

## Concluding Remarks

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**Abstract.** I review the reason for considering the prime purpose of the program of measurements of the fundamental parameters of cosmology to be the tests of cosmological models. I comment on the philosophy by which we are approaching this goal, offer an assessment of where we stand, and present some thoughts on where the tests may be headed.

### 1. Introduction

These Proceedings document impressive progress toward a satisfactory completion of the great program of measurements of the parameters of cosmology that commenced in the 1930s. It has taken a long time, and has brought into play many phenomena and measurements that could not have been anticipated in the 1930s. We may at last be approaching closure of this program, and it is appropriate to reflect on why we are so interested in these measurements and how it informs our interpretation of the results.

### 2. The Significance of the Cosmological Tests

I take the literal reading, that the purpose of the cosmological tests is to test models, in particular the commonly accepted relativistic Friedmann-Lemaître cosmology. It certainly is useful to have byproducts such as the demonstration of the presence of a term in the stress-energy tensor that acts like Einstein's cosmological constant  $\Lambda$ , which may help guide us to a resolution of the perplexing physics of the energy density of the vacuum, and a measurement of the radius of curvature of space sections at constant world time, which may prove to be a clue to what the universe was like before it could have been described by the Friedmann-Lemaître model. But all this is true only if we have convincing reason to trust the basis for these results.

A part of cosmology we can trust is the near homogeneous evolution of the observable universe from a denser hotter state. The list of evidence is familiar but worth repeating to make the point: we have compelling reason to believe this is what happened. Deep counts of objects at wavelengths ranging from radio to gamma rays are close to isotropically distributed across the sky. Either we are close to a center of spherical symmetry or our universe is close to homogeneous. If the latter, and the distribution is expanding so as to preserve homogeneity and isotropy, the recession velocity satisfies Hubble's law. The low redshift part of the SNeIa measurements is an impressively tight demonstration of the

redshift-distance relation. The cosmological interpretation of quasar redshifts passes demanding tests, such as the tight correlation of Lyman-limit and Mg II absorption lines with galaxies at close to the same angular position and redshift, showing quasars are behind lower redshift galaxies. If the expansion traces back to very high density galaxies at high redshift are seen as they were closer to the time when galaxies could not have existed, and ought to look younger than nearby ones. The effect is amply demonstrated. The 3 K radiation (the CBR) could not have relaxed to its thermal spectrum in the universe as it is now because space is not opaque at the Hubble length: radio sources are observed at  $z \sim 1$ . We can understand the thermal spectrum if the universe has expanded from a denser, hotter state that is optically thick within the Hubble length. The angular position of the peak of the spectrum of angular fluctuations of the CBR agrees with the conventional physics of the evolution of primeval adiabatic mass density fluctuations if the universe has expanded and cooled by a factor much larger than  $z_{\text{eq}} \sim 1000$ , the redshift of decoupling of matter and radiation. Helium and deuterium are natural byproducts of expansion from still higher temperature.

This list offers no guidance to what happened at very large redshift, or well outside the Hubble length. The inflation concept has shown us how easy it is to imagine the universe at great distance is not at all like what we see, but determining whether such an “island universe” picture is realistic is outside the current round of cosmological tests as I would define them. I don’t know whether the people in the 1930s who pioneered the program of cosmological tests gave much thought to the spatial and temporal limitations of empirical evidence within their cosmology. If not we have to adjust the program.

You can add to the list of evidence for evolution, depending on how much you want to rely on models, but I think the point is clear: it employs a broad variety of phenomena observed in quite different ways. Individual entries could be wrong, but it would be absurd to imagine all quite consistently point in the wrong direction. Thus Hoyle, Burbidge & Narlikar (1993) accept cosmic evolution, but argue the last substantial addition to the entropy in the CBR could have occurred at a much more modest expansion factor than in the standard model. This is a considerable difference, but it should not obscure the point that the redundancy of evidence has forced us to the answer to Hubble’s (1936) question, is the cosmological redshift the result of the general recession of the nebulae? It is, and the general recession is associated with cosmic evolution.

