supplied by astrophysical sources (via hydrogen burning or via gravitational collapse), but this proved of course a blind alley and distraction.

I can lay claim to two minor positive contributions directly related to the CMBR. One (Rees and Sciama 1968) concerned what is now sometimes called the "Rees–Sciama effect" – the perturbation in the CMBR due to a transparent gravitational potential well along the line of sight (for example, a cluster or supercluster of galaxies). In the linear regime, this is subsumed in what is normally called the "integrated Sachs–Wolfe effect" – it is non-zero except (to first order) in the Einstein–de Sitter universe. However there is a distinctive effect due to virialized clusters. Had Sciama and I known then the actual amplitude and scale of clustering, we would not have felt it worthwhile to explore these higher-order effects. But at that time there was no way of ruling out large-amplitude density fluctuations on gigaparsec scales (indeed there were early – and in retrospect misleading – indications of such clustering from the distribution of quasars over the sky). This effect has only recently been detected. My second contribution (Rees 1968) addressed the possible polarization of the CMBR. The simplest illustrative examples of this effect arose in anisotropic but homogeneous models (though the effect was obviously present in more general models). This work stimulated an early search by Nanos (1974, 1979), but it was more than 35 years before polarization was actually detected.

In CMBR studies, a consensus has generally quickly developed whenever there has been an advance – this is in contrast to (for instance) the prolonged debate and perplexity about the physics of AGNs and quasars. This is because the CMBR data, though challenging to obtain, are "cleaner," and the relevant fluctuations are in the linear regime. Successive developments – the CDM paradigm, the CMBR fluctuation spectrum, and so forth – have led to a well-established set of cosmological parameters. It has been a privilege to have followed a subject where progress has been sustained so consistently for 40 years, and to have known many of the scientists to whom these historic advances are owed.

4.9 Critical reactions to the hot big bang interpretation

4.9.1 Geoffrey R. Burbidge and Jayant V. Narlikar: Some comments on the early history of the CMBR

Geoffrey Burbidge is Professor of Physics at the University of California, San Diego. He served for six years as Director of the Kitt Peak National Observatory. His latest major award, jointly with Margaret Burbidge, is the Gold Medal of the Royal Astronomical Society. Jayant Narlikar served as

Founder Director of the Inter-University Centre for Astronomy and Astrophysics in Pune, India, until his retirement in 2003. He is now Emeritus Professor at IUCAA. Among his current interests is exobiology.

Both of us were asked to describe our views of the ways we first approached this topic. We have decided to combine our contributions but present them separately because we came to the basic ideas from different directions. Geoffrey Burbidge became interested in the CMBR from his early association with the fundamental problem of the origin of the chemical elements. Jayant Narlikar had been interested in alternative cosmologies and was, therefore, concerned with the problem of how the CMBR could be produced without a hot big bang. Each of us has given a "first person" account. As we had the benefit of close interaction with Fred Hoyle we have folded in his views also wherever necessary.

The Approach Taken by Geoffrey Burbidge. My first interest in this area came during the period 1955 to 1957 when Margaret Burbidge, Fred Hoyle, Willy Fowler, and I were solving in detail the problems of the origin of the elements (Burbidge *et al.* 1957).

I realized that the large abundance of helium in stars ($M_{\rm He}/M_{\rm Baryon} \equiv Y \cong 0.24$) meant that there must be a very special place, or an era, when there had been a great deal of hydrogen burning. At that time, the value of the Hubble constant was thought to be $180\,{\rm km\,sec^{-1}\,Mpc^{-1}}$ (Humason, Mayall and Sandage 1956), so that the Hubble time was $H_0^{-1} \simeq 6 \times 10^9$ years. Taking the luminosity of the Milky Way to be about $10^{44}\,{\rm erg\,sec^{-1}}$, this meant that over 6×10^9 years the total mass of helium that was produced by hydrogen burning would be far less than 24% of the total mass of helium $\simeq 2.5 \times 10^{10}$ solar masses.

