*Colloques internationaux du C.N.R.S.*
N° 263 - L'ÉVOLUTION DES GALAXIES ET SES IMPLICATIONS COSMOLOGIQUES

# GENERAL DISCUSSION

S. VAN DEN BERGH: The general discussion is organized as follows: We have a panel of four astronomers who will start off with short talks on their impressions of what has happened here, and how far we have got, and what the main problems are. After that the session will be transferred to the floor. We will then begin with questions and brief comments directed to our two speakers at the end of the morning session, and after that cover various topics.

M. BURBIDGE: I want to make three comments. The first concerns the question of errors of measurement of redshifts from galaxy spectra, which was raised earlier in the conference, and the other two relate to the talks given to-day by G. Burbidge and M. Rees.

1. The accuracy of measurement is much greater when emission lines can be used. It has been suggested that systematic differences might arise between redshifts measured by absorption lines and by emission lines, because the H and K absorption lines fall where the continiuum intensity is dropping rapidly, so estimation of absorption-line centers might be affected. However, I believe this matter has been cleared up and in any case I do not believe the very large errors suggested by S. Simkin are likely to occur. One does not try to measure an absorption line which is overlaid by a great strong mercury line!

2. Commenting on what Dr. Rees said, I would like to remark that, as a person who is not unaccustomed to looking at astronomical plates, I have no need of drawings to show me the bridges that Dr. Arp described! I can see them perfectly well on the photographs.

3. Dr. G. Burbidge mentioned the interaction between prejudices of the observers and the search for good data on anomalous redshifts. I believe this is an important point. With the shortage of big telescopes and the large number of observers seeking to use them, most observers are allocated less telescope time than they really need. Thus they have to make hard choices on what they will use their observing time for. It is here that prejudices may enter - an observer may choose not to spend time making a difficult observation which may turn out to support a hypothesis against which he or she has a prejudice!

M.S. ROBERTS: I should try to make some general remarks, also. In a gathering like this one would hope, perhaps optimistically, that we come away with answers to some questions. It is not clear to me that we will even come away with well-phrased questions, as for instance what sort of test is necessary to distinguish between the two general competing hypotheses, the cosmological and the non-cosmological explanation? I asked Marten Schmidt this question a number of years ago. At the time he had no answer. He said that he had thought that finding quasars in clusters of galaxies would solve it. Of course, quasars had been seen in direction of clusters but the general problem is obviously still with us.

One obvious aspect that came up in both Rees' and G. Burbidge's talk was the question of the surface density of quasars. This is critical too much of what has come up in Arp's discussions. We take a value that has been given in the literature of the order of five per square degree. There is a factor or fifty lacking then. If one wants to question this number density, who is going to carry out a well-defined experiment and find out what it is to everyone's satisfaction? It seems to me that this is one critical test. There may be others. I would hope that at the minimum this gathering could come up with such an answer or a set of answers.

One of the titles of this symposium dealt with the evolution of galaxies. It was very appropriate in terms of cosmology, but the question in itself was not dealt with. I always wondered what a young galaxy looks like, and I would appreciate if before the end of the afternoon - either privately or in public - somebody could inform me what it really is like, so that I can go out and observe it with a radio-telescope.

Finally, I would like to make some comment about anisotropy, specifically the Rubin-Ford effect, which has been discussed here by a number of people. Mrs Rubin describes this effect as due to motion of our Galaxy and the Local Group and perhaps an even larger co-moving region, an undefined co-moving region. If she had set out to do this experiment in a different way, namely to find out if the Local Group is moving with respect to a shell of galaxies at a hundred Megaparsecs

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

and she had come up with the answer: yes, it is moving with zero velocity: Would you believe her? So the question really is not whether it is moving or not, but what is the magnitude of the vector. So, if you want to build in anisotropy into your particular theory, please, allow a little bit of motion of the Local Group.

