Jim Peebles (*far left*), Alison Peebles (*center left*), Eunice Wilkinson (*center right*) and Dave Wilkinson (*far right*) on the occasion of Dave's award of the National Academy of Sciences Watson Medal.

Seeing Cosmology Grow

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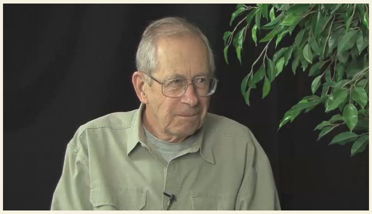

Watch a video interview with P.J.E. Peebles

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Abstract

The modest science of cosmology I encountered a half century ago has grown into big science. I comment on steps in this development I think I understand because I was there or, in some cases, wish I had been. Wonderful insights—or lucky guesses—and elegant deductions from measurements were accompanied by the usual mix of unlucky guesses and disregard of unwelcome evidence. I say "usual" because I suspect the course of development of any other natural science is similarly erratic. An example in cosmology is Einstein's homogeneous Universe, which was largely accepted as a working hypothesis when there was scant evidence and seriously challenged after we had a reasonable case for homogeneity. Similar mixes of insight and inattention led to the eventual identification of the 3K microwave background, the demonstration that large-scale structure grew by the gravitational instability of the expanding Universe, and the completion of a tight network of cosmological tests. A half century ago, we had little idea what would become of what we were doing in cosmology. We have a better picture now, but I expect there to be more surprises.

1. INTRODUCTION

These recollections[1] of how I came to study the large-scale structure of the observable Universe—the science of cosmology—may afford some understanding of what formed the approach I have taken to this subject, the lessons I draw from what happened, and my thinking about where the science may be headed.

I remember admiring the starry skies over the plains of Southern Manitoba, and aurora, but took no particular interest in the names of planets or stars or constellations. I still admire the sky, and my career has fed on what astronomers are doing, but I still cannot get interested in where the planets are.

I was fascinated by how things around me work, particularly things I was allowed to take apart. I remember reading, in an older sister's schoolbook, an account of compound pulleys. I thought that was neat and still do: To me, physics is compound pulleys, all the way down. The faculty at the University of Manitoba (U of M) showed me that I like doing physics and taught me a lot about how to do it. Fellow students added to my education in physics, instructed me on rudiments of social behavior, introduced me to Alison, who was studying microbiology at the U of M, and saw us married before we left Manitoba in the Fall of 1958.

Ken Standing, who had returned from Princeton with a Ph.D. in physics (1955), instructed me to go there for graduate study. A friend at the U of M, Bob Pollock, graduated a year before me and also went to Princeton. Professor Rubby Sherr was the department's grand master of nuclear physics, and Bob, with Ken before him, worked in this group. Professor Donald Hamilton sought me out to see if I might fit into his research on atomic beam measurements of the spins and magnetic dipole and electric quadrupole moments of radioactive nuclei. A short conversation showed Don that I am not another Standing or Pollock. Rubby now lives closer to the nuclear physicists at the University of Pennsylvania. Ken's present research at the U of M includes a major program of time-of-flight mass measurements of the macromolecules of interest to biophysicists. Bob and his wife Jean (who studied geology at the U of M) moved to Indiana University, and we keep in touch. Bob is doing elegant tabletop experiments on non-neutral plasmas. I stayed at Princeton.

The Princeton physics graduate general examinations introduced me to cosmology. There were occasional problems on general relativity, including properties of the Friedman-Lemaître solution, and I dutifully learned how to compute them. To me, this solution was an oversimplified exam problem, like a frictionless elephant on an inclined plane, not a serious model for the real world. I was not disabused of that by my textbooks on general relativity, *The Classical Theory of Fields* (Landau & Lifshitz 1951) and *Relativity*, *Thermodynamics and Cosmology* (Tolman 1934), which present beautiful theoretical physics but little phenomenology. A fellow graduate student, Ken Turner, showed me Bondi's (1960) *Cosmology*. It has interesting phenomenology, but it took me some time to see that. I was put off by the philosophy and shocked by the steady-state cosmology: They just made that up.

I came to Princeton intending to study particle physics [and even wrote one paper (Peebles 1962) to which ADS credits one citation]. That was changed by yet another graduate student from the U of M, Bob Moore, who took me to meetings of Professor Robert Henry Dicke's gravity research group. Bob Dicke liked astronomy: His first paper was on a gas sphere model for

[1]More recollections of how cosmology grew are in *Finding the Big Bang* (FTBB; Peebles, Page & Partridge 2009). I have taken a single story line in this account, and hope friends and colleagues who would have been mentioned in a more complete recollection of my life in science understand. I apologize also to colleagues whose contributions to this story are not mentioned through failed judgment or memory.

Figure 1

Bob and Annie Dicke with their first child, Nancy, in Cambridge, Massachusetts, in 1945. This is about the time Bob was moving from the MIT Radiation Laboratory to the Department of Physics at Princeton University.

a globular star cluster (Dicke 1939). As part of war research at the Radiation Laboratory at MIT Bob invented a microwave radiometer (Dicke 1946); Dicke & Beringer (1946) used it to measure microwave radiation from the Sun and Moon, and Dicke et al. (1946) used it to show that "there is very little (<20° K) radiation from cosmic matter at the radiometer wave-lengths" of 1 to 1.5 cm (Dicke et al. 1946, p. 340). **Figure 1** shows the Dickes at about the time of this very interesting measurement. **Figure 2** shows Bob 35 years later with two colleagues who also had large effects on my career.

When Dicke joined the Princeton faculty he decided he ought to do laboratory physics. His remarkable contributions include (Dicke 1953) collisional line narrowing and (Dicke 1954) superradiance that describes many particles radiating as a pure quantum state. Appreciation of the latter took time. His paper on superradiance received 15 ADS citations in the first decade after publication and 1042 citations in the past decade.

Dicke (1957, p. 52) explained his change of direction in the proceedings of a conference at Chapel Hill, North Carolina:

> . . . the situation with respect to the experimental checks of general relativity theory is not much better than it was a few years after the theory was discovered, [despite] the tremendous improvement in experimental techniques now available.

Figure 2

From left to right: Dave Wilkinson, Bob Dicke, Ed Groth, and Jim Peebles in February 1981 on the occasions of a celebration of the Nobel Prize award to Val Fitch and Jim Cronin and the Ph.D. defense of Bob's student Jeff Kuhn. At the time, Bob was working on tests of gravity physics, paying particular attention to the possibility that a rapidly rotating solar interior produces a gravitational quadrupole moment large enough to affect the orbit of Mercury and the test of general relativity. Dave was occupied by ground-, balloon-, and satellite-based measurements of cosmic radiation backgrounds. Ed and I with other colleagues had nearly completed a program of statistical measures of how galaxies are distributed, oriented, and moving. Some of this is mentioned in Section 4. I had just published *The Large-Scale Structure of the Universe* (Peebles 1980) and was exploring ideas of how structure formed. The photograph was taken by Jane Groth.

Dicke mentioned work in his group to improve the experimental situation: a pendulum for precision measurement of the gravitational acceleration to "detect possible annual variations" (Dicke 1957, p. 59), a repetition of Eötvös's demonstration that the gravitational acceleration of a free test particle depends very little on its composition, and the development of a rubidium atomic clock to better measure the motion of the Moon. He recalled Dirac's idea that the strength of the gravitational interaction might be small because it has been decreasing for a long time, and Bob discussed how that might be detected. At the conference John Wheeler discussed the argument by Ernst Mach and others in the nineteenth century that inertial motion has relative rather than absolute meaning. Bob did not mention this, but we heard a lot about it in his gravity group meetings.

The gravity research group discussed a wonderfully free flow of ideas to be explored by experiments, mostly benchtop, consultation of the literature on geology and astronomy, and exploratory calculations of the sort I enjoy. I published a few, some with Bob. He decided I was better suited to his new research interest rather than particle physics and invited me to look into constraints on possible variations of the strength of the electromagnetic interaction, which usually is represented by the fine-structure constant α. A change of α over geological times would have different effects on the Coulomb energies of different atomic nuclei, depending on their charges and structures, which would change decay rates, resulting in inconsistent radioactive decay ages derived from different isotopes and different alpha, beta, and fission decays. I noticed that the β-decay energy of ^{187}Re to ^{187}Os is particularly small, so the decay rate is particularly sensitive to the value of α. My source, Strominger, Hollander & Seaborg (1958), lists only rough estimates of the decay energy, so I drew up a plan to measure it. Bob suggested I stay with theory. My dissertation included a covariant gravity theory with variable α that does not violate the Eötvös experiment, and I obtained a limit from terrestrial and meteorite radioactive decay ages on the change of the fine-structure constant, $|\delta\alpha/\alpha| < 1 \times 10^{-3}$ in the past 4.5 Gyr. Current bounds are two orders of magnitude better and trace twice as far back in time.

FTBB: *Finding the Big Bang* (Peebles, Page & Partridge 2009)

The essay by Peter Roll and the interview of Dave Wilkinson in *Finding the Big Bang* (Peebles, Page & Partridge 2009, hereafter FTBB) describe how their lives were changed by Dicke's suggestion that they build a Dicke radiometer that might detect thermal radiation from a Hot Big Bang. Bob's invitation to think about theoretical consequences of finding or not finding this radiation set my career. I was uneasy about the slight empirical basis for cosmology, but could think of a few interesting things to work out, which led to others, which led to the realization that this was fertile ground. I taught a graduate course on cosmology in the fall of 1969. John Wheeler insisted that I turn my lectures into a book. To encourage that, he sat in on my lectures and at the end of each presented me with notes in his elegant hand. That so unnerved me that I wrote the book *Physical Cosmology* (Peebles 1971). I did not attempt to assess the subtleties of astronomical distances and the properties of stars. That left room for a reasonably complete survey of most lines of research in cosmology in a book of modest size. How many volumes and experts in the various parts of modern cosmology would a similarly complete survey require now?