I did not realize at the time that my argument was very similar to that which had been made by Alpher, Bethe and Gamow (1948) a decade earlier. At the time of the first calculation by Gamow, Alpher and Herman, Hubble and Humason (1931) had given a value of $H_0 = 550\,{\rm km\,sec^{-1}\,Mpc^{-1}}$, so that $H_0^{-1} \simeq 2 \times 10^9$ years and the discrepancy between the observed abundance of helium and the amount which could be attributed to hydrogen burning in stars was even larger. However, in contrast to me, Gamow and his colleagues had discussed the basic physics of the big bang and concluded that helium could only have been made in the early universe. Up until then it had been assumed that in Friedmann models, in the beginning the rest mass energy is much greater that the radiation energy. The immediate effect of the change to a radiation-dominated universe was to require that the scale factor of the universe $a(t)$ is proportional to $t^{1/2}$. Omitting electron–positron

pairs, the radiation temperature T is inversely proportional to a. Thus the radiation temperature T is proportional to $t^{-1/2}$. With radiation alone and no neutrinos $T_9 = 15.2 \times t^{-1/2}$ where T_9 is measured in units of 10^9 K and t in seconds. However, the numerical coefficient 15.2 is modified by the presence of electron–positron pairs and by neutrinos. For temperatures high enough for the electrons and positrons to be relativistic, and for two mass-less neutrino types, the numerical coefficient is changed from 15.2 to 10.4. So long as the energy in the early universe is dominated by radiation the equation above holds.

But the next step in the discussion was completely *ad hoc*. The mass density of stable nonrelativistic particles, explicitly neutrons and protons, decreases with the expansion of the universe at a rate proportional to a^{-3}, i.e. as $t^{-3/2}$. Calling this density ρ_b, Alpher and Herman (1948) took $\rho_b = 1.70 \times 10^{-2} t^{-3/2}\,\mathrm{g\,cm^{-3}}$ with the coefficient 1.70×10^{-2} being the *ad hoc* step. There is *nothing* in the theory which fixes this value. It is a free choice, chosen to make things right, in this context to obtain the calculated value of the helium abundance Y to agree with observation. Thus, while the big bang theory can explain the microwave background, it tells us nothing about the helium abundance unless we *choose* a numerical value which enables us to do this.

This is fine if you come to the problem of the helium with a belief in the big bang. And this is what most contributors to this book have done. But I came to the problem with no cosmological beliefs.

In the 1950s a debate was going on between the majority of cosmologists, who believed in a beginning, and a few, particularly Hoyle, Bondi, and Gold, who had developed an alternative, the steady state cosmology (Bondi and Gold 1948; Hoyle 1948). By the late 1950s, standing on the sidelines in Cambridge, I realized how unpopular the steady state theory was, since at the time there was a very unpleasant dispute going on between Ryle and his group on one side, and Fred Hoyle. In the early 1960s, Hoyle and Narlikar (1961) gave an alternative interpretation of the radio source counts to show them as consistent with the steady state theory, whereas Ryle insisted these provided strong evidence against the steady state.

Returning to my own work on the origin of helium, I made a calculation assuming that all of the baryonic matter of the universe with a density $\rho_b = 3 \times 10^{-31}\,\mathrm{g\,cm^{-3}}$ had the same helium abundance. I then showed that if it were produced by hydrogen burning the energy density must amount to $\approx 4.5 \times 10^{-13}\,\mathrm{erg\,cm^{-3}}$ (Burbidge 1958; see also Bondi, Gold and Hoyle 1955).

In my paper I offered several possible scenarios for the production of helium. It could have been produced in the early universe if there was one;

it could be due to higher luminous phases in galaxies for periods during their lifetimes; or I speculated it was possible that we were overestimating the real cosmic abundance of helium because the ratio of helium to hydrogen was much smaller in the low-mass stars which make up a large part of the total mass, than it is in the hot stars and nebulae in which the abundances can be determined spectroscopically.

