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

ARGUMENTS AND EVIDENCE CONCERNING NON-COSMOLOGICAL REDSHIFTS -- A PARTIAL SUMMARY

G. Burbidge
University of California, San Diego, and
Max-Planck-Institut für Astrophysik-München

Les arguments et les preuves en faveur des décalages non cosmologiques vers le rouge, sont résumés.

Martin Rees and I have an almost impossible task -- to summarize on both sides of the argument the state of affairs in this field, but we agreed to try and entertain you.

Let me start by saying a few words about the first two days devoted largely to the Hubble relation and the attempts to determine H_o and q_o. From the welter of difficulties I discerned faintly that to bridge the differing views of the authorities we must summarize by concluding that H is known (probably) to within a factor of 2 (locally). There seems to be a view developing, led primarily by de Vaucouleurs, that the value of H_o currently estimated by Sandage and Tamman is too small. The method which has always appealed to me and clearly to Dr. Tamman is to simply measure diameters of galaxies and compare with the size of our Galaxy or M31. Hoyle and I once wrote a paper (around 1960) doing this using the galaxies in the Hercules cluster. We were persuaded not to publish it, but as I recall we obtained a value quite close to Sandage's estimate at that time of 75 km sec^{-1} Mpc^{-1}.

As far as the determination of q_o is concerned, it is clear that the real difficulties involved in determining it have finally been recognized, and it is not clear whether its true value can ever be obtained. This is not because the observational situation will not eventually be cleared up, but because of the difficulty of correcting for uncertain effects associated with the evolution of galaxies.

It does seem to me that a good case has been made for the reality of the Ford-Rubin-Roberts effect, and it poses quite a severe problem, though Bernard Jones has clearly been given the brief by his establishment advisers of explaining it away according to the Princeton-Cambridge-Moscow folklore of galaxy formation.

Now let me turn for the remainder of my talk to the problem of the redshifts. Let us start at the beginning.

What is the nature of the redshift phenomenon? We have all inherited the ideas dating from work of 40 or 50 years ago that the galaxies are all receding from us and that the redshifts are attributed to an expanding universe. With very few exceptions this dogma, which still remains unproven, has been accepted, and only very recently has it been challenged again. As you all know, Hubble himself began to have doubts as to this interpretation some ten years after he and Humason had first made the fundamental discoveries which led to the view that we live in an expanding universe. His reasons for doubting were doubtless unfounded -- they were tied to the difficulties he was having in understanding what we would now call the log N - log S relation for optical galaxies, and were apparent more than real.

By then, of course, the dogma had become totally entrenched -- probably correctly so.

With the discovery of the QSOs the fundamental question was raised again, originally by Terrell, who not having been educated as an astronomer was not conditioned to the idea that redshifts must all be attributed to expansion. After ten years of discussion of the topic, we have now come to this symposium.

Some of the background to the symposium is itself indicative of the state of the dispute. While our French hosts were very eager to have this discussion, there was clearly great reluctance on the part of some senior members of the I. A. U., and in fact we have two symposia, only the second of which is concerned with the topic in question (and not under I. A. U. auspices technically). Not only that, early attempts were made to arrange the program so that radically new things would not play a significant role in the program. While this has been corrected, some of those most involved in the argument, but on the conventional side, have refused to attend and have attempted to label the symposium a crank affair. However, we are meeting in a center right across from a psychiatric hospital and near to a major prison, so radicals, beware!

Why all of this pressure? Because, as is always the case when scientific questions are really fundamental, new ideas which, if they prevail, will overturn the old ones, are resisted by all means, in the name of science, but by any means that come to hand.

What kinds of unorthodoxy are being propagated at present, what following do they have, and what weight can we reasonably assign to them?

(1) A New Theory of Cosmology. The most extensive recent work in this direction is by Segal. His chronometric theory is very hard to describe physically, and I shall not attempt to do that here. The theory predicts that at small redshifts there should be a quadratic relation between redshifts and apparent magnitudes instead of a linear relation and Segal claims, as did Hawkins before him, that the field galaxies and

other groups of galaxies follow this relationship, while the linear relation for the brightest cluster galaxies he feels is meaningless since the galaxies are chosen in a non-random way. He also claims to be able to explain the redshift distribution of QSOs and the N(m) relation in a simpler way than is done in the framework of the Friedmann cosmology. I think that Segal has great difficulty in accounting for the origin of the microwave background in his theory and clearly there are many severe difficulties with it. However, I do believe that some attention should be paid to his claim that various of the observational quantities fit better on the theoretical curves predicted by his metric than they do on those given by the conventional theory. Experienced observational astronomers need to look carefully at this work. Until this is done and until the theory is explained in physical terms, I do not believe that it can be considered as a very serious competitor to conventional theory.

