcall?

    Dark matter in the disk of our Galaxy is particularly conveniently
placed for the study of the nature and distribution of dark matter. It is
difficult, although not impossible, to see how weakly interacting particles
like axions or neutrinos could have become concentrated in a disk. If we
could convince ourselves that the local dark matter is baryonic -- brown
dwarfs or stellar remnants -- it would encourage studies of the possibility
that dark matter found elsewhere is also baryonic. We would also have the
challenge of understanding how an appreciable fraction of the local baryons
were converted into a dark state after galaxies formed.

LYNDEN-BELL (to J. Bahcall): Do there exist suitable star-count data in the
south, and do they agree with those in the north?

J. BAHCALL: The only appropriate samples at this stage are in the northern
hemisphere. I think that it would be useful to hear about the new programs
being carried out in the southern hemisphere by Paul Schechter on the
K dwarfs and Ken Freeman on the K giants. They can't yet answer your
question, but they can tell you how their samples are designed to answer
all of the questions that have been raised.

FREEMAN: We are getting a complete sample of K giants near the south
galactic pole; they are all bright and within about 1 kpc of the Sun.
We are going to do DDO photometry on the whole lot, which will give us
metallicities and luminosities. Then we really will have a pure sample of
K giants. We will also get slit spectra for all the stars. I think the
obvious thing to do will be to use the relatively metal-rich stars, which
we can identify pretty reliably with the old thin disk.

PEEBLES: What is known about the proper motions of the M dwarfs? Can one
properly infer a mass per unit area from a mass per unit volume at the low-
mass end of the mass function?

J. BAHCALL: From what we know about dwarfs brighter than M_V = 16, I don't
think that there is any hope that the M dwarfs contribute significantly to
the density of observed matter. By any extrapolation, even a flat one, they
contribute < 0.01 $M_\odot$ pc^{-3}, and I think the observers here will call that
an overestimate.

PEEBLES: You are quoting a mass per unit volume. How well can you get a
mass per unit area?

J. BAHCALL: Even if you integrate over 1 kpc rather than 300 pc, you don't
get a useful contribution. But Larson's remark that the luminosity function

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may have two peaks could apply. He talks about a second peak in the white
dwarf region, but it could just as well be at 0.03 $M_\odot$.

FABER: In light of Jim Gunn's remark about the possible spread in the
absolute magnitudes of the F stars, what can you say to reassure us?

J. BAHCALL: I think he made a good point. All of the F dwarfs of Hill,
Hilditch and Barnes were observed in Strömgren four-color photometry; they
also had spectroscopy for a representative sample. I think that is the best
that can be done, and it is sufficient for the F dwarfs. There is a bigger
problem for the K giants. We have MK classifications only for the Upgren
sample, not for the Oort sample. But the Oort and Upgren samples turn out
to have the same densities, within the errors. I estimate that these errors
contribute $\leq$ 20 % to the error in the total amount of matter. The real
test of this work will come in 3 - 5 years, when we have new samples like
the ones by Freeman and Schechter.

FREEMAN: I just want to remind you of the work on face-on galaxies that I
and Piet van der Kruit discussed. This uses comparable dynamical techniques
and gives M/L ratios similar to those found in the K_z analysis.

(2) *Dwarf Galaxies:* Does anyone not believe Marc Aaronson?

PEEBLES: Studies of extremely low-luminosity galaxies may reveal distinctive
properties of dark matter. The Cowsik-McClelland-Tremaine-Gunn phase-space
argument tells us that neutrinos with masses of a few tens of eV would have
space distributions broader than the stellar distributions of some dwarfs.
If these galaxies were dark because they lost most of their baryons, then
they could be left with canonically deep potential wells of dark matter. On
the other hand, if dwarfs formed by dissipation out of debris from large
galaxies, they may have mass-to-light ratios characteristic of purely
stellar systems.

J. BAHCALL: Aaronson removed some stars because of evidence that they are
binaries. Suppose he observed a representative sample of stars. Are the
velocity differences measured for the binaries sufficient to account for the
entire observed velocity dispersions of the galaxies?

PEEBLES: Aaronson is no longer here.

MATHIEU: I have done Monte Carlo simulations to study this problem in open
clusters, and Marc and I are doing them for his dwarf spheroidals. My gut
feeling is that binaries will not account for observed dispersions as large
as 1 or 2 km s^{-1}. But a problem that Aaronson has not taken into account is
the possibility that the galaxies contain very massive objects that inflate
the observed central velocity dispersions.

FABER: If you think about what types of binaries you need to give a
dispersion of 9 km s^{-1} when the stars have masses of $\sim$ 1 $M_\odot$, you conclude
that the separations are $\sim$ 25 AU and the periods are like that of Saturn
around the Sun. So at the moment we might just be on the hairy edge of

being able to rule out binaries. I would feel a lot better if we had
followed these stars for ten years.