Our answer to Hubble depends on local physics and symmetry arguments, but it makes little use of general relativity theory (hereinafter GR); I did not even mention the relativistic relations among observables that Tolman (1934) listed.<sup>1</sup> The observations of supernovae of type Ia probe one of the relations, between magnitude and redshift, and detect a departure from the Einstein-de Sitter case. This magnificent accomplishment is in no way depreciated by noting that by itself it is not a cosmological test. In addition to the slight

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<sup>1</sup>The theory of the origin of the light elements assumes the expansion rate equation,  $H^2 = 8\pi G\rho/3$ . This follows from local physics; relativity enters only in the expression for active gravitational mass. The CBR anisotropy computation uses the angular size distance-redshift relation, a highly nontrivial application of the large-scale spacetime geometry, but the success of the prediction forces its inclusion in my list of elementary evidence for cosmic evolution.

chance some quirk of the physics of supernovae has avoided the thorough checks for systematic error (or, to be really cautious, that Nature has put us at the center of a spherically symmetric universe with a slight radial density gradient), the conventional interpretation depends on GR, and, within this theory, the measurement is readily fitted by the adjustment of free parameters. Hoyle, Burbidge & Narlikar (1993) might similarly fit the measurement by suitable choice of parameters within their theory.

The elegant logic of general relativity theory, and its precision tests, recommend GR as the first choice for a working model for cosmology. But the Hubble length is fifteen orders of magnitude larger than the length scale of the precision tests, at the astronomical unit and smaller, a spectacular extrapolation. The extrapolation is tested by checking for consistency of the cosmological parameters derived from different aspects of the geometry of spacetime. The Robertson-Walker line element figures in Tolman's (1934) list of cosmological relations. The computation of the CBR anisotropy spectrum uses GR to propagate the irregularities in the radiation distribution through spacetime that is predicted to be strongly curved over the expansion factor  $z \sim 1000$  since decoupling, and it uses GR to predict the dynamics of small fluctuations in the distributions of matter and radiation at  $z \sim 1000$ . The dynamical estimates of galaxy masses from rotation curves and streaming velocities assume the latter aspect of GR, the inverse square force law for gravity. Weak and strong gravitational lensing use this law, with the usual factor of two correction. A tight check of consistency of the parameters derived from these different phenomena would be a demanding test of GR and the cosmology.

The spectrum of angular fluctuations of the CBR offers a wonderfully rich basis for these tests. These Proceedings discuss the constraints on the density parameters in dark matter and in Einstein's cosmological constant  $\Lambda$  (or a term in the stress-energy tensor that acts like  $\Lambda$ ), to be compared to what is indicated by the dynamical measurements of masses of galaxies and systems of galaxies, by the curvature of the redshift-magnitude relation, and by the measurement of  $H_0 t_0$ ; the density parameter in baryons, to be compared to the theory and observational tests of the origin of helium and deuterium at high redshift and to the observational baryon budget at low redshift; the density parameter in neutrinos, to be compared to laboratory and atmospheric oscillation experiments; and the amplitude of the primeval density fluctuations, to be compared to measurements of the distributions of galaxies and mass at low redshift.

The impressive consistency of constraints that have already emerged from such different applications of GR and the cosmological principle suggests the theory and the cosmology are on the right track. We should be cautious about the details, however, because the interpretation of the CBR anisotropy also assumes the adiabatic cold dark matter (CDM) theory for structure formation, and the tests of this model depend on some subtle issues of astronomy.

### 3. The Model for Structure Formation and the Issue of Voids

We pay particular attention to simple and elegant ideas in physical science because Nature tends to agree with us. We have examples in cosmology: GR, Einstein's cosmological principle, and the adiabatic CDM model for structure

formation.<sup>2</sup> But Nature is quite capable of surprising us, as witness the evidence for a significant cosmological constant, which a few years ago was generally considered to have no socially redeeming value. Since many of the cosmological tests depend on the CDM model we must consider its empirical tests.