2. MACH'S PRINCIPLE AND EINSTEIN'S HOMOGENEITY

I came to cosmology in the middle of research that eventually showed that the observable Universe is close to homogeneous and isotropic: no edges, no preferred center or direction. I like the story of how this happened as an illustration of the circuitous paths natural science takes.

Einstein's theory of general relativity allows for a universe with a single galaxy outside of which space-time is asymptotically empty and flat. The galaxy could rotate, with all the usual effects of rotation, but it would be rotation relative to empty space. Einstein (1923, p. 109), in *The Meaning of Relativity*, wrote that if the Universe were constructed this way then "Mach was wholly wrong in his thought that inertia, as well as gravitation, depends upon a kind of mutual action between bodies." I interpret this to mean that Einstein (1917) argued for homogeneity because it prevents violation of Mach's thought. [I pass over the argument for homogeneity made by Einstein (1917), which does not seem carefully considered.] Mach's (1893) book, *The Science of Mechanics*, is worth reading for elegant demonstrations of what is now termed classical mechanics, acerbic commentaries on the shortcomings of its philosophical basis, anticipation of Lense-Thirring dragging of an inertial frame (p. 277 in my 1960 edition), and the opportunity to reflect on the path from Mach to Einstein to the discovery of near homogeneity of the observable Universe.

The path was not straight. In *Relativity, Thermodynamics and Cosmology*, Tolman (1934, p. 363) wrote that homogeneity

> ...is to be regarded merely as a working hypothesis, suggested by the present state of observational knowledge, ... perhaps subject to far-reaching modification if more powerful telescopes should reveal a systematic lack of uniformity in different parts of the universe.

In my edition of *The Classical Theory of Fields* (Landau & Lifshitz 1951, the English translation of the 1948 Russian first edition), a footnote on page 332 cautions that although observations offer some basis for the assumption of homogeneity, "it remains an open question whether this situation will not be changed even qualitatively as new data are obtained." This sensible remark is the more striking because the magisterial series of books on theoretical physics by Landau & Lifshitz is almost entirely free of phenomenology. In *Cosmology*, Bondi (1952, p. 168) warned that it is "quite possible that looking into more distant spaces will completely upset present concepts." There was some evidence of homogeneity: Hubble's (1936) galaxy counts scale with apparent magnitude about as expected if the galaxy distribution were close to homogeneous, and the linear relation between galaxy redshifts and distances (Hubble & Humason 1931) agrees with homogeneous and isotropic expansion. These measurements probe to recession velocities ~10% of the velocity of light, an impressively large distance but far from the edge of the observable Universe. In an authoritative assessment of the observational situation, Oort (1958) stressed that there are large fluctuations in the counts of galaxies across the sky, but he was willing to conclude his review with an estimate of the cosmic mean mass density, assuming there is one. Tolman (1934), Landau & Lifshitz (1951), and Bondi (1952) went further, working through detailed analyses of properties of homogeneous world models. That was fair enough: It was a working hypothesis, but little more.

Gérard de Vaucouleurs (1970) was right to emphasize this, but tests were emerging. For example, in Klein's (1966) and Alfvén's (1966) world picture, a bounded cloud of galaxies is expanding into empty flat space-time. Velocity sorting would put the faster-moving galaxies further away, producing Hubble's linear velocity–distance relation. The highest velocities would have to exceed a tenth of the velocity of light to fit Hubble's counts and Hubble & Humason's (1931) redshift measurements. That would be quite an explosion, but you might imagine it. The problem for this picture is that we are in a sea of radiation that is close to uniformly bright across the sky. The X-ray background was known to be isotropic to a few percent (Schwartz 1970), and the microwave background was known to be isotropic to one part in 10^3 (Partridge & Wilkinson 1967, Conklin & Bracewell 1967). Because space was known to be transparent to microwave radiation—radio sources were observed at high redshifts—radiation produced in the Klein-Alfvén explosion at the start of the expansion would have long since left the cloud of galaxies. If the radiation we observe came from the galaxies, then we would have to be at the center of the cloud to a part in 1,000, and the cloud would have to be isotropic to like accuracy, which seems absurd. If instead the sea of radiation uniformly filled the space into which the galaxies are moving then our Galaxy would have to be moving relative to the sea at less than about 0.001 times the velocity of light while the vast majority of galaxies—apparently equally suitable homes—are moving relative to the sea much faster, up to a tenth of the velocity of light. That also seems absurd. The more reasonable interpretation is that, contrary to Klein and Alfvén, the radiation is flowing with the expansion of the homogeneous Universe of galaxies (Peebles 1971, p. 40). I used to hear the complaint that this draws a large conclusion from limited and indirect data, but that is how science operates. The case for homogeneity is still tighter when there is independent evidence for the same conclusion, of course. We have that, as is discussed below.

Another alternative to Einstein's homogeneity picture is an unbounded clustering hierarchy, a fractal galaxy distribution. The idea has a long history (Bondi 1952, p. 14), but Benoit Mandelbrot

(1975), in *Les objects fractals*, made the community pay attention. He argued for a scale-invariant galaxy distribution with fractal dimension $D = 1$, meaning the typical number of galaxies within distance r of a galaxy scales as $N(<r) \propto r^D \propto r$, because that makes the gravitational potential GM/r independent of length scale: There is no relativistic divergence of space-time curvature on large or small scales. At the time, the statistical analyses recalled in Section 4 showed that on scales of less than about 10 Mpc the low-order galaxy position correlation functions agree with $D = 1.23$. More importantly, Mandelbrot would have it that the fractal distribution continues to the largest observable scales, while I interpreted the evidence to be that on scales larger than 30 Mpc the fractal dimension is close to $D = 3$, which is statistical homogeneity. Consider that in a scale-invariant fractal Universe the fluctuations $\delta N/N$ of galaxy counts across the sky are independent of the depth to which they are counted (assuming a sensible nonsingular distribution of distances of galaxies selected by apparent magnitude). This is what scale invariance means. It predicts that the large fluctuations across the sky in the Shapley & Ames (1932) catalog of bright nearby galaxies are repeated in the Lick counts of fainter more distant galaxies (Shane & Wirtanen 1967, Seldner et al. 1977). That is quite contrary to the galaxy maps I used to show fractal advocates (usually to little effect). A quantitative version of this argument is in Section 4.1.

Cosmic microwave background (CMB): 3K cosmic thermal microwave background radiation

The case for homogeneity now rests on the tight network of cosmological tests (Section 5.1). My choice of the single most relevant piece of this evidence is the consistency of the statistics of fluctuations across sky of the temperature of the cosmic microwave background (CMB) (Larson et al. 2011) and the spatial fluctuations in the large-scale galaxy distribution (Percival et al. 2001). This depends on, and tests, many elements of the standard cosmology, but the point is that measurements of the radiation explore what happened near the edge of the observable universe, and measurements of the distribution of galaxies explore what happened much closer to us. The consistency requires that conditions were quite similar across the sky at these two quite different ranges of distance.

My early thinking about homogeneity was influenced by the near isotropy of the CMB and by the strikingly uniform map of radio source angular positions in the Second Cambridge Catalog (figure 3 in Shakeshaft et al. 1955). (At the time, radio galaxies were much better surveyed than optically selected galaxies.) I used to be asked why I kept talking about the indirect evidence from angular distributions when a straightforward question was before us: Do counts of optically selected galaxies (with due correction for evolution) average to a satisfactory approximation to a statistically homogeneous random process at an observable depth? The question is fair (Sylos Labini, Montuori & Pietronero 1998) and is being addressed (Hogg et al. 2005). Maybe further work will surprise us: Maybe there are structures on the scale of the Hubble length that escape detections in low-order correlation functions. But such ideas are best reserved for night thoughts.

I take the liberty to pause to reflect on four examples of the power of ideas in this story. (*a*) Bondi (1952, 1960) pointed out that large-scale homogeneity is an assumption subject to observational test, but he expounded at much greater length on the philosophical appeal of what was then known as the cosmological principle and its value in formulating cosmologies. This cosmological principle has dominated research in cosmology since Einstein (1917). (*b*) Mandelbrot's idea that we ought to pay more attention to roughness, as in fractals, was a stroke of genius and broadly influential. With Bondi, he noted the philosophical appeal of homogeneity for observers in a scale-invariant fractal Universe, which is so contrary to the customary meaning of the cosmological principle. Not all elegant ideas can be right, of course, and observations show our Universe fits Einstein's vision, not Mandelbrot's. (*c*) My professor of continuing education, Bob Dicke, was a conservative experimentalist who laughed at ideas, including some of mine, that had little chance of making contact with measurements, yet he spent much of his career exploring a speculative idea he termed Mach's principle. If, as Mach argued, the material content of the Universe defines inertial motion, which is the idea that led Einstein to argue for a homogeneous Universe, might the Universe have other effects on local physics? Perhaps motion relative to the rest frame defined by the Universe has an effect on laboratory physics in some more subtle way than the velocity of light, maybe local gravity physics. Perhaps cosmic evolution affects laboratory physics, maybe changing the strength of gravity or the electromagnetic interaction. Bob was attracted to this line of thought because it suggests new and maybe informative measurements, which is the point of natural science, and because he shared with Dennis Sciama the philosophy of the *Unity of the Universe* (Sciama 1959). Donald Lynden-Bell (2010, p. 1) began his contribution to this series with "Is space-time only brought into being by its energy content? The jury is still out." Examples of variants of Mach's philosophy are in *Are the Fundamental Constants Varying in Spacetime?* (Molaro & Vangioni 2009). Philosophy this durable can't be all bad. (*d*) An even more durable idea is that the world operates by rules we can discover, in successive approximations to a reality thus defined. We are surrounded by results of this spectacularly productive philosophy. I like the example of homogeneity, a simple result established in a roundabout way. I continue with this line of thought in Section 6.