The key point that I missed, as did Bondi, Gold and Hoyle (1955), who had made a similar calculation in 1955, arguing that the energy must have come from red giants (in 1958 I had missed the Bondi, Gold and Hoyle paper), was that the energy density corresponding to the production by hydrogen burning when the energy was degraded to blackbody form would give a blackbody temperature of 2.75 K!

If these results had been publicized, they might have been seen as predictions based on observed quantities of what the temperature of the blackbody radiation would turn out to be, if it were detected. But of course this never happened.

As he told me many times later, Fred Hoyle had realized all along that the hydrogen burning in stars was a possible source of the helium and that it would lead to a powerful background radiation field. Much later he and I took very seriously the fact that the CMBR energy density is so close to what the prediction from the hydrogen burning origin would give, and concluded that all of the light isotopes D, ^{3}He, ^{4}He, and ^{7}Li also have a stellar origin. In other words *all* of the isotopes in the periodic table are due to stars. Our paper on this topic was rejected by *Physics Review Letters*, obviously because very convinced big bang advocates refereed it. However, it was finally published in 1998 in the *Astrophysical Journal Letters* (Burbidge and Hoyle 1998).

A key point that most physicists were unaware of throughout the 1950s, 1960s, and 1970s, and in particular the large number of those who believe in the standard model still appear to be unaware of it, is that in 1941 A. McKellar at the Dominion Astrophysical Observatory in Victoria made an estimate of the radiation field in which the interstellar molecules CN and CN$^+$ are bathed, and stated that if this was blackbody the radiation temperature is $1.8\,\mathrm{K} < T < 3.4\,\mathrm{K}$. The exact quote from his paper (McKellar 1941) is as follows:

> Dr. Adams has kindly communicated to the writer his estimate of the relative intensity, in the spectrum of ζ *Ophiuchi*, of the $\lambda 3874.62$, $R(0)$ interstellar line of the $\lambda 3883$ CN band and the $\lambda 3874.00$, $R(1)$ line, as 5 to 1. $B_0 J''(J''+1)+\ldots$ has the values 0 and $3.78\,\mathrm{cm}^{-1}$ for the 0 and 1 rotational states and for the two lines

$R(0)$ and $R(1)$ the value of the intensity factor i are, respectively 2 and 4. Thus from (3) we find, for the region of space where the CN absorption takes place, the "rotational" temperature,

$$T = 2^{\circ}\!.3\ K.$$

If the estimate of the intensity of $R(0)/R(1)$ were off by 100 percent, this value of the "rotational" temperature would not be changed greatly, $R(0)/R(1) = 2.5$ giving $T = 3^{\circ}\!.4\,\mathrm{K}$ and $R(0)/R(1) = 10$ giving $T = 1^{\circ}\!.8\,\mathrm{K}$.

Had this been generally known in the 1950s, and been put together with the result quoted earlier, the history of what most people want to believe about the CMBR and its origin might be different.

At the time in the early 1960s when Fred Hoyle and George Gamow were debating cosmology, Fred was aware of this result, and used it when Gamow would argue that the temperature was likely to be much higher. I first learned of this result from Fred in that period.

My view of the subsequent history (*as I saw it*) is as follows. In the early 1960s Robert Dicke and J. Peebles reworked the ideas of Gamow, Alpher, and Herman. Since Dicke was a superb experimentalist, he proposed that an attempt be made to detect the radiation. This is what he and David Wilkinson set out to do. But, of course, before they achieved any result there was the serendipitous discovery by Penzias and Wilson (1965a).

But throughout the 1960s the ideas emanating from Princeton and also from Moscow from Zel'dovich's group led almost everyone to believe that the radiation could only be a remnant of a big bang and would be of blackbody form.[15] It would be proof that the steady state theory was wrong. With the Penzias and Wilson discovery, while there was still no proof that it was blackbody, it was thought that the verdict was in.