H.E. SMITH: There are a couple of comments that I think might be relevant. In the first instance it is the question of the physics behind all of what we have been talking. To me as an observer, who tries to keep an open mind, there are some very strong arguments that there is an expansion of the universe or at least a relationship between redshift and magnitude and other parameters which are easily fit by conventional physics. But there are a number of things which someone who wanted to believe in conventional physics would call troublesome. It is to me somewhat of a question what one calls conventional physics. There are difficulties with QSOs and their energy production, in the problem of the strong variations on very short time scales, and in the necessity of extremely compact objects. It seems to me that it is simply a matter of what sort of new physics one wants to work with; whether one wants to work very hard on explaining these within one frame or whether one would be happier investigating the physics to see if there are other mechanisms for producing redshifts. I do not see that either one is particularly necessarily unconventional physics, and I think to do so would be biasing things very strongly. One of the important aspects of this conference has perhaps been that there are a number of people who are working on all aspects of physics. I think one can adopt a picture which is consistent with conventional physics and use that as a starting point and then see where inconsistencies lie, but it will be very wrong not to investigate the other areas of physics.

The second point I would just like to comment on is the question of the samples that we have been looking at in the past few days discussions. We seem to be dealing here with the difference between 3.2 and 2.8 sigma effects in samples which examplify the difficulty of astrophysics as a non-laboratory experimental science. I would just like to point out, or ask people to think about, the necessity for very careful selection of samples and the very great selection effects

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

which go into the way we do things. An excellent example is the question that Roberts posed on the background density of QSOs. We have a number which was based on studies of radioquiet stellar objects with ultraviolet excesses. Now, the work that has been done at Cerro Tololo by Malcolm Smith and is beginning to be done at Kitt Peak by Art Hoag has produced another selection effect. On these objective prism plates we are finding a different kind of QSOs, generally high redshifts QSOs, they are generally red and would not have been found previously. To me at least, it seems impossible to understand how one takes the new numbers - Margaret Burbidge had a number of 16 probable QSOs in one square degree down the 21st magnitude in a field of the Hercules cluster - and tries to compare with a number that comes from a sample that was selected completely differently. I am afraid much of the discussion we have been going through relates to the choice of our samples and what one believes about an effect which is of marginal statistical significance.

A. YAHIL: I should like to draw attention to what I would call the relation between the microstructure and the macrostructure of the Universe. It seems to me that a fruitful avenue of research in the coming years will be the question of the nature of the interactions between galaxies and clusters, and their relation to the general expansion of the Universe. We can no longer accept Hubble's picture of galaxies scattered in the Universe as tennis balls on a court, with little interaction beyond occasional clustering. For one thing this would imply an adiabatic decay of the peculiar velocities. If today they are, say, $\sim 100$ km $s^{-1}$, then they would have been 1000 km $s^{-1}$ at $z = 10$, 10000 km $s^{-1}$ at $z = 100$, and so on. Furthermore, Peebles has demonstrated observationally that galaxies show strong positional correlation out to a distance of $\sim$ 10 Mpc ($H_o = 50$ km $s^{-1}$ $Mpc^{-1}$), and weaker correlation to greater distances.

If we accept that galaxies are strongly interacting over large distances, we should also alter some of our traditional concepts. We cannot really say where a cluster ends. For example, some of the nearby galaxies which have traditionally been excluded from the Local Group might in fact be dynamically interacting with it. The extent of the Virgo cluster is another problem of interest, which has been with us

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

for quite some time. Perhaps we should begin to describe clustering not only in terms of density enhancements, but should also consider the velocity perturbations which they create. I think this problem of mapping the local velocity field should be given high priority, because it will give us our first dynamical understanding of the interaction of galaxies. Some questions of interest might be: Is the Hubble expansion of galaxies within, say, 5 Mpc slower as a result of the existence of the Local Group? How is the Local Group moving in relation to nearby galaxies and the Virgo cluster? Are we falling toward Virgo as Peebles suggests? Is there also a rotation as de Vaucouleurs would argue? Or is it a random motion? I have looked at the data, and have not been able to decide. More data, with good sky coverage is badly needed.

S. VAN DEN BERGH: At this point we move into the second part of our general discussion.

H. ARP: I would like to make two points. 1) As far as the a posteriori statistics are concerned, this has no relevance to the association of quasars with galaxies. In 1966 I started from the fact that radio sources are aligned and associated with galaxies and were commonly considered to be ejected. I asked whether another kind of radio sources, the quasars, also behaved this way. I got a positive answer which had a high probability of not being accidental. Subsequent associations were predicted from that result.

Whether Jim Gunn calculated the probability of an association with galaxies of the same redshift or calculated the opposite we are testing a prediction. If the probability of the observation seems to be accidental then we forget it. If the probability seems to indicate that the observation is not an accident then we go on to make more tests. It is as simple as that. The a posteriori argument, I strongly believe, is a red herring.