(2) Various attempts have been made in recent years to construct theories or scenarios in which the redshifts do not arise from the expansion. In this area we have the work of Hoyle and Narlikar in which it is argued that it is the mass of the particles which changes and is responsible for the shifts (i.e., it is the Rydberg which is changing), and that of Dirac who has revised his earlier theory that the large natural dimensionless numbers increase with epoch. This leads him to a non-expanding evolving universe in which continuous creation is required. The redshifts naturally arise due to the time shift -- the fact that the objects emitted at earlier epochs when atomic clocks were slower. The theory appears to be in difficulty as far as the microwave background is concerned, because since the number of photons is continuously increasing, the blackbody character is not preserved.

Narlikar spoke briefly about the Hoyle-Narlikar theory here, and again I feel that those ideas are worth further investigation.

The other approach which has been made is a new attempt by the French group to explain the redshift phenomenon by considering a modification of the theory of weak interactions so that the photons emitted in a strong radiation field will be slightly degraded (redshifted) without being dispersed. It is not clear to me whether these authors are claiming that they can explain the whole redshift phenomenon this way, or whether they are attempting to explain only the discrepant redshifts.

It is important to realize that only a theory of this type or of the type proposed by Hoyle and Narlikar could explain the existence of two objects close together in space but with very different redshifts.

(3) Since 1967 various attempts have been made to see whether or not there are any systematic effects in the distribution of redshifts. Initially it was claimed that there was a peak in the absorption-line redshifts at 1.95 and a peak in the emission-line redshifts also at 1.95(5). This was based on a small number of objects. A detailed analysis of more than 300

emission- and absorption-line redshifts by Burbidge and O'Dell in 1972 strongly suggested that there is a periodicity in the distribution with $\Delta z = 0.031$, but the obvious peaks at $z = 0.061$ and $z = 1.95$ are not statistically significant. Much of the weight of the 0.031 result comes from emission-line N systems which are clearly closely related to the QSOs, and for which in most cases no stars can be unambiguously shown to be present at the emission-line redshifts. Sturrock and his colleagues have repeated the analysis and have disputed the level of significance of the results. From analyses of larger numbers of redshifts by Wills and Ricklefs and Green and Richstone in 1976, it has been claimed that the periodicity has disappeared. However, Wills and Ricklefs do not give their basic data and they also apparently exclude the N systems which were important in the original analysis. In 1971 Karlsson presented evidence that the large-scale distribution of emission redshifts has peaks that form a geometric series in $(1 + z)$. He found that $(1 + z_{i+1})(1 + z_i) = 1.227$ or $\log[(1 + z_{i+1})/(1 + z_i) = 0.0888$. His more recent results confirm the earlier conclusions, the number of redshifts having doubled. The peak values corresponding to the series are 0.06, 0.30, 0.60, 0.96, 1.41, and 1.96. This establishes a connection between the obvious peaks at 0.06 and 1.96, and also the peaks at 0.30 and 0.60 discussed by Burbidge and O'Dell. Barnothy (1976) has recently found a rather similar result.

Tifft has claimed to find very small period effects in the redshifts of normal galaxies in the Coma cluster and perhaps in other clusters as well. It seems to me that his results for the Coma cluster at least are hard to refute.

None of these results has gained general, or even tentative acceptance. If any true periodicity in the redshifts of the QSOs and emission-line objects exists, it almost certainly suggests that the redshifts are intrinsic and have a very different origin from those of the galaxies which follow the Hubble law. However, it is certainly not ruled out that small intrinsic components exist in normal galaxies. It must always be remembered that the redshift that we observe z_{obs} is the sum of several components, i.e.

$$(1 + z_{obs}) = (1 + z_c)(1 + z_i)(1 + z_r) \quad ,$$

where z_c, z_i, z_r are the cosmological, intrinsic, and random motion components, respectively. Since we suppose in general that $z_r \ll z_c$, the different cases are:

a) If intrinsic redshifts are large in the QSOs, then $z_i \gg z_c$, and $z_i \gg z_r$, so that $z_{obs} \simeq z_i$.

In such a situation even comparatively small, but finite cosmological redshift components $z_c \lesssim 0.02$ would add so much noise that it would be very difficult to see peaks or periodicities in z_{obs}. This is why I continue to be intrigued by the obvious peaks in the distribution, even if

https://doi.org/10.1017/S0252921100053975 Published online by Cambridge University Press

the statistical arguments do not necessarily support the idea that these are real.

b) The case described by Tifft is one in which $z_i \simeq z_r$, but $z_i \ll z_c$. In such a situation the effect will only be seen in a cluster for which z_c is the same for all of the galaxies, and z_r is a measure of the random motion of the cluster galaxies.

Probably the only features in the histogram of redshifts and related objects which there will be general agreement on here is that there is a steep drop off in redshifts beyond about 2.2 and that there is a gross peak which has persisted close to 2 (1.95) ever since the objects were first discovered. (The recent successful attempts to discover QSOs using objective prism techniques will artificially enhance this peak.)