Even Fred Hoyle began to doubt the correctness of the steady state cosmology, and in his address to the British Association in September 1965 he came as close as he ever did to concluding that the steady state would not work. Starting at that time, he began to discuss a modification of the steady state which in the 1990s, with J. V. Narlikar and me, was turned into the QSSC – an oscillating model still over the long term a steady state universe (Hoyle, Burbidge and Narlikar 1993).

[15] It was in this period that my view that cosmological ideas are driven as much by the views of leading scientists as by actual observations was strengthened. I was present at meetings where early rocket observations were reported which did not confirm the blackbody idea. Those were immediately severely criticized by leading theorists who did not understand the experimental details but were absolutely convinced that the blackbody nature must be correct. They eventually turned out to be right, but their prejudice was obvious.

Jayant V. Narlikar's View. I recall that one day in 1964, Fred Hoyle walked into his office in the DAMTP in a rather disturbed mood. He confided: "I believe, I have found the strongest proof for the big bang." With his previous encounters with Martin Ryle and his colleagues in the Cavendish, I wondered if there was some new evidence from radio astronomy that had unsettled Fred. "No," he added, "my own calculations suggest that helium was mostly made not in stars but in a high temperature epoch in the past. I find that if the density–temperature relationship is properly adjusted one can get almost 25% helium."

For someone who had worked long and hard on stellar nucleosynthesis to demonstrate that most of the chemical elements were made in stars, this finding had come as a shock, even though it was he himself who had done the calculation. His work with Roger Tayler was subsequently published in *Nature* (Hoyle and Tayler 1964) and quickly became a much-cited paper... probably it was the only paper Fred wrote with conclusions close to favoring the big bang scenario. Nevertheless, he left an alternative possibility open, namely the existence of supermassive objects that allow stellar nucleosynthesis to generate adequate helium. This possibility is also discussed briefly in the classic paper on nucleosynthesis by Wagoner, Fowler and Hoyle (1967).

Even so, Fred did not relate the 1964 finding with the possible existence of relic radiation. The result struck him as very important only in 1965 after the discovery of the radiation by Penzias and Wilson (1965a). Although the blackbody nature of the radiation had not been established in 1965, its finding together with helium abundance apparently had the effect of convincing him of the existence of a high temperature phase early in the universe.

It was against this background that he delivered his oft-quoted speech to the British Association for the Advancement of Science (Hoyle 1965) in which he came close to supporting the big bang cosmology at the expense of his own steady state theory. One popular magazine in the USA likened this reaction to the problematic situation of Lyndon Johnson abandoning his membership of the Democratic Party to join the Republicans!

I had worked with Fred on many aspects of the steady state theory, and felt that Fred had "given in" too soon. Dennis Sciama, another strong adherent of the steady state idea, also felt the same, although within a couple of years he changed over to the big bang point of view. In the meantime, Fred had second thoughts on the matter. Both he and I, along with Chandra Wickramasinghe, felt that alternative explanations of the radiation background should be looked for. The reasons were mainly as follows:

(i) There are radiation backgrounds at various other wavebands and these are mostly traced to astrophysical sources. Can the microwave background be shown to originate from astrophysical sources radiating mainly in infrared and microwaves?

(ii) Following a more general line of argument, there are galactic and extragalactic astrophysical processes with energy densities comparable to the newly discovered microwave background (CMBR), for example cosmic rays, magnetic fields, and galactic starlight. So to ascribe a relic interpretation to the CMBR gives an unexplained coincidence of energy density.

(iii) The fact that if all helium in the universe were made in stars the resulting energy density would be comparable to that of the microwave background which has already been highlighted in this paper suggested a nonrelic interpretation.

I will discuss these possibilities briefly from a modern standpoint.

It was shown by Wolfe and Burbidge (1969) that the multiple source hypothesis would generate a microwave background that was too inhomogeneous for agreement with the preliminary limits on anisotropy. The only way to escape from this conclusion was that the sources were far more numerous than galaxies and typically weaker than galaxies. Such a population was considered rather unlikely and has not been found.