2) As far as the reason for the sketch Heidmann showed of NGC 7603, I am sure it was simply because he could not lay his hands on a photograph. It happens that this is one of the strongest connecting filaments. I would be willing to donate a very large picture of NGC 7603 to Dr. Rees if he would hang it on the wall opposite his desk and con-

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

template that, before we consider extra sensory perception, we should first perfect sensory perception.

REES: I am glad to have got this chance to correct a misapprehension which Arp and other people may have got from my talk. Some people seem to have felt that I was being rather hard and sceptial about Arp and people who look for a number of these effects. This was not my intension. My personal view, for what it is worth, is that I bet at least 30 per cent that he is basically right and more than 30 per cent that some unconventional physics is unvolved somewhere in quasars. Misapprehension could have arisen because when I likened it to ESP, people might be likely to dismiss ESP, but I take ESP seriously just as I take ARP seriously.

W.G. TIFFT: I would like to present in turn 3 diagrams. First the Coma redshift-magnitude band diagram. Two correlations are the bands themselves and morphological separation along at least the brightest band. Samples are complete within specific radius and magnitude limits. Photometry and redshifts have withstood tests of verification. The effects seem clear.

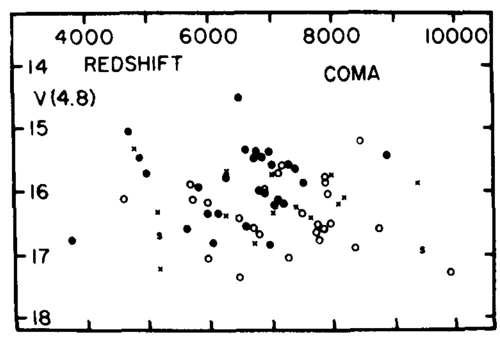


Fig. 1

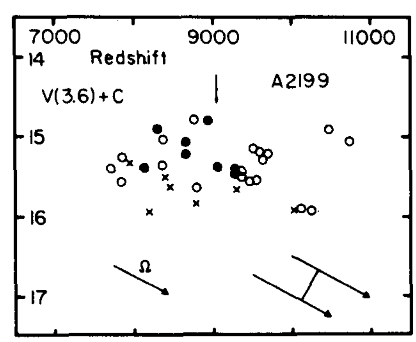


Fig. 2

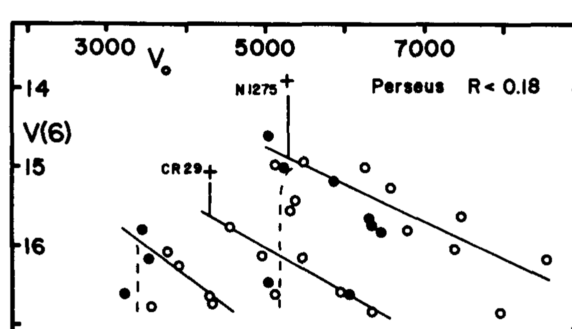


Fig. 3

Fig. 1) Coma V (4".8) - $V_o$ diagram, central region.

Fig. 2) A 2199. V (3") - $V_o$ diagram, central region

Fig. 3) Perseus. V (3") - $V_o$ diagram, centralregion.

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

Granting the estimation of significance can be questioned on a-posteriori grounds one proceeds to the second and more clusters. This was done first in A 2199. Slope, spacing, and morphological pattern were now predicted and verified.

Now Perseus shows bands closely as predictable again from Coma. Morphological data is not yet available in detail. A forth cluster, N 545, discussed in my paper above, shows the morphological pattern as predicted.

I believe that enough well defined samples now exist with predictive statistical value. I have heard no direct statement on the band phenomenon, its reality and/or meaning. I would like to hear from Prof. G. Burbidge in particular his impressions.

G. BURBIDGE: I think that I believe in Dr. Titt's bands in the Coma cluster and perhaps in the Perseus cluster. However, my instinct suggests that I should not, because if I do, I do not understand hardly anything about normal galaxies.

J.P. VIGIER: 1) Dr. Rees' classification is not correct since the introduction of non-velocity redshifts is not necessarily incompatible with other types of models, i.e. expansion or chronogeometry. It only raises difficult disentanglement problems.

2) His statement on the way to tackle discrepant data would have led in the past to difficult positions. Arp's data are facts, not ESP, i.e. not manipulated data. He might find himself in Lord Kelvin's position who thought physics was finished and that one should not overestimate the discrepant date i.e. the Michelson, Morley experiment and the black body radiation.

