Finding The Radiation from the Big Bang

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Preface

Many contributed to this book. The list begins with colleagues who in informal conversations now only vaguely recalled led us to appreciate the two reasons why we have a story worth telling: this is a substantial advance in science, and it is a close to unique opportunity for a near saturation of recollections of what happened.

All the main steps in this advance — the detection and identification of the fossil radiation from the big bang — have been clearly and accurately presented in histories of science. But these histories do not have the space to give an impression what it was like to live through those times. We sense a similar feeling of incompleteness in many histories of science written by physicists as well as by professional historians and sociologists. And there is a well-established remedy: assemble recollections from those who were involved in the work. We have been guided by a shining example in the broader field of cosmology, the collection of interviews in *Origins: the Lives and Worlds of Modern Cosmologists* (Lightman and Brawer 1990). We are indebted to Michael D. Gordin for instructing us on the existence of similar operations in other fields of science, and on the lessons to be drawn from them.

The close to unique feature of the recollections of early research on the big bang fossil radiation is the relatively small number of people involved. It means we could hope for complete coverage of recollections from everyone who was involved in a significant way and is still with us. We did not reach completeness: we suppose it is inevitable that a few colleagues would have well-established reasons for not wanting to take taking part. We are grateful that a substantial majority of everyone who was significantly involved in this slice of research in the 1960s and is still with us were willing to contribute their recollections.

The contributors are well along in life now, but they have not slowed down: all had to break away from other commitments to complete their assignments. We are deeply indebted to these people for taking the time and trouble to make this collection possible, and for their patience in enduring the chaos of assembly of the book.

We are indebted to participants also for their help in weeding out flaws in the introductory chapters, the ensemble of essays, and the glossary and bibliography that are meant to guide the reader through the essays. John Shakeshaft must be specially mentioned for his substantial reduction of the error rate, though he certainly does not share the blame for the remaining flaws in commission and omission.

Some steps toward the organization of this project ought to be recorded. Bernie Burke, Lyman Page, Jim Peebles, Tony Tyson, Dave Wilkinson and Bob Wilson met in Princeton on 9 February 2001, for an informal discussion over dinner of the story of the detection and identification of the fossil radiation. Wilson's written notes agree with Peebles' undocumented recollection of the general agreement that the story is complicated, and worth telling for that reason. But that enthusiastic agreement led nowhere; we all returned to other interests. In a second attempt to get the project started, George Field, Jim Peebles, Pat Thaddeus and Bob Wilson met at Harvard on 8 August 2003. That led to a proposal that was circulated to some 12 proposed contributors. (The number is uncertain because we have failed record keeping.) It yielded three essays — they are in this collection — but attention again drifted back to other things. The third attempt commenced with a chance encounter between Bruce Partridge and Jim Peebles in September 2005 at the Princeton Institute for Advanced Study. Our discussion led us to a blunt actuarial assessment: if the story were to be told in a close to complete way it would have to be done before too many more years had passed. That generated the momentum that led to completion of this project.

We sent a proposed outline of the book with an invitation to contribute to 28 people on 7 December 2005. The project continued to mature. As one might expect, the outline changed as we better understood what we were attempting to do. More unnerving is that, although we had given the list of contributors careful thought, we have continued to identify people who ought to contribute: we have some half dozen additions to the December 2005 list. A simple extrapolation suggests we have forgotten still others: we likely have not been as complete as we ought to have been. We hope those we inadvertently did not include will accept our regrets for our inefficiency. We hope all who did contribute to this book, in many ways, are aware of our gratitude.

Chapter 1. Introduction

This is the story of the discovery of thermal radiation that smoothly fills space. The radiation is a fossil, a remnant from a time when our universe was radically different from now, denser and hotter. Its discovery is memorable because, like other fossils, measurements of its properties tell us a good deal about the past.

The story of how this fossil radiation was discovered is memorable too for the complex set of considerations, in some cases overlooked for quite a while, in the many lines of research that led to the realization that this fossil exists, may be measured, and may inform us about the large-scale nature of the physical universe.

The complexity of this discovery story is well known. We suspect that is largely because the result was a big enough advance in a small enough subject then that people were led to look with particular care at how it happened. Look into the details of any other significant advance in science and you are likely to find a complicated story. That is, we believe our particular story offers some general lessons on how science actually is done. The essays in this volume tell what happened when the fossil radiation was recognized and first studied in the 1960s in the most complete way we can manage, by collecting remembrances of what they were thinking and doing from most of the scientists who were involved in this slice of research.

The stories of search and discovery in science that we tell each other usually are much too schematic to show what research is really like: they ignore all the wrong paths taken and the painstaking learning curves that experimentalists, observers and theorists follow in sometimes finding better paths. Scientists as well as historians and sociologists complain about the distortions, but our "tidied up" stories do serve a useful purpose in helping us keep track of the central ideas as well as in reminding us that our subject does have a history. As a practical matter this is about the best scientists can do: those who know the history best seldom are willing to take the time from research to tell it better. Even if they did, the rest of us would have little time to spare to read about it, and when we did we would find it difficult to pick out the threads that led to advances rather than dead ends. But a few examples that explore in all feasible detail what people remember doing are surely useful to working scientists, to historians and sociologists of science, and indeed to anyone who takes an interest in how we have arrived at our present understanding of the physical world.

The example presented in this book is the recollections of events in the 1960s that led to the detection, identification and exploration of the thermal cosmic microwave background radiation (the CMBR for short) left from the early stages of expansion of the universe, what is familiarly known as the hot big bang. This was a major step in the development of cosmology — the study of the large-scale nature and evolution of the physical universe — from the small science and limited observational basis of the 1950s to the big science it has become.

Few enough people were involved in research related to our narrowly defined topic during the relatively short span of time in the 1960s, and it happened recently enough, that we have been able to assemble recollections from most of them. These people have a broad variety of histories. A few continued this line of work after 1970, but most have gone on to other things. Some were led to work on cosmology and the CMBR in the 1960s by the elegance of the issues: does the world as we know it last forever, or if not does it end in fire or ice? Others were reluctant to get involved because the data one could bring to bear on such questions were so exceedingly limited. Some of these people were drawn into cosmology by the challenge of a particular measurement or calculation. Others became involved by accident, not realizing that their work would become important to the study of the evolving universe. We have descriptions of what it was like to be a student then, or to be further along into a career in science, along with accounts of how the contact with this subject shaped careers and lives. There were many opinions in the 1960s on what might be a reasonable model for the physical universe, and they were hotly debated. The advances in the observational evidence since then have greatly reduced the options, but these essays show a considerable range of opinions on how close we are even now to a full and accurate theory of the large-scale nature of the physical universe.

Our story cannot be complete because some of the actors are no longer with us. That includes Yakov Zel'dovich, who led a research group in the USSR that came close to the discovery of the radiation and, after its discovery, contributed much to the exploration of its significance. In the USA losses include Robert Dicke, Allan Blair and David Wilkinson. Bob Dicke suggested the search for this fossil radiation, using the technology he had invented two decades earlier. Al Blair with colleagues at the Los Alamos Scientific Laboratory was one of the pioneers in the measurement of the fossil radiation above the atmosphere. Dave Wilkinson, his colleagues and students, and in turn their students, have played a central role in the measurements of the properties of the radiation, from the time of its discovery and continuing through to the two spectacularly successful satellite missions, COBE and WMAP, that have given us precision measures that imply demanding constraints on the large-scale nature of the universe. In England we have lost the pioneers of the steady state cosmology, Fred Hoyle, Hermann Bondi and Thomas Gold, and a close associate, Dennis Sciama. In the late 1960s Sciama became persuaded by the evidence for a hot big bang, while Hoyle continued to lead the spirited exploration of alternatives to the relativistic big bang cosmology. We have recollections by close associates of some of these people; they are an important part of the story presented here.

The essays describe experimental, observational and theoretical work that follows a familiar and healthy pattern in science. On the empirical side, people were introducing new methods of observations in the 1960s. Equally important, they were building on earlier experience in experimental methods. Both aspects, the passing on of skills and the introduction of new ones, are part of the progress along the learning curve for how to deal with the many obstacles to the spectacular precison of present-day measurements of the CMBR.

On the theoretical side of cosmology, as of all physical science, we are guided by ideas of elegance. Our ideas of elegance are informed by what observation and experiment teach us, and the ideas in turn inspire new observations. This interplay of theory and practice has been a part of cosmology since the 1920s, though for many years the scant observational basis allowed considerable and perhaps even unhealthy room for theoretical debate. The big change in the 1960s that one sees described in the essays was the recognition that space is filled with a sea of microwave radiation whose properties can be examined and interpreted within ideas about the physics of the large-scale structure of the universe. That drove theorists along their own learning curves on how to characterize the universe that the measurements were revealing.

Research in cosmology in the 1960s was particularly confused because we were attempting to draw large conclusions about the nature of the universe from exceedingly limited data. Some at that time felt that cosmology was not likely to develop beyond a largely speculative subject. Among the more optimistic there naturally were considerable differences of opinion on how best to build a better science. The situation is very different now: popular directions of research in this subject are tightly directed by a theory that has passed searching experimental and observational tests. But there still is a confusion of opinions on the new frontiers of research, which focus on the study of how the theory may be better tested and improved. This confusion is characteristic of research in any branch of science, of course, and surely also of anything else people do that invites close attention. The confusion is apparent in the essays.

The counterpoint to the confusion of research in science is the development of webs of evidence that can become so tightly and thoroughly cross-checked that we can be confident they show us true aspects of an objective physical reality. It may seem particularly unlikely that we can establish a tight web of evidence about cosmology based on our limited view from our confined position in space and time. The physicist W. A. Fowler gave a sensible assessment of the hazards of this enterprise in the 1960s: "Within its limitations special relativity is faultless. Whether this be true of general relativity remains to be seen. Cosmology is mostly a dream of zealots who would oversimplify at the expense of deep understanding. Much remains to be done – experimentally, observationally and theoretically. *Relativity and Cosmology* — Robertson's legacy made manifest by Noonan — surveys the fruit of past endeavors and is an almanac for the harvests to come."

When he was writing this foreword to a book by Robertson & Noonan (1968) Fowler may have been aware of the detection of the microwave background radiation, though the book makes no mention of it. But in the mid-1960s Fowler was skeptical of the proposal that the radiation is a fossil from the past rather than the result of processes operating in the universe as it is now. He was right to be cautious, and he was right also to caution that the use of Einstein's general relativity theory to describe the large-scale nature of the universe is an enormous extrapolation from the tests of this theory, which at the time were not very demanding even on the length scale of the Solar System. If the observational and experimental basis for cosmology were as schematic now as it was in the 1960s the discovery of the cosmic microwave background radiation still would be an interesting development but perhaps much less important to science than it has proved to be. That is because the measured properties of this radiation form a considerable part of the web of evidence that now so tightly constrains ideas about the large-scale nature of the universe, including stringent tests of aspects of general relativity theory applied on the enormous scales of cosmology. Fowler predicted the present situation: much has been done and it has yielded the rich harvest that is surveyed in Chapter 5 of this book.

The essays describe work in the 1960s along the lines Fowler recommended: experimental, observational and theoretical. This was an ongoing part of research that already had a long history, of course, and the essays by and large assume the reader knows what happened earlier. Our two introductory chapters attempt to supply this information. Chapter 2 is a guide to basic concepts in relativistic cosmology: the meaning of the expansion of the universe, the behavior of thermal radiation in an expanding universe, and an inventory of what the universe in its present state contains in addition to the CMBR. We describe in Chapter 3 the lines of research that led up to the situation in the 1960s discussed in the essays. This account is selective: we pay particular attention to the developments in cosmology of concepts relevant to the thermal CMBR and the light elements left from the early hot stages of expansion of the universe. The chapter concludes with a broader assessment of the state of the theory and practice of cosmology in 1960: the observations and ideas that were most closely discussed and those that might have merited more attention.

Our introductory chapters are presented in the standard style for scientists: we almost exclusively rely on what appears in the published scientific literature, and we present it as a generally linear and orderly advance of knowledge. That is not the whole story by any means: we have omitted all the wrong steps that no longer seem relevant and all the other rough places that the essays are meant to illustrate. But as we have indicated this is a well-tested and efficient way to present the main elements of the science.

We have attempted to make the introduction intelligible to interested nonspecialists. There are equations, for the pleasure of those who like them, but they are not needed: the text is meant to convey the ideas to those who prefer words. Experts may find the science familiar, but unless they have long memories they would be well advised to look over Chapter 3, because the situation in cosmology in the early 1960s was very different from today.

The style in Chapter 4 is an abrupt change from our simplified linear description of what came before to the chaos of remembrances of what actually happened in the 1960s. Our guidance to contributors in the first round of invitations is summarized in the statement that we

> invite your account of personal experiences. What did you know then about cosmology and what did you think of it as a branch of physical science? What issues of research or lines of thought led you by plan or serendipity to be involved with the idea of a primeval fireball (as it was then called)? What were your reactions to the discovery of the radiation, and what effect did the discovery have on your research?

One could do better by going into the field to add interviews to the essays, and maybe even dig through notes and letters, though none of that is a practical plan for us. Lightman and Brawer (1990), in *Origins: the Lives and Worlds of Modern Cosmologists*, interviewed several of the people who have contributed to these essays, and their questions are similar to ours, though not confined to as narrow a range of time and topic. They had the advantage of being able to ask a series of follow-up questions. But one may respond differently in an interview and an invitation to write an essay, and we think we see the difference in the comparisons of what people who appear here and in *Origins* have to say. An analog of the follow-up question in an interview is the sharing of recollections of dates and events by some of our contributors. Apart from gentle hints, and a few corrections of well-established points, we have not contributed to this interaction or otherwise attempted to enhance the content or coherence of the essays. In science one seeks significant patterns in complex situations. The reader has the opportunity of applying this tradition to the set of essays.

The essays are informed by a considerable variety of philosophies of the theory and practice of science. To that we must add the random aspects of what happened to be each contributors' research interests at the time, their present choices of what they considered relevant for this story, and the accidents of what they happen to remember or be able to recover from fragmentary records. We have attempted to guide the reader through the confusion of the essays by offering the more linear — though less accurate — history of what happened up to 1960 in chapters 2 and 3. We also offer a glossary with summary definitions of terms — including the inevitable jargon — along with more detailed and technical discussions of some of the elements of the science. The glossary is meant to serve as an index to guide the reader to the relations among ideas and issues discussed in the essays and the introductory and concluding chapters. We offer references to the scientific literature for those who want to get into the really technical details. The citations are by the names of the authors and the date of publication, and the references are listed in the bibliography at the end of the book. The page numbers at the end of each reference in the bibliography serve as a supplementary index.

We have tried to make our guide to the science accessible, but we know it is not easy reading. A gentler but still authoritative introduction is in Steven Weinberg's (1993) *The First Three Minutes*. Helge Kragh's (1996) *Cosmology and Controversy* surveys the rich history of research in this subject in more detail for more topics, and it is based on a greater variety of sources. We think of Kragh's style as intermediate between our spare presentation in Chapters 2 and 3 and the full-blown details and complexities of the essays in Chapter 4. The reader will find that the essays are not fully concordant with these other accounts, careful though they are, or even with each other. History is complicated.

By 1970 it was clear that the cosmic microwave background radiation is real and therefore interesting. But it was not at all obvious then that this radiation would prove to be a key part of the present remarkably detailed and well-checked network of evidence on the large-scale nature and evolution of the observable universe. Some of the essays comment on these later developments. Chapter 5 presents a more systematic assessment of the outcome of the work that commenced in the 1960s or earlier: how later research has built on and added to what is described in the essays, and what we have learned. Chapter 4 offers an example of how science is done. Chapter 5 offers an example of the remarkable power of science to inform us about aspects of physical reality.

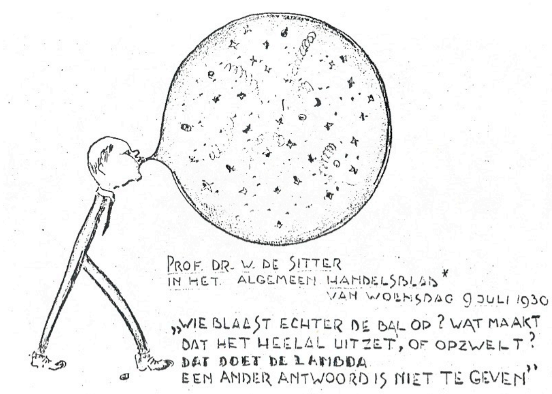

Figure 1: A sketch of Willem de Sitter on the occasion of his explanation of the idea of an expanding universe in a Dutch Newspaper in 1930. His body is sketched as the Greek symbol lambda, or λ, which represents Einstein's cosmological constant. As will be discussed this constant was taken seriously then and is back in fashion.

Chapter 2. A guide to cosmology

The universe is observed to be close to homogeneous and isotropic in the large-scale average.[1] That means we see no preferred center and no edge to the distribution of matter and radiation, and what we see looks very much the same in any direction. Stars are concentrated in galaxies, such as our Milky Way. The galaxies are distributed in a clumpy fashion that approaches homogeneity in the average over scales larger than about 30 Megaparsecs (30 Mpc, or about 100 million light years, or roughly one percent of the distance to the furthest observable galaxies).

Space between the stars is filled with a sea of electromagnetic radiation with peak intensity at a few millimeters wavelength and with spectrum — the intensity at each wavelength — characteristic of radiation that has relaxed to thermal equilibrium at a definite temperature, in this case $T = 2.725$ K. This thermal radiation is much more smoothly distributed than the stars, but its temperature does vary slightly across in the sky. (The temperature differs by about one part in 100,000 at positions in the sky that are separated by one degree.) The evidence developed in this book is that this radiation is a fossil remnant from a time when our expanding universe was much denser and hotter, and that the slight temperature vari-

[1] This situation is termed the cosmological principle. It is an assumption that Einstein (1917) introduced and is now observationally well supported.

ations originated by its interaction with matter as the galaxies grew by the gravitational attraction of matter out of a very close to homogeneous early mass distribution.[2]

We offer in this chapter a guide to basic ideas behind the interpretation of the radiation. We begin by explaining the concept of a universe that is homogeneous and expanding in a homogeneous and isotropic way. Section 2.2 describes the meaning of thermal radiation and its behavior in this expanding universe. In the concluding section we present a list of the main known forms of matter and radiation in the universe as it is now. This inventory figures in the analysis of the properties of fossil remnants from the early stages of expansion of the universe: the thermal radiation and the isotopes of the light chemical elements. Early work on the properties of these fossils is described in the essays. That is part of the developments that have led to the present state of understanding described in Chapter 5.

2.1. *The expanding universe*

The expansion of the universe means that the average distance between galaxies is increasing. Figure 1 shows an early use of a model that helps illustrate aspects of the situation. Imagine you live in only two spatial dimensions, on the surface of a balloon. Do not ask what is inside or outside the surface — you are confined to your two-dimensional space on the rubber sheet of the balloon. In your two-dimensional space you see a uniform distribution of galaxies: there may be local clustering, as we observe in the real universe, but the mean number of galaxies per unit volume is the same everywhere. As the balloon is blown up the galaxies move apart. Another caution is in order here: the galaxies themselves are not expanding. An observer at rest in any galaxy sees that the other galaxies are moving away, at the same rate in all directions, as if the observer's galaxy were at the center of expansion of this model universe. But an observer in any other galaxy would see the same motion of general recession in all directions. The key point illustrated here is that this model universe is expanding but has no center of expansion: it is happening everywhere in the two-dimensional space. In the cosmology of our universe an observer in any galaxy in our

[2] The distributions of mass and this thermal radiation are seen to be homogeneous by the special class of "comoving" observers who are at rest relative to the mean motion of the matter and radiation around them. An observer moving with respect to this frame sees gradients in the distributions of matter and radiation. This is not a violation of relativity theory, which of course allows observation of relative motion, in this case motion relative to the comoving rest frame defined by the contents of the universe.

three-dimensional space sees the same effect: the other galaxies are moving away.

A little thought about this expanding balloon model may convince you that an observer at rest in a galaxy sees that galaxies at greater distance r from the observer are moving away at greater speed v, following the linear relation

$$v = H_0 r. \tag{1}$$

The same argument and linear relation applies to the expansion of our three-dimensional universe.

Equation (1) is called Hubble's law, after Edwin Hubble (1929), who was the first to find reasonably convincing evidence of this relation. The constant of proportionality, H_0, is called Hubble's constant. (In the standard cosmology this constant of proportionality changes with time.)

The speed of recession, v, is inferred from the Doppler effect. Motion of a source of light toward an observer squeezes wavelengths, shifting features in the spectrum of the source toward shorter — bluer – wavelengths, while motion away shifts the spectrum to the red, to longer wavelengths. The spectra of distant galaxies are observed to be shifted to the red, as if the light from the galaxies were Doppler shifted by the motion of the galaxies away from us. This is the cosmological redshift.

You will recall from the balloon model that in this expanding universe an observer in any galaxy would see the same pattern of redshifts, and hence also observe Hubble's relation $v = H_0 r$. It is of course a long step from the observation that the light from distant galaxies is shifted to the red to the demonstration that all observers in our universe actually see the same general expansion. The role of the thermal radiation that fills space in testing this idea is a topic that appears through this book.

A numerical measure of the redshift is the ratio of the observed wavelength $\lambda_{\rm obs}$ of a spectral feature in the light from a galaxy to the wavelength $\lambda_{\rm em}$ of emission at the galaxy. In an expanding universe the ratio $\lambda_{\rm obs}/\lambda_{em}$ is greater than one. Astronomers subtract one from this ratio, defining the cosmological redshift as

$$z = \frac{\lambda_{\rm obs}}{\lambda_{em}} - 1. \tag{2}$$

Thus when the redshift vanishes, $z = 0$, the wavelength is unchanged.

The redshift z does not depend on the wavelength of the spectral feature used to measure z. That means we can define a single measure of the wavelength shift by the equation

$$1 + z = \frac{\lambda_{\rm obs}}{\lambda_{em}} = \frac{a(t_{\rm obs})}{a(t_{\rm em})}. \tag{3}$$

The radiation was emitted from the galaxy at time $t_{\rm em}$ and received by the observer at the later time $t_{\rm obs}$. The parameter $a(t)$ defined in this equation serves as a measure of how the wavelength of radiation moving from one galaxy to another is changing now and has changed in the past.

Now let us consider how distances between galaxies change with time. As the universe expands the distance d between a well-separated pair of galaxies increases. Very conveniently, the theory says that the distance is stretched in the same way as the stretching of the wavelength of light moving from the one galaxy to the other. That means the distance between the galaxies — any two galaxies — is increasing as $d(t) \propto a(t)$.[3] Thus we call $a(t)$ the expansion parameter. When its value has doubled the mean distance between galaxies has doubled. It follows that the mean number density of galaxies is decreasing as $n(t) \propto 1/a(t)^3$ (as long as galaxies are not created or destroyed). In short, if we knew $a(t)$ we would have a measure of the history of the expansion of the universe. It is an interesting exercise for the student to calculate the rate of change of the distance $d(t)$ between a pair of galaxies in terms of $a(t)$, check that the result agrees with Hubble's law in equation (1), and find Hubble's constant H_0 in terms of the present values of $a(t)$ and its first time derivative. The rest of us may move on.

The present standard cosmology is described by Einstein's general relativity theory, the currently accepted — and so far very successful — theory of gravity. The use of this theory in the early days of cosmology was speculative, because there were no significant observational tests. But the theory strongly influenced people's thinking, as follows.

In general relativity theory the acceleration — the second time derivative — of the expansion parameter $a(t)$ in equation (2) satisfies the equation

$$\frac{d^2a}{dt^2} = -\frac{4}{3}\pi G\rho a + \frac{1}{3}\Lambda a. \tag{4}$$

This second derivative is a measure of the rate of change of the rate of expansion of the universe. In this equation Newton's constant of gravity is

[3]To reduce confusion we note again that the galaxies themselves are not expanding: they are bound by gravity. Also, gravitationally bound clusters of galaxies are not expanding. The general expansion refers to the increasing distances between galaxies that are well enough separated that we can ignore the local clumping of mass in galaxies and clusters of galaxies.

G and the mean mass density averaged over local irregularities is ρ. The minus sign in front of this mass density term signifies that the gravitational attraction of the mass tends to slow the rate of expansion of the universe. Einstein's cosmological constant, Λ, in the last term is mentioned in the caption in figure 1. (The style has changed: people nowadays write it as an upper case Greek lambda, Λ, reserving the symbol λ for wavelength. Note that the artist drew λ backwards from the current convention.) If Λ is positive it opposes the effect of gravity. If Λ is positive and large enough it causes the rate of expansion to increase. The evidence is that this is the situation in the universe now.

Einstein (1917) found that his original form of general relativity theory, without the Λ term, cannot apply to a universe that is homogeneous and, as he supposed, unchanging. You can see that from equation (4): if the universe were momentarily at rest then in the absence of the Λ term gravity would cause the universe to start collapsing. That led Einstein to adjust the theory by adding the cosmological constant term, which he could choose so that the right-hand side equation (3) vanishes. That allows the static universe that made sense to him (since he was writing before Hubble's discovery). In this universe the cosmological constant "balances" the attraction of gravity and the universe thus is neither tending to expand nor contract. It takes nothing away from Einstein's genius to note that he overlooked the instability of his model universe: a slight disturbance would set it expanding or contracting (or more generally would cause some parts to expand and others to contract, eventually making the universe much more clumpy than is observed).

Alexander Friedmann (1922), in Russia, was the first to show that general relativity theory allows Einstein's homogeneous universe to expand or contract, but he had the misfortune to do it a few years before there was a hint from astronomical observations that the universe is in fact expanding. The Belgian Georges Lemaître (1927) rediscovered Friedmann's result and recognized that it means Einstein's static universe is unstable. Lemaître also saw that the expansion of the universe might account for the astronomers' discovery that the spectra of galaxies are shifted toward the red, perhaps by the Doppler effect. Figure 1 (page 13) shows de Sitter's explanation of Lemaître's idea. De Sitter is quoted as saying, "what causes the balloon to expand? That is done by the lambda. Another answer cannot be given." De Sitter is explaining Lemaître's idea that the universe was in Einstein's static condition, and that some disturbance had allowed the Λ term to push the universe into expansion.

Lemaître (1931) soon saw that the expansion could instead trace back to an exceedingly dense early state that he termed the primeval atom. The evidence developed in this book is that the universe did expand from a state that was dense, as Lemaître proposed, and hot. We will use the more familiar term for it, the hot big bang.[4]

People soon recognized that the expansion of the universe does not require the cosmological constant, provided you are willing to live in a universe that expanded from a big bang. Einstein accordingly proposed that we do away with the Λ term. He has been quoted as saying that his introduction of Λ was his greatest blunder. We suppose Einstein meant that if he had stayed with his original theory, and kept to the idea that the universe is homogeneous, he could have predicted that the universe is evolving: expanding or contracting. It is a curious historical development that Einstein's cosmological constant has come back in style, for reasons indicated in the essays and Chapter 5.

If the Λ term did not prevent it then general relativity theory predicts that there was a time in the past when the expansion parameter $a(t)$ in equation (2) vanished. The effect may be easier to see qualitatively by imagining the expansion of the universe running backward in time. The distances between galaxies are smaller in the past, and approach zero as $a(t)$ approaches zero going back in time. This means there was a moment in the past when the density of matter was arbitrarily large. If we ignore for a moment the decelerating effect of gravity and the effect of Λ, we can even see from equation (1) that this moment of formally infinite density happened at time H_0^{-1} in the past, or about 10 billion years ago.[5] It is conventional to speak of this past moment as the beginning of the history of the universe as we know it, at the moment when $a = 0$. Many suspect that better physics to be discovered, perhaps within the concept of cosmological inflation, will remove this singularity, and teach us what happened "before the big bang," or "at the big bang," or whatever is the suitable term.

In the early 1960s another world view was under discussion. In the steady state cosmology proposed by Bondi and Gold (1948) and Hoyle (1948) matter is continually created — at a rate that would be unobservably small in the laboratory — and collects to form young galaxies that fill the space that is opening up as older galaxies move apart. The mean distance between galaxies — about 10 million light years (or about 3 Mpc) for large ones like the Milky Way — thus would stay constant. The universe on the whole would not be changing: there would be no singular start to the expansion

[4]Hoyle is said to have coined the name big bang in 1950.

[5]For this reason H_0^{-1} is called the Hubble time, or the Hubble length measured in light travel time.

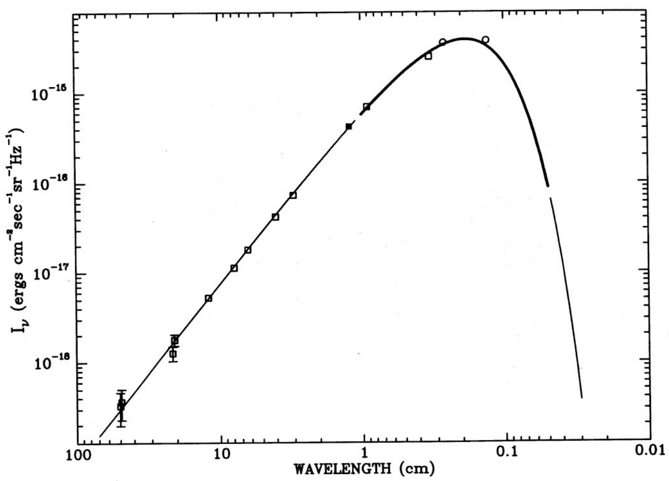

Figure 2: The spectrum of radiation that uniformly fills space. It is called the cosmic microwave background radiation, or CMBR, because the intensity is greatest at microwave wavelengths. The thin line in this figure is the theoretical Planck blackbody spectrum of radiation that has relaxed to thermal equilibrium at temperature $T_0 = 2.725$ K. The thick line running over the peak is the measurements by the COBE and UBC groups. The symbols represent other measurements at more widely spaced wavelengths.

and no end of the world as we know it. In the early 1960s there was lively debate on the relative merits of the steady state and big bang pictures. The debate was settled by the discovery of the sea of thermal radiation that fills space and, we now know, is almost certainly a fossil remnant from a time when the universe was very different from now.

2.2. *The thermal cosmic microwave background radiation*

A warm body radiates: you can feel the thermal radiation from a hot fire. In a closed cavity with walls at a fixed temperature the thermal heat radiation relaxes to a spectrum — the intensity of the radiation at each wavelength — that is uniquely determined by the temperature of the walls. The time it takes for the radiation to relax to this thermal spectrum depends on how strongly the walls absorb and emit radiation. If the walls are perfectly absorbing — black — the relaxation time is comparable to the time it takes radiation to cross the cavity. That suggested a commonly used name: blackbody radiation is radiation that has relaxed to thermal equilibrium at a definite temperature. The thin line in Figure 2 shows the spectrum of blackbody radiation at temperature 2.725 degrees kelvin above absolute zero. This is the spectrum of the thermal radiation — the CMBR — that fills our universe.

Max Planck discovered the first successful theory for the spectrum of blackbody radiation in 1900; it was also the first step to the discovery of quantum physics. Tolman (1931) noticed that radiation in a homogeneous universe could relax to a thermal spectrum, if there were enough matter to absorb and reemit the radiation energy often enough to cause it to relax to equilibrium. In effect, the whole universe could be the blackbody "cavity." He also showed that the expansion of a homogeneous universe would cool the radiation. Most important, Tolman showed that once the radiation has relaxed to thermal equilibrium the expansion of the universe preserves the characteristic blackbody spectrum, with no further need for matter to promote or maintain thermal equilibrium. The expansion of the universe causes the temperature to decrease in inverse proportion to the expansion parameter in equation (2), that is,

$$T \propto a(t)^{-1}. \tag{5}$$

Once blackbody the radiation stays that way; only the temperature of the radiation changes as the universe expands. This is the essential signature: if the spectrum of radiation filling our universe is close to thermal we have evidence that conditions in our expanding universe were at one time right for relaxation to thermal equilibrium.

Figure 2 shows measurements of the intensity of the radiation that uniformly fills space at wavelengths near 3 mm. The thick black line running over the peak shows measurements of the intensity at a densely sampled range of wavelengths. These measurements were made above the atmosphere, to avoid radiation from molecules in the air, independently from the NASA COBE satellite (Mather et al. 1990) and from a UBC (University of British Columbia) rocket flight (Gush, Halpern & Wishnow 1990). The measurements are very close to Planck's blackbody spectrum over a wide range of wavelengths.

The universe we see around us is close to transparent at wavelengths near the peak of this radiation. We know that because distant galaxies that are sources of radio radiation are observed at these wavelengths. This means that the universe as it is now cannot force radiation to relax to the distinctive thermal spectrum shown in Figure 2. And this means that the universe has to have evolved from a very different state, one that was hot and dense enough to have absorbed and reradiated the radiation, forcing

Table 1. Cosmic Mass Inventory

Category		Components	Totals
the dark sector			0.954
	dark energy	0.72	
	dark matter	0.23	
thermal big bang remnants			0.001
	electromagnetic radiation	0.00005	
	neutrinos	0.001	
baryons			0.045
	diffuse plasma	0.042	
	stars	0.0020	
	atoms and molecules	0.0008	
	stellar remnants	0.0006	
stellar radiation			0.000004
	electromagnetic	0.000001	
	neutrinos	0.000003	
gravitational radiation			0.00000003

it to relax to its blackbody spectrum. That is, we have evidence that this cosmic microwave radiation is a fossil from a different state of the universe. Contrary to the classical steady state cosmology, the universe we see around us is not forever: it is expanding and cooling from a very different early state.

One learns from fossils what the world used to be like. The fossil thermal microwave background radiation is no exception: we have learned a lot from the close study of its properties. The thermal radiation has also played an important dynamical role in determining the history of the universe, including the thermonuclear reactions that produced light elements in the early stages of expansion and the dynamics of the growth of the mass clustering that we observe as galaxies and concentrations of galaxies. The study of both aspects, the radiation as a signature of what things were like and as a dynamical player in what happened, are recurring themes in this book. Our discussion of these themes commences with an inventory of the other important dynamical players. What does the universe contain in addition to the fossil thermal radiation?

2.3. *What is the universe made of?*

The world is full of many things, and we surely have discovered only a small part of them. But we do have credible evidence about what things are made of and about the relative amounts of types of mass involved. Table 1 lists contributions to the total mass of the universe by some of the more important types of matter and radiation.[6] The numbers are fractions of the total: each column adds to unity (within rounding errors). They are known as density parameters. The last column in the table lists the fractions of the mass in five main categories. The middle column shows mass fractions in a finer division of components within categories. The total mass is such that, within general relativity theory, space sections at constant world time are not curved. Spacetime is curved, but space sections at constant time have Euclidean geometry.

People and planets and stars are made of baryons, with enough electrons to keep the electric charge close to neutral. The baryons include protons and neutrons in the various combinations that make up the atomic nuclei of the chemical elements. The mass in the inner parts of our Milky Way galaxy is largely in baryons in stars. The same is true of the central parts of the other large galaxies. The outer regions of the galaxies contain plasma, but the mass is largely dark matter that is not baryonic. In the average over much larger scales the biggest contribution is shown as the entry for the first component in the table, dark energy. This is the new name for Einstein's cosmological constant, Λ.

The gravitational action of dark energy is illustrated in Figure 1 (page 13). In general relativity theory the positive pressure of a fluid adds to the gravitational attraction produced by the mass equivalent of its energy. Near the end of the life of a massive star the pressure grows large, and that contributes to its final violent relativistic collapse to a black hole. The tension in a stretched rubber band is in effect a negative pressure. This negative pressure slightly reduces the gravitational attraction produced by the mass associated with the energy of the rubber. Einstein's Λ acts like a fluid that has near constant energy density, and pressure that is negative and large enough in magnitude that its gravitational effect overwhelms the gravitational attraction of the energy. The result is a contribution to the gravitational field that pushes matter apart. (It is best left as an exercise for the student to see why this push has little or no effect on how the dark energy itself is distributed, and why the negative pressure allows the energy density in this component to remain close to the same value at every point in space as the universe expands.) The name, dark energy, comes from the intuition felt by many that Λ has something to do with an actual energy

[6]This table is adapted from Fukugita and Peebles 2004, who discuss the observational basis for these mass estimates and their uncertainties. This paper also gives estimates of the smaller mass fractions in a considerable variety of other components.

density, and that, like other forms of energy, Λ need not be exactly constant. But all we can say with confidence is that this term is needed to make sense of the evidence collected in this book and reviewed in Chapter 5.

The second component in the table is dark matter. It acts like a gas of particles that move freely, apart from the effect of gravity. Fritz Zwicky (1933) was the first to notice the dark matter effect. He showed that the observed mass in stars in the Coma Cluster of galaxies (so called for the constellation in which it appears in the sky) is much too small to gravitationally confine the motions of the galaxies. (The motions are deduced from the Doppler shifts of the galaxy spectra.) It seemed unlikely that the cluster could be flying apart, because the distribution of galaxies near the center of the cluster is smooth and quite compact. But what might be holding the cluster together?

We now know that Zwicky's effect applies to the other rich clusters of galaxies: the galaxies in a cluster are moving too rapidly for the cluster to be held together by the mass seen in the galaxies. The same applies to the motions of stars and gas in the outer parts of individual galaxies outside clusters. The mass that is needed to hold clusters together, and to do the same for the outer parts of individual galaxies, used to be known as "missing mass." It is now termed dark matter, but we still do not know what it is, apart from one clue. The evidence developed out of work described in this book is that the dark matter cannot be baryons, for that would contradict the successful theories for the origin of the light elements and of the galaxies. The evidence we have now fits the idea that the dark matter is a gas of freely moving nonbaryonic particles. Discovering the nature of these mystery particles, and the nature of the dark energy — Einstein's Λ — is a wonderful opportunity for search and discovery by the next generation.

The second category in the table is the thermal electromagnetic radiation and neutrinos left from the hot big bang. The radiation — the CMBR — has the spectrum shown in Figure 2 (page 19). This radiation now contains about 400 thermal photons per cubic centimeter. The mass equivalent to the mean energy of one of these photons is so small that the radiation mass density adds only a trace to the total. But you will recall that the cosmological redshift (shown in eq. [2]) reduces the photon energy as the universe expands. In the early universe the thermal photons were energetic enough that their mass density was the largest contribution to the total. (This is discussed in more detail in the footnote on page 29.)

The energetic photons in the early universe took part in the creation and annihilation of neutrinos by the reactions to be discussed in the next chapter. That would have produced a thermal sea of neutrinos. The number of neutrinos plus antineutrinos in each of the neutrino families is now 4/11 times the number of thermal photons, or roughly 100 neutrinos per cubic centimeter at the present epoch. The present energy density is larger in these fossil neutrinos than in the radiation, because the neutrinos have rest masses. (The neutrino masses are not yet tightly measured. The number entered in the table for the fraction of the mass of the present universe contributed by these neutrinos is thought to be accurate to a factor of two or so. We can be sure that there is not enough mass in the known families of neutrinos to serve as the dark matter. We need another kind of mystery particles.)

The third of the categories is the baryons. The total mass density in this form is inferred from arguments that are again developed through this book. Most of the baryons must be in the form of diffuse plasma, because any other physically reasonable state would have been observed. There is a trace amount of this diffuse plasma in the disks of spiral galaxies such as the Milky Way. There is a larger amount in hot plasma in clusters of galaxies, and a still larger amount in coronae of plasma gravitationally bound to the outer regions of individual galaxies. There also is diffuse plasma spread through the enormous spaces between the galaxies. The relative amount in the last two forms is not yet well measured.

The second component in the baryon category in Table 1 is that in stars that are radiating energy from burning hydrogen in their central regions. The stars in the nearly spherical bulges of spiral galaxies such as the Milky Way generally formed when the universe was much less than half its present age. Most of the stars in elliptical galaxies also are old. The stars in the disk of the Milky Way have a broader distribution of ages. Stars are still forming in disks and in lower mass irregular galaxies such as the Magellanic Clouds, largely out of the neutral atoms and molecules entered as the third component in this category. But the overall rate of star formation is markedly decreasing from what it was when the universe was half its present age. There still is a large mass of baryons in the diffuse plasma, but it is cooling too slowly to supply baryons for ongoing star formation at the past rate.

As the energy supply in a star is exhausted, some baryonic matter is ejected in stellar winds and explosions and some is left in stellar remnants: white dwarfs, neutron stars, and black holes. The last component in the baryon category is an estimate of what has accumulated in these stellar remnants. There are baryons in many other fascinating forms, including planets and people, but they are thought to contain a very small fraction of the total.

The fourth category is the accumulated energy released by stars in the forms of electromagnetic radiation — starlight — and neutrinos. The larger amount in neutrinos is a result of the copious emission accompanying the collapse of dying massive stars. The fifth category is an estimate of the energy density in gravitational radiation produced in the formation of black holes. Several of the contributors to Chapter 4 are keenly interested in detecting this gravitational radiation, but that is another story.

As we have said, the tasks of discovering the physical natures of dark energy and dark matter are Golden Apples for future generations. One of our tasks for the rest of this book is develop the lines of reasoning and observation that have led to the conclusion that we do have credible evidence that these dark components really exist. We begin in the next chapter with an account of the early development of ideas that led to the identifications of two very helpful fossils from the early universe, the thermal cosmic microwave background radiation and the isotopes of hydrogen and helium.

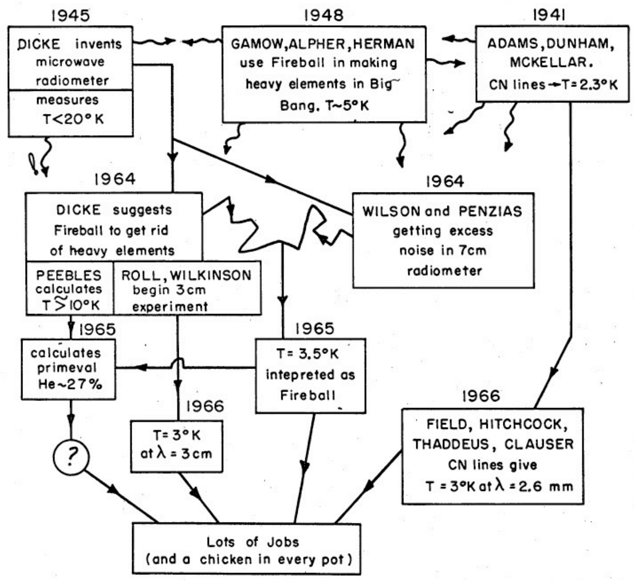

Figure 3: This illustration of how the CMBR was and could have been identified was made in 1968 by David Wilkinson with other members of the Princeton Gravity Research Group.

Chapter 3. Origins of the Cosmology of 1960

To understand the essays you have to appreciate the nature of research in cosmology in the early 1960s. To understand the nature of this research you have to consider its history. The illustration in Figure 3 of the major steps leading to the identification of the CMBR as a fossil remnant from the big bang was made by members of Robert H. Dicke's Gravity Research Group. David Wilkinson, who was its main author, used it in lectures on cosmology starting in 1968 (and it is a good illustration of his style). This figure was eventually published in Wilkinson and Peebles (1983).

The figure maps relations among the topics we discuss in this chapter. This is a complicated map because the story is complicated, but there are two major themes. We begin with the first of these, the development of the idea that the relative abundances of the stable isotopes of the lightest elements, hydrogen and helium, were determined by thermonuclear reactions in the early hot stages of expansion of the universe (with modest adjustments for what happened in stars much later). We consider next a subsidiary theme, the line of thought that led Roll and Wilkinson to search for the

CMBR. We then turn to the second major theme, the development of the means of detecting and measuring the properties of the radiation left from the hot big bang. We conclude this chapter with a summary assessment of what people were thinking and doing in research in cosmology at the start of the time of the essays.

3.1. *Nucleosynthesis in a hot big bang*

In the 1930s people were exploring two ideas about where the chemical elements might have formed, in stars or in a hot big bang. The former was suggested by the growing evidence that the Sun and other stars radiate energy released by the fusion of atomic nuclei into heavier nuclei. One could imagine that the heavy elements produced in stars by this nuclear burning were ejected by stellar winds or explosions and the debris collected in new stars and in planets like Earth. In the other picture one assumes that temperatures and densities in the early stages of expansion of the universe were large enough to have forced nuclear reactions among atomic nuclei to produce elements heavier than hydrogen. The amount of element production would be determined by the temperature and density and by how rapidly the hot early universe expanded and cooled. As it has turned out, the now well tested theory is that the heavier elements originated in stars while most of the helium and lighter atomic nuclei are fossil remnants of the hot big bang, along with the thermal CMBR. We review here the main steps in the development of the latter concept. Alpher and Herman (2001) also describe this history and present recollections of the work by them and colleagues in the 1940s and 1950s on the introduction of main features of the concept. The essays in Chapter 4 add to the story.

Early discussions of the hot big bang picture assumed that the relative abundances of the chemical elements and their isotopes had relaxed to thermal equilibrium at some hot early state of expansion of the universe. (An example is the analysis by Chandrasekhar and Henrich 1942.) The concept can be compared to that of blackbody radiation. At thermal equilibrium the intensity of the radiation is determined by just one quantity: the temperature. At equilibrium the relative abundances of the elements and their isotopes are fixed by two quantities, the temperature and the density of matter. As the universe expanded and cooled thermal equilibrium would have shifted to favor an increasing proportion of the heavier elements. A large proportion of heavy elements is not acceptable, however: hydrogen is the most abundant element, helium amounts to about 25% by mass, and only about 2% of the baryon mass is found in heavier elements. Thus in this picture the nuclear reaction rates would have to have been fast enough to force relaxation to equilibrium at high temperatures, in the early stages of the expansion, and then slow enough to have broken away from the equilibrium as the universe expanded and cooled.

The physicist George Gamow took the lead in improving this line of thought. Gamow's 1942 paper points to two reasons to doubt that one should assume thermal equilibrium ever obtained for the elements in the early universe. First, there is no temperature at which the observed abundances of the elements agree with an equlibrium distribution. Second, general relativity theory predicts that at the high densities of the very early universe the rate of expansion is rapid (as you see in eq. [4]: when the mass density ρ is large the rate of expansion has to be large). That means that instead of relaxation to equilibrium one must consider the rates of those nuclear reactions that can occur rapidly.

Gamow (1946) repeated these arguments and made another point: if free neutrons were abundant in the early universe they would react rapidly with protons and heavier atomic nuclei, as wanted. (That is because neutrons have no electric charge. The positive electric charges of atomic nuclei tend to slow their fusion by pushing the nuclei apart.) Thus he proposed that the heavy elements might have been formed by the capture of neutrons followed by nuclear β decays that convert neutrons to protons (accompanied by the emission of electrons, or what is known as β radiation).

The paper by Alpher, Bethe and Gamow (1948) presents the first analysis of this neutron-capture idea. Ralph Alpher was Gamow's student, and this work is the topic of the published version of his doctoral dissertation at the George Washington University (Alpher 1948). Hans Bethe's name was added to produce an approximation to the first three letters of the Greek alphabet.

Central parts of this neutron-capture concept figure in the now standard and successful theory for the origin of the lightest elements — hydrogen, deuterium and helium — in the hot big bang. They also figure in the theory of the formation of the heaviest elements, but transferred from the hot big bang to exploding stars — supernovae — in what has become known as the r-process. This was a memorable advance. But we must also consider the introduction of several other important ideas.

The Alpher-Bethe-Gamow paper was submitted for publication on February 18, 1948. On June 21 Gamow submitted another paper (Gamow 1948a, with more detail in Gamow 1948b) that presents the first discussion of the role of thermal radiation in element formation in the early rapidly expanding universe.