RICHSTONE: Two reasons to be skeptical: (1) The observed distribution of
velocities looks flat, not Gaussian (although there are few points). (2)
We know that giant ellipticals have anisotropic velocity dispersions and
that velocity anisotropy can affect M/L determinations even if the velocity
dispersion is known precisely.

OSTRIKER: Two comments on why the high values of M/L for dwarf spheroidals
may be right. (1) White and Davis have suggested from binary-galaxy
considerations that M/L varies as $L^{-1/4}$. Then it isn't surprising that
when L is very small, M/L is very large. (2) If we know the rotation
curve of the Galaxy at large radii, we can determine M/L for the dwarf
spheroidals on the assumption that they are tidally limited. Faber and Lin
have shown that this gives high values for M/L.

GUNN: There are several scenarios, some of them quite prosaic, in which one
would expect very high M/L ratios in dwarf systems. For example, one can
remove the metallicity, using the simple continuum model I talked about to
reduce the yield by pushing the mass function to very low values. This can
explain systems with a very low light density and high mass-to-light ratio.
However, it is worth remembering that the dwarf spheroidals that have been
measured show an enormous range in M/L, from values of about 10, perhaps
a little higher than one would like to believe on the basis of population,
to values like 100. Because of that it is difficult to believe that there
is one simple picture for their formation. I have thought for a long time
that the dwarf spheroidals are a key, a Rosetta stone, to galaxy formation.
I can't see how any of the suggestions for how they form would introduce
such an enormous dispersion in M/L. I'm a bit skeptical purely for this
reason. Also, the two cases in which M/L is particularly high are the most
difficult objects in the sample.

DEKEL: I believe that dwarf galaxies must have extended halos (but not
necessarily very high M/L within the visible region). Self-gravitating
gas-loss models fail to reproduce the observed relations between
luminosity, radius and metallicity. On the other hand, the simplest
model of substantial gas loss inside massive halos is very successful in
reproducing the observed relations (e. g., Dekel and Silk, this volume).
The observational constraints indicate further that the halos originate
from cold dark matter perturbations. Our model provides a simple physical
mechanism for biased formation of bright galaxies.

(3) *Primordial Nucleosynthesis:* Does anyone not believe Jean Audouze?

PEEBLES: Nucleosynthesis is advertised to provide a constraint on the
mean mass density in baryons. For "reasonable" values of the Hubble
constant, the density in baryons has to be less that $\sim$ 10 % of the critical
Einstein - de Sitter density predicted by inflation with negligible present
cosmological constant. As Audouze emphasizes, to justify this we need to

make a precise comparison of computed abundances with observed abundances extrapolated back to primeval. Also, we need to consider alternatives to the canonical paradigm, such as primeval non-linear isocurvature fluctuations.

STEIGMAN: The difference between the results of Audouze and those summarized by Boesgaard and Steigman (1985, *Ann. Rev. Astr. Ap.*, **23**, 319) is his use of the *estimated* primordial abundance of ^{4}He to determine the nucleon density. To use ^{4}He requires that the abundance be known to 3 decimal places. I don't think that is presently possible. Boesgaard and Steigman suggest that the primordial ^{4}He abundance was Y_p = 0.24 $\pm$ 0.02.

AUDOUZE: With this value of Y_p the conclusions of our work do not change! The reasons why I advocate that $\Omega_B \leq 0.06$ are the following. As I said yesterday, the error bars concerning all the four elements D, ^{3}He, ^{4}He and ^{7}Li are still extremely high. But at this point I do not agree with the statement of Gary Steigman that it is safer to start first to find a good agreement from D, ^{3}He and ^{7}Li and *then* to check ^{4}He. The reason is that ^{4}He is much less affected by chemical evolution than D and ^{3}He. I want to state again that in order to find an agreement between the predictions of the standard Big Bang and our present knowledge regarding these elements, one should invoke specific models of chemical evolution concerning D and ^{3}He. Moreover, we obtain a limit on Ω_B more stringent than the majority of the participants may think. But I still believe that progress will take place when better measurements of primordial ^{4}He are available.

PEEBLES: Jean, where do we go from here? What are the directions of research that will lead us toward better answers?

AUDOUZE: Let me cite Kunth and others who try to measure the primordial He abundance in blue compact galaxies and places where the metallicity is low. Vigroux *et al.* find an abundance of $\sim$ 20 %. Kunth and Sargent find a very large spread in Y, from 20 to 26 %, for just one value of [O]/[H]. My way of getting consistency in all these numbers is to have $\Omega < 0.06$. I'm sorry if that creates trouble.

PEEBLES: Any other forward-looking remarks?