J.-C. PECKER: I would like to note, in reply to Martin Rees's question to me yesterday and to his comments this morning, the following: We should indeed eliminate from our vocabulary probably the word "cosmological" - and certainly the word "conventional". He reproached me not to produce spectra of QSS which could confirm our "tired-light" mechanism. No, I did not; we did not indeed try to build a theory of QSS - so far! Neither did Martin Rees (he is fully right in noting that we should not reproach him for that) explain the energy output of QSS, assumed to be at their "cosmological" distance, or the overlight velocities. Indeed, he, himself, has introduced a velocity of 0.995 C (for double separating sources) a magnificent "abnormal" redshift indeed! I do believe that neither of us is "conventional" or "non-conventional" in his approach; a long way is in front of us, and many problems to solve - happily! - whatever the point of view.

M. REES: My main reason for emphasizing superlight velocities in my talk was that in the written version of his paper yesterday Dr Pecker says that it is because the superlight velocities that he believes that there is overwhelming evidence that quasars are not at cosmological distances. In my view it is just one of the many difficulties that any theory of quasars has to explain.

Going back to Dr. Vigier I understand his point that the cosmological redshift is not necessarily entirely due to the tired-light effect. I would like to make a general point which applies to his theory and also to that of Dr. Barnothy and that of Dr. Segal. That is that any of these theories where the redshift is decoupled from the dynamics of the expanding universe have the disadvantage that there is no reason why there should be even an order of magnitude agreement between the Hubble time, derived from the Hubble-constant, and the age of astronomical objects. Dr. Tammann emphasized that we do know these are in accord within a factor of 2, at least. It seems in these other theories to be no particular reason why this accordance should occur.

J.M. BARNOTHY: It is well known that galaxies are surrounded by several hundred to thousand globular clusters. At the 142th meeting of AAS (Bull. AAS 6, 212, 1974) I have shown that on account of their particular mass distribution, globular clusters are powerful gravitational lenses. When a globular cluster becomes aligned and intensifies a background object which has similar spectral characteristics as quasars (for instance, Seyfert galaxies), a quasar will be observed near a galaxy. Hence, astonishing large numbers of galaxy-quasar associations, as observed by Burbidge, Arp, Burke, can be expected. From this explanation it follows that the probability to find the quasar farther from the galaxy increases with the size of the galaxy and the angular distance between galaxy and quasar decreases with the redshift of the galaxy. Galaxy-quasar associa-

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

tions prove the cosmological nature of the redshift of both, galaxies and quasars.

J. TERRELL: I am pleased to hear that Martin Rees would be delighted to find evidence of non-cosmological redshifts. There are now many quasars known with multiple redshifts; most of these redshifts, at least, cannot be proper indicators of distance, and I would like to know whether he is delighted by these discoveries.

Secondly, Maarten Schmidt stated in a recent survey article (in Galaxies and the Universe, Vol. IX, Stars and Stellar Systems) that, if quasars are indeed at cosmological distances, it is curious that no single independent confirmation of such great distances has been discovered. I would like to know whether Martin Rees agrees with Schmidt's opinion.

M. REES: There are two classes of theories. In one the absorbing material is shot out from the quasar with a speed major than half the speed of light, in the other theory the absorption lines are due to intervening galaxies, intervening clouds sometimes, with smaller cosmological redshifts. It seems to me that the issue between these two classes of theories is still quite open, although if they were partly due to intervening galaxies this would provide the direct evidence, which one would certainty like to have, that they are indeed at the distances implied by the cosmological interpretation of redshifts.

I.E. SEGAL: There has been no opportunity here to make more than passing reference to the chronometric cosmology, but it seems appropriate to claryfy explicit points which have been raised, such as that of apparent ages. Dr. Rees indicated his impression that the apparent coincidence of $H^{-1}$ with the age of the universe is beyond the power of the chronometric cosmology to treat. In fact, this is not the case; it predicts that the age a of a random object in the universe has the Cauchy distribution, which is proportional to $(1 + \frac{1}{4} a^2)^{-1}$. This is such a long-tailed distribution that it has an infinite mean, but it has of course a finite median and finite percentile points. Here a is measured in units of R/c, where R is the 'radius of the universe'. With conventional values for the redshift and distance to Virgo, this implies that half the objects in the universe should have apparently observed ages of the order $10^9$ to $10^{10}$.

