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

THE "CONVENTIONAL" VIEW OF REDSHIFTS

M. J. Rees
Institute of Astronomy,
Cambridge, England.

Exposé du point de vue "conventionnel" sur les décalages vers le rouge.

In Peebles' well-known textbook, one chapter is entitled "a child's garden of cosmological models". Maybe a "jungle" would better describe the lush diversity of theories expounded at this exceptionally interesting conference. If there is a dominant orthodoxy in cosmology, the proceedings here have successfully obscured it - a participant without prior exposure to the subject would not have gleaned from this week's discussions what views were "conventional" and what were not. Anyway, I presume that my brief is to assess the status of the cosmological views that would commend themselves to Peebles and his like: that is to say, the package of ideas in which there was a "hot big bang", galaxies and clusters condensed via gravitational instability, the quasar phenomenon is related to galactic nuclei, and all large redshifts (except perhaps quasar absorption lines) are due to the expansion of the universe. This, at least, is the framework within which we "conventional" people attempt to interpret the data - or (in the view of some "radicals") the self-imposed blinkers by which our vision is confined.

The unconventional models we have heard about fall into two categories: some involve a whole-hog reappraisal of the entire expanding universe concept; but others have the more modest aim of explaining isolated "anomalies" in quasars, etc. Barnothy's old FIB model and Segal's newer "chronometric" cosmology fall squarely in the first category as do some of Hoyle and Narlikar's ideas; and the views adumbrated by Terrell, Arp and others - on which we have actually heard little at this meeting that is new - are of the second kind. The sophisticated new variant of the "photon fatigues" theory proposed by Pecker, Vigier and their associates is intermediate: it can (but need not necessarily) account for the bulk of the ordinary Hubble redshift; but, independently of this, it claims also to account for local anomalies and excess redshifts in objects with high radiation density.

Dr Burbidge has summarised the arguments that lead some people to suspect that orthodox cosmologies are inadequate. If one accepts this, the obvious next step is to decide which (if any) of the unconventional theories now before us accounts best for the alleged data. And one must not overlook the fact that the alternative unorthodox models that we have heard about differ among themselves at least as greatly as they differ from the standard model. It is not clear to me that we yet have any specific theory that can accommodate all the "anomalies" (and Dr Burbidge and I are doubtless equally convinced that "more work needs to be done" on <u>all</u> redshift theories). None of the "whole-hog" revisions of conventional cosmology yet incorporate any dynamics. Nor do they naturally account for the order-of-magnitude concordance between the Hubble time and the astrophysically-estimated ages of galaxies, etc. Above all, we must not forget the enormous body of kinematic, dynamical and astrophysical data (including of course the microwave background) which is consistent with conventional ideas, and which any serious rival theory must accommodate as well.

We have heard several authoritative (and sometimes conflicting) assessments of the status of optical observing programmes aimed at determining H_0 and q_0. A clean and unambiguous determination of q_0 seems an increasingly remote goal, there now being a consensus that evolutionary corrections are important and still inadequately understood. But even if we grant that the universe does indeed have the remarkable simplicity associated with "Robertson-Walker" symmetry, cosmology still encompasses more than just "the search for two numbers". The results reported this week actually offer great encouragement to "conventional" people who hope to use objects with redshifts up to $z \simeq 5$ to probe ~ 90 per cent of cosmic history. We can reasonably expect soon to find quasars with $z \gtrsim 4$, and ordinary galaxies with $z \simeq 1$ as well. "Young" galaxies at $z \gtrsim 2$ may also be detected - indeed they may have been observed already. The relationship of quasars to galactic evolution should then become less mysterious. The studies by Dr Rubin and her colleagues are certainly telling us more about the large-scale clustering of galaxies. (Moreover, investigations of small-scale structure in the X-ray and microwave background should elucidate the early stages of galaxy and cluster formation, though this topic was beyond the scope of the present conference.) Determinations of the geometry of the universe are inextricably linked with astrophysical issues, so we can expect firm estimates of q_0 only as a byproduct of much broader investigations of galactic evolution and "astrophysical" cosmology.

Even if we do not wish to jettison the standard "big bang" scenario in its entirety, we should still envisage (in an open-minded way) the possibility of anomalous effects in particular classes of objects, or in restricted regions of space. The arguments bearing on this issue have traditionally focussed on the nature of quasar redshifts.

What are the arguments favouring the cosmological interpretation of quasar redshifts? Theoretical considerations are still indirect and unconvincing, and continuity arguments are "double-edged"; but there are three points which now seem quite compelling:

(i) The redshift-magnitude correlation for the brightest quasars. This suggests that z is indeed a distance indicator: no such correlation would be expected if z were intrinsic and the objects uniformly distributed.

(ii) The angular size-redshift relation for double sources identified with quasars. There are two significant features here: (a) the upper envelope is z-dependent (which again suggests that z is a distance indicator); and (b) this envelope more or less coincides with that for bona-fide radio galaxies.