3. FINDING THE COSMIC MICROWAVE BACKGROUND RADIATION

The 3K thermal cosmic microwave background radiation (the CMB) shows that our Universe expanded from a hot, dense state, just as fossil bones and footprints show that dinosaurs walked the face of Earth. FTBB has recollections of research leading to the CMB. I offer these highlights as another example of research in the nominally exact sciences.

Bob Dicke on occasion wondered aloud what might have been happening before the Universe was expanding. He liked the idea of a bounce from an earlier collapse. So did John Wheeler, who thought of "a glove which is turning itself inside out one finger at a time" (Wheeler 1958, p. 115; this was before singularity theorems). Bob began his proposal to look for the CMB by asking us to consider what would have become of the heavy elements produced in stars in the last cycle of an oscillating Universe. Formation of the atomic nucleus of a heavy element out of hydrogen releases the energy to produce some million starlight photons. Each would be blueshifted during a contraction phase in an oscillating Universe; and if the contraction were deep enough, a few of the blueshifted photons would be energetic enough to photodissociate the heavy nucleus, providing hydrogen to power stars in the next cycle and leaving the rest of the photons to be thermalized, filling the newly expanding Universe with a sea of blackbody radiation. We wrote one paper on this production of entropy, i.e., Dicke & Peebles (1979), but the CMB idea took on its own life, and bounces were largely forgotten.

To explain why the CMB cools as the Universe expands, Bob invited us to imagine placing in the uniform sea of thermal radiation a box that is expanding with the general expansion of the Universe. The box is filled with the sea of radiation and has perfectly reflecting walls inside and out. Photons that were headed into the box are reflected, but they are replaced by the reflection of photons inside the box that were headed out. (The box can be large enough that the boundary conditions at the walls have negligible effect at wavelengths of any interest.) I recall him then saying that we all know the expanding cavity cools the radiation it contains. I expect he also knew that the expansion preserves the thermal spectrum, without the need for a thermalizing agent. One way to see this uses Planck's expression for the mean number of photons in a mode of oscillation of the electromagnetic field in the cavity, $\langle \mathcal{N} \rangle = (e^{hc/kT\lambda} - 1)^{-1}$ at temperature T. The mode wavelength λ expands with the box, and the box expands with the Universe in proportion to the expansion parameter $a(t)$, so $\lambda \propto a$. If the period λ/c is much shorter than the cosmic expansion time, the stretching is adiabatic, so the number $\mathcal{N}$ of photons in a mode is conserved. The radiation wavelength thus scales as $\lambda \propto a$, which is the cosmological redshift, and the mode temperature scales at $T \propto \lambda^{-1} \propto a(t)^{-1}$. Because this temperature scaling applies to all modes, the spectrum remains thermal as the radiation cools. Tolman (1934, p. 427) showed this another way. Interaction with matter can change the CMB spectrum, but the CMB energy density is so large that a significant perturbation to the spectrum is difficult to arrange.

George Gamow found an application. In papers leading up to Alpher, Bethe & Gamow (1948), he developed the idea that the chemical elements were built up by successive neutron captures during the early rapid expansion of a Friedmann-Lemaître Universe. In his Ph.D. dissertation, Gamow's student Ralph Alpher (1948) saw an inconsistency. The condition for appreciable but not excessive production of heavier elements in expansion time t at baryon number density n is $\sigma vnt \sim 1$, where σ is the radiative neutron capture cross section and v is the relative velocity. In relativistic cosmology, the expansion time is $t \sim (G\rho)^{-1/2}$. It was natural to take the mass density to be $\rho = mn$, where m is the baryon mass. These conditions, with measured σv, make t much larger than the neutron half-life. One could imagine neutrons were created at the Big Bang, along with everything else, but where would neutrons have come from so late in the career of the Universe? Gamow (1948) did not acknowledge the problem but proposed a brilliant solution: that the early Universe was so hot that the mass density was dominated by thermal radiation, which would speed the expansion. [Alpher, Bethe & Gamow (1948) had noted another advantage of a hot Universe: Hot neutrons avoid low-energy resonances that would spoil the Gamow-Alpher anticorrelation of σv with element abundance.] Gamow analyzed the first step in the proposed buildup of the elements, which is radiative capture of neutrons by protons to form deuterons, $\mathrm{n}+\mathrm{p} \rightarrow \mathrm{d}+\gamma$. At high temperature, the reverse process, $\mathrm{d}+\gamma \rightarrow \mathrm{n}+\mathrm{p}$, suppresses accumulation of deuterons. When the temperature falls to $T \simeq 10^9$ K, the photon energies become too low for photodissociation, so deuterium can accumulate and burn to heavier elements by faster reactions like $\mathrm{d} + \mathrm{d} \rightarrow {}^3\mathrm{He} + \mathrm{n}$. The temperature $T \simeq 10^9$ K fixes the thermal energy density, which determines the expansion time, $t \sim (G\rho)^{-1/2} \sim 100$ s, and the condition $\sigma vnt \sim 1$ with Gamow's estimate $\sigma v \sim 10^{-20}$ cm^3 s^{-1} gives baryon number density $n \sim 10^{18}$ cm^{-3} at $T \sim 10^9$ K. This with refinements has become the well-tested theory of the origin of helium and deuterium.

Gamow certainly understood that the thermal radiation would remain after element formation. He saw (I think by intuition, not calculation) that the epoch of equal mass densities in matter and radiation marks the onset of the gravitational growth of structure, and he found the temperature then, $\sim 10^3$ K (Gamow 1948). Gamow did not compute the present temperature at his usual estimate of the present mass density, $mn = 10^{-30}$ g cm^{-3}, but the easy exercise gives $T \sim 10$ K. Alpher & Herman (1948) took this bold step. The Dicke et al. (1946) experiment showed $T <$ 20 K. Bernie Burke's judgment in FTBB is that this experiment could have been improved to detect the CMB at $T = 2.7$ K in the 1950s. But it was not attempted until 1964.

The first indirect detection of the CMB is the observation of absorption of starlight from the first rotationally excited level of the interstellar molecule CN. McKellar (1941) converted the ratio of populations in the excited and ground levels to an effective thermal excitation at temperature $T = 2.3$ K. Herzberg (1945, p. 496) wrote that this has "a very restricted meaning." I expect he meant the excitation could be caused by particle collisions rather than radiation. In FTBB, George Field recalls concluding that particle excitation is unlikely because radiative relaxation of CN to the ground level is fast, but he did not publish. Nick Woolf recalls mentioning the CN spin temperature to Dicke when he was considering a search for the CMB, but Bob didn't reply; maybe he didn't understand. Fred Hoyle (1950) understood. In a review of *Theory of Atomic Nucleus and Nuclear Energy-Sources* (Gamow & Critchfield 1949), Hoyle (1950, p. 195) wrote that Gamow's hot Big Bang theory, which is presented in an appendix, would lead to a "temperature of the radiation at present maintained throughout the whole of space much greater than McKellar's determination for some regions within the galaxy" (Hoyle 1950, p. 195). Hoyle recalled Gamow (in about 1956) "explaining his conviction that the universe must have a microwave background" (Hoyle 1981, p. 522) and his telling Gamow that McKellar "set an upper limit of 3 K for any such background" Hoyle also recalled that he "missed it again in exactly the same way in a discussion with Bob Dicke" (Hoyle 1981, p. 522) in 1961. I have seen no evidence that Bob remembered this discussion, or that Hoyle influenced Bob's thinking in some more subtle way, or that anyone noticed that the CN spin temperature might be a signature of a Hot Big Bang until after direct detection of the CMB [at $T = 2.7$ K, which is consistent with McKellar (1941) within the uncertainty].

A second signature is the high cosmic abundance of helium. At a summer school at the University of Michigan, Gamow (1953a) explained that, while element buildup in a Hot Big Bang has problems producing heavier elements, it readily yields helium abundance ~30% by mass, "in good agreement with the observed relative amount of Hydrogen and Helium in the universe" (Gamow 1953b, p. 12). He proved to be right, but I am not sure how convincing those observations were, and Gamow tended not to bother with documentation. Geoff Burbidge and Don Osterbrock were at the summer school. (They are pictured with Gamow in the conference photograph taken by Ed Spiegel and published in figure 1 in Gingerich 1994). Burbidge (1958) later concluded that the galaxy contains at least 10% helium by mass, and that this large amount is difficult to understand at the present rate of nuclear burning. He did not mention a Hot Big Bang. Osterbrock & Rogerson (1961, p. 133) put the helium mass fraction at about 32%, noting that this

> . . . could be at least in part the original abundance of helium from the time the universe formed, for the build-up of elements to helium can be understood without difficulty on the explosive universe picture.

Their reference for the explosive Universe picture was Gamow (1949). Hoyle & Tayler (1964) knew the cosmic helium abundance is high and naturally explained by a Hot Big Bang, or, they pointed out, by many little bangs. The spectrum of the radiation accompanying the process would distinguish them. Tayler recalled that early drafts of Hoyle & Tayler's paper mentioned the thermal radiation in a Hot Big Bang, but "with such a low temperature that it was not surprising that it had not been discovered" (Tayler 1990, p. 372).

In FTBB Yuri Smirnov recalls that Yakov Borisovich Zel'dovich (who is pictured in **Figure 3**) thought that the cosmic helium abundance is low, perhaps 2.5% by mass. Zel'dovich (1963, p. 372) wrote that "We cannot make any estimate of the reliability of these results," but he went on to show how a Cold Big Bang could avoid producing helium: In a degenerate sea of equal number densities of protons, electrons, and neutrinos, the neutrino degeneracy energy would prevent formation of neutrons by inverse beta decay.