The search for an astrophysical process to generate the CMBR in the Milky Way Galaxy or in clusters of galaxies led Hoyle and Wickramasinghe to various scenarios involving interstellar dust: dust that could convert starlight or other energy into a thermalized form with the energy density found in the CMBR. Narlikar, Edmunds and Wickramasinghe (1976) wrote a paper suggesting how this could happen using dust grains in the form of whiskers. The scenario was plausible but it was not clear that it would meet the various observational constraints that were being placed on the properties of the CMBR.

The idea of Narlikar, Edmunds and Wickramasinghe (1976) could be applied to a situation in which it was assumed that there had been a lot more starlight initially because of greater stellar activity, which led to most of it being thermalized by whiskers. This idea, however, ran into problems with the original formulation of the steady state theory, which would not allow any epoch-dependent process. Nevertheless, Hoyle and Wickramasinghe persisted with the efforts to study the thermalization process in detail.

Eventually the process was shown to work, not in the original steady state cosmology but in its variant, the *Quasi-steady state cosmology.* This

cosmology was proposed by Hoyle, Burbidge and Narlikar (1993) and it envisages a long-term steady state universe with short-term oscillations. The e-folding time of the long-term steady state is around 1000 Gyr, whereas the period of a typical oscillation is around 50 Gyr. We refer the reader to the details given in Hoyle, Burbidge and Narlikar (2000) and to later references (Narlikar *et al.* 2003). So far this alternative is able to achieve the following:

(i) Explain the CMBR as a relic of stars burnt out in the previous oscillations with the present temperature of 2.7 K related to stellar activity at present observed in the universe. See Hoyle, Burbidge and Narlikar (1994) for details.
(ii) A Planckian spectrum at all wavelengths except possibly at wavelengths longer than 20 cm. (There the galactic noise anyway dwarfs the cosmological effect.)
(iii) An angular power spectrum that explains the main peak at around $l = 200$, as arising from typical clusters at the last minimum scale epoch (Narlikar *et al.* 2003).
(iv) The dust density required for thermalization being consistent with that needed for dimming distant supernovae.
(v) A weak polarization on the scale of clusters arising from magnetic alignment of whiskers scattering the radiation.
(vi) Independent evidence for the existence of whisker dust from various astrophysical scenarios.

Fred Hoyle firmly believed that an alternative interpretation of the CMBR along the above lines would turn out to be closer to reality than the standard interpretation. What were the attitudes of the other two coauthors of the steady state theory? I never had the chance to discuss the CMBR with Tommy Gold. By 1965 he had already moved away from cosmology and I do not think he worried too much about the issue. Hermann Bondi had likewise developed other interests. However, I had met him on several occasions. Once in an interview on the All India Radio, Pune, during the 1990s I had asked him what he felt about the steady state theory in the light of the observations of the CMBR, especially by COBE. He replied that to him the steady state theory had been attractive from the Popperian point of view: it made definite statements which could be checked against observations. That the CMBR spectrum had turned out to be so close to the Planckian was, in his opinion, a very difficult observation for the steady state theory to explain. So he had felt that the theory was no longer viable. Like most cosmologists he had been unaware of the above work on alternative cosmology, but seemed

pleased that perhaps such an explanation of the origin of the CMBR might succeed.

Going back to 1965, one can say today that while the big bang scenario has been taken a good bit forward in the last four decades, the alternative explanation has also made considerable progress and deserves to be critically examined side by side with the standard explanation.

4.9.2 David Layzer: My reaction to the discovery of the CMBR

David Layzer is the Donald H. Menzel Professor of Astrophysics Emeritus at Harvard University. He is the author of two books, Constructing the Universe *and* Cosmogenesis, *and was an associate editor of the* Annual Reviews of Astronomy and Astrophysics *for 30 years.*

Cosmology became a science in the 1920s. During that decade Hubble's observational program with the 60- and 100-inch telescopes on Mt. Wilson supplied compelling evidence for the hypothesis that guided his program and was its central finding: that the observable universe is a fair sample of the universe as a whole. Friedmann's (1922) theory of a uniform, unbounded fluid, based on Einstein's theory of gravitation in its original form, predicted that such a fluid cannot be static but must expand from an initial singular state in the finite past. And to round off the decade, measurements of the redshifts of faint distant galaxies by Hubble and Humason showed that the system of galaxies was in fact expanding in the way predicted by Friedmann's theory. The next major advance in observational cosmology was the discovery of the CMBR by Penzias and Wilson (1965a).