(iii) Evidence for intervening galaxies. Drs Condon and Wolfe and their associates have shown that the HI cloud causing the high-column-density 21 cm absorption feature in the spectrum of 3C 286 has a large enough transverse extent to cover two of the three radio components of this source. This cloud answers the description of an intervening galaxy.

But what about the arguments on the other side?

https://doi.org/10.1017/S0252921100053987 Published online by Cambridge University Press

I agree with Dr Burbidge that the least shaky arguments are those based on measured redshift differences between objects which appear to be physically linked. Such effects include the claimed systematic excess redshift in the companions of nearby galaxies (critically discussed by Dr Heidmann in his comprehensive review); the QSS-galaxy associations that were first quantitatively discussed by the Burbidges, Solomon and Strittmatter; and the alleged excess of "quasar pairs". The statistical significance of the latter depends crucially on the background quasar density: are there ~ 1, ~ 10 or ~ 100 quasars of $\lesssim 19^{th}$ magnitude per square degree? Everyone must surely agree that a better answer to this question is an urgent desideratum for quasar studies in general.

Even harder to assess is the evidence for direct physical connections - bridges, jets, chains, etc. One has here to ask three questions: (i) Are they real? Some sceptics have suggested that the apparent "bridge" in (for instance) M 205 may be photographic artefacts, (as the "jet" once claimed in 3C 287 turned out to be). (ii) Are they significant? To answer this question one needs to know how many galaxies would appear, on deep plates, to be "disturbed", with bridges, whiskers or other excrescences which do not terminate at (or point towards) an interesting object. (iii) Are they mysterious? Even though it would be difficult to devise conventional interpretations when large redshift differences are involved, this is less implausible in more typical cases. The work of the Toomres and others has taught us that straightforward gravitational interactions during ordinary galactic encounters can generate strikingly peculiar configurations; and conventional physics certainly permits galactic nuclei to give rise to slingshot ejections, radiation beaming, etc. Nobody could deny that Dr Arp's recent pictures of NGC 1097 show something rather odd, but it by no means follows that it is inexplicable conventionally.

To account for these (still unexplained) phenomena is certainly a challenging - and very worthwhile - task for the astrophysicist. In the meantime one should surely suspend judgement on the more comprehensive and fundamental manifesto in, for instance, Dr Arp's review article in Science (and here I quote): "whole classes of objects, including quasars, compact galaxies, blank field radio sources, luminous material, lines of galaxies and companion galaxies appear to be ejected in opposite directions from the nuclei of large active galaxies".

Advocates of non-cosmological redshifts have from time to time adduced various other statistically odd effects in support of their case - redshift periodicities, etc. The problem here is that it is all too easy to perceive patterns in random data - patterns which may (when their statistical significance is tested "a posteriori") be improbable at the one per cent level. The methodological dangers here are surely now well known, if not always fully appreciated. The crucial test is whether a hypothesis has predictive power, and applies not just to the objects in which the alleged effect was first noticed, but to some new samples as well. I have the impression that most of the peculiarities in the redshift distribution have been diluted (even if they have not disappeared) as more data have accumulated. However new effects have often surfaced to replace the old. It is inevitable, however, that, as the subject proceeds more and more surprising effects should be discovered. Unless these effects can nearly all be incorporated into a single theory which is as specific and well-defined as the cosmological hypothesis, these effects cannot be claimed as adding cumulative weight to an unorthodox

https://doi.org/10.1017/S0252921100053987 Published online by Cambridge University Press

viewpoint. Moreover, when there is said to be only one chance in (say) 200 of getting a particular effect by pure coincidence, we should not blindly accept this as statistically significant without applying a "discount" to allow for all the other similar effects which might equally well have been found but were not. For instance, Dr Tifft showed us his "bands" in the magnitude-redshift plot for the Coma clusters, but (leaving aside the objections made by Dr Simkin and others) we should not forget all the other scatter diagrams that we have seen during the week which did not have evident "bands" in them, but might, a priori, have equally well done so.

The cogency of the case for anomalous redshifts rests largely on arguments of the above kind, and I suggest that even the most enthusiastic radicals would accept a verdict of "not proven".

But there is a further line of argumentation which (though often used) is surely very weak indeed: I mean the frequent assertion that particular features of quasars - power requirements, radiant energy density, rapid variability, etc. - raise insurmountable astrophysical difficulties (which would be evaded if quasars were local) and that this "therefore" supports a non-cosmological interpretation. Surely only an excessively self-confident astrophysicist would take such an argument seriously. Many aspects of quasars are indeed very mysterious and problematical, and not much is satisfactorily explained. But the same can be said of many better-known and better studied phenomena - in astrophysics, and even in terrestrial and laboratory physics. It would be surprising if the puzzles of quasars had been cracked by the efforts - limited both in duration and quality - of the small band of astrophysicists interested in the subject. So it would seem premature to assert that quasars transcend the limits of conventional physics (and not merely of present-day conventional physicists!). Progress has been slower than we hoped, but not necessarily slower than could reasonably have been expected; and certainly we have not reached such an impasse as to justify unthinking abandonment of orthodox ideas in favour of novel or unconventional models which, if made equally specific, would perhaps reveal even more glaring difficulties.