Key information may be overlooked, as happened to Zel'dovich, or forgotten. Hoyle & Tayler seem to have forgotten the CN temperature Hoyle mentioned to Dicke, who forgot the conversation. We had to remind Dicke that he already had placed a bound on the CMB temperature, $T < 20$ K (Dicke et al. 1946). In an analysis of the structure of Jupiter, I used the Osterbrock & Rogerson helium abundance in an estimate of the equation of state (Peebles 1964). The explosive Universe picture meant nothing to me then, and I had forgotten about the Osterbrock & Rogerson helium abundance when Dicke suggested I look into theoretical implications of a radiometer search for the CMB.

Thoughts of a Hot Big Bang made me think of an exploding pressure cooker, which made me think of out-of-equilibrium particle reactions and light element formation. I didn't know about Zel'dovich, but I, too, wanted to prepare for a low helium abundance or a null result from the radiometer measurement, which led me to think of effects of degenerate neutrinos. I talked about Hot and Cold Big Bangs in a colloquium at Wesleyan University on December 2, 1964. There is no evidence in my notes that I was aware of Gamow's work. A letter dated February 1, 1965

Figure 3

A photograph I took of Zel'dovich and his wife in Hungary in 1987 on the occasion of IAU Symposium 130. I met Zel'dovich only twice, at this meeting and in 1977 at IAU Symposium 79 in Estonia. I was not eager to travel to the Soviet Union, and Zel'dovich could not travel out of it, so we tended to work in parallel, which can be productive. At the 1987 symposium, the CDM model was generally accepted as a promising framework for analyses of structure formation, and simulations presented at the meeting suggested galaxies were assembled by mergers and accretion at low redshifts. I don't remember what Zel'dovich thought. I was skeptical and still see problems with the successor, ΛCDM, on this score.

to Hoyle and Tayler shows I had learned I was reinventing the wheel. I set out to do a more careful computation, as did Bob Wagoner with Fred Hoyle and Willie Fowler. Bob Wagoner and I compared notes at a December 1965 conference at the Fontainebleau Hotel in Miami Beach, Florida. Not atypical for the time was the fancy setting—we shared conference facilities with the Jewish Funeral Directors of America—and the participation of a good fraction of the cosmology community, apart from the usual absence of people from the USSR. Computation of element buildup in a Hot Big Bang is complicated by the extreme changes of the various nuclear reaction times relative to the cosmic expansion time as the model Universe expands and cools. I learned from Wagoner that Hoyle and Fowler knew about this from analyses of stellar evolution. I relied on carefully checked numerical fixes. I was ready to submit a paper (Peebles 1966) that takes account of the relatively few reactions relevant for helium and deuterium. Wagoner, Fowler & Hoyle (1967) looked at more reactions relevant for trace production of elements heavier than helium.

The evidence reviewed in FTBB is that the first serendipitous detection of the CMB was by communication systems at the Bell Telephone Laboratories. Dave Hogg describes in FTBB the several generations of receiving systems with low-noise traveling wave maser preamplifiers, and he and Arno Penzias and Bob Wilson recall the increasingly awkward problem of reconciling detected and expected system noise. Doroshkevich & Novikov (1964, p. 113) were the first to point out the relevance of the Bell work reported by Ohm (1961) "for experimental checking of the Gamow theory." They were not aware that Bell had an excess noise problem, and the Bell people were not thinking about a sea of microwave radiation.

In 1964, at Bob Dicke's suggestion, Peter Roll and Dave Wilkinson built a Dicke radiometer to look for the CMB. I was the unwitting medium for recognition that Bell had the signal Roll and Wilkinson were seeking. I had asked Dave if I might mention the Roll-Wilkinson experiment in colloquia. He saw no problem: "no one could catch up with us now." At the Applied Physics Laboratory at Johns Hopkins University (where, I later realized, Alpher and Herman were based in 1948), on February 19, 1965, I mentioned the experiment. Ken Turner from Princeton graduate student days attended the colloquium. He recalls in FTBB telling Bernie Burke, who advised Penzias to talk to Dicke. That led to recognition of a new type of cosmic radiation, the CMB (Penzias & Wilson 1965), maybe a fossil from a Hot Big Bang (Dicke et al. 1965). The reaction I recall at Princeton was excitement that there actually is something to detect, as Roll & Wilkinson (1966) did a few months later, maybe from the Hot Big Bang, but at least something to be analyzed. The Nobel Prize Committee was right to recognize Penzias & Wilson: They demonstrated that Bell had a problem, and they complained about it until the community responded. But Dicke should have been included. He invented key technology, and he initiated the experiment that led to the recognition of this most informative fossil from the Big Bang.

Despite Gamow's elegant Hot Big Bang theory, the high spin temperature of interstellar CN, the high cosmic abundance of helium, and the unexplained radiation in the Bell communications systems, the catalyst for recognition of the CMB came from yet another direction: Dicke's idea of entropy from a bouncing Universe. I hope we cosmologists are not exceptionally obtuse. The extenuating circumstance I see is that there were too few actors to encounter each other often enough to be forced to bear in mind many points of view. In this major advance in slow motion, it is easy to see the missteps. I would like to think that if you examined major advances in any other natural science in comparable detail you would see similar confusion.

4. STATISTICAL MEASURES OF COSMIC STRUCTURE

Statistical measures of the variation of the CMB across the sky and of the spatial distribution and motions of the galaxies have become cornerstones of physical cosmology. I can date my interest in this to a colloquium on the newly identified CMB at the University of Toronto on March 17, 1966. While there, Sidney van den Bergh showed me maps of angular distributions of the rich clusters of galaxies in George Abell's (1958) catalog. Sidney pointed out that these maps do not bring to mind a homogeneous world model. I asked if we might be seeing shot noise in a sparse sample. He said I could check that. I agreed. It led me to look into what I pictured as a choppy sea of galaxies, with the vague feeling that measurements might offer clues to why the sea is the way it is.

The autocorrelation or two-point correlation function is broadly useful, and it is natural to apply it to the distribution of galaxies. [I was not aware of it then, but there already was a considerable literature on the galaxy correlation function, including Katz & Mulders (1942); Zwicky (1942); Neyman & Scott (1952); Limber (1954); Rubin (1954); Layzer (1956); Neyman, Scott & Shane (1956); Irvine (1961); Kiang (1967); Kiang & Saslaw (1969); and Totsuji & Kihara (1969).] To me the choice was between that and its transform, a power spectrum. Blackman & Tukey (1959), in *The Measurement of Power Spectra*, explain that estimates of the power spectrum at different wavenumbers may be close to uncorrelated, whereas the correlated noise in a autocorrelation function may be horribly complicated. Tukey (1966, p. 27) put it that "the tragic accident that killed H. R. Seiwell . . . destroyed the only man who could usefully look at a plot of autocorrelation against lag." I had been hearing similar comments in Dicke's group. So I started with power spectra.

We might pause to note that measured galaxy distances are not accurate enough for small-scale mapping of the spatial distribution (except in our immediate neighborhood), so one instead measures angular distributions in samples selected by some measure of distance, such as apparent magnitude, and translates to spatial statistics under the assumption we have a fair sample of a statistically homogeneous and isotropic point process in an imagined statistical ensemble of universes. The two-point spatial and angular correlation functions, $\xi(r)$ and $w(\theta)$, are defined by the probabilities of finding a point in the volume element δV at distance r from a randomly chosen point, and of finding a point in the element of solid angle $\delta\Omega$ at angular distance θ from a point,

$$\delta P_r = n[1 + \xi(r)]\delta V, \quad \delta P_\theta = \mathcal{N}[1 + w(\theta)]\delta\Omega. \tag{1}$$

The mean number densities of points are n per unit volume and $\mathcal{N}$ per steradian. The analog of a Fourier representation for point positions θ_i, ϕ_i on the sky is

$$a_l^m = \sum_i Y_l^m(\theta_i, \phi_i), \quad u_l = \langle |a_l^m|^2 \rangle/\mathcal{N} - 1 = 2\pi\mathcal{N}\int_{-1}^{1} w(\theta)P_l(\cos\theta)d\cos\theta. \tag{2}$$

The second expression is the ensemble average relation between the angular power spectrum u_l and the angular correlation function $w(\theta)$, where P_l is a Legendre polynomial. The distances between zeros of the real and imaginary parts of the spherical harmonic Y_l^m are close to π/l (except near the poles where $Y_l^m \to 0$). This corresponds to the spacing π/k of the zeros of the real and imaginary parts of $e^{i\vec{k}\cdot\vec{r}}$. If a homogeneous and isotropic process is sampled at different effective depths, d, with distributions of distances r that scale as $dP/dr \propto f(r/d)$, as in selection by apparent magnitude, and clustering is measured on scales much less than d, then the angular functions scale with depth as

$$u_l(d) \simeq U(l/d), \quad w(\theta, d) \simeq d^{-1}W(\theta\, d), \tag{3}$$

with $\mathcal{N} \propto d^3$. These scaling relations were of prime importance for a check of statistical homogeneity, and, more important, a test for degradation of the data by systematic errors such as irregular local obscuration or inhomogeneous sampling. The limited part of the sky in an astronomical sample requires a correction to the estimates of u_l and introduces a correlation among the a_l^m. This and more is in a paper (Peebles 1973) with the pretentious subtitle "Paper I," though I don't think I had any idea how many more there might be. (The last is Paper XII, when work in statistics was in directions quite different from Paper I.)

John Tukey was at Princeton, and in November 1972 I presented a seminar for his group. He, Geoff Watson, and the others were encouraging but we agreed that there was no need to collaborate. The complications to be feared were in the astronomical data. As Tukey might have expected, power spectra are the better statistic for the large-scale galaxy distribution and the CMB anisotropy. But I found that the two-point and higher order correlation functions are better suited to the strongly nonlinear clustering of galaxies on scales $\lesssim$10 Mpc. I have to mention Nelson Limber's (1954, p. 656) account of estimating the correlation of Lick galaxy counts:

> The number of nebulae per square degree along [a parallel of latitude] was recorded separately on each of two strips . . . one strip was displaced ϕ degrees with respect to the other, and then the values on the two strips which were adjacent after the displacement were multiplied together, and the mean value of these products were obtained.