In these Proceedings Carlos Frenk and John Peacock present impressive observational successes of the CDM model. There are a few clouds on the small-scale part of the horizon, however; an example that has particularly impressed me is the void phenomenon (Peebles 1989). Carignan and Freeman's (1988) "dark galaxy," DDO 154, seems to be a close approximation to one of the failed galaxies that figure in commonly discussed interpretations of numerical simulations of the CDM model. In these simulations there is appreciable mass in the voids defined by the positions of dark mass concentrations that are massive enough to qualify as homes for normal  $L \sim L_*$  high surface brightness galaxies. This void medium contains low mass halos that would seem to be acceptable homes for galaxies like DDO 154. So why are galaxies like DDO 154 not found in the voids?

There are void galaxies; nearby examples are the pair NGC 6946 and NGC 6503. The former is an Arp (1966) peculiar galaxy, but only because a supernova was seen in it. Sandage and Bedke (1988) give a magnificent image of this galaxy; I have been assured it looks like an ordinary large near face-on spiral, though maybe unusually gas-rich. The other appears to be an edge-on spiral; it is classified as Scd in the *Nearby Galaxies Catalog* (Tully 1988). The CDM model simulations show occasional substantial upward mass fluctuations in generally low density void regions that could be homes for  $L \sim L_*$  void galaxies, but how would the baryons in these isolated mass peaks get spun up to form normal-looking if isolated spirals?

To me the most remarkable and challenging phenomenon is that observable objects respect the same voids. This applies to giant and dwarf galaxies, and low and high surface brightness ones (eg. Pustil'nik et al. 1995; Popescu, Hopp & Rosa 1999; and references therein); to gas clouds observed in emission (eg. Zwaan et al. 1997); and to high surface density gas clouds observed in absorption (Lanzetta et al. 1995; Steidel, Dickinson & Persson 1994).<sup>3</sup>

A common and defensible opinion is that the astrophysics by which void matter becomes visible as a galaxy of stars or an HI or MgII absorber is so complicated as to quite confuse the interpretation of void phenomena. Cen & Ostriker (2000) give an example: in their physically motivated prescription for galaxy formation the void probability for all galaxies identified in a simulation is much larger than for the mass. Cen & Ostriker conclude observed voids are not an argument against CDM-like models. This is a valuable example of the subtlety of the astrophysics, but I am even more impressed by the presence of dwarf galaxies on the outskirts of the Local Group, isolated enough to seem

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<sup>2</sup>One can think of many other arguably less elegant models for structure formation; you can trace through astro-ph my list of alternatives, each killed by the inexorable advance of the measurements.

<sup>3</sup>Shull, Stocke & Penton (1996) show that gas clouds detected as very low surface density Lyman  $\alpha$  absorbers avoid dense galaxy concentrations. My impression is that they also avoid the voids, but that is a subject of work in progress by Shull and colleagues.

to be primeval rather than products of physical processes operating within the large galaxies. These dwarfs are visible; why should similar primeval halos in the voids be so cunningly hidden?

If void phenomena ruled out the CDM model we could turn to alternatives. Bode, Ostriker & Turok (2000) show that if the CDM is replaced by warm dark matter it greatly reduces the numbers of small dark mass halos, tends to produce dwarfs at lower redshift, and yields smooth patches of dark matter within the voids outlined by the massive halos. All are positive changes from CDM. But their figures 4 and 5 show caustics of dark matter threading the voids. If these caustics fragmented into low mass halos would the model predict greater numbers of dwarf or irregular galaxies extending into the voids than is observed? If the caustics remained smooth would the model predict more void absorption line systems than is observed? It looks like a serious challenge.