Gamow's argument begins with the remark that at high enough temperatures atomic nuclei are broken up into protons and neutrons (as was noted also in the thermal equilibrium picture for element formation). In Gamow's dynamical picture the build-up of elements would start with the capture of neutrons by protons to make deuterons (the nuclei of the stable heavy isotope of hydrogen). Each capture would be accompanied by the release of a photon (a quantum of electromagnetic radiation, at this energy usually written as γ), in the reaction

$$n + p \leftrightarrow d + \gamma. \tag{6}$$

The two-headed arrow means the reaction can go either way: a sufficiently energetic photon can break up a deuteron. Gamow noted that the critical temperature for the survival of deuterons and hence their accumulation is

$$T_{\rm crit} \sim 10^9 \text{ K}. \tag{7}$$

At higher temperatures radiation breaks up deuterium as fast as it forms. When the temperature falls below $T_{\rm crit}$ the dissociation reaction going from right to left in equation (6) markedly slows because the cooler radiation does not have many photons energetic enough to break apart deuterons. This means deuterium starts to accumulate. As it does the deuterium can rapidly burn to helium by particle exchange reactions.

The key point introduced in Gamow (1948a) is that at the critical temperature $T_{\rm crit}$ in equation (7), and at the matter density that would produce a reasonable element production, the total mass density is dominated by the energy density of the thermal radiation that would accompany the hot plasma.[7] Another important point, noted earlier in Alpher, Bethe and Gamow (1948), is that when the temperature has dropped to $T_{\rm crit}$ the number density of baryons — neutrons and protons — must be large enough to allow some accumulation of deuterium, but not so large that it burns

[7] The mass density in radiation is smaller than in matter now, as indicated in Table 1. But at the time of helium formation the mass in radiation is the largest component. That is because the energy in each photon, and its equivalent in mass, is decreasing as the universe expands. The ratio of the number densities of baryons and photons is close to constant, so the mass density in radiation is decreasing faster than the mass density in radiation. In more detail, the wavelength of a CMBR photon is increasing as $\lambda \propto a(t)$, where $a(t)$ is the expansion parameter in equation (2). Thus the photon energy is decreasing as $\epsilon = h\nu = hc/\lambda \propto a(t)^{-1}$, where c is the velocity of light and h is Planck's constant. The numbers of baryons and photons per unit volume are decreasing as the volume of the universe increases, in proportion to $a(t)^{-3}$. So the mass density in baryons varies as $\rho_m \propto a(t)^{-3}$ while the mass density in radiation varies as $\rho_r \propto T^4 \propto a(t)^{-4}$. When the temperature was $T_{\rm crit}$ (eq. [7]) the mass density was dominated by the radiation. The mass density sets the time elapsed from a really hot beginning to $T = T_{\rm crit}$, $t_{\rm crit} \simeq 100$ s.

an unacceptably high fraction of the hydrogen into heavier elements.[8] This consideration led Gamow to conclude that if the neutron capture picture for element formation is right then when the temperature in the early expanding universe had fallen to $T_{\rm crit}$ the number density of baryons — neutrons and protons — would have to have been $n_{\rm crit} \sim 10^{18}$ cm^{-3}.

Alpher and Herman (1948) corrected numerical errors in Gamow's analysis, bringing the baryon density at $T_{\rm crit}$ to $n_{\rm crit} = 8 \times 10^{16}$ cm^{-3}. More important, they pointed out that when the subsequent expansion of the universe had lowered the baryon density to

$$n_0 = 1 \times 10^{-8} \text{ cm}^{-3}, \tag{8}$$

their estimate of the present value,[9] the blackbody radiation temperature would have cooled to the present value

$$T_0 = 5 \text{ K}. \tag{9}$$

This is strikingly close to what is now well measured,

$$T_0 = 2.725 \text{ K}. \tag{10}$$

[8]More formally, Gamow's condition is $\sigma_{\rm crit} n_{\rm crit} v_{\rm crit} t_{\rm crit} \sim 1$, where $\sigma_{\rm crit}$ is the radiative capture cross section for eq. [6], $v_{\rm crit}$ is the relative neutron-proton velocity, and $n_{\rm crit}$ and $t_{\rm crit}$ are the baryon number density and expansion time, all evaluated when the temperature is $T = T_{\rm crit}$.

[9]Alpher and Herman (1948) do not state this quantity; it is derived from the numbers in their paper. Mass density estimates at that time, based on measures of galaxy masses and counts, were larger. Hubble (1936) estimated that the mean mass density is no less than $\rho_{\rm min} = 1 \times 10^{-30}$ g cm^{-3} and may be as large as $\rho_{\rm max} = 1 \times 10^{-28}$ g cm^{-3}. Alpher (1948) used Hubble's value $\rho_{\rm min}$, but Alpher and Herman used $\rho_{\rm min}/60$. If they had used $\rho_{\rm min}$ they would have predicted that the present radiation temperature is $T_0 = 20$ K. This is the bound on T_0 that Dicke, Beringer, Kyhl and Vane (1946) obtained in the measurement discussed on page 42. One may wonder how this larger predicted value for T_0 might have affected radio astronomers' motivation in the 1950s to search for this radiation (as discussed by Burke on pages 119 to 120). It is worth noting that Hubble's mass density estimates are large by a factor of 60 because his distance scale was underestimated by a factor of 7.6. After correction to the present distance scale his value for $\rho_{\rm min}$ is within a factor of two of the modern value for mass density in stars (Table 1 on page 21). That is consistent because he used observations of the luminous parts of the galaxies, which are dominated by the mass in stars. Hubble's larger estimate, corrected for the distance scale, is similarly close to the total mass density in Table 1. This is again consistent because Hubble used for $\rho_{\rm max}$ the mass per galaxy in clusters, which we now know contain a close to fair sample of the dark matter. Finally, we might note that the Alpher and Herman (1948) prediction of T_0 is close to the measured value because they used a value for n_0 (eq. [8]) that is fairly close to the modern value of the mean baryon density.

The details of the argument relating the abundance of helium to the temperature of the CMBR have since been refined, as will be described. But the Alpher and Herman consideration remains part of the standard cosmology.

Gamow recognized that the thermal radiation would not only be present but would remain an important dynamical actor after the early episode of element formation. (For example, the paper Gamow 1948a presents an estimate of the time well after element formation when the mass densities in matter and radiation were equal and, as Gamow argued, the expanding universe became unstable to the gravitational growth of concentrations of matter.) Alpher and Herman (1948) clearly recognized that the thermal radiation would be present even now. Alpher and Herman (1950) went so far as to convert the present temperature to the present mass density in radiation, producing the first estimate of the fifth line in Table 1. But they did not recognize that the radiation might be detected by methods to be described later in this chapter.

Yet another refinement of the theory of formation of the light elements in a hot big bang is the process that fixes the relative number n/p of the neutrons and protons that enter the first step of element-building in equation (6). In the paper Gamow (1948a) the value of n/p is left open: Gamow was content to establish orders of magnitude. Hayashi (1950), on the other hand, recognized that in this hot big bang cosmology n/p is determined by particle physics reactions, as follows.

When the temperature in the early stages of expansion of the universe was above about 10^{10} K, and the universe had been expanding for less than a second, the radiation was hot enough to produce a thermal sea of electrons and their antiparticles, positrons, and a sea of neutrinos and antineutrinos (ν and $\bar{\nu}$), mainly by the reactions

$$\gamma + \gamma \leftrightarrow e^+ + e^-, \qquad e^+ + e^- \leftrightarrow \nu + \bar{\nu}. \tag{11}$$

These particles convert protons to neutrons and back again by the reactions

$$p + e^- \leftrightarrow n + \nu, \quad n + e^+ \leftrightarrow p + \bar{\nu}, \quad n \leftrightarrow p + e^- + \bar{\nu}. \tag{12}$$

At temperatures above about 10^{10} K the reactions drive the ratio n/p of numbers of neutrons and protons to its thermal equilibrium value,

$$n/p = e^{-Q/kT}, \tag{13}$$

at temperature T.[10] Here $Q = (m_n - m_p)c^2$, where $m_n - m_p$ is the difference of mass of a neutron and of a proton, and k is the Boltzmann constant.

[10] In fact, n/p also depends on the lepton number, which is the sum of the numbers of e^-

Hayashi found that as the universe expanded and cooled below 10^{10} K the reactions in equation (12) slowed to the point that the value of n/p froze, and then n/p more slowly decreased as neutrons freely decayed to protons (by the last reaction in eq. [12]) going to the right). By the time the temperature had dropped to $T_{\rm crit}$ the ratio of neutrons to protons had fallen to about $n/p \sim 0.2$. In the standard cosmology most of the neutrons present at this time combined with protons to form deuterons, and most of the deuterium burned to the heavy isotope of helium ^{4}He, with a trace amount of ^{3}He.

The paper Alpher, Follin and Herman (1953) presents a detailed application of Hayashi's idea. Their analysis of how the ratio n/p of the numbers of neutrons and protons varies as the universe expands and cools is essentially the modern computation. Enrico Fermi and Anthony Turkevich (in work that is not published but is reported in Gamow 1949, ter Haar 1950, and in more detail in Alpher and Herman 1950, 1953) worked out the chains of particle exchange reactions that burn deuterium along with neutrons and protons to helium and trace amounts of heavier elements. These analyses complete the formulation of all the essential pieces of the established theory of the origin of most of the isotopes of hydrogen and helium.

The hot big bang has become the standard model because it fits demanding tests. In the 1960s it was not at all obvious that that would happen, however, and people were discussing alternative ideas. They are part of the story of how we arrived at a standard cosmology.

3.2. *Nucleosynthesis in alternative cosmologies*

The evidence developing in the 1950s was that the heavier elements were produced in stars. If so, might the stars also produce light elements? If that were so helium production in a hot big bang could be a problem: it might predict too much helium. But that can be fixed: one can adjust the prediction by adjusting the assumptions in the big bang model, or one can go to a steady state cosmology, for example. We review here some of the alternatives people were considereing. The point to notice is that these ideas

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and ν particles minus the numbers of e^+ and $\bar{\nu}$. The reactions in equations (11) and (12) do not change the lepton number: its value had to have been set by initial conditions very early in the expansion of the universe. Equation (13) assumes the absolute value of the lepton number density is small compared to the number density of CMBR photons. A positive and large lepton number suppresses n/p, and a strongly negative lepton number increases n/p. This point figures in the cold big bang model discussed later in this section. The present observational constraints are consistent with the small lepton number assumed in equation (13).

arguably are as elegant as the standard hot big bang. We need observations to show the way through the thickets of elegance. The following sections trace developments of the evidence.

Let us consider first what came of the idea that the heavy elements were formed in the big bang along with the light elements. A problem with this idea is that there is no stable atomic nucleus with mass 5 (that is, a total of five neutrons plus protons). That means the abundant isotope of helium, with mass 4, cannot capture a neutron and then another one and subsequently decay to an isotope of lithium by the emission of an electron. This strongly suppresses the build-up of elements heavier than helium during the rapid expansion of the early universe. Alpher (1948) remarks on the problem, and a like situation at mass 8, in the published version of his doctoral dissertation. The analysis mentioned above by Enrico Fermi and Anthony Turkevich failed to find a nuclear reaction that might carry significant nuclear burning in the early universe past the mass-5 gap. But Gamow (1949) noted a possible way out that is worth considering here even though we know now that it does not work.

Gamow's (1949) idea was that if the mass density in baryons when the temperature of the universe was $T_{\rm crit}$ were much larger than previously considered, and n/p were much smaller (as was later realized follows from Hayashi's 1950 analysis of eq. [12]), then after all the neutrons had combined with protons to form heavier elements a substantial fraction of the baryons would be left as protons, the nuclei of hydrogen atoms. That agrees with what is observed. The larger matter density in the early universe would cause faster nuclear burning of deuterium, and perhaps push nuclear burning past mass 5.

Hayashi and Nishida (1956) presented an analysis of this idea. They considered the possibility that the baryon number density at a given temperature is about a million times what is assumed in Gamow (1948a) and Alpher and Herman (1948). That lowers the present temperature of the CMBR by a large factor, which Hayashi and Nishida would not have counted as a problem because the CMBR was not known. They took account of the helium-burning reactions

$$^4\mathrm{He} + ^4\mathrm{He} \leftrightarrow ^8\mathrm{Be} + \gamma, \qquad ^8\mathrm{Be} + ^4\mathrm{He} \leftrightarrow ^{12}\mathrm{C} + \gamma, \tag{14}$$

which by then were known to be important in the evolution of stars after all the hydrogen in the central regions had burned to helium. In this "cool" big bang model universe, Hayashi and Nishida found significant production of carbon and oxygen. The deuterium abundance coming out of this model is much too small, according to what is now known, and the helium abundance is too large, though not by a large factor. (That is because almost all the neutrons that survive to the time when the temperature has fallen to $T_{\rm crit}$ are burned to helium, and the value of n/p when deuterium starts accumulating is not very sensitive to the density of matter.)

This cool big bang universe produces helium in an amount that might not have seemed unreasonable at the time. It also produces a not insignificant amount of heavy elements. Layzer and Hively (1973) pointed out that the heavy elements produced in a cool big bang might form grains that were able to absorb and reradiate starlight effectively enough to produce the thermal CMBR spectrum out of starlight. Here is an example of an idea that is interesting but was not pursued, and, we now know, is not viable. The light element abundances are wrong, and it cannot account for the relation between the large-scale distributions of matter and the CMBR that is discussed in Chapter 5.

Zel'dovich (1963, 1965) proposed a cold big bang cosmology, in which element production is left entirely to the stars. He was led to this by his interpretation of the observational evidence[11] to mean that the background radiation temperature "does not exceed 1° K (Ohm 1961)" and that "the initial helium content was below 10-20%." Smirnov (1964, at At Zel'dovich's suggestion, according to Smirnov's acknowledgment) reanalyzed element production in a hot big bang. Smirnov showed that the small primeval helium abundance could be accommodated in the hot big bang picture by lowering the matter density at a given radiation temperature, but that that that would imply an unacceptably large abundance of deuterium. (At Smirnov's lower density, neutrons and protons combine to form deuterium, but the burning of deuterium to helium is incomplete.) Zel'dovich (1965) noted another apparent problem for the cool case: the radiation temperature at the present matter density is unacceptably large. We now know that the CMBR temperature and helium abundance both are larger than Zel'dovich had supposed, and they are consistent with each other within the hot big bang. But Zel'dovich was led to propose an alternative.

In Zel'dovich's cold model the very early universe contained equal number densities of protons, electrons and neutrinos, all very nearly uniformly distributed, and with energies that are as low as possible. This means one

[11] Osterbrock (page 63) describes what was known then about the helium abundance. The temperature estimate Zel'dovich mentioned was based on the Project Echo measurement discussed by Hogg, Novikov and others in chapter 4. Zel'dovich's reaction to news of the identification of the CMBR is described by Novikov (page 70) and is illustrated also by Zel'dovich's letter to Dicke quoted on page 132.

has to consider the effect of the exclusion principle that limits the allowed number densities of electrons and neutrinos at a given energy. The high density of electrons in the early universe would cause the electrons to fill all their available states up to a large energy. But the energetic electrons cannot force themselves onto protons to make neutrons (by the first reaction going to the right in eq. [12]) because that requires the production of neutrinos, and in this picture all the neutrino states with the energy allowed by the reaction are already taken.[12] In this universe star formation would commence with nearly pure hydrogen. This is yet another example of an interesting universe that proves not to be the one we live in.

Hoyle and Tayler (1964) knew that the helium abundance is large, and greater than seemed reasonable for production in stars. They too reconsidered the hot big bang model, but they also pointed to another possibility. In the steady state cosmology the universe always has been as it is now: there would be no fossil helium. Hoyle and Tayler suggested that the helium could have been produced in the "little bangs" of very massive exploding stars. The local evolution of a very hot exploding star is similar to the evolution of an expanding universe, and element formation is similar too. This is another interesting universe that we now know is not ours. Like the cool big bang, it cannot account for the thermal CMBR and its spatial distribution.

Yet another alternative, that could eliminate fossil helium but not the fossil thermal radiation, was inspired by the following considerations. A measure of the relative strengths of the gravitational and electromagnetic interactions is the ratio of the gravitational and electric forces of attraction between an isolated electron and proton,

$$\frac{f_{\rm grav}}{f_{\rm el}} = \frac{G m_e m_p}{e^2} \sim 10^{-40}. \tag{15}$$

The charges of a proton and electron are $\pm e$, and G is Newton's gravitational constant. Since both forces vary in the same way with the separation of the particles (on laboratory scales) this ratio does not depend on the separation. Its very small value led Dirac (1938) to ask whether the strength of the gravitational interaction might be decreasing: maybe gravity is weak now because the universes is old. Dicke liked Dirac's proposal: to him it

[12]Another way to put this is that Zel'dovich assumed the lepton number mentioned in footnote 7 is positive and large enough to force the equilibrium ratio of neutrons to protons at high density and low temperature to a value close to zero. Zel'dovich's idea of adjusting the cosmic lepton number can be extended to a hot big bang model; it changes the relation between the helium abundance coming out of the big bang and the CMBR temperature. The evidence now is that the lepton number is negligibly small.

fit the idea that local physics depends on the large-scale nature of the universe. Perhaps gravity physics is determined by the large-scale distribution of matter, and the thinning of the mass as the universe expands decreases the strength of gravity. Dicke termed this idea Mach's principle, after Ernst Mach's (1883) proposal that inertial motion is not absolute but rather is determined relative to the motion of matter at large. Dicke (1964) reviews these ideas. They inspired the proposal that general relativity be adjusted to a scalar-tensor gravity theory in which the number in equation (15) decreases as the universe expands (Brans and Dicke 1961). Dirac's argument independently led Pascual Jordan to the same theory and to his own considerations of the observational consequences (Jordan 1962).

Among the consequences of the Jordan-Brans-Dicke theory would be a considerable increase in the rate of expansion of the early universe relative to general relativity. That would change the relation between the CMBR temperature and the thermonuclear production of chemical elements in the early universe. If the expansion were rapid enough there would be no particle reactions. This universe could have the observed present CMBR temperature and baryon mass density and no light element formation outside stars. This is another idea that fascinates but failed. (Ideas tend to have long lives, however. The Jordan-Brans-Dicke theory, and the idea that numbers such as the one in eq. [15] may vary with time, still figure in debates on early universe physics.)

3.3. *Thermal radiation from a bouncing universe*

The experiment that led to the recognition of the thermal cosmic microwave background radiation, and helped us sort out ideas about the nature of the early universe, was inspired by a line of thought we have not yet mentioned: perhaps our expanding universe bounced from a previous state of collapse. As we will explain, the bounce might be expected to have filled space with thermal radiation — the CMBR.

Lemaître (1933) expressed the feeling that, from a purely aesthetic point of view, a universe that successively expands and contracts to exceedingly small size has "un charme poétique incontestable et faisaient penser au phénix de la légende." Wheeler (1958) asked whether the bounce in an oscillating universe might be compared to "a glove which is turning itself inside out one finger at a time." And Hoyle and Narlikar (1965) considered the idea that the universe is in a steady state overall, but that "pockets of creation" could set part of the universe into a local oscillation. That is, the concept of bouncing universe certainly was not ignored. But the important idea for our purpose is Tolman's (1934) point that a bounce likely would produce entropy, in the form of thermal radiation similar to what is now known to be present.

In the 1960s Robert Dicke (in an unpublished discussion that is described more completely in the essays) made Tolman's argument more tangible. Dicke noted that the nuclear burning of four protons — the nuclei of hydrogen atoms — into the nucleus of one helium atom in a star releases enough energy to produce about one million starlight photons. The burning of helium to heavier elements produces still more starlight. These starlight photons are shifted toward the red as the universe expands. If the expansion eventually stopped and the universe then collapsed back to high density the starlight photons would then be shifted toward the blue, to greater energy. If the blueshift were large enough then just a few blueshifted starlight photons would have enough energy to break apart each heavy atom, giving back the protons. These protons would serve as fuel for nuclear burning in new generations of stars in the next cycle of expansion and collapse. The rest of the starlight photons would be thermalized, that is, turned into the CMBR. A few hundred bounces could make the observed energy density in thermal radiation out of starlight, if the bounces conserved the numbers of neutrons, protons and photons.

We now know that, according to general relativity theory, a collapsing universe does not bounce. But there are two reasons to highlight Dicke's idea. First, we don't know that general relativity theory provides a full and complete description of the very early stages of the expansion of the universe. More to the point of this book, Dicke's line of thought was interesting enough that he was led to persuade two members of his Gravity Research Group, Peter Roll and David Wilkinson, to build an instrument capable of detecting a sea of thermal radiation. News of the Roll-Wilkinson experiment reached Arno Penzias and Robert Wilson, who had a detection of microwave emission they couldn't interpret. How that happened is recalled in the essays in the next chapter.

3.4. *Detecting the cosmic microwave background radiation*

We turn now to the observational and experimental work that led to the detection and eventual interpretation of the cosmic microwave background radiation.

A direct measurement of the intensity — or the equivalent (Rayleigh-Jeans) temperature — of a cosmic sea of photons employs a detector that measures the flux of incident radiation. From that one must subtract all the contributions from local sources in the ground, the atmosphere and our

Milky Way galaxy. There is another indirect approach that employs "thermometers" in the form of interstellar molecules. Both methods produced tantalizing clues to the existence of the CMBR well before it was identified. We begin with a description of the "thermometers," because they provided an actual measurement of the present temperature of the universe two dozen years before the radiation was recognized in 1965.

The indirect method is based on a result from quantum physics. The energy of an isolated object such as an atom or molecule has discrete possible values: there is a ground level with energy E_0, a first excited level with energy E_1, a second level at E_2, and so on. The energy levels of an object as large as a person would be fantastically closely spaced if we were able to truly isolate someone, but it can't be done: we are tightly coupled to our environment. The quantization of energy, however, is clear and distinct on the much smaller scale of atoms and molecules. In a dilute gas of molecules bathed in blackbody radiation at temperature T, the ratio of numbers in the first excited level and the ground level is given by the equation

$$n_1/n_0 = e^{-(E_1-E_0)/kT}. \qquad (16)$$

This has the same form as equation (13) for the equilibrium ratio of numbers of neutrons to protons in the early universe, but here applied at much lower energies and temperatures and much later in the history of the expanding universe.

The ratio n_1/n_0 for a species of molecules in interstellar space can be measured by comparing the strength of absorption of light from a background star by the molecules in the two energy levels. Starlight photons may be absorbed by a molecule in its ground level, with energy E_0, leaving the molecule in some highly excited level, with energy E_*. The photon has to supply the energy difference, $E_* - E_0$. From Planck's condition $E = h\nu$ we see that this absorption produces an absorption line at frequency $\nu_a = (E_* - E_0)/h$ in the spectrum of light from the star. A starlight photon with the lower frequency $\nu_b = (E_* - E_1)/h$ can be absorbed by molecules in the first excited level, leaving the molecule at energy E_*. This produces a second absorption line at ν_b. The ratio of the amount of absorption at the two frequencies is a measure of n_1/n_0. Since the energy difference $E_1 - E_0$ is known equation (16) gives us a temperature. We have a thermometer.

There is the problem that the temperature measured by the ratio n_1/n_0 is determined not only by the temperature of electromagnetic radiation bathing the molecules, but also by collisions with interstellar particles. The molecule cyanogen (CN, a carbon atom bound to a nitrogen atom) in interstellar matter has the useful property that it recovers quickly from collisions with particles, and its energy levels are well spaced for the measurement of radiation temperatures near that of the CMBR. (The energy difference $E_1 - E_0$ for cyanogen corresponds to the microwave wavelength 2.6 mm.) CN thus provides a very convenient thermometer.

McKellar (1941) used equation (16) to translate observations of absorption of starlight by interstellar CN molecules in the two lowest levels to the temperature

$$T \simeq 2.3 \text{ K}. \tag{17}$$

With hindsight, the inference we would draw is that interstellar CN molecules are bathed in the radiation field having this temperature. But the measurement was not so interpreted in the 1940s by either McKellar or by Adams (1941), who made the measurements. Herzberg (1950) comments that this temperature "has of course a very restricted meaning." The restriction he had in mind likely is that, as we have noted, the excited levels of CN might be populated by particle collisions rather than radiation. Some astronomers knew that that would require a curiously high particle collision rate, however, as will be discussed (page 59).

Astronomers are accustomed to dealing with complex situations that require them to remember many curious things. In the 1960s some remembered McKellar's temperature and recognized the possible relation to the hot big bang cosmology. How that happened is one of the threads running through the essays.

The direct detection of the CMBR requires a receiver operating at some frequency and an antenna to define the beam, that is, the range of directions in the sky from which the radiation is received. The amount of radiation detected must be corrected for the radiation that originates in the receiver and the thermal radiation emitted by our warm immediate surroundings, including the atmosphere. Two approaches to the unwanted "noise" radiation originating in the receiver figure in our story. One used low noise receivers developed at the Bell Telephone Laboratories for the purpose of communication. The other used a technique pioneered by Dicke: rapidly switch the receiver between the sky and a reference "load" that is producing thermal radiation at a known temperature. The difference subtracts the radiation originating in the detector. The time average beats down the noise in the difference signal. That leaves a measurement of the difference $T_s - T_l$ between the wanted sky temperature T_s and the known load temperature T_l. Dicke invented this radiometer in the 1940s as part of war research at the Radiation Laboratory at the Massachusetts Institute of Technology. An early application was the measurement of the absorption of microwave radiation by the atmosphere, which limited the push to radar at shorter wavelength which would have provided better resolution. (Since microwave emission from the atmosphere will become important later in our story, it is worth remarking here that if material at a nonzero temperature absorbs radiation then it also emits radiation. Wilkinson [page 139] emphasizes another point to bear in mind: the standard technique used by radio astronomers to measure the radiation received from an object is to compare the energy flux received when the detector beam is on the source and when the beam is directed to a point in the sky slightly off the source. The subtraction eliminates a lot of the noise from the sky, ground and detector. But it does not work for observations of the CMBR because this radiation is uniformly distributed across the sky.)

The essays describe how the CMBR was detected in measurements in New Jersey that used the Bell Laboratories low noise receivers. Penzias and Wilson found convincing evidence that this detection is real by using a cold reference load to establish a zero point for the measurements. The CMBR was detected again a few months later in an independent experiment, also in New Jersey and just 30 miles away, that used a Dicke microwave radiometer. This was the experiment by Roll and Wilkinson.

Figure 4 (from Lawson and Uhlenbeck 1950) is Dicke's illustration of the sensitivity of his radiometer. In this example, the reference load was at room temperature and the switching was done by a wheel that swings the load into and out of the waveguide connecting the detector and antenna. More recent measurements use electronic switching and loads with temperatures that are colder and more closely matched to the CMBR. A Dicke radiometer "sees" thermal microwave radiation wherever the horn antenna is pointed, whether at the ground, or people, or the atmosphere. The strip chart recording shown in Figure 4 indicates the change in response when the antenna was pointed at chimneys that were in use, and so slightly warmer than their surroundings. (Notice that the temperature scale is inverted: higher recorded temperatures are indicated as lower positions in the chart.)

The top line in Figure 4, measured with the antenna scanning at an angle of 75° from the zenith, indicates a more or less uniform temperature of about 125 K. Variations in the temperature from one part of the sky to the other are small, less than about 10 K. This means the instrument used at MIT was capable of detecting temperatures as small as 10 K. Note also that the temperature measured well away from hot chimneys increases as the angle from the zenith is increased from 75 to 90°. Some of the increase in detected temperature at the larger zenith angle is the result of microwave emission by the Earth's atmosphere. When the instrument is aimed closer to the horizon,

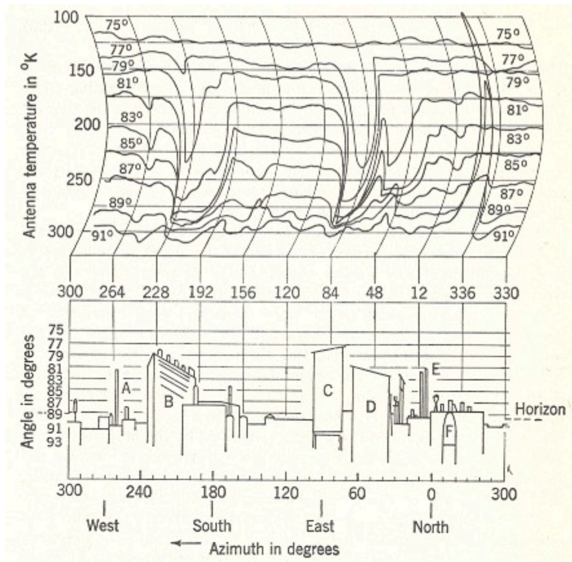

FIG. 5-3.—Optical and microwave radiometer panorama of the Cambridge, Mass., skyline from a point on Building 20, MIT. The labels on the curves of the upper diagram are the antenna angles measured from the vertical. Similarly, the angles indicated on the left side of the lower figure are measured from the vertical.
(A) Chimney of MIT powerhouse
(B) Radiation Laboratory, Building 24
(C) and (D) Sheds on Building 20
(E) Lever Brothers Company chimney
(F) Electrostatic accelerator, MIT

Figure 4: An illustration of the ability of a Dicke radiometer to detect thermal radiation. Below is a sketch of the skyline of Cambridge in 1945, seen from the roof of a temporary building on the MIT campus. Above is a strip chart recording of the response of the radiometer to the warm chimneys. A higher temperature, as in the direction of the building marked C, is recorded as a lower position on the chart.

it receives more atmospheric radiation because it is looking through a longer path through the atmosphere.[13] By measuring how the temperature varies with the zenith angle, one can extrapolate to what would be detected in the limit of no atmospheric emission. This would be the observed temperature of space beyond the Earth's atmosphere. We should add that some of the increase in measured temperature shown in Figure 4 results from what radio astronomers call "side-lobe pickup." A radio frequency antenna does not produce a sharply defined beam on the sky. Rather, there are subsidiary diffraction maxima, known as side lobes, that allow radiation to leak into the receiver from substantial angles away from the optical axis. As the zenith angle increases, these side lobes pick up more and more radiation from the

[13]In the approximation of the atmosphere as a plain-parallel slab of emitting material the detected atmospheric emission varies with the secant of the zenith angle.

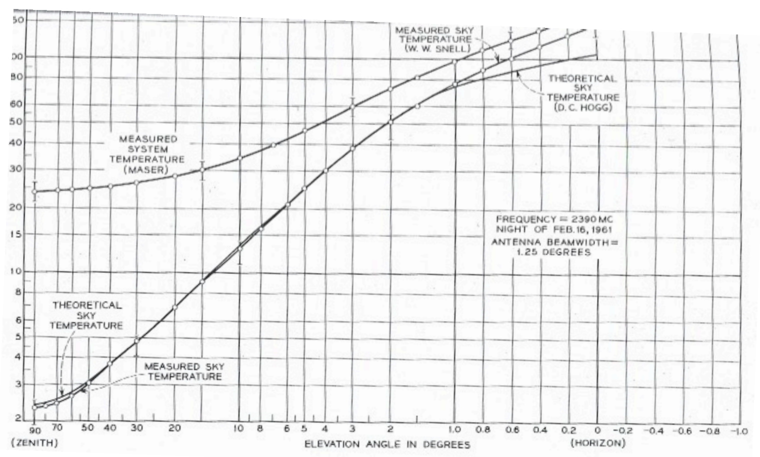

Figure 5: A tipping measurement (Ohm 1961). The top curve shows the sum of the microwave radiation flux from the detector, ground, atmosphere, and whatever comes in from above the atmosphere. The lower curve is the result of subtracting estimates of what came from the instrument and ground.

$\sim 300\,\mathrm{K}$ ground. Side lobe pickup bedeviled early attempts to measure both atmospheric emission and the CMBR spectrum.

Dicke, Beringer, Kyhl and Vane (1946) used a “tipping experiment” to establish that “there is very little ($< 20°$K) radiation from cosmic matter” at the microwave wavelength (~ 1 cm) they employed. Ironically, this paper appears in the same volume of the *Physical Review* as Gamow’s 1946 letter on nucleosynthesis. Gamow was not yet discussing a hot big bang, however, so it is not surprising that neither paper made reference to the possible significance of the other. The bound Dicke and colleagues placed on how hot space might be is well above what would be expected in the cool big bang situation Hayashi and Nishida (1956) later analysed. It is not far from the situation Gamow (1948a) had proposed and Alpher and Herman (1948) calculated, however, and it is close to what we now know is the temperature of the fossil radiation from the big bang, $T_0 = 2.725\,\mathrm{K}$. But the connection between what Dicke’s radiometer can measure and what might be expected from a hot big bang was not noticed for another two decades.

In the course of research on other subjects in the 1950s and early 1960s measurements equivalent to the Dicke *et al.* tipping experiment were repeated with better sensitivity, and the CMBR detected. Eventually the radiation was recognized. That happened first as a byproduct of research at Bell Telephone Laboratories to develop low noise maser amplifiers for receiving systems for communication (DeGrasse, Hogg, Ohm and Scoville 1959; Ohm 1961; Jakes 1963).

Figure 5 shows a particularly detailed tipping measurement from this communications program. These data are from Project Echo, which demonstrated communication by microwave signals sent from the ground and reflected back to the ground by a satellite (a large balloon with a conducting surface). The paper Ohm (1961) presents the following numbers. When the Echo receiver was pointed to the zenith it detected microwave radiation equivalent to blackbody radiation at temperature $T_{\rm system} = 22.2 \pm 2.2\,{\rm K}$ at 2390 MHz (12.6 cm wavelength). When the instrument was tipped away from the zenith the system temperature increased because it was looking through more atmosphere. From that variation one could estimate that the atmosphere contributed the equivalent of $T_{\rm atm} = 2.3 \pm 0.2\,{\rm K}$. That plus estimates of the radiation originating in the instrument and the radiation entering the horn antenna from the ground was, in this experiment, estimated to total $T_{\rm local} = 18.9 \pm 3.0\,{\rm K}$. The difference,

$$T_{\rm excess} = T_{\rm system} - T_{\rm local} = 3.3 \pm 3.7 \text{ K}, \tag{18}$$

is a measure of what might be entering the atmosphere from cosmic sources. This is a considerable improvement over the Dicke, Beringer, Kyhl and Vane (1946) bound, $T_{\rm excess} < 20\,{\rm K}$. The central value in equation (18) is consistent with the Alpher and Herman (1948) estimate of the Gamow condition (eq. [9]), within the uncertainties, and it is close to what we now know is the CMBR temperature (eq. [10]). The stated uncertainty in $T_{\rm local}$ on equation (18) is conservative because the errors were added in absolute value, but even if added in quadrature $T_{\rm excess}$ is formally consistent with zero.

The measurement was repeated in the Telstar Project that demonstrated transmission of a television signal from the ground to a satellite that reradiated the signal back down to the ground. Jakes (1963) reports that (at 7.3 cm wavelength) "The over-all system noise temperature was measured to be somewhat less than 17°K pointing at the zenith, which included about 4.5°K for waveguide losses, 2.5°K sky noise, 2.5°K for antenna side lobes and heat losses and 5°K for the maser." The sum and difference — which Jakes does not state — amounts to

$$T_{\rm excess} = T_{\rm system} - T_{\rm local} = 2.5 \text{ K}. \tag{19}$$

This is close to the central value of $T_{\rm excess}$ from Project Echo, and again close to what is now known to be the CMBR temperature.

The consistency of central values of T_{excess} from the Echo and Telstar systems did not force attention to the idea that there might be a detectable sea of extraterrestrial microwave radiation: a reader of these papers could imagine that local sources of radiation had been underestimated. That might be what Ohm (1961) had in mind in writing that 'the "+" temperature possibilities of Table II [listing local noise contributions] must predominate.' We know in hindsight that there is no need to assume the system temperatures in the Echo and Telstar systems were systematically underestimated; the contribution of radiation entering the horn antenna from the ground actually is an overestimate. All this became clear later in the 1960s when Penzias and Wilson added a low temperature calibrator to the Telstar system, for purposes they explain in Chapter 4. That made the difference T_{excess} between what was detected and what was expected from the instrument, ground and atmosphere a clear and pressing issue for them, and it soon became a pressing issue for the cosmology community.

Let us consider next how the reaction of the community was conditioned by the state of research in cosmology in the early 1960s.

3.5. *Cosmology in 1960*

In our experience the book *Cosmology* by Herman Bondi (in two editions, Bondi 1952 and Bondi 1960a) gives a good picture of research in this subject in the 1950s and early 1960s. Bondi reported the vigorous debate on the relative merits of the steady state and relativistic big bang cosmologies, and he assessed the state of observational tests of these and the other cosmologies then under discussion. And he showed us a vivid picture of the role of the philosophies that implicitly or explicitly inform our approaches to the theory and the observations.

Bondi also surveyed a broad range of issues about the fundamental basis for physical cosmology. Is the universe really close to homogeneous in the large-scale average? Are the redshifts of the galaxies really due to the expansion of the universe, as opposed to a "tired light" effect? Perhaps light tends to shift toward longer wavelength as it moves across the immense distances between the galaxies (Zwicky 1929). If the redshift is an effect of expansion how do we know the universe is evolving, as opposed to the idea that continual creation of matter is keeping it in a steady state? If the evidence were that the universe is evolving how would we know that the evolution is well described by general relativity theory? In an evolving universe would the laws of microscopic physics really be the same now and in the remote past when the universe is supposed to have been so very different? If the relativistic cosmological model were a good approximation what would be reasonable values for its parameters? Must we imagine that an evolving universe expanded from a formal singular state of arbitrarily high density, or might we suppose that we observe expansion now following a bounce that terminated an earlier collapsing state? If our universe is evolving how might it end, in a big freeze or a big crunch?[14] And does it make sense to allow the possible role for Einstein's cosmological constant that is illustrated in Figure 1 on page 13?

This is a disconcertingly long list of questions, but it does not mean that the cosmology Bondi described was an empty science. People were assembling observational evidence, in part out of simple curiosity, in part driven by the goal of testing theoretical ideas, and the observations were in turn driving theoretical developments.

Sandage's (1961) pioneering paper, *The Ability of the 200-inch Telescope to Discriminate Between Selected World Models*, explored the feasibility of testing cosmological models, and perhaps distinguishing between the steady state and big bang models, from the observations of galaxies at low and high redshift. The steady state cosmology has the distinct advantage for observers that it makes definite predictions: the galaxy populations at great distances, seen as they were in the past because of the light travel time, are supposed to be statistically the same as what is observed nearby. This means the counts and apparent magnitudes (brightness in the sky) of the galaxies are predicted functions of their redshifts. The big bang model has free parameters — space curvature and Λ — and it predicts that the galaxies observed at great distances are younger and statistically different from what we see nearby (though the expected degree of difference still is difficult to predict). The theoretical basis for the tests Sandage considered was understood in the 1930s. The new development in the early 1960s was the availability of the large collecting area of the 200-inch Hale telescope.

Sandage (1961) concluded that with the technology at hand the best way to distinguish between the steady state model and the family of big bang models was the relation between redshifts and absolute magnitudes of the most luminous galaxies, which seem to have close to the same luminosities. He presented an application of the redshift-magnitude test for observations that reach galaxies at redshift $z = 0.46$, meaning the universe has expanded by the factor 1.46 since the light we receive left the galaxy (as expressed in eq. [2]). This is an impressively deep probe out in space and back in time. But he concluded that, even with the power of the 200-inch telescope, distinguishing between the steady state and big bang models will be "difficult

[14] Bondi (1952, 1960a) avoids these terms, along with big bang.

and perhaps marginal."[15]

Another suite of issues, that generated considerable research activity in the early 1960s, concerned the ages of the stars, from the theory of stellar evolution, and the age of the elements, from the decay of long-lived radioactive isotopes. There was debate on three aspects of these time scales. First, in the standard big bang model the universe has expanded from densities and temperatures too large for stars to have existed. That means that in this cosmology the oldest stars are younger than the cosmic expansion time. Sandage (1961) concluded that the big bang cosmology can satisfy this constraint, but that it might require the postulate of a positive cosmological constant. Second, in the big bang model the epoch of galaxy formation might have ended by now, but in the steady state picture we expect to see around us a mix of young and old galaxies from the contimual creation of matter. There are apparently young galaxies nearby. Hoyle and Narlikar (1962) took that as an argument for the steady state picture. Gamow (1954), on the other hand, made the now generally accepted point that the colors of most nearby galaxies do not scatter much, consistent with a close to uniform age of most of the stars, and inconsistent with the steady state picture. Third, galaxies observed at great distance are seen as they were in the past, because of the time required for the light to reach us. Thus in the Big Bang cosmology galaxies observed at high redshifts would be expected to appear younger than nearby ones, while in the steady state cosmology the mix of young and old galaxies was the same in the past as it is now. That led Bondi (1960b) to state that if distant galaxies were observed to be systematically different from those observed nearby then "the steady-state theory is stone dead." In the early 1960s there was no credible evidence for a systematic difference between galaxies at low redshift and high. This was a good argument that was not widely acknowledged.[16]

[15]Modern applications of this redshift-magnitude test use the light from supernovae rather than galaxies, and reach expansion factors $1 + z$ close to 3. The apparently modest but deeply important increase in the distances the observations reach indicates that the redshift-magnitude relation is close to the steady state prediction. The task of explaining why this is now taken as evidence for the effect of the cosmological constant Λ illustrated in Figure 1, rather than as evidence for the steady state cosmology, is left to Chapter 5, which addresses the current and accepted answers to the list of questions in the second paragraph of this section.

[16]The situation began to change in the mid 1960s with the discovery of quasars, which are considerable more abundant at redshifts $z > 0.5$ than nearby. The interpretation proposed then, and now well checked, is that quasars were more abundant in the past (Longair 1966; Sciama and Rees 1966). The use of Hubble's law (eq. [1]) to convert quasar redshifts to distances certainly could be questioned (Terrell 1964; Hoyle 1965; Hoyle and Burbidge 1966). But that has since been checked by the demonstration that

Another issue under discussion in the early 1960s was the origin of the chemical elements. In the steady state cosmology matter is continually created. Bondi opined that creation of matter with the observed relative abundances of the chemical elements and their isotopes would be bizarre. That led him to conclude that a demonstration that the elements could not be produced — presumably in stars — in the universe as it is now would be strong evidence against the steady state cosmology, while a demonstration that the elements did come from stars would reduce the list of arguments for a big bang. Bondi (1952), in his first edition, had references to work on the theory of element formation in stars and in a hot big bang, but the discussion was brief. By the time of the second edition, Bondi (1960a) could report significant advances in the theory of element formation in stars (Burbidge, Burbidge, Fowler and Hoyle 1957; Cameron 1957). This important development was encouraging for the steady state philosophy. As Gamow (1956) noted, however, it was not necessarily a challenge for the big bang picture: one could image nuclear reactions in stars somewhat altered what came out of the big bang. But the abundance of helium offered a critical test.

Burbidge (1958) recognized that the helium abundance in the Milky Way is larger than might be expected from production in known types of stars in the numbers indicated by the stellar luminosity of the galaxy. He pointed out that some other galaxies are very luminous in radio emission, and that the energy released by conversion of hydrogen to helium might power these sources. He did not mention the possibility of helium production in a big bang. Gamow (1956) did: "the calculations in that direction, carried out by the present writer," (Gamow 1948) "and later in some more detail by Fermi and Turkevich," "lead to a value of the H/He ratio which is in good agreement with observational data." Gamow did not document the observational evidence he mentioned. In the second edition of his book, *Cosmology*, Bondi (1960a, p. 58) added this assessment of the situation:

> Since it has also been shown that any hot dense early state of the universe could not have left us any nuclei heavier than helium, the origin of such nuclei is no longer a question of cosmology.

when a quasar appears in the sky close to a galaxy at lower redshift the gas in the galaxy produces absorption lines in the quasar spectrum. The quasar clearly is behind the galaxy, as the conventional interpretation of its redshift indicates. Modern observations also show that more distant galaxies indeed appear distinctly younger, containing larger numbers of massive short-lived stars. And while the data on ages are much improved from what Sandage had in 1961, they still seem to require the postulate of a cosmological constant (or something that acts like one).

> It might however be said that the abundance of helium may conceivably be greater than would be accounted for by ordinary stellar transmutation and so might have to be explained on a cosmological basis, but the evidence as yet is far too slight to merit serious consideration now.

Bondi did not give references to the measurements of helium abundances. The big bang theory for helium formation is not mentioned in the text either, but there is a reference in the bibliography, to Alpher and Herman (1950).

Osterbrock and Rogerson (1961) presented measurements of the abundances of helium and heavier elements in the plasma around and between the stars and in the Sun. They concluded that the mass fraction, Y, in helium is considerably larger than the mass fraction in heavier elements, and that Y is not much different in the Sun from what is observed in the present interstellar plasma. At the time it was known that asteroids are about 5 billion years old (from radioactive decay ages; e.g. Patterson 1955). This is a reasonable estimate for the age of the Solar System. Osterbrock and Rogerson concluded

> It is of course quite conceivable that the helium abundance of interstellar matter has not changed appreciably in the past 5×10^9 years, if the stars in which helium was produced did not return much of it to space, and if the original helium abundance was high. The helium abundance $Y = 0.32$ existing since such an early epoch 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 formation picture.

Their reference is to Gamow (1949).

We believe that in this paper Osterbrock and Rogerson presented the first well-documented proposal of a relation between the theory and observational evidence of the presence of a fossil from the early universe. It appeared in *Publications of the Astronomical Society of the Pacific*, a journal that was (and is) familiar to astronomers and even to some physicists. But we have found no evidence that anyone recognized the significance of this paper for cosmology before the mid 1960s, when the evidence for detection of the CMBR was recognized.

It is worth noting several papers in the early 1960s that referred to Osterbrock and Rogerson (1961), and some that did not. Peebles (1964) used their helium abundance in a study of the structure of the planet Jupiter, but did not notice the big bang connection. This discussed on page 127.

O'dell, Peimbert and Kinman (1964) added to the evidence for a large helium abundance in old stars, and reemphasized Burbidge's (1958) point that the production of this amount of helium in an early generation of stars would require that the galaxy passed through an exceedingly luminous phase. They referred to Osterbrock and Rogerson's paper but did not mention the possibility of helium production in a big bang. We have noted the reanalyses of the theory of light element production in big bang by Smirnov (1964) and Hoyle and Tayler (1964). Smirnov did not know the Osterbrock and Rogerson paper: he thought the abundance of helium in the oldest stars is less than about 10%. Hoyle and Tayler knew the evidence that the helium abundance is larger than that, and that the big bang model could account it, but their do not include Osterbrock and Rogerson (1961).

The idea of another fossil, a remnant thermal sea of radiation that might be expected to accompany the production of helium in a hot big bang, was clearly stated by Alpher and Herman (1948, 1950).[17] The idea was not very visible in the early 1960s, however, as is illustrated by the fact that Bondi (1952, 1960a) did not mention the radiation. Alpher and Herman (2001) recall asking radio astronomers about the possibility of detecting the radiation, Tayler (1990), in his recollections of the work with Hoyle in 1964 on helium production, mentioned their thoughts about remnant radiation from a big bang, and Hoyle (1981) recalls discussions of the possible temperature of a microwave background in conversations with Gamow and with Dicke. It seems clear that the idea that space might be filled with a sea of thermal radiation left from the early stages of expansion of the universe was "in the air" in the early 1960s. But it was not very visible in the literature.

There were early measurements that constrained the possible temperature of this radiation. We noted in section 4 of this chapter that Dicke, Beringer, Kyhl and Vane (1946) had placed a limit $T_0 < 20\,\mathrm{K}$ on the effective temperature of background radiation at microwave wavelengths. They made no mention of cosmology. The same is true of Medd and Covington (1958), who found that the sky temperature (the radiation from the atmosphere plus what we now know to be the CMBR) at 10.7 cm wavelength is 5.5 K, with a probable error of about 6 K, of Denisse, Lequeux and Le Roux (1957), who placed a limit of about 3 K on the background temperature at

[17] The idea was less clearly expressed by Gamow (1948); he did note that the radiation would be present after element formation, and that it could be an important dynamical actor in structure formation. This is discussed on page 51. Gamow (1956) arrived at a present temperature of about 6 K, but from a mistaken computation of cosmic expansion times, not from his earlier and now accepted considerations of helium production.

33 cm wavelength,[18] and of Shmaonov (1957), who found about 4 ± 3 K at 3.2 cm. Within the uncertainties of the measurements, these all are upper bounds, not unrecognized detections.