Jer Yu and I had the great advantage of programmable computers when we introduced the use of power spectra and spherical harmonic expansions.

4.1. Measuring the Galaxy Spatial Distribution

Jer Yu, my first graduate student, is from Hong Kong, and is back at the City University of Hong Kong. He was an undergraduate at the University of Michigan, where he met then graduate student David Wilkinson. Dave moved to Princeton as an instructor in the Department of Physics in 1963, Jer arrived as a graduate student in 1964, Dave directed him to me, and I directed Jer to the measure u_l of clustering of the Abell clusters. We found significant position correlation (Yu & Peebles 1969), but I worried about possible effects of uneven sampling across the sky. Mike Hauser joined me in a more detailed analysis. He came from particle physics (and went on to play leading roles in the Infrared Astronomical Satellite and as PI of the DIRBE experiment on the COBE satellite). Paper II (Hauser & Peebles 1973) showed that the spectrum estimates for the Abell cluster positions at fixed l and different m have the exponential distribution $P(u_l^m > x) \sim e^{-x/\beta(l)}$ that Jer Yu pointed out follows from the central limit theorem. We better understood the scaling relations (Equation 3), and we found the predicted scaling of u_l and $w(\theta)$ with limiting Abell distance class. This evidence that our statistics were reliable measures seems trivial now, but it was really encouraging then.

At the Lick Observatory in California, Donald Shane led a major survey of the large-scale galaxy distribution (well described by Vasilevskis & Osterbrock 1989). Shane and Carl Wirtanen, with help from others, counted 1.2 million galaxies (some more than once where fields overlap; there are 800,000 different galaxies) brighter than apparent magnitude $m_{pg} \simeq 19$ and north of declination $\delta = -23°$. Galaxies were identified by eye using a traveling microscope to scan 17-inch square photographic plates, each covering a $6° \times 6°$ field. The counts in each of the 1,296 10 arcmin $\times$ 10 arcmin cells in each field were recorded by hand. A sample data sheet is shown in **Supplemental Figure 1** (follow the **Supplemental Material link** from the Annual Reviews home page at **http://www.annualreviews.org**). The counts commenced in late 1947 and were completed in 1959. Donald is on my list of heroic figures.

Supplemental Material

Shane & Wirtanen (1967) published the counts summed in $1° \times 1°$ cells, and Totsuji & Kihara (1969) used these data to show that the galaxy spatial two-point function is close to a power law,

$$\xi = (r_o/r)^\gamma, \tag{4}$$

with $\gamma = 1.8$ and $r_o \sim 5$ Mpc. We (Peebles & Hauser 1974, Paper III) had a consistent result and could add a test because we had Zwicky's catalog (Zwicky, Herzog & Wild 1961–68) of brighter, generally closer galaxies. The angular function $w(\theta)$ is much smaller in Lick than Zwicky, because the deeper Lick counts sum through more clumps along the line of sight, averaging out angular fluctuations. The difference agrees with the scaling relations (Equation 3), again a key check that Equation 4 is not corrupted by systematic error.

I was eager to try analyzing the unpublished 10-arcmin data. A trip to the Shanes' home in a beautiful grove of second-growth redwoods near the University of California, Santa Cruz, on October 27, 1972, rewarded me with Mary's and Donald's generous hospitality and microfilm copies of more than 1,000 data sheets, one for each $6° \times 6°$ field. I needed help—I wrote to Donald that "I am not sure we could afford to handle that many numbers"—and it materialized. Jim Fry, Ed Groth, Mike Seldner, Bernie Siebers, and Ray Soneira (some of whom are pictured with Donald Shane in **Figure 4**) showed me that the university's computer was adequate (though digitizing the data on IBM punch cards by a commercial service, checking the sum of digitized counts in the 36 10-arcmin cells in each 1° block against the published 1° count, and analyzing

Figure 4

A photograph I took of Donald Shane when he visited Princeton in about 1976. Some of the people who worked on the Lick data reduction are Jim Fry (*center left*), Ray Soneira (*center right*), and Mike Seldner (*far right*).

the data took four years). Despite astronomers' tendencies to enter the occasional inscrutable dot or colon, we were happy with the data. Our computer was modest compared to what is on my desk now, but far superior to when the counts commenced. Donald could have had no reason to expect the 10-arcmin data ever would be fully analyzed, but he had the courage to take great care, including the valuable allowance of generous plate overlaps, 1° strips on the four sides of each 6° × 6° field. The independent counts of the same parts of the sky by different observers, at different times, and maybe different plate emulsions enabled detailed checks for systematics [as discussed by Shane & Wirtanen (1967); in more detail by Seldner et al. (1977); and still further in Groth & Peebles 1986a,b)]. The offer by Seldner et al. (1977, p. 253) of the raw and corrected data "on receipt at Princeton of an IBM compatible 2,400-ft magnetic tape" pleased Donald: He considered the Lick data published at last (though now the several generations of storage of these data are obsolete).

I earlier made maps from Zwicky's catalog of nearby galaxies and sent him copies. I received a polite reply from a great but not always happy scientist. I used a mechanical plotter driven by the university's computer (a big operation then) to represent the Lick 1° counts as squares with sizes proportional to the counts, and Alison and I sat at our dining room table to black in the squares. This map is the frontispiece of *The Large-Scale Structure of the Universe* (Peebles 1980). **Supplemental Figure 2** is a better copy. The 10-arcmin Lick maps were made by a film scanner at the Princeton Observatory that was acquired and modified by John Lowrance to read digital data as well as plot it. This was part of Lyman Spitzer's goal of a space telescope. Heiles & Jenkins (1976) describe the scanner and show its application to astronomical data. **Supplemental Figure 3** shows improved digital versions of the 10-arcmin Lick maps.

It was a pleasure to bring Donald the 10-arcmin Lick maps in 1976 and ask, Is this what you saw? I believe I have an accurate memory of his answer: "I was looking at this one galaxy at a time." Stewart Brand, editor of the *Whole Earth Catalog* in its many editions, contacted me with the proposal to publish the Lick map as a poster. I never thought to ask how he knew about the map. The poster was published in 1978. We received no royalties, but enjoyed seeing this elegantly printed poster taped to office walls.

ΛCDM: CDM adjusted to include a cosmological constant that allows lower mean mass density while keeping flat space sections

It was good also to find a positive scaling check of consistency of the two-point functions from the 10-arcmin Lick counts, from the 10,000 galaxies cataloged in a single, much deeper 6° × 6° field by Rudnicki et al. (1973), and from the shallower Zwicky sample (figures 13 and 14 of Groth & Peebles 1977). We had moved on to the three-point function (Peebles & Groth 1975; while I enjoyed the hospitality of UC Berkeley), which was harder to measure, and the scaling check was less detailed but still meaningful. Jim Fry worked through the four-point function (Fry & Peebles 1978). No one volunteered for the five-point function. At the range of galaxy separations we could measure, $0.03 \lesssim r \lesssim 10$ Mpc, the galaxy two-point function $\xi(r)$ is close to the power law in Equation 4, and the three- and four-point functions are well approximated by products of two-point functions in the form of a fractal with dimension $D = 3 - \gamma = 1.23$.

Ray Soneira and I checked this picture by making model galaxy distributions to compare to the Lick map. We placed galaxies in randomly placed fractals of dimension $D = 1.23$ with bounded size and a spread of richnesses (constrained by the two-, three-, and four-point functions), and used a galaxy luminosity function to project the model distributions onto the sky at the depth of the Lick catalog. We at first used fractals far broader than 10 Mpc, meaning the power-law forms of the correlation functions extend to a large separation, which I supposed wouldn't matter because the correlation functions at large separations are small. The maps showed I was wrong: They were, in Ray's term, "blotchy." This is because the mean number of model galaxies in excess of random within distance r of a randomly chosen galaxy scales as $\int_0^r \xi(r) d^3r \propto r^D = r^{1.23}$. If the power law continues to large r it makes this excess large, placing the galaxies in disgusting blotches. I checked that by cutting a model map into squares of size ~5° (linear size ~20 Mpc) and gluing the squares onto a sheet of paper at random. That removed the large-scale correlation and made a more pleasing map. A two-point function $\xi(r) \propto r^{-\gamma}$ at $r < 20$ Mpc truncated to $\xi(r) = 0$ at $r \geq 20$ Mpc produces an angular function that slowly bends away from a power law, contrary to the measurements. To remedy that we made $\xi(r)$ rise above the power law at $r \sim 10$ Mpc and then fall below it at $r \sim 20$ Mpc, producing a shoulder. The functional form in figure 6 of Soneira & Peebles (1978) shares these features with modern measurements (Zehavi et al. 2004).

Our argument for the existence of the shoulder in $\xi(r)$ was sound, I believe, but too qualitative to have much chance of being influential. I nevertheless count it as an example of how qualitative impressions can complement objective measures. A more ambiguous example is my impression that linear features in the Lick Map may only illustrate the tendency to see patterns in noise. Indeed, our model map presents linear features that are in the eye of the beholder; they were not built in. I don't know whether some statistical measure could have deduced the existence of walls of galaxies from the Lick data: recall the danger of designing a statistic to respond to what you have seen. The frothy character of the galaxy distribution seen in dense redshift surveys (Davis et al. 1982; de Lapparent, Geller & Huchra 1986) is a qualitative impression, and real.