PACZYNSKI: I would like to point out that there is a low-mass binary system, CM Draconis, that offers a possibility to determine helium content in the unevolved Population II stars. The binary is eclipsing, both masses, radii and luminosities are known, and preliminary analysis (Paczynski and Sienkiewicz 1984, *Ap. J.*, **286**, 332) implies Y = 0.3 $\pm$ 0.1. This estimate may be considerably improved in the near future.

OSTRIKER: One thing which would be very useful and which could be done moderately soon is the measurement of galactic gradients in the light elements. It would better tie down the galactic evolution model if we knew, for example, whether deuterium increases or decreases with increasing metallicity.

(4) *The Early Universe:* Does anyone not agree with Mike Turner?

PEEBLES: New ideas from particle physics have greatly stimulated some old
speculations in cosmology. Inflation provides a beautiful explanation for
the isotropy of the Universe and lends respectability to prejudices against
cosmological models with non-zero space curvature. Phase transitions may
produce computable density fluctuations that end up as black holes or
superclusters. And we are presented with a rich catalogue of forms of
matter, from gravitational waves to cosmic strings, whose abundances may
be computable and whose physics may help to account for the properties of
galaxies and superclusters. But apart from the general enthusiasm, are
astronomers justified in accepting any of this as received knowledge?

J. BAHCALL: I have a question for Mike Turner about the shadow Universe. It
was not clear to me in reading the particle physics scenarios that we get
what we need from the shadow Universe, namely that the shadow matter is in
the same place as the matter that we see. Could it be that there are shadow
galaxies out there, but that they don't coincide with the ordinary matter?

M. TURNER: Well, if you are asking about primordial adiabatic perturbations,
then the shadow and ordinary matter will both participate in gravity, and
you can't separate them (assuming they were well mixed in the first place)
until non-gravitational forces become important.

J. BAHCALL: Why would they be well mixed in the first place? Why do they
have to be coincident even on a scale of 10 kpc? Why isn't it just as
likely that shadow galaxies are out there where there is practically no
normal material, while our Galaxy is made mostly out of non-shadow material?

M. TURNER: OK, it is possible that at the start they were not well mixed.

OSTRIKER (*sotto voce*): Are there statistical requirements necessitating a
shadow - John Bahcall asking the same question at the same time? How close
together do they have to be? (outbreak of mirth in Ostriker's vicinity)

GUNN: I think, John, that all you need is that both Universes were
relatively homogeneous early on. Even the development of the perturbations
doesn't require that they be adiabatic, as long as the shadow Universe had
a decoupling phase. The perturbations are linked; you can show very easily
that only the potential matters; this is described by them both; the matter
follows the potential, and so the shadow matter will follow the ordinary
matter. So one would expect a shadow galaxy here with more or less the same
properties as ours, at least in the halo.

SPERGEL: If inflation is important and occurs after $E8 \times E8'$ breaks, the
relative number density of $E8'$ matter would be much lower. If $E8'$ matter is
the dark matter, the physics in its sector would have to be different from
the physics in the $E8$ sector.

FELTEN: If "believing Mike Turner" includes believing that $\Omega = 1$, it would
be wise to keep perspective by noting that there is as yet to my knowledge
no confirmed prediction of any observable from the inflationary theory.

There are explanations for a few problems which had been noticed previously, such as the horizon problem, but these are explanations after the fact, not predictions. If I am wrong, maybe Mike, or Gary Steigman, would comment. The observational evidence suggests, if anything, that $\Omega < 1$. The evidence for the theory is not compelling.

M. TURNER: I think it is hard not to believe me, because I'm an honest guy and I always tell the truth (laughter). But mainly, the message of my talk was that the early Universe is just starting to come into focus. Most of us believe in primordial nucleosynthesis. But at earlier times we are getting only hints. Inflation is extraordinarily attractive: for the first time there is a possible explanation of the origin of density inhomogeneities. The idea of relics is very attractive. But there is nothing conclusive yet. There are hints that might focus the effort in understanding how structure and dark matter formed.

TREMAINE: In fairness, it may be too early to demand a confirmed prediction from the inflationary model. The average rate of discoveries in cosmology is about one per twenty years, and inflation has been around for only four or five.

TREMAINE: OK, let's move on from character assassination. I want to ask a number of questions. First, *what* is the dark matter? My understanding of the consensus is that we certainly need some baryonic dark matter, given that there is a problem of missing mass in the galactic disk. We don't *need* non-baryonic dark matter, although it is very attractive. At the moment we seem to need up to four kinds of dark matter, one for the disk, one for the halo, one for rich clusters and one for Alan Guth. Does anyone believe that the initial mass function of the stellar population is the same as a function of position and time? Almost certainly not, but everybody assumes it anyway. Does anyone believe estimates of the local initial mass function for $M < 0.2\ M_\odot$? I have a question about that: How well is the main-sequence mass-luminosity relation determined at very low masses, and how much could it affect our understanding of the initial mass function? Does anyone have any comments on that question, or on any of these others?