Not everyone was surprised. George Gamow had suggested that heavy atomic nuclei were formed by successive neutron captures in an early hot universe. Using measured neutron-capture cross sections he and his colleagues deduced the temperatures that would have had to prevail when the expanding universe was dense enough for successive neutron captures to produce (approximately) the observed relative abundances of heavy nuclei. On this basis they predicted that the radiation field, eventually decoupling from the matter, would retain its thermal character and would now have a temperature of about 10 K. (Of course, as we now know, this prediction rested on a false premise. The heavy nuclei were formed in the cores of massive stars, not in a hot, dense cosmic medium.)

Others were surprised. The steady state cosmology, put forward by Hermann Bondi and Thomas Gold (1948) to explain a discrepancy between

the estimated age of the universe (based on measurements of Hubble's constant) and the estimated age of the Earth, was still popular, especially among British cosmologists. In Sweden, Bertil Laurent and Oskar Klein had suggested that the universe is finite and bounded, an expanding island floating in empty space. These cosmological models became instant casualties of Penzias and Wilson's discovery. A thermal radiation field with a temperature of 3 K couldn't be formed in either of them.

Proponents of an initially cold Friedmann universe were also surprised. Lifshitz's (1946) theory of the growth of density fluctuations in a Friedmann universe had shown that thermal fluctuations in a uniform gaseous medium were many orders of magnitude too small to evolve into self-gravitating systems. To overcome this difficulty Zel'dovich (1962) suggested that an initially cold cosmic medium would solidify when its density reached approximately one tenth the density of water. Then, as it continued to expand, it would break up into solid chunks large enough to cohere under their internal gravitational attraction.

The path that led me to Zel'dovich's hypothesis was different. In 1951 I was a postdoctoral fellow in Ann Arbor, working on problems in atomic physics, when I came across a copy of Otto Struve's (1950) book *Stellar Evolution*. I was especially intrigued by Struve's account of binary stars and theories of their origin. Though half the stars in our neighborhood belong to binary or triple systems, neither of the two main hypotheses for the formation of binaries – the fission hypothesis and the capture hypothesis – could account for this fact. It occurred to me that if stars had formed in close proximity to one another – if the cosmic medium had once been a uniform distribution of strongly interacting protostars – then, as the medium continued to expand, most of the protostars would have ended up in small groups, the most stable of which would be binaries.

This thought immediately suggested to me that all self-gravitating systems might have been formed in this way, as clusters of smaller systems. The earliest stage in this process of hierarchical gravitational clustering would have been the formation of the smallest objects held together by their own gravity rather than by chemical cohesion. Clusters of these objects would evolve into planetary systems, clusters of these evolving systems would come together in larger self-gravitating clusters, and so on, up to galaxies, clusters of galaxies, and clusters of galaxy clusters. I wrote a short paper (Layzer 1954) in which I argued on the basis of this picture that the Solar System could have evolved from a cluster of marginally self-gravitating chunks of matter. I argued that this picture could explain why satellite systems like those of Jupiter and Saturn mimic the Solar System.

But it was just a picture, not a theory. Atomic physics was still the focus of my research. I hadn't studied general relativity nor read Lifshitz's (1946) seminal paper. I knew that the universe was expanding, and I assumed (correctly but for no good reason, then) that self-gravitating systems were not expanding with it. And that was the extent of my knowledge. So I began to study general relativity, with a view to acquiring more insight into the interplay between the disruptive tendency of the cosmic expansion and the tendency of overdense regions to contract.