I don't consider the void issue a very serious challenge to the Friedmann-Lemaître cosmology. Maybe I'm fooled by the astrophysics, as Cen & Ostriker (2000) argue. Maybe the CDM model must be adjusted, perhaps along the lines of Bode, Ostriker & Turok (2000), perhaps in some other way. The magnificent prediction of the measured first peak of the CBR angular fluctuation spectrum shows the CDM model very likely is close to the right picture. It would be less surprising to learn an improved structure formation model yields somewhat different constraints on the cosmological parameters, of course. Here is a worked example. Suppose at  $z \sim 1000$  there were objects with strong Lyman  $\alpha$  emission lines, like quasar spectra with suppressed ionizing radiation; maybe primeval black holes. The Lyman  $\alpha$  photons would delay recombination, preserving the height of the first peak of the CBR fluctuation spectrum, but shifting it to a larger angular scale for given cosmological parameters, and biasing this measure of space curvature (Peebles, Seager & Hu 2000).

#### 4. Is Cosmology a Science?

Disney (2000) asks whether cosmology "is a science at all," while I have been presenting it as a healthy and productive quantitative physical science. We might get some insight into the origin of these very different assessments from two considerations.

First, our commonly accepted cosmology did grow by the introduction of hypotheses to fit phenomena. Some hypotheses have been checked and established, as cosmic evolution. Some are being checked, as dark matter. The dynamical mass estimates quite consistently indicate the cosmological density parameter is low,  $\Omega_m \sim 0.2$ . The SNe redshift-magnitude relation and the measurement of  $H_0 t_0$  both favor this low value of  $\Omega_m$ . As discussed in the last section we are assuming GR, but applying it in quite different ways. The consistency at the level we now have is an elegant though not yet very precise test of GR and the dark matter hypothesis. In short, cosmology does depend on hypotheses, but we have nontrivial progress in testing them.

Examples of work in progress are worth listing as a reminder of how broadly based the cosmological tests are becoming. Consider the projects to measure the predicted secondary peaks of the CBR temperature fluctuation spectrum and give us a first look at the polarization anisotropy, to test delayed recombination

among other things; measure the shape of the redshift-magnitude relation at redshifts well above unity, to test the prediction that the expansion becomes matter-dominated; establish the constraint on parameters from the rate of gravitational lensing of background AGNs by galaxies; improve the measurement of  $H_0 t_0$  to the point that it can distinguish between low density models with and without  $\Lambda$ ; improve the constraints on the amount and distribution of mass from measurements of galaxy distributions, peculiar motions, and gravitational lensing; check the theory of structure formation through X-ray, optical, infrared, and radio surveys of the evolution of the intergalactic medium, galaxies, and clusters of galaxies; and maybe even test the dark and  $\Lambda$ -matter hypotheses through advances in particle physics. This work may yet lead us to an impasse, hypotheses multiplying faster than the data. That would drive us to new ideas, which would be exciting. The alternative is that we end up with an extensive and compellingly tight network of tests of a cosmology close to what we have now, which would be gratifying.

The second consideration compares two lines of research. In his contribution to these Proceedings Neil Turok considers what the universe might have been like at redshifts so high the Friedmann-Lemaître model certainly could not have applied. Turok very correctly emphasizes that the question is open and absolutely must be addressed. But we have to live with the fact that an empirical validation of the answer may be a long time coming. Most papers in these Proceedings deal with the more limited goal of understanding the large-scale nature of spacetime and its material content within our Hubble length, now and back in time through some ten orders of magnitude of expansion factor. This certainly is not a modest program either, but the empirical situation is remarkably good: the standard theory has passed demanding observational tests, and work in progress promises substantial improvements. The empirical basis for research on the early universe is a lot more limited. This is an example of the ways in which the well tested and established cosmology is incomplete; another is that we can't say what the dark matter is. But any active physical science is similarly incomplete: each has a well-tested center around which is the exciting confusion of ongoing research.