SILK: At the moment, we have at least two, if not three, dark matter problems. I would like to propose a means of reducing our difficulties to only one dark matter problem. The argument is as follows. The evidence for more-or-less spherical halos is highly biased. It consists of polar ring galaxies and of X-ray-emitting ellipticals. However, both are likely to be ongoing mergers or merger products, and we would expect a merged halo to be fairly round. However, isolated spirals may have very flattened halos. In fact, in at least one scenario, this is highly probable. I suspect that what I have to say would apply to any generic pancake scenario for galaxy formation, but let me consider the particular example of warm dark matter. All small-scale structure in the primordial fluctuation spectrum is suppressed by free streaming, and the collapse on massive halo scales happens very asymmetrically. The dark matter is most likely to form a sandwich containing a layer of denser, dissipating baryons. No doubt the

dissipation will further help to drag in the dark matter. Direct formation
of a filament is less probable, but the sheet should be unstable and should
turn into a highly flattened triaxial halo. Within this, the galaxy forms
in the usual way. I suspect that such a halo could simultaneously explain
the disk dark matter and flat rotation curves. On larger scales, warm dark
matter is indistinguishable from cold dark matter. One other implication is
worth mentioning: A very prolate halo would have interesting consequences
for the velocity ellipsoid of old stars, galaxy rotation curves and warps;
it might even be desirable.

TREMAINE: There is yet another possible way to build a disk, which I think
I first heard from Jim Peebles. In some scenarios you might form small
clusters of cold dark matter. If they had some initial angular momentum,
dynamical friction might drag some of them down into the disk, at which
point they would be tidally shredded and would form a thin disk of non-
baryonic matter. It may be worth investigating possibilities like this.

GUNN: There is another strong constraint on the shape of halos that hasn't
been discussed. In the Galaxy at 3 or 4 kpc beyond the solar radius, the
HI disk flares very strongly. It flares in precisely the way you would
expect for a disk of velocity dispersion 7 - 10 km s^{-1} (as observed for
the vertical velocity dispersion in other galaxies) if the disk became
non-self-gravitating at that radius. If you assume that the disk falls
off exponentially with radius, you can show that it *should* go non-self-
gravitating at that radius. And from the behavior of the scale height in
the gas disk *versus* radius you can put constraints on the ellipticity of the
halo. It must have a flattening of < 2:1. Now at the time I made these
suggestions, I don't think that everyone believed that the rotation curve
of the Galaxy was flat. And if there is no halo, the disk still becomes non-
self-gravitating and it still flares. But observations of other galaxies
now suggest that our own would be very strange if it didn't have a flat
rotation curve and therefore a halo. So now this argument based on the
flaring of the disk can be taken seriously.

TREMAINE: But it doesn't rule out the possibility that the dark matter in
the disk is non-baryonic.

GUNN: True. But it implies that most of the matter that supports the
rotation curve cannot be in a flattened system.

SANCISI: It is true that the HI disk seems to flare in our Galaxy and in
several others (e. g., NGC 891). But can you explain why, at a radius of
three times the optical radius, 50 kpc or so, the gas is still close to the
disk plane in NGC 891 and in NGC 5907? If disk formation is recent, how
does the gas find the plane so quickly when there is no matter there?

GUNN: I have no answer. That is a very difficult problem.

SILK: It is an argument for dark matter in the plane of the galaxy.

OSTRIKER: A comment on Joe Silk's intriguing suggestion. A hot disk would
stabilize things as well as a halo. And one would indeed expect it to be

triaxial. But then I don't see why rotation curves are so symmetrical.
There are a couple of cases of galaxies with unsymmetrical rotation curves
at large radii, but if Silk's suggestion is right, they should be the rule
rather than the exception.

SANCISI: I think that asymmetries *are* very likely the rule. When you go
very far out in radius, at some point you don't believe most rotation curves
any more because they are not symmetric. In some cases the rotation curve
even turns down, so it becomes doubtful whether you are seeing circular
motion.

SILK: Let it be said that the possibilities are rather broad. There is a
certain probability of collapsing to a very thin sheet, but collapse could
equally well be to other configurations. I can imagine a wide variety of
complicated triaxial shapes.

VAN DER KRUIT: The kind of arguments just given by Gunn and Sancisi
concerning flaring of HI layers, complemented by similar work on stellar
populations, gives information on at least the positional dependence of
the IMF. These data imply that M/L and therefore the general form of the
IMF are not varying in disks. Also, I believe that these estimates of M/L
(including Bahcall's K_z analysis) are the only ones that can tell us what a
"reasonable" M/L is.