Zel'dovich, in his 1962 paper, had used a theory of the growth of cracks in a stressed solid to estimate the sizes of the primordial fragments. His aim was to show that random (square root of N) fluctuations in a uniform distribution of these fragments would be large enough to evolve into self-gravitating systems. My approach centered on energetic considerations. Its aim was to understand not just how an initially uniform cosmic medium could ever become unstable against the growth of density fluctuations but to understand how it could become and remain unstable against the growth of density fluctuations on progressively larger scales. I reasoned that because the gravitational interaction has no inherent scale, gravitational clustering would have to be a self-similar process. Thus a log–log plot of (primordial) binding energy per unit mass against cluster mass would have to be a straight line, extending from the smallest self-gravitating systems to clusters of galaxy clusters. Observational evidence supported this conclusion; and the predicted slope of the relationship (based on a theory developed in Layzer 1968 and 1975, my 1968 Brandeis lectures in Layzer 1971, and my book *Cosmogenesis*, Layzer 1990) agreed with the observed slope. Moreover, the theory predicted a coincidence first pointed out, I believe, by Fred Hoyle (1953): the gravitational binding energy per unit mass of our own planetary system (and, presumably, others as well) is approximately equal to the chemical cohesion energy per unit mass of a typical solid (and of solid hydrogen).

By 1965 most of this work had been done, though not all of it had been published. So I greeted Penzias and Wilson's announcement with mixed feelings. Like most people who had opinions on such matters, I found the experimental findings and their interpretation convincing. Also like most people, I recognized that they would have momentous consequences for cosmology. At the same time, I felt pretty confident that the picture of hierarchical gravitational clustering was essentially correct. So I had to face the question: Can the existence of a thermal radiation background with a temperature of 3 K be reconciled with the picture of gravitational clustering in a cold universe?

If, as most people assumed, the background radiation was the remnant of a primordial fireball, its almost precisely thermal character would be easy to understand. On the other hand, if it was created by the burning of hydrogen into helium later in the history of the universe, two conditions would have to be met. The universe had to have been opaque to the background radiation (at the temperature it had then). And the mass density of hydrogen converted into radiation had to be less than the closure density. These conditions work in opposite directions. The farther we go back in time, the easier it is to construct conditions under which the universe will be opaque to radiation at the appropriate temperature. But because the energy per unit mass of the radiation field diminishes like the reciprocal of the cosmic scale factor, the second condition puts a lower limit on the epoch at which the radiation could have been created. Could both conditions be met?

A quick and dirty calculation suggested that they might be – though it would be a tight squeeze. So there seemed to be no reason to abandon the scenario of gravitational clustering in a cold universe – at least not yet. But to survive, the scenario needed to pass more stringent tests.

In the cold universe, as in standard hot models, helium is formed during an early era of nucleogenesis. Following a preprint by Jim Peebles, Michele Kaufman (1970) studied under what conditions this could be done in an initially cold universe. Her results were promising, but left unanswered a key question: Would helium created in an early cold universe be subsequently transformed into still heavier elements? Subsequently, Anthony Aguirre (1999) devised reasonable cosmological models that are cold enough to solidify at the appropriate time but warm enough to prevent helium from being consumed in the production of heavier nuclei.

Can the background radiation be adequately thermalized in an initially cold universe? The most recent calculations, again by Aguirre (2000), indicate that the answer is yes.

An attractive feature of the cold universe scenario is that it requires a large fraction of the (ordinary) matter in the universe to be nonluminous. For in the cold universe, the background radiation is produced by an early generation of massive (and supermassive) stars, whose ejecta supply both the dust that thermalizes the radiation and the nonluminous matter that makes itself known through its gravitational effects. This is attractive because it makes the existence of dark matter/missing mass a necessary feature of the universe, required by the production of the background radiation. And it makes two testable predictions. It predicts that the dark matter is ordinary matter and it predicts a small range of possible values for the ratio between dark matter and bright matter.