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

On another point just mentioned, that of clearest evidence that quasar redshifts are generally indicative of their distances, I might mention a simple, robust, statistical test which seems relevant. A Spearman rank correlation test between the visual magnitudes and redshifts of the quasars in the De Veng et. al. list gives a probability $< 10^{-3}$ of obtaining by chance the observed tendency for fainter magnitudes to be associated with larger redshifts. The probability is probably still less for the larger sample now known; and tests of their type could be adapted to determine, in any fundamentally fully specified model such as the chronometric one, whether any significant portion of the quasars have redshifts which are not primarily of cosmological origin.

K.I. KELLERMANN: Rees mentioned the two possible interpretations of absorption line redshifts. At one time the interpretation as absorption in an intervening galaxy appeared unlikely due to the apparent excess of multiple absorption systems compared with what would be expected from a random distribution of intervening galaxies. What is the present situation?

M. REES: One question is whether all quasars with emission redshifts more than 2 have comparable numbers of absorption systems. There is some new evidence supporting that they do but it is still controversial. The other question is if there are enough galaxies. If you take the conventional sizes of galaxies there are certainly not enough but, then, it is argued by people who believe in intervening clouds, that the effective cross-section of a galaxy, which is relevant in this kind of calculation, could be an order of 100 times larger, because you do need only a small column density of gas etc. Therefore, I do not think one knows sufficient to argue against the intervening cloud hypothesis.

M. BURBIDGE: Concerning the absorption lines in QSOs: it is not true that the number of redshifts varies from object to object in a way that is consistent with a "normal" distribution of intervening galaxies or intergalactic clouds. This distribution is only obtained by selection of objects and averaging numbers of absorption redshifts. The actual distribution, from object to object, ranges from on the one hand something like that in PKS 0237-23, with at least 12 complex redshift systems,

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

going from z = 1·36 - 2·20, and splitting up into perhaps as many as 50 separate systems, and on the other hand some objects with zero number of absorption redshift systems - perhaps even no absorption lines, or perhaps just 1 or 2 individual lines that cannot be assigned a redshift. Thus I believe the great majority of absorption systems should be connected with the QSOs themselves. But the discussion of this would take us too far from what has been the subject matter of this symposium, and involves a great deal of detailed QSO spectroscopy.

J.-C. PECKER: Dr. Roberts asked for "young" galaxies. It occurs to me that, in yesterday's discussion, an ambiguity has been apparent. Double radio-sources, such as Cen A, and many others, appear as if they were symmetrically ejected by a central galaxy (generally, the expulsion seems to me "polar"). I feel (cf. my paper in Astron. Astrophys. 1972) that such galaxies are young, active. I would say that these pairs of radio-sources are not QSS, but that the QSS is indeed the "exploding" phase of the galaxy as such ( See a paper by Vigier and myself in Astrofizika 1976, in press).

F.D.A. HARTWICK: I like to report some very preliminary results on a slightly different way of determining the Hubble constant. A motivation for this method is that one would like to use direct distance determination methods, at least primary distance indicators directly. There are luminous enough galaxies outside the Local Group to observe to relatively higher redshifts. Thus, we have done DDO luminosity classification for 666 Sb galaxies in Nilson's catalogue and found that 33 of these galaxies, or roughly 5 % of the total, are Sb I and II, which of course are the same as M 31 and M 81. The idea is to use M 31 and M 81 as a base line and determine the Hubble line from these other galaxies. There are only 10 galaxies in our list of 33 for which data is available at the present time and for which the redshifts available are over 1000 km/sec. I think it is interesting that the preliminary results indicate that H comes out to be 83 $\pm$ 24; this is using the Sandage and Tammann calibration for the mean luminosity of M 31 and M 81. The errors may seem rather large but we hope that, by getting more data on the rest of the galaxies in the sample, the errors will be brought down.



https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

G. DE VAUCOULEURS: The major question at the present time is whether the Hubble law applies rigorously (i.e. linear isotropic expansion) within the Local Supercluster as stated by Sandage and Tammann or not as indicated by all our analyses of magnitude - redshift data since 1958, and confirmed by a re-analysis of the Sandage-Tammann Sc I data (Astrophys. J. 205, 13, 1976) and by Peebles' study within the concept of spherical symmetry of the supercluster.