One well-known and much-aired difficulty concerns the general energetics and nature of the central object - is it a massive black hole or a dense star cluster? The overall energy problem is of course no worse in quasars than in radio galaxies (and the latter has been appreciated since Dr Burbidge's pioneering work in the 1950s) but the concentrated nature of the power output raises extra difficulties. When quasars were discovered they seemed qualitatively different from anything hitherto known. But maybe the astrophysical difficulties would have looked less daunting (and the "local" theory would never have attained such wide currency) if quasars had been discovered later, when we were already familiar with (i) non-stellar activity in the nuclei of galaxies with bona fide redshifts, and (ii) models for pulsars and stellar-mass black holes, in which gravitational effects can plausibly convert rest-mass into non-thermal radiation with $\sim$10 per cent efficiency.

The superlight velocities, reviewed for us by Dr Kellermann, are another conundrum. The VLBI observations now definitely suggest systematic expansions rather than "Christmas tree" effects. If the variations are intrinsic to the sources and the redshifts are cosmological, one must seek an explanation which utilises (via some combination of phase velocities $\gtrsim$c or relativistic bulk motions) the well-known kinematic effects which permit

https://doi.org/10.1017/S0252921100053987 Published online by Cambridge University Press

apparent transverse velocities >>c for appropriate geometries. There are various models in the present or forthcoming literature, but time does not allow me to assess these here. It is not implausible that a relativistic plasma should acquire a bulk velocity that is itself $\sim$c, and a variety of models can then give rise to apparent superluminous expansions. It is harder to explain expanding doubles, particularly when the two components appear comparably bright and/or the axis is "remembered" from one outburst to the next. However Drs Blandford and McKee (and the author) have devised some models which do not seem excessively contrived, and can display expansion rates up to (6-8) c without invoking any special orientation (and higher apparent speeds when viewed from special directions). This class of model would run into difficulties if systematic expansions much exceeding $\sim$10c occurred in too big a fraction of observed cases.

There are four sources where expansions have been well-studied: 3C 120 ($z = 0.03$), 3C 273 ($z = 0.158$), 3C 279 ($z = 0.536$) and 3C 345 ($z = 0.594$). The expansion velocities are (3-8)c. The previously-apparent proportionality between redshift and apparent expansion velocity (which cast doubt on the validity of z as a distance-indicator) has now disappeared. There remains, however, a splendid and unexplained correlations between z and 3C number!

The cosmological hypothesis is at least specific enough to be vulnerable to confrontation with the data: we can all agree on which observations raise difficulties for it and which do not. But advocates of local theories have not yet provided an equally well-defined target to shoot at, so the current debate has an inherently asymmetrical flavour. Indeed, it would be hard to devise a theory that could accommodate more than a fraction of the claimed anomalies - for instance, the redshift periodicities and quasar-galaxy correlations (if significant) are just as inconsistent with Dr Terrell's ideas as they are with the conventional model. Dr Arp's scheme (in which the "youngest" and most recently ejected material displays the largest excess redshift) is so vague that I cannot think of anything that would refute it! Indeed the status of these ideas is rather like the status of ESP investigations: a hotchpotch of apparently bizarre things are happening, but one cannot quantify their significance and has no coherently-formulated framework to guide the search for corroborative evidence. However, even though many of us remain sceptical - as we do of ESP - we may nevertheless agree that, because the pay-off would be of such fundamental significance, such investigations should be supported, and (wherever possible) pursued in an increasingly systematic fashion.

Dr Burbidge has laced his talk with a number of psychological and sociological comments. Although our prime concern should be the validity and cogency of the arguments themselves, rather than the reasons which lead particular people to advocate particular views, I would nevertheless like to conclude with a few impressionistic comments in this vein. One observes three contrasting attitudes. Some astronomers (in my view; and, a fortiori in Dr Burbidge's) adhere to "conventional" views with excessive dogmatism: such persons would be genuinely "disturbed" at any novel development or complexity that would prevent (or even delay) the systematic delineation of the overall "big picture"; and they do not believe in anomalous redshifts. But other equally distinguished astronomers fervently wish to discover "new physics", and would welcome an attendant upheaval in our cosmological ideas. They want a revolution (especially if they can lead it!); and on the whole take the existing evidence "anomalies" very seriously. But there is a third (and rather disappointed) category of people to which I myself belong. We also would be elated rather than disturbed by the prospect that astronomy could contribute something so interesting and fundamentally new, but are still unconvinced that a revolution is yet justified. I'm therefore sorry not to have heard more compelling arguments during this conference, and think that astrophysicists should still attempt to interpret phenomena within the conventional framework . While retaining the hope that observers will eventually come up with something which convinces even the most hard-boiled sceptic, I'd bet increasing high odds against this happening.