2019 NOBEL LECTURE

How Physical Cosmology Grew

I began studying the large-scale nature of the universe in 1964, on the advice of Professor Robert Henry Dicke at Princeton University. Bob guided my doctoral dissertation and from then on I counted on him as my professor of continuing education.

The usual thinking at the time was that the universe is homogeneous in the large-scale average, and that it is expanding and evolving as predicted by Einstein's general theory of relativity. The schematic nature of this cosmology, and its scant observational support, worried me. But I saw a few interesting things to look into, the results suggested more, and that continued through my career. I review my story at length in the book *Cosmology's Century* (Peebles 2020). Here I recall a few of the steps along the path to the present standard and accepted cosmology that is so much better established than what I encountered in the early 1960s.

Cosmology became more interesting with the discovery that the universe is filled with a nearly uniform sea of microwave radiation with a thermal spectrum at a temperature of a few degrees Kelvin. This CMB (for cosmic microwave background radiation) proves to be a remnant from the hot, early stages of expansion of the universe. Theory and observations in this great advance converged in a complicated way.

In 1964 Bob Dicke explained to three junior members of his Gravity Research Group—Peter Roll, David Wilkinson, and me—why he thought the universe might have expanded from a hot dense early condition. In this hot big bang picture space would be filled with a near uniform sea of thermal radiation, left from the hot early conditions and cooled by the expansion of the universe. Bob suggested that Peter and David build a microwave radiometer that would detect the radiation, if it were there, and he suggested that I think about the theoretical implications of the result. We knew there might be nothing to detect. But we were young, the project did not seem likely to take too much time, and it called for interesting experimental and theoretical methods. I expected I soon would return to something less speculative. That did not happen because the sea of radiation was discovered and gave employment to David and me for the rest of our careers.

Peter Roll went on to a career in education, putting computers into teaching laboratories. Figure 1 shows David and me with Bob Dicke, in a photograph taken about a decade after identification of the presence of the sea of microwave radiation. A balloon carried the instrument in front of us above most of the atmosphere, and a

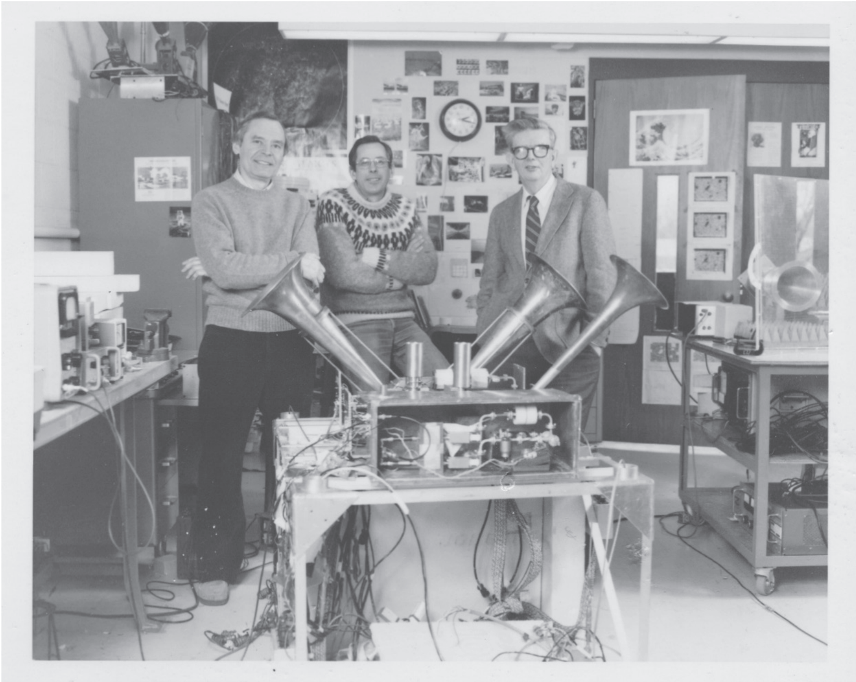

Figure 1.—Left to right: David Wilkinson, Jim Peebles, and Bob Dicke in the late 1970s.

radiometer detected the difference of responses to a pair of horn antennae separated by 90°, so each is tilted 45° from the vertical. As the instrument rotated around its vertical axis this difference of responses made a precision map of variations of the radiation intensity across the sky. There are four horns: two pairs of antennae that operated at two radiation frequencies. This is one of a series of experiments by David and colleagues, along with groups at a few other places, that placed increasingly tight bounds on the departure from exact isotropy and led to the critical developments in the early 1980s to be discussed.