B.F. MADORE: Recent spectroscopic and photoelectric observations of longperiod Cepheids in the Magellanic Clouds indicate that large and patchy reddening exist for Cepheids in extragalactic systems. Applying calibrations of the period-luminosity function which account for this systematic effect show that the distance to NGC 2403 (the only galaxy outside the Local Group to have its distance gauged by Cepheids) may have been previously under-estimated by 30 to 50 %. This single uncertainty is systematic and enters the uncertainty of the local distance scale at a fundamentally early stage.

G.A. TAMMANN: During the last 40 years the value of $H_o$ has been revised several times. Each major revision was due to a newly discovered effect: the scale error in the old magnitude sequences, the incorrect P-L relation of Cepheids, and the confusion of HII regions with the brightest stars. In the "Step Toward the Hubble Constant" we have allowed for yet another effect: the dependence of some distance indicators on the sample size; in addition we believe to have paid increased attention to the crucial problem of defining samples. Both factors work in the sense as to yield a low value of $H_o$. It should be stressed that if the old value of $H_o \approx 75$ should eventually be restored, it could hence not be for the old reasons, but it would have to be for newly discovered effects.

At this colloquium there seems to have prevailed some favor for $H_o$ being considerably larger than 50. However, the reasons for this increase were widely different. It was suggested that the fundamental Local Group distances could be smaller by 20 percent, that NGC 2403 was nearer by $0\overset{m}{.}8$, that M 101 was only $1\overset{m}{.}2$ more distant than the M 81 group, and that the Virgo cluster lied a mere $1\overset{m}{.}6$ beyond M 101. It is not the place here to discuss these discrepancies in detail, but it would seem to me that our Eiffel Tower has more than only three legs.

The cumulative effect of all the "corrections" proposed would place the Virgo cluster at a distance modulus of $\sim 29\overset{m}{.}2$, corresponding to $H_0 \approx 160$! I dare say nobody here has meant to propose such a value. It would lead us back by more than twenty years, to a time when we could neither understand the time scale of the universe nor solve the conditions imposed on our Galaxy by the Copernican principle.

M. ROWAN-ROBINSON: Personally I am very confused about $H_0$, and disappointed not to be leaving the meeting with a clear, agreed value. I wonder if it would help to focus attention on the areas of disagreement to ask those who have given values for $H_0$ what distances they assign to M 101, the nearest ScI galaxy, and to the Virgo cluster.

| | M 101 | | Virgo | | |
|---|---|---|---|---|---|
| | d (Mpc) | V (km $s^{-1}$) | | d (Mpc) | V (km $s^{-1}$) |
| Tammann | 7 | 400 | | 20 | 1100 |
| de Vaucouleurs | 5.1 | | Spirals | 14.1 | 1100 |
| | | | ellipticals | 12.3 | 1100 |
| Tully | 7 (bydefinition) | | | 14.5 | 1100 |
| Hanes | | | | 12.5 | 1100 |
| Heidmann | | | Tully-Fisher relation | 13.2 | 1100 |
| | | | luminosity-diameter relation | 13.8 | 1100 |
| Bottinelli | 5.62 | | | | |

K.I. KELLERMANN: I wonder if there is a fourth class of astronomers beside the ones which Martin Rees spoke about: These are the ones who want to create a controversy where perhaps there really is not one. I have the impression that a value of $H_0$ = 70 km/sec/Mpc is consistent with everything that has been said here. Does anyone object to this.

\- SILENCE -

J.P. VIGIER: What is the exact upper limit on H put by nearby experimental data? If it is lower than the high H value ($\sim$ 80) then this would

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

raise serious problems on its real physical nature.

G.A. TAMMANN: I think it is not me who should answer that question. I think Dr Tinsley has specific points as to the minimum age required by nucleochronology, and for instance Professor Refsdal could put errors on the believes on the age of globular clusters. It was of course unsafe to say that the upper limit of the Hubble constant is settled, but I would still maintain that now, and until new effects are found, the corrections for the Hubble constant essentially have been fundamental corrections such as I referred to above.

Of course, a totally new fundamental error can be found any day, and then the value is undetermined or the error range can not be predicted.

B. TINSLEY: Using nucleochronology for a lower limit to the age of the Galaxy, it might be safe to take the age of the solar system plus $3 \times 10^9$ yrs. Therefore the age of the Universe $t_o > 8 \times 10^9$ yrs. If we demand that $\Lambda = 0$, so the dimensionless product $H_o t_o < 1$, then the limit on $H_o$ is only $H_o < 125$ km $s^{-1}$ $Mpc^{-1}$. To get a better limit by this method, we first need to understand further details about chemical evolution in the Galaxy.