LAKE: Are halos spherical? This is an important question, but the work on
polar rings has not answered it. In order to stabilize both rings, we must
be looking down the intermediate axis of a triaxial mass distribution. The
counteraligning of closed orbits relative to the potential will make the
observed velocities equal in these flattened potentials.
    One thing that has disappeared from the oral saga is that we know the
core radii and asymptotic velocities of halos. Deconvolutions of rotation
curves now yield a halo contribution to V that is linear in r. We don't
know any velocity scales of halos from rotation curves. What information
can we compare to binary studies?

PACZYNSKI: Next come the questions dealing with low-mass stars. There are
lots of rumors that the velocity dispersion for low-mass, late-M dwarfs is
smaller than that of the brighter stars. Where do we stand on that?

GILMORE: The status of current observational evidence that very low-mass
luminous stars might provide a significant contribution to the total density
of matter in the solar neighborhood is as follows. Recent automated red-
sensitive photometric surveys over large areas to intermediate depth
(Gilmore *et al.* 1985, *M. N. R. A. S.*, **213**, 257) and small areas to greater
depth (Gilmore and Hewett 1983, *Nature*, **306**, 669; Boeshaar and Tyson 1985,
A. J., **90**, 817) have provided the first volume-limited samples of stars
which are complete to the absolute magnitude at the theoretical minimum
mass for hydrogen burning (0.085 $M_\odot$). These surveys are in excellent
agreement, and show that the stellar luminosity function has a broad maximum
near M_V = +12, and then a slow decline to M_V = +19. Conversion of this
function to a mass function is hampered by the very small number of data

points available to calibrate the mass-luminosity relation below $\sim 0.25\ M_\odot$ (Fig. 3 of Gilmore and Reid 1983, *M. N. R. A. S*, **202**, 1025). The available data show that the stellar mass function has a maximum near 0.25 $M_\odot$ and then a decline at lower masses. The existence of a maximum ensures that the integral of the mass function converges before the minimum mass for hydrogen burning is reached. Stars with masses below 0.20 $M_\odot$ therefore do not contribute more than about 0.005 $M_\odot$ pc^{-3} to the total mass density near the Sun. There are two major caveats to this conclusion, each relating to the possibility that the lowest-luminosity stars have short luminous lifetimes. First, several stars are now known with reliable (trigonometric-parallax) absolute magnitudes fainter than M_V = +16, which corresponds to the theoretical minimum mass for hydrogen burning. If these stars really do have masses below the theoretical limit, they will only briefly be luminous, and their derived space density must be increased by the ratio of their luminous lifetime due to the release of gravitational energy to the age of the galactic disk. This could be a large factor, implying a very large total mass in such "stars" and their remnants. Reid and Gilmore (1984, *M. N. R. A. S.*, **206**, 19) show that a large correction factor is unlikely but not totally excluded. They show further that the observed faint stars lie at or just below the hydrogen-burning main sequence in an $M_{bol} - \log T_e$ HR diagram, while none lies near a plausible gravitational cooling track. The dispersion below the nominal minimum absolute magnitude is therefore probably due to a combination of cosmic dispersion and observational and theoretical uncertainties. The exception to this is the companion to VB8, which is certainly well below the minimum mass for hydrogen burning (McCarthy *et al.* 1985, *Ap. J. (Lett.)*, **290**, L9). However, it is not an isolated star.

The second possible problem relates to the discovery by Poveda and Allen (1985, *Ap. J.*, in press) that stars in the immediate solar neighborhood with masses below $\sim 0.2\ M_\odot$ have a variety of properties consistent with young age. The most compelling of these is their apparently small velocity dispersion. If this result is valid for a larger sample, then the usual correlation of velocity dispersion with age suggests that these stars are young. The only sign that this result may not generally be valid comes from the deep photometric surveys of Gilmore and Hewett (1983, *Nature*, **306**, 669) and Boeshaar and Tyson (1985, *A. J.*, **90**, 817). These show that the space density of very low-mass stars found at substantial distances from the galactic plane is consistent with that found in the solar neighborhood convolved with an exponential decrease in density with a scale height of a few hundred parsecs. This scale height implies a much larger velocity dispersion for these stars than that found by Poveda and Allen. The resolution of this paradox is not known.

While the data are not yet conclusive, it remains true that there is no strong evidence that low-mass stars ($M < 0.2\ M_\odot$) provide more than about 0.005 $M_\odot$ pc^{-3} to the total mass density near the Sun.

LARSON: I agree that the biggest worry for the determination of the mass function at the low end is the mass-luminosity relationship for very faint stars. I'm fairly persuaded that the luminosity function drops off. Some of the most impressive evidence is due to Frank Low, who showed that the main sequence, while remaining well-defined, thins out very remarkably

toward the bottom end. Anything like a conventional mass-to-luminosity relationship translates this into a steep drop in the mass function. I have looked at the data available for very faint binaries, and so have Scalo and Gilmore. For what it's worth, these stars define a very nice linear relationship between absolute visual magnitude and the logarithm of the mass right down to $M < 0.1\ M_\odot$. So if you want to convert the falling luminosity function into a rising mass function, the mass-luminosity relationship has to do something strange, which is not suggested by the data.