The evidence I know is that the sea of microwave radiation was first detected in the late 1950s as unexpected excess noise in experiments in microwave communication at the Bell Telephone Laboratories. To account for this excess the engineers assumed that radiation from the environment entering through the side and back lobes of their antenna contributes about 2 K to the total noise received (DeGrasse et al. 1959). But this was a fudge; their antenna rejects ground radiation better than that. The unexplained excess consistently appeared in later experiments. It remained a "dirty little secret" at Bell Labs until 1964, when Arno Penzias and

Robert Wilson, both new to the Bell Radio Research Laboratory at Crawford Hill, New Jersey, resolved to look into the problem. They carefully searched for the explanation of this puzzling excess microwave noise, whether originating in the instrument or somehow entering from the surroundings. News of the Princeton search for radiation from a hot early universe showed them a possible solution: maybe the Bell excess noise is from a sea of radiation.

Bell Laboratories showed us in Princeton credible evidence that we are in a sea of microwave radiation, and that the radiation is close to uniform because the excess noise is close to the same wherever in the sky the antenna points. It proves to be what Dicke had suggested we look for, a fossil from the hot early stages of expansion of the universe. How did the Princeton group react to being scooped by Penzias and Wilson? My recollection is excitement at the realization that there actually is a sea of microwave radiation to measure and analyze. Why did the Nobel committee not name Dicke with Penzias and Wilson for the identification of this radiation? Naming Penzias and Wilson was right and proper because they refused to give up the search for the source of the excess noise and, equally important, they complained about it until someone heard and directed them to Bob Dicke. Bob directed the search for the radiation that explains the Bell Labs anomaly that so puzzled Penzias and Wilson.

At Bob Dicke's suggestion I had been thinking about the significance of finding or not finding a sea of radiation. A negative result, a tight upper bound on the radiation temperature, would have suggested an interesting problem. The great density of matter in the early stages of expansion of an initially cool universe could have made the electron degeneracy energy large enough to have forced conversion of electrons and protons to neutrons. The problem with this is that neutrons and their decay protons would have readily combined to heavier elements, contrary to the known large cosmic abundance of hydrogen. So I proposed a way out: postulate a sea of neutrinos with degeneracy energy large enough to have prevented electrons from combining with protons. In the Soviet Union Yakov Zel'dovich saw the same problem with a cold big bang and he offered the same solution, lepton degeneracy. Since Zel'dovich was an excellent physicist it is no surprise that he reached the same conclusion, given the problem. The interesting thing is that we saw the problem at essentially the same time, independently. The consideration somehow was "in the air." I think any experienced physicist can offer other examples of apparently independent discoveries. It seems to have taken a sociologist, Robert Merton (1961), to recognize that this is a phenomenon that deserves to be named. He termed it "multiples in scientific discovery." He also named the phenomenon "singletons in scientific discovery," which he argued may be less common.

I saw that a universe hot enough to have left a detectable sea of thermal radiation would have tended to leave the abundances of the elements in a mix characteristic of the rapid expansion and cooling of the early universe. In an unpublished preprint in late 1964 I estimated that a reasonable upper bound on the primeval helium abundance requires a lower bound on the CMB temperature, $T_o \gtrsim 10$ K, in the absence of degeneracy. My estimate of the foreground radiation from observed stars and radio-loud galaxies indicated that a sea of thermal radiation at this temperature would be readily detected above the foreground.