G. BURBIDGE: May I add something briefly about this. The point really is that you have several phases. You make the elements, but you got to make the stars to make the elements in. Depending upon how long this takes and which stars you consider important, you have various ages of elements and finally of an age for the time the solar system was segregated out. But the early history depends very delicately upon the galactic evolution and the kind of stars you make the elements in, so there is the uncertainty.

B. TINSLEY: No, that is not the main source of uncertainty. What you measure on the chronometers is the mean age of the elements in the best, and even if you assume that they come from the most massive stars the mean age can be either half the total age of the Galaxy or infinitely long.

G. BURBIDGE: Yes, it can be much longer but from the age of the elements you get essentially a lower limit.

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

O. JOHNS: An upper limit to the duration of nucleosynthesis is very dubious. But the lower limit may be more believable. Since it corresponds to a production function peaked at earliest times, it may indeed be a limit. But I caution that the recent determination of the limits to the age of nucleosynthesis quoted by Dr. Tammann is based on the Os-Re chronometric pair, which in turn depends on the errors quoted for the radioactive lifetime, as well as on our correct understanding of the relation between the r- and s- processes.

E.L. TURNER: I would like to report on a re-analysis of the Rubin-Ford data by P. Schechter. He has obtained a simultaneous fit of the seven (?) parameters of the Rubin-Ford model to their data. His resulting velocity (and its direction) agrees roughly with theirs, but his velocity uncertainty is roughly twice theirs. The difference arises from Schechter's proper treatment of the non-independence of the model parameters. The less than completely overwhelming statistical significance of the effect makes the error treatment a critical consideration.

V. RUBIN: The solution of Schechter is mentioned in my paper above. Schechter's analysis of all 184 ScI velocities and magnitudes gives $V_{\odot} \sim 715 \pm 250$ km s$^{-1}$, $l = 184^{\circ}$, $b = 0$; $V_{GM} \sim 175$ km s$^{-1}$ toward $l \sim 215^{\circ}$, $b = 0$. I take this to be essential confirmation of our result. A detailed examination of the differences of our approach and his is interesting, but really outside the range of this discussion.

H.E. SMITH: I was personally very impressed by the Rubin-Ford effect as one that was found in a small sample of objects and from what we have heard has been well confirmed by the larger sample. One question that I do not think has been addressed is the apparently severe discrepancy between the motion of our galaxy and the isotropy of the microwave background. Are there comments?

J.M. BARNOTHY: I think that when trying to interpret possible velocity anisotropies, not sufficient attention is given to the sidereal time periodicity of cosmic radiation. It is now well established that the extragalactic component of the cosmic radiation spectrum (above 300 Gev) displays an anisotropy which is compatible with a 250 $\pm$ 100 km/s velocity of the solar system relative to the universal cosmic radiation field;

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

and is directed toward ~ $90^{\circ}$ galactic longitude and ~ $0^{\circ}$ galactic latitude. This velocity, which was predicted by Compton and Getting in 1932, as being a Doppler effect caused by the rotation of the Galaxy, proves that there cannot be any other significant velocity component of our Galaxy relative to the rest system of the universe. The lack of this periodicity in the microwave background radiation, should its non-existence be proven without doubt, could ring the death toll of big bang cosmologies.

P.C. VAN DE KRUIT: If the discrepancy between the anisotropy in the Hubble flow and the upper limit to galactic motion with respect to the 3 K background in expressed in standard deviation of the formal internal errors in both measurements, would the difference definitely be a 3 $\sigma$ result?

V. RUBIN: Yes.

A.M. WOLFE: In 1970 Dan Schwartz analyzed QSO data of the X-ray background. About 1/2 of the sky was measured with no apparent cos $\theta$ effect present. The upper limit on our velocity with respect to an isotropic observer is 800 km $s^{-1}$. Perhaps newer Uhuru data would provide a better limit and help to clarify the conflict between Mrs. Rubin's data and the microwave results.