With the clarity of hindsight we conclude that the Bell Laboratories measurements (DeGrasse *et al.* 1959a, Ohm 1961) that were discussed commencing on page 43) were the first likely — though not recognized — detection of the CMBR to be presented in the literature, and that McKellar (1941) presented the first analysis of a likely indirect detection from interstellar absorption lines. Hindsight is needed, of course, because until the events in 1965 described in the next chapter no one had recognized the possible significance of these measurements. Doroshkevich and Novikov (1964) likely were the first to recognize the importance of the Bell Laboratories measurements for cosmology. Doroshkevich and Novikov might be counted as the second authors, after Osterbrock and Rogerson (1961), to make a connection between measurements and a theory of a candidate fossil from the early universe. (The connection in this case was somewhat less complete, however, because Doroshkevich and Novikov did not realize that the Bell Laboratories had a likely detection).

The recollections in the next chapter make it clear that some astronomers in the early 1960s remembered the evidence we have reviewed (commencing on page 38) that the spin excitation temperature of the interstellar molecule cyanogen is surprisingly large, and that that suggested the presence of a microwave radiation background at an effective temperature of a few degrees above absolute zero. But only after the candidate fossil radiation had been recognized in direct detection was it realized that the spin temperature is

[18]Lequeux, one of the authors of this last paper, offers this comment on the suggestion by Le Floch and Bretenaker (1991) that this experiment may have yielded a detection rather than a limit: "In the winter of 1954-55, we measured the antenna pattern of a former German "Wurzburg" radar equiped with a 33-cm receiver built by Le Roux, that we used for mapping the Galaxy. This involved measuring the signal received from a remote transmitter while pointing the antenna in various directions. Then we calculated the contribution of the ground and the atmosphere to the antenna temperature as a function of the direction pointed by the antenna, and compared to observation (far from the galactic plane, of course). The observed antenna temperature was calibrated with blackbodies. Then we concluded that any contribution from the sky would be less than 3K, and would be rather uniform. This is what is published in the Comptes Rendus; and signed by Le Roux.

"Given our equipment, and in spite of careful measurements, it would have been foolish to claim a positive detection. Our remote antenna lobes were considerably stronger than those of the horn used by Penzias and Wilson. Thus in the Comptes Rendus paper we only claim an upper for the CMBR, admittedly close to the actual value, but only an upper limit."

about what is wanted to account for the abundance of helium.

There was one other – very indirect – hint to fossils from the big bang, from large-scale structure: galaxies and clusters of galaxies. Bondi (1960a, p. 176) gave references to studies developing the idea that the present-day concentrations of mass observed in galaxies and in clusters of galaxies grew by gravity out of small departures from an exactly homogeneous mass distribution in the early universe. However, he does not take note of two of Gamow's (1948a,b) remarks that have proved to be part of our present standard model. First, the CMBR temperature set the matter temperature in the early universe (because at high redshift the matter was ionized and thermally coupled to the radiation), and the matter temperature set the minimum mass of a cloud of matter that gravity can hold together against the matter pressure. Second, the gravitational growth of nonrelativistic mass concentrations such as galaxies commenced when the mass density in the CMBR fell below the mass density in matter. Prior to that, the rapid expansion driven by the large mass density in radiation suppressed the gravitational growth of concentrations of matter. Gamow's two points were little noted at the time we are considering: the only reference we have found in the literature, apart from his immediate colleagues, is by ter Haar (1950, p. 129). The essays indicate how the role of the CMBR in the formation of galaxies and clusters of galaxies came to the general attention of the community.[19]

A central theme of this chapter is that we find our way through elegant ideas to the ones that actually approximate reality by an iterative consultation of theory and practice. The process tends to be haphazard. Most of the pieces needed to identify two fossils from the early universe — helium and the CMBR — were known in 1960, but no one person knew enough of them then to put them all together. Many have wondered why this last step took so long. The essays in the next chapter offer the best hope for an understanding of the answer.

[19] A related issue is the effect of the growing concentrations of mass on the spatial and angular distribution of the CMBR. The basic principles were developed in the 1960s after the identification of the CMBR (Sachs and Wolfe 1967; Silk 1967; Peebles and Yu 1970). Detection of the effect took some two more decades of work. The learning curve for how to make these measurements is a central topic of Chapter 5. The precision results are central to the network of cosmological tests that confirm basic elements of the present standard general relativity hot big bang cosmological model.

Chapter 4. Cosmology in the 1960s

Here are the essays, preceded by these few cautionary remarks.

The plan of ordering of the essays is to group by topic, with chronological order within groups and of the groups. Because the focus of research tends to evolve with time the result is that these recollections of what happened are presented in a roughly chronological order. For example, in the second half of the 1960s a first order of business on the experimental side was the measurement of the spectrum of the CMBR, and on the theoretical side it was the exploration of ideas about what a significant departure from a thermal spectrum might mean. These continued to be pressing issues in the 1970s, but there was increasing interest in the search for departures from an exactly isotropic distribution of the radiation and for the theory of the disturbances to the CMBR distribution that might be expected to accompany the known departures from an exactly homogeneous and isotropic distribution of the matter. Thus we present the essays whose main focus is the spectrum before those largely concerned with the anisotropy of the CMBR.

In practice this plan requires many arbitrary and debatable decisions on topics and dates. The result, we have checked by observation, is that it is not readily apparent that we had a plan. This is a realistic illustration of what was happening in the 1960s, of course.

The confusion extends to the recollections: they are not always even consistent with each other. The reader therefore must be prepared for a distinct change of style from the linear — but we hope efficient — history in the previous chapter to the chaos of the real world.

David C. Hogg: Early Low-Noise and Related Studies at Bell Laboratories, Holmdel, N.J.

The US National Academy of Engineering cites Hogg's election to the Academy for his "contributions to the understanding of electromagnetic propagation at microwave frequencies through the atmosphere." A native of Saskatchewan, Hogg's current interest is the composition of music.

A giant in radio science, Harald T. Friis (1971) was head of the Bell radio-research laboratory in Holmdel. Having pioneered work on the superheterodyne receiver in the late 1920s, he played a key role in Karl Jansky's initial experiments and the beginning of radio astronomy in the early 1930s. His interests then turned to shorter wavelengths which eventually led to the construction of a nationwide microwave radio-relay system employing "horn-reflector" antennas that he patented with Al Beck. This design of antenna is highly efficient and was used in all of the low-noise microwave systems to be discussed here. These remarks are made to indicate that high-quality equipment, designed for very practical purposes, can be used as a tool for first-class science. It is to the credit of the United States that the ATT Bell Laboratories existed, allowing such broadminded interactive research to be done.

In the 1950s, not long after John R. Pierce had traveled to the U.K., including Oxford, he brought Rudi Kompfner to Bell Labs. They asked me to calculate the thermal noise from the earth's atmosphere over the microwave band. This noise level was needed for their calculation of the feasibility of microwave communication by reflection from an orbiting balloon (Pierce & Kompfner 1959). It was fortunate that some time earlier, with Arthur B. Crawford, I had measured the millimeter-wave absorption by the oxygen and water vapor in the sea-level atmosphere (Crawford & Hogg 1956). Thus the broadening constants for computation of "sky noise" were determined and that calculation was completed (Hogg 1959).

However, no sky noise measurements were available to corroborate this theory. Nevertheless, again fortunately, at that time Derek Scovil and Bob De Grasse at the Bell Laboratory, Murray Hill, N. J., were well along in developing microwave traveling-wave (TWM) solid-state masers, with noise temperatures on the order 10 K (De Grasse, Shulz-Du-Bois & Scovil 1959). Again encouraged by Kompfner and Pierce, we therefore combined this maser and antenna to produce a "low-noise" receiving system.

Here we discuss and compare several systems at various microwave frequencies, with emphasis on the technology and low-noise results that pointed the way toward a determination of the microwave cosmic background noise.

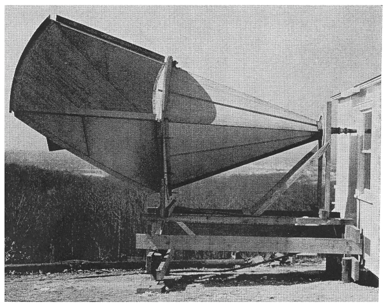

Figure 6: The antenna in the first low-noise microwave system

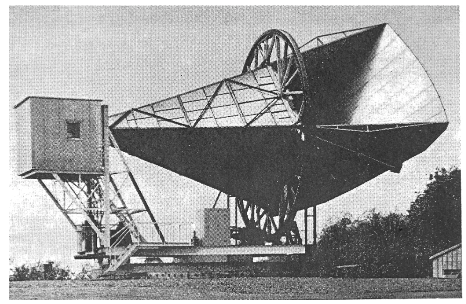

Figure 7: Horn-reflector antenna used in the Project Echo experiment

In all three cases, the equipment was designed and built to demonstrate the feasibility of satellite communications, and the cooperation of NASA and the Bell Laboratories System Department were important factors.

The first low-noise microwave system (De Grasse, Hogg, Ohm & Scovil 1959a,b) operated at 5.65 GHz. The antenna mount, constructed of wood, on the lip of Crawford Hill, allowed manual beam pointing in elevation only; a photograph is shown in Figure 6. The rectangular waveguide input to the TWM was fed via a rotating joint in the circular waveguide from the antenna. The output from the TWM preamplifier was then fed to a conventional superheterodyne. This combination resulted in a (zenith) system noise temperature of 18.5 K.

At that time I was invited by John Shakeshaft to Cambridge, England, and gave these results in Maxwell's lecture room at the Old Cavendish. A seminar also was given at the old McDonald physics building at McGill University, Montreal, Canada, where Ernest Rutherford did research on the alpha particle and helium.

Although this system was unsophisticated, it did serve as a prototype for the following two systems that were used for actual communications via satellites: Echo and Telstar.

The second low-noise microwave system (Ohm 1961) was designed and built specifically for the Echo satellite project at a frequency of 2.39 GHz, for receiving signals reflected from an orbiting balloon. The receiver design was engineered by Ed A. Ohm and the project engineer was W.C. (Bill) Jakes Jr. The antenna (Crawford, Hogg & Hunt 1961), a twenty-foot aperture horn-reflector, is shown in Figure 7; design and construction was managed primarily by Arthur B. Crawford and Henry W. Anderson. The 2.39-GHz measured radiation patterns and gains were found to agree well with theory

(Crawford, Hogg & Hunt 1961); both azimuth and elevation pointing were available. The dual-channel TWM, provided by the Derek Scovil group at the Murray Hill Laboratory, received waves of both clockwise and counter-clockwise circular polarization, with a noise temperature of 7 K. Ohm carried out an exhaustive study of the uncertainties in the measured and estimated noise contributions. The overall system noise temperature (at zenith) was 21 K.

The third low-noise system operated at 4.17 GHz as a sensitive receiver for the Crawford Hill station of the Telstar Project (Jakes 1963). The antenna was the same as for the 2.39 GHz Echo experiment (Fig. 7), but gain, beamwidths, and pointing characteristics were checked at the new shorter wavelength and found satisfactory when compared with theory. The TWM, with a 5 K noise temperature, was provided by the Scovil group to amplify both senses of circular polarization.

The "first-ever" live TV from Europe was obtained via Telstar with this receiver. The overall zenith system noise temperature was just less than 17 K. W. C. Jakes Jr. was project engineer. The main U.S. ground station was at Andover, Maine.

Table 1. Summary of Satellite Communication Systems

Experimental Communication System		1	2	3
Frequency		5.65	2.39	4.17
Estimated antenna backlobe and resistive noise		3.5	2.3	2.5
Zenith atmospheric noise	measured	2.5	2.3	2.5
	estimated	2.75	2.4	2.5
Zenith system noise	measured	18.5	21	17
	estimated	18.5	19	14.5

Note. — noise temperatures are in degrees Kelvin, frequencies in GHz

Table 1 shows that the zenith atmospheric sky noise, measured by tipping the antenna beam in elevation, is within 0.25 K of the theoretical computation in the microwave band; the theoretical values are computed for average summer conditions of temperature and humidity (U.S.A).

The table also shows that in no case does the estimated system temperature exceed the measured system temperature. Of course, none of the estimated system temperatures contain any contribution from the cosmic background radiation. However, it is amusing that, in the case of system 1, the estimated is the same as measured; this indicates sizable uncertainties, probably in both quantities.

Many of the uncertainties in the noise contributions from the microwave circuitry can be avoided by switching between the antenna and a precisely calibrated cold load located near the antenna input *per se* as shown by Penzias and Wilson (1965), at 4.08 GHz. With this improved measuring system they were able to deduce 3.5 K excess to the expected antenna temperature; this excess is interpreted as the cosmic background radiation.

The very low-loss switch used in these measurements is a treasure in microwave radiometry. It is made of a gently squeezed section of waveguide of circular cross section. Penzias and Wilson quote only 0.027 dB of loss for their switch; to my knowledge, it was first mentioned by George C. Southworth in his book on microwave technology and was first used in radiometry by Douglas H. Ring at K-band.

However, measuring the contribution of the lower hemisphere (back lobes) to the antenna temperature is quite another matter (for some estimated values, see Table 1). Ideally, one would measure the antenna radiation pattern over the lower hemisphere, measure the ground (etc.) radiation over that same hemisphere, and integrate the product of those two over the hemisphere. This radiation is comprised of both emission from the ground surface and reflection of sky temperature by that surface (Hogg 1968). Penzias and Wilson calculated a net contribution of just less than 1 K for the antenna *per se*. Apparently further research on antenna design, measurements and siting is called for.

As implied in the historical introduction, some equipment, designed and built for practical (economic) application, can impact scientific studies, provided the quality is good. An example of this is the fruitful use of electromagnetic and electronic equipments, designed for microwave satellite communications development, in pursuing the microwave cosmic background radiation.

Recently, the importance of science and engineering innovation to the U.S.A. has been emphasized in a proclamation by the President of the U.S., backed by a report issued by a panel supported by the National Academies, and chaired by Norman Augustine. That there is fruitful feedback between the two is well exemplified by the exercise we have just discussed. The cosmologists and astronomers who carry on such research and innovation are to be commended.

Neville J. Woolf: Conversations with Dicke

Nick Woolf is a professor of astronomy at the University of Arizona. He was a postdoc in the Princeton University Department of Astrophysical Sciences in 1962 – 65. His current research interest is astrobiology.

I have these memories that tell me that I cost Bob Dicke the Nobel Prize.

One evening in the attic of Palmer, I think in early 1964, Bob turned to me and asked me whether there was any way to know the amount of the background radiation. He had already turned Roll and Wilkinson onto the topic, but I believe they had only just started.

I said "Well, there were your own measurements in 1946." He grunted. And I said, "and then there are the interstellar molecules."

He didn't say a word. "Oh", I thought, "I must have said something stupid" and I shut up. If I had said more, about the searches for excitation in Iron and CN and the other stuff, I am sure he would have picked up on it and he would have been ahead of Penzias and Wilson - but that is the world of Might have Been.

I also mentioned the molecules to George Field during this time, or slightly earlier, and George said something about that he thought they were excited by collisions. Later I asked him, and he said that he had tried a calculation around that time, but later realized that it had been wrong.

Finally, when I was at the Institute for Space Studies in 1965, Bob Dicke wanted Bill Hoffman and me to fly a balloon to detect the background radiation. Well, I knew that we were far from that level of precision, though in a couple of years later Bill did detect the 100 micron radiation from the galactic center. So I hurriedly diverted Bob to the molecules. And in the hurry of the moment I left him with a reference to McKellar's paper before he had measured the excited state. So Bob got Pat Thaddeus into the picture, and Thaddeus tracked down the literature - but this was all after Penzias and Wilson (1965) had observed the background.

Anyway, once Bob knew of the excitation he visited me at the institute, and asked who was working on CN at that time. "Guido Munch" I said. "Call him, and ask if there is anything new," said Bob, so I picked up the phone and called Guido. I asked about the cyanogen, and Guido said "Are you working with George Field?" "No, why?" "Well George called me yesterday about this."

Later I found that at almost the same time Shklovsky gave a colloquium in Moscow on the same topic.

So that the story of how one post doc's hesitation lost Bob the Nobel prize. And I believe it would be worth telling the tale, so that some other young person next time is not as hesitant as I was.

And of course, there it is in Herzberg's book about the temperature being 3 K, "but this number has no physical significance whatever." ... I quote from memory.

And like Gamow I have now moved into astrobiology.

George B. Field: Cyanogen and the CMBR

George Field has retired from director of the Harvard-Smithsonian Center for Astrophysics. His current research interest is turbulence in astronomical settings.

My encounter with the microwave background began in 1955. I had come to Harvard as a postdoc, intending to search for intergalactic hydrogen by looking for 21-cm absorption in the spectrum of the radio source Cygnus A. While I was making the observations at the Harvard 28 footer, I studied the problem of the excitation of the upper level of the line, as that is crucial in calculating the absorption coefficient. I realized that it can be excited by fluorescence Lyman α photons, by collisions with atoms or free electrons, or by absorption of 21-cm photons from whatever source. Ed Purcell and I calculated the collision cross section, I estimated the effect of Lyman α radiation, and I proposed that we measure the continuum near 21 cm to get the radiation field that would excite the line. When I asked Doc Ewen how to measure the continuum, he said it could not be done at that time because it would require an absolute measurement whose zero was known. So I extrapolated continuum maps at 21 cm to the coldest point and estimated 1 K. Clearly that was a lower limit, because without a zero point, there was no way to know what the coldest point represented. I published the result in 1959. Of course we now know that the zero point is 3 K.

In 1957, I joined the faculty at Princeton, where I had taken my PhD in astronomy in 1955. I knew about interstellar molecules from Lyman Spitzer, who was studying optical interstellar lines at Mt. Wilson as part of his research at Princeton on the interstellar medium. In fact, in my first published paper, in 1955, Lyman and I mentioned an unidentified line that appeared in our tracing that later was identified as interstellar CH^+. But I was particularly intrigued by a reference in Herzberg's (1950) book on Diatomic Molecules that stated that one of the lines of intestellar CN (cyanogen) arose from a rotationally excited state (J = 1) in the ground electronic and vibrational state. The exitation temperature was estimated to be 2.3 K. This was unique in interstellar studies, and so with my experience with atomic hydrogen, I calculated the excitation to be expected from collisions and fluorescence radiation transitions. They failed by a large factor to account for the excitation. To calculate the effect of radiation at 2.6 mm wavelength, which might excite the molecule from the J = 0 to 1 levels, I needed two things: the permanent dipole moment of CN, in order to calculate the Einstein B coefficient, and the mean intensity of 2.6 mm radiation at the positions of the interstellar molecules.

The dipole moment had never been measured, so I estimated it from CO, for which it is 0.1 Debye, to be 0.05 Debye, enough to couple the excitation to the radiation field at 2.6 mm. Just as in the case of the 21-cm line, the mean radiation intensity, expressed as a radiation temperature, had not been measured either, but I convinced myself that from the CN observations themselves, 2.3 K was a good estimate. I wrote all this up, and concluded that there must be previously unrecognized source of radiation at 2.6 mm. I gave the paper to Lyman Spitzer to read. He thought it was too speculative to submit for publication, probably because the dipole moment, which determines the coupling to the radiation field, was only an estimate. All this took place before 1960, because I was then at the old Observatory on Prospect Street in Princeton, whereas we moved to a new building at that time.

One event that took place in the new building was a visit from Arno Penzias. I recall standing in the door of his office discussing his plans to observe 21-cm radiation emitted by atoms in intergalactic space. If the hydrogen is excited solely by the background radiation, no emission will be detected, as it is exactly cancelled by the absorption of the background. Thus we were lead to think about the temperature of the background radiation. As I recall, Arno was not optimistic about the absolute measurements required.

The Dicke group was working on the Brans-Dicke (1961) theory of gravitation at Palmer Lab. I knew Dicke and Peebles, and recall attending a seminar there by Jim Peebles explaining his work on helium production in the Big Bang, of course in Brans-Dicke cosmology. I went up afterward and told Jim that colleagues of George Gamow, including Alpher and Herman, had done similar work. I think I knew at that time of the prediction of 5 K for the background radiation by Alpher and Herman, but I don't recall mentioning it. Moreover, it did not occur to me to mention my work on CN either, because I had not made the connection with the Big Bang.

I also recall that while teaching a course in Palmer I noticed a microwave horn out of the window, pointing to the vertical. It must have been Roll and Wilkinson's experiment, but again I did not make the connection.

Fast forward to 1965, when the discovery was published in the New York Times. I missed it, perhaps because I was packing to move to Berkeley that summer. However, I soon learned about it from a call from Bernie Burke that I got in my Berkeley office. When he said "3 K" I at once realized that it could be the source of radiation that I had predicted in my work on CN before 1960. Unfortunately, my manuscript on the subject at that time is either lost or in cold storage.

Nevertheless, I thought maybe the CN data would be useful. I knew that to make the case I needed to find a value for the CN dipole moment. By a strange coincidence, it was hiding in my wastebasket. At the time, I was writing an article for the Annual Reviews of Astronomy and Astrophysics, and the editor had sent me proofs from another article as a guide to marking my own proofs. The article was on The Spectra of Comets, by Claude Arpigny, in which he discusses how to predict the emission spectra of molecules — including CN — using rate equations for level populations. One of the parameters is the dipole moment of the ground electronic state, which he had adjusted to fit the data. His number, 1.2 Debye, was not far from a more recently measured laboratory value, 1.4 Debye. Arpigny's dipole moment enabled me to calculate the coupling of the J = 1 rotational level of CN to the radiation field at 2.6 mm. I found that the coupling to radiation is stronger than to collisions or fluorescence by a large factor. Much stronger, even, than I had concluded before 1960, by the square of the dipole moment, a factor of 200. I knew then that we had a radiation thermometer at 2.6 mm.

Another coincidence occurred the same day. When John Hitchcock, a graduate student working in the next office, heard what I was doing, he came in and told me that at that moment he was working on observations of the rotational excitation of CN. He was reducing data that he had taken from six plates that George Herbig had taken of the spectrum of zeta Ophiuchi at the wavelength of the interstellar CN lines. Suddenly we had new data to which to apply the theory of excitation. Together with George Herbig we wrote an abstract of a paper for the 120$^{\text{th}}$ meeting of the American Astronomical Society, which was meeting in Berkeley (another coincidence). At that meeting, held December 28 -30, 1965, we presented evidence that the background radiation follows a blackbody spectrum over the 28-fold wavelength interval from 7.4 cm to 2.6 mm. Our value of the temperature was given as 2.7 to 3.4 K (Field, Herbig and Hitchcock 1966).

John and I published two more papers on the subject.(Field and Hitchcock 1966). One in *Physical Review Letters* gave a result of 2.7 to 3.6 K for zeta Persei, a star on the other side of the sky from zeta Oph, and 300 pc distant from it. Thus the hypothesis that the radiation is universal passed the test. In a later paper in *the Astrophysical Journal* we considered the possibility that the spectrum of radiation is not blackbody after all, but as suggested to us by Nick Woolf, dilute blackbody at a higher temperature. We were able to rule out this hypothesis with reasonable certainty. It is interesting that the peak of the blackbody curve in frequency units is 1.7 mm. With our measurements at 2.6 mm, we were climbing the peak.

Pat Thaddeus

to come...

Donald E. Osterbrock: The Helium Content of the Universe

Donald Osterbrock is Professor Emeritus of Astronomy and Astrophysics at the University of California, Santa Cruz. He is author of the influential book, Astrophysics of Gaseous Nebulae and Active Galactic Nuclei (1989), and coauthor, with Gary J. Ferland, of the greatly expanded second edition (2006). His main research interest is the study of AGNs.

I have never done any research in cosmology, but as an onlooker I have been interested in it for many, many years. I was inclined toward science from boyhood, partly no doubt because of my father's background as an engineering professor, and my mother's as a chemistry assistant in an industrial laboratory in Cincinnati, where both of them, my brother, and I were all born and grew up. My high school had an excellent library, and in it, and also in books from our local public library, I read a lot about astronomy. I had a small amateur-made reflecting telescope with an alt-azimuth pipe mounting, and could look at the poor images that it produced of the moon, planets, and bright nebulae like the Ring and Orion. My father took me to occasional meetings of the local amateur astronomical society when a famous professional came to town, and I remember especially Harlow Shapley and Otto Struve.

I graduated from high school six months after Pearl Harbor, and in another seven months I was in the Air Force, training to be a weather observer. On a troopship from Honolulu to Okinawa, we proceeded by way of Eniwetok, Guam and Saipan, and on the way I first saw Fomalhaut and the Southern Cross. After the war ended, I was able to enter the University of Chicago under the so-called GI Bill of Rights, and in three years completed a bachelor degree in physics, and a master in astronomy and astrophysics. Chicago had the best faculty in physics and astronomy in the country at that time, in my opinion, and I was especially inspired by courses in quantum mechanics, taught by Gregor Wentzel, and nuclear physics, by Enrico Fermi. There were no active cosmologists there, but I attended colloquia by George Gamow, on what we call the Big Bang today, but he called the ylem-theory then, and by Maria Goeppert-Mayer on her new interpretation of the so-called "magic-number" nuclei in terms of nuclear shell structure with strong spin-orbit and spin-spin coupling. These two colloquia seemed quite reasonable to me and, I noticed, to nearly all the professors who were there too.

Then, for three years at Yerkes Observatory I again had excellent teachers, especially Struve, S. Chandrasekhar, W.W. Morgan, Bengt Strömgren and Gerard P. Kuiper. All of them taught us about stars, nebulae and galaxies, even Kuiper, although he also lectured on the solar system, on which he had begun working during and just after the war. I did my thesis with Chandra, on the gravitational interaction between stars and cloudy interstellar matter, which we would call giant molecular clouds today. For two or three weeks in the summer of 1951, I went to a "summer school," organized by Leo Goldberg at the University of Michigan. I wanted to hear the lectures of George C. McVittie, on hydrodynamics of interstellar clouds (though Chandra advised me not to go — he said he had already taught me more than McVittie knew on the subject!). The "school" was held in the old Detroit Observatory building at the UM campus, where all the professors and grad students had offices. I believe I was the only student from outside UM who attended, and so I shared an office with McVittie and with David Layzer, who had just joined the faculty there that summer, with his fresh Harvard PhD degree.

Once a day we all got together in the main room of the observatory, to have coffee and talk about astronomy. In those conversations McVittie and two of the older professors, Dean McLaughlin and Freeman Miller, were scathing in their remarks on Fred Hoyle's Steady-State theory of cosmology, involving continuous creation of matter. McVittie was a classical mathematical cosmologist, and I had soon seen from his lectures that Chandra had been right. He had little if any physical insight, and his criticisms of Hoyle's ideas were ridiculous, I thought. Basically, he said continuous creation just couldn't happen, and McLaughlin and Miller chimed in as his conservative claque.

After I completed my PhD at Yerkes in 1952, I was fortunate to be appointed a postdoc at Princeton for a year. There I worked out the internal structure of red-dwarf stars, which turned out to have deep outer convective zones, but radiative centers with the main energy production by the proton-proton reaction. I had learned of the problem in Strömgren's stellar-interiors course at Yerkes, and he encouraged me to follow it up at Princeton. Martin Schwarzschild and his students were working on red-giant stars, and he helped me tremendously in my work. Lyman Spitzer, the head of the astronomy department, asked me to teach the stellar atmospheres graduate course the second semester I was at Princeton, so he could spend full time on his research on deriving energy for peaceful uses from controlled nuclear reactions, called Project Matterhorn at that time. I was glad to teach the course; there were only four grad students in it, Andy Shumanich, Jack Rogerson, George Field and Leonard Searle. As they all had long and successful careers as research astrophysicists, I can't help thinking that at least I didn't hinder them in this first course I ever taught.

Hoyle came to Princeton that year as a visitor, working with Schwarzschild on the structure and evolution of red-giant stars for two or three months. Fred's office was next to mine, in the quiet rear of the old observatory building, and we often discussed his research and mine. He was extremely hard working, brilliant, and knew a lot of astrophysics. I was impressed by Hoyle, and although he was not doing cosmology there at that time, I still had an open mind on it. We never discussed cosmology, so far as I can remember. Hoyle was all business on red giants there, as I was on red dwarfs, and those were the two subjects we talked about.

After one year at Princeton, I was appointed to the faculty of Caltech's then very new astronomy and astrophysics department, headed by Jesse Greenstein. My wife and I drove west in the summer of 1953, stopping for a month at Ann Arbor for a second astrophysics summer school, again organized by Goldberg. This one was much more successful than the earlier one, with Walter Baade and Gamow the two main lecturers, backed up by Ed Salpeter and Kuiper for shorter series of talks. About thirty grad students, postdocs, and young faculty members were there. I was most interested in learning from Baade, but Gamow's lectures, mostly on his cosmology, were quite good. He was always humorous, but with plenty of good ideas. By that time in his life he was a fairly heavy drinker, but it never seemed to mar his thoughts nor his lectures.

Baade was a fantastically inspiring lecturer, and I was glad indeed to have him and Rudolph Minkowski as my chief mentors in Pasadena. At that time the Caltech and Mount Wilson (now Carnegie) astronomers shared the 200-in and 100-in telescopes, and I worked largely on nebular spectroscopy, with some forays into emission-line galaxies, but never into cosmology. There were too many interesting things for me to do with objects in our own and nearby galaxies. Hoyle came to Caltech two or three times while I was there, mostly to work with Willy Fowler and Geoff and Margaret Burbidge, who came there on visits, on nucleosynthesis in stars. Fred was a visiting professor for one quarter, lecturing on the same subject, and I sat in on most of his lectures. But I never discussed cosmology with him then, nor heard him discuss it with others around the astrophysics lunch table in the faculty club, except to utter an occasional disparaging remark about the "big bang."

From Caltech I went to the University of Wisconsin in Madison with Art Code, to help him build up a full-size graduate astronomy and astrophysics department there. Again, I continued largely observational research there with our smaller telescope, using its excellent photoelectric scanner which made it highly effective for nebular problems.

Then in 1960-61 I had a Guggenheim Fellowship to go back to Princeton on leave, this time as a visiting fellow at the Princeton Institute for Advanced Study, where Strömgren had recently become the professor of astrophysics, "the man who got Einstein's office." Among the other visiting fellows then were Anne Underhill, who had worked with Bengt at Yerkes and in Copenhagen, Su-shu Huang, another Yerkes PhD, and Hong-yee Chiu. We had weekly astronomy lunches with Spitzer and Schwarzschild, and Field and Rogerson, who had come back as assistant professor and research associate, respectively, and others. These were held in a faculty cafeteria upstairs in Firestone Library, not as spacious or well appointed as the IAS dining room that was built later, but still quite a step up from the aluminum-sided diner on Nassau Street where we had gone in 1952-1953.

I think Martin suggested to Rogerson and me that we review the status of the helium abundance in the objects we knew best, the Sun, on which Jack had done a lot of research while a Carnegie postdoctoral fellow at Mount Wilson, and gaseous nebulae, with which I was familiar. I had seen Rogerson often in his two years in Pasadena, and we were good friends.

The helium abundances in nebulae were simple; we used the measurements of the Orion H II region and several planetary nebulae, made by my first Ph.D. thesis student at Caltech, John Mathis, who had also calculated the relations between line-strength and abundances of helium and hydrogen. These were supplemented by somewhat later theoretical calculations by Mike Seaton. Our results were that the helium to hydrogen ratio was very nearly the same for planetary nebulae (mean value N(He)/N(H) = 0.16), and for the Orion nebulae (N(He)/N(H) = 0.15). They contradicted the idea that the helium content in our Galaxy might have increased with time, from when the stars had formed that were at present in the planetary-nebula stage (then estimated as 5×10^9 yr ago) to today.

For the Sun we used absorption-line strengths Rogerson had measured for weak [O I] lines in the solar spectrum to determine the relative abundance of oxygen as a representative of the heavy elements (usually called "metals," an especially poor term for all the elements heavier than helium, in my opinion!) to hydrogen. Then from the relative abundances to oxygen of all those heavy elements, often described in earlier years as the "Russell mixture," but using more recent compilations, we derived the abundance ratio by mass, $Z/X = 6.4 \times 10^{-2}$. In this notation X, Y, and Z represent the fractional abundance, by mass, of hydrogen, helium, and heavy elements.

The other relation we used for the Sun was derived from a series of solar interiors models, that Ray Weymann had recently calculated at Princeton under the guidance of Schwarzschild. These new models were then current state-of-the-art, taking into account a shallow outer convection zone, an intermediate, unevolved radiative zone, and a large inner radiative but hydrogen-burning region, in which the results of nuclear processes over 4.5×10^9 yr had affected the variation of hydrogen and helium content with distance from the center. Energy production was mostly but not entirely by the proton-proton reaction, and there was no central convective core. These were the best models then available, but in addition I liked them personally because Ray had been the brightest and best undergraduate student I had taught at Caltech, and also because his models took into account revisions and extensions of my early research on red dwarfs by Nelson Limber, my close friend from Yerkes days. Nelson had also gone on to Princeton as a postdoc after me.

The well observed solar radius, luminosity and mass gave $X = 0.67$, $Y = 0.29$, $Z = 0.04$ for the original abundances in the Sun, at its formation 4.5×10^9 yr ago. This set of abundances is not quite the same as we had derived for the planetaries and the Orion nebula had given, but well within the estimated error, we believed. In the end the best overall fit we adopted was $X = 0.64$, $Y = 0.32$, $Z = 0.04$, essentially unchanged for the past 5×10^9 yr. Our evidence was that that the helium abundance in the Sun is essentially the same as the results mentioned above for planetary nebulae and the Orion nebula based on the very straightforward recombination-line theory for H+ and He+.

Although many of the numerical values have been revised slightly on the basis of better measurements and improved theoretical interpretations of nebular and solar spectra, our conclusion has remained unchanged. The abundance of helium in our Galaxy, and presumably in other galaxies as well, had changed little from their earliest days. Most of the helium must have been formed in the Big Bang. Personally, I could have accepted the idea that both helium and hydrogen had been created together in a Steady-State universe, but evidently Hoyle, Hermann Bondi, and Tommy Gold could not, nor could other later theoretical cosmologists.

Rogerson and I had done our paper because Schwarzschild suggested it at the time. I don't remember why he thought it was important, but I don't think it was for cosmology. Certainly I did not have that idea in my mind back then. I was interested in it chiefly because Martin seemed to me so uncertain about what the helium abundance was in stars near the Sun. He had used various abundances for it in his early stellar interiors and evolution papers with students, postdocs, and visitors at Princeton as collaborators. Looking back now (I didn't realize this at the time), he had even used $Y = 0$ (no helium at all)! This was heresy to me, as all grad students at Yerkes were indoctrinated from early on with the interpretation of the spectral sequence as basically a temperature sequence in stellar atmospheres, all with the same abundances in them, with luminosity as a secondary criterion, but only a very few minor abundance variations which Morgan, Keenan & Kellman (1943) had noted in bright stars, and Nancy Grace Roman (1950) had found more somewhat fainter ones in her postdoctoral research. It was evident that helium was much more abundant than anything else except hydrogen from the great strengths of its lines in hot stars, though we didn't know just how abundant it might be. All the astronomers I talked with in 1953-58 at Mount Wilson and Palomar Observatories had the same general idea, I believe.

Only Martin did not have it in 1952-53, and he didn't seem to in 1960-61, although maybe he was just pretending, to convince Jack and me to prove it. I now realize that Schwarzschild had calculated those models with $Y = 0$ to compare with earlier calculations by Hoyle and Lyttleton (1942). The assumption $Y = 0$ agreed with Hoyle's interpretation of the Steady State theory. As I mentioned above, I could have accepted continuous creation of both hydrogen and helium if that fitted observational data. Perhaps by that time, 1961, Hoyle was already semi-convinced that continuous creation was dead because he knew from his contacts with American observational astronomers that Y does *not* equal zero anywhere in our Galaxy. But I may be wrong, and I do not want to put words into his mouth or in Martin's either!

In addition to Burbidge, Burbidge, Fowler, and Hoyle (1957), three early theoretical papers that I know of had treated the expected helium abundance in our Galaxy as a result of nuclear reactions in stars. Burbidge (1958) estimated its increase with time from the approximately known luminosity of the whole Galaxy, Maarten Schmidt (1959) formulated and calculated an early "closed-box" model, and Mathis (1959) carried out a somewhat less exhaustive one. All three assumed that the initial helium content was zero, and built up gradually with time, as a result of nuclear processing in stars and return of matter to interstellar space from evolved stars, but all three found, in one way or another, that this hypothesis would not work, although they did not put it that directly. None of these authors considered how the heavy-element content might have increased; that was still an unknown process.

When the CMBR was discovered in the 1960s, I readily accepted it as a confirmation of the Big-Bang picture. I believed, and still believe, in following the observational evidence, as long as it was based on sound theoretical interpretations. However, I think it is a great mistake to trust any detailed numerical values, derived from observational measurements, too far. The theory is always too simple to match reality "exactly." For instance, I have heard lectures, and seen cosmological papers, in which values of X and Y derived from nebular spectrophotometry are quoted and used to three significant figures. Observers are often overly optimistic in stating their probable errors, and theorists who use them can be even more so. But in addition all the available calculations of the H I and He I emission-line intensities that I know are based on simplified model nebulae, either with one "mean" temperature and one "mean" electron density, or on models in which local means, varying only with distance from the photoionizing star or stars, are used. Yet direct images of nebulae show that down to the finest resolution we have been able to achieve to date, even at excellent seeing-sites on high desert mountains or from space with the Hubble Space Telescope, fine structure, "filaments," and "clumps" are present in nebulae. No doubt these contain a range of densities, temperatures, and excitation conditions down to very small scales. The "mean" values may not represent these conditions to high accuracy, as many current papers are showing. As our understanding of the effects of fine structure, and also perhaps of hydromagnetic heating of nebular gas, improves, the precision of the derived relative abundance will also increase.

Igor D. Novikov: Cosmology in the Soviet Union in the 1960s

Igor Novikov served as head of the Department of Relativistic Astrophysics at the Space Research Institute and head of the Department of Theoretical Astrophysics at the Lebedev Physical Institute, both in Moscow, and then as director of the Theoretical Astrophysics Center of the University of Copenhagen. He is now at the the Niels Bohr Institute.

The beginning of my scientific career in the middle of the 1960s coincides with the events that caused the astrophysical community to become aware the real existence of the CMBR.

I outline here the development of the situation at this period in the Soviet Union. It so happened that I played a part at this stage of the story. This description relies on my recollections, published and unpublished material of my colleagues, and special recent discussions with participants of the events. I have used also some material from the books Zel'dovich and Novikov (1983) and Novikov (1990), and from the paper Novikov (2001).

I would like to remind you that at the beginning of the 1950s the theory of an expanding, indeed an evolving, Universe with the beginning of time at some finite period ago was practically forbidden in the USSR. There was a postulate that only an eternal Universe without directed evolution as a whole is compatible with the materialistic attitude.

Only at the beginning of the 1960s did the first serious discussions and publications on the physics of the expanding Universe became possible. At that period some important works on the structure of the cosmological singularity and gravitational instability of the expanding Universe were published by E. Lifshitz, I. Khalatnikov and I. Novikov. But these works did not discuss the physical conditions in the early Universe. In the early 1960's Yakov Zel'dovich began turning his attention to cosmology. He very quickly became one of the greatest cosmologist of the last century.

In 1962 Zel'dovich published a paper (Zeldovich 1962) in which he modernized the cold Universe scenario. According to this scenario at the initial stage of the evolution of the Universe the matter consists of a mixture of protons, electrons and neutrinos in equal amounts and the entropy is low. Then at high density (on the order of nuclear density) and at zero temperature neutrinos and electrons form a degenerate relativistic Fermi gas. The process of interaction of protons with electrons with the formation of neutrons and neutrinos is forbidden since the neutrino states that are energetically obtainable in this process are occupied. Upon expansion such a substance remains pure cold hydrogen. It was assumed that all other elements were generated much later, in stars. According to this model the CMBR radiation should not exist in our epoch.

Zel'dovich's hypothesis was widely discussed in the USSR. One of the main reasons for the hypothesis was some indication in the literature at that time that the helium mass fraction in the oldest stars is much less than 20%. Probably this means that the primeval helium mass fraction is essentially smaller than predicted in the theory of the Hot Universe, $\approx 25\%$. Zel'dovich believed also that according to the Hot Universe theory the matter density of the CMBR should be of the order of the modern average nucleon density. In this paper his conclusion was: "These deductions are incompatible with the observations".

Curiously, Ya. Zel'dovich at that period, as well as the originators of the hot model, were mainly interested in the integral properties of the relic radiation (CMBR) — its density, pressure and temperature — but not its spectrum.

Here my story begins. At that time I had just completed the postgraduate course at Moscow University; my science adviser was Professor Zel'manov. My adviser was mostly interested in the mechanics of motion of masses in cosmological models when no simplifying assumption are made about their uniform distribution. He was less interested in specific physical processes in the expanding Universe. At that time, I knew almost nothing about the hot Universe model.

Not long before the end of my postgraduate term, I was attracted to the following problem. We know how different types of galaxies produce electromagnetic radiation in different ranges of wavelength. With certain assumptions about the evolution of galaxies in the past, and having taken into account the reddening of light from remote galaxies owing to the expansion of the Universe, one can calculate the present distribution of the integrated galactic emission as a function of wavelength. In this calculation, one has to remember that stars are not the only sources of radiation, and that many galaxies are extremely powerful sources of radio waves in the meter and decimeter wavelength ranges.

I began the necessary calculations. Having completed the postgraduate term, I joined the group of Professor Ya. B. Zel'dovich; our interests focused mostly on the physics of processes in the Universe.

All calculations were carried out jointly with A. Doroshkevich, who I met when I joined Zel'dovich's group. We obtained the calculated spectrum of galactic radiation, that is, of the radiation that must fill today's Universe if one takes into account only the radiation produced since galaxies were born and stars began to shine. This spectrum, shown in Figure 8, predicted a

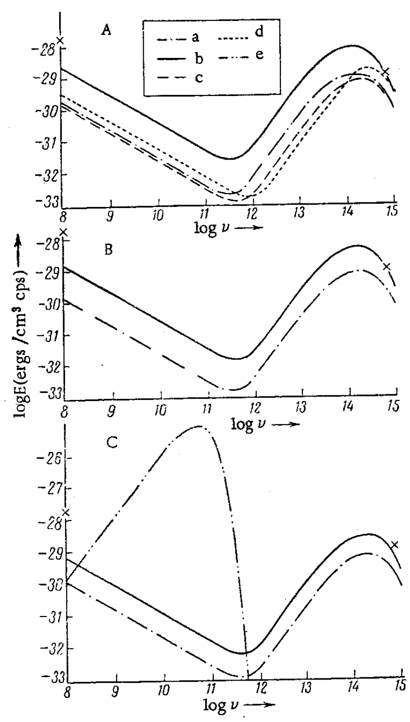

Figure 8: From Doroshkevich and Novikov (1964). Spectrum of the metagalaxy. [Curves (a) to (e): the integrated radiation from galaxies under several assumptions about the cosmology and the evolution of the galaxies.] Curve (e): equilibrium Planck radiation with $T = 1\,\mathrm{K}$. Crosses denote experimental points.

high radiation intensity in the meter wavelength range (such wavelengths are strongly emitted by radio galaxies) and in visible light (stars are powerful emitters in the visible range), while the intensity in the centimeter, millimeter and some still shorter wavelength ranges of electromagnetic radiation must be considerably lower.

Since the hot and cold Universe scenarios were eagerly discussed in our group (consisting of Zel'dovich, Doroshkevich and myself), the paper that Doroshkevich and I prepared for publication added to the total the putative radiation surviving from the early Universe if it indeed had been hot. This hot Universe radiation was expected to lie in the centimeter and millimeter ranges and thus fell into the very interval of wavelengths in which the radiation from galaxies is weak! Hence, the relic radiation (provided the early

Universe had been hot!) was predicted to be more intense, by a factor of many thousands or even millions, than the radiation of known sources in the Universe in this range of wavelengths.

This background could, therefore, be observed! Even though the total amount of energy in the microwave background is comparable with the visible light energy emitted by galaxies, the relic radiation would be in very different range of wavelengths and thus could be observed. Here is what Penzias (1979) said about our work with Doroshkevich (Doroshkevich and Novikov 1964) in his Nobel lecture:

"The first published recognition of the relic radiation as a detectable microwave phenomenon appeared in a brief paper entitled 'Mean Density of Radiation in the Metagalaxy and Certain Problems in Relativistic Cosmology' by A.G. Doroshkevich and I.D. Novikov in the spring of 1964. Although the English translation appeared later the same year in the widely circulated 'Soviet Physics - Doklady', it appears to have escaped the notice of other workers in this field. This remarkable paper not only points out the spectrum of the relic radiation as a blackbody microwave phenomenon, but also explicitly focuses upon the Bell Laboratories 20-ft horn reflector at Crawford Hill as the best available instrument for its detection!"

Our paper was not noticed by observers. Neither Penzias and Wilson, nor Dicke and his coworkers, were aware of it before their papers were published in 1965; Penzias told me several times that this was very unfortunate.

I want to mention a strange mistake related with an interpretation of one of the conclusions of the Doroshkevich and Novikov (1964) paper. Penzias (1979) wrote: "Having found the appropriate reference (Ohm 1961), they [Doroshkevich and Novikov] misread its result and concluded that the radiation predicted by the 'Gamow theory' was contradicted by the reported measurements." Also in the paper Thaddeus (1972) one can read: "They [Doroshkevich and Novikov] mistakenly concluded that studies of atmospheric radiation with this telescope (Ohm, 1961) already ruled out isotropic background radiation of much more than 0.1K."

Actually in our paper there is not any conclusion that the observational data exclude the CMBR with the temperature predicted by the Hot Universe. We wrote in our paper: "Measurements reported in [14] [Ohm 1961] at a frequency $\nu = 2.4 \cdot 10^9$ cps give a temperature 2.3 ± 0.2 K, which coincides with theoretically computed atmospheric noise (2.4 K). Additional measurements in this region (preferably on an artificial earth satellite) will assist in final solution of the problem of the correctness of the Gamow theory."

Thus we encouraged observers to perform the corresponding measurements! We did not discuss in our paper the interpretation of the value 2.4 K obtained by Ohm with help of a techology developed specially for measuring the atmospheric temperature (see discussion in Penzias 1979).

Below I will tell more about our discussions with some radioastronomers in the USSR.

At that time the possibilities for communications with our foreign colleagues were very restricted. I learned about the discovery of the CMBR radiation at a conference in London in the summer of 1965. When I was back in Moscow I informed professor Ya. Zel'dovich.

At the first moment when I told Zel'dovich about the discovery he obviously did not remember the details of my paper with Doroshkevich and started to scold us that we had not included in our paper the figure with the predicted spectrum of the CMBR. When I immediately showed him the corresponding figure in the reprint of our paper he started to scold us for the absence of the effective propaganda of our paper.

It was clear that this discovery means the strict proof of the Hot Universe. This discovery was widely discussed among Soviet physicists and astronomers. Ya. Zel'dovich abandoned his hypothesis of the cold Universe and became an ardent proponent of the theory of the Hot Universe. In his letter to Professor Dicke he wrote on 15 September 1965 (unpublished and kindly provided to me by J. Peebles): "I am not more so cock-sure in my colduniverse hypothesis: It was based on the assumption that the initial helium content is much smaller than 35% by weight. Now I understand better the difficulty of helium determination."

Ya. Zel'dovich began active work on the Hot model even before the discovery of the CMBR. V.M. Yakubov, a collaborator of Ya. Zel'dovich, repeated the earlier calculations of Hayashi (1950) of the process of the nucleosynthesis in the Hot Model. These calculations were much simpler and more transparent and based on new knowledge of the weak processes. These calculations were set forth in the paper Zel'dovich (1965). Thus the process of the Big Bang nucleosynthesis became known and understandable for us.