One comment on the question of very young stars: I, also, am fairly persuaded by the evidence of Poveda and Allen that these very low-mass stars are young. There is not only the kinematic evidence, but also some spectroscopic evidence. If I recall correctly, there is a high abundance of flare stars among these very young stars. Their interpretation is that the hydrogen-burning main sequence ends at $0.2\ M_\odot$ and that these objects are on their way down to invisibility. That interpretation is ruled out immediately by the fact that the main sequence in the HR diagram remains well defined to much below $0.2\ M_\odot$. Anyway, I haven't heard any expert in stellar interiors suggest that the end of the hydrogen-burning main sequence occurs at $0.2\ M_\odot$. I would suggest an alternative interpretation, which is that the IMF and the characteristic mass have changed with time, and that you are indeed looking at recently formed objects.

TREMAINE: The next question is: *Where* is the dark matter? What is in the voids and what is not in the voids? What do we learn from gravitational lenses? And a point that is designed to be provocative: We have now observed a lot of rotation curves of galaxies, but have had little success in explaining them theoretically. So why should we observe any more rotation curves? Also, why are rotation curves so flat and so similar? Jim Gunn addressed this question, but it would be interesting to know if anyone else has any ideas. Are there any mass determinations that we should abandon? I was struck by the lack of argument about mass determinations based on tidal radii and the almost complete lack of discussion of binary galaxies. This suggests that people have given up on them. And: since gravitational lenses give you some evidence that you could have dark things that don't contain galaxies, by the same token, could you have a galaxy without a dark halo? If so, what would it look like? Are there any candidates? Any comments?

RUBIN: There are two situations in which rotation curves should be valuable: (1) Galaxies in binary samples: Linda Stryker and Kirk Borne have rotation curves for the binary galaxies studied by Linda Schweizer. These may help us to interpret the results on binary galaxies. (2) Galaxies in very dense environments: I hope that spectra of galaxies in the compact Hickson groups will teach us something about how halos are altered, or perhaps fail to form, in dense environments.

TULLY: I think it is worth while to continue to measure rotation curves. One very interesting point which is still unresolved is the presence of the dip that occurs in the inner parts of the rotation curves of some bulge-dominated spirals.

MILGROM (to Gunn): You gave an argument to explain why the contributions
of the disk and halo to the rotation curve would be similar at the optical
radius. The argument was based on the value of an angular momentum
parameter. Can it also be applied to ellipticals? (We have some evidence
that the two contributions are similar in ellipticals, too.)

GUNN: One can wave one's hands about elliptical galaxies, but much less
convincingly than for spirals. We know almost nothing about the relative
contributions of baryonic and halo matter in ellipticals, but such evidence
as there is suggests that there is continuity with spirals. And there are
theoretical arguments about the amount of halo matter, although they are
qualitative. One such argument that I find fairly persuasive says that
as long as the local density is strongly dominated by the non-dissipative
halo, you can't form clumps in the baryons. So you can't begin to form
the stars of which ellipticals are made until baryons begin to dominate.
Qualitatively, this gives you the same kind of picture as for spirals. How
strongly we should believe these arguments, I don't know.

GERHARD: A comment on the radius at which the dark matter begins to
dominate. This is inferred from fitting maximum-disk rotation curves, and
so depends heavily on a small inner part of the measured curve. I wonder
if significant velocity dispersions and/or non-axisymmetries in the inner
disk could result in rotation velocity measurements smaller than the true
circular velocity. We may then underestimate the disk M/L and the radius
at which the halo takes over.

OSTRIKER: Tremaine asked about the utility of binary-galaxy mass estimates.
I find that, when scaled appropriately for starting assumptions, the studies
agree well, and agree with extrapolated rotation curves. New larger samples
could better determine such unknowns as orbital eccentricity distributions
and whether or not mass and luminosity are correlated.

WHITE: A comment about Jim Gunn's Big Black Lumps. Although it is
relatively easy to think of a way in which one of these might form with no
associated galaxy, it is hard to see how it could manage to form with no
ordinary matter at all. As Doroshkevich pointed out a couple of years ago,
one might therefore hope to see such objects as weak, extended X-ray sources
with no associated visible objects. Limits on such objects from Einstein
could put useful constraints on the abundance of Big Black Lumps.

SCHECHTER: On the same point: We see a good number of galaxies which
are interacting with other galaxies, and might expect to see examples of
galaxies with tidal streams which are interacting with nothing or with
something that doesn't look big enough to produce a significant effect.
Maybe this is a case of suppressing something we don't like: we only pay
attention to such galaxies when we see the companions.