We might pause to review why I had a lower bound on T_o. During the course of expansion of the early universe, when the temperature fell through the critical value $T_c \simeq 10^9$ K, detailed balance would have switched from suppression of deuterons by photodissociation to accumulation by radiative capture. When deuterons accumulate they can merge to heavier isotopes by the more rapid particle exchange reactions. The smaller the present temperature T_o, the further back in time the temperature passed through T_c, hence the greater the baryon density at T_c, thus the more complete the incorporation of neutrons in deuterons before the neutrons can have decayed, which means the greater the helium production. The amount of element production is determined by the combination ρ_b/T_o^3, where ρ_b is the present baryon mass density. My upper bound on the primeval helium abundance, $Y=0.25$ by mass, is reasonably close to the present standard value, and my lower bound on the mass density, $\rho_b = 7 \times 10^{-31}$ g cm^{-3}, which I of course took to be all baryons, is not much above the established value of the present baryon density. So my 1964 bound on the CMB temperature is a factor of three high. I have not attempted to discover why.

After I had worked out these considerations, I learned that George Gamow already presented the physics of element buildup in a hot big bang in two memorable papers published in 1948 (Gamow 1948a, 1948b). Gamow had earlier proposed that the chemical elements were produced in the hot early stages of expansion of the universe by successive neutron captures, beta decays keeping the atomic nuclei in the valley of stability. His graduate student, Ralph Alpher, computed the element abundances to be expected in this picture, and he and his colleague Robert Herman (1948) found the first estimate of the CMB temperature based on Gamow's picture. Their value is closer than mine, $T_o \simeq 5$ K. Their story is complicated, however, because they used a smooth fit to the measurements of the neutron capture cross-section as a function of atomic weight, and they extrapolated this smooth fit to lower atomic weight through atomic mass 5. Alpher knew there is not a reasonably long-lived isotope at mass 5, so he made the sensible working assumption that nuclear reactions to be discovered bridge the gap. Eliminating this and the other gaps allowed a computation of the buildup of the heavy elements. The Alpher and Herman normalization of ρ_b/T_o^3 is based on their fit to measured abundances of the heavy elements. The detective work establishing this is in Peebles (2014).

Following up an idea with a detailed computation was not Gamow's style. But Enrico Fermi and Anthony Turkevich at the University of Chicago soon worked the first computation of the buildup of element abundances in a hot big bang using realistic nuclear reaction rates. They established that there would be little element buildup beyond helium, a result of Alpher's mass-5 gap. Gamow (1949) reported their result. I could compute in more detail and show evidence that the predicted light isotope abundances coming out of a hot big bang could match the observations. I first analyzed this in 1964, unpublished because I realized I had reinvented the wheel. Soon after that we realized there is a sea of microwave radiation, and after that, I published a better computation (Peebles 1966).

Meanwhile, in the Soviet Union, Jakov Zel'dovich knew about Gamow's ideas but thought they must be wrong because the theory predicts an unacceptably large primeval helium abundance. To check the prediction, he asked Yuri Smirnov (1964) to compute element production in the hot big bang model, along the same lines I was taking in the USA.

In the UK, Hoyle and Tayler (1964) knew the evidence that the helium abundance in old stars is large, and not inconsistent with Gamow's (1948a,b) ideas. Fred Hoyle asked John Faulkner to check Gamow's estimate of deuterium buildup. That was followed by more detailed computations by Wagoner, Fowler, and Hoyle (1967). Tayler (1990) recalled that in 1964 he and Hoyle realized that Gamow's theory predicts the presence of a sea of thermal radiation, a fossil from the early hot conditions, but they supposed it would be obscured by all the radiation produced since then.

So consider the situation in 1964. In the USSR, Zel'dovich thought Gamow's hot big bang theory was wrong because it overpredicted the helium abundance. In the UK, Hoyle knew the evidence that the prestellar helium abundance is large, and maybe consistent with Gamow's theory. But Hoyle expected the fossil radiation that would accompany it would be uninterestingly small. In the USA, I did not know about Gamow yet, but I knew there was a chance of detecting fossil radiation from a hot big bang that made helium because the foreground at microwave frequencies looked likely to be small. Also in the USA, thirty miles from Princeton, Penzias and Wilson had a clear case of detection of microwave radiation of unknown origin. All of this was tied together the following year. It is a charming example of a Merton multiple.