Recent observations of the microwave background and of the redshifts of distant galaxies seem hard to reconcile with the cold universe scenario. On the other hand, the standard hot scenario still lacks a compelling account of the origin of self-gravitating systems in the expanding universe. Whatever our views on the issue of hot versus cold – unlike most of my colleagues I remain an agnostic – we can all agree that Penzias and Wilson's discovery has changed not just the face but the character of theoretical and observational cosmology.

4.9.3 Michele Kaufman: Not the correct explanation for the CMBR

Michele Kaufman is a scientist in the Ohio State University departments of Physics and Astronomy. Her current research uses the Very Large Array of radio telescopes, the Hubble Space Telescope, and the Spitzer Space Telescope.

When I was an undergraduate, I heard Dr Tommy Gold say in a public lecture that the density and temperature of intergalactic gas were uncertain by factors of 10^{12}. Later, as a graduate student at Harvard in 1964, I started research under David Layzer's supervision by calculating the expected radio-to-microwave background radiation produced by a combination of emission from discrete extragalactic radio sources and intergalactic free-free emission. The goal was to try to place limits on the amount of intergalactic ionized hydrogen. I included the effect of self-absorption. An earlier paper on the intergalactic free-free spectrum by Field and Henry (1964) had omitted self-absorption.

Before the Penzias and Wilson (1965a) result was widely announced, Arno Penzias visited Princeton, MIT, and Harvard, and at Harvard, he was directed to talk with me. Thus I learned that Penzias and Wilson had measured the background radiation at 4.08 GHz. This provided my model with an important constraint on the values of the intergalactic electron temperature and density, and in the summer of 1965 I published a paper in *Nature* on this with the conclusion that intergalactic free-free emission could account for the background measured by Penzias and Wilson (Kaufman 1965). This paper attracted some attention as the then only published alternative to fossil thermal radiation from a hot big bang. After the microwave background was measured at other frequencies, it was clear that intergalactic free-free emission was not the correct explanation for the CMBR. Reviews of the CMBR continued to reference my 1965 paper as a suggestion that did not pan out.

I later switched research areas from cosmology to galaxies, especially individual spiral galaxies. My research in the past 25 years has included detailed studies of spiral tracers in the grand-design spiral M81 and detailed multi-wavelength studies of galaxy pairs involved in grazing, prograde encounters (with Debra and Bruce Elmegreen). Our HST image of NGC 2207/IC 2163, part of the latter study, has appeared everywhere in the national news media, including the front page of *The New York Times* as well as scholarly journals (Elmegreen *et al.* 2006).

4.10 Measuring the CMBR energy spectrum

4.10.1 Jasper V. Wall: The CMB – how to observe and not see

Jasper Wall served as Director of the Royal Greenwich Observatory and of the Isaac Newton Group of Telescopes, La Palma. He is now Visiting Professor, University of Oxford, and Adjunct Professor, University of British Columbia.

In 1965 Donald Chu, Allan Yen and I made extensive sky brightness measurements at 320 and 707 MHz. Comparison told us that something was wrong with the zero point, wrong by the same few degrees at each antenna and at each frequency. Here is the story.

Engineering was in my blood, via father and grandfather. I grew up in the Ottawa Valley, in a happy and stimulating household in which the mantra was "This works so well we must take it apart to see why." Clocks, toasters, cars, plumbing, house electrics, lawn mowers, washing machines, hi-fi; nothing was safe from my Dad and his two young sons. Inevitably it was off to do Engineering at Queen's University, from where I graduated in 1963. But well before 1963 I had found the conventional branches of engineering to be less interesting than I had wished. I headed off into Engineering Physics, great training for applied research postgrad studies. But in what? I had spent a couple of summers at the National Research Council in Ottawa, working in the radio astronomy group. It seemed to me at the time that astronomy was perhaps of passing interest and might offer decent engineering challenges. The astronomy got me in the end, but the engineering background paid rich dividends at various times in my later professional life. The immediate challenge was radio astronomy instrumentation, which I set out to do in a Master's degree program in the Department of Electrical Engineering at the University of Toronto, starting autumn 1963.