Qualitative impressions can inspire useful quantitative models. Jerzy Neyman and Elisabeth Scott proposed to use the galaxy N-point correlation functions to constrain a model that in its original version (Neyman & Scott 1952) places all galaxies in clusters (now termed halos). The number of halo members is a random variable, the position of each galaxy within its halo is a universal random variable, and the halos are placed at random. The modern halo occupation distribution (HOD) framework generalizes this: The halo mass is a random variable matched to the dark matter halo mass function in the ΛCDM cosmology (discussed in Section 5); the number of galaxies in a halo and their luminosities, morphologies, radial distributions, and motions are random variables that depend on the halo mass; and the halo space distribution is modeled on the ΛCDM dark matter power spectrum (Cooray & Sheth 2002, Berlind & Weinberg 2002).

The successful HOD interpretation of the galaxy two-point correlation function $\xi(r)$, from pairs of galaxies in the same halo and in different halos, and the variation of the two-point function with galaxy luminosity and redshift, establish HOD as a useful representation (Zehavi et al. 2004; Conroy, Wechsler & Kravtsov 2006). There is no arguing with success, but qualitative impressions tell me there is more to the story. Neyman, Scott & Shane (1956, p. 96) wrote that,

> Because of the observations, originally made by Shapley . . . and more fully confirmed by Shane and Wirtanen . . . that the clusters of galaxies tend to congregate in small groups, it appears interesting to deduce formulas allowing for the possibility of "superclustering" . . . .

Einstein-de Sitter (EdeS) model: relativistic Friedman-Lemaître cosmological model with no space curvature and zero cosmological constant

The *Nearby Galaxies Atlas* (Tully & Fisher 1987) beautifully illustrates the local situation. We are in a distinctly nonlinear flat concentration of galaxies, the supergalactic plane. In addition to the galaxies around the Milky Way and M31 in the Local Group, the supergalactic plane contains the M81 group, the galaxies around Centaurus A, and the more massive concentrations around Maffei and IC342. Just above the supergalactic plane is a very empty part of the Local Void (illustrated in figure 1 of Peebles & Nusser 2010). And, with Peter Shaver (1991), I expect it is significant that the nearest six Abell clusters are close to the supergalactic plane (figure 3.7 of Peebles 1993). This hierarchy of nonlinear structures includes the supergalactic plane with diameter ∼100 Mpc that contains groups and clusters, some of which contain substructures, that contain the structures observed on scales ∼10 kpc as galaxies. I doubt this range of nonlinear structures can be accommodated within HOD; it speaks of some version of the clustering hierarchies Neyman, Scott, Shane, Soneira, and I modeled. The issue is recognized in a modified HOD that places galaxies in subclusters in clusters (Sheth & Jain 2003, Giocoli et al. 2010). I expect HOD will continue to evolve into some kind of clustering hierarchy over the considerable range of scales of structures seen in our neighborhood, maybe to be constrained by the higher order correlation functions Neyman, Scott, and I so valued.

In the early days, before establishment of the gravitational instability picture for cosmic structure formation that inspired HOD, I liked to point out that if structure formed by explosions that pushed matter into piles, moving matter by distances $\sim R$, it would make $\xi(r)$ negative at $r \sim R$. The inverse square law of gravity allows gravitational growth of mass concentrations that nowhere lowers the density (in the ensemble average), so it need not drive $\xi(r)$ negative anywhere, in line with what we were finding (Peebles 1974a). This is no surprise now, but it was interesting then.

I also liked the evidence that the small-scale, nonlinear clustering of galaxies has N-point correlation functions that are close to scale-invariant power laws. Classical gravity has no scale. Among the Friedman-Lemaître models, Einstein-de Sitter (hereafter EdeS) has no scale. What could be more natural than scale-invariant clustering of matter in a scale-invariant Universe (Peebles 1974a,b; Davis, Groth & Peebles 1977)? Scales are built in by the interaction of matter and radiation in the early Universe (Section 5) and by the transition from linear fluctuations on large scales to small-scale nonlinear clustering, but the thought was that fully developed nonlinear clustering may forget initial conditions, in the manner of fully developed turbulence. Nature had other ideas.

Recall that the expansion rate in the relativistic Friedman-Lemaître models satisfies

$$\left(\frac{\dot{a}}{a}\right)^2 = H_o^2\left[\Omega_m\left(\frac{a_o}{a}\right)^3 + \Omega_k\left(\frac{a_o}{a}\right)^2 + \Omega_\Lambda\right], \quad \Omega_m + \Omega_k + \Omega_\Lambda = 1. \tag{5}$$

Hubble's constant is H_o, the mean separation between galaxies scales as $a(t)$, with present value a_o, the constant Ω_m is a measure of the mean mass density in matter (radiation may be ignored here), Ω_k represents the effect of space curvature, and Ω_Λ represents Einstein's cosmological constant, Λ.

EdeS has no space curvature and no cosmological constant ($\Omega_k = \Omega_\Lambda = 0$, $\Omega_m = 1$). Because there is no scale, the EdeS model expands as a power of time, $a(t) \propto t^{2/3}$. Bondi (1960, p. 166) remarked that the "outstanding simplicity" of EdeS "deserves attention." Perhaps he meant that if Ω_k or Ω_Λ were appreciably different from zero we would flourish at a special time, during a shift from one dominant term to another in Equation 5. We used to talk about the inelegance of such a coincidence (e.g., Dicke & Peebles 1979). I was also much taken by the elegant match of the scale-invariant EdeS cosmology to scale-invariant clustering. Developments reviewed next led me to abandon EdeS as it was becoming the community standard, which was later replaced by a now well-established model with a cosmological constant $\Lambda > 0$ (Section 5) that predicts that the mass two-point function is distinctly different from the galaxy power law that pleased me. Coincidences happen: We must learn to live with Λ, and maybe with galaxy clustering that is only a transient, accidentally excellent approximation to a power law (Jenkins et al. 1998; Watson, Berlind & Zentner 2011).

4.2. Statistics in Redshift Space and the Mean Mass Density

The redshift z of a galaxy at distance d, expressed as a radial velocity, is $cz = H_o d + v_p$. Exactly homogeneous and isotropic expansion produces recession velocity $H_o d$, and v_p is the radial component of the peculiar velocity relative to this ideal flow. If we can ignore effects of nongravitational stresses such as explosions, we can deduce masses from peculiar velocities and compare the mean mass density to cosmological models.

The mass density parameter (Equation 5) is well measured now: $\Omega_m = 0.275 \pm 0.015$ (Komatsu et al. 2011). Gott et al. (1974) give a good picture of what was known a quarter century earlier, largely from relative peculiar motions in pairs, groups, and clusters of galaxies. Their estimate, $\Omega_m \sim 0.06 \pm 0.02$, is well below what I was expecting from EdeS, but there was evidence of mass outside the visible parts of galaxies (Ostriker & Peebles 1973; Ostriker, Peebles & Yahil 1974; Einasto et al. 1974; Einasto, Kaasik & Saar 1974), maybe enough for $\Omega_m = 1$.

Bill Irvine (1961) took the first step toward a statistical probe of this dark mass. He derived an elegant cosmic energy equation (CEE) relating the mean square peculiar velocity $\langle v^2 \rangle$ to the mass two-point correlation function $\xi_{\rho\rho}(r)$ under the assumption that matter interacts only by gravity. Michael Fall (1975) applied CEE to our estimates of the galaxy function $\xi(r)$ and concluded that if galaxies trace mass, $\xi_{\rho\rho}(r) \simeq \xi(r)$, then either Ω_m is well below unity or the galaxy velocity dispersion $\langle v^2 \rangle$ is well above usual estimates. The latter is hard to check because it is difficult to measure velocities relative to the cosmic mean. The mean square relative velocity $\sigma(r)^2$ of pairs of galaxies as a function of separation r of a few megaparsecs can be much more reliably measured, and the gravitational acceleration that determines $\sigma(r)$ at dynamical equilibrium depends on the two- and three-point mass functions on length scales comparable to r, which can be measured provided galaxies are useful tracers of mass. Margaret Geller and I introduced the ideas for this statistical measure (Geller & Peebles 1973), and I worked them into a cosmic virial theorem (CVT) relating $\sigma(r)$ to an integral over the two- and three-point mass functions (Peebles 1976). My application to 422 nearby galaxies with redshifts compiled by Gérard and Antoinette de Vaucouleurs suggested $\Omega_m \sim 0.05$ to 0.5, which I "presented only to complete the example. It is hard to judge whether they are even reasonable limits" (Peebles 1976, p. 17). Davis, Geller & Huchra (1978) added to the de Vaucouleurs' sample and applied the data to CEE and CVT. They too kept the conclusion modest: "[W]e feel our results are inconsistent with $\Omega < 0.1$" (Davis, Geller & Huchra 1978, p. 18). Herb Rood (1980, 1982) also added to the data and improved the then important correction to $\sigma(r)^2$ for measurement error. That yielded pretty clear support for my guess that the distribution of relative line-of-sight peculiar velocities is exponential, and it indicated $\Omega_m = 0.60 \pm 0.25$ (Peebles 1981a).

These early analyses used galaxies selected by apparent magnitude. It is better to select to fixed minimum luminosity within a volume large enough to approach a fair sample. Kirshner, Oemler & Schechter (1978) took a first step in this direction in samples of eight deeper fields, from which I found $\Omega_m = 0.4 \pm 0.2$ (Peebles 1979). On p. 734, I entered an enthusiastic conclusion:

CfA sample: the pioneering redshift catalog obtained at the CfA

> It is remarkable that a sample of just 166 galaxies gives . . . such a clear presentation of peculiar redshifts. This suggests that a considerable improvement of the situation should come fairly soon, with the larger "random" redshift surveys now in progress.