Yet another multiple was the recognition of the role of the sea of thermal radiation in the gravitational growth of the galaxies, arrived at independently by Gamow, Zel'dovich and his group, and me. I hit on what might be a singleton, the analysis of the effect of the dynamical interaction of matter and radiation in a hot big bang cosmology (in Peebles 1965 and many later papers). The early universe would have been hot enough to have thermally ionized matter, and the Thomson scattering of the CMB by free electrons and the Coulomb interaction of electrons and ions would have caused plasma and radiation to act as a viscous fluid. That meant small departures from exact homogeneity in the early universe would tend to oscillate as acoustic waves. Oscillation would be terminated when the plasma cooled to the point that it combined to neutral atoms, freeing the radiation and allowing gravity to draw the baryonic matter into clumps. The termination of acoustic oscillations is a boundary condition that favors discrete wavelengths. That imprints distinctive patterns on the distributions of matter and radiation. The effects became known as BAO, for baryon acoustic oscillations. By the late 1960s the hot big bang cosmology community had grown large enough that several of us, particularly Joe Silk (1967), more or less independently worked out the viscous fluid description of the evolution of departures from homogeneity. I developed the basic ideas of the modern approach to the growth of cosmic structure that describes the radiation by its distribution in phase space. My first graduate student, Jer-tsang Yu, and I applied this theory in the numerical solutions of the effects of BAO on the distributions of matter and radiation published in Peebles and Yu (1970).

It took some time to connect BAO theory to observations of the effect in the distributions of matter and radiation. The BAO effect in the angular distribution of the CMB was discovered and well measured at the turn of the century, and at the time there was a hint of detection in the galaxy space distribution (as reviewed in Peebles 2020). The BAO signature in the galaxy distribution is particularly well seen in the galaxy two-point position correlation function. The matter power spectrum shown in Peebles and Yu has a series of roughly equally spaced bumps, at the wavelengths of the modes favored by the boundary condition, the decoupling of matter and radiation. The correlation function is the Fourier transform of the power spectrum. The Fourier transform of a sine wave is a delta function. The Fourier transform of a series of bumps, which is an approximation to a sine wave, produces an approximation to a delta function, a bump in the correlation function. I presented the prediction of this bump in Peebles (1981, Fig. 5). Tom Shanks (1995) set out to find it, but the available data were not adequate. Daniel Eisenstein rediscovered the bump through a consideration of the Green's function for the matter. It is a different argument on the face of it but physically equivalent to mine. Eisenstein and colleagues demonstrated the bump in data from the Sloan Digital Sky Survey (Eisenstein, Zehavi, Hogg, et al. 2005). It is a sign of the times that my 1981 paper is single-author and the 2005 paper lists 50 authors. But they were needed for the data to detect this subtle effect. And we might note that the bump is much weaker than what Yu and I had considered because the nonbaryonic dark matter weakens the BAO effect. Anyway, I consider the connection of theory and observation of the effect of BAO to be a multiple.

For most of the time between BAO theory and observation it was not at all clear to me that there would be a detection. The BAO theory assumes standard physics, including the general theory of relativity. That is an extrapolation from the tests in the Solar system and smaller, on scales $\lesssim 10^{13}$ cm, to the scales of cosmology, $\sim 10^{28}$ cm. Would you be inclined to trust an extrapolation of fifteen orders of magnitude? The theory also assumes cosmic structure grew out of departures from homogeneity associated with spacetime curvature fluctuations that are small and nearly scale-invariant. There were other possibilities. What is more, there was a hint that some of these assumptions fail because they predict that the sea of microwave radiation, the CMB, has a close to thermal spectrum. Prior to 1990 the measurements suggested a significant excess over thermal at wavelengths shorter than the theoretical Wien peak. That might mean violent events in the early universe released a lot of energy, contributing some of it to the CMB and some to rearranging the matter. Or maybe the universe is not very close to homogeneous; maybe we observe a mix of radiation temperatures from different regions. Either might be expected to have spoiled the BAO signatures computed in linear perturbation theory.