When I started to work on these notes (autumn, 2003) I asked Professor V. Slysh (Lebedev Physics Institute, Moscow) for his recollections of the events of that period in the group of Professor I. Shklovsky at the P.K. Shternberg State Astronomical Institute in Moscow. He told me the following. In 1965 just after learning about the discovery of the CMBR I. Shklovsky asked V. Slysh to find in the Institute Library papers, published around 1940, concerning the interstellar absorption lines in the spectrum of the light coming from the star ζ Ophiuchi; the absorption was caused by the cyanogen molecule CN. I. Shklovsky himself was a specialist in the physics of the interstellar medium and remembered the papers. V. Slysh had found the papers by Andrew McKellar (1940, 1941). In these papers McKellar concluded that these lines (in the visible part of the spectrum) could have arisen only if the light was absorbed by rotationally excited cyanogen molecules. The rotation must be excited by radiation at a temperature about 2 to 3 K. In the paper McKellar (1940) wrote: "Effective temperature of the interstellar space ...$<$ or $=$ 2.7 K." In another paper McKellar (1941) wrote that the "Rotational temperature of interstellar space is about 2 K".

On the basis of this information I. Shklovsky wrote and published a paper Shklovsky (1966) where he declared that this temperature was the temperature of the whole Universe rather than a temperature of only the interstellar medium as it had been declared by McKellar.

We are not yet through the chain of missed opportunities that plagued the discovery of the relic radiation.

Let us return to the question about the technical feasibility of detecting the cosmic microwave background. At what time did this become possible? Weinberg (1977) writes: "It is difficult to be precise about this, but my experimental colleagues tell me that the observation could have been made long before 1965, probably in mid-1950s and perhaps even in mid-1940s." Is this correct?

In the autumn of 1983, Dr. T. Shmaonov of the Institute of General Physics, Moscow, with whom I was not previously acquainted, telephoned me and said that he would like to talk to me about things relevant to the discovery of the cosmic microwave background. We met the same day and Shmaonov described how, in the middle of the 1950s, he had been doing postgraduate research in the group of the well-known Soviet radioastronomers S. Khaikin and N. Kaidanovsky: he was measuring radio waves coming from space at a wavelength of 3.2 cm. Measurements were done with a horn antenna similar to that used many years later by Penzias and Wilson. Shmaonov carefully studied possible sources of noise. Of course, his instrument could not have been as sensitive as those with which the American astronomers worked in the 1960s. Results obtained by Shmaonov were reported in 1957 in his Ph.D. Thesis and published in a paper (Shmaonov 1957) in the Soviet journal Pribory i Tekhnika Eksperimenta (Instruments and experimental methods). The conclusion of the measurements was: "The absolute effective temperature of radiation background ... appears to be 4 ± 3 K". Shmaonov emphasized the independence of the intensity of radiation on direction and time. Errors in Shmaonov's measurements were high and his 4 K estimate was absolutely unreliable, but nevertheless we now realize that what he recorded was nothing other than the cosmic microwave background. Unfortunately, neither Shmaonov himself, nor his science advisors, nor other astronomers who saw the results of his measurements, knew anything about the possibility of the existence of the relic radiation and so failed to pay the results the attention that they deserved. They were soon forgotten. When Doroshkevich and I, having completed our calculations, were calling in 1963 and 1964 on several Soviet radioastronomers with the question 'Do you know any measurements of the cosmic background in the centimeter and shorter wavelength ranges?' not one of them remembered Shmaonov's work.

It is rather amusing that even the person who made these measurements failed to appreciate their significance, not only in the 1950s — this is easy to explain — but even after the discovery of the microwave background by Penzias and Wilson in 1965. True, at that time Shmaonov was working in very different field. His attention turned to old results only in 1983, in response to semiaccidental remarks, and Shmaonov gave a talk on the subject at the Bureau of the Section of General Physics and Astronomy of the USSR Academy of Sciences. This event took place 27 years after the measurements and 18 years after the publication of the results of Penzias and Wilson.

Shmaonov's observations were reanalyzed by N. Kaidanovsky and Yu. Parijsky (1987). Their conclusions were: "Thus, one can conclude that the contribution of the relic radiation [CMBR] was $T_{rel} = 2 \pm 1$K. Of course the result is rough but unambiguous". In autumn 2003 I discussed the situation with both N. Kaidanovsky and Yu. Parijsky. They confirmed this conclusion. Kaidanovsky emphasized that at the time of the measurements (1955-1956) the reality of Shmaonov's results were out of any doubt, but unfortunately there was not any theorist who could tell them the possible interpretation.

Fate takes unexpected and tortuous turns. Nevertheless, the entire story is very instructive. To hit upon a phenomenon is not yet equivalent to discovering it. One has to realize the significance of the find, and give the correct explanation. A combination of circumstances and sheer luck do play a role here — no doubt about it. Nevertheless, success does not come by accident. Success requires lots and lots of work, vast knowledge, and persistence in the work itself and in bringing the results to the attention and recognition of others.

In conclusion I want to say that just after the discovery of the CMBR we in the group of Prof. Ya. Zel'dovich started to work on the theory of the origin of galaxies and large scale structure of the Universe (see for example Doroshkevich, Zel'dovich, Novikov, 1967) and on other physical processes in the Hot Model. Analogous works started in the group of Prof. V. Ginsburg, Prof. I. Shklovsky and other groups in the USSR.

Andrei G. Doroshkevich: Cosmology in the Sixties

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For cosmology the sixties were a very active period. Over these years the rapid development of observational techniques offered many discoveries, including the relic radiation (the CMBR), X-ray sources and quasars. At the same time significant progress had been achieved in the understanding and description of the nature of black holes. These discoveries have demonstrated the complex nature of the Universe composed of a set of objects with a wide variety of properties and evolutionary histories.

This progress immediately rearranged cosmology and transformed it to the physical theory focused on the interpretation of new phenomena and the construction of corresponding physical models. These models have opened up new fields of application for classical and quantum physics in exotic situations such as the early stages of the evolution of the Universe and objects with super-strong pressure, magnetic and gravitational fields. Theoretical analysis of such problems had begun already in earlier publications of Landau, Oppenheimer, Volkoff, Snyder, Tolman, Gamow, Alpher, Herman and some others. However, only at this period did the application of abstract theoretical constructions to observed objects become possible.

For cosmology as a whole the discovery of the CMBR was most important. In this context the key role must go to G. Gamow, who formulated the hypothesis of the hot Universe using very limited observational estimates of the chemical composition of old stars. Unfortunately his expectations of the direct observations of the CMBR were not so promising and they were not realized over an extended period after his predictions.

The development of Soviet cosmology at this period is well reviewed here by I. Novikov. I can only repeat that in 1963 – 1964 Novikov and I began to work in Zel'dovich's group and were interested in manifestations of the properties of galaxies. Thus, we calculated the background radiation produced by galaxies and found that it is quite small in the millimeter wavelength range. (It is interesting that this inference remains valid up to now.) For comparison we plotted in the same figure the Planck spectrum of the relic radiation for the temperature $T = 1\,\mathrm{K}$. From this figure it was obvious that the CMBR can be successfully observed by high precision measurements at suitable wavelengths. The main problems were linked with the technical limitations and reduction of atmospheric noise. Unfortunately, owing to very limited contacts between Soviet and Western astronomers, our publication (Doroshkevich and Novikov 1964) has remained unknown many years.

In the sixties the very limited available information about the discovered phenomena stimulated construction of a wide set of cosmological models, the majority of which are forgotten now. None the less some of such models can be restored in the light of the new observational evidence in favor of weak large-scale anisotropy of the Universe. Of course, such models must be close in main features to the now accepted ΛCDM cosmology which quite well describes the main observations. Beyond that point, according to presently discussed simple models of cosmological inflation, the primordial anisotropy, global rotation and magnetic field catastrophically decayed. This means that there are problems that call for further observational and theoretical investigations.

However, the list of the parameters used for the analysis of the intensity and polarization of the CMBR can be extended and, for example, include the possible contribution of small-scale initial entropy perturbations and possible complex composition of the dark matter component. Such analysis strongly restrict amplitudes of these "hidden parameters" but cannot reject them. At the same time, it can noticeably change the derived parameters of the ΛCDM cosmological model.

These comments are called on to demonstrate a succession of many basic ideas in cosmology which have been discussed in the sixties and remain relevant up to now.

Rashid Sunyaev

to come...

Arno Penzias: Encountering Cosmology

Arno Penzias shared the 1978 Nobel Prize in Physics with Robert W. Wilson for their discovery of the cosmic microwave background radiation. He joined the staff of Bell Laboratories after graduate school and remained there until retiring as Vice President of Research and Chief Scientist. He then joined New Enterprise Associates, a Silicon Valley venture capital firm, where he advises emerging companies in the fields of information technology and alternative energy sources.

My first serious brush with cosmology came in 1958, when Charles Townes accepted me as one of the students in his radio astronomy group at Columbia University. My project was to be the first maser-based astronomical study of 21-cm line emission from neutral atomic hydrogen. At that time, the only known source of radio line radiation, neutral atomic hydrogen had by then been studied by several groups of observers. Since my system would yield an order of magnitude improvement in sensitivity over the best systems available to other radio astronomers, it seemed to me that I could extend trails already blazed in interesting directions. All I had to do was pick the most interesting body of prior work.

The choice of this observing project stemmed from a review of the then current radio astronomy literature, most notably a special (January 1958) issue of the Proceedings of the Institute of Radio Engineers devoted entirely to radio astronomy. From the first, I was most taken by an article (Heeschen and Dieter 1958) that addressed an interesting puzzle: clusters of galaxies appeared to contain more mass (determined from dynamical studies) than could be accounted for by the sum of the masses of their constituent objects. According to the data reported in this article, that discrepancy could be accounted for by the large amounts of neutral atomic hydrogen observed within each of the clusters investigated by the authors. Having selected an HI survey of clusters of galaxies as my target, I proceeded to design the maser preamplifier and other components that I would need, to create a low-noise radiometer for my radio telescope—an 85-foot parabolic antenna owned by the US Naval Research Laboratory. (In practice, that meant installing my equipment on a mount that could only be reached by a forty-foot scaffold, and servicing it with cryogenic liquids. Small wonder then, that I later became so attracted to the cozier geometry of the Bell Labs' horn-reflector antennas).

In order to stabilize my system against gain fluctuations, I employed a scheme in which the maser input was switched between the antenna's feed horn and an attenuator immersed in the same helium bath that cooled my maser preamplifier. When finally completed and installed on the radio telescope, the system performed perfectly, yielding scans across the sky with unprecedented sensitivity, limited only by the thermal noise expected from the system. To my dismay, however, my data showed little, if any, trace of the hydrogen the literature promised. I made further searches at longer integration times to improve sensitivity, but found nothing more than traces of continuum radiation from individual galaxies. By then, my time on the telescope had run out, leaving me with enough to qualify for my PhD, but far less data than I had hoped for.

With my degree, and hands-on knowledge of how best to apply cryogenic radiometry to microwave radio astronomy, I applied for a temporary job at Bell Labs Radio Research Laboratory, an organization in which David C. Hogg was then a leading contributor, and began working there in the late spring of 1961. At that time, this group's satellite communications research infrastructure made it the best place to continue my project and bring it to a more satisfactory conclusion. In his essay, Dave Hogg describes that project in the broader context of the work which surrounded it, together with accounts of the first glimpses of the CMBR.

When I arrived at Bell Labs early in May of 1961, the 20-foot horn reflector was still being used in the last stages of the Echo satellite project (Fig. 9). In the interim, preparing for my planned project left me with time to complete the write-up of my thesis, and to initiate a search for line emission from interstellar OH radicals, using the same horn reflector that Dave Hogg and his collaborators (DeGrasse, Hogg, Ohm and Scovil 1959) had used in the pioneering 5-cm studies recounted in his section.

During this time, I also helped my engineering colleagues by applying radio astronomy techniques to solve a series of technical problems — starting with devising a way to calibrate the pointing accuracy of satellite receiving systems by tracking radio astronomy sources as they moved across the sky.

In the pointing project, I made use of the fact that Bell Labs experimental satellite receiving systems were designed to function as radiometers as well as receivers — so as to provide a convenient means of measuring each system's sensitivity (normally expressed in units of equivalent noise temperature), as well as a way of monitoring atmospheric attenuation. As a result of this work, most early commercial satellite receiving systems were also configured to operate in a radiometric mode. In that way, operators could use celestial radio sources as reference objects for antenna pointing as well as measuring overall sensitivity. This practical work allowed me to stay connected to the work going on around me, even though the majority of my time continued to be spent on radio astronomy.

Figure 9: The 20-foot "home-made" horn reflector antenna in the foreground designed by A. B. Crawford (head of the Bell Labs department I joined in 1961) served as the Lab's receiving station for Echo, the world's first communications satellite project. Unlike this horn antenna, whose response pattern is tightly grouped about its main beam, the commercial 60-foot (diameter) parabolic antenna, shown in the background, picks up an appreciable amount of radiation from the ground, via spillover of the (downward-facing) feed horn located at its focus.

In the meantime, the 13-cm Echo receiver was removed from the 20-foot antenna and replaced by a 7-cm receiver— the wavelength employed by Telstar, the follow-on satellite project to Echo, thereby delaying my access to that antenna until shortly after Telstar's successful launch in July of 1962. At that point, the Holmdel horn, and its new ultra low-noise 7-cm traveling wave maser, became available for radio astronomy — subject only to the concurrence of local management: Rudi Kompfner, the Director of our Laboratory. All I had to do was give a seminar-like talk outlining the research topics that seemed most interesting. Reasons to use the 7-cm system before moving to 21-cm seemed almost self-evident. Two-wavelength measurements of astronomical objects (most notably our own galaxy) with the same instrument would yield valuable spectral information. This stroke of good fortune came at just the right moment. A second radio astronomer, Robert Wilson, came from Caltech on a job interview and was hired. In addition to finishing our separate projects, we set to working together early in 1963.

At that time, Bob was also working with Dave Hogg, who had came up with a novel way of measuring the effective collecting area of the Andover antenna (AT&T's primary satellite ground station). The idea was to measure our 20-foot horn by means of a helicopter-borne source, use that calibration to measure the absolute flux of strong "radio stars," and then use the antenna temperature obtained with the Andover antenna with those sources, to determine the collecting area of that antenna (Hogg and Wilson, 1965).

This addition to our program appears to have left an indelible mark on the folklore of cosmology. Once the relatively-elaborate helicopter data had been collected, we were unable to modify the antenna in any way, until the related flux measurements of discrete radio astronomy sources (intended as intermediate flux standards) had been completed. As a result, while we were able to evict a pair of band-tailed pigeons from their preferred resting place in the throat of our antenna, removing all signs of their prior presence had to be deferred for several weeks after the start of our observations. Once we had measured the flux densities of Cas A and the other discrete radio sources whose absolute fluxes we wished to establish as calibration objects for future use, we cleaned the throat of bird droppings and found, as expected, no measurable increase in antenna efficiency, and only a minor diminution in antenna temperature.

In putting our radio astronomy receiving system together we were anxious to make sure that the quality of the components we added were worthy of the superb properties of the horn antenna and maser that we had been given. We began a series of radio astronomical observations, including the ones that I had proposed so as to make the best use of the careful calibration and extreme sensitivity of our system. Of these projects, the most technically challenging was a measurement of the radiation intensity from our galaxy at high latitudes. In particular, we needed to resolve the uncertainty surrounding the seeming extraneous sources of system noise encountered by several of our Bell Labs colleagues, and described in Dave Hogg's section (beginning on page 53).

This multi-year endeavor, which resulted in our discovery of the cosmic microwave background radiation — the CMBR — is described in detail in Bob Wilson's (1979) article on the subject. Briefly, we spent most of 1963 converting the horn to radio astronomy. A mechanically-based coordinate converter which allowed us to move the antenna in right ascension and declination, together with the cold load, a carefully built switch and back end electronics, were the main items that we added.

Since we planned to depend on our "cold load" as a noise standard (Penzias 1965), I decided to first design the microwave device I wanted, and then worry about how I might cool it. Clearly, I would use an absorber immersed in liquid helium, and connected to its (room temperature) output flange by a waveguide. Instead of the plated stainless steel generally used in cryogenic microwave spectroscopy, I opted for a meter-long section made of the well-behaved high-copper brass alloy used in AT&T's microwave radio towers because of its low attenuation. In addition to thinning the walls of the waveguide by machining away material from its outside surfaces, to reduce its thermal conductivity, I added a series of gas baffles to allow evaporating liquid helium to cool the transmission line as this gas flowed upward toward the vacuum pump connector. Calibrated thermistor diodes, attached to each of the baffles as well as other key points along the waveguide, allowed us to monitor its temperature profile — thereby allowing us to calculate the noise temperature at its output flange to greater accuracy.

Owing to the large thermal mass and size of the Dewar flask which contained the cold load, each day's fill consumed the contents of a 25-liter helium container. Since each such fill lasted through a full day and night of observation, we were almost always ready to quit working well before our helium ran out. Remarkably, our local carpenter shop — headed by Carl Clausen, a long-time employee who had built the antenna that Karl Jansky had used in the 1930s — managed to build the 20-foot horn for a mere $20,000. On our part, Bob and I almost certainly spent more than that amount on liquid helium during the years we used that antenna for our observations.

Those observations began in late May of 1964 — with us working to collect data, while also tracing possible sources of the excess antenna temperature which proved to be the CMBR. By then, it seemed unlikely that the excess temperature was due to measurement errors, since three independent measurements had yielded similar results. Was it then due to the receiver, the antenna, or something outside the maser systems themselves? Our first observation exonerated the receiver. Figure 10 contains readings from each of the cold load's (eleven) thermistors, together with a temperature reading from a thermometer attached to a variable attenuator which connected the cold load to one of the two input ports of our waveguide switch. The attenuator — a standard Western Electric component with its resistive absorber replaced by a much less lossy material — had a range of 0.12 decibels, or about 10 K when used at room temperature.

As can be seen from the chart, the antenna temperature at 90° elevation was observed to be approximately 3 K ($\sim$ 0.04 dB) hotter than the noise temperature of our cold load. We knew from our prior calibration that our cold load had an output temperature of about 5 K with the attenuator set at

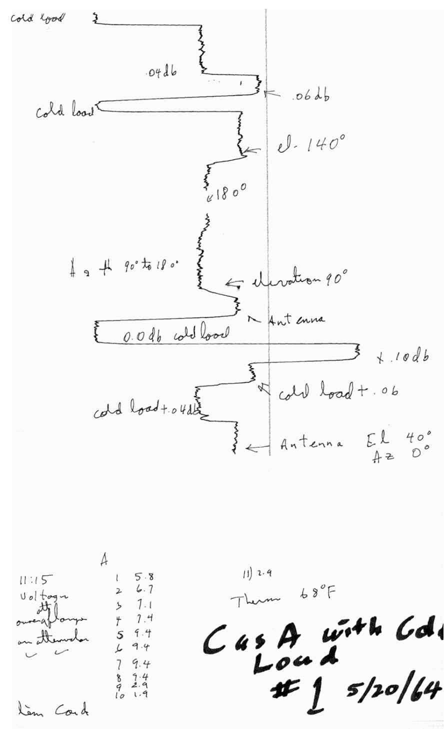


Figure 10: Record of our first 7-cm observation. Data were recorded on rolls of paper by means of a Leeds and Northrop chart recorder, with the output voltage of a detector located at the output of our radiometer plotted as a function of time. Because of the inherent stability of our system, synchronous detection was not used. The switch was turned manually between the antenna feed and the reference arm (denoted as "cold load," along with the setting on the variable attenuator described in the text).

zero — with its precise temperature determined by the physical temperature distribution of the connecting waveguide, and calculated from the thermistor readings. Since the atmosphere's contribution to the antenna temperature at zenith was about two degrees less the physical temperature of the liquid helium bath in which the cold load's absorber was immersed — together with the fact that the antenna throat was expected to introduce roughly the same small amount of noise as that due to waveguide in the reference arm – we knew immediately that the excess noise temperature must be coming from outside our apparatus.

By the late fall of 1964, we had made all the absolute flux measurements we needed, and had exhausted an extensive list of possible terrestrial noise sources, as well as known astronomical sources. What to do? We wanted to publish our result, but were hesitant about writing a stand-alone paper. In those days, a considerable fraction of the radio astronomy literature was taken up with spurious results, and we didn't want run the risk of having our first joint publication to be cited as totally wrong. We therefore decided to include our detection of excess temperature as a section in one of the other papers then in preparation — but fate intervened.

In December of 1964, Bernie Burke and I met at an American Astronomical Society meeting in Montreal, exchanging accounts of our work and promising to keep in touch. He called me a few weeks later (late February, as I remember it) to tell me of a talk he had heard about (from Ken Turner) — saying that there was "a guy from Princeton" with a theory predicting "ten degrees at X-band" (radio engineering jargon for the microwave band around 3-cm wavelength). Bernie's mimeographed copy of Jim Peebles' preprint arrived in my office a few days later. Sure enough, the abstract contained a prediction of 10 K radiation, confirming what Bernie had told me over the telephone. I was happy to find a theoretical explanation for our puzzling phenomenon, even though I wasn't sure that the general model described in the paper was necessarily the right one. I don't remember paying much attention to the details of the cosmological theory, other than that it mentioned a cyclical universe model, apparently proposed by Bob Dicke, who had organized an experimental search for this phenomenon at 3-cm wavelength.

I immediately picked up the phone, and was soon speaking with Bob Dicke — catching him in the middle of a meeting with Jim Peebles, Peter Roll and Dave Wilkinson. Rather than saying that he would call me back after his meeting, as I thought he would, he and I began a conversation that lasted for some considerable time as I told him about our discovery, and the additional work that we had done in this connection in the months that had followed it. At the end of our conversation, I invited him to come and have a first-hand look at our apparatus and data, resulting in a visit by Dicke, accompanied by Peter Roll and Dave Wilkinson, to Crawford Hill shortly thereafter.

As soon as the group arrived, Bob Wilson and I brought them to the horn antenna where all five of us managed to squeeze into our control cab in order to give our visitors a first hand look at our equipment. Bob Dicke looked over what we had, asked a few questions, nodded and agreed that we had a real result. From there, we moved to a conference room in our main building, where I gave a presentation explaining the motivation behind this portion of our work in the context of our galactic continuum project.

Apparently, I assumed more understanding of radio astronomy than the group possessed at that time, because Peter Roll remembers me talking about M31 (the nearby galaxy in Andromeda) and him thinking that we were interested in the sky background in order to aid in our measurements of that galaxy's emission. At that time, I understood that they knew more about their areas of expertise than we did, but it didn't occur to me that the inverse of my assumption (that they knew less about radio astronomy techniques than we did) could be true as well.

This latter situation became clearer when Bob and I paid a return visit to Peter and Dave's lab a short while later. Thanks to synchronous detection, they had effectively eliminated the effects of random noise in their measurement. But they had done less well with systematic uncertainties — especially with their cryogenic noise standard. In particular, I remember the waveguide being covered with frost and condensed water where it emerged from a metal flange atop their liquid helium bath. To me, they seemed to be making many of the mistakes that I had made in my first encounter with such problems in my thesis experiment, and had solved in designing the "cold load" and related apparatus for our 7-cm system. On the other hand, they might just have underestimated the precision they would need, expecting a more intense level of background radiation than the level we had detected. Either way, I went through some of the ways in which they might improve their design details — an area that we hadn't touched upon during their visit to our facility.

Earlier on, toward the end of his visit to Crawford hill, I remember discussing publication with Bob Dicke and suggesting a joint paper. For his part, Dicke refused immediately, leading me to then propose a pair of back-to-back papers in the *Astrophysical Journal* — the same place that our Cassiopeia-A, and galactic continuum papers were soon to go.

Our paper (Penzias and Wilson 1965) consisted of a bare-bones account our measurement — together with a list of the possible sources of interference we had eliminated — along the lines of what I would have included, had this result been a section of our paper on the 7-cm galactic continuum (Penzias and Wilson 1966a). As a result, we submitted the write-up without a single mention of astronomy. We only added an additional sentence — stating this phenomenon could not be accounted for in terms of sources known to exist in the present universe — some days after we had sent the original version off to the journal. By the time our correction arrived however, the editor had already accepted the original version for publication. Not wishing to withdraw the paper, and replace it with a revised copy, we accepted the editors offer of including that sentence as a "note in proof."

Notwithstanding the rapid acceptance of our paper, actual publication of the pair of CMBR papers was held up until July 1, with the issues themselves mailed out in the early fall. In the interim, another form of publication took over on May 21st, with a front-page New York Times article headlined: "Signals Imply a 'Big Bang' Universe." Walter Sullivan (1965), then the dean of American science writers, apparently had a "mole" in the *Astrophysical Journal* editorial office. At that time, Sullivan was hoping to get an early look at an expected submission by Alan Sandage, whom he thought was then about to report observations of particular cosmological significance.

The article reported our discovery and the prediction at Princeton, noting that: "It is clear that Dr. Dicke, and others would like to see an oscillating universe come out triumphant. The idea of a universe born 'from nothing' in a single explosion raises philosophical and well as scientific problems." At the time however, the likelihood of resolving such cosmological issues seemed remote to me. My first reading of Jim Peebles' preprint had linked it to the cyclic model in my mind (and only later made the Gamow connection) even though Jim didn't have a strong connection to oscillation. Jim seemed to favor a cold early Universe — more like the one I heard about from David Layzer later that same year. In those days, each of the principal cosmological theories seemed to be as much about of personal preferences as it was about data, at least as far as those of us outside the field could tell. After all, it had taken until the mid nineteen fifties for the Hubble age of the universe to catch up with the age of the oldest stars.

But then, the link between theory and data began to strengthen markedly once the Times article appeared. Most importantly, unexpected confirmation appeared from an unexpected direction, in the form of a trio of independent analyses by George Field and John Hitchcock (1966), Pat Thaddeus and Paul Clauser (1966), and Iosef Shklovsky (1966) — each inferring a 3 K temperature of the CMBR at millimeter wavelengths, and all making use of published optical spectra which indicated an otherwise puzzling excitation of interstellar radicals.

Ironically, George Field and I had discussed the optical CN data and its possible connection to radio astronomy, albeit in an entirely different context. In writing up my thesis, I had found myself faced with puzzling theoretical issues I couldn't figure out on my own, so I sought help from George, who was still at Princeton in those days. Some time later, I sought George's help again in connection with a search for line emission from interstellar OH radicals. In both cases, excitation of the emitting gas came up as an issue, and I recall discussing McKellar's CN observations with him, although our memories differ a bit. I recall George mentioning it during our OH discussion, while George remembers it taking place in connection with intergalactic hydrogen. Nonetheless, George made that connection for me with respect to my spectral studies, and later connected the CN excitation phenomenon to the CMBR. For my part, I didn't. While I adopted an estimate of 2 K as the lower limit of radiative excitation for OH radicals (Penzias 1964), I assumed that this "radiative excitation" was due to starlight, that is, confined to wavelength regions much shorter than the one associated with the 17-cm and 21-cm lines studied in my observations.

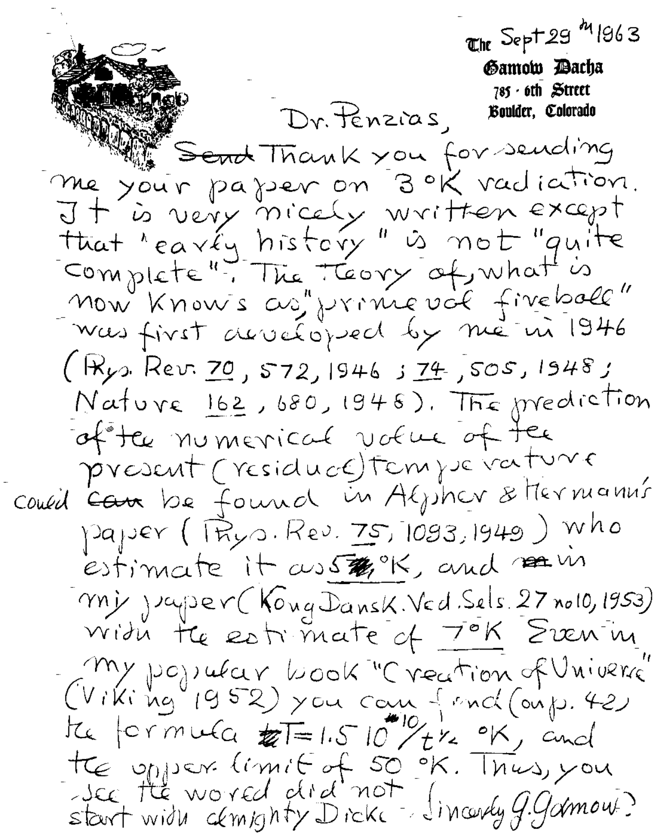
I realized my oversight a short while after the New York Times article appeared, when I visited Pat Thaddeus in his office. As Pat greeted me with "There's another way of measuring the ...", I glanced down and saw Herzberg's book on the table in front of him. The pieces of the puzzle were coming together faster than I could have imagined just a few weeks earlier.

As for another piece of the puzzle, the connection to the prior work done at Bell Labs, I remember being astonished to learn about Doroskevitch and Novikov's (1964) linking of Ohm's (1961) report of his 13-cm noise measurements to the CMBR implications stemming from the "Gamow Theory." At a time when being "plugged in" usually meant being on key colleagues' preprint lists, keeping up in astronomy generally depended on participating in the informal exchanges that marked life in academic departments — something that Bell Labs couldn't be expected to provide for its radio astronomers.

Fortunately, I soon found such a connection, when Lyman Spitzer invited me to give a colloquium sponsored by Princeton's astronomy department. From that time on, I became an increasingly active participant in the science and teaching of that department — a relationship that lasted well into the l980s.

Other than the single pair of March 1965 visits already touched upon, Bob and I had little direct contact with the members of Dicke's group during the remainder of that year. In the meantime, Bob and I made the two additional 7-cm CMBR measurements described in his section, confirming our original result in both cases. By the time Peter Roll and Dave Wilkinson reported the results of the 3-cm measurements made with their reworked system, the following January (Roll and Wilkinson 1966), they had evidently solved the problems we had noticed in their earlier attempt, judging from the fact that their result produced "the right answer" — matching our 7-cm values, the earlier Bell Labs results at 5-cm and 11-cm, and the work done (on what was by then being called the 3 K radiation) at 2.3 mm from the CN results.

In the meantime, the connection with Gamow's earlier work, and the



The Sept 29th 1963
Gamow Dacha
785 - 6th Street
Boulder, Colorado

Dr. Penzias,

~~Send~~ Thank you for sending me your paper on 3°K radiation. It is very nicely written except that "early history" is not "quite complete". The theory of what is now knows as "primeval fireball" was first developed by me in 1946 (Phys. Rev. 70, 572, 1946; 74, 505, 1948; Nature 162, 680, 1948). The prediction of the numerical value of the present (residual) temperature could ~~can~~ be found in Alpher & Hermann's paper (Phys. Rev. 75, 1093, 1949) who estimate it as 5°K, and in my paper (Kong Dansk. Ved. Sels. 27 no 10, 1953) with the estimate of 7°K. Even in my popular book "Creation of Universe" (Viking 1952) you can find (on p. 42) the formula $T = 1.5\ 10^{10}/t^{1/2}$ °K, and the upper limit of 50 °K. Thus, you see the world did not start with almighty Dicke.

Sincerely G. Gamow

Figure 11: Gamow's letter.

predictions that stemmed from it, gained increasing attention in the scientific community — in my case, via a personal letter from George Gamow himself. This letter (in Fig. 11, misdated 1963, for some reason) begins by thanking me for sending him my "paper." Since this could only have referred to a preprint of our 1965 *Astrophysical Journal* article, which included a sentence connecting our findings to the accompanying article by Dicke *et al.*, I assumed that someone else had sent him a mimeographed copy. In those pre-computer days, a dedicated organization in Bell Labs distributed copies of papers submitted for outside publication by means of the same system used for internal technical memos, a kind of paper-based "Google" that employees could search by looking through an index based on authors and topics, and then "downloading" content for delivery via our company's internal mail service. In addition to the copies that Bob and I sent to colleagues, some of our colleagues in the Physical Sciences Division likely sent copies to some of their friends as well.

As I noted earlier, the connection to the predictions cited in Gamow's letter had been made by others even before the events recounted above. As time went on, and the agreement between theory and data grew stronger, many of us began to wonder why the measurements involved hadn't been attempted earlier. In my case (Penzias 1979), I went so far as to attribute Ralph Alpher's apparent endorsement of Gamow's position (that bolometric measurements of the relict radiation would be confused with other sources of radiant energy, as outlined in a 1948 letter to Alpher and Robert Herman) as a demonstration of his having overlooked the possibility of microwave measurements. As I learned from Bernie Burke only recently, however, Alpher had indeed queried at least one radio astronomy group about the possibility of making microwave measurements like those recounted in the present volume, but was told that it couldn't be done (page 120).

With the results of present-day CMBR measurements judged significant enough to be taught even in some high schools, it may be hard for contemporary readers to imagine a circa 1950 radio astronomer turning down such an "opportunity." Nonetheless, a more careful look at the state of radio astronomy in those days makes such a turn-down far more understandable. First of all, there were no idle radio astronomers. The first few radio observatories were just being set up, and almost anything they did would break new ground — at least as long as the rudimentary equipment they used worked well enough to produce useful data. Given such circumstances, together with the amount of effort a CMBR measurement would have required, it's not hard to imagine someone being likely to consider such an undertaking outside the realm of possibility.

Thanks to experience and improved techniques however, CMBR measurements began to look almost easy just a few years after the initial report of our discovery. In 1967, for example, Dave Wilkinson and his co-workers reported a trio of highly-consistent CMBR measurements done at three different wavelength regions (Wilkinson 1967; Stokes, Partridge and Wilkinson 1967). Given the speed and precision of this work, it's understandably easy to overlook the fact that the same group's first 3-cm measurement took the better part of two years from start to submission for publication. Moreover, that 3-cm project had far better resources than any that would have been available fifteen or so years earlier — along with the additional advantage of experience gained from familiarity with a successfully-completed project similar to theirs.

Small wonder then, that a potential CMBR experimenter would have balked at anyone proposing such an undertaking back in the nineteen fifties — especially with no more incentive than what was then a tenuous link between an unproven theory and hypothesized data. Under such circumstances, it's not hard to imagine a radio astronomer of that year saying "it can't be done," nor is it hard to imagine the subsequent frustration felt by George Gamow and his colleagues as the events of 1965 began to unfold.

By early 1966, Bob and I had completed our observations with the 7-cm system, and installed a newly-built 21 cm system in its place. Our CMBR measurements at this new wavelength went smoothly, and we were able to report that result later the same year (Penzias and Wilson 1966b). Here, for the first time, we found "company" in the form of a similar measurement made by Howell and Shakeshaft (1966), allowing us to compare the results of two independent measurements at the same wavelength. Since the raw data (the sum of the CMBR and galactic radiation) in the two measurements differed by only 0.2 K, the combined result yielded an accurate determination of our galaxy's spectral behavior — one of the items on my earlier research agenda. While we continued our 21-cm HI studies for another year or so, our CMBR studies had come to an end. In its place, we began a long-term effort aimed at following the CMBR's companion thread in cosmology — the origin of the elements — by studying the chemical and isotopic composition of interstellar space.

In this endeavor, Bob and I once again moved to a new wavelength range — this one centered on the atmospheric window which stretches from 75 to 150 GHz (4-2 mm). In contrast to the small handful of hyperfine lines available to microwave radio astronomers, the then still-unexplored millimeter-wave portion of the astronomical spectrum encompasses a rich variety of molecular rotation lines. Fortunately, several of the key components required for such work had been developed for communications research purposes. With much help from Charles Burrus, one of our Bell Labs colleagues, Bob and I assembled a millimeter-wave receiver. Completed in the spring of 1968, I carried it to a precision radio telescope owned and operated by the National Radio Astronomy Observatory at Kitt Peak, Arizona, for preliminary continuum observations. Until we introduced our receiver, millimeter-wave observations with that telescope had been limited to bolometric measurements. Following the success of our continuum work, and the subsequent installation of an NRAO-built spectrometer "back end," we — together with a number of collaborators from other institutions — discovered and studied a number of interstellar molecular species, thereby revealing the rich and varied chemistry which exists in interstellar space.

Since that time, millimeter-wave spectral studies have proven to be a particularly fruitful area for radio astronomy, and are the subject of active and growing interest, involving a large number of scientists around the world. The most personally satisfying portion of this work for me was using molecular spectra to explore the isotopic composition of interstellar atoms — thereby tracing the nuclear processes that produced them. Most notably, our discovery of the first deuterated molecular species found in interstellar space (Wilson, Penzias, Jefferts and Solomon 1973) enabled me to trace the distribution of deuterium in the galaxy. This work (Penzias 1979b) provided the first direct evidence for the cosmological origin of this unique isotope, which by then had earned the nickname "Arno's white whale" among my observing colleagues. Of all the nuclear species found in nature, deuterium is the only one whose origin stems exclusively from the explosive origin of the Universe. Because deuterium's cosmic abundance serves as the single most sensitive parameter in the prediction of cosmic background radiation, these measurements provided strong support for the "Big Bang" interpretation of our earlier discovery.

Robert W. Wilson: Two Astronomical Discoveries

Bob Wilson shared the 1978 Nobel Prize in Physics with Arno Penzias for their discovery of the cosmic microwave background radiation. Wilson is a Senior Scientist at the Harvard-Smithsonian Center for Astrophysics and Technical and Computing Leader of the Sum-Millimeter Array Project.

As a child I acquired an interest in electronics from my father. I also learned from him that I could take apart almost anything around the house, probably fix it, and then reassemble it successfully. In my high school years I fixed radios and later television sets for spending money and built my own hi-fi set. Thus when I enrolled at Rice University, I declared a major in electrical engineering. During my freshman year I switched to physics after realizing that much of the EE course work would be in power engineering. Having read my father's copies of *Review of Scientific Instruments* I realized the physicists had the interesting instruments (good toys). At Rice and later at Caltech I had two formal courses in electronics for physicists. My earlier interest had prepared me to enjoy and absorb this material thoroughly. My senior thesis at Rice was centered on designing and building a current regulator for a high field magnet in the low temperature physics group. These early experiences, especially the trouble-shooting skills I learned in my high school days, have served me well while fixing many problems with radio telescopes.

I entered graduate school in the physics department at Caltech in 1957 after receiving my B.A. in physics at Rice earlier that year. I had no clear idea of what I wanted for a thesis topic. During my first year I became friendly with David Dewhirst who was visiting from Cambridge University and was using the original Palomar Sky Survey plates in the basement of the astronomy building for identifying 3C radio sources (the Third Cambridge Catalog [Edge, Shakeshaft, McAdam, Baldwin and Archer 1959]). After David learned of my interest in instrumentation as well as physics, he suggested that I consider working with the new radio astronomy group which John Bolton had formed. There was the added enticement that they wanted to make maser amplifiers for the telescopes. The original Owens Valley Radio Observatory 90-foot antennas were nearing completion and it was an ideal time to join such a group.

My thesis was intended to be interferometric observations with these antennas at the 21-cm hydrogen line. I built the local oscillator and other parts of the receiver system for those observations. That project stretched out and my actual thesis was based on an intervening project John Bolton had started me on; making and interpreting a map of the plane of the Milky

Way at 960 MHz (Wilson and Bolton 1960). We used one of the two Owens Valley 90-foot antennas before interferometric observations started. I used load switching against a liquid nitrogen cooled load and scanning or drifting from the west to the east across the Milky Way. I covered up to about 20 degrees either side of the plane of the Milky Way — enough that the radiation was falling off very slowly at the edges of my map. Having no better reference, I took the edges of my map to be zero. Since we are inside the Milky Way, it was clear to me that this technique only worked because the Milky way is very thin compared to its diameter. I knew I did not have a true zero reference for my map. It is interesting in retrospect that I added 2.8 K to my observations to improve the comparison to a lower frequency survey in analyzing the radiation from the Galactic plane into thermal and non-thermal components (Wilson 1963).

My only cosmology course at Caltech was taught by Fred Hoyle. While I had not had a course in general relativity, Hoyle's lectures did not require an understanding of the tensor math which general relativity is based on. Philosophically, I liked his steady state theory of the universe except for the fact that it relied on untestable new physics.

After a one year post doc at Caltech doing 21-cm line and polarization interferometry, I took a job at Bell Labs' Crawford Hill Lab. A major attraction there was the 20-foot horn-reflector antenna, and the promise that Arno Penzias and I could use it for radio astronomy. A second reason I was favorably inclined toward Bell Labs resulted from the help they had given the radio astronomy group. They had offered Caltech the opportunity to send someone to work in the group which had designed traveling wave masers and make a pair for the observatory. Traveling wave masers were the lowest noise receivers at that time. I had hoped to be the person to go, but because I needed to finish my thesis, V. Radhakrishnan was chosen to go to Bell Labs. I worked closely with him to put the masers to use and developed a very positive opinion of the people and the working atmosphere at Bell Labs.

In the late 1950s, plans were made to start working on communication satellites at the Bell Labs Crawford Hill site. John Pierce (1955) had had a long-time interest in communication satellites resulting from his science fiction writings. The first satellite tests were planned with NASA's Echo balloon. It was known that the return signal from Echo would be very weak because a sphere scatters the incoming radiation in all directions. While reading a paper by John Pierce describing the parameters required for a satellite system, Rudi Kompfner had the idea of using a traveling wave maser. Derek Scovil and his group at Murray Hill (De Grasse, Shulz-Du-Bois

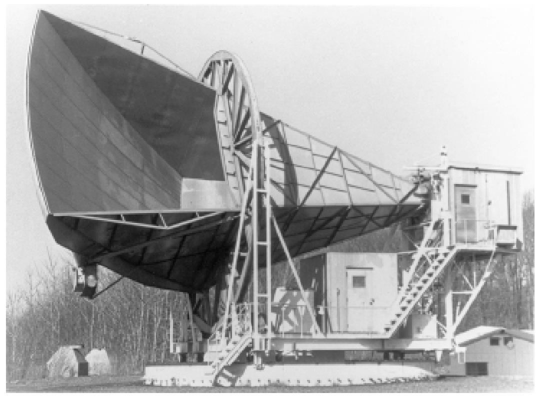


Figure 12: The 20 foot horn-reflector with its parabolic reflector on the left and cab on the right. Since the cab does not tilt, almost any kind of receiver can be conveniently put at the focus of this antenna (apex of the horn). It is clear that the horn shields the receiver from the ground, especially when it is looking up..

and Scovil 1959c) had developed them for a high sensitivity military radar. They worked at liquid helium temperatures and had a noise temperature of a few Kelvin. Even after making a room-temperature connection to it, one could have a receiver with a noise temperature of 10 K or less.

It was natural to combine a traveling wave maser with a horn-reflector antenna. The horn-reflector was invented at Holmdel by Al Beck and Harold Friis for use in a microwave relay system. In addition to turning the corner between the waveguide going up a tower and the horizontal communication path, the horn-reflector has the distinct advantage that when two of them are put back-to-back on a tower and have a very weak signal coming in on one side, a strong regenerated signal can be transmitted from the other side without interference. Its front-to-back ratio is very high. The corollary of this is that a horn-reflector put on its back, will not pick up much radiation from the Earth and will be a very low noise antenna. Therefore, Art Crawford built the large (20-foot aperture) horn-reflector pictured in Figure 12, to be used with a traveling wave maser to receive the weak signals from Echo (Crawford, Hogg and Hunt 1961).

Figure 13 shows a polar diagram of the gain of a smaller horn reflector antenna compared with the gain of a theoretical isotropic (uniform response) antenna. If we put an isotropic antenna on a field with the 300 K ground down below and zero degree sky up above, we expect it to pick up 150 K; half of its response comes from the ground. The response of the horn-reflector is

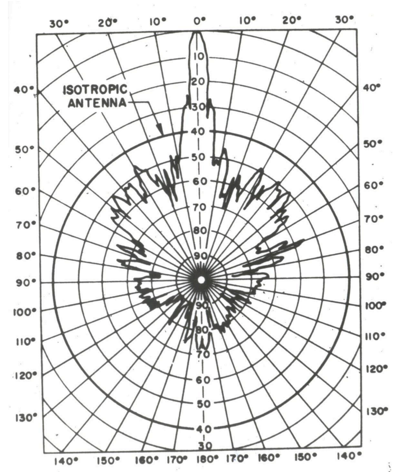


Figure 13: Polar gain pattern of a horn-reflector microwave antenna. The radial units are dB and the gain is normalized to 0 dB at its peak.

more than 35 db (a factor of about 3000) less responsive to the ground than the isotropic antenna. So one would expect less than a tenth of a Kelvin for the ground pickup from the horn-reflector.

In December of 1962 I went on a recruiting trip to Bell Labs. Of the groups I was interviewed by, I was most interested in the Radio Research Lab at Crawford Hill. I met Arno Penzias there and he showed me his OH experiment and the 20-foot horn-reflector. At that time, he had been there a year and a half. We had much more time to talk a week later at the winter American Astronomical Society meeting, where I gave a talk. He was clearly trying to get me to join him at Crawford Hill. Setting up and carrying out an observing program with the horn-reflector was certainly a job better done by two people than by one.

We were very different people and, as it turned out, had complementary skills. We made a good team for that job. Arno was as garrulous as I was reserved. He was interested in the big picture and tended to think of ways to most effectively use the resources at hand. I tended to be shy, persistent at getting all of the details correct and liked to do things myself and with my own hands. As a graduate student Arno had built a maser amplifier and made observations with it. While I had had some had experience with the maser from Bell Labs, I had worked much more on the back end signal processing electronics and antenna control at the Owens Valley Observatory.

We were both intent on making accurate measurements.

Crawford Hill in 1963 offered a remarkable environment for us to work in. Several of the people there had been at the original Bell Labs building in Holmdel in the '30s when Karl Jansky discovered extraterrestrial radio waves. They included our first department head, Arthur Crawford (unrelated to the family for which Crawford Hill was named). Crawford Hill, although part of the research arm of Bell Labs, was somewhat more mission-oriented than the rest of research. Long-haul communications was their primary focus. George Southworth had developed the waveguide at Holmdel in the '30s. They had been a strong force in designing the first microwave relay system. Many of the Members of Technical Staff (MTS) had started there before the technology of microwaves was developed and had contributed to its development. There was a strong curiosity about new things and a feeling that new fields should be understood rather than exploited for the easy solutions which might be found. Bell Labs had "written the book" on many new fields and writing comprehensive books was still an ongoing endeavor. The Bell Labs merit review system rewarded good research and recognized the value of cooperative and interdisciplinary work, something which seemed to be missing at many universities. I could find experts on many subjects at Crawford Hill or other parts of Bell Labs who were happy to help.

The Crawford Hill building was built to house the original Holmdel group when the land they had occupied since the '30s was taken over for the big lab at Holmdel. They moved in 1962. The front part of the building has a long hall with MTS offices on the front side and laboratories on the back side. Nearly every experimental MTS had a lab and often a technician to help him build things. The back part of the building had an extensive machine shop, a three person carpenter shop and a well-supplied stockroom. The machinists had a lot of experience building microwave components and were used to working from hand sketches rather than formal drawings. The head of the carpenter shop, Carl Clausen had built Jansky's original antenna in the '30s. When I arrived they were building a replica from the original drawings for the National Radio Astronomy Observatory in their spare time. These resources were available to us with little evidence of limitations from accounting.

At that time, there was no computer at Crawford Hill. Mrs. Curtis Beatty, a mathematician who had come from Murray Hill, would either write and run Fortran programs for us or take care of the complexities of running our programs on the Holmdel or Murray Hill computers. She would often fix small errors by changing the assembly code to avoid the cost of running the Fortran compiler again.

It is reported that Karl Jansky, in common with many others of his era, had built a measuring set as his first job. There were "standard Holmdel measuring sets" in many of the labs whose design probably dated from the '40s, but were logically derived from the Friis design which Jansky had used. They were very simple but effective and were capable of measuring with 0.01 dB accuracy over 10s of decibels in the microwave bands which were used for communication. I was to use these extensively in building and measuring components for our receiver for the 20-foot horn-reflector.