I was thinking of Marc Davis, who was well aware of the value of a better redshift survey from life as a graduate student in the Princeton gravity group. He moved to Harvard where he saw that the Harvard-Smithsonian Center for Astrophysics (CfA) had the needed facilities. The CfA sample (Davis et al. 1982) gave $\Omega_m = 0.2e^{\pm 0.4}$ (Davis & Peebles 1983). There are much better data now—the two-point correlation function in redshift space in our figure 4 is a crude approximation to the beautiful result found by Peacock et al. (2001)—but the situation was clear: Either Ω_m is well below unity or mass is less strongly clustered than galaxies.

Reactions to this result were conditioned by the inflation picture (Guth 1981), which offers an elegant explanation of large-scale homogeneity: Early rapid expansion driven by a temporarily very large Λ (as ideas in particle physics suggest is not unreasonable) could stretch wrinkles to scales too large to see, and stretch away space curvature in the process. Cosmological space curvature thus fell out of vogue. Because Einstein's cosmological constant in the present Universe never seemed reasonable to many, EdeS ($\Lambda = 0$, $\Omega_m = 1$) was promoted to a general community standard. But if $\Omega_m = 1$, then dynamical mass estimates must be biased low. The generally accepted idea was that this is because galaxy formation was more efficient where the mass density was higher, leaving low-density regions with few galaxies and a lot of mass that would not be counted in the mass concentrated around galaxies. This biasing idea had to be explored, but it had a problem from the start. Galaxies that did manage to form in supposedly inhospitable low-density regions would be predicted to bear the stigmata of a troubled youth, tending to be small and irregular. That is not what the CfA sample maps show [see figure 2a and 2d of Davis et al. (1982)]. It should be noted that the rare most luminous cD galaxies are more strongly clustered, along with the rich clusters of galaxies where they are found, but recent data confirm that normal galaxies, such as the Milky Way, and fainter ones, have quite similar distributions (Peebles 2001) and are useful mass tracers. Related to the old biasing picture is the idea that galaxies are affected by environment. Numerical simulations indicate this and, indeed, cDs prefer dense environments, whereas gas-rich galaxies avoid them. But this line of thought does not so readily fit the observations that properties of elliptical galaxies are remarkably insensitive to environment (Bernardi et al. 2006; Park et al. 2007; Nair, van den Bergh & Abraham 2011) and that pure disk galaxies seem not to have interacted with their environment at all (Kormendy et al. 2010)—fascinating puzzles for future work.

My reading of the evidence from the CfA sample maps along with other arguments for $\Omega_m < 1$ is presented in Peebles (1986). I heard complaints that I only did it to annoy. I was serious, but I have to admit that I enjoyed commenting on the struggle to save EdeS from all the contrary evidence.

Trust in ideas, as in the compelling logic of EdeS, has a solid historical basis—consider that general relativity passes a tight network of tests on cosmological length scales some 14 orders of magnitude larger than the scale of Einstein's test from the orbit of Mercury—as well as a history of unintended consequences. I offer personal examples. Mike Seldner and I compared the measured cluster-galaxy cross correlation function ξ_{cg} to the cluster-mass function $\xi_{c\rho}$ modeled as a limiting isothermal sphere normalized to cluster galaxy velocity dispersions. We assumed $\xi_{c\rho} \simeq \xi_{cg}$, and matched the two at radius ~3 Mpc, where the mix of morphological types is fairly close to the field. The result, $\Omega_m = 0.69 \pm 0.11$ (Seldner & Peebles 1977), might be expected to be biased high because of the high ratio of mass to starlight in clusters. Indeed, it is on the high side of our other early estimates and below unity. But before the CfA sample changed my mind, I liked the EdeS scale-invariance and told myself the uncertainties in these measurements are hard to assess. I had earlier invited Marc Davis to join in a dynamical analysis of galaxy clustering using theoretical methods for nonideal gases [following Bill Saslaw (1972)]. The thought was that galaxy clustering may be simple enough to analyze, and the forms of the galaxy N-point functions suggested an elegant closure *ansatz* for the moments of the distribution of particle positions and velocities (Davis & Peebles 1977). Marc spent a lot of time on this, but the approach did not enter mainstream cosmology. People still are working on such lines, which is good: We ought to piece together the physical considerations that account for phenomena. I am reminded that Marc's doctoral dissertation, with Dave Wilkinson and Mike Hauser, was a search for the "primeval galaxies" Bruce Partridge and I proposed (Partridge & Peebles 1967, Davis & Wilkinson 1974). This was good science too, though again maybe a quarter century ahead of its time. The consequences of these premature ideas may have slowed Marc's career. But maybe they helped too: He went on to do many things to shape cosmology.

5. STRUCTURE FORMATION AND THE COSMOLOGICAL TESTS

Large-scale structure enters the network of tests that eventually established the present cosmology because structure grew by gravity out of initial conditions that on large scales can be deduced from the observations and distinguished from signatures of what happened as the Universe expanded. This comes about because classical gravity has no length scale, which tells us that, absent nongravitational effects such as pressures that define scales, small departures $\delta\rho(\vec{x}, t)$ from a homogeneous mass density $\rho(t)$ have to evolve as a power of time. A calculation gives

$$\frac{\delta\rho}{\rho} \propto t^{2/3}, \quad \phi \sim \left(\frac{a x_p}{t}\right)^2 \frac{\delta\rho}{\rho} \sim \text{constant}, \tag{6}$$

where ϕ is a measure of the perturbation to space-time curvature, x_p is the coordinate size of a region with mass density $\rho + \delta\rho$, the physical size is $a x_p$, and the time of expansion from very high density is t. This is very different from the exponential growth of irregularities in unstable laminar fluid flow that develop into turbulence that loses memory of initial conditions. It also is very different from structure formation dominated by stresses such as explosions or cosmic strings [as reviewed by Peebles & Silk (1990)]. In these scenarios, the study of large-scale structure would have instructed us on the nature of the process that produced it, not cosmology.

Lifshitz (1946) found Equation 6 and more that was rediscovered many times and now is standard lore. His summary, "we can apparently conclude that gravitational instability is not the source of condensation of matter into separate nebulae" (Lifshitz 1946, p. 116) caused confusion, which need not be documented, but calls for three comments. First, Lifshitz computed in linear perturbation theory, meaning he assumed $|\phi| \ll 1$. Under this assumption $\delta\rho/\rho$ has to be small when $a x_p \gtrsim t$. Measurements of weak gravitational lensing now show space-time curvature fluctuations on large scales are small, $|\phi| \ll 1$, as Lifshitz assumed, so theory and observation do not allow a history of large mass density fluctuations on scales $\gtrsim t$ (apart from the decaying mode that makes little physical sense). Second, classical general relativity cannot explain why the Universe is close to homogeneous or why there are departures from homogeneity: It can only predict evolution of what is encoded in initial conditions (Peebles 1967). Third, the Universe is gravitationally unstable: An initially quite smooth mass distribution grows clumpy. Lifshitz made this point, as did Lemaître (1933). All this is commonplace now, but there was a time when serious people did not agree that "the remarkable uniformity of the Universe must await a deeper theory, perhaps a quantized gravity theory" (Peebles 1972, p. 58). I made my first numerical simulations to illustrate this gravitational instability at the Los Alamos National Laboratory in 1969. Because I was an alien (Canadian), I was not allowed to be alone with computers I suppose were used for classified research, but I was allowed to use them, which may seem as archaic now as the 300 particles in the simulations. **Supplemental Figure 4** is a somewhat later illustration of the evolution of our Universe from order to chaos. I used to enjoy making this point in popular and technical lectures.

Supplemental Material

Recognition of the CMB (Section 3) suggested that matter in the nearly homogeneous early Universe was thermally ionized and recombined (or combined) to largely neutral atomic hydrogen and helium when the temperature fell to about 3,000 K. Before that, scattering of the radiation by free electrons causes plasma and radiation to behave as a fluid, and the radiation pressure prevents formation of bound nonrelativistic concentrations of matter (or, as we now have to say, concentrations of baryons) until matter recombines and decouples from the CMB (Peebles 1965). Residual free electrons after recombination interact with the CMB to keep the baryons warm, and the matter temperature fixes the Jeans mass of the first generation of bound baryonic systems. Free electrons also catalyze formation of molecular hydrogen ($e + \mathrm{H} \rightarrow \mathrm{H}^- + \gamma$, $\mathrm{H}^- + \mathrm{H} \rightarrow \mathrm{H}_2 + e$), and radiation by H_2 can cause the Jeans mass to shrink, maybe to a stellar mass. The situation has been analyzed (e.g., Wise et al. 2012) in far more detail than in our early calculation (Peebles & Dicke 1968), but we knew enough then for clear motivation for computing recombination. (I still wonder whether structure formation really failed to leave a signature of the primeval Jeans mass, so tantalizingly in the range of masses of globular star clusters.)

A hydrogen atom forms when a proton captures an electron, in the process releasing an ionizing photon or a Lyman-α or other resonance photon. An ionizing photon almost inevitably ionizes another atom. Resonance photons place atoms in excited levels that are more easily thermally ionized, but some Lyman-α photons are redshifted out of resonance, and others are destroyed by two-photon decay from the 2*s* level. Rashid Sunyaev recalls in FTBB a conversation in September 1966 that was the first step by him and colleagues in the Soviet Union toward the computation of cosmological recombination (Zel'dovich, Kurt & Sunyaev 1968). My first step was a conversation with Bruce Partridge in the spring of 1966, followed by a summer project for Russell Hensel, who had just graduated from Princeton University with a major in physics. I completed the computation in the fall of 1967 (Peebles 1968). This close timing of independent work in the US and USSR is not surprising. Both groups knew the relevance for cosmic evolution.

There were even more independent analyses of the evolution of fluctuations in the distributions of plasma and radiation approximated as a viscous fluid. Sunyaev in FTBB recalls the approach taken in the Soviet Union. Richard Michie had worked out main elements of the physics by 1967 (Michie 1969), but illness prevented his work from getting past the preprint stage. Joe Silk recalls in FTBB how he came to this problem. He published (Silk 1967). I worked it out too, and presented it at the January 1967 *Texas Symposium on Relativistic Astrophysics* in New York City. Turmoil at the publishing house prevented publication (but inclusion here as **Supplemental Text 1** is publication of sorts).