This uncertain situation was resolved in 1990 by two brilliant experiments, one carried by the USA NASA satellite COBE, the other by the Canadian University of British Columbia rocket COBRA. Both established that the spectrum is very close to thermal (Mather, Cheng, Eplee, *et al*. 1990; Gush, Halpern, and Wishnow 1990). That demonstration, a clear multiple, eliminated a serious challenge to the BAO theory. John Mather rightly was named a Nobel Laureate for his leadership in the spectrum measurement. Herb Gush was equally deserving; awards can be capricious.

Prior to the demonstration that the CMB spectrum is wonderfully close to thermal I had to consider the possibility that there is a real and substantial departure from that equilibrium condition. The interpretation would be messy. I didn't want to think about it, so, while awaiting clarification of the spectrum measurements, I turned to another program, statistical measures of the galaxy distribution and motions relative to the mean homogeneous expansion of the universe. There were several catalogs of galaxy positions ready and waiting for analyses. Most important was the catalog assembled by Donald Shane and his collaborators, mainly Carl Wirtanen, at the Lick Observatory of the University of California. They counted galaxies in small cells in the sky, logging some one million galaxies by scanning photographic plates with a traveling microscope. This heroic effort took them ten years. Converting to data suitable for computation of statistical measures was a considerable effort too. Graduate students in physics seem to have a sense of where interesting things are happening and gather around. Graduate students Jim Fry, Mike Seldner, Bernie Siebers, and Raymond Soneira did much of the heavy lifting, along with my colleague on the faculty, Ed Groth.

Since I like images I was pleased with the map we made of the large-scale galaxy distribution. And I was delighted to have the chance to show the map to Donald Shane and ask whether it looks like what he saw. He laughed and said, "I was looking at this one galaxy at a time."

The Lick and other catalogs are compilations of angular positions of galaxies with approximate distances. The statistical measures I used are N-point position correlation functions and their Fourier or spherical harmonic transforms. These statistics allow convenient translations from angular to the wanted spatial functions. And the N-point functions scale in a predictable way with the characteristic distances of the galaxy samples, assuming the universe is a stationary random process. That was particularly important because it allowed a test for systematic errors by checking the scaling of the angular correlation functions with depth. Another singleton in my career is the successful demonstration of scaling published in Groth and Peebles (1977). It showed that we had reliable measurements of the low-order galaxy position correlation functions at separations from a few tens of kiloparsecs to a few megaparsecs. Methods and results for this program are assembled in my book, *The Large-Scale Structure of the Universe* (Peebles 1980).

Why did I devote so much effort to this program? I enjoy this kind of analysis. And I had the vague feeling that the results might offer a hint to how the galaxies and their clumpy space distribution got to be the way they are. That happened, more or less, as follows:

By 1980 it had become clear that the sea of microwave radiation is far smoother than the space distribution of the galaxies. But the mass concentrations in galaxies and groups and clusters of galaxies were supposed to have grown by gravity out of the initially close-to-homogeneous early universe of the hot big bang theory. How could this growth of mass concentrations have so little disturbed the CMB? Surely the gravitational gathering of mass concentrations in the early universe would have drawn the radiation with it, dragged by the coupling of plasma and radiation. That would have seriously rearranged the radiation. Such a disturbance to the radiation was not observed in the measurements by David Wilkinson and his students and by others in the growing community of empirical cosmologists. Bruce Partridge (1980), who had moved on from the Princeton Gravity Group to Haverford College, presented a considerable list of the increasingly tight bounds on the CMB anisotropy we had in the years around 1980.

So why is the CMB so smooth? In yet another of Merton's multiples, I and Zel'dovich's group in the USSR independently guessed the answer: Suppose the baryonic matter that stars and planets and people are made of is only a trace element, and that most matter is dark and interacts weakly if at all with radiation and our type of baryonic matter (Doroshkevich, Khlopov, Sunyaev, Szalay, and Zel'dovich 1981; Peebles 1982). The CMB would slip freely through this nonbaryonic dark matter, allowing mass concentrations to grow while disturbing the CMB only by the weak effect of gravity and by the interaction with a modest amount of our baryonic matter.