So far I have discussed theories which imply an unconventional interpretation of the redshifts, or numerology associated with the redshift distributions. Realistically, only a very small minority of us really feels that this work is taking us in the right direction, though of course this may simply be due to the fact that these ideas are premature.

More significant are the observations of various types which make it progressively harder for all of us to accept without question the cosmological redshift hypothesis for all classes of extragalactic objects. I briefly discuss some of these:

(4) The rapidly varying QSOs and BL Lac objects, which if they are at the distances suggested by their redshifts have luminosities coming out of very small volumes which give radiation densities which lead to the Compton paradox in a much more severe form than it has ever been found before. Objects like AO 0235+164, 3C 279 (in its outburst in 1937) and others are emitting at peak power levels > 10^{48} erg sec^{-1} if they lie at cosmological distances, and their maximum sizes based on light travel time arguments are < 10^{17} cm. This leads to extremely severe problems. I know of no easy solution. Bringing the objects closer is one of the easier ones.

These arguments are all based on the evidence of rapid light variations. The radio flux variations also lead to great complications. In particular there were reports at the Cambridge meeting of more QSOs showing low-frequency flux variations.

(5) VLBI studies of a number of QSOs and related objects like 3C 273, 3C 279, 3C 345 and 3C 120 were described by Kellermann. Christmas tree models do not seem to work, and highly relativistic expansion with all of the attendant difficulties are implied unless the objects are local.

(6) More apparent physical associations between objects with very different redshifts have been reported. They include:

a) More associations between comparatively bright galaxies and QSOs described by Arp here and by Bolton in Cambridge. The correlation between the angular separations of galaxies and QSOs, and the distances of the galaxies (corresponding to approximately constant projected linear separations) which was originally found for the 3CR QSOs has held up. Bolton added three more pairs and Arp has added seven or eight.

b) A number of very close pairs of QSOs (separations $< 2'$) have been found by Bolton, Peterson, Wills and Wills (Ap. J. Lett. in press). Using about 100 radio QSO candidates they found that about 10% of them had a second candidate QSO very close to it. Spectroscopic investigations have shown that in at least five cases both members of the pair are genuine QSOs. In none of these cases are the redshifts the same, and in three or four pairs they are totally different.

c) Arp has shown us several more spectacular examples of galaxies with very different redshifts which appear to be associated. In particular, the elliptical with a redshift of $\sim$ 13,000 km sec^{-1} clearly lying in front of a spiral with a much smaller redshift was particularly impressive.

Some results of this type have been criticized because the statistical arguments which have been used to evaluate the significance of the results have been done a posteriori. Clearly this should not be done, and has not been in all cases, but I would like to stress that this method has been, and is, widely used in astronomy and is generally accepted except in cases in which the hypothesis under consideration has not gained general acceptance. A good example is the case of PKS 2251+11, a QSO which lies close to a group of galaxies. Gunn and later Wampler and Robinson attempted to measure the redshift of two of the faint galaxies and concluded that at least one of them had a redshift very close to the QSO. Then, after the discovery and using the observed parameters, Gunn calculated the probability that this was accidental association and found that it was very small. The result was then widely publicized and still remains for many one of the strong pieces of evidence for cosmological redshifts.

In summary it appears to me that the observational evidence just described under heading (6) is the strongest evidence that we have that objects with large non-cosmological redshift components do exist. Only if the surface density on the sky of QSOs is one to two orders of magnitude larger than the estimates which are currently used based on the work of Sandage and Luyten, can we reasonably argue that apparent associations are accidental. In any case, in my view the number of accidents is becoming embarrassingly large.

The other arguments I have listed are suggestive but not always compelling.

Now, of course, there is evidence on the other side. There are a number of cases of QSOs close to galaxies at the same redshifts. There are also the various continuity arguments which are attractive to many people, and also the studies of the Hubble relation for QSOs and the correlations discussed here by Petrosian. Perhaps there are two kinds of QSO. This has been suggested in the past. If it is the case, it will be that much harder to get to the truth.

Finally, I would like to say a few words about the way this kind of science is being done. In this field at present you find that with very few exceptions, if the astronomer is well known, you know before he speaks what position he will take. Even more disturbing, if he or she is not so well known, but comes from one of the great centers of learning, you also know once you know where the individual comes from, what his or her position will be. The field has become almost totally polarized. And the observers tend to get the results that they expect. Many of them certainly know what they are looking for, and are not likely to discover anything new. To the few open minded theoreticians among you, I would say, Beware of observers, particularly optical observers, bearing gifts!

Research in extragalactic astronomy at UCSD is supported in part by the National Science Foundation and in part by NASA under grant no. NGL 05-005-004.

https://doi.org/10.1017/S0252921100053975 Published online by Cambridge University Press