G.O. ABELL: The distribution of all galaxies to $m_v$ = 18.3 in a 6 x 6 - degree region centered on the Coma cluster shows a strong concentration that is identified as the cluster itself. However, the distribution of those galaxies we can identify as spirals in the same region (spirals identified to $m_v \sim 16$) show a roughly random distribution. Nevertheless, most have about the same radial velocity, and there is certainly an enhancement in the surface density of spirals in the Coma region as compared to parts of the sky, say, $30^{\circ}$ away.

We reconcile these data, I think, with the picture of the universe that has emerged over the past 20 years, namely that matter appears to be concentrated in large inhomogeneities of the order of 100 Mpc in diameter. These are the "superclusters" described by various investigators since the 1950's and recently demonstrated anew by the analyses of Peebles and his associates. The Coma cluster is in one of the second-

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

order clusters I catalogued in 1961, and is not a totally isolated system. Typically such an inhomogeneity contains about 2 great clusters (such as those in the Abell catalogue); some contain a dozen or more, and others, like the local supercluster, contain no great clusters. The great clusters - like Coma - are, then, imbedded in these inhomogeneities as dense concentrations, rather like the downtown areas of great metropolitan areas. Co-existing with the great clusters, in the same supercluster, are increasing numbers of clusters and groups of decreasing richness, and perhaps individual galaxies as well. The spirals in the Coma region, in this picture, belong to the same large inhomogeneity, but are not necessarily dynamically associated with the core of the Coma cluster itself.

Incidentally, the meager evidence we have suggests the superclusters are expanding with positive energy. If gravitation is negligible between the components of superclusters, the spread of velocity across them should be about 5000 km/s. With this possibility in mind, we should exercise caution in interpreting radial velocities of all galaxies in the directions of rich clusters.

N.V. VIDAL: Dr Yahil and I have analyzed statistically the velocity distributions of some twenty clusters of galaxies (that have at least 10 observed redshifts) and found that they are consistent with a gaussian. Four different small-sample statistical methods were used. Therefore it would be difficult to say, at the moment, that clusters may be large unbound inhomogeneities in the Universe.

W.G. TIFFT: Several observational programs (Tifft and Gregory, Chincarini and Rood, Fisher and Tully) appear to suggest that there are insignificant numbers of isolated galaxies. All galaxies appear to belong to well defined groups. Various references are to "field" galaxies and their role in large scale dynamics. We need to clarify further the term field galaxy and recognize (and further define) the real density contrast and its effect on our theoretical modeling.

H. ARP: Bill Tifft, do you not agree that the association of groups and clusters into superclusters, that George Abell discussed, is characteristic of the situation that observers see and believe?

https://doi.org/10.1017/S0252921100054002 Published online by Cambridge University Press

W.G. TIFFT: Not entirely, because even within the area where he has the separate dots, those dots are also quite well isolated (as in our Local Group) from each other. There is not a general haze even throughout that area.

G. DE VAUCOULEURS: In the Survey of Nearby Groups (Stars and Stellar Systems, vol. 9) we found that 10 to 20 percent of the nearer galaxies could not be associated with known groups. The concept of field galaxies cannot be completely rejected.

J.R. GOTT III: It was remarked this morning that investigation of anomolous redshifts requires studies of well defined samples of objects with close separations in the sky. The Turner-Gott groups of galaxies are selected as surface density enhancements of 4.65 in the sky without regard to redshift. 16 of the Turner-Gott groups now have complete redshift data, 5 of the groups having been done by Kirschner. These groups contain 68 galaxies, 17 of which have redshifts which one would regard as discrepant with respect to those of the bulk of the group members. Typically the bulk of the members have a dispersion in velocity of $\leq$ 200 km $s^{-1}$, with the "anomolous" redshift galaxies having redshifts that are far different. From the surface density enhancement of 4.65 used to select the groups we expect on the average that 15 galaxies out of the 68 should be background or foreground galaxies. Consistent with the number found with the groups thus cleaned of apparent background and foreground contamination the remaining groups and subgroups have a mean $m/L \approx 100$. This is a m/L value too low to close the universe but still larger than m/L values deduced from galaxy rotation curves. Motivated by Arp's work we decided to check this sample to see if the brightest galaxy in a group or subgroup had a tendency to have the lowest redshift in the group. Out of 20 cases (including 12 binaries) the brightest galaxy had the lowest redshift in 11 cases. (8 expected statistically), while in 6 cases the brightest galaxy had the largest redahift consistent with one over root N statistical errors. As more redshift data becomes available we can apply this test on a larger sample. This is the kind of well defined sample that is needed for this type of study.