In pursuing this line of thought I had some advantages over Zel'dovich and colleagues, the other main group active on the theoretical side of empirically based cosmology in those days. They assumed the dark matter is one of the known neutrino families with a rest mass of a few tens of electron volts. (This is the mass allowed by the condition that the mass density of the neutrinos thermally produced along with the CMB not exceed what cosmology would allow. It also is the mass indicated by a laboratory experiment, which was influential for a while. But the measurement proved to be wrong.) The rapid motions of these neutrinos in the early stages of expansion of the universe would have smoothed the primeval mass distribution to a mass scale typical of rich clusters of galaxies. That would mean the first generations of bound mass concentrations were much larger than galaxies. These concentrations would have to have fragmented to form the galaxies. But I knew rich clusters are rare, and most galaxies are not near any of the clusters. And we all know that gravity tends to gather together, not cast away. Thus it was pretty clear to me that the USSR scenario is not viable. Wanted instead was nonbaryonic dark matter that had been effectively cold in the early universe, meaning its pressure had not suppressed the early gravitational formation of small clumps of matter that would have merged to form the hierarchy of clumps we observe around us. I knew that elementary particle physicists had been speculating about forms of nonbaryonic matter that would have this wanted property. I also knew the relativistic prediction of the gravitational disturbance to the radiation produced by the departure from a homogeneous mass distribution. Rainer Sachs and Arthur Wolfe (1967) had worked that out. And I had a well-checked statistical measure of the space distribution of the galaxies, which I took to be the wanted measure of the mass distribution needed to normalize the model.

The model I put together from these pieces predicts that the disturbance to the CMB caused by the formation of the observed matter distribution would cause the CMB temperature to vary across the sky by a few parts per million. That is much less than the upper bounds from the CMB anisotropy measurements we had when I published this prediction in Peebles (1982). The CMB anisotropy was detected some 15 years later, and found to agree with my computation within the modest uncertainties. This is no surprise because I guessed at the right physical situation, the computation is not complicated, and I had a reliable calibration from the galaxy space distribution.

The new form of matter in my 1982 proposal became known as cold dark matter, or CDM, the "cold" meaning the dark matter pressure in the early universe was small enough not to have excessively smoothed the primeval mass distribution. I added the assumption that general relativity survives the immense extrapolation to the scales of cosmology, and that mass concentrations grew out of primeval spacetime curvature fluctuations. The introduction of this CDM cosmological model might be counted as a singleton, because I don't know that anyone else independently put all these pieces together. I just assembled pieces I already had, to be sure, but that's not unusual; we build on what came before.

There was a remarkable multiple in 1977. Five groups, independently as far as I can tell, introduced the idea of a new class of neutrinos with rest mass of ~3 GeV. This became known as WIMPs, for weakly interacting massive particles. WIMPs have the properties I needed, though the particle physicists who proposed WIMPs in 1977 certainly couldn't have foreseen that. And they were at best vaguely aware of the astronomers' evidence of subluminal mass around galaxies. Yet the WIMP idea appeared not long after the astronomers had good evidence of subluminal mass around galaxies, and not long before I needed nonbaryonic cold dark matter to account for the smoothness of the CMB.

My 1982 CDM cosmology was greeted with more enthusiasm than I felt it warranted because I could think of other models that would equally well fit what we knew then. The CDM model is particularly simple, to be sure, but does that mean it is the best approximation to the real world? In particular, my 1982 paper assumed for simplicity that the universe is expanding at escape velocity, but by that time I already knew what I considered to be reasonably good evidence that the expansion is faster than that.