One might ask why two young astronomers wanted to work with such a small antenna as the 20-foot horn-reflector with its collecting area of only 25 square meters. While other radio observatories all had much larger antennas, we knew it had very special properties. First, it is a small enough antenna that one could measure its gain very accurately. It was necessary to be only about a kilometer away to be in the far field for making an accurate gain measurement. And that, in fact, had already been started by David C. Hogg. (Hogg and Wilson 1965)

The traveling wave maser amplifiers, which were available at several frequencies, would make this small antenna sensitive enough for work even with small diameter sources. For sources which were large enough to fill its beam, it would have been the most sensitive radio telescope in existence at the time. The other important thing is that we expected to be able to account for all of the sources of noise and make absolute brightness measurements. Radio astronomers don't often understand the background temperature when they do the usual on-off experiment (subtracting a measurement pointing away from the source from the measurement on the source), but the 20-foot horn-reflector offered the possibility of absolute temperature measurements. My interest in that possibility, of course, came directly from my thesis work at Caltech with John Bolton.

Soon after I went to Bell Laboratories, the 20- foot horn-reflector was released from the various satellite jobs it was doing. It had been designed for the Echo experiment which required operation at 13 cm wavelength, but it had later been used to receive a beacon from the Telstar(R) satellite. Thus when Arno and I inherited it, there was a 7.3-cm maser receiver on it (Tabor and Sibilia 1963). At that time it had a communications receiver with three low noise amplifiers connected in series which a radio astronomer would find hard to believe. The maser was followed by a low-noise nitrogen-cooled parametric amplifier which was followed by a low-noise traveling wave tube amplifier. The gain stability was unbelievably bad. Our jobs were to turn all of this into a radio telescope by making a radiometer, finish up the gain measurement, and then proceed to do some astronomy projects.

We thought about what astronomy we ought to do and laid out a plan that would take a few years. The first project was an absolute flux measurement of Cassiopeia A, the brightest discrete source at that wavelength, as well as several other bright sources. We were planning our radiometer so that we could know its sensitivity to one or two percent accuracy based on physical temperatures we could measure.

Shortly after arriving I had joined Dave Hogg to make a accurate gain measurement of the 20-foot horn-reflector. Putting these together would let us measure the standard astronomical calibration sources more accurately than had been done before. This would be a service to both radio astronomers and to the Bell System (and anyone else buying satellite earth stations). The sensitivity of an earth station could be accurately and easily checked by measuring its signal-to-noise on one of our calibrated radio sources.

I planned to follow up on my thesis by taking a few selected cuts across the Milky Way Galaxy and then confirm the spectrum of some of the sources that I had looked at. Next we wanted to check our ability to measure absolute temperatures so we could look for a spherical or halo component of the radiation from the Galaxy. Extrapolating from a lower frequency, we did not expect to see any galactic halo at 7-cm wavelength. We wanted to prove that when we did try to make such a measurement, we got a null result. After doing these projects, our plan was to build a 21-cm receiver scaled from our 7-cm receiver. We already had the maser in hand. We would then make the halo measurement and do a number of 21-cm line projects including reworking Arno's thesis of looking for hydrogen in clusters of galaxies.

At one point during that time John Bolton came for a visit so we laid out this plan of attack and asked his opinion. He said that the most important thing to do in that list is the 21-cm background measurement. He thought that it was an unexplored area and something that we really ought to do.

By the time I joined Bell Laboratories, Arno had started making a liquid helium-cooled noise source (cold load) (Penzias 1965). Figure 14 is a drawing of it with an odd perspective. There is a piece of standard Bell System 90% copper 4 GHz waveguide,, which runs from the room-temperature output flange down inside the six-inch diameter Dewar to the absorber in liquid helium. About halfway down, the waveguide is thinned to reduce its heat conductivity, and finally there is a carefully-designed absorber in the bottom. There is a sheet of mylar in the angled flange near the bottom which keeps the liquid helium out of the upper part of the waveguide and makes a smooth transition from gas to liquid. Some holes in the bottom

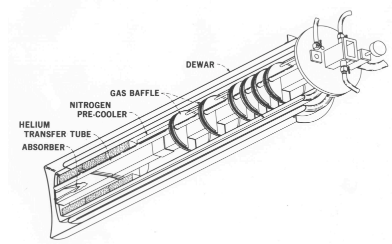


Figure 14: The cold load for our radiometer.

section allow the liquid helium to surround the absorber and there was no question of the physical temperature of the absorber itself. The heat flow down the waveguide which otherwise would have boiled the liquid helium rapidly has been taken care of by the baffles. They exchange heat between the cold helium gas leaving the Dewar and the waveguide. We realized that we had to know the radiation from the walls of the waveguide, so there is a series of diode thermometers on the waveguide for measuring its physical temperature distribution. We calculated the radiation of the walls using these temperatures and the measured loss in the waveguide.

When we first transferred the contents of a 25-liter Dewar of liquid helium into the cold load, it would fill up to a high level. We calculated the radiation temperature at the top to be approximately 5 K — just eight-tenths of a Kelvin above the temperature of the liquid helium. After fifteen hours or so (we usually ran down before the helium did), the liquid helium level would be down near the absorber and we would calculate the flange temperature to be about 6 K. Comparing it to the horn-reflector, the change agreed within something like a tenth of a degree over that period, so we felt we had a reasonably good calibration of what was going on in our cold load.

While Arno was doing that, I set up the radiometer shown in Figure 15 (Penzias and Wilson 1965b). As with most of our astronomical equipment at Bell Laboratories, this is somewhat unusual. The 20-foot horn-reflector was fitted with an electroformed throat section which made a smooth transition from the square tapering horn to the circular waveguide which had been used in the Echo receiver. After a waveguide rotary joint, a second electroformed waveguide made the transition to circular 4 GHz waveguide. We decided to use this in a switching scheme which Doug Ring and others at Crawford Hill had used in the past. It takes advantage of the fact that two orthogonal polarizations will pass through circular waveguide. The polarization coupler

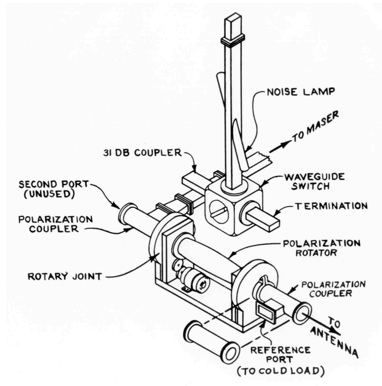


Figure 15: The switch and secondary noise standard of the radiometer used for our measurements of the flux density from the radio source Cassiopeia A and the CMBR. The noise injected by the noise tube and its coupler was calibrated in three ways against thermal sources.

near the antenna couples the signal from the reference noise source into the horizontal polarization mode traveling toward the maser and allows vertical polarization from the antenna to go straight through. The polarization rotator is the equivalent of a half-wave plate. It is a squeezed piece of waveguide with two rotary joints; another polarization coupler at the back picks one polarization off and sends it over to the maser.

By rotating the squeezed waveguide, we could switch between the reference noise and the antenna. An important aspect of this radiometer design is that except for the unused port, all ports of the waveguide were terminated at approximately the same low radiation temperature. Thus small reflections would not have a large effect. We adjusted all parts of the system to be well matched, however, and the unused port could be opened to room temperature with no effect on measurements. In addition, I added a motor to turn the squeezed waveguide to switch between the antenna and the reference noise source at 10 Hz. This, combined with a phase-sensitive detector I constructed, formed a “Dicke Switch” which was useful when measuring weak signals. After stabilizing the room temperature and all of the components of our system, the stability was so good that we usually just rotated the squeezed section by hand and recorded the receiver levels on a pen recorder.

Figure 16 shows a picture of the actual installation. The rotary joint that allowed the horn-reflector to turn while the receiver stayed stationary

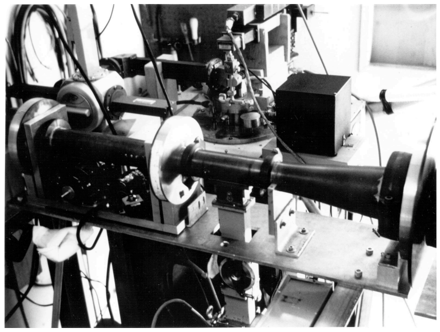


Figure 16: Our radiometer installed in the cabin of the 20-foot horn-reflector.

in the cab is at the right edge of the picture and the polarization rotator is on the left. An adjustable 0.11 dB attenuator seen at the bottom of the picture connects the cold load which is below the picture to the reference port of the switch. It could add well-calibrated additional increments of noise. The top of the maser is seen above the polarization coupler for the reference port and its massive magnet is hidden from view. The relatively large, strong cabin of the 20-foot horn-reflector, which does not tilt with respect to gravity, allowed us the freedom to build our receiver almost as though it were in a lab room and be with it during observations. The ease of working in the cabin undoubtedly contributed to our success. As graduate students Arno and I had both attached masers cooled with liquid helium to conventional antennas in which the focus tilted with elevation angle. We very much appreciated this arrangement.

Before we started making measurements with this system, there had

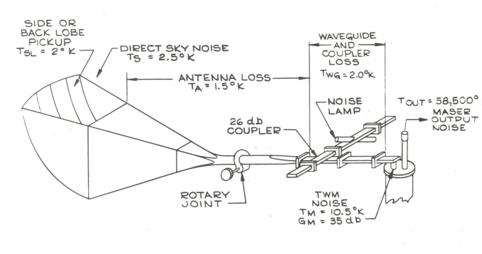


Figure 17: The assignments of contributions to the system noise temperature in the De Grasse *et al.* (1959b) radiometer.

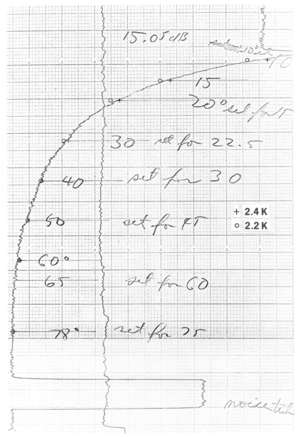


Figure 18: A measurement of the radiation from the atmosphere by a tipping experiment with the 20-foot horn-reflector. The small circles and crosses are theoretical fits of the atmosphere to the measurements and show excellent agreement with the measurements.

been careful measurements of horn-reflectors with traveling wave masers at Bell Laboratories. First, before going to the trouble of building a 20-foot horn-reflector, Dave Hogg had been asked to calculate the "sky noise" in the microwave band (Hogg 1959). To confirm his calculations the antenna and maser groups had put together a test system (De Grasse, Hogg, Ohm and Scovil 1959b). They had a 6 GHz maser and a small horn-reflector antenna. They hooked the two up with a calibrating noise lamp and saw that indeed they got a system temperature of 18. K, which was very nice, but they had expected to do a little better. You see in Figure 17 that contrary to the expected value of less than 0.1 K for ground pickup from the antenna, they have assigned 2 K to it. They assigned 2.5 K for atmosphere, and 10.5 K for the temperature of the maser. The makers of the maser were not very happy with that number. They thought they had made a better maser than that. However, within the accuracy of what the whole group knew about all the components, they solved the problem of making the noise from the components add up to the measured system temperature by assigning additional noise to the antenna and maser. Arno had used this horn-reflector for his OH project and was aware of the extra 2 K that had been assigned to it. One of the reasons that he built the cold load was to improve on their experiment.

This group had measured the atmospheric radiation (sky noise) by the

Table II — Sources of System Temperature

Source	Temperature
Sky (at zenith)	2.30 ± 0.20°K
Horn antenna	2.00 ± 1.00°K
Waveguide (counter-clockwise channel)	7.00 ± 0.65°K
Maser assembly	7.00 ± 1.00°K
Converter	0.60 ± 0.15°K
Predicted total system temperature	18.90 ± 3.00°K

the temperature was found to vary a few degrees from day to day, but the lowest temperature was consistently 22.2 ± 2.2°K. By realistically assuming that all sources were then contributing their fair share (as is also tacitly assumed in Table II) it is possible to improve the over-all accuracy. The actual system temperature must be in the overlap region of the measured results and the total results of Table II, namely between 20 and 21.9°K. The most likely minimum system temperature was therefore

$$T_{\text{system}} = 21 \pm 1\text{°K.*}$$

The inference from this result is that the "+" temperature possibilities of Table II must predominate.

Figure 19: Ed Ohm's (1961) tally of instrumental noise.

same technique that Dicke had first reported on in 1946 (Dicke, Beringer, Kyhl and Vane 1946). Figure 18 shows a chart of a such a measurement Arno and I made with the 20-foot horn- reflector. It shows the radiometer output as the antenna is scanned from the zenith (90° elevation angle) down to 10°. This is a chart with power increasing to the right, and shows what the power out of the receiver did. The circles correspond to the expected change if the zenith sky brightness is 2.2 K and the crosses to 2.4 K. You can see that the curve is a very good fit to the expected values down to at least 10° elevation. A well-shielded antenna makes an accurate measurement of the atmospheric radiation very easy.

After the 20-foot horn-reflector was built and was being used with the Echo satellite, Ed Ohm, who was a very careful experimenter, added up the noise contribution of all the components of the system and compared it to his measured total. In Figure 19 we see that from the sum of the components he predicted a total system temperature of 18.9 K, but he found that he consistently measured 22.2, or 3.3 K more than what he had expected (Ohm 1961). However, that was within the measurement errors of his summation, so he did not take it to be significant.

Our first observations with our new system were somewhat of a disappointment because we had naturally hoped that the discrepancies I have mentioned were just errors in the experiments. Figure 20 is the first mea-

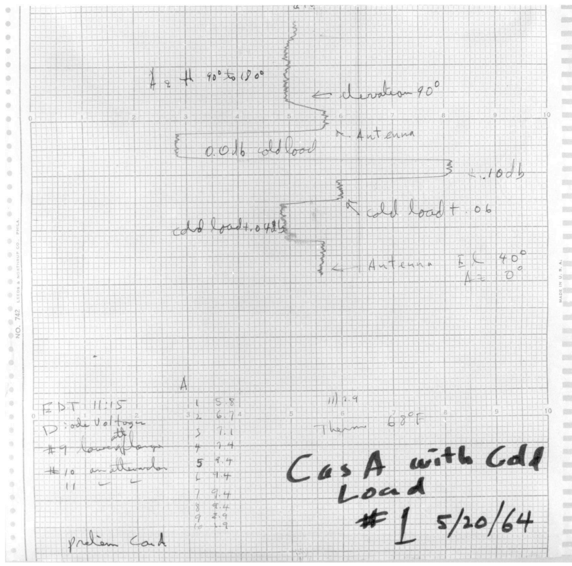


Figure 20: Record of our first 7-cm observation.

surement with our receiver. At the bottom and top, the receiver is switched to the antenna and in between to the cold load. The level from the antenna at 90° elevation matched that from the cold load with .04 dB of attenuation (about 7.5 K total radiation temperature). At the bottom I recorded measurements of the temperature-sensing diodes on the cold load.

That was a direct confrontation. We expected 2.3 K from the sky and 1 K from the absorption in the walls of the antenna, and we saw something that was obviously considerably more than that. It was really a qualitative difference rather than just quantitative because the antenna was hotter than the helium reference and it should have been colder. But we knew that the problem was either in the antenna or beyond. Arno's initial reaction was "Well, I made a pretty good cold load!" The most likely problem in such an experiment is that you do not understand all the sources of extra radiation in your reference noise source, but it is not possible to make it have a lower temperature than the liquid helium.

It initially looked like we could not do the Galactic halo experiment, but at that time our measurements of the gain of the antenna had started (Hogg and Wilson 1965) and we wanted to go on with the absolute flux measurements before taking anything apart or trying to change anything. We ended up waiting for almost nine months before doing anything about our antenna temperature problem; however, we were thinking about it all that time.

We thought of several possible explanations of the excess antenna temperature. Many radio astronomers at the time thought the centimeter-wave atmospheric radiation was about twice what we were saying. That would have gone a long way toward explaining our problem. However, the curve for the zenith angle dependence in Figure 18 indicates that we were measuring the atmospheric absorption and emission correctly. It turned out later that the centimeter astronomers had applied refraction corrections to their measurements of radio sources in the wrong sense. John Shakeshaft finally straightened this out (Howell and Shakeshaft 1967b).

Since Crawford Hill overlooks New York City, perhaps man-made interference was causing trouble. Therefore we turned our antenna down and scanned around the horizon. We found a little bit of super-thermal radiation, but, given the horn-reflector's rejection of back radiation, nothing that would explain the sort of thing that we were seeing.

Could it be the Milky Way? Not according to extrapolations from low frequencies. The galactic poles should have a very small brightness at 7-cm and our actual measurements of the plane of the Milky Way did fit very well with the extrapolations.

Perhaps it was a large number of background discrete sources. The strongest discrete source we could see was Cas A and it had an antenna temperature of 7 K. Point sources extrapolate in frequency in about the same way as the radiation from the Galaxy, so they seemed a very unlikely explanation.

That left radiation from the walls of the antenna itself. We calculated nine-tenths of a degree Kelvin for that. We took into account the actual construction of the transition between the tapering horn and the circular waveguide of the radiometer, which is the most important part. It was made of of electroformed copper and we measured waveguides of the same material in the lab to determine the loss under under real rather than theoretical conditions.

We had to wait some time to finish the Cas A flux measurement, but in the spring of 1965, almost a year later, we had completed it (Penzias and Wilson, 1965b). The Earth had almost made a complete cycle around the Sun and nothing had changed in what we were measuring. We pointed to many different parts of the sky, and unless we had a known source or the plane of the Galaxy in our beam, we had never seen anything other than the usual antenna temperature. In 1962 there had been a high-altitude nuclear explosion over the Pacific which had greatly increased the amount of Plasma in the van Allen belts around the Earth. We were initially worried that something strange was going on there, but after a year, the population of van Allen belts had gone down considerably and we had not seen any change.

There was a pair of pigeons living in the antenna at the time, and they had deposited their white droppings in the part of the horn where they roosted. So we cleaned up the antenna, caught the pigeons in a havahart trap, and put some aluminum tape over the joints between the separate pieces of aluminum that made it up. All of this made only a minor improvement.

We were really scratching our heads about what to do until one day Arno happened to be talking to Bernie Burke about other matters. After they had finished talking about what Arno had phoned him for, Bernie asked Arno about our Galactic Halo experiment. Arno told him about our dilemma of excess noise from the horn-reflector, that the Galactic halo experiment would not work, and that we could not understand what was going on. Bernie had heard from his friend Ken Turner about Jim Peebles' recent colloquium at Johns Hopkins where he described calculations of microwave radiation from a hot big bang. Bernie suggested that we get in touch with Dicke's group at Princeton. So of course Arno called Dicke. Dicke was thinking about oscillating big bangs which he concluded should be hot. After a discussion on the phone, they sent us a preprint and agreed to come for a visit. When they came and saw our equipment they agreed that what we had done was probably correct. Afterwards our two groups wrote separate letters to *the Astrophysical Journal* (Dicke, Peebles, Roll and Wilkinson 1965; Penzias and Wilson 1965a).

We made one last check before actually sending off our letter for publication. We took a signal generator, attached it to a small horn and took it around the top of Crawford Hill to artificially increase the temperature of the ground and measure the back lobe level of the 20-foot horn; maybe there was something wrong with it. But the result was as low as we expected. So we sent the letter in!

Arno and I were very happy to have any sort of an answer to our dilemma. Any reasonable explanation would probably have made us happy. In fact, I do not think either of us took the cosmology very seriously at first. I had come from Caltech and had been there during many of Fred Hoyle's visits. Philosophically, I liked the steady-state cosmology. So I thought that we should report our result as a simple measurement; after all the measurement might be correct even if the cosmology turned out not to be!

The submission date on our paper was May 13, 1965 and a few days later on May 21 my father was visiting us as part of a business trip. As was typical for him he woke up before I did and went for a walk. He came back with a copy of the New York Times which had a picture of the 20-foot horn-reflector and an article by Walter Sullivan entitled "Signals Imply a 'Big Bang' Universe" on the front page (Sullivan 1965). Besides being a very satisfying experience, this awakened me to the fact that the world was taking the cosmology seriously.

At the time of our paper, the spectrum of the CMBR was only determined by our measurement and an upper limit at 404 MHz which was dominated by Galactic radiation. This was only enough to rule out ordinary radio sources. Soon after our result became known, George Field (Field, Herbig, and Hitchcock 1966), Pat Thaddeus (Thaddeus and Clauser 1966) and Iosif Shklovsky (Shklovsky 1966) independently realized that the absorption of stellar optical radiation by CN in interstellar clouds, which had been known since 1940, could be used to measure the radiation temperature in those clouds. The measurements of those three groups indicated about 3 K for the radiation temperature at 2.6 mm wavelength. The Princeton group (Roll and Wilkinson 1966) completed their first measurement at 3.2 cm by the end of the year. Arno and I repeated our 7.35-cm measurement with a smaller horn-reflector with consistent results. We then installed a 21-cm receiver on the 20-foot horn-reflector and made a measurement (Penzias and Wilson 1967) which was consistent with Howell and Shakeshaft's 21-cm measurement (Howell and Shakeshaft 1966) made about the same time. In approximately a year there were seven measurements consistent with a 3 K CMBR, but it would be more than a decade before the spectrum was proven to be a black body spectrum rather than a gray body, and thus definitively from the early universe. The details of these early measurements are covered by other essays in this collection.

Looking back, it is a bit surprising how quickly our results were accepted among the astronomers I talked to. It probably helped that the steady state theory was failing to fit observations and Bell Labs had a reputation for doing good science. There were only a couple of occasions where I was challenged about the correctness of our measurements. More often, paradigm changes of that magnitude are resisted much more by established scientists.

It is interesting to compare the equipment Arno and I used to that which Roll and designed for the purpose of detecting the CMBR. Theirs had a large amount of symmetry between the path to the sky and that to the helium reference source, just the sort of thing a physicist would design. Ours required very careful measurement of the loss in the separate paths for making the comparison, but the high-sensitivity receiver and high-gain antenna had advantages in measuring the radiation from the Earth's atmosphere and in looking for and rejecting interference and foreground radio sources. We could make a measurement with a tenth of a Kelvin accuracy in a second whereas they had to integrate a long time for that accuracy.

The ability to make meaningful tests in a short time can be invaluable when working with equipment which is not doing what you expect. In short, I think that our equipment inherited from other Bell Labs projects was ideal for finding something unexpected, but similar to what we were looking for, and theirs was more suited to a high accuracy measurement. With hindsight, we should have explored the degree and larger scale isotropy of the CMBR more carefully before moving to 21 cm. Analyzing the records made for flux measurements of a number of sources on one day we were able to put a limit of 0.1K on the large-scale anisotropy (Wilson and Penzias 1967). We could have made a measurement on a one degree to tens of degrees in angular scale which would have been the most accurate for several years.

In 1966 Roy Tillotson, who had succeeded Rudi Kompfner as the director of our laboratory (an organizational unit of several departments at Bell Labs), told us two things which I still remember. First, he told us to identify and preserve our first record which showed the CMBR. Second, he reminded us that we had agreed (probably as a result of Congress having created COMSAT and taken the international satellite business away from AT&T) to each spend half time on radio astronomy and do things for the Bell System in the other half. Therefore since we had been doing astronomy almost full time for several years, we should make good on the second part of the bargain. Over the next several years Arno and I continued to do 21-cm measurement with the 20-foot horn-reflector, but we were also involved in projects more directly targeted to communications.

For the first such project, Arno and I set up a propagation measurement at 10.6-micron wavelength between Crawford Hill and the Holmdel building a couple of miles away using one of Kumar Patel's first high-power CO_2 lasers. It was hoped that one could communicate over short distances in the far infrared much more readily than in the optical and near infrared. Dave Hogg had shown that those wavelengths were highly attenuated over the same path in foggy weather. Alas, 10 microns was much better, but not nearly good enough to be practical. It was, however, fun to convert the parts we got into a reliably operating laser.

I set up a small radio telescope to automatically track the Sun every day and measure its brightness as a way to explore the possibility of using bands at 1 and 2 cm wavelengths for domestic satellite communications. I showed that those bands were useful except during very heavy rains. I also found that if one were willing to have two Earth stations 5 or 10 miles apart one could work around the heaviest rain cells. I did this using fixed pointed radiometers which measured the radiation of the Earth's atmosphere from which I calculated the attenuation. A somewhat longer wavelength band is currently used for direct broadcast satellite TV.

I was having considerable success and fun with the the millimeter-wave propagation experiments and was drifting toward working more of the time on them, but we also continued our 21-cm work, especially with Pierre Encrenaz, a Princeton graduate student at that time.

Then in 1968 Arno suggested using a millimeter-wave receiver based on Schottky barrier diodes with NRAO's recently completed 36-foot antenna on Kitt Peak. Charlie Burrus, who was just down the hall from us, had developed the diodes and mixer assembly for a millimeter-wave (pre optical fiber days) broadband communications system. This initial experiment demonstrated the feasibility of this effort, but produced little in the way of new science. We left that 90 GHz receiver for NRAO to use in developing the antenna. Two years later Sandy Weinreb of NRAO offered to provide a spectrometer and frequency control equipment for the 36-foot. We returned with a higher frequency Burrus receiver. Arno talked Keith Jefferts (a Bell Labs atomic physicist interested in mm-wave spectroscopy) and me into integrating the Bell Labs receiver into an NRAO receiver box that would fit at the focus of the 36-foot antenna. We would then go back to look for carbon monoxide in interstellar space. At one point in this process, Keith remarked that Arno had the two best technicians at Bell Labs wiring the receiver for him.

The payback came when Keith and I joined Sandy at Kitt Peak to get it all working. After several frustrating days, Sandy had to leave, but the next day we got it all tenuously working and put it on the antenna. I asked the telescope operator to point at the BN/KL source in the Orion nebula. I was watching the rather crude output of the spectrometer when some of the center channels increased from their somewhat random previous outputs. The operator confirmed that we had just reached the source. I asked him to go off the source and the channels went back down. Thus in a few seconds, using a system which was hundreds of times less sensitive than the one on the 20-foot horn-reflector, we discovered carbon monoxide in an interstellar cloud. I had picked the BN/KL source because it was the source in our list of candidates which was overhead at the time, but it turned out that it is the strongest CO source in the sky. Arno arrived the next day to find that the key discovery had been made (Wilson, Jefferts and Penzias 1970)..

The carbon monoxide and other simple molecules that we and others have found since can be thought of as stains which allow us to measure the structure and dynamics of the interstellar molecular clouds. The clouds are so cold that their main constituent, hydrogen, doesn't radiate. The radiation from simple molecules has shown that these dense molecular clouds exist, star formation is active in them and they are common in galaxies. Since that time, a large number of astronomers have worked on understanding the physical and chemical conditions in these clouds and the formation of stars within them. For several years after the discovery, Bell Labs gave Burrus diodes to other observatories and taught other groups how to make them.

This discovery changed the direction of my career. We spent five exhilarating years exploring interstellar clouds and discovering new molecules and their isotopic variants with our receivers and the 36-foot antenna at Kitt Peak.

I then became project director for the 7-meter antenna. It was designed to do millimeter wave astronomy when the weather was good and satellite propagation measurements at 1 and 2 cm wavelength in weather bad enough to affect that band. We then had almost two decades of additional studies of molecular clouds and the cores around young stars which are embedded in them. The Crawford Hill astronomy group grew to include several additional people at its peak. Later the astronomy effort became less relevant to AT&T's need to prosper in the post-divestiture days and therefore declined. The Sub-Millimeter Array which I am working on now is an aperture synthesis array that spends most of its time observing radiation from the simple molecules and dust in these star- forming regions.

This work has taken me much closer to the origin of the Earth and perhaps the organic molecules from which life originated, as opposed to the universe. On that larger scale, however, I have found the beautiful spectrum of the CMBR measured by COBE, and the evolving page full of accurate numbers derived from its fluctuations, immensely satisfying.

Bernard F. Burke: Radio astronomy from first contacts to the CMBR

Bernie Burke is the William A. M. Burden Professor of Astrophysics, Emeritus, at the Massachusetts Institute of Technology.

Iet me start out with some personal background. When I was a graduate student at MIT, 1950-53, working in Woody Strandberg's microwave spectroscopy laboratory, I was exposed to radio astronomy through three routes. Woody had known Martin Ryle when he was posted to TRE Malvern during the war, as the Radlab representative. He worked with Martin on countermeasures — he said that the tension had been tremendous, and the radar people at TRE were "burnt out." He had heard about the use of Michelson interferometry by Martin, and of the Lloyd's mirror interferometer at CSIRO Radiophysics, and thought they were an excellent example of using cleverness instead of brute force to do radio astronomy. Woody had known Taffy Bowen; he also had known Hanbury Brown and Richard Twiss. The director of MIT's RLE, where I was working, invited Taffy to come to MIT to give three lectures on radio astronomy: one was about the Sun, one about the Moon, and the third about everything else. I was impressed, and was further impressed in the same year, 1951, when I heard Ed Purcell describe the discovery of the 21-cm hydrogen line at the joint Cambridge Monday-night physics colloquium. Through Woody's contacts, I also did some of my thesis work in Charlie Townes's lab at Columbia. It was a rival lab, but the atmosphere was wonderfully open; Charlie showed a new gadget that he was working on, called a "Maser," and explained how it worked.

As the end of my thesis work approached, I had to find a position, and I interviewed for a job with Harold Friis at Holmdel, the Bell Laboratories field station. He emphasized that they were in the telephone business. Radio astronomy was never mentioned. I met and got to know Bob Dicke, a good friend of Woody's, and knew about his K-band radiometer measurements on the roof of Building 20. The famous picture of Bob holding the "shaggy dog" in front of the radiometer horn was well-known, and his derivation of the atmospheric K-band absorption that degraded K-band radar was well-known. I probably knew about his upper limit to the cosmic background (Dicke, Beringer, Kyhl and Vane 1946), but its future connection to radio astronomy did not make much of an impression at the time.

I tried to obtain a Fulbright fellowship with Martin Ryle, but that did not work; I then found out that Merle Tuve, director of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington (DTM), was starting a radio astronomy effort. I had met Merle at an MIT summer study on undersea warfare (the Hartwell project) in 1950, so I contacted him at the DTM, received a postdoc offer, and joined the fledgling program in September, 1953. Merle had imported Graham Smith from Ryle's group at the Cavendish, and my education in radio astronomy began. Our first big project was the 22 MHz Mills cross, and two years later Ken Franklin and I discovered Jupiter's radio bursts. This continued a long tradition of making a discovery in radio astronomy, but not the discovery that the radio telescope had been designed for. I got to know Grote Reber, a marvellous person to talk with, and gained an appreciation of his ability. Fred Haddock called him "not a scientist, but a scientific pioneer," which captures his maverick quality. Reber's personal account of his motivation and work in Proc. IRE (1957) is a masterly description of how pioneering science is done. There is a curious historical note that can be added. Edwin Hubble, the founder of modern observational cosmology, was taught in elementary school by Grote Reber's mother!

It should be remembered that the state of astronomy in the 1950s was quite different from today. There was an unresolved discrepancy between the Hubble age of the universe and the age of the Earth. There had been a few identifications of radio sources and the two brightest had just been identified: Cygnus A and Cassiopeia A, resolving the fierce controversy that had raged, led by Tommy Gold, who maintained that most were extragalactic, and opposed by Martin Ryle, who maintained that most were in our own galaxy. This had been followed by the bitter controversy between Ryle, who maintained that the 2C source counts disproved the steady-state universe, against Bondi, Gold, and Hoyle, who (quite correctly) maintained that the survey was so flawed that it did no such thing.

Now, back to the DTM. In the fall of 1953, Jesse Greenstein and Merle Tuve were at work, arranging a symposium to be held at at the Carnegie Headquarters in Washington, with the evident intent of instigating a resurgence of radio astronomy in the U.S. Jesse was chairman of the National Science Foundation's advisory committee on astronomy, the first such group that the NSF had convened, and he could be sure of close attention from that fledgling organization. Merle was well-connected throughout the government, and the two, despite some philosophical differences, had considerable influence in official circles. They gathered together an outstanding group of participants, who assembled at the Carnegie headquarters on P Street under the aegis of Vannevar Bush in January, 1954. The group included Lee DuBridge, president of CalTech, Leo Goldberg from the University of Michigan, Ed Purcell and Bart Bok from Harvard, Rudolph Minkowski and Walter Baade from Mt. Wilson, the optical astronomers who identified Cygnus A and Cassiopeia A, Bernard Mills from CSIRO, and Graham Smith, both of whom had provided the accurate positions of radio sources that were needed for the identifications, John Hagen from the Naval Research Laboratory (NRL), John Kraus from Ohio State, and Charlie Townes from Columbia, who was in the process of inventing the maser and the laser. There were the cosmologist Fred Hoyle, Bob Dicke and Lyman Spitzer from Princeton, Henk van de Hulst, Taffy Bowen, and many other prominent researchers. Lloyd Berkner, who would play a key role in establishing the National Radio Astronomical Observatory (NRAO), attended; he was president of AUI (Associated Universities Incorporated, a non-profit corporation composed of representatives from nine northeastern research universities). John Firor and I, along with several of the young people from the Washington area, were also invited. For me, it was a grand introduction to the bright lights of physics and astronomy, and a broad-ranging tutorial in astronomy. The real purpose of the tutorials, however, were aimed at the NSF and Department of Defense officials who attended. Here was a new field of science, clearly related to various national interests, demanding attention from those who were funding science in the U. S.

Although radio astronomy was being pursued at the NRL, Ohio State, Cornell, as well as at the DTM plus a fledgling group at Harvard, it was at the Washington Conference that American radio astronomy moved to join Britain and Australia as a major power in radio astronomy. Things moved fast. Caltech, Berkeley, Michigan, Illinois, and Stanford all began major projects. The NRAO was established, and while the 140-ft telescope project writhed in agony, the 300-ft telescope was started as a stopgap measure. The result was that less than two years later, observations began with the 300-ft transit telescope, which was built, as John Findlay put, for the price of sugar — 68 cents per pound. I was an early user, and made a map of the entire visible sky at 234 MHz, including an absolute brightness calibration. The cosmic component had no place in my thinking, for I was pursuing the question of the galactic radio halo, which was much more flattened than the Cavendish measurements implied. The results were published in the Carnegie Year Book, but the map itself was never published. Otherwise, the early 1960s were an eventful time. Otto Struve left the NRAO, an unfortunate case of capping an outstanding career with a conspicuous failure, and he died shortly afterwards. Joe Pawsey, from CSIRO Radiophysics, agreed to take his place, but he was stricken by a fatal brain tumor and never took office. In this critical time for the NRAO, David Heeschen was named interim director; in fact, he had been the intellectual leader of the observatory from the beginning, even though he did not have the authority to influence major policy issues such as the finishing of the 140-ft telescope. Meanwhile, the search for a permanent director continued, unsuccessfully, and Heeschen was appointed director in 1962, making official what had been, in fact, the case since the NRAO was founded.

A new direction in radio astronomy was developing at the Bell Labs Holmdel station. The director was now Rudy Kompfner, a physicist with a broad range of vision. As in the case of Karl Jansky, the project started as a system to help plan for telecommunications. The Bell Labs knew that they had to look into satellite communications, and were performing scatter experiments on the Echo satellite, a simple aluminum-foil sphere. For sound engineering reasons, they wanted to develop the best possible low-noise antenna/receiver system, and calibrate it carefully. The frequency was 2390 MHz; the antenna was a shielded horn (it looked like a sugar-scoop), and the low-noise amplifier was a state-of-the art ruby maser. Their results were published by Ohm, the project leader, in the July, 1961 Bell System Technical Journal, where they reported that separately the total noise of the system from all components was 18.90 ± 3.00 K (Ohm 1961). The total noise, measured on the sky, was 22.2 ± 2.2 K, and this meant that the microwave background of the sky was undetectable. It is said, however, that their initial measurements had smaller error bars, and an implied background temperature of 3.3 K was observed repeatedly, but the engineers talked each other into assigning larger error bars. Whatever the actual facts were, they missed the discovery, and their main fault, as engineers, was that there was an unknown source of noise that they did not pursue. My own contact with this work was almost nil – the case of the dog that did not bark in the night? I would say that I was aware of Ohm's work, but I had not seen the article in BSTJ, and I believe that the discrepancy went unnoticed by my colleagues.

For the young American radio astronomers of that time, it was a marvelous era. We all knew one another, and that included the graduate students. Arno Penzias was a student of Charlie Townes, who, to complete his PhD thesis, had taken his low-noise maser receiver to the Naval Research Laboratory and installed it on their 50-ft dish. The entire Washington astronomy community was close-knit, helped by the quarterly community meetings that the Naval Observatory hosted. I knew the graduate students at Caltech, including Bob Wilson. I think that we took it as a good omen when the two decided to go to work at the Bell Labs field station at Holmdel.

Arno Penzias joined the Bell Labs in 1961, and was followed a year later by Bob Wilson. Here I have a personal story to tell. In 1962 or 1963 I shared an airplane journey with Arno (my recollection it was to Ottawa), and I asked him what his plans were at Holmdel. He said that he was going to determine the absolute brightness of the sky at C-band. I said that he would certainly find that it was so low that it was undetectable, based on the synchrotron spectrum, and he said yes, he knew that, but it had never been measured and the equipment at Holmdel was the best in the world for that purpose. Arno remembers that I mentioned the earlier upper limit set by Dicke at the Radlab. I don't remember that explicitly, but since I was aware of the measurement, it is entirely possible that I did.

Two years later, in 1965, a colleague at the DTM, Ken Turner (a PhD from Dicke's lab at Princeton), told me about an interesting colloquium that he had attended at the Johns Hopkins Applied Physics Lab. Jim Peebles, a theorist working in Dicke's group, said that there was good reason to suspect that if the "big bang" cosmology was correct, there should be a remnant microwave glow in the sky, the redshifted remnant of the time when the hot gas recombined at a redshift of about a thousand, and Dicke's group were in the process of measuring it. At the DTM, we had a lunch club, with the staff taking weekly turns as cook. It is my recollection that, on the very same day that Ken told me the news, the telephone rang during lunchtime (it may have been a day later, but the interval was very short). I was called to the telephone, and it was Arno, calling about some side issue, possibly about URSI matters, and after we finished our business, I asked Arno "How is that crazy experiment of yours coming?" Arno replied "We have something we dont understand." I then said "You probably should call Bob Dicke at Princeton to discuss it." Arno called Princeton, talked to Bob while he was meeting with his group, and the rest is history. Penzias and Wilson received the Nobel prize, quite deservedly, but it is a shame that it was not shared with Dicke. He shares the distinction of many friends who might have become Nobel laureates, were well-deserving of the honor, but who were passed over.

Another footnote story can be told about how Bell Labs profited in a practical way from the CMBR discovery. Rudy Kompfner told me that the space relay system that Ohm's work had been designed for needed a reliable calibration system for the relay stations in the field. The engineers planned to launch a calibration satellite to do the job, when Arno and Bob pointed out that there were radio sources already in the sky that could calibrate the system, with no cost to Bell Labs! Shortly afterwards, the engineers were contemplating an 8-millimeter telephone relay system, but again a calibrator was needed, especially to get a statistical record of atmospheric attenuation. Again, a calibration satellite was proposed, and again Arno and Bob pointed out that the Sun could serve as the calibrator, again at zero cost! The radio astronomy program saved Bell Labs several hundred million dollars in satellites that were not needed.

Many discoveries have precursors, and the discovery of the CMBR has some history of that sort. Joe Weber at the University of Maryland told me this example. He served in the USA Navy in the Pacific in World War II. After Joe resigned his commission in 1948, his expertise earned the offer of a professorship at the University of Maryland, provided he get a PhD. That led to an interview by George Gamow, who was in the midst of his calculations of a nuclear "big bang" at the beginning of the universe. Gamow asked him "Young man, what do you do?" Joe answered, "I'm a microwave physicist." Gamow replied "I'm sorry, but we don't have anything suitable for you at George Washington." It does not seem likely that Gamow had seriously considered how the relict radiation might be detected.[20]

Gamow may have missed an opportunity, but his two graduate students, Ralph Alpher and Bob Herman, may not have. After Joe Weber told me his story about his interview with Gamow, he continued with a second story that illustrates how major discoveries have antecedents, might-have-beens, that for one reason or another did not happen. This was true for pulsars, and particularly for the Crab pulsar; both the radio and optical discoveries had failed precursors. Joe's story is that Alpher and Herman visited the Naval

[20] Trimble has this recollection of the encounter.

"Joe Weber was an amateur radio operator in his early teens and, at the time of the Sicilian invasion, was the skpper of one of the first submarine chasers to have a 6-cm radar (SC 690). As the war wound down, the Navy moved him to a desk job in Washington in electronic countermeasures, largely to descope the effort, but also to hand out some grants. When he decided to resign his commission (as lieutenant commander), several grantee organizations offered him jobs, but he accepted instead a full professorship of electrical engineering at the University of Maryland. The fall 1948 appointment was contingent on his obtaining a PhD in something quite soon, since his highest degree was a 1940 BS from the US Naval Academy.

"Thus summer 1949 found Weber visiting Washington-area universities in search of a PhD project and advisor. One of the first places he visited was George Washington University, and one of the people he talked with there was George Gamow. "Do you have any interesting thesis problems?" Weber enquired. "What can you do, young man?" responded GG. "I'm a microwave spectroscopist," said JW. "No, I don't think of any interesting problems" concluded Gamow. So Weber went on to Catholic University, where he completed a 1950 PhD dissertation (degree 1951) with Keith Laidler on the inversion spectra of normal and deuterated ammonia. Since Weber at the time knew about the technology for detecting faint radio signals, whether the story is funny depends on whether you think Gamow should have had radiation from the early universe in mind in 1948. It is, of course, a second hand story, but I was married to Joe from 1972 until his death in the year 2000, and men, as you probably know, like to tell war stories. There is also a good one about the inhabitants of Tonga Tabu, following the sinking of the Lexington in the battle of the Coral Sea in May 1942."

Research Laboratory, almost certainly in 1948 or 1949, to see if detection of the microwave background was a possiblity. In radio astronomy, NRL was the only show in town at that time, and it is likely that they talked to the head of the radio astronomy group, John Hagen. He told them that the experiment was too hard, so they did not pursue the matter. I asked Ed McClain, one of their talented young engineers, if he had ever heard from Hagen about the experiment, and he said that he certainly had not. I remember Ed saying "That's odd, because John had a very good nose for new science." I doubt that Joe Weber's story is incorrect, because the Washington radio community was a close group, where everybody knew everybody, but it is, nevertheless, a second-hand story. Might the NRL group have succeeded? Hagen had a powerful team, including Ed McClain, Connie Mayer, Fred Haddock, and Russell Sloanaker, all of them talented microwave engineers and good physicists, familiar with the latest microwave technology. On the other hand, they had concentrated mostly on solar radio astronomy, a strong source where sensitivity is less important, but receiver stability is vital. Nevertheless, they were clearly interested in fainter radio sources because, in 1950, they persuaded the Navy to fund a steerable 50-foot dish, which they placed on top of the central building at NRL (a terrible location). A simple calculation shows that they might have been able to do the experiment: their crystal mixers had a double-side-band noise temperature in the range 2000-3000 K. Typically, their IF bandwidth was 10 MHz, so in one second a Dicke radiometer would have had an rms fluctuation of about 1 K for a one second integration. Connie Mayer, in particular, was meticulous in calibrating radiometers, and all of them knew about the hazards of atmospheric and stray radiation. They had access to liquid helium, so they could make cold loads. I conclude that they had a good chance of being successful if NRL gave them the resources. The conclusion, though, is clear: the experiment was not pursued, and it joins the long list of lost opportunities in science. Arno Penzias and Bob Wilson, on the other hand, were at the right place, at the right time, and their work is a model of how forefront science should be done.

A brief coda is in order. Kipling's ditty

> As the dog returns to its vomit,
> and the sow returns to its mire,
> and the burnt fool's bandaged finger
> goes wobbling back to the fire . . .

comes to mind, for after I returned to MIT in 1965, the lure of the CMBR pulled me in. I had a talented graduate student, Marty Ewing, and along with Dave Staelin we hatched a plan to measure the CMBR at a shorter wavelength. We chose 9 mm, because atmospheric transparency is good there, and we chose White Mountain, east of the Owens Valley, as the obvious site. Nello Pace of Berkeley had established a high-altitude physiology lab there, so there were electric power, living facilities, and road access. Common sense said that it had to be, at 12,400 feet altitude and east of the Sierras, an uncommonly good place to do the experiment. The Princeton group evidently thought so too, and so in the summer of 1967, side-by-side, we measured the CMBR. The leaders of the Princeton team were Dave Wilkinson and Bruce Partridge, and we became friendly competitors. They were carrying out their measurements at three wavelengths, a better experimental design, but at least we confirmed their results, using a different calibration technique. We used a Dicke radiometer, switching against a liquid helium cold load, and calibrated the overall system by using a helium-cooled "shaggy dog" (actually, a shaggy egg-crate) (Ewing, Burke and Staelin 1967). Dave Wilkinson's group calibrated by using the same reflector to look at the sky and to look down at an egg-crate in a bath of liquid helium (Stokes, Partridge and Wilkinson 1967).

There is a final twist to the story. In about 1970, there was a rocket experiment that tried to measure the CMBR temperature above the Planck maximum, and they found that the sky was much too hot. I doubted the result (which turned out to be caused, not by a hot universe but by hot rocket gases), and sought out Rainer Weiss, a colleague at MIT and a friend of many years. Rai is a great experimental physicist, at the time doing fancy things with lasers, and I think I communicated my enthusiasm. A balloon-borne radiometer was obviously the way to go. In addition to sending data by a radio link, Rai wanted an on-board recorder, and this I borrowed from my close colleague Al Barrett, who had been carrying out balloon radiometer observations of the Earth and its atmosphere for some time. Al was reluctant to lend his precious gear, and on the first flight the wrong squib was fired, and the experiment fell 100,000 feet to the earth. Al's recorder was among the casualties. I told Al that Rai would buy him a better recorder, and the next balloon experiments worked; The CMBR still showed 3 K beyond the peak. Otherwise, I had little to do with the experiment (Lyman Page was the graduate student who helped Rai with the heavy lifting) but some years later, Rai paid me the ultimate compliment: "Bernie, you wrecked my lab."

A more extended history is given in my article, "Early Years of Radio Astronomy in the U.S.," Burke (2005).

Kenneth C. Turner: Spreading the Word — or How the News Went From Princeton to Holmdel

Ken Turner has done research in radio astronomy at the Arecibo Observatory, Puerto Rico, and served as Program Officer for Extragalactic Astronomy and Cosmology at the USA National Science Foundation. His current interests include the study of psychology.

After finishing up my Ph.D. at Princeton in 1962, I was awarded a Post Doctoral Fellowship at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. We were located in Northwest Washington, DC, and I was working with Bernard F. Burke learning radioastronomy, mostly related to the study of neutral hydrogen, and utilizing a sixty-foot radio telescope at nearby Derwood, Maryland.

At Princeton I had been a member of the Dicke group investigating the experimental foundations of general relativity and any other cosmological or gravitational effect we could think up. Jim Peebles was also a member of the group and a good friend, so when I heard that he was going to give a talk at the Johns Hopkins Applied Physics Laboratory in Baltimore I made it a point to attend. Jim outlined the current activities of the group which included an experiment to look for the red-shifted primordial radiation of the "big bang," which was expected to peak in the microwave region. Although this had been predicted by Gamow and Alpher some twenty or so years before, that prediction was "lost in the literature" and was unknown to the Dicke group at the time.

I was much taken by the idea of this experiment and when I returned to the DTM I told Bernie Burke all about it. A short time afterward he was visiting Arno Penzias at Bell Laboratories, and, as I recall the story, Arno had told him that they were trying to make an absolute calibration of the big horn antenna there and were having trouble accounting for the last few degrees of noise temperature they had measured. At that point, Bernie told Arno and Bob Wilson, who was working with Arno on the experiment, about the background radiation that the Princeton group was tooling up to look for.

Arno and Bob immediately saw the implication of their "difficulty" and published their discovery of the radiation of the "primeval fireball," a phrase coined by John Wheeler to characterize the effect Peebles had predicted from his calculations of the conditions thought to prevail in the very early universe.