WHITMORE: In gravitational lenses I don't see why we should expect the
center of mass to be very near the center of light. For example, if the
halo extends 120 kpc on one side, but only 80 kpc on the other, the center
of mass could be displaced by about 20 kpc.

BURKE: A displacement of only 20 kpc won't work in 0957; the displacement of the unseen matter has to be greater than that to explain the data.

WHITMORE: Can you give us numbers? What sort of displacements are needed?

BURKE: The separation has to be $\geq$ 50 kpc. That's not an unique determination, of course. A recent paper by Gorenstein, Falco and Shapiro indicates the range of solutions that are acceptable.

TREMAINE: The question on gravitational lenses was partly designed to demonstrate that what we learn from them seems very unfocussed (laughter).

DEKEL: Some biased galaxy formation scenarios, (e. g., Faber, this volume; Dekel and Silk, this volume) predict that dwarf galaxies would not avoid the voids, but rather trace the mass. This is testable. There are already indications that dwarfs are clustered more weakly than bright galaxies (in the UGC, and in Perseus-Pisces). To make a more direct test, one should search for dwarfs in regions which are known to be void of bright galaxies. Oemler and I are currently making such a search.

REES: A comment on neutrinos and antibiasing. As I understand it, the neutrino model is consistent with observations if you can prevent galaxies from forming in regions where matter eventually accumulates. Can one rule out the possibility that there are, in those regions, the neutrinos and a lot of very hot gas that has not had time to cool down? The gas may not even have a tremendous overdensity because it can't cool down, and therefore it won't be too conspicuous in X-rays. This seems to be one way to salvage the neutrino model and also to get large dark objects for lenses.

FELTEN (to J. Gunn): I'd like to question your value of Ω = 0.2, for the following reason. It seems to me that within the context of your review, if you deny the scale dependence of M/L, the tendency is to push Ω down, maybe to 0.1 $\pm$ 0.05. To defend your Ω of 0.2, I wonder if you could quote for us two numbers. Pick your magnitude system and your Hubble constant, and then tell us the M/L required to close the Universe and the mean M/L for galaxies, weighted by luminosity. It seems to me that if Ω = 0.2, these two numbers should differ by a factor of 5. I would like to see what pair of numbers you assume, and whether that pair of numbers meets general assent.

GUNN: The value of Ω = 0.2 is a consensus arrived at by several people. I think that the scale dependence of M/L has disappeared because of the growing realization of several things which at this point can't entirely be quantified. There is the question of accounting for the gas in clusters, which is a large fraction of the total baryonic content. Also -- and because of uncertainties about the initial mass function, one should take this with a grain of salt -- we don't really know that the natural value of M/L for an elliptical is different from that for the disk of a spiral. Our beliefs involve an assumption about the location of most of the mass that we don't see. If we assume similar initial mass functions, then it is natural to assume that the value of M/L in an elliptical is three times the value

in a spiral, because the blue stars which are contributing most of the light in spirals are absent in ellipticals. Thus the progression of M/L from 75 for groups to 200 - 300 for clusters can be explained by stellar population differences and by the hot gas content of clusters. Now I'm not sure that that is a quantitative justification for saying that the ratio of invisible to visible matter is constant as a function of scale. I'm simply saying, and I think Marc Davis would agree with me, that these points remove the evidence for variation.

FELTEN: The point I'm trying to make is that galaxies in rich clusters are not typical of galaxies in the field. To get Ω, you have to know the luminosity-weighted average properties of galaxies in the field. And it seems to me that the numbers are such that unless you can take the absolutely largest M/L that you can get in rich clusters and apply that to all galaxies, you come up with a number substantially smaller than $\Omega = 0.2$. I see that Jim Peebles is writing something relevant on the blackboard. Perhaps he would like to comment.

PEEBLES: M/L for closure is 1500. Density parameter = 0.2. Ratio of total to ordinary mass $\sim$ 15, which says that the mass-to-light ratio of ordinary matter is 20. This is high, which I think is Felten's point.

Who wants to vote on what Ω will turn out to be? All in favor say "Aye".

THE MAJORITY: Aye!

RUBIN: No!

DEKEL: Write down their names! (much laughter)

PEEBLES: What will Ω turn out to be? We will count hands.

<div align="center">

The Poll

Ω	votes
$1.001 < \Omega$	2
$0.999 < \Omega \leq 1.001$	28
$0.05 < \Omega \leq 0.999$	29
$\Omega \leq 0.05$	2
Don't know	71
Don't care	0

</div>

PEEBLES: It has been pointed out by Juan Uson that right here at Princeton there is a remarkable experiment going on to measure the density parameter. This is a new test by Ed Loh and Earl Spillar. In the usual test for Ω, one plots a function such as magnitude *versus* redshift. The curvature is then used to constrain the cosmology and evolution. If you can measure redshifts wholesale, you can get a function of two parameters, redshift and magnitude. A way to measure redshifts wholesale is to use the Baum

method, which many people have tried and found difficult. Loh and Spillar
have improved this method to the point where they believe they have firm
evidence that it works. With these data they can deduce both evolution and
cosmology.