Expansion at escape velocity, in the relativistic Einstein—de Sitter cosmological model that assumes space curvature and Einstein's cosmological constant may be ignored, would mean that whenever we happened to flourish and take an interest in the expanding universe we would find that the rate of expansion is at escape velocity. That is, we would not have flourished at any special time in the course of expansion of the universe. This seems comforting somehow. I liked the thought, prior to 1982, but it proves to be wrong. The early indication came from Marc Davis, who had been a graduate student in Dicke's Gravity Research Group and moved on to Harvard and the Smithsonian Center for Astrophysics. Marc had worked with me in analyses of the theory of evolution of the galaxy distribution. He knew my hunger for measurements of galaxy redshifts that would improve the statistical measures, and he found that his new position had the resources for a systematic galaxy redshift survey. That was something new then. Marc invited me to join him in the data analysis. The results in Davis and Peebles (1983) surprised me by suggesting that we do flourish at a special epoch.

These redshift data yielded a probe of the relative motions of the galaxies. That gave a measure of galaxy masses, which indicated that the mean mass density is less than required for escape velocity. The community opinion was that this seems quite unlikely. One way out supposes that most of the mass is not in the galaxies, but is more broadly spread, which would reduce the gravitational attraction of neighboring galaxies, reducing their relative velocities, as wanted. But that didn't seem right to me. Davis and I found consistent galaxy mass estimates from the relative motions of galaxies over a range of a factor of ten in separation. If mass were more broadly distributed than galaxies shouldn't we see that more of the mass is detected as we increase the scale of the measurement? Also, the popular idea then was that mass is more broadly distributed than galaxies because galaxy formation had been suppressed in regions with lower mass density. It would have made galaxies more tightly clustered than mass, as wanted. But if galaxy formation were suppressed in low-density regions then galaxies that did manage to form there ought to show signs of a deprived youth: irregulars or dwarfs. This was not seen in the Center for Astrophysics data.

From the early 1980s through the mid-1990s I played the role of Cassandra, emphasizing the growing evidence that the universe is expanding faster than escape velocity to people who for the most part would rather not think about it. I remember a younger colleague saying I only did it to annoy. I knew it teases, but I meant it, and I regret nothing. The evidence was reasonably good then, and it is well established now, that we flourish at a special time in the course of evolution of the universe, as the rate of expansion is becoming significantly more rapid than escape.

In 1984 I introduced the accommodation to the low mass density that proves to work: add Einstein's cosmological constant, Λ (Peebles 1984), in what became known as the ΛCDM theory. At the time others were starting to pay attention to my arguments for low mass density and were thinking about the benefits of adding Λ. Turner, Steigman, and Krauss (1984) proposed it, for example. The largest part of their paper is a discussion of the idea that the mass of the universe is dominated by relativistic products of the recent decay of a postulated sea of massive unstable particles. Their last three paragraphs are considerations of the benefits of adding Λ. From the choice of emphasis I take it that they considered the hypothetical particle species with their relativistic decay products to be less adventurous than the addition of Λ. And Λ is odd indeed. Anyway, I think I was the first to present actual computations of the effect of adding Λ. Einstein wrote his constant as λ. I don't know who introduced the change to Λ. I believe Michael Turner, of the University of Chicago, introduced the change of name to dark energy. But whatever the name, we don't understand the physical interpretation, though it's clear now that we need something that acts like Λ.

In the years around the mid-1990s I again acted in my self-appointed role of Cassandra, because I was not at all confident that the ΛCDM theory is a good approximation. The tests were not yet all that tight, and I could think of other models that fit the data about as well. In the late 1990s I was finishing my latest and maybe most elegant alternative to ΛCDM when I learned that the CMB anisotropy measurements revealed features characteristic of the theory Jer Tsang Yu and I had worked out a quarter century earlier. So I abandoned the search for alternatives.

I remain surprised and impressed at how well ΛCDM passes ever more demanding tests. But I continue to hope that challenges to ΛCDM will be found and help guide us to a still better more complete theory.