P. James E. Peebles: How I Learned Physical Cosmology

Jim Peebles has been at Princeton University since 1958 and is now Albert Einstein Professor of Physics Emeritus.

I arrived in Princeton in 1958 from the University of Manitoba as a graduate student intending to study particle physics. At Princeton Bob Dicke somehow saw that I was much better suited to work on his new research interest, gravity physics. He certainly was right.

Dicke had recently changed directions from research in quantum optics and precision measurements in atomic physics to the study of the physics of gravity. At the time we had an elegant theory, general relativity, but very limited tests. Dicke set out to improve the situation. By the time I arrived a considerable number of people were working with him, including undergraduate and graduate students, postdocs and junior faculty. Bob Moore, who had been two years ahead of me at the University of Manitoba and was one of Dicke's graduate students, brought me to a meeting of Dicke's Gravity Group. I was fascinated by the variety of topics under discussion, and intimidated by how much everyone knew. Dicke in particular seemed to have a ready and well-informed assessment of every issue that arose. But he was drawing from a deeper well of understanding of the physics of the real world than anyone else I have encountered before or since then.

Dicke encouraged me to join the group. I wrote a doctoral dissertation under his direction, on constraints on the time variability of the strength of the electromagnetic interaction (Peebles 1962), and stayed on in Princeton as Dicke's postdoc and then a member of the faculty.

I learned about the general relativity theory solution for a homogeneous and isotropic expanding universe as part of preparation for the physics department graduate general examinations. I remember feeling a little surprised that peple might consider this a serious model for the real world rather than one of the simplified problems you solve in exams, along with the acceleration of a frictionless elephant on an inclined plane. My textbooks on general relativity and cosmology, including Landau and Lifshitz's (1951) *Classical Theory of Fields* and Tolman's (1934) *Relativity Thermodynamics and Cosmology*, present beautiful theoretical physics but little phenomenology. I eventually came to see that cosmology does have meaningful connections to experimental physics and observational astronomy, and wrote a book on the subject, *Physical Cosmology* (Peebles 1971).

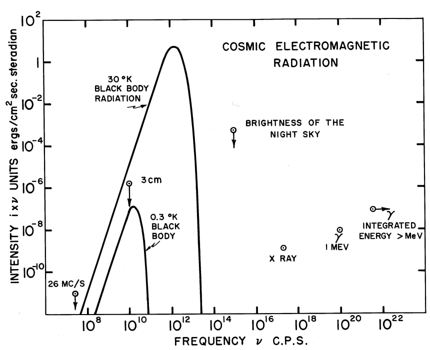
I don't remember much about the Gravity Group meeting at which Dicke explained why we might want to look for a sea of blackbody radiation that nearly uniformly fills space. But I think it was at this meeting that he gave an explanation that sticks in my mind for why the radiation would cool as the universe expands. He invited us to imagine placing a box with perfectly reflecting walls in the sea of radiation, with the same radiation temperature inside and out. The walls are expanding with the general expansion of the universe. They have no effect on the radiation (at wavelengths small compared to the box size) because for every photon that approaches the box from outside and is reflected there is on average an interior photon that bounces off the wall to replace it. I think I remember his concluding remark: we all know that radiation is cooled by the adiabatic expansion of the cavity. It was obvious to Bob that the spectrum remains thermal as the radiation cools. I don't remember whether he explained that. I convinced myself of it by a variant of the argument that is presented in the glossary under the CMBR spectrum.

Bob persuaded Peter Roll and David Wilkinson to build a Dicke radiometer to look for this radiation. His casual remark that I might look into the theoretical implications of the outcome of the experiment set the direction for my career. Great people can do that.

I have some notes from that time, but rarely put dates on them, so can only say for sure that by the Fall of 1964 I was making progress on two ideas. One was that thermonuclear reactions during the early rapid expansion of a hot universe, when the radiation temperature was $T \sim 10^9$ K, could produce appreciable amounts of helium. The other was that when the temperature was greater than about 3000 K matter would have been thermally ionized and radiation drag on the plasma would have strongly affected the growth of the clustering of mass we observe now in galaxies and concentrations of galaxies. In 1965 I learned that much of the first idea had already had been worked out. And in that same year my second colloquium on what I was doing led to the connection between the Princeton search for the radiation and the Bell Laboratories detection.

I presented my first colloquium on this subject at Wesleyan University in Connecticut on 2 December 1964. Henry Hill, a former member of the Gravity Group, invited me. He wanted to explore the possibility of my moving to Wesleyan. I was impressed by the faculty, and particularly remember Thornton Page for his instruction on aspects of astronomy. But I don't remember any feedback on cosmology, and nothing came of the job idea.

In the colloquium I showed the two graphs in Figure 21. The curves in the panel on the left are examples of thermal spectra. The hotter one would have about the energy density of the Einstein-de Sitter cosmological model. (The mass in this model is such that the universe in effect is expanding just at escape velocity). The symbols show measurements or upper bounds on



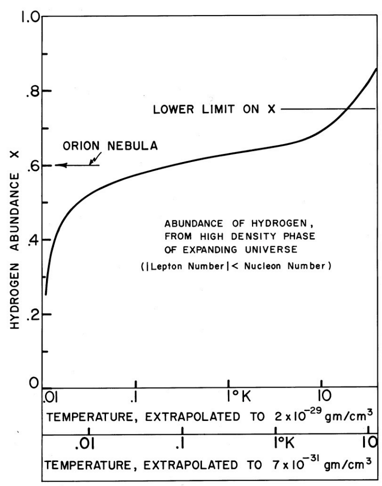


Figure 21: Measures of the background radiation spectrum and calculations of the helium abundance in December 1964

the cosmic radiation energy density across a broad range of wavelengths. It was known then that space is filled with a near uniform sea of X-ray to γ-ray radiation. The amount of energy in this form is much less than the equivalent of the mass in matter. There was an upper bound on the cosmic mean energy density at optical wavelengths. Now we have a measurement of the accumulated amount of starlight, an important advance that required a lot of work (Hauser & Dwek 2001). The point at the left edge of the graph shows a measured upper bound on the background radiation at about 1 m wavelength. The energy in the radio background contributed by the observed sources was known; I added that my next version of the figure. I think the upper limit at microwave wavelengths refers to a Bell Laboratories paper we were discussing in the Gravity Group, Hogg & Semplak (1961). We interpreted it as giving an upper bound of about 15 K. If so, I made a mistake in the figure: the wavelength is 5 cm. I don't know why we overlooked the better reference, Ohm (1961). And we had not yet noticed — and pointed out to Bob Dicke — that he had placed a bound $T < 20\,\mathrm{K}$ at 1 cm wavelength (Dicke *et al.* 1946). It is at these microwave wavelengths that fossil thermal radiation might appear: not so hot as to have an unacceptably large energy density for the relativistic big bang cosmology nor so cool as to be unobservable.

I later learned that Doroshkevich and Novikov (1964). had made a similar study of the cosmic radiation energy density. The focus of their analysis was the mean energy density from the accumulated amounts of starlight and radio radiation produced by the galaxies. But they remark that the "Gamow theory" would produce a thermal spectrum at microwave wavelengths, and they refer to the paper by Ohm (1961) on the Bell Lab communications experiments discussed in Chapter 3. It is a better bound than the one I showed.

The right-hand graph in Figure 21 shows my computation of the mass fraction X left in hydrogen at the end of the big bang thermonuclear reactions discussed in Chapter 3. Almost all of the rest of the baryons, with mass fraction $Y = 1-X$, would be in helium. I did not compute the production of heavier elements, but felt it would be small. The arrow on the left is an estimate of X in the interstellar plasma, and the line on the right is my guess of a reasonable lower bound on X in the earliest generations of stars. The horizontal axis shows the present radiation temperature computed for two possible values of the present mean mass density (in baryons; I wasn't thinking about nonbaryonic dark matter until the early 1980s). According to my notes for the Wesleyan talk I pointed out that an interesting value of Y in a universe with a hot big bang could be associated with microwave background radiation that would be warm enough to be detectable. I don't know whether I mentioned the Princeton experiment aimed at its detection.

We were of course thinking about how we might interpret the experiment if it were found that there is undetectably little microwave radiation. According to the assumptions used in the figure the low temperature would imply an unacceptably low value of X (a large helium abundance). But there are ways out; I mentioned two at Wesleyan (according to my notes, which were my security blanket in those days). The first is that the big bang was cold. That can be reconciled with large X by postulating that there are enough neutrinos to prevent formation of neutrons. This is what is meant by the comment about leptons in the figure. Zel'dovich (1963, 1965) independently proposed a cold big bang with lepton number equal to twice the baryon number. Another was out was that the universe is not even approximately homogeneous and isotropic: maybe there was no big bang. The homogeneity assumption is well supported by the observations now, but not then.

In the paper Dicke & Peebles (1965) (which was submitted before we knew about Penzias and Wilson, but it has a comment added in proof) we mention a third possibility, that general relativity theory is not valid. It is after all an enormous extrapolation of the theory from the meager tests we had then to its application on the scales of length and time of the expanding universe. At the time Dicke was very interested in the idea that the strength of the gravitational interaction may have been large in the early stages of expansion of the universe. If it were large enough the rate of expansion and cooling of the universe would have been rapid enough to have prevented significant element building.

We should have mentioned yet another possibility: in the steady state cosmology the continuous creation of matter could have produced little helium or microwave radiation. When I learned about this cosmology I was shocked: they had just made it up. I came to see only later that the same is true of the relativistic big bang. We have compelling evidence now for the latter and against the classical steady state cosmology. But that was not the case then.

According to my notes for the Wesleyan talk I had nothing useful to say about the astronomical determinations of the cosmic helium abundance Y. But we were learning that Y is larger than seems likely to be accounted for by production in stars, and maybe is in line with a hot big bang. We mention that in Dicke and Peebles (1965), with a reference we had just learned to Hoyle & Tayler (1964), who knew a lot more about the astronomy than we did.

We did not refer to Osterbrock and Rogerson (1961). I believe they were the first to present evidence for a critical result: the older stars that contain fewer heavy elements appear to contain about the same helium mass fraction as younger stars. They also point out the possibly very significant implication, that "the build-up of elements to helium can be understood without difficulty on the explosive formation picture (Gamow 1949)." I only realized when writing this essay that I had referred to Osterbrock and Rogerson in a study of the structure of the planet Jupiter (Peebles 1964). I had concluded that Jupiter has a relatively small mass in heavy elements in a core, most of the mass outside the core is hydrogen, and the helium abundance is roughly consistent with the Osterbrock and Rogerson estimates and what Martin Schwarzschild told me about the composition of the Sun. The remark about the explosive formation picture would not have meant much to me when I was making models of Jupiter. When I started thinking about a hot big bang I should have remembered the evidence for large Y in old stars. Bob Dicke liked to say that "we get too soon old and too late smart."

My second colloquium on cosmology was at the Applied Physics Laboratory at the Johns Hopkins University in Maryland, on 19 February 1965. I don't know why I was invited; maybe it had something to do with the fact that Alpher and Herman were at the Applied Physics Laboratory when they were developing the physics of element production in a hot big bang. But I learned about that connection much later.

In this colloquium I presented updated versions of the helium production calculation and the cosmic radiation spectrum in Figure 21. I had added to the latter a bound on the energy in microwave radiation from the absence of a discernible effect of its drag on energetic cosmic ray protons. That pretty convincingly ruled out the idea that the mass of the universe might be dominated by radiation. But it did leave room for an interesting fossil thermal background. I had asked David Wilkinson whether it would be appropriate to mention the Roll-Wilkinson experiment. You want people to know about your work, but only when it's unlikely someone else might be inspired to do it first. Dave assured me that no one could catch up with them at that point, so I mentioned the experiment. Ken Turner, a friend from our graduate student days in the Gravity Group, attended the talk. He told Bernie Burke about it. Burke brought the news to Arno Penzias and Bob Wilson. They were at the Bell Laboratories in Holmdel NJ, not far from Princeton. They did not have to catch up: they had already done the experiment. Arno telephoned the news to Bob Dicke.

What was our reaction to the telephone call? I remember relief and excitement. They had shown us that there actually is something to be measured, always a good thing. That overwhelmed any chagrin over priority, and to me it still does, with one exception. The Nobel Prize rightly went to Penzias and Wilson: they made very sure of the reality of an unexpected result, and they made sure the world knew about it. But the Nobel committee should have included Dicke.

When and how did I learn that my first computations of light element formation largely repeated earlier work? My records reveal a few data points. I submitted a paper on my calculations to the Physical Review. The referee recommended rejection, saying that my calculations had already been done, and by whom. I revised and resubmitted several times. I have a draft dated January 1965 that has a reference to Alpher and Herman (1953), but I don't know whether this draft was the first to recognize that I was repeating old analyses. I have a copy of a letter I wrote to Hoyle and Tayler on February 1 1965 acknowledging their prior work. I have a copy of my letter to the Physical Review dated 23 June 1965 in which I withdrew the paper. By then I had faced up to the fact that to make a meaningful contribution I would have to do a distinctly better computation.

Fred Hoyle also saw the need for a better computation, and he, Willy Fowler and Bob Wagoner got to work. I met Bob Wagoner at a conference in Miami in December 1965. We exchanged ideas but not techniques of computation. I devised fixes for the numerically unstable reaction equations that work but likely would be close to incomprehensible to anyone else. Bob used the more familiar (to Fowler and Hoyle) techniques from the analyses of nuclear burning in stars, and he considered a larger set of nuclear reactions. I believe their code evolved into some used today. But our results, in Peebles (1966) and Wagoner Fowler & Hoyle (1967), agree.

Our ignorance in 1964 about the literature of this subject is legendary in the cosmology community, and legends beguile. I see the effect in Bob Dicke's comment (unpublished, dated 1975):

> There is one unfortunate and embarrassing aspect of our work on the fire-ball radiation. We failed to make an adequate literature search and missed the more important papers of Gamow, Alpher and Herman. I must take the major blame for this, for the others in our group were too young to know these old papers. In ancient times I had heard Gamow talk at Princeton but I had remembered his model universe as cold and initially filled only with neutrons.

I think Bob apologized too much. I have the greater share of blame for poor homework: Bob was careful to stand back and let younger people in his group get on with research on their own. Dicke and Peebles (1965) did not give proper references to earlier work on the hot big bang, but we remedied that pretty quickly. I believe the citations are normal and proper in Dicke, Peebles, Roll, & Wilkinson (1965), the paper that offers the hot big bang interpretation of the Penzias and Wilson (1965) detection. Because I have on occasion encountered the myth that our paper did not refer to earlier work I list our relevant references: Alpher, Bethe & Gamow (1948), Alpher, Follin Herman (1953), and Hoyle & Tayler (1964). The list is brief, but this is a brief paper. Dave Wilkinson systematically advertised the history of ideas in his lectures, as one sees in his Figure 3 in Chapter 3. The element of levity is typical of Dave. I told the history at conferences and colloquia too, to judge by my notes. And I think there is a full and accurate account of the history in Chapters V and VIII of *Physical Cosmology* (Peebles 1971).

Bob hated sloppy physics, a term he used on occasion to express strong disapproval. I don't remember his ever applying those feared words to me, though I do remember clear reprimands for less than careful work. My homework in 1964 could be termed sloppy. But I don't remember Bob or anyone else in the group chiding me about it then or later. We were caught up in the excitement of exploring rich and sparsely worked ground.

The other rich slice of physics I started pursuing in 1964 is the effect of the CMBR on the gravitational growth of small initial departures from an exactly homogeneous mass distribution into the present strong clustering of mass on the scale of galaxies. Here again Gamow (1948a) got there first. He pointed out that the matter temperature and density in the early universe determine the pressure, and the pressure sets the size of the smallest cloud of matter that gravity can cause to break away from the general expansion. This is the analog in cosmology of the Jeans criterion for the balance of gravitational attraction and the pressure gradient force of repulsion of a cloud of matter. Gamow also argued that the gravitational instability to the growth of mass clustering commences when the mass density in matter becomes larger than that in radiation. He was right, though his argument is not what we use today. A brilliant physicist can do that.

I was able to add something new. The drag of the radiation on the motion of the plasma prevents the gravitational formation of a nonrelativistic cloud of baryons. That situation would have changed when the temperature dropped to about 3000 K, the plasma combined to largely atomic hydrogen and helium, and matter and radiation abruptly decoupled. I published the idea in Peebles (1965) (with, I am relieved to see, appropriate reference to Gamow 1948a on the Jeans length).

At the January 1967 Texas Symposium on Relativistic Astrophysics I presented a more detailed analysis of the behavior of the matter-radiation fluid prior to decoupling in general relativity theory. I treated the radiation as a fluid dissipatively coupled to the plasma, and analyzed how the dissipation suppresses small-scale density fluctuations that act as pressure waves. I presented my paper on these considerations for publication in the conference proceedings, but because of turmoil at the publisher the proceedings never appeared in print. Michie (1967) independently worked out main elements of this physics, and so did Joe Silk, who published (Silk 1968). The effect is properly known as Silk damping.

The departures from an exactly homogeneous mass distribution perturb the radiation. Sachs and Wolfe (1967) worked out the gravitational perturbation that, in general relativity theory, dominates on large scales. While on sabbatical leave at Caltech in 1968-69 I took the next step by working out the radiative transfer treatment that is needed to compute the residual irregularities in the radiation distribution on smaller scales. Jer Yu, who had been my first graduate student, joined me in the numerical integrations to get solutions for the distributions of matter and radiation after decoupling (Peebles and Yu 1970). This is the physics now used in the standard analysis of the measurements of the variation of the CMBR temperature across the sky, including the wonderfully precise WMAP data.

The CBBR is perturbed also by its interaction with plasma: scattering by the hotter electrons pushes the CMBR spectrum down from blackbody at long wavelengths and up at the short wavelength end. The plasma in clusters of galaxies is hot and dense enough to produce an observable effect that has become a very useful diagnostic of structure formation. Ray Weymann was the first to analyze this important effect. He writes:

> I then became interested in understanding the coupling (and subsequent decoupling) of the matter and radiation and came to the realization that the Compton interaction was the dominant interaction mechanism. To derive the frequency-dependent interaction and its diffusion approximation, one does need special relativity, and I was helped by a paper and correspondence with Wllard Chappell in Boulder, Colorado, who helped me over an obstacle. The resulting paper was published in *Physics of Fluids* (Weymann 1965). Not very long after that I applied that diffusion equation to study the temperature history of the matter and radiation, and that involved studying the recombination era. I wrote up two papers, and I believe one was published (after a struggle with the referee: Weymann 1966) but the other only appeared as a Steward Observatory preprint. One of these papers calculated departures from the Planck function that would result under various (and I later realized mostly unrealistic) heating mechanisms.
>
> Shortly after this I received a letter from Zel'dovich who pointed out that the diffusion equation I had derived had already been derived by Kompaneets, though I was totally unaware of it, as it was in a Soviet Journal. About then the Zelodovich and Sunyaev (1969) paper came out. If you read that paper you will see that my paper was referenced fairly extensively by them, but my paper had two serious defects: I did not derive the analytic expression which they did, but relied only on numerical work, and I only applied the work to the cosmic expansion and not to finite clouds of electrons.
>
> My only regret in all this is that since I did all that work at Arizona, there was at that time no other theoretician there to talk to and so I was too isolated and the work I did there suffered from that.

I had found the effect by 1970, but that was well after Sunyaev and Zel'dovich. I at first didn't think it would be large enough to be observable. Dave Wilkinson soon straightened me out on that. A convincing detection did take a lot of work, but the Sunyaev-Zel'dovich effect now is an important part of the network of cosmological tests.

These early analyses assumed the CMBR really is fossil radiation from the very early universe. An alternative that had to be considered in the 1960 was that the CMBR was produced by sources in the universe as it is now or at modest redshifts. Galaxies are sources of optical and radio radiation. Might they also produce the microwave background? The idea was discussed by Sciama (1966), Gold & Pacini (1968) and Wolfe & Burbidge (1969). Layzer's (1968) proposal was that radiation released in explosions could have been absorbed by dust and reemitted as microwave radiation. These were serious possibilities in the 1960s, and they demanded tests. The first and most basic is the spectrum: if a fossil from the hot big bang the spectrum ought to be close to blackbody. If the CMBR were produced by microwave sources at low redshift the spectrum would not likely be close to the characteristic blackbody form. In the picture of Layzer (1968), and later Hoyle, Burbidge and Narlikar (1993), absorption and reradiation by dust relaxes the CMBR spectrum toward blackbody. A second test that tests that is the variation of the radiation temperature across the sky. The signature of the interaction of the radiation with matter in the angular distribution of the radiation would be different if the interaction were at low redshift in the dust model, or at high redshift in the big bang model. There was considerable progress toward applications of both tests in the 1960s.

My notes for a colloquium on 17 March 1966 at the University of Toronto show significant advances toward the measurement of the spectrum. In addition to the Penzias and Wilson detection at wavelength $\lambda = 7.4$ cm I could show the very recently published Roll and Wilkinson (1966) measurement at 3.2 cm and the CN temperature measurement (as in eq. [16]) at 2.6 mm by Field, Herbig and Hitchcock (1966). The fit to a thermal spectrum certainly looked promising. And there was another data point, the consistency with the helium abundance. By this time I was arguing that we had a significant case for the hot big bang model, and a serious challenge therefore for the steady state cosmology.

I can describe a few other early reactions to our proposed interpretation of the CMBR. Bob Dicke showed us a letter he had received from Zel'dovich. In this letter, dated 15 September 1965, Zel'dovich writes

> I am not more so cock-sure in my colduniverse hypothesis: It was based on the assumption that the initial helium content is much smaller than 35% by weight. Now I understand better the difficulty of helium determination. You draw some conclusions from the observed helium content 25%. Are you sure it is not 35% or 15%?
>
> It seems to me very desirable to measure the Planck spectrum corresponding to $3 - 4^\circ$K at its maximum, at the wave-length ~ 1 mm, although it is a difficult task.
>
> Undoubtedly your work will raise the interest to all sides of the problem

> and I sincerely congratulate you and your team on a success.

Novikov (page 70) comments further on Zel'dovich's reaction.

In his letter Zel'dovich argued that the oscillating universe model is "untenable as a consequence of unlimited growth of entropy." We knew the argument, but I think I recall that we were not so sure that entropy need be conserved in the bounce. This was before the development of the the serious cosmological singularity theorems Ellis discusses, but we were aware of the general idea. I remember Bob saying, in effect, that general relativity predicts that a collapsing universe develops a singularity but it don't say whether the singularity applies to the whole universe or just a bit of it, the rest bouncing. What would happen to the accumulation of singularities as well as entropy? I was willing to think of something else.

In his reply to Zel'dovich, dated 5 October 1965, Bob suggests that "the helium content of the proto-galaxy could very well have been zero." He was fascinated by the possibility that the strength of the gravitational interaction decreases as the universe expands. That would make the rate of expansion of the early universe much larger than in the standard model. If it were fast enough there would be negligible light element production at high redshift.

At the time of this letter there was in the literature just one directly measured point on the CMBR spectrum. Quite a few people reminded me that the blackbody interpretation is a considerable extrapolation from that. I remember in particular that Phil Morrison bet me one guinea that measurements at other wavelengths would not follow the thermal spectrum. It was at about the time of the Toronto colloquium that that he agreed that he had guessed wrong and paid me the one pound and one shilling.

By the end of 1966 Howell & Shakeshaft (1966) and Penzias and Wilson (1966b) had added a data point at 21 cm wavelength to the measurements at 7 cm, 3 cm and 2.6 mm. The spectrum up to the expected peak looked encouragingly close to blackbody. Not long after this measurement, in a letter dated 21 December 1966, Dennis Sciama wrote to Bob Dicke,

> I have recanted from the steady state theory, and have taken such a liberal dose of sackcoth and ashes that I am now more orthodox than the orthodox (though I don't suppose this phase will last long).

Sciama's new phase did last: he continued to work on the increasingly promising relativistic big bang cosmology, with particular attention to clues to the physics of the dark matter (Sciama 2001).

By 1970 three groups had attempted to measure the CMBR spectrum at wavelengths near 1 mm, where the spectrum is expected to break away from the power law form that applies at longer wavelengths. As Zel'dovich had remarked, and Harwit (page 199) and Weiss (page 211) describe, that "is a difficult task." From 1970 to 1990 a series of experiments indicated that the CMBR spectrum significantly differs from blackbody near and shortward of the blackbody peak. The beautiful experiments by Mather *et al.* (1990) and Gush, Halpern & Wishnow (1990) at last showed that the spectrum is wonderfully close to thermal. I have no complaints about the two decade long anomaly — we were seeing first-rate science in progress — but it did confuse the subject and it led me to think about other things. That mainly was the statistical analyses of the clustering and the dynamical analyses of the motion of matter on large scales. At the time that was a better thing to work on anyway. The field was ripe for exploration, and it grew into a component of the second critical test of the cosmological interpretation of the CMBR, the signature of its interaction with the growing inhomogeneity in the mass distribution.

I can date my work on measures of the cosmic clustering of matter to the March 1966 colloquium in Toronto. Sidney van den Bergh asked me how I could be sure the universe really is close to homogeneous in the large-scale average. I offered as evidence of it the CMBR, which we already knew is quite smooth, consistent with a near uniform large-scale mass distribution. The argument is pretty indirect, of course. Sidney countered that George Abell's map of the distribution of rich clusters of galaxies (Abell 1958) does not look very smooth. I said it doesn't look all that rough, considering the sparse sampling. I think I can remember Sidney's words, "you could check that." I worked out a method of checking it on the flight back home. Jer Yu improved and applied it in his PhD thesis, which was published in Yu and Peebles (1969)

I continued this analysis of statistical measures of the distributions of extragalactic objects, and of the dynamical evolution that might produce the observed clustering of matter, for more than a decade. There was a positive reason: this was rich fallow ground to explore. And there was a negative reason: the spectrum anomaly beclouded my thoughts about the CMBR.

Though I like to work alone, I needed help in the analysis of large-scale structure, and it appeared. Along with Jer Yu, I am deeply grateful (though it may not have always been apparent at the time) for my time in research on this subject in collaboration with Marc Davis, Jim Fry, Margaret Geller, Ed Groth, Mike Seldner, Bernie Siebers and Raymond Soneira. (All volunteered to join me. People somehow tend to sense where things interesting to them are happening.)

In 1969 I gave a graduate course at Princeton on current topics of research in cosmology. John Wheeler insisted that I turn the course into a book, and he sat in the back of the room and took notes until I agreed. That so unnerved me that I wrote *Physical Cosmology* (Peebles 1971). By then I understood that cosmology is a real physical science that offers fascinating issues of theory and observation. It was a science with a limited empirical basis, to be sure. A measure of that is that I could present a reasonably complete survey of the science (apart from the subtleties of the astronomical observations that the title was meant to indicate I would not attempt to address) in just 280 pages. I marshaled evidence for the homogeneity assumption that I concluded was encouraging but not definitive. A decade later the case was much stronger, but resistance to the assumption died out more slowly, a not unusual phenomenon. The last section in the chapter on the Primeval Fireball — the name John Wheeler had suggested for the CMBR — has the title "Is This the Primeval Fireball?" My answer was cautious, largely because of the apparent anomaly in the measurements of the spectrum at wavelengths near 1 mm. The case for the fossil interpretation of the CMBR is close to compelling now. We have a vastly improved spectrum measurement. We have detailed evidence that the radiation has the predicted disturbance caused by its interaction with the mass distribution at decoupling and along the line of sight. And we have the elegant concordance of the theory and observations of helium and deuterium. But all that is the subject of Chapter 5.

The observational basis for cosmology is far better than anything I would have imagined in the 1960s, and the case for the hot big bang far more compelling. But my assessment of the current situation has to be colored by the many changes I have seen through the decades in community opinions of what is a reasonable and sensible universe. Perhaps this accounts for my unease about declarations that we now know the physics relevant for the evolution of the universe from redshifts $z \sim 10^{10}$ to the present. We are attempting to draw large conclusions from what still is very limited data, and this very active field of research surely will yield more surprises.

David T. Wilkinson: Measuring the Cosmic Microwave Background Radiation

Dave Wilkinson's leadership in the exploration of the CMBR, through his own research and the education of other key players, continued from the identification of this radiation to his central role in a last great experiment, the Wilkinson Microwave Anisotropy Probe. David Wilkinson was one of the group who planned this book. He did not live to write a contribution, but Dave's voice comes through in this transcript of an interview conducted by Michael D. Lemonick on 25 July 2002 and recorded by The Educational Technologies Center, Princeton University.

DW: My name is David Wilkinson. I'm a professor in the Physics Department at Princeton. I work in cosmology and astrophysics; I do experiments. I came to Princeton in 1963, was lucky enough to find a hot research topic and rode that to tenure, so Ive been here ever since.

Q: What do you think led to your being a scientist, and in particular a physicist?

DW: I became a physicist because of a course in engineering I took in college, called "Cement." I couldn't imagine taking a whole course in cement. I enjoyed my freshman physics course, so I decided I would become a physicist and not a cement engineer. That really was the reason. Plus I really liked physics!

Q: Where did you go to college?

DW: The University of Michigan. I went to school at the University of Michigan.

Q: And where did you grow up?

DW: I grew up about 30 miles west of Ann Arbor in a little town called Michigan Center.

Q: Were either of your parents scientists?

DW: No. My father didn't graduate from high school. My mother worked her way through teachers college at Kalamazoo and ended up teaching math. So I think I got some of her genes for the math and science side. But I got the practical genes from my dad; that's why I'm an experimentalist. He could build anything and fix anything.

Q: So you majored in physics in Ann Arbor. Was there any particular area of physics that you specialized in?

DW: No, not as an undergraduate. As a graduate student, first of all I got a degree in nuclear engineering because that was the hot topic at the time and one could walk out with a Masters and get a fantastic salary of $10,000 a year. But I soon decided I didn't want to build reactors and I went into more of a particle physics mode. I did my PhD measuring how strong a magnet an electron is. It had little to do with cosmology but it was a lot of fun. And I had a great thesis advisor. [Dick Crane]

Q: How did you come to Princeton?

DW: Fortunately my PhD thesis turned out to be pretty important. Bob Dicke here at Princeton, people at Columbia, Harvard, and Yale, had all tried to do this experiment, but Dick Crane and I did it better. So the old boys network went to work and I got my choice of where I wanted to go. Things were a lot different in those days. And I decided I wanted to come here and work with Bob Dicke on gravitation.

Q: In what sense did you work on gravitation?

DW: When I first got to Princeton I worked on gravitation with Bob Dicke. He was doing ground-based experiments and had just started working on the shape of the sun and I was intrigued by that project. In the end, I didn't work on it but I realized that I had a real, fundamental interest in astronomy. Then Bob suggested a project which involved building a small radio telescope and that just completely clicked with what I wanted to do.

Q: What were you going to do with this radio telescope?

DW: About the time I came to Princeton, Bob Dicke independently had dreamed up the idea of a microwave background left over from a hot phase earlier in the Universe. Not only had he gotten the idea that the universe was filled with this thermal radiation, perhaps, but he had invented in 1946 the instrument to find it — the so-called Dicke radiometer which is famous in radio astronomy circles. So he sort of drew a picture on the blackboard and said, OK boys, go build this. So Peter Roll and I went off to build this little radio telescope which ended up on the top of Guyot Hall (Fig. 22, page 145) on one of those turrets of the building.

Q: Did you at that time have any preference for any particular model in cosmology? Were you in favor of the big bang?

DW: Cosmology was just completely in its infancy when I came to Princeton. There was still a huge debate going on whether it was big bang or a steady state universe. The steady state universe always looks like it does now, in the past and in the future. You have to play a few tricks with physics to do that, but philosophically it's very satisfying to think the universe will always look like it does now. And of course there was the big bang theory named by Fred Hoyle as a joke. It said that the universe started in a very hot condensed state and then expanded out and its still expanding. So the theories were both crude at that point. There was no data except that the universe was expanding. It was very hard to have any kind of an objective opinion. Of course if Dicke's idea worked out, the big bang was favored. Incidentally this was an idea that had been well published by George Gamow and his colleagues twelve years earlier, but we did not know about it. If Dicke's idea worked out, that was very strong evidence for a big bang. There was no way that this heat radiation could be naturally produced in the steady state.

Q: So you went out to build this radio telescope. How was Jim Peebles involved in this project?

DW: We formed a little group based on Dicke's idea to explore it. Jim did the theory behind it. If there was a big bang would this radiation still look like heat radiation? Would it have the spectrum (intensity versus wavelength) that one expects from heat radiation? Or would that have gotten distorted somehow between the big bang and now? That was the key calculation that had to be done. Jim also did a calculation on making the elements in an early universe which also, unbeknown to us, had been done by Gamows group earlier. So Jim did the theory, Peter Roll and I built the instrument and Bob was the great advisor.

Q: Even though you didn't have a personal opinion about which cosmological model was correct, did you have any sense that if you found this radiation it would be a very big deal?

DW: Yes. If we found this radiation it was certainly going to be a big deal because it would resolve this basic argument between big bang and steady state. Plus it would give us a tool for examining the physics in the very early universe before any stars or galaxies formed. And that was unprecedented: to have a probe that came right out of the big bang. There was a lot of anticipation. There wasn't a whole lot of hope.

Q: Why wasn't there hope?

DW: The idea that we might actually find this radiation seemed kind of remote to us. First of all, there was no other data to indicate that we were living in a big bang universe. It seemed rather fantastic that this remnant heat would be around and nobody would have discovered it before. Its not a weak phenomenon. Why hadn't the radio astronomers found it? It turns out, the way radio astronomers do their work, they have much better sensitivity than they need but they couldn't detect this cosmic radiation because it's coming equally from all directions. So the more we thought about it and read papers in radio astronomy, the more we realized that, yes, this stuff could be out there and nobody would have seen it. You need a very special type of radio telescope to see it.

Q: Tell us the story, the now famous story, of the day you were sitting in Bob Dicke's office having lunch and the phone rang.

DW: The group that was looking for this microwave background consisted of four people, Bob Dicke, the leader; Peter Roll and I, the experimentalists; and Jim Peebles, the theorist. Every Tuesday at lunch we would meet in Bob's office and discuss the progress and problems and so forth and try to figure out what we needed to do next. There were some very specialized pieces of equipment we had to build, and it wasn't obvious how to do it. Well, one Tuesday we were sitting there and the phone rang (that often happened: Dicke was a famous guy so people called him all the time). He picked up the phone and we went on with our conversation as usual, and then we heard him say "horn antenna." Well, that was one of the very special things you needed to do this experiment. And then he said, "cold load" — cryogenic load — and that was the other thing you needed to do this experiment. So now we were pretty tuned in because at this stage we were about halfway through building our apparatus with a horn and cold load. We hadn't gotten it on the roof to observe yet. So we listened to the rest of the conversation which didnt go on more than 5 minutes. Dicke hung up the phone and he said — I'll never forget his words — "Well boys weve been scooped." He realized immediately in this 5 minute discussion with Arnold Penzias that Penzias [and Bob Wilson] had been looking at this microwave background, this heat from the big bang, for a year thinking it was something wrong with their instrument. And to their great credit, Arnold Penzias and Bob Wilson stuck to it. Often experimentalists will sort of write off problems and say, "OK; well here's a little fact that I don't want to deal with; there's probably no important science in here; it's just some quirk in my apparatus," and they overlook it and go on and do their measurements. Well Penzias and Wilson didn't do that. They stuck in there. Improved their measurements. That's why Dicke was convinced so quickly, because they had their ducks lined up. They could answer all of our questions. So we went up and visited Bell Labs about a week later. We looked at their data, we looked at their apparatus, and it was obvious that they were seeing what we were looking for.

Q: When you found out that you had been scooped, did you stop working on your experiment?

DW: Oh no! Just because they had found the radiation we didn't stop. In fact we sped up because our apparatus was designed to measure a different wavelength than Penzias and Wilson had used. And this was the crucial test of the idea. No one would believe that what they were seeing was heat from the big bang without measuring its spectrum. Thermal radiation has a very special shape. So we charged ahead in order to try and verify this spectrum at a different wavelength than they had used. The discovery papers came out on my birthday in May and our measurement paper came out that fall. So we were about six months behind them.

Q: What was the reaction of the astronomical community to these papers? Was the big bang accepted pretty much immediately?

DW: The astronomers did not like it much, and the physicists didn't like it much for completely different reasons. The physicists didn't understand any cosmology at that time. It's completely different now, a lot of physicists work in cosmology. At that time, Dicke's group and a few others were the only ones working in cosmology. So there was no way that physicists could evaluate the science. And certainly with one measurement at one wavelength everybody was skeptical including us. We stuck our necks out and published a paper interpreting Penzias and Wilson's result in saying we think this is heat from the big bang. That was pretty roundly laughed at. Even after we got our data point, I got a lot of questions at meetings and got grilled. But gradually people started accepting it. These big paradigm shifts in science are always hard to swallow because whether you like it or not you're in one camp or another. Certainly the steady staters did not like this at all. The big bangers also had a little trouble with it because why did it take till 1964 to discover this stuff. Radio astronomy had been around for 15 years. So there was a lot of sort of detailed knowledge that needed to be accumulated before you really realized that these measurements probably indicated discovery of heat from the big bang.

Q: Once the idea of the big bang started to be accepted and people really did accept that this was radiation leftover from the fireball, what did you decide to do next? Did you have any thought of leaving cosmology and doing some other experiments?

DW: As the idea was gradually accepted that this radiation really was from the big bang, more and more people started coming into the field and making measurements of all different sorts. All of which agreed with predictions of the big bang theory. I saw this as a wonderful opportunity to do some groundbreaking research. Here was a brand new phenomena coming from the very early universe, something we never had before, not even come close to it. This radiation dates from when the universe was only about 300,000 years old and that's in a 14 billion year old universe. So this stuff came right from the beginning and it looked like we probably had an opportunity to do some really fundamental measurements on the early universe. I would have been crazy to get out of the field at that point. There were just too many opportunities!

Q: What was your next series of experiments? What did you decide to do next?

DW: The next thing we did after the initial verification of the spectrum, was to ask ourselves, "Is this stuff really coming from everywhere in the universe?" That was another crucial test. If this was some kind of new radiation from our own galaxy then it would be concentrated in the Milky Way. If it was truly a universal phenomenon, then it should be coming the same from all directions. Bruce Partridge, who joined us in 1965, and I modified the original apparatus to scan the sky and look for little wiggles in intensity. This is the so-called anisotropy in the radiation, which is a big industry these days. Well we didn't have very much sensitivity. Radiometers these days are a million times more sensitive than the thing we had. We set this thing up on top of Guyot again. Part of the experiment was to try and switch the beam around the sky. So we had a big reflector that would come up in front of the antenna and deflect the beam up to the North Pole. Then it would go down and the beam would go off to the equatorial plane. Well, we didn't quite apply enough oil to this thing so it started squeaking and the undergraduates were really annoyed by this thing because it went on all the time so it was squeaking away at night. So somehow those guys scaled the wall of Guyot, went up there and dismantled our reflector. This is one of those funny stories about Princeton undergraduates and what they'll do to do something different. Anyway that experiment went on for a year. All the data came off on chart recorders with pen and ink. Bruce and I would come in every day and spend about 2 hours reading those charts by eye, writing down long columns of numbers because at this point computers weren't around. There was no way to record the results digitally. And after a year we concluded that yes, this radiation was very isotropic, better than a tenth of a percent. Again it fit the prediction of the big bang theory. But this was really a part-time thing to carry us over to when we could build new technology. Meantime, while we were taking this anisotropy data, we were building much more sensitive receivers.

Q: When did you start using the more sensitive receivers?

DW: We started using the more sensitive receivers in the late 60s early 70s. Took them to mountaintops because water vapor in the atmosphere bothered us so we wanted to minimize the amount of water overhead. Mountaintops seemed like a good idea. Turned out it wasn't because of all the turbulence going over the top of the mountains. So the next thing we did was put our radiometers in scientific balloons and fly them from Texas. I had wonderful graduate student named Paul Henry (who's very active in the graduate alumni association). He built a radiometer with my help, trotted off to Texas, attached this thing to one of these big balloons and sent it up to 90,000 feet. Very successful piece of work, pushed the limit on the fluctuations down quite a bit. We almost discovered what is called the dipole in the radiation. That is, half the sky is warmer than the other half because we're moving through the radiation, so there is a Doppler shift that makes half the sky look warmer. Peebles had predicted this, predicted its magnitude. And if you look at Paul's data, he saw it but not with enough conviction that we were willing to say we've discovered dipole. It's another curious story. Paul saw the dipole about the right magnitude but almost completely in the opposite direction from what we had predicted. To predict the direction you assume that the center of our galaxy is fixed with respect to the radiation and that we are moving through the radiation because of rotation of the galaxy. So you know in which direction we're moving and that should be the warmer direction in the sky. Well it turned out the warmer direction was the other way. There was a lot of head scratching about that. I spent several days in the library trying to convince myself the astronomers had the right sense of rotation of the galaxy (which they did). So the only interpretation was that the whole galaxy was actually moving very quickly in the opposite direction and it's turned out that's the case. But it was one of those surprises in science, those things you don't expect to happen. You do a lot of head scratching before you publish something like that.

Q: So you could have discovered the dipole and the Hubble flow in the same experiment?

DW: Yeah. In this really crude apparatus that Paul and I built. It was a Rube Goldberg by today's standards.

Q: Meanwhile, while you were doing these balloon experiments and looking for the dipole and maybe even seeing it, I understand that by the mid 70s you also began talking to people about satellite experiment.

DW: It became clear that a space experiment was what one needed to look really carefully at the anisotropy and to look very carefully at the spectrum of the radiation to see if it fit the classical thermal spectrum. You had to get the atmosphere out of there. These are pretty delicate measurements and the atmosphere causes all kinds of trouble, mainly from water vapor and oxygen emission. And the water is clumpy in the atmosphere so you see all these clumps as the radiation goes through it. Makes your signal noisy. Several of us who were active in the business, Ray Weiss, John Mather, Mike Hauser, Ed Cheng, and Im sure Im forgetting somebody, got together at Columbia and started talking about a satellite to do both of these jobs, to look at the spectrum and to look for fluctuations if there were any. That was a long haul ...

Dave then goes on to describe the genesis of COBE and the demonstration that the CMBR has the signatures of cosmic radiation left from the big bang.

Peter G. Roll: Recollections of the Second Measurement of the CMBR at Princeton University in 1965

Peter Roll is retired, after 25 years as a university administrator of technology. He is currently working on the development of a community web portal for the retirement community in which he and his wife live, near Austin, Texas.

My perspective on the 1965 discovery of the cosmic microwave background radiation is quite different from that of other contributors to these essays. Dave Wilkinson and I had our first measurements of the CMBR in the summer of 1965. We satisfied ourselves and our colleagues – Bob Dicke and Jim Peebles – that they were valid. Shortly after this, I left Princeton to join the staff of the Commission on College Physics in Ann Arbor. One thing led to another in my career, and by 1971 I had gone into academic administration full-time at the University of Minnesota. Research became, for the remainder of my life, a spectator sport in which I played a support role in a variety of administrative ways. I've remained an active spectator, keeping up with scientific press reports on developments in which most other authors of these essays were directly involved.

Dave Wilkinson and I began work on the Princeton measurement of CMBR in 1964 – I had finished work on the Eötvös-Dicke experiment the previous year and we had written it up for publication (Roll, Krotkov and Dicke 1964), and Dave Wilkinson had recently joined Bob Dicke's research group. Dicke set both the theoretical context for our work— looking for remnant radiation from the big bang — and the experimental approach — using the Dicke radiometer he had invented in 1946 at the MIT Radiation Laboratory. Jim Peebles was doing the theoretical calculations, keeping us informed of how they were related to our experimental work. Dave and I were experimental physicists with no previous experience in radio astronomy and little experience working with microwave electronics and liquid helium. But we learned, and we designed and tested the equipment, with encouragement from Dicke and other groups in the Palmer Physical Laboratory. The work progressed well, and by February 1965 we expected to get data that summer.

In his last interview, Dave Wilkinson described the telephone call from Arno Penzias to Bob Dicke during one of our weekly lunch meetings. Dave's description is exactly as I remember it, with the exception of the length of the call. Dave described it as short, about 5 minutes, while I remember it as long, about 30-40 minutes. Visits were exchanged with Penzias and Bob Wilson at the Bell Laboratories Holmdel site and Princeton, and we

Figure 22: The first Princeton CMBR experiment, on Guyot Hall. Peter Roll is wearing the checked shirt; David Wilkinson is holding the screwdriver.

all knew that the Princeton group was going to be number 2 rather than number 1 on the discovery.

What did we discuss during these exchange visits? Two things have stuck in my mind, though I can't trust my recall too far after 40 years. The first is that Dave and I quizzed Penzias and Wilson on the details of their equipment — how they had dealt with the many difficult problems to eliminate sources of systematic errors. They satisfied us that they had done this properly, and we shared information on what we were doing about these same problems. We learned about pigeon droppings in the horn reflector antenna that Penzias and Wilson were using. Our equipment at Princeton was smaller for the shorter wavelength (our 3 cm vs. their 7.3 cm) and we could more easily cover it when not in use. Even though the birds themselves were plentiful on our Guyot Hall observing tower (Fig. 22), heat radiation from pigeon droppings in the antenna was not a significant issue for us.

The second detail we asked about was what Penzias and Wilson were looking for when they started their measurements. My recall is that they started out to make an absolute measurement of the radio flux from the Andromeda galaxy, for which they would need an accurate measurement of any background flux from the sky around Andromeda. I suspect my recall of this detail may be, at best, a little oversimplified and incomplete.

The story of how the two research groups learned of each other is told well enough in the 1978 Public Broadcasting System Nova program, *Whispers* *from Space*. Jim Peebles gave a talk on his work at the Johns Hopkins Applied Physics Laboratory in early February 1965, with Dicke thinking that we were far enough along with our apparatus to finish and get data before anyone else could do so — because it would take them longer than that to build the equipment. Professor Bernard Burke, a radio astronomer at MIT, either attended the lecture or heard about it — he knew a little about what Penzias and Wilson were up to and suggested to them that they should contact Dicke. What Bob Dicke quickly recognized was that, if someone already had the apparatus and had started or completed the measurement, they would beat us.

We had been pretty sure that, if what we were looking for turned out to be cosmological, it would be an important scientific discovery. Realizing we would be number 2 created, I think, a certain amountof "awkwardness" among those of us at Princeton. It was a disappointment, of course. I hope I speak for Dave Wilkinson and Jim Peebles in saying that we all felt more disappointment for Bob Dicke than for ourselves — Bob had been so close to a big one more times than most scientists. Dicke, I suspect, felt more disappointment for Wilkinson and me than for himself.

We understood that these things happen, and that being number 2 was still **very** important. At that time, the explanation was by no means certain, and none of us could be dead sure that there wasn't something wrong with the measurements. Penzias and Wilson were initially stymied by what they had, and we hadn't yet gotten data to examine. Number 2 would be important to confirm the result and get a second point on the spectrum — if it wasn't thermal, then our explanation could not be correct. At Princeton, we all got back to work, knowing that we were on to something important.

Shortly after the exchange of visits between the Princeton and Bell Labs groups, Bob Dicke informed us that each group would publish a letter, to appear back-to-back in the July 1965 *Astrophysical Journal*. The first letter would be by Penzias and Wilson (1965) announcing the discovery, followed by a second from the Princeton group interpreting the result as remnant thermal radiation from the big bang (Dicke, Roll, Peebles and Wilkinson 1965). We got to work on our letter, based largely on the work of Peebles and Dicke, but including a description of the work Wilkinson and I had begun.

Over the years, the "awkwardness" associated with the discovery has been, for me, explaining the situation to others. Broadcast of the PBS Nova program in 1978, followed shortly by the Nobel Prize award in the fall of that year, was the first time this work had been in the public eye enough to trigger these questions. Did I — or the Princeton group — feel in some way "cheated by circumstances?" I certainly didn't feel that way. All of us with the Princeton group at the time were sorry that Bob Dicke had not been included. I think we all understood also, in different ways, why the Nobel Committee did not do it that way. A 25-year history of research findings and conjecture preceded the discovery — it is summarized well by Peebles and Partridge, and documented in detail by many of the other essays in this collection. If nothing else, this left a confused situation for the Nobel Committee, and one about which there are differences of opinion. The discovery of the CMBR certainly deserved recognition. The Nobel Committee made a good choice, and it may have been the only one they could make.