USON: This morning, Ed Loh told me that he gets $\Omega = 1.15 \pm 0.25$ (cheering
and applause). Ed is getting the redshift using six wideband colors; the
bandwidths are 1000 Å and they all have comparable sensitivity. This is
a significant improvement over previous attempts. Ed's error bars are
estimated on the assumption that 10 % of the redshifts are totally wrong.
He is continuing to increase the size of the sample.

PEEBLES: Are there any final remarks?

TULLY: A comment about the large-scale structure of the Universe. Scott
Tremaine instructed us that during crises we seek ways to magnify the
breakdown in the paradigm. I would like to present a result which I think
is important in this regard. A look at the distribution of Abell clusters
with respect to supergalactic latitude shows that on a scale of a tenth of
the event horizon, the Abell clusters lie in the very same (supergalactic)
plane as nearby galaxies. The second interesting fact is that the nearby
galaxies show secondary peaks in number density, suggesting that their
distribution is stratified in layers parallel to the supergalactic plane.
I don't think this was anticipated by any existing theory.

OSTRIKER: What did you plot to show this?

TULLY: The distance from the supergalactic plane in Mpc was plotted
against number counts. And let me point out that the concentration to the
supergalactic plane contains 100 Abell clusters of 10^{18} $M_\odot$.

LYNDEN-BELL: The objects are not in the same direction but are at the same
displacement?

TULLY: Yes. They are distributed all over the sky, in both the northern and
southern hemispheres.

KAISER: Could contamination by faint foreground galaxies increase the number
counts near the supergalactic plane?

TULLY: I don't think the effect is statistically important, although it
could creep in in a small way.

OSTRIKER: Isn't your effect due to the fact that there is more area near the
equator than near the poles?

TULLY: That has been taken into account in the normalization of the data.
There are also corrections for the way I sample the data and for the fact
that there is some galactic obscuration.

TYSON: This result may tell us something when compared to an experiment that
Seitzer and I have just completed. In 6 widely-spaced high-latitude fields

we see a remarkably constant density of faint field galaxies to J = 27.

PEEBLES: These results needn't contradict each other. One could have a
situation where the ups and downs are more prominent in some regions than
others, while the mean is more nearly uniform. ·In fact that's what we find
when we compare the Lick sample, for example, with the Abell sample.

SANDERS: I want to address the remarks made by Scott Tremaine at the
beginning of this session. If we are really in a crisis in the sense
described by Kuhn, a crisis leading to a scientific revolution, then that
would seem to call for a revision or extension of physical laws. Now, is
a hypothesis like cold dark matter (which requires an undiscovered heavy
particle) actually revolutionary or is it just a patch-up of the existing
paradigm? It seems to me to be analogous to the 19th-century attempts to
explain the anomalous precession of the perihelion of Mercury by inserting
an unseen planet close to the Sun. The only truly revolutionary idea
discussed at this meeting is that of Milgrom and Bekenstein.

TREMAINE: I don't want to get into a long discussion of the sociology of
science, but Kuhn takes as a revolution any large change in the way in which
a community looks at a problem. A revolution doesn't *require* a fundamental
revision of physical laws.

J. BAHCALL: I would like to rephrase one of Tremaine's earlier questions
as a general impression of this conference and of the way things are
proceeding. I think you might want to ask: Is there any reason any of
us are doing anything *other* than measuring rotation curves? Because I'm
enormously impressed by the speed with which our ideas have clarified as
a result of rotation-curve measurements. Already everyone is taking NGC
3198 as a classic case, yet it appeared in preprint form only a month or two
ago. Vera Rubin and David Burstein's work in preprint showing that galaxies
of different Hubble types have the same rotation curve is believed to be
showing something very fundamental. The similarity of the rotation curves
of NGC 891 and NGC 7814 is very new. The work by Ken Freeman and Claude
Carignan on bulgeless systems, which also have flat rotation curves, is very
new. I think there is an enormously rich future for us in rotation-curve
measurements. Maybe we will find one which has all the symmetries that
Renzo Sancisi would like but which decreases at large radii. This would
solve a lot of problems. I am not convinced that any of us should be doing
anything other than measuring rotation curves.

TREMAINE: I want to close with a quotation, again from Kuhn's book. This
was produced by a frustrated monk called Alfonso in the 12th or 13th century.
He was trying to predict planetary positions using Ptolemaic theory, which
at that time was in a dreadful state. He was feeling very depressed, and
this was his comment: "Had I been present at the Creation, I would have
given some useful hints for the better ordering of the Universe".

<center>(laughter and applause)</center>