I have written four books on the state of research in cosmology. I meant the title of the first, *Physical Cosmology*, to indicate that I did not intend to get into the subtleties of what might be termed astronomical cosmology: evidence from stellar evolution ages and the extragalactic distance scale. I don't think I thought of it at the time, but the title also helps distinguish my book from the earlier bloodless treatises on cosmology. I meant to explore the physical processes that are observed to have operated, or might be expected to have operated, in an expanding universe, and to explore how theory might be shaped to observations. At about the time of my book's publication, in 1971, Steve Weinberg published his book, *Gravitation and Cosmology* (1972). It is more complete in the mathematical considerations. Mine is more complete in the considerations of phenomenology and of how the phenomenology might be related to physical processes. The two books signal the change of physical cosmology from its nearly dormant state in the early 1960s to the start of a productive branch of research in physical science by the late 1960s.

My second book on cosmology, *The Large-Scale Structure of the Universe*, published in 1980, is a sort of catalog of the statistical measures I had devised and applied, the methods of analyses of how these measures might be expected to have evolved in an expanding universe, and the observational consequences of the evolution. I did not aim to arrive at a standard model for cosmology. Ideas about that were much too confused, a result of the still quite limited evidence. I meant this book to be a working guide to how we might proceed in research in physical cosmology. As it happened, thoughts about a standard model were seriously disrupted a few years later by my argument for dark matter that is not baryonic. Writing this book helped me introduce what came to be known as the Cold Dark Matter cosmological model, in 1982. I still consult *The Large-Scale Structure of the Universe* for reminders of methods.

My third book, *Principles of Physical Cosmology*, is much larger than the second, which in turn is much larger than the first. This one was published in 1993, at about the end of the time when it was practical to aim to present in one volume a reasonably complete assessment of the state of research in the physical science of cosmology. One certainly would not consider aiming for that now. Research in cosmology in the mid-1990s was an active turmoil of multiple ideas and promising-looking but confusing results from model fits to measurements in progress. That situation quite abruptly changed at the end of the decade, when research converged on a well-tested standard model, the ΛCDM cosmology.

The convergence was driven by three great observational programs. One is the tight measurement of the redshift–magnitude relation that reveals the departure from the linear low redshift limit. That feat generated a Nobel Prize. Second is the precision measurement of the cosmic microwave radiation anisotropy spectrum. That was a comparably important accomplishment that certainly merits a Nobel Prize. The third, the measurement of the cosmic mean mass density, was the main focus of empirical research in cosmology from the early 1980s through the mid-1990s. Its story is more complicated, and not as well recognized and understood as it ought to be. The three made the case for a cosmology that is hard to resist. I have once again given into the impulse to write a book. This one, *Cosmology's Century*, describes how these three programs, with other results from brilliant ideas and elegant experiments, along with the wrong turns taken and opportunities missed, got us to a well-tested cosmology (Peebles 2020).

The establishment of cosmology is a considerable extension of the reach of well-tested physical science, and the story is simple enough that it offers a good illustration of the ways of physical science. In particular, I am impressed by the many examples of Merton's multiples in scientific discovery. I have mentioned examples from the history of cosmology, and this story has quite a few more. We all can think of examples in other branches of physical science. Some multiples may be coincidences, pure and simple. Some may be artifacts of our tendency to present the history of science in a linear fashion that makes unrelated developments appear related. I can imagine some multiples grew out of hints communicated by gestures or thoughts not completed that suggest meaning within our shared culture of physical science. It happens in everyday life, why not in science? And I picture the broad general advance of physical science as a spreading wave that touches many and might be expected to trigger any particular idea more than once, apparently independently. As we sometimes say, thoughts may be "in the air." But I must leave a firmer assessment to those better informed about the ways we interact.

Meanwhile, let us not forget the great lesson that the established social constructions of science are buttressed by rich and deep webs of evidence. Surely there is a better more complete cosmology than ΛCDM. But we may be confident that the better theory will predict a universe that is a lot like ΛCDM, with something analogous to its cosmological constant and dark matter, because the universe has been examined from many sides now and found to look a lot like ΛCDM.

I confess to having been unhappy with the Nobel Prize Committee for not recognizing Bob Dicke's deep influence in the development of gravity physics and cosmology. The committee had their reasons, of course; their considerations can be complicated. But I am satisfied now because my Nobel Prize is closure of what Bob set in motion, his great goal of establishing an empirically based gravity physics, by the establishment of the empirically based relativistic cosmology.

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