My family and I first watched the PBS Nova program as a rerun in January 1979, after its first broadcast a few months earlier and after the Nobel prize had been announced. We watched it, in fact, in an empty hospital room that nurses had set up for us across the hall from where my wife was recovering from surgery. Our children ranged from 9^{th} grade to college junior in age, and I had previously told them the story and explained why Dave Wilkinson and I should not have received or shared the Nobel prize, I thought they understood. But when Penzias and Wilson appeared on screen showing their apparatus, the kids began exclaiming, *Is that them, dad — are those the guys that won the prize? ... Boo! Hiss! Boo! You guys took the prize away from our dad!* So I explained it to them again. Their outburst on that occasion was a somewhat more candid and immature way of expressing what many others have asked. With a few more years behind them, I know they understand now. Soon I will show the tape and explain it to two very bright grandchildren.

I've told the story of this discovery many times, including to classes on cosmology I've given several times in the Senior University of Georgetown. (I rely on the 1978 Nova video tape to tell the part about Princeton and Bell Labs, however, and I leave the rest to questions.) This Senior University is an "institution" formed by several fellow residents of the "active adult community" in which my wife and have I lived for the past 10 years. Its 600-odd students are almost all nonscientists — bright, mature adults with a lot of experience and accomplishments in their lives. They are fascinated by the story of how our universe began, but also by how and why scientists do this kind of work and arrive at some really strange conclusions — conclusions that are supported by a web of evidence from many different fields of research. Despite the fact that they are supposed to be objective, each scientist experiences the story personally and tells it differently. In this regard, science is no different than any other area of human endeavor.

The inside story of the discovery of CMBR, and the understanding of our universe to which it led over the past 40 years, is a magnificent example of the scientific method — messy, as it *really* is:

Looking back from 1965: Early research in the creation of heavy elements, intergalactic molecular spectra, the radiometer developed in radar research at MIT — a quarter century of missed hints and clues, almost but not quite pieced together more than once — finally pieced together, and two groups coming up with results at nearly the same time.

Looking forward from 1965: A discovery that was initially controversial has been so well-documented with a variety of measuring techniques, and new and much more sensitive detectors used at high altitudes, from satellites, and over a wide range of wavelengths. The CMBR was first examined and thought of as uniform in all directions. It became possible to measure the direction and speed of our galaxy's motion through absolute space by looking at a small asymmetry in the intensity of CMBR – hotter in one direction than in the opposite direction. Theories emerged on the earliest history of the universe, including Guth's strange super-expansion in the first instant of the big bang; and on how and when stars and galaxies began to form. These theories had to be consistent with one another, or they wouldn't be accepted. It became possible to calculate the distribution of tiny fluctuations in the CMBR and to measure these fluctuations from the WMAP satellite, distinguishing between some valid and invalid theoretical concepts and establishing numerical values for some of the important properties of the universe in which we live.

What a different understanding of the universe this is now, compared to the time when the steady state and big bang theories were actively contending with one another! During these past 40 years, many other concepts and variations were tried and found wanting, either because of theoretical inconsistencies or observations that did not support them.

The story of this one discovery has several of hallmarks of the scientific method, in addition to the messiness mentioned above:

Hypothesis about what you are looking for. How do you expect the research to turn out? Whether this concept is well-founded or speculative is beside the point. Dicke's real contribution to the original CMBR work was just that — it was his idea to look for red-shifted thermal radiation from the big bang. A corollary to this principle, however, is to be skeptical and challenge your own conclusions, especially if they support your biases. In drafting the second of the back-to-back letters to the *Astrophysical Journal* in the spring of 1965, Dicke incorporated a statement that the CMBR detection was evidence for a closed universe that would one day contract back on itself — a concept tied to his work on the Brans-Dicke scalar theory of gravitation. In a chance meeting of the two of us, I argued that he should remove this statement, leaving intact the discussion of ramifications of the CMBR for open and closed models of the universe (flat was thought to be very improbable at the time). I was quite uncomfortable disagreeing with a person for whom I had the utmost respect. Neither of us could have guessed that results, 40 years later from the WMAP satellite, would show the distribution of tiny fluctuations in the CMBR and confirm a flat universe so convincingly.

Careful documentation. When Dave Wilkinson and I completed our first measurements, we took time and care to document, in a 1967 article in the *Annals of Physics*, what we did and how we did it, in complete detail (Roll and Wilkinson 1967). I had done this earlier with the results of the Eötvös-Dicke experiment, because the validity and limits set by a null result are the important part of the experiment. We did likewise with our CMBR measurement, because, at the time, it was controversial and not at all accepted that it was a thermal spectrum; if there was anything wrong with our methods and analysis, we wanted others to be able to find it. In hindsight, this was completely unnecessary. There have been so many measurements by so many techniques confirming the blackbody properties of the CMBR and more, that the specifics of how we did the second measurement have become almost irrelevant. Nevertheless, it was important to both of us at the time to complete the job properly.

Persistence. There are three examples of this among the people I worked closely with at Princeton. The first is Bob Dicke, who devoted the last half of his professional life to gravitation and cosmology — devising conceptual/theoretical models, experiments, and observations to understand better the nature of this basic law of physics and the physical nature of our universe.

The second is Dave Wilkinson, who spent his entire career after graduate school following the trail of the cosmic microwave background, eventually into space. Dave's scientific legacy is his two decades of work on satellite observations of the CMBR, culminating in the Wilkinson Microwave Anisotropy Probe satellite. Results are still coming out of data from WMAP, as recently as two weeks before I write these words. All of us who knew him, even from way back, grieve that he is not still among us to witness the results of his dedication.

The third is Jim Peebles, who started as Bob Dicke's student and stuck with his study of galaxies, cosmology and related matters from a more theoretical perspective — but still related closely to observations and measurements. I am not as familiar with the details of Jim's work since I left the field, but I hear about it often enough to know that he has been at it consistently and persistently for 40 years.

Im quite sure that all three of my former colleagues have contributed as much, to science and society, by the students they trained and mentored as by the research they have pursued. Some of them have become successful scientists in their own right — others have gone off into other fields, as I did, and contributed in other ways.

An autobiographical appendix: notes on what drove me to physics, and then to leave for a different career. I came to physics from a family with no particular interests or talents in things scientific. From an early age, I had a knack and interest in things mechanical and quantitative. I entered Yale as an undergraduate with many interests. Before my senior year, I had taken no physics at Yale beyond a non-calculus introductory course. (I did sit through several graduate courses at Heidelberg during my junior year on an exchange scholarship.) My first job out of Yale (nuclear reactor design at Westinghouse), and my graduate work in experimental nuclear physics back at Yale, were both interesting and rewarding. But I've also played the French horn all my life. My motivation for physics was at least partly to understand the physics of that treacherous instrument, so that I might improve my skills as a performer. This, however, was not a fashionable area of physics research, and gravitation and cosmology turned out to be far more interesting.

Finally, when I became a full-time administrator at the University of Minnesota in the 1970s, I was able to continue teaching a course in Musical Acoustics and engage in a little research and dissertation supervision with the Departments of Music and Music Education. I learned from the late Arthur Benade (Case Western Reserve) that the basic physics of the French horn and other brass instruments is governed by the Webster horn equation (Bell labs, ca. 1916), which is none other than the Schrödinger equation with a transformation of variables. And I did learn how to play the horn better because of this work in the 1970s.

In 1965 I left Princeton for a year with the Commission on College Physics in Ann Arbor. A major activity that year was a report on "Computers in Physics Education" – the first ever report on the role of computers in higher education. When I joined the Physics faculty at the University of Minnesota in 1966, I was quickly identified as "...an expert on computers in education..." By 1971 I was serving on so many committees, doing interesting work for the University and the state, that I moved full-time into academic administration, with a portfolio including computers; radio, television and audio-visual services; and library technology. In 1984, I moved to Northwestern University as Vice President for Information Technology, leaving behind my vestigial teaching and research in musical acoustics. From there I moved in 1992 to Executive Director of *netILLINOIS*, a non-profit internet service provider mostly for Illinois educational institutions in the early days of the Internet. In 1995, I retired and moved with my wife to a new Sun City development in Georgetown, Texas.

In hindsight, it turns out that the theme in my life since 1971 has been networking and communities, rather than physics. This began with my appointment to a Cable Television Advisory Committee of the Metropolitan Council of the Twin Cities in late 1971, where the theme was cable TV as a community service network. Through most of the 1970s and 1980s, I was a Board member of EDUCOM, an organization that pioneered networking to support academic communities and introduced higher education to the Internet. At Northwestern, I set the stage for a proper networked campus, though it did not get far off the ground during my tenure there. As I approached retirement, it was clear that the Internet was the platform for the "community network" that so many of the activist younger generation were promoting in the 1970s in Minnesota. And so I moved to Sun City, Texas with an interest in seeing how the Internet might become a community network as it matured. And this is a work in progress. We started with a Computer Club that now has 2,000 members (out of 7,800 residents) and are finally in the process of implementing a community web portal, which will be our community network.

Throughout this 35-year period, the scientific and engineering research communities have been the creators of the platform for community networks of all kinds — ARPANet, BitNet, Usenet, TCP/IP, and all the others. These networks migrated into the larger society, finally, after 1989, when Tim Berners-Lee developed the WorldWideWeb at CERN, and in 1993 when Larry Smarr, an astrophysicist and Director of the National Center for Supercomputing Applications at the University of Illinois Urbana-Champaign, fathered the first web browser, Mosaic. The Internet as we know it today was catapulted into society and the economy by the particle physics and astrophysics research communities, as a tool that has improved scientific communication and made progess in science faster, more efficient, and more accessible. It has transformed not only research, but also society and the economy. Even retirement communities such as the one in which we now live.

One of the issues which interests many of our fellow retirees is why the US taxpayer should fund research in basic science. Cosmology really doesn't have that much impact on everyday life. I conclude my Senior University classes in Cosmology with this question: What is the return on this investment in basic research? The answer to this is now unbelievably easy. The economic impact of the Internet is the return on investment in particle physics and astrophysics research forthe last n years – you pick the number of years, and the dollars work out just fine.

But this economic impact is all an accident – it's not why any of us do or have done research in things like the CMBR – those reasons are much more personal and complex.

Robert V. Wagoner: An Initial Impact of the CMBR on Nucleosynthesis in Big and Little Bangs

Bob Wagoner is Professor of Physics, Emeritus, Stanford University. A continuing research interest is the physics of compact objects, including their roles as sources of gravitational radiation detectable by LIGO and other facilities.

Timing may not be everything, but it certainly can help. In 1960, when I was a mechanical engineering undergraduate at Cornell, I attended the Messenger Lectures of Fred Hoyle on cosmology. That experience, and books such as Dennis Sciama's *The Unity of the Universe*, opened my mind. I received my Ph.D. in physics at Stanford in 1965. My thesis was on general relativity, although I spent part of one summer working (amid many spiders) on Ron Bracewell's radio telescopes. I was on my way to a research fellowship at Caltech just as the discovery of Penzias and Wilson (1965) was announced. Soon after my arrival, Willy Fowler invited me to join him and Fred in an exploration of the consequences of this sea of photons, using nuclear astrophysics as a cosmological probe.

Details of my view of the development of primeval nucleosynthesis through 1973 can be found in a review (Wagoner 1990), where references that I have omitted here can be found. In keeping with the scope of this volume, my focus here will be mainly on the 1960s.

However, I begin by mentioning the first prediction of a cosmic radiation temperature (5 K) by Ralph Alpher and Robert Herman (1948), based on their work with George Gamow on what is now called Big Bang nucleosynthesis. It may not be well known that they neglected the overwhelming influence of neutrinos in establishing the neutron-proton ratio (and thus the synthesis of heavier nuclei), so that the approximate agreement with the eventual observation was fortuitous. Hayashi (1950) provided the correct interaction rates, and Alpher, Follin and Herman (1953) provided the first complete description of the standard model of the evolving major constituents (but no baryons except protons and neutrons) of the early universe.

It is somewhat of a mystery why this knowledge was not employed to recalculate the abundances until Zel'dovich (1963) and Hoyle and Tayler (1964) considered the production of the key nucleus, helium (^{4}He). Fermi and Turkevich had developed a nuclear reaction network just before 1950. Zeldovich obtained a high temperature (20 K), apparently because he believed indications of a low observed primordial abundance of helium (see the contribution by Novikov). This led him to (temporarily) abandon the big bang model.

Hoyle and Tayler provided more details of their (approximate but realistic) calculation, showing that the neutron-proton ratio when the weak interactions "froze out" essentially determined the abundance of helium, which was weakly dependent on the photon-baryon ratio. They noted, however, that conditions within exploding supermassive stars could be similar to that in the early universe (but with fewer photons per baryon). They also noted that the observed energy density of starlight only required the production of 10 percent of the observed amount of helium. Thus they concluded "that most, if not all, of the material of our everyday world has been 'cooked' to a temperature in excess of 10^{10} K." It also may not be widely known that they were the first to note that the number of types of neutrinos affects the expansion rate and thus the abundance of helium.

I was very fortunate to be a postdoctoral fellow (1965-1968) in Caltech's Kellogg Lab when it was a major hotbed of theoretical astrophysics. The emerging revelations of the nature of quasars only added to the excitement produced by the realizations of the consequences of the cosmic microwave radiation. The enthusiasm of Willy Fowler for many aspects of science and life (parties, etc.) infected everyone.

In our collaboration, Fred's point of view was of course influenced by his continuing belief in the steady-state universe and thus the production of helium and other light elements within exploding supermassive stars (which we dubbed "little bangs"), complementing the ordinary stellar production (Burbidge, Burbidge, Fowler and Hoyle 1957; Cameron 1957) of the heavier elements. However, he was also impressed by the fact that if the helium was produced mainly by ordinary stars and their resulting luminosity was somehow universally thermalized at a time close to the present epoch, the radiation temperature would be close to 3 K.

The most critical element in my computer code was the nuclear reaction data provided by Willy and his group and many other nuclear physicists. Of course, we also had to extrapolate or otherwise estimate the rates of a few reactions that had not been measured at the relevant effective energies (usually 0.1 to 0.5 MeV, except for neutrons).

I presented our first results at the April, 1966 annual meeting of the National Academy of Sciences (Wagoner, Fowler and Hoyle 1966). The calculation involved 40 nuclei and 79 nuclear and weak reactions. At about the same time, Jim Peebles' calculation of the abundances of helium and deuterium within both the standard model and universes with different expansion rates appeared (Peebles 1966). The accuracy of his predictions of the abundance of ^{2}H and ^{3}He was reduced by the limited number of nuclear reactions included.

Our results were published the following year (Wagoner, Fowler and Hoyle 1967). As indicated above, we considered a large range of the baryon-photon ratio, corresponding to big and little bangs. Our major conclusion was that reasonable agreement with observed abundances of ^{2}H, ^{3}He, ^{4}He, and ^{7}Li could be achieved if the universal baryon (matter) density was about 2×10^{-31} g cm^{-3} (a factor of 2 less than the presently accepted value from WMAP and other data sets; Spergel *et al.* 2006). However, the abundance data that was available was from within the Solar System (Earth, Sun, and meteorites), so we did not know how relevant it was. On the other hand, the predicted abundances have stood the test of time. We also explored the effects of inhomogeneity and neutrino degeneracy (large lepton-photon ratios). Within little bangs (larger baryon-photon ratios), carbon and heavier nuclei were produced, but the abundances did not closely resemble those observed unless the bounce occurred at a temperature of about 10^9 K. Fred believed that this could happen in the first generation of (supermassive) stars (usually termed Pop III stars, after the classification of stellar populations into the younger Pop I and the older Pop II).

My summer of 1967 (and 1971) at Fred's new Institute of Theoretical Astronomy at Cambridge was very memorable. Willy, Don Clayton, and I occupied the first "office" in the hut in the sheep pasture behind the present Institute. The only building housed the IBM 360-44 computer, which I had to myself a large part of the time to tune my nucleosynthesis code. Many discussions with Fred focused on the properties of supermassive stars (sometimes over martinis while watching cricket), and with Willy and the Burbidges on abundance issues.

My involvement in nuclear astrophysics essentially ended a decade later. Exploration of other big bang models (Wagoner 1967, 1973), including the results of Peebles (1966) and those within anisotropic universes (Hawking and Tayler 1966; Thorne 1967) revealed to me that in general, only three factors affected the abundances produced. They were

1. The number of baryons per photon.

2. The expansion rate, dependent upon the theory of gravity, anisotropy, ane other forms of mass-energy density (other neutrino types, gravitational radiation, magnetic fields, etc.).

3. The neutron-proton ratio, dependent upon the lepton (neutrino) number per photon and the neutrino phase-space distribution (if the expansion was anisotropic).

The agreement of the abundance of ^{4}He with that produced within the standard model, and the detection of interstellar deuterium (Rogerson and York 1973) then strongly supported the conclusion that the density of ordinary matter was far short of that required for a flat universe. It was very gratifying that the early universe produced precisely those nuclei that stars or cosmic ray spallation could not.

The power of this deep probe of the early universe is based upon the fact that its physics is known, from the heroic efforts of many nuclear physicists (Fowler, Caughlan, and Zimmerman 1967, 1975) and the discovery and subsequent measurements of the blackbody flux of cosmic microwave radiation.

Martin Rees: Cosmology and Relativistic Astrophysics in Cambridge

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When I enrolled as a Cambridge University graduate student in October 1964, after undergraduate work in mathematics, I had no particular research project in view, and minimal confidence that I'd made the right choice – indeed I seriously thought of switching to economics. But I ended up with few regrets, because of two bits of excellent luck which I couldn't initially forsee.

First, I was assigned as one of Dennis Sciama's supervisees. I already knew of Sciama through his splendid lecture course on relativity, and had read his book *The Unity of the Universe* (Sciama 1961). He had charisma; he inspired his research group with his infectious enthusiasm; he followed developments in theory and observation along a broad front, and he was a fine judge of where the scientific opportunities lay. When I joined this privileged group, George Ellis had completed his PhD, and was starting a postdoc; Stephen Hawking was still a graduate student, two years ahead of me; my closest contemporaries in the group were Brandon Carter, Bill Saslaw and John Stewart. Within a few months I felt I had made a fortunate choice.

But there was a second piece of luck: the mid-1960s were years of ferment in observational and theoretical cosmology. The discovery of the CMBR was of course the pre-eminent event, but these years also saw the emergence of "relativistic astrophysics:" the first high-redshift quasars, the discovery of neutron stars, and the first results from space astronomy (especially X-ray astronomy).

Dennis Sciama was "plugged in" to all these developments. He encouraged his students to interact, and to learn from each other. He eagerly shared new preprints (and correpondence, news of conferences, and so forth) with his students and postdocs, and with other colleagues such as Roger Tayler. (For instance, I learnt during coffee-time sessions about Hoyle and Tayler's work on helium formation, and the parallel work of Peebles. Also about the debate with the Moscow relativists about the nature of singularities.)

In the late 1940s, Fred Hoyle, Thomas Gold and Hermann Bondi. – then all in Cambridge – had proposed the steady state theory, according to which the universe, although expanding, had existed in the same state from everlasting to everlasting: as galaxies moved away from each other owing to the expansion, new atoms were continually created, and new galaxies formed in the gaps. This theory never acquired much resonance in the USA (and still less in the Soviet Union). But its three advocates were vocal and articulate people; and in the UK, especially in Cambridge, the theory was widely publicised and discussed. And it was indeed a beautiful concept. Sciama himself espoused it, and indeed described himself as its most fervent advocate apart from its three inventors.

The steady state theory was (rightly) touted as being a good theory because it was vulnerable to disproof. It made definite predictions that everything was the same, everywhere and at all times (i.e. at all redshifts). Therefore if things were different in the past from now, that was evidence against it. Even if there were evolutionary changes, optical astronomers in the 1950s were unable to detect objects at sufficiently large redshifts (and look-back times) for such changes to show up. However, radio astronomers realised that some of the discrete sources detected in their surveys were "exploding galaxies" too far away to be detected optically. Although the redshifts of individual objects were unknown, it was possible to draw inferences from the relative numbers of apparently strong and apparently weak sources (since the latter would, statistically at least, be at greater distances). In particular, the number of sources brighter than flux density S would scale as the -3/2 power of S in an uniform Euclidean universe; and when expansion and redshift were taken into account, the $\log N - \log S$ plot in a steady state universe would be flatter than the Euclidean slope. The first credible evidence against a steady state came from Martin Ryle's radio astronomy group in Cambridge (based in the Cavendish Laboratory), and from the Australian group headed by Bernie Mills. The slope (at least at the bright, high-S, end) was steeper than -3/2. Such a steep slope was incompatible with steady state: Ryle interpreted it (correctly as we now recognise) by postulating that we lived in an evolving universe where galaxies in the past (when young) were more prone to indulge in the "explosive" behaviour that rendered them strong radio emitters.

For me, coming fresh to the subject in around 1964, the scepticism that greeted Ryle's evidence was perplexing. Ryle's claims – indeed everything he had claimed from 1958 onward – seemed compelling to me (and have indeed been vindicated by later developments). But I later realised that the scepticism of the "steady statesmen" was not simply irrational obstinacy. Some of Ryle's previous data, in particular the earlier 2C survey, had turned out to be unreliable, owing to "confusion" caused by inadequate angular resolution. Moreover, he had initially vehemently opposed the suggestion that the so called "radio stars" — discrete radio sources with no obvious optical counterpart — were actually distant galaxies. To add even more irony, it was actually Thomas Gold who first made that suggestion – which of course became the cornerstone of Ryle's later argument in favour of an evolving universe. This "baggage" dating back to the early 1950s perhaps helps to explain why the steady statesmen held out against the evidence of the source counts. There was also, it has to be said, a personal antipathy between Hoyle and Ryle — two outstanding scientists of very different style.

Sciama took Ryle's data seriously, but when I joined his group in 1964 he was still clinging to the steady state theory. He conjectured that many of the unidentified sources were nearby. The apparent steepness of the $\log N$ – $\log S$ relation could then (he argued) reflect nothing more fundamental than a local deficit. But when the sources were revealed to have high redshifts, he abandoned this model (and never went along the route of saying that redshifts were non-cosmological). The clinching evidence that led Dennis Sciama to abandon the steady state was a very simple analysis that he and I did together on the redshift distribution of quasars (Sciama and Rees 1966). By 1966, more than twenty radio sources in the 3C catalogue had been identified with quasars with known redshifts (extending up to $z = 2.01$ for 3C9). We applied to this small sample a crude version of the "luminosity/volume" or V/V_m test developed by Rowan-Robinson (1968) and by Schmidt (1968). If the universe were in a steady state, the quasars of the highest intrinsic luminosity should have been uniformly distributed in comoving volume. But when we split them into redshift bins, each bin corresponding to a shell containing the same comoving volume as the others, the quasars were concentrated in the high redshift bins. This evidence suggested that quasars were more common (or more luminous) in the past — just as Ryle had argued was the case for radio sources.

In a big bang model, the redshift-distribution of quasars tells us little about the geometry of the universe, but something about the astrophysical evolution of galaxies — indeed I still work on the implications of such data for galaxy formation, reionization of the intergalactic medium, and cosmic structure formation. The detection of the CMBR of course offered far stronger evidence for an evolving universe than the radio source counts. Attempts to attribute the CMBR in a steady state model to a population of discrete sources were even more contrived than those required to reconcile the theory with radio source counts and quasar data. The attraction of the steady state model was that everything of cosmic importance must be happening somewhere now, and therefore must in principle be accessible to observations. The theory's advocates believed — as was reasonable in the 1950s — that in a big bang model crucial processes would be inaccessible. But it has turned out that we can indeed observe "fossils", of the formative early eras of cosmic history soon after the big bang. The CMBR itself, of course, is one such relic; so also are cosmic helium and deuterium, and the fluctuations in the CMBR. So Sciama's disappointment was short-lived and he became quickly reconciled to the big bang — indeed he espoused it with the enthusiasm of the newly converted.

In parallel with these observation-led advances, the 1960s saw a renaissance in general relativity — a subject which had for several decades been rather sterile, and sidelined from the mainstream of physics. The impetus came from Roger Penrose. In my first year as a graduate student, I heard Penrose speak in Cambridge about his concept of a "trapped surface." I undersood little of it, but was nonetheless fascinated. Roger Penrose is the kind of person who, even if you don't understand (or don't believe) what he's saying, gives the impression that an unusually insightful brain is at work. His thinking is not merely much deeper than most of us can manage — it is of a very special geometrical nature. Sciama was quick to seize on the importance of Penrose's new concepts. (Indeed it was he who had persuaded Penrose, whose PhD was in pure mathematics, to shift his interests to relativity). Sciama encouraged some of his students to attend a lecture series that Penrose was giving in London. The most important outcome was Stephen Hawking's subsequent collaboration with Penrose, which led to the singularity theorems for gravitational collapse. The main import of Penrose's work for cosmology — as described in the article by George Ellis (page 288) — was an adaptation of these arguments to show that there must have been a "singularity" in the past of our universe, even if it was irregular at early times.

There was, at that time, a substantial research effort (spearheaded by George Ellis and a series of collaborators) aimed at investigating and classifying the various classes of homogeneous but anisotropic cosmological models. This was an interesting exercise in its own right. However a special motivation came from Charlie Misner, who spent the academic year 1966-67 on sabbatical in Cambridge. It was from Misner that we learnt about the so-called "horizon problem," that causal contact become worse in the early phases of a Friedmann (decelerating) universe, rendering it a mystery that the present universe seemed so uniform and synchronised. Misner noted that causal contact would have been better if the early expansion had been anisotropic – best of all in the "mixmaster" model where there was an alternation in the axes of fast and slow expansion. The aim of the "Misner program" was to show that a universe could have started off (and homogenised) via a mixmaster phase, but that the initial anisotropies would have been erased, either dynamically or via neutrino viscosity. This program failed —

and until the invention of the "inflationary" universe, more than a decade later, most of us probably thought that an explanation of global homogeneity would have to await a quantum-level understanding of the singularity. It was coincidental that the theoretical advances in relativity, instigated by the new "global methods" that Penrose pioneered, happened concurrently with the discovery of the CMBR.

It was a further coincidence that, during the 1960s, objects were discovered where general relativity was crucial, rather than a trivial refinement of Newtonian gravity — discoveries that stimulated the new research area of "relativistic astrophysics." The discovery of quasars (and, later, of neutron stars) indicated that objects probably existed in which the crucial features of Einstein's theory would have to be taken into account. Black holes of course are the most remarkable prediction of Einstein's theory. The Schwarzschild solution, discovered in 1916, represents the simplest black hole. They were speculated about in a rather half-hearted way by astronomers and cosmologists in the 1930s to 1950s. But the term "black hole" was not used until 1968, when it was coined by John Wheeler, and it was only in the late 1960s that theorists really clarified the nature of black holes.

A more general solution, discovered in 1963 by Roy Kerr (1963), was believed to be a description of a collapsed spinning object. The biggest breakthrough actually came from the work of Israel, Carter, Hawking and others. They showed that Kerr's solution was generic, in the sense that any black hole would end up being described by this particular solution of Einstein's equations. Any gravitational collapse leads, after the emission of gravitational waves, to a black hole described exactly by two numbers, its mass and its spin. So black holes proved to be just as standardised as an elementary particle.

The number of people involved in these theoretical developments was even smaller than the experimental and observational community — indeed most relativists were associated with one of three "schools," those centred in Princeton, in Cambridge and in Moscow. Communications were far less immediate than today (especially, of course, between East and West in the Cold War era). However the interactions that occurred were almost invariably cooperative and friendly. My own work was mainly on astrophysics and on galaxy formation: for this work, the new paradigm of the hot big bang was the essential backdrop, rather than being at the focus. My aim was to understand how galaxies produced so much radio power, how they became quasars, etc. It was already fairly clear that the power generation involved gravity, although, despite early advocacy by Salpeter, Zel'dovich and Novikov and (especially) Lynden-Bell, it wasn't as clear as it would become in the 1970s that a single huge black hole was implicated.

I continued to be uneasy, until the early 1970s, about the apparent coincidence between the energy in the CMBR and the energy that could be 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 (e.g. 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 virialised 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 polarisation of the CMBR. The simplest illustrative examples of this effect arose in anistropic 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 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.

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 - 57 when Margaret Burbidge, Fred Hoyle, Willy Fowler and I were solving in detail the problems of the origin of the elements (Burbidge, Burbidge, Fowler and Hoyle 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 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} 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 $H_0 = 550$ 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 $10^9\,\mathrm{K}$ 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 massless 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 non-relativistic 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 $\rho_b = 1.70 \times 10^{-2} t^{-3/2}$ gm 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 $\rho_b = 3\times10^{-31}$ gm cm^{-3} had the same helium abundance. I then showed that if it were produced by hydrogen burning the energy density must amount to $\approx 4.5 \times 10^{-13}$ 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 over-estimating 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 which I missed, as did Bondi *et al.* (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 *et al.* paper) was that the energy density corresponding to the production by hydrogen burning when the energy was degraded to black body form would give a black body 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 black body 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 black body the radiation temperature 1.8 K<T<3.4 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 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 $T = 3^{\circ}\!.4$ K and $R(0)/R(1) = 10$ giving $T = 1^{\circ}\!.8$ 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 1960's 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 the serendipitous discovery by Penzias and Wilson came in 1965.

But throughout the 1960's the ideas emanating from Princeton and also from Moscow from Ya. B. Zel'dovich's led almost everyone to believe that the radiation could only be a remnant of a big bang and would be of black body form.[21] It would be proof that the steady state theory was wrong.

[21] 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 at which early rocket observations were reported which did not confirm the black

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, 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 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).

Jayant V. Narlikar's View: I recall that one day in 1964, Fred Hoyle walked into his office in the Department of Applied Mathematics and Theoretical Physics 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 favouring 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 (1965). Although the blackbody nature of the radiation had not been established in 1965, its finding together with helium abundance apparently had the effect of

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body idea. Those were immediately severely criticized by leading theorists who did not understand the experimental details but were absolutely convinced that the black body nature must be correct. They eventually turned out to be right, but their prejudice was obvious.

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 U.S.A. likened this reaction to the problematic situation of Lynden 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 these:

1. 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?

2. 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.

3. 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 non-relic 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, Wickramasinghe and Edmunds (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, Wickramasinghe and Edmunds (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:

1. 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 *et al.* (1994) for details.

2. A Planckian spectrum at all wavelengths except possibly at wavelengths longer than 20 cm. (There the galactic noise anyway dwarfs the cosmological effect.)

3. 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).

4. The dust density required for thermalization being consistent with that needed for dimming distant supernovae.

5. A weak polarization on the scale of clusters arising from magnetic alignment of whiskers scattering the radiation.

6. 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 co-authors 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.

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 cosmic microwave background radiation by Penzias and Wilson (1965).

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 Ya B. 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 Zeldovich'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.

Zeldovich, 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 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 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.

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 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 (1965) 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 in *Nature* I published a paper 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).

Jasper V. Wall: The CMBR – How to Observe and Not See

Jasper 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, lawnmowers, 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'd found the conventional branches of engineering to be less interesting than I'd wished. I headed off into Engineering Physics, great training for applied research postgrad studies. But in what? I'd 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 Masters Degree programme in the Department of Electrical Engineering at the University of Toronto, starting autumn 1963.

My joint supervisors were Donald MacRae, Professor and Head of the Department of Astronomy, and the brilliant and enigmatic J. L. (Allan) Yen, Professor of Electrical Engineering, theorist, instrumentalist, expert on Toronto Chinese cuisine (chopsticks were an early part of my graduate education) and a man who required almost no sleep. I saw both my supervisors but rarely, and then only when I was in trouble with them, this more frequently than was comfortable. I learnt through the standard apprenticeship system, the senior grad students mentoring the new student intake. I learnt most from Ernie Seaquist, who was well into his PhD programme in the Astronomy Department. He was patient and generous to me with time precious for his own extensive radio astronomy programme, and by example he taught me far more than just radio astronomy.

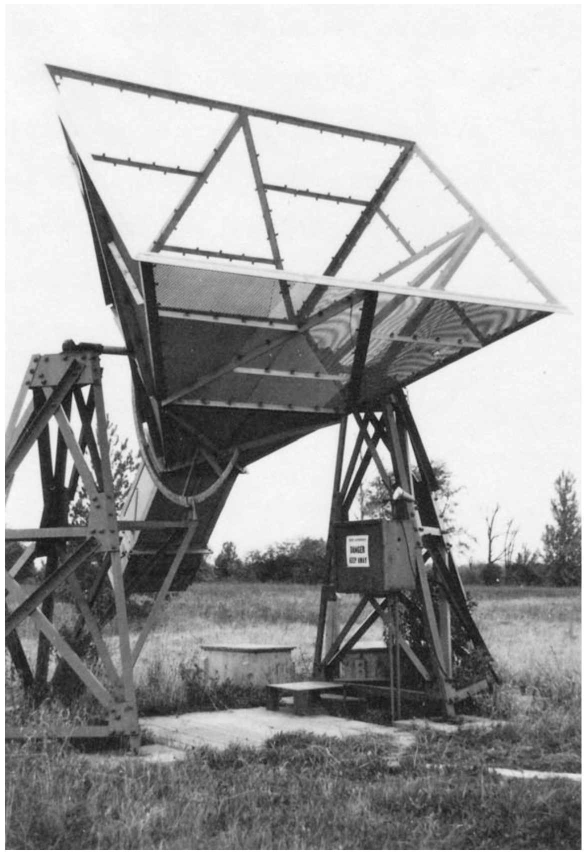

Figure 23: The pyramidal horn antenna, aperture 3.7 by 2.8m, used at 320 MHz for my Galactic background temperature measurements.

My project was to measure absolute temperatures of the Galactic background at 320 MHz, using the pyramidal horn already installed at the David Dunlap Observatory (DDO), Richmond Hill, 19 miles north of Toronto. The horn itself (Fig. 23) was in relatively good shape, needing some cleaning to remove certain avian deposits of the sort that Penzias and Wilson (1965) encountered in their researches. The challenge as I mapped it out was a) to build a reasonably low-noise amplifier and Dicke-switching receiver and b) to design and build a reference cold-load for the switching system, one with absolute temperature known to specified accuracy. The measurements were then simple drift scans, with the horn turned to the North Celestial Pole at periodic intervals for a reference level. This level would be calibrated by replacing the horn input with the reference cold-load input. There were impedance-matching subtleties involved, as long-serving radio astronomers will recognize.

First task – to build a new receiver at 320 MHz. Field effect transistors, FETs, had just become available, actually working at this high a frequency! Low noise as well! But they cost real money, all of $34 each. In a rare interview with Allan, I got the money, and the transistor. Next day I blew it up. (In retrospect I begin to understand the supervisor problem.) I managed to extract funds for a second one, and after walking around it for an afternoon, made a decision on how to handle it which helped me the rest of my life. It's just another transistor! Handle with ordinary care - otherwise I couldn't see how I'd get anywhere. It worked. I applied the lesson later when dealing with original astronomical plates. Treat them as you treat glass, with respect, but without awe. More tense and more 'careful' $\equiv$ greater risk and less research.

The second FET ran throughout the project. The new receiver was built with help of George Watson, a solitary soul working out at Richmond Hill: a craftsman, a perfectionist and a delight, whose stories, unrepeatable and certainly unprintable, enlivened many of my days and nights in the little frozen cabin at Richmond Hill, while adding a certain breadth to my graduate education. More supervisor trouble ensued when in the course of transporting a frequency generator to the cabin (they weighed about 150 kg in those days), I settled the old radio-astronomy station wagon axle-deep into the Observatory grounds in soft spring mud.

The cold load was a real challenge. Nobody really knew how to proceed, and the one I fashioned was the best technical achievement of my MSc. It did work well, and I was confident of its noise temperature - but note that it was a liquid nitrogen cold load, at about 80 K. This was close to the mean Galactic brightness temperatures; but of course a long way away from CMBR values.

I heard/read of the CMBR as my observations progressed. Reaction (a): nothing to do with me; I'm a Galactic (semi-) astronomer, working at too low a frequency and too high a mean brightness. Reaction (b), with minimal cosmic consciousness and from a radio astronomy point of view: surprise, Ryle was right after all – but a singular beginning? Steady state was conceptually much easier to handle...

And following this 2 minutes of deep thought, back to reality – the horn antenna had half-power beamwidths of $19.0° \times 22.5°$ degrees. Absolute temperature mapping requires correction for the response in side and back lobes, of course. Thus I built a scaled version of the horn, complete with

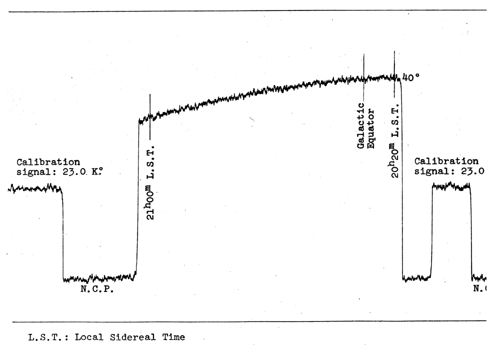

Figure 24: A chunk of drift scan, this one at declination $\delta = 40°$ complete with periodic visits to the North Celestial Pole and calibration-signal injections.

supporting structure, smaller by a factor of 9 and operating at 2.88 GHz. I mounted this on the antenna range turntable on the roof of the Electrical Engineering building, with a distant horn reflector plus S-band generator to provide the signal. The main-beam and first sidelobe patterns agreed remarkably well with the main-beam measurements of the main horn using drift scans of the sun, a point source (only 30 arcmin in size!) to the fat beam of the horn. The side and back lobes enabled me to estimate the spillover radiation.

There were many delays, including my MSc course load and stormy winter weather. Measurements began in February 1965 and continued to June; I covered the hottest part of the sky but by June (Fig. 24), interference from the USAF Buffalo base essentially halted the observations. I could not finish the cold (Galactic anticentre) parts, another sore point between me and supervisors. My MSc thesis, complete with the iterative calculations to remove side and back-lobe responses, was completed in October 1965. In parallel Donald Chu ran a sister set of measurements at 707 MHz, using a 2.5 m precision horn reflector at the Algonquin Radio Observatory of the National Research Council of Canada. The techniques he used followed mine precisely, including construction of a scaled model of the horn reflector. His measurements and mine were to be used to calibrate in absolute terms higher-resolution Galactic Plane surveys at DDO with a new 10m paraboloid reflector (for which I did commissioning and feed design.) These together with polarization measurements which Ernie Seaquist was working on were to provide comprehensive data on the Milky Way emission. This grander scheme never happened.

In November I set off for Australia, where I had been offered a scholarship at the Australian National University to do a PhD in a collaborative radio-optical programme between Mount Stromlo Observatory and the Australian National Radio Astronomy Observatory at Parkes. John Bolton was to be my supervisor. My seduction by astronomy was complete. Engineering cropped up later in life in building CCD systems, commissioning telescopes etc; but it was astronomy now where my commitment lay.

Donald Chu, finishing the same patch of sky I had done, likewise left for different things, a proper job in his case with the then largest computer company.

In the excitement of starting a new life in a country where snow drifts across the telescopes were no longer a problem, the brightness temperature measurements were temporarily laid aside.

The rest of the story has a certain inevitability about it. Donald Chu had made some tentative comparisons of his data with mine; he found unsatisfactory answers. We knew roughly what the emission spectrum of the Galactic background was - this synchrotron emission continuum from long-blown supernovae had a brightness spectral index of about -0.5 to -0.7 (Yates and Wielebinski 1967). Comparison of the 320 – 707 MHz results at independent map points by Donald and myself yielded a spectral index of -0.3, far too flat. Trying to reach indices in the 'recognized' range meant zero point errors outside our estimates. In 1965 we had left it at this: we'd both moved on.

In 1967 or 1968, as cosmological consciousness dawned, I realized what had happened. Subtracting 3 K from both our sets of measurements yielded spectral indices in agreement with the 'known' results (Fig. 25). I collected the data together, re-digitized it, and finally wrote up the experiments (Wall, Chu and Yen 1970). There was no great urgency at this stage.

In retrospect a dedicated CMBR measurement would have been simple. We had only to cover the colder parts of the sky, put our two sets of measurements together with a prior on the Galactic emission spectral index, and a measurement of the excess radiation was there. We were a bit late in the time frame — but if we'd got on with it in the first years of our MSc degrees rather than spending them wading through forgotten courses on plasma physics, the result was waiting for us.

The most astonishing aspect to me in hindsight was just how easy it would have been to make the measurement successfully, using the horns we already had, and a financial outlay of almost nothing.

I blame VLBI (partially). If Allan Yen had not become preoccupied with

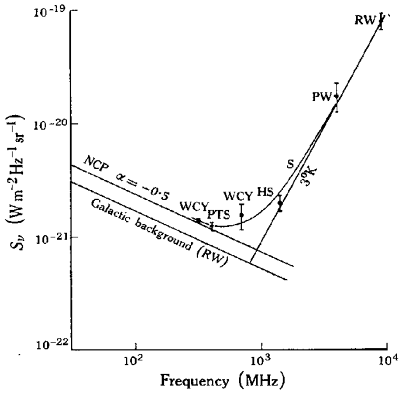

Figure 25: The surface brightness measurements, circa 1969, from Wall, Chu and Yen (1970). PTS – Pauliny-Toth and Shakeshaft (1962); PW – Penzias and Wilson (1965); HS – Howell and Shakeshaft (1966); RW – Roll and Wilkinson (1966); WCY – Wall *et al.* (1970).

this (Broten et al. 1967) I know his razor-sharp mind would have seen the possibility; he read everything and was on top of everything. I know that excess radiation was in his mind — although he never mentioned CMBR or excess radiation to me, his annoyance when I had been unable to finish measuring colder parts of the sky convinced me of this. This too came in retrospect.

The CMBR subsequently played little part in my career of observational cosmology. I stuck to AGNs and their spatial distribution, together with schemes of (unified) beaming models. Most of this was with radio-selected samples. There were perhaps just three points of contact:

i. In carrying out the (1984 version) deepest survey at 5 GHz with the VLA, Ed Fomalont, Ken Kellermann and I put limits on CMBR fluctuations in the range of an arc minute and a bit less. These were the best upper limits at the time; but they were far from real detections at these angular scales, as we now know. Perhaps our main contribution was to determine how to minimize cross-talk between the antennas, a help to subsequent experiments. Even so, the VLA for all its power was never the instrument for CMBR fluctuations.

ii. The standard model has the CMBR dipole, 1 part in 1000, explained as the Earth moving at 370 km s^{-1} relative to the rest frame, with

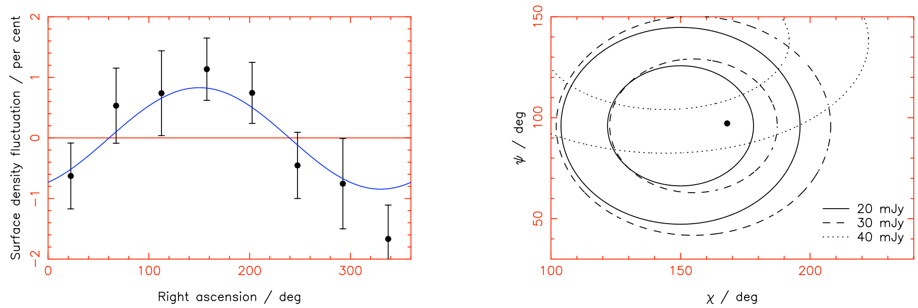

Figure 26: Left: measured amplitudes of the deviation from mean surface density for NVSS sources, as a function of right ascension. (Note that the direction of the CMBR dipole lies – accidentally – close to the Celestial Equator.) The predicted amplitude is shown as the solid line. Right: error circles (1σ, 2σ) representing the direction of the NVSS dipole for samples selected at different flux-density levels. The point denotes the direction of the CMBR dipole.

apparent temperature brighter in the direction of motion. The predicted motion should be visible in the number counts of distant objects, their combined surface brightness enhanced in the direction of motion of the earth. There are serious difficulties in looking for this dipole in discrete objects: how distant, how to select, how to perform widescale calibration; what to do about obscuration, how to get beyond the cluster-dominated epoch. A uniform all-sky survey of radio sources offers hope, however, as Ellis and Baldwin (1984) pointed out. After completion of the superb NRAO VLA Sky Survey (NVSS; Condon et al. 1998), that hope could be really entertained. It took much work to understand the systematics of the survey, and much work to remove the nearby objects from it — but in the end Chris Blake and I succeeded in observing the dipole (Blake and Wall 2002), agreeing in magnitude and direction with Earth motion as implied by the CMBR (Fig. 26). This remains the only detection of the velocity dipole in discrete galaxies, objects formed long after the epoch at redshift $z \sim 1100$ corresponding to the last scattering surface from which we see the CMBR. The mean redshift of our radio galaxies is about unity. The Universe is therefore showing large-scale homogeneity at this epoch, and further analyses coupled with new deep and wide sky surveys can refine this result. Although few doubt the interpretation of the dipole in the CMBR, the detection in real objects represents one of the tests the CMBR needs to pass if it is truly a relic of the Big Bang (Ellis 2002).

iii With superb results from WMAP (Bennett *et al.* 2003), and with the Planck mission on the horizon, we would like some reassurance that the fluctuations we see in the CMBR are not contaminated by extreme inverted-spectrum populations of radio-mm sources. To this end, with Rick Perley, Robert Laing, Joe Silk and Angela Taylor, I recently proposed a 43-GHz VLA survey of some 2 square degrees of the northern sky to search for such a population. This is the highest frequency search for extragalactic radio sources – and it found very few (Wall *et al.* 2006). We conclude that at small angular scales and the high frequencies of the power spectrum of the fluctuations in the CMBR, have little to fear from discrete-radio-source contamination.

I offer some conclusions.

1. The CMBR was there all the time in our 1965 data; and we could have done the measurements earlier with specific attention to detecting it as a part of our absolute measurements. It would have come in somewhere between 3 and 5 K at a guess. I think it's a stretch to say that we would have believed it on its own; our frequencies were a little low. But had there had been contact with cosmologists such as between Penzias and Wilson (1965) and Dicke, Peebles, Roll and Wilkinson (1965), then it might have been different. Too if my cosmic consciousness had not dawned so slowly, it might have been different.

2. I cannot have any regrets. My MSc project was superb for starting research in observational astronomy. How better to learn everything about the basics of radio astronomy? Every aspect in the process was revealed to me in glaring detail, all the pitfalls, noise, bandwidth, line-loss, mismatch, spillover, ground radiation, antenna patterns, conversion of antenna temperature to brightness temperature... it was baptism by fire, and I did love it, I think. It is next to impossible for a student nowadays to learn about instrumentation in depth at any wavelength, and I grudge a big vote of thanks to my supervisors Donald MacRae and Allan Yen for so comprehensively dropping me into it.

3. It is possible to observe and not see. After all Donald Chu and I were only a couple of engineers playing around with horn antennas...

John Shakeshaft: Early CMBR Observations at the Mullard Radio Astronomy Observatory

John Shakeshaft is an Emeritus Fellow at St Catharine's College, Cambridge. He served for many years as an Editor of Monthly Notices of the Royal Astronomical Society

At the time of publication of the Penzias and Wilson (1965) paper, I was a member of staff in the Radio Astronomy Group of the Cavendish Laboratory, the physics department of the University of Cambridge, having been an undergraduate and graduate student at Cambridge, the latter under the inspiring supervision of Martin Ryle. I had had an interest in cosmology and measurements of cosmic radio radiation for over ten years. Indeed my first published scientific paper, in 1954, had the title *The Isotropic Component of Cosmic Radio-frequency Radiation*, although I advise readers not to bother to search it out. At that date, low-noise receivers for the microwave range had not yet been developed, so interest was concentrated at lower frequencies. Westerhout and Oort (1951) had shown that the survey of galactic radiation at 100 MHz (or Mc/sec as we called it) by Bolton and Westfield (1951) could be explained by assuming that most of the radiation came from "radio stars" distributed through the Galaxy in the same way as the common Population II stars of types G and K, although it was necessary to add in an isotropic component besides. They suggested three possible explanations for this extra component but found none to be satisfactory. Subsequent to their paper, extragalactic sources much more intense than normal galaxies had been identified, such as the so-called "colliding galaxies" Cygnus A, and I attempted an estimate of the integrated contribution due to these. Interestingly perhaps, in view of later controversies about the number-counts of radio and their cosmological significance, I concluded that the isotropic component could be accounted for by standard relativistic cosmological models but not by the steady state theory. Shortly after publication, however, the general realization that galactic radio emission is largely due to synchrotron radiation from cosmic ray electrons in the interstellar magnetic field vitiated both the Westerhout and Oort model and my conclusion from it.