We all can make pretty good judgments about which elements of our subject are well and reliably established, and which are working hypotheses, when we put our minds to it. And the community has a reasonably accurate calibration of where each of us may tend to err on the side of caution or optimism. Our colleagues in other fields can't be expected to make these calls, and we shouldn't be surprised that when they have do so they may arrive at unduly pessimistic conclusions. We know how to remedy this, and should put our minds to it.

I considered cosmology a real physical science decades ago, though with a meager well-established center. The big recent change has been the rate of addition to the established center. But I don't think we're in danger of running out of meaningful research on open issues any time soon.

## 5. A Next Generation of Cosmological Tests

Martin Rees comments on the future of research in cosmology once the present round of tests is satisfactorily concluded. Here I add some thoughts on another round of cosmological tests of the physics of the very early universe.

The rules of evidence in science have evolved to admit quite indirect approaches. The community agrees that the many laboratory tests of quantum mechanics fully validate it as a real and magnificently successful physical science, even though no one has ever seen a state vector in nature. If the CBR revealed a distinctive signature of the tensor curvature fluctuations predicted in some implementations of inflation then I think most of us would accept it as an indirect but strong piece of evidence that inflation really did happen, even though none of us was there to see it.

One version of the deconstructionist picture of science as I read about it is that clever people make up internally consistent stories to fit agreed-upon conditions, and that another group could have made up another story, equally consistent, with an equally satisfactory fit to some similar or maybe different set of agreed-upon conditions. Those of us who believe we have convincing evidence physical science describes aspects of an objectively real world, even on scales very different from what we can hold in our hands, reply that our theories have been validated by agreement with tightly over-constrained and cross checked empirical tests. Inflation as we now understand it can be adjusted to fit a broad range of possible empirical results. This situation is unnervingly close to the deconstructionist picture unless we stipulate that inflation is a working hypothesis.

Michael Turner has asked whether empiricists like me would promote inflation from working hypothesis to established science if advances in basic physics produced a unified fundamental theory that is internally consistent, passes all laboratory tests, and predicts fields and interactions that unambiguously produce inflation. If this fundamental theory allowed no free parameters to be adjusted to fit the astronomy, and within the uncertainties of the astrophysics it predicted the full suite of observations, it would be a brilliant addition to established cosmology. A less perfect fundamental theory might have free parameters, some of which could be fixed by laboratory measurements, while others would have to be determined by the constraints from a cosmology established through the rules of evidence one sees applied in these Proceedings. Should we be satisfied if this theory could be adjusted to fit all the observations? We would be well advised to adopt it in most of our analyses of astronomy, but not to accept an adjustment of the rules of evidence to admit it as the established picture. I suppose most of us think of the early universe as something that really happened, and things that happen tend to leave traces. Let us cling to the hope that something will turn up.

## 6. Concluding Remarks

The evidence assembled in these Proceedings favors the existence of several kinds of matter: one that acts like Einstein's cosmological constant, with density parameter  $\Omega_\Lambda \sim 0.75$ , nonbaryonic low pressure matter with density parameter

$\Omega_{\text{DM}} \sim 0.2$ , baryons with density parameter  $\Omega_{\text{baryons}} \sim 0.05$ , and neutrinos with  $\Omega_{\nu} \sim 0.001$ . I believe it is too soon to add the first number to the list of firmly established elements of the science of cosmology, because it depends on the model for structure formation, and some of us see apparent problems with the model. People have been discussing the cosmological parameters for seven decades; we can wait a few more years to determine whether we have got the science right.

If advances in the applications of the cosmological tests firmly established the values of the fundamental parameters of the Friedmann-Lemaître model it would mean general relativity theory has satisfied demanding tests on the scales of cosmology, and that we have a well-tested history of structure formation. But I would not be surprised to find that this advance leaves us with the challenge of establishing the physics of the very early universe.

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