I took the next step, the radiative transfer analysis needed for a more complete computation of the residual patterns in the distributions of matter and radiation, while on sabbatical leave at Caltech in 1968–1969. Jer Yu, whose earlier work with me is recalled in Section 4.1, joined in the numerical solutions (Peebles & Yu 1970). We derived what is now termed the baryon acoustic oscillations in the galaxy power spectrum. The corresponding peak in the galaxy correlation function is in Peebles (1981b, figure 5). Both are quite different from what is measured (Percival et al. 2001, Eisenstein et al. 2005), of course, because we did not take account of nonbaryonic dark matter. Jer and I gave an awkward representation of the relic CMB anisotropy, and our numerical methods were archaic, but we were close to the standard physics of analysis of large-scale structure in the distributions of matter and the CMB temperature. I did not continue with this, in part because I had trouble imagining that such tiny disturbances to the CMB could be detected and in part because of recurring indications that radiation energy released during structure formation may have caused much larger disturbances to the distributions of matter and radiation. As Dick Bond (1990) put it (on page 51),

CDM: EdeS with mass dominated by nonbaryonic cold dark matter and scale-invariant adiabatic initial conditions

> On fairly broad theoretical grounds we expect backgrounds such as the submillimetre excess found by the Berkeley-Nagoya team (Matsumoto et al. 1988) to arise. Although current observations allow large energy releases, the surprising part of the BN data is its apparent magnitude.

Bond went on to discuss how energy releases could disturb the spectrum of the radiation and its spatial distribution, a messy situation. I preferred to think about something else and saw ready work in measures of the galaxy distribution and motions (Section 4). It was a better subject for me; I like the instant gratification of data.

I returned to the CMB anisotropy when two groups announced detection of a quadrupole $\delta T/T \sim 10^{-4}$ (Fabbri et al. 1980; Boughn, Cheng & Wilkinson 1981). I dutifully produced a theory to fit the measurements (Peebles 1981c). The Princeton group soon withdrew their result and lowered the possible anisotropy. I responded with a theory that predicts anisotropy below their new upper limit (Peebles 1982). This model assumes the mass of the Universe is dominated by a gas of particles that interact weakly, if at all, with baryonic matter and radiation. These dark matter particles have small primeval velocities, hence the eventual name, cold dark matter, and the model name, CDM. The CDM model grew out of the proposal that the mass of the Universe is dominated by a massive neutrino family (e.g., Doroshkevich et al. 1981). That soon led to many dark matter candidates offered by ideas in particle physics (e.g., Bond, Szalay & Turner 1982; Blumenthal, Pagels & Primack 1982; Abbott & Sikivie 1983; Preskill, Wise & Wilczek 1983). Because cold dark matter would not be dragged by radiation, its distribution could have been growing clumpy while the baryons were still ionized and coupled to the CMB. This is a big help in reconciling the small CMB anisotropy with the present very clumpy matter distribution. I assumed the primeval mass fluctuations produce curvature fluctuations (in the sense of Equation 6) that diverge only as the logarithm of the length scale. It was known then that an approximation to this spectrum is naturally produced by inflation, but I was more taken by scale invariance (Harrison 1970, Peebles & Yu 1970, Zel'dovich 1972). I first used EdeS, but the CfA sample led me to work out the result of going to lower mass density with a cosmological constant to allow flat space sections (Peebles 1984). I computed only the quadrupole CMB anisotropy in the CDM model, to compare to the measured upper bound, and estimated the anisotropy in the low-density version by scaling from CDM. Kofman & Starobinskii (1985) did that better. This low-density version came to be known as the ΛCDM model.

I liked CDM, but distrusted its early use in analyses of structure formation. I had just made up CDM to save the gravitational instability picture, which didn't make it right, and I set out to show that by inventing other viable models. I gave up (a last example is Peebles 1999) because the rapidly advancing observations were ruling out my models as fast as I could put them on astro-ph, and they were agreeing wonderfully well with ΛCDM. Earlier than that, the beautiful measurements by Mather et al. (1990) and Gush, Halpern & Wishnow (1990) had shown that the

CMB spectrum is very close to thermal, removing the worry about serious perturbations to the CMB by explosions. Still earlier, Bond & Efstathiou (1987) were willing to put aside my worries and compute. Their CMB anisotropy spectrum looks much like modern measurements. Sometimes it pays to prefer ideas to observations (though observations trumped ideas in the addition of the previously unpopular cosmological constant).

The CMB temperature anisotropy is part of the network of evidence that now establishes ΛCDM as a very successful approximation. In the 1990s, there were other models of structure formation to consider (Peebles & Silk 1990), there were arguments for EdeS and for a lower density Friedman-Lemaître solution, and there was the less widely discussed but very relevant issue of reliability of the extrapolation of general relativity to the enormous scales of cosmology. The conference *Cosmic Velocity Fields* gives a fair sample of the arguments for the EdeS mass density that led to Sandy Faber's summary remark that "... Ω might actually be close to 1" (Bouchet & Lachièze-Rey 1993, p. 491). But this was work in progress, and it is worth listing the growth of a considerable variety of evidence for lower Ω_m and ΛCDM. Sandage (1961) suspected the expansion time in EdeS may be shorter than stellar evolution ages of the oldest stars, a problem that could be avoided if $\Lambda > 0$ with $\Omega_m < 1$. This gained weight (Gunn & Tinsley 1975; Turner, Steigman & Krauss 1984; Chaboyer et al. 1996) with advances in stellar evolution theory and measures of the cosmic distance scale (leading to Freedman et al. 2001). Sandage (1961) concluded that the relation between redshifts and apparent magnitudes of distant objects is a promising cosmological test. He had in mind galaxies. The successful application to supernovae indicates $\Omega_m < 1$ and $\Lambda > 0$ (Riess et al. 1998, Perlmutter et al. 1999). Other early evidence for $\Omega_m < 1$ includes the evolution of cluster galaxy velocity dispersions (Bahcall, Fan & Cen 1997) and intracluster plasma temperatures (Henry 1997), the cluster plasma mass fraction (White et al. 1993), and the relative velocities of galaxies (Davis & Peebles 1983; Shaya, Peebles & Tully 1995). Assuming some variant of the CDM model, measures of the maximum extent of correlated structure could be interpreted as $\Omega_m < 1$ with open space curvature (Blumenthal, Dekel & Primack 1988), perhaps with a power-law tilt of the initial conditions (Vittorio, Matarrese & Lucchin 1988) or ΛCDM (Silk & Vittorio 1987; Efstathiou, Sutherland & Maddox 1990). This constraint also allows EdeS with a feature in the initial conditions that happens to mimic scale-invariant initial conditions in a low-density Universe (Efstathiou, Bond & White 1992), but we are saved from that ugly prospect by all the evidence that the mass density is less than EdeS. By the end of the decade, the first peak of the CMB temperature anisotropy spectrum was well identified (figure 2 of Miller et al. 1999). Fitting the peak to CDM with EdeS requires an absurdly long distance scale, whereas the fit to ΛCDM with low Ω_m agrees with the astronomers' distance scale. These last two constraints assume variants of the CDM model and help establish this structure formation model and the relativistic cosmology. There are more tests now, and they are tighter (as reviewed in FTBB), but these examples make the key point. Observations of a broad variety of phenomena allow cross-checks of reliability of the measurements and searching tests of the cosmology by looking at the Universe in many independent ways. This means that if there is a better cosmology than ΛCDM, it will predict a Universe that looks much like ΛCDM.

6. FUTURE DIRECTIONS

Cosmology has grown far beyond anything I, or I suppose anyone, dreamed a half century ago, but it remains incomplete. Recall Lifshitz (1946, p. 116): "... gravitational instability is not the source of condensation of matter into separate nebulae." If early Universe studies prosper to the point of substantiating inflation or some other theory, it may inform us of Lifshitz's source of condensation, but I expect the theory to remain incomplete in Lifshitz's sense: It will require hypotheses about still deeper physics. Maybe there is a final theory in Weinberg's (1992) sense, and from it an effective theory for cosmology; but if so I imagine deriving that effective theory will require approximations to be debated by theorists and tested by observers. Thus I suspect it will never be known whether cosmology is being played with a full deck, with all the physics relevant for all that could be measured. But this lack of completeness is normal in all natural science.

These days it usually is taken as well demonstrated that we have a complete theory for cosmology after baryogenesis: ΛCDM with allowance for modest adjustments of initial conditions, maybe a variable Λ, maybe dark matter that does something a little more interesting than CDM. My thinking about this has to be colored by my experience: The many wrong turns taken on the path to ΛCDM, and the many right turns overlooked, do not inspire confidence that there will be no more unexpected developments. Some may be quite modest: I expect the great range of levels of nonlinear structures will drive generalizations of HOD (Section 4.1). Some may be more serious: I see the tendency of galaxies to act as island universes (e.g., Peebles & Nusser 2010; Nair, van den Bergh & Abraham 2011; Peebles 2011) as a challenge to established ideas about galaxy formation, maybe to be remedied by better methods of analysis of ΛCDM, maybe by adjustments of this theory. But ΛCDM is a good approximation, and it is good tactics for the community to act as if we had the complete theory. It concentrates research, and if this leads to general agreement that an adjustment to ΛCDM is required, then the evidence very likely will be right and point to a better theory.

While awaiting unexpected developments it can be good tactics for some to seek issues that seem seriously problematic for conventional lines of thought. I have a few favorites (Peebles 2001, Peebles & Nusser 2010); others are thinking in other directions. When something demonstrably challenging to established wisdom turns up, the community should hear about it. I like Fred Hoyle's advice: Publish and be damned—but keep it short.

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