Towards the end of that decade I began work, with graduate student Ivan Pauliny-Toth, on a survey of the background radiation at 404 MHz (λ = 74 cm). This was intended as part of a study over a range of frequencies to determine the spectrum of the galactic radiation as a function of direction, which could provide information on the dependence of cosmic ray electron density and interstellar magnetic fields on position within the Galaxy. It was therefore important, if brightness temperatures at different frequencies were to be compared, for these temperatures to be absolute values rather than merely relative values in different directions. We used an 8-m diameter dish on an alt-azimuth mount (it was in fact a German radar dish "liberated" after the Second World War by Martin Ryle, and one of the two dishes that Graham Smith used as an interferometer to determine accurate positions of the sources Cygnus A and Cassiopeia A, enabling their optical identifications by Walter Baade and Rudolph Minkowski), an electron beam parametric amplifier and a Dicke-type radiometer with a liquid nitrogen reference source. The published survey (Pauliny-Toth and Shakeshaft 1962) was used later both at Cambridge and elsewhere to correct CMBR measurements for the contribution due to galactic radiation The experience gained on determining losses in the antenna and connections, and ground radiation in the far-out sidelobes, was also helpful in measurements of the CMBR a few years later.

The news in 1965 of the serendipitous result from Holmdel of 3.3 K CMBR at 4080 MHz (the second such major astronomical discovery from there, the first being Karl Jansky's accidental finding of the galactic radio emission in 1931) was received at the Mullard Observatory with great interest but no real surprise, since our work on radio source surveys over the previous 12 years had left us convinced that the Universe was evolving and not in a steady state; the discovery of radiation from a big bang therefore fitted naturally with these ideas. Although the data from the 2C Survey had been over-interpreted in terms of actual sources, the ingenious $P(D)$ probability analysis by Peter Scheuer (1957) of the deflections D of the interferometer records themselves, without the identification of individual sources, showed conclusively (to us, at least) that the slope of the radio source counts $N(\geq S)$ was proportional to $S^{-1.8}$, significantly steeper than the $N(\geq S)$ proportional to $S^{-1.5}$ expected for a uniform Euclidean model, and even more so than the values expected for Friedman and steady state models. By 1965, the increase in the numbers of actual identifications of distant radio galaxies and quasars had confirmed the excess of sources at large redshifts, and subsequent studies have shown that Scheuer's result for the slope was indeed correct. One of the merits of the steady state theory was said to have been that it gave specific predictions, unlike the Friedman models, but its proponents seemed very reluctant to accept that these predictions were in conflict with the observations.

I realized that we were in a position fairly easily to check the Holmdel temperature value at a different frequency, namely 1407 MHz ($\lambda = 21.3\,\mathrm{cm}$), which would help to determine whether this component of radiation had a thermal spectrum as predicted. With the aid of graduate student Tim

Howell, a copper horn of beamsize $13^\circ \times 15^\circ$ was set up inside the 8-m dish mentioned above, itself surrounded by a wire mesh screen, 30 m square, lifted at the edges, so the horn was doubly screened from ground radiation. The horn was connected to a Dicke radiometer, with a termination in liquid helium as the reference source. This consisted of a metal film resistor at the end of a 75-cm length of low-loss coaxial line. The temperature distribution along the line was measured and the effective noise temperature at the upper end calculated to be $T_{\mathrm{L}} = 5.9 \pm 0.2\,\mathrm{K}$. When the leads from the horn and cold load were interchanged in the circuit, the alteration of receiver output gave a direct measure of the temperature difference with no contribution from any asymmetry of the switch and leads. The receiver output was calibrated by measuring the differences between terminations immersed in water at various temperatures.

Observations were made at night with the horn directed towards the zenith (declination $\delta = 52^\circ$) at a right ascension such that the galactic radio emission was at a minimum. A temperature difference $T_{\mathrm{H}} - T_{\mathrm{L}} = 0.9 \pm 0.1\,\mathrm{K}$ was found, implying that $T_{\mathrm{H}} = 6.8 \pm 0.3\,\mathrm{K}$, representing the combined contributions from (a) galactic radiation and the CMBR, (b) atmospheric emission, (c) ground radiation, and (d) losses in the horn and waveguide-coaxial connection. For (b) we took the value of 2.2 K, derived by Dave Hogg for a wavelength of 20.7 cm (see below), and assumed an error of $\pm 0.2\,\mathrm{K}$. For (c), we measured the polar diagram of the horn and estimated a value less than 0.1 K, and for (d) we calculated a contribution of $1.3 \pm 0.2\,\mathrm{K}$. The sum of (b), (c) and (d) was $3.5 \pm 0.3\,\mathrm{K}$, leading to a value of $3.3 \pm 0.5\,\mathrm{K}$ for the minimum background brightness temperature. The galactic contribution to this was found by convolving the reception pattern of the horn with the brightness temperatures measured in the survey at 74 cm mentioned earlier and scaling the result to 20.7 cm by assuming T to be proportional to $\lambda^{2.7}$.[22] The result was $0.5 \pm 0.2\,\mathrm{K}$, leaving a CMBR value of $2.8 \pm 0.6\,\mathrm{K}$ (Howell and Shakeshaft 1966), which turns out to be gratifyingly — if fortuitously — close to the currently accepted value of 2.725 K.

The atmospheric absorption for frequencies up to 8 GHz is due predominantly to non-resonant absorption by molecular oxygen, and Hogg

[22] To my embarrassment, Jim Peebles, in reviewing this piece, has noticed that the actual wavelength corresponding to a frequency of 1407 MHz is of course 21.3 cm rather than 20.7 cm, as appeared in the original paper and as I unthinkingly copied above. He is the first person in the last 40 years to have pointed out this blunder to me. At this late date I do not have the original working material available and so cannot determine whether the quoted temperature of $2.8 \pm 0.6\,\mathrm{K}$ might require modification. Any such change would only be in the second decimal place, already omitted due to the size of the error.

(1959) had calculated values from 400 MHz up, on the assumption of a line-broadening constant of 0.75 GHz per atmosphere. Our search of the literature for experimental measurements of the absorption by observations of the extinction of extra-terrestrial sources as a function of zenith angle had revealed a relatively wide scatter of values, with some in very poor agreement with Hogg's predictions. To throw more light on this problem, we carried out measurements of our own at 408 and 1407 MHz. The interpretation of these involved consideration of the change of apparent angular size of the source in question due to differential refraction in the atmosphere. After applying the necessary corrections, our results fitted well with Hogg's curve, but we then realized that some of the earlier workers had either not applied the refraction correction or had applied it with the wrong sign. Judicious reworking of the earlier results, where necessary, then produced a satisfactory agreement between theory and experiment (Howell and Shakeshaft 1967a).

On completion of the initial measurement of the CMBR at 1407 MHz, we tried to check whether this component of the cosmic radiation could be detected at the lower frequencies of 408 and 610 MHz, although the dominance of galactic radiation in this range would cause increased uncertainties. Studies by Peter Scheuer (1975)[23] and by Ray Weymann (1966) had suggested that deviations from a blackbody spectrum might be present at low frequencies. We used optimal scaled horns with beamwidths of 15°, screened from ground radiation by wire mesh, and Dicke radiometers with liquid helium reference sources as before. After applying corrections for other contributions as at 1407 MHz, the effective brightness temperatures from the region of the celestial North Pole were $24.3 \pm 0.9\,\mathrm{K}$ at 408 MHz and $10.4 \pm 0.7\,\mathrm{K}$ at 610 MHz, the ratio of these, 2.3 ± 0.2, being significantly less than the ratio of 3.1 ± 0.1 expected for the galactic contribution with a temperature spectral index of -2.8 ($T \propto \lambda^{2.8}$). This implied that there was indeed an extra component of radiation characterized by a temperature close to independent of wavelength, that is, a blackbody spectrum. Further analysis indicated that, if the spectrum of this component were blackbody, the excess temperature would be $3.7 \pm 1.2\,\mathrm{K}$ (Howell and Shakeshaft 1967b). Unfortunately, the error in this value was such that no new upper limit could be put on the epoch of ionization of the intergalactic gas.

This work concluded for over twenty years observational studies at the Mullard Radio Astronomy Observatory of the CMBR, since other groups much better equipped for work at high frequencies had vigorously entered

[23]Presented in an article written in 1965 for *Galaxies and the Universe*, and eventually published in revised form in 1975

the field, but they were subsequently taken up again with the building by Paul Scott and others of the Cosmic Anisotropy Telescope (CAT), a three-element interferometer which, in 1996, was the first telescope to detect structure in the CMBR on angular scales smaller than the main peak in the angular spectrum (Scott *et al.* 1996). This was followed by the Very Small Array (VSA), now observing from Tenerife the anisotropies on angular scales between 15 arcminutes and 2 degrees, and the Arcminute MicroKelvin Imager (AMI) to study the Sunyaev-Zeldovich effect in high-redshift clusters and proto-clusters of galaxies. In addition to this observational work, there has been theoretical modelling of background fluctuations, and the Cambridge Planck Analysis Centre has been set up in preparation for the launch in 2008 of the European Planck Surveyor satellite.

Other authors in this volume have noted that, if the attention of observationalists had been drawn to the matter, the CMBR could perhaps have been detected (or recognized as such) years earlier than in fact it was. It is, for example, unfortunate that in neither of the two editions (1952 and 1960a) of his influential textbook *Cosmology* did Hermann Bondi refer to the possibility, nor did Fred Hoyle in his paper *The Relation of Radio Astronomy to Cosmology* at the *Paris Symposium on Radio Astronomy* (1958). We must hope that sufficient of the astronomical literature is now available on the World Wide Web for rapid searches which could prevent oversights of this kind in the future.

William "Jack" Welch: Experiments with the CMBR

Jack Welch retired from teaching Astronomy and Electrical Engineering at UC Berkeley in 2005 but continues as the Alberts Professor in the Search for Extraterrestrial Intelligence. He was Director of the Radio Astronomy Laboratory at Berkeley from 1972 to 1996 during which time the BIMA Millimeter Telescope array was built and operated. He continues his research in the interstellar medium and star formation and is curently working on completion of the Allen Telescope Array.

My introduction to the question of the absolute radio brightness of the sky came from a talk that I heard at a meeting of the IEEE Antennas and Propagation group held in Palo Alto in 1961 or 1962. The Speaker was R. W. De Grasse, one of the team of engineers at the Bell Telephone Laboratories that had developed a communication system for the Echo project. He described the horn-reflector antenna and maser receiver amplifier that had been built at Crawford Hill in New Jersey. As a young radio engineer just beginning work in radio astronomy at Berkeley, I was enormously impressed with the quality of the instrumental work and the care taken with the system noise measurements. I remember him saying that they assumed the sky background temperature to be zero but were uncertain about an excess of a couple of degrees or so in their summary of system noise contributions. The excess was thought to be pick-up in the antenna sidelobes (Ohm 1961). At the time, I had no idea what to expect for the background.

A few years later, I read the *Astrophysical Journal* letter by Penzias and Wilson (1965) describing their beautiful background measurements with that same antenna. Using a new receiver at 4.08 GHz with a new reference load (Penzias 1965), they were able to report with certainty an excess of about 3.5 K that had to be ascribed to the cosmic background. The companion paper by the Princeton group (Dicke, Peebles, Roll and Wilkinson 1965) with the plausible interpretation that the radiation was the blackbody radiation remnant of an earlier stage of an expanding universe was very exciting. George Field, who had recently joined the Berkeley Astronomy Department, was very taken with the new finding and realized that earlier observations of the excitation of interstellar CN (Herzberg 1950) might be consistent with the new radio observations. The excitation of the first rotational level of the CN line corresponded to background radiation at a wavelength of 2.6 mm, suggesting that the excess radiation was that of a blackbody in agreement with the Princeton group interpretation. At the time, our group was developing receiving equipment at wavelengths near 1.0 cm for radio astronomy and studies of atmospheric emission with a small antenna. George urged us to attempt a measurement of the cosmic microwave background radiation to help determine its spectrum at the shorter wavelengths.

We decided to take a detour from our other program to study the background at a wavelength of 1.5 cm. An important piece of information about the universe had been found. We might be able to add to that, and it would be an interesting instrumental challenge. At 1.5 cm wavelength, the background emission from the atmosphere is rather high at sea level sites, and we planned an observation from the High Altitude Barcroft Laboratory of the University of California White Mountain Research Station. Sam Silver, the Director of the Space Sciences Lab at the University of California, had outfitted a trailer for remote observations, and we were able to take it to the Barcroft Laboratory for our observations. The atmospheric emission brightness is typically only 3 to 4 K at the 12,400 foot altitude of the Barcroft Laboratory. Our technique was conventional. We used a Dicke radiometer that compared the brightness of the sky as detected by a standard gain horn and associated receiver with that of blackbody loads at known temperatures, and we made tipping measurements to extrapolate the brightness to zero air mass. One difference in our system was that we used a load at the temperature of liquid nitrogen as our low temperature reference rather than a liquid helium load. We felt that we could characterize it well and it would be easier to manage at the remote site than a liquid helium load such as those used by the other groups. As a check, we measured a liquid helium load in the lab at Berkeley with our system and found the correct temperature. We spent the summer of 1966 making background observations at the high altitude site.

Our reported result, 2.0 ± 0.8 K, was disappointing (Welch, Keachie, Thornton and Wrixon 1967). The final uncertainty was large. The reproducibility of individual measurements was limited by the scatter in the measurements of the liquid nitrogen load brightness. Because of the greater temperature difference between the sky brightness and that of liquid nitrogen, the extrapolated results were subject to greater random errors. In addition, our mean value was low in comparison with the results of the other measurements available at the time of our publication. The average, particularly including the first radio detections (Penzias and Wilson 1965; Roll and Wilkinson 1966) and the temperatures derived from CN measurements (Field and Hitchcock 1966; Thaddeus and Clauser 1966) were pointing to a blackbody temperature of 3.0 K or even higher, outside our error limit. As the more accurate measurements, shortly thereafter from the Wilkinson group (Partridge and Wilkinson 1967) and others, and finally from the COBE satellite (Mather *et al.* 1990) came in, we were somewhat relieved that the limit of our error just included the final blackbody temperature, 2.74 K.

A year or so after our publication, I was reexamining the characterization of the pyramidal horn for some other calibrations that we were planning and discovered that I had made a mistake in the model tipping curve that we had used for the background measurements. Correcting for that properly, we would have had 2.3 ± 0.8 K for our result, a little closer to the final accurate temperature. Since that miscalculation was small compared to our random errors, we did not think it appropriate to publish it. In retrospect, I realize that was a mistake. The systematic error is, of course, different from the random errors, and it should have been reported.

We subsequently returned to our original program of getting a short wavelength telescope running for other astronomical observations, particularly for studies of solar system objects and the interstellar medium. There we had some nice results with the first discoveries of polyatomic molecules in the interstellar medium revealing the molecular clouds where stars are born (Cheung et al. 1968, 1969). Then we proceeded to develop interferometry at the short wavelengths for interstellar medium and star formation studies as well as for other fields.

Our most recent encounter with CMBR studies occurred when we discovered that we were making some accurate ground-based flux measurements of Jupiter at the same time that they were being made by the WMAP satellite in the course of its calibration (Page *et al.* 2003). We had just completed our study when the WMAP results were announced. Our measurement was made at a wavelength of 1.05 cm (Gibson *et al.* 2005), in between the two longest WMAP receiver bands and close to the center of the Jovian ammonia inversion absorption band. Our accuracy for the Jovian flux was about 1.5% and it fell nicely between the Jovian fluxes of the two adjacent WMAP observations which had comparable accuracies. I think that everyone was pleased with the good agreement between these independent calibrations of Jupiter. Our result enabled us to get a fairly accurate measure of the upper Jovian atmospheric ammonia abundance. Absolute calibration to 1-2% accuracy was essential for getting a good Jovian atmospheric model, and the WMAP results helped with that as well.

Some of the best memories from the earlier period were of discussions with Dave Wilkinson, an experimentalist of extraordinary capability.

Paul Boynton

to come...

Robert A. Stokes: Early Spectral Measurements of the Cosmic Microwave Background Radiation

Robert Stokes is President and CEO of Versa Power Systems, a solid-oxide fuel cell development company in the Denver, Colorado area. After completion of his Ph.D. at Princeton in 1968 he received an appointment as an assistant professor at the University of Kentucky where he continued work on the CMBR. Later he managed the engineering physics division at Battelle, Pacific Northwest Laboratories, served as Deputy Director of the National Renewable Energy Laboratory, and was Senior Vice President at the Gas Technology Institute.

As an undergraduate student at the University of Kentucky in the 1960s, I was part of a generation with a growing interest in space science encouraged by the US educational system's response to the Soviet launch of the Sputnik satellite. At the end of my junior year I was selected to attend one of the first Goddard Institute summer study courses in space science at Columbia University organized by Robert Jastrow. After an intense summer of focusing on planetary astrophysics, our group was treated to a memorable tour of several US space science facilities, traveling aboard a chartered DC6 aircraft in August of 1963. The tour included visits to the NSF astronomical observatory at Kitt Peak, Arizona; the Marshall Space Flight Center in Huntsville, Alabama (the tour conducted by none other than Werner Von Braun); the NASA launch facility at Cape Canaveral; and NASA headquarters in Washington, DC. As a result, my interest in space science was greatly intensified, and when presented with the opportunity to attend graduate school at Princeton, a focus on space science was a foregone conclusion.

After a year of graduate study at Princeton and a couple of stimulating classes taught by John Wheeler, I managed to land a summer appointment working as a student research assistant for Bob Dicke and Mark Goldenberg, taking data on a special ground-based telescope designed to measure the solar oblateness as a test of the predictions of the Brans-Dicke theory. That summer, Paul Henry, another graduate student, and I traded off making observations while observing practice by the Princeton hammer-throw athletes, hoping all the time that our apparatus would not be damaged by a mis-thrown 16-lb steel ball. By the end of the summer I had become a part of the graduate student cadre associated with the gravity group, led by Dicke and Wheeler and including their junior colleagues Peter Roll, Jim Peebles, Dave Wilkinson, Mark Goldenberg, Kip Thorne, and Bruce Partridge.

Jim Peebles had already begun some theoretical work on the implications of a hot-fireball model for the early universe and the nature of any remnant radiation. Earlier modeling work by Alpher and Gamow (Alpher, Bethe and Gamow 1948) and measurements of atmospheric radiation by Dicke, Beringer, Kyhl and Vane (1946) had laid the groundwork for the research not pursued in earnest until the mid-1960s. But by 1965, Peter Roll and Dave Wilkinson had already begun operation of a 3-cm radiometer specifically designed to test the blackbody radiation hypothesis.

By the time the Princeton group had connected up with Arno Penzias and Bob Wilson at Bell Labs and published the famous 1965 companion papers (Penzias and Wilson 1965a; Dicke, Peebles, Roll and Wilkinson 1965) on the cosmic microwave background radiation, I had just completed my Ph.D. qualifying exams and was looking for a dissertation topic. Roll and Wilkinson (1966, 1967) had sent their their confirming radiometric measurements to *Physical Review Letters* in January of 1966 and there was great interest in investigating the spectral nature of the newly discovered isotropic microwave radiation.

Dave Wilkinson agreed to take me on as his first doctoral student at Princeton and set me to work building a 1.58-cm radiometer to make coordinated measurements with two radio telescopes he and Bruce Partridge were constructing for a second series of measurements at 3.2 cm and 8.56 mm. We had made arrangements to conduct several months of measurements at a high-altitude laboratory operated by the University of California at Berkeley in the White Mountains along the California/Nevada border during the summer of 1967 to establish a more precise temperature for the background radiation field.

The hypothesis that the microwave background radiation is, in fact, the primeval fireball rests heavily on the spectrum being that of a blackbody. Measurements completed prior to 1967 had all been consistent with a spectral index $\alpha = 2$ over a considerable wavelength range in the Rayleigh-Jeans region of a 3 K blackbody.

To be convinced one is seeing true blackbody radiation and not that of a hot graybody, it is necessary to go to short wavelengths and look for the curvature in the spectrum due to quantum statistical effects. In early 1967, Dave Wilkinson, Bruce Partridge, Paul Boynton and I began a series of experiments aimed at refining the absolute radiometric techniques and extending the wavelength coverage to 3.3 mm, a wavelength sufficiently short to differentiate between a true blackbody and a hot graybody.

Four Dicke radiometers were constructed by the group using similar designs at wavelengths of 3.2 cm, 1.58 cm, 8.56 cm, and 3.3 mm and taken to mountain-top observing sites to reduce atmospheric background. The 3.2-cm and 1.58-cm measurements were repeated to check the earlier work and to

Figure 27: Robert Stokes with a 3.3-mm Radiometer — Colorado 1968.

provide an accurate determination of the spectral index. The 8.56-mm and 3.3-mm points were expected to show deviations from the frequency-squared dependence in the spectrum of a hot graybody, the deviations amounting to 20 percent at 8 mm and 300 percent at 3 mm.

A great deal of experience was gained from the Roll and Wilkinson radiometer that was operated atop a building on the Princeton University campus, and Dave Wilkinson in particular was able to build on his exacting electron $g-2$ Ph.D. work at Michigan (Wilkinson 1962) to design an approach that dealt with systematic errors in the experiments. A number of other articles in this volume provide a good bit of detail and photos of the experimental apparatus used by the Princeton group (e.g. the article by Bruce Partridge starting on page 252), so I will not repeat the details here.

The 3.2-cm, 1.58-cm, and 8.56-mm experiments (Stokes, Partridge and Wilkinson 1967; Wilkinson 1967) were performed at an altitude of 12,470 feet at the Barcroft facility of the White Mountain Research Station, Bishop, California, during July and August of 1967. The 3.3-mm result (Boynton, Stokes and Wilkinson 1968) was obtained in March 1968 from an altitude of 11,300 feet at the NCAR High Altitude Observatory, Climax, Colorado. Figure 27 shows a photo of the author with the 3.3-mm radiometer at the Climax site.

Paul Boynton had completed a nuclear physics Ph.D. at Princeton (Boynton 1967) and decided to stay on as a postdoctoral student in the gravity group. At about the time that Dave Wilkinson, Bruce Partridge and I departed for Mount Barcroft, California, via Yuma Arizon, Boynton started designing and procuring parts for the new 3.3-mm Dicke radiometer that was to be employed for the follow-up series of measurements.

Whereas the microwave radiometer components for the longer-wavelength radiometers were mostly commercially available items, the microwave mixers for the 3.3-mm superheterodyne receivers were still very much a development-stage component in 1967. After my return from California, Boynton and I spent several months attempting to procure or develop an acceptable microwave mixer that would work at 90 GHz (3.3 mm). We visited several military development labs and received considerable help and loaned components from the Aberdeen Proving Ground staff in pursuit of a working radiometer in late 1967. At the beginning of 1968 we made the decision in consultation with Dave Wilkinson to give up on locating a reliable microwave mixer and put together a portable laboratory to transport to Colorado so that we could construct the detectors ourselves from GaAs wafers and gold-alloy sharpened cat whiskers. It seemed a lot like the early days of radio experimentation, but it worked! However, for the Colorado observations, we typically needed to change out the mixer once or twice during each of the all-night runs.

The choice of wavelengths for the radiometers was dictated by the location of atmospheric windows in the millimeter band. This absorption and subsequent reemission are the result of closely spaced pressure-broadened resonance lines that occur in the water molecule near 1.3 cm and 0.27 cm wavelength, and in the oxygen molecule near 0.5 cm and 0.26 cm wavelength. To further minimize atmospheric effects, measurements were performed at high altitudes during times of low absolute humidity. In addition to the usual problems with absolute measurements, the cosmic background radiation spectral measurements were made more difficult by the impossibility of modulating the signal due to its isotropy. Since the microwave background signal is the residue after one has accounted for everything else, control of the systematic effects and careful calibration were crucial.

The results of the four radiometer measurements made by the Princeton group using these techniques were all consistent with a 2.7 K blackbody. A

Figure 28: Paul Boynton (third from left) at NASA Ames in 1971.

graybody spectrum fitted to the 3.2 cm and 1.58 cm result would have predicted a result 5 standard deviations above the result at 3.3 mm, thus these were the first direct radiometric measurements indicating spectral curvature.

Paul Boynton and I used an improved version of the original 3.3-mm radiometer carried to an altitude of 14.9 km in the NASA Ames Research Center Learjet to get the first direct radiometer measurement in which the atmospheric contribution was less than the cosmic background. The radiometer was not calibrated using a primary calibration source during the airborne measurements. It was calibrated before and after flight. This experiment was the result of follow-on work by Paul Boynton and me after we left Princeton. Paul had taken an Assistant Professorship at the University of Washington, and I had an appointment as an Assistant Professor at the University of Kentucky. Much of the final preparations for the airborne experiment were facilitated by my spending the summer of 1971 at Battelle, Pacific Northwest Laboratories in Washington State.

A typical airborne experiment involved a flight to an altitude of 55,000 feet in order to perform the measurements above the tropopause of the earth's atmosphere. Even though the Learjet was pressurized, we were required to wear oxygen masks in case of a failure of the modified safety hatch that carried the radiometer antenna. On our last flight, after we announced to the ex-Navy pilot that the experimental results looked good, the pilot treated us to a perfect 1-g barrel roll without losing a drop of liquid helium from the calibration reference Dewar flask. Figure 28 is a photo of Paul Boynton and the pilot (left) just before takeoff on one of the flights.

Boynton and I published the airborne results in a letter to Nature (Boynton and Stokes 1974). The measurement clearly showed the expected short-wavelength departure from the Rayleigh-Jeans spectrum. At this point any questions about the cosmological nature of the microwave background had been put to rest, and attention shifted to detailed measurements of the anisotropies in the CMBR with the eventual launch of the Wilkinson Microwave Anisotropy Probe on June 30, 2001, named in honor of Dave Wilkinson who died in September 2002.

My subsequent career choices have taken me away from space science and cosmology to a focus on energy technology, however I continue to follow developments in cosmology and space science as a highly interested individual.

Martin Harwit: An Attempt at Detecting the Cosmic Background Radiation in the Early 1960s

Martin Harwit is professor emeritus of astronomy at Cornell University and a former director of the National Air and Space Museum. He is a Mission Scientist on the European Space Agency's Far-Infrared Submillimeter Telescope project, Herschel, of which the National Aeronautics and Space Administration is also a sponsor.

In 1963 I initiated an effort to look for the cosmic background radiation from space (Harwit 1964). The small research groups I started, first at the Naval Research Laboratory in Washington, DC, and later at Cornell University, designed and constructed cryogenically cooled rocket telescopes to detect this radiation. My calculations showed that we would be hindered by zodiacal foreground radiation. Our telescopes eventually confirmed this, by detecting the zodiacal glow along with a number of strong diffuse Galactic sources. We also obtained painfully false results on the submillimeter component of the 3K microwave background radiation, due largely to emission by contamination carried aloft by our rockets (Shivanandan, Houck and Harwit 1968). In order to depict the many mishaps, missteps and misconception that motivated me to initiate background observations in the early 1960s, I begin my account ten years earlier, when I was a graduate student.

In the spring of 1954 I found myself standing in the Physics Department office of Prof. David M. Dennison at the University of Michigan in Ann Arbor. Dennison was an eminent molecular theorist. Sitting behind his desk, he was finding it difficult to tell me that I would not qualify for a PhD in physics because I really had no aptitude for science. Perhaps I should look at other occupations, because science clearly was not my metier.

Two months earlier, I had turned 23, and my career as a scientist already was reaching an unfortunate conclusion. I had come to Michigan to study chemical physics. Although my undergraduate major had been physics, I had taken an advanced chemical physics course before coming to Michigan and read through Linus Pauling's (1948) excellent book *The Nature of the Chemical Bond*, and Gerhard Herzberg's (1945, 1950) two books on molecular spectroscopy. The field looked genuinely exciting. At Michigan, I was assigned to carry out near-infrared spectroscopic work on peptide bonds in the laboratories of Prof. G. B. B. M. Sutherland, who later became Director of the National Physical Laboratories in Britain. During my one-year apprenticeship, I learned a lot about infrared techniques but did not accomplish much. I was studying for my doctoral exams at the time, and neither the research nor the exams went well, which was why I was standing in Prof. Dennison's office that day.

Having to leave the Physics Department and wanting some time to figure out what to do next, I stayed on in Ann Arbor that summer, and found a job in the laboratories of Prof. Leslie Jones in the School of Engineering. He and his group were conducting upper atmosphere research with rocket-borne instrumentation. I did some optical design with the group and tackled whatever jobs needed doing. It was the first time I had real fun in science.

The war in Korea had not yet ended in 1954. In the fall of that year I received the then-standard letter from my draft board, which began with the ominous words, "Greetings from the President of the United States." and explained how my "friends and neighbors" had selected me to serve in the United States Army. I was to report for my two year stint of duty in January 1955.

Because I had earned an MA in Physics at Michigan by this time, the Army assigned me to the Chemical Corps at the Army Chemical Center in Edgewood, Maryland. This is where I began my real scientific training. Most of the civil servants in the Corps were chemical engineers and knew little about physics. But now, ten years after the end of World War II, the government was asking them to work on radioactive fallout, neutron doses from nuclear bomb bursts, and similar problems. I found that with a few visits to the base library, I could usually figure out what needed to be done, and although I was just an army private, I was given a lot of responsibility. Nevertheless, my civil service supervisor would send me to places like MIT or Woods Hole to verify with known experts that my calculations had been correct, and I enjoyed the opportunities offered by those visits.

In my second year in the Army, I was sent to Eniwetok and Bikini atolls in the Pacific for a few months to participate in what at the time was believed to be the first hydrogen bomb drop from an aircraft. We attempted to measure neutron doses at different distances from nuclear explosions, big and small. Some of them could vaporize an entire island in the atoll, others just left a small crater. To while away the time between work and snorkeling in the waters of the atolls, I had taken along a number of books, among them a popular astronomy book by Fred Hoyle. I no longer recall whether it was his *Frontiers in Astronomy* or *The Nature of the Universe*. Both books were out in paperback by that time, as were all the books I had taken along. Though Hoyle used no formulae and little technical language, I began to think that I would be able to do the calculations he was describing. It was quite fascinating.

At the end of my two years' service, I applied to graduate schools and was accepted by MIT, on the strengths of what must have been great recommendations from Leslie Jones, my supervisors at the Army Chemical Center, and one of the MIT professors the Army had sent me to consult.

At MIT, there was no course requirement in one's major subject. But for a minor, a student was required to pass three advanced courses. I signed up for an astrophysics minor. It is hard to believe, today, but in 1957 MIT had no astrophysics curriculum. However, an exchange arrangement with Harvard permitted me to take three graduate courses there.

Tommy Gold had just been given a Harvard professorship. At the time, he was postulating that dust on the Moon would hop around in response to electrostatic bombardment from the solar wind. Inspired by this, I begged and borrowed some equipment in the MIT Laboratory of Electronics, learned how to blow glass so I could construct a vacuum tube for bombarding dust with electrons, and then saw the dust disperse when I turned on the electron beam. Tommy came down to MIT to see this late one evening. He was delighted and asked whether I might like to switch to astrophysics after receiving my PhD. I had done some calculations in one of his seminars, and he thought I should post doc with Fred Hoyle. Of course, I was very pleased, though I still had my thesis work to complete.

I had come across the recently discovered Hanbury Brown / Twiss effect, and read the controversy surrounding it that aired in the journal *Nature* at the time. Edward M. Purcell's clean resolution of that controversy was particularly illuminating. I thought that the techniques developed for detecting the HB/T effect might provide a first opportunity to directly detect Bose Einstein fluctuations in electromagnetic radiation from a source in thermal equilibrium. None of the experimentalists in the MIT Physics Department were particularly interested in my making these measurements, but Prof. William P. Allis, a leading plasma theorist, said he'd be willing to supervise the thesis if I could find the means to build the requisite apparatus.

The Naval Supersonic Wind Tunnel located on the MIT campus at the time was run by Prof. John R. Markham of the MIT Aeronautical Engineering Department. One of the problems they were tackling was the detection of the hot exhausts of rockets and jet engines. This necessitated devices sensitive to the infrared radiation from these plumes. Improved sensitivity could be achieved by using not one detector, but two, and correlating their signals. This correlation technique was also needed for the Hanbury Brown / Twiss apparatus. The Aeronautical Engineering Department offered to buy as much of the requisite equipment as could be commercially obtained. They would use the apparatus during working hours, and I was free to use it for my thesis work at night. They generously also provided me with the assistantship I would need to finish my thesis work.

The fluctuations to be measured were minuscule, and detectors available at the time were still quite insensitive, but by April 1960 I had reasonably reliable results, and was finished with my thesis (Harwit 1960). Early in May, my wife Marianne and I embarked on the USS United States for me to spend a NATO sponsored postdoctoral fellowship year with Fred Hoyle in Cambridge. Four years earlier, I had been inspired by his popular writing. Now, I would be working with him. I hardly believed my good fortune!

When we arrived in Cambridge, Fred was away on one of his prolonged visits to Caltech, and I had time to finish a paper I had begun while still at MIT. I had found a small error in a paper on galaxy formation in a steady state universe by Dennis Sciama. When I redid the calculation, it showed quite clearly that there was no way that a steady state universe could form galaxies at the replenishment rate required by the expansion of the Universe, unless forces other than gravitation were at play.

I submitted the paper to the *Monthly Notices of the Royal Astronomical Society*, and some time later received an acceptance and an invitation to present the work at one of the monthly meetings in Burlington House (Harwit 1961). To my dismay, a week before my scheduled talk, I saw an announcement on one of the Cavendish Laboratory's bulletin boards that Hermann Bondi, one of the original creators of the steady state theory, was going to give a talk at Kings College, London, on precisely the same topic of steady state galaxy formation the week after my talk at the RAS. I had heard that Bondi was a fierce debater. As secretary of the RAS he would undoubtedly be present at my talk. I expected a punishing onslaught, and at once began to prepare myself by reading everything Bondi had ever written on related subjects.

On the day of the meeting, I gave my talk, sparred with Bondi, but felt that I had acquitted myself reasonably. At the end of the session, I approached Bondi and introduced myself. At his suggestion we went to eat a hamburger and chat for a while before he had to take the train home to Sussex and I returned to Cambridge. He told me he had been the referee on my paper which had suggested some further work to him. Would I have time to come to Kings College the following week to hear his talk? I was delighted, of course.

Munching on our hamburgers that evening after my RAS talk, I remember us talking about the future. I mentioned that on my return to the United States, I hoped to set up equipment to carry out infrared astronomical observations. Nobody was active in that area, and yet it seemed highly promising for astrochemical studies with infrared spectrometers.

After a great year in Cambridge, working with Fred Hoyle after his return from Caltech and writing a few papers with him, I returned to the US, to take up an NSF postdoctoral fellowship, this time at Cornell University where Tommy Gold had invited me to come. He had just moved to Cornell to start a powerful new department.

After my fellowship year, I accepted a one-year assistant professorship at Cornell, at the end of which I was free to take a leave of absence. I knew I wanted to carry out infrared astronomical observations and felt that ultimately infrared spectroscopy would offer great insights. But the Earth's atmosphere absorbed much of the infrared spectrum and, even worse, glowed strongly in the infrared. To obtain a clear view of the sky in this wavelength band, I knew I would need to take telescopes above the atmosphere; moreover, these telescopes would have to be cooled to cryogenic temperatures. Otherwise the glow from the telescope would be far stronger than any celestial signal. At MIT I had built sensitive, cooled infrared apparatus. At Michigan, in Sutherland's laboratory, I had gained experience with spectroscopy, and in Leslie Jones's group, I had learned how to build apparatus carried aloft in rockets. All I needed to do was to put all this together.

At Tommy Gold's suggestion, I visited Herbert Friedman of the U.S. Naval Research Laboratory (NRL) in Washington, early in 1963, to propose the possibility of starting an infrared astronomy program using rocket-borne telescopes. NRL had impressive credentials in ultraviolet and X-ray observations from rockets, but had not ventured into the infrared.

Friedman was very receptive. In a friendly meeting held in his offices, we agreed that I would come to work at NRL in the fall of 1963 and stay for a year, with fellowship support from the National Science Foundation. During this year, I would set up a group of NRL scientists and engineers to conduct a program in rocket infrared astronomy. At the end of the year, I would return to Cornell to set up a similar program there, and the two research groups established in this way would thereafter continue to compete in the newborn field.

During the summer of 1963, I sought to clarify the steps we would take. It was clear from the start that we needed to keep our efforts simple; the telescopes would have to be small. Our first efforts would have to be broadband photometry; spectroscopy would have to be delayed until we had more experience with the far simpler photometry. But even with these limitations, we thought we should be able to obtain reasonable measurements of large-scale features and an isotropic background. For background observations, a small telescope would suffice as long as it had a high throughput, i.e., it maximized the product of telescope aperture and angular beam dimension on the sky.

The background radiation I hoped we would observe was radiation I thought should have been emitted in the conversion of hydrogen into helium over the eons. Even though I had written two papers, while in Cambridge, to show the difficulties the steady state theory had in accounting for galaxy formation, I still thought that all the helium now observed must have been produced in stars. Like most astrophysicists at the time, I was unaware of the pioneering work of Ralph Alpher and Robert Herman (1948). Unfortunately, most of it had been largely ignored, forgotten or discounted.

In 1963 the helium content of the Universe was known to account for approximately one quarter of all the atomic mass in the Universe. If the conversion of hydrogen into helium had all taken place in stars, then some of the energy liberated in the process should be observable in the infrared. I no longer recall why I thought the observation was feasible, but this was an easy calculation, and there were so many things like that "in the air" at the time. There just weren't very many astrophysicists then interested in cosmological questions, and many of these thoughts simply remained unpublished, though knowledgeable people were aware of them and exchanged ideas about them over tea or coffee. These were quick ideas that were not sufficiently substantive to warrant publication. They were somehow too obvious.

With thoughts about the accumulation of starlight in mind I presented a paper at a colloquium held at the University of Liège in late June, 1963. In the proceedings of the conference I wrote (Harwit 1964)

> (A)n interesting infrared observation concerns the frequently discussed suggestion that the overall cosmic background radiation might amount to as much as 3×10^{-11} watt/cm^2 in the infrared ...

To this I added a cautionary note.

> (T)he cosmic flux could only be detected from the immediate vicinity of the Earth, if the radiation were concentrated in a very long wavelength spectral range where interplanetary dust grains are expected to emit inefficiently.

I showed that the thermal emission of the zodiacal (interplanetary) dust cloud would dominate the brightness of the infrared sky in the near- and mid-infrared part of the spectrum and wrote,

> One now is in a position to discuss the detrimental effects that zodiacal dust reradiation will have on infrared astronomical observations ... (T)he nature of the most promising infrared observations is different from much of the work in the visible region. One often hopes to obtain information about diffuse sources of radiation, so that the zodiacal foreground glow may be an important hindrance ... At $42\,\mu$ this cloud would radiate of the order of 4×10^{-13} watt/cm^2-sterad-μ at large elongation angles within the plane of the ecliptic.

Even today, four decades later, the zodiacal glow remains an obstacle to determining the true extragalactic background in the near- and mid-infrared. We may ultimately have to rely on tera electron volt TeV observations of distant active galactic nuclei to determine the rate at which this gamma radiation is destroyed through electron-positron pair formation, as it transits through the cosmic infrared background in extragalactic space.

The Cornell/NRL collaboration started in earnest in September 1963. A large number of technical problems had to be overcome in just twelve months if the work of the first year was to culminate in demonstrable success. NRL provided major resources to the effort. Joining me were scientists Douglas McNutt, Kandiah Shivanandan, and Blair Zajac, mechanical engineer Henry C. Kondracki, and electronic engineer John M. Reece.

Though the ultimate goal of the group was to construct telescopes cooled to liquid helium temperatures which would offer unencumbered observations across the entire spectral range from 1μ out to several hundred microns, we quickly realized that the design of a liquid nitrogen cooled telescope would be considerably more simple. Such a telescope, though not as cold, would still make possible near-infrared observations of great sensitivity, since a telescope cooled to the temperature of liquid nitrogen, $\sim 80\,\mathrm{K}$, would emit negligible thermal radiation at short wavelengths, and the near-infrared detectors in any case should operate optimally at this temperature. Once sufficient experience in the construction of these near-infrared telescopes was gained, we intended to quickly turn to the technically more difficult task of constructing liquid-helium-cooled telescopes that could be operated at temperatures of $4\,\mathrm{K}$ with the helium at atmospheric pressure, or $\sim 2\,\mathrm{K}$ if the helium was pumped down to very low pressure.

Many of the first launches were failures. Today, rocket launches have a better track record. But in the mid-1960s, failures of small sounding rockets to de-spin, pointing mechanisms to correctly orient the payload, delayed launches, and other problems often led to dismaying setbacks. Our efforts, like those of many others, were plagued by these difficulties.

I returned to Cornell University in the fall of 1964, whereupon Douglas McNutt took over the direction of the NRL group. We continued to collaborate on efforts that had been jointly started, but as these were completed, the two groups began to work independently and compete.

Shortly after my return to Ithaca, discussions with Dr. Nancy Roman, in charge of the astrophysics program at the National Aeronautics and Space Administration (NASA) resulted in grants to Cornell of an initial sum of $250,000 and annual budgets of $100,000, sufficient support to conduct a viable research program, initially with Aerobee 150, and later with the larger Aerobee 170 rockets. While NASA provided this initial outlay, we also obtained funding from the Air Force Cambridge Research Laboratories (AFCRL).

At Cornell I hired Henry C. Kondracki, who left NRL to move to the Ithaca, NY area as full-time mechanical engineer. William Wernsing, an electrical engineer in Ithaca, also joined the group, as did Jim Dunston, a local jack-of-all-trades technician. James R. Houck, a graduate student just finishing a PhD at Cornell in solid state physics joined our small group after a couple of years. He was soon asked to join the Cornell faculty and the two of us established a long-lasting collaboration.

Constructing a liquid-helium-cooled telescope turned out to be a major engineering effort. A cryogenically cooled telescope had to be launched under vacuum. Otherwise, atmospheric gases would immediately condense on the optics. But vacuum vessels at that time tended to be constructed with thick steel walls making them far too massive to be launched on small rockets. A sufficiently light-weight design was needed. The thermal/mechanical design problem of constructing such a telescope, which could survive the vibrations and linear accelerations of launch, and yet have minimal heat-conduction paths to the outer shell at room temperature, was difficult to solve.

Since the sensitivity of cryogenically-cooled detectors in a cryogenically-cooled telescope would be extremely high, observations were possible at high speeds. The bolometers favored by many ground-based observers were too slow to take advantage of this speed. Some of the photoconductors that had been developed for military purposes were far more promising. But it was soon apparent, that the very low radiative background that a fully cooled telescope provided, minimized the photon flux on these detectors, and correspondingly lowered the conductivity of the detector material. The detectors then attained extremely high resistances ranging up to $10^{11}\,\Omega$. Even small capacitive effects would then produce unacceptably slow response times. A major effort had to be undertaken to decrease response times and take full advantage of the detectors' potential sensitivity and speed.

The Earth's surface brightness in the infrared was expected to be nine orders of magnitude higher than the basic signals the detectors were able to detect from their $\sim 1^\circ$ fields of view on the night sky. Extreme care had to be taken to baffle the telescope to eliminate any stray light from the Earth's limb that might be scattered or diffracted into the telescope.

It took us five years, and a succession of failures, before we were able to produce a successfully working liquid-helium-cooled astronomical telescope. Early designs incorporated a parabolic primary mirror with 18 cm aperture and focal ratio f/0.9. At altitude the entire telescope, except for the entrance aperture, was surrounded by liquid helium. We flew three different types of detectors on these flights, copper-doped germanium, gallium-doped germanium, and n-type indium-arsenide hot-electron bolometers to cover progressively longer wavelengths between $5\,\mu$ and 1.6 mm (Harwit, Houck and Fuhrmann 1969).

We had, of course, been aware of Penzias and Wilson's 1965 discovery of the microwave background radiation. Its stunning cosmological implications were widely discussed. This was truly exciting work and we were eager to check it out. But, it was not until 1968-9 that our liquid-helium-cooled telescopes began to reliably work and we were able to attempt the detection of the expected submillimeter component of a background flux at $\sim 3\,$K.

Even with a well-working telescope, we encountered difficulties in background radiation measurements. We had not expected rocket exhausts and other gaseous and particulate ejecta to accompany the payload to great heights to form a diffuse, radiating cloud surrounding the telescope. These ejecta produced a false signal with all the characteristics of an isotropic flux at the longest wavelengths, $400\,\mu$ - 1.3 mm. My colleagues and I initially reported these signals as possibly of cosmic origin (Shivanandan, Houck and Harwit 1969). However, as we took increasing care to ascertain the origin of this flux, we realized that this radiation was not astronomical but was due to contaminants and to diffracted Earth shine.

The first successes of our rocket flights involved two quite different types of detections, and resulted from separate flights on December 2, 1970, and half a year later, on July 16, 1971. The first of these discovered and accurately measured the infrared radiation emitted by the circumsolar zodiacal dust cloud (Soifer, Houck and Harwit, 1971). We detected radiation in three spectral ranges, at 5 - 6, 12 - 14, and 16 - $23\,\mu$. At 70 to $130\,\mu$ we could initially only place an upper limit. More careful analysis provided a detection even at these long wavelengths (Pipher 1971). Both the three- and four-color photometry put the dust temperature at $\sim 280\,\mathrm{K}$. My greatest surprise in these findings was that the dust radiated significantly more powerfully than I had predicted (Harwit 1964), indicating that the zodiacal dust grains were unexpectedly dark, scattering only a small fraction of the incident light, absorbing and re-emitting an appreciably larger portion. Thirty years later, I was pleased to see that the far more comprehensive COBE results of Kelsall *et al* (1998) showed good agreement with the surface brightness of the zodiacal dust our rocket instrument had recorded.

The second discovery, made with the Cornell liquid-helium-cooled telescope on July 16, 1971, was the magnitude of the total infrared flux emanating from the Galactic Center and four other regions in central portions of the Milky Way at 5, 13, 20 and 100 μ (Houck, Soifer, Pipher and Harwit 1971). The 85 - 115 μ integrated flux over an area of $3^\circ \times 2^\circ$ around the Galactic center was $7 \times 10^{-20}\,\mathrm{W\ m^{-2}\,Hz^{-1}}$, in excellent agreement with the balloon borne result that had previously been obtained by Hoffmann, Frederick and Emery (1971). Excellent agreement for this wavelength range was also obtained for the Galactic ionized hydrogen regions Messier 8 and NGC 6357. But the Cornell rocket flight also recorded the previously inaccessible flux from these three regions at 5 - 6, 12 - 14, and 16 - 23 μ (Soifer, Pipher, and Houck 1972). Additional results cited by the same authors from an earlier flight, provided the 100μ flux for NGC 1499, a region previously unobserved at this wavelength.

More than a dozen years later, scans of the Galactic Center were also undertaken by the Infrared Astronomical Satellite, IRAS (Gautier *et al.* 1984). Though these authors did not compare their results to any previous work, the 100 μ maps of the Galactic Center published by the IRAS team gave peak fluxes which, within normal calibration uncertainties, were essentially identical to the Cornell rocket results published a dozen years earlier. Within such uncertainties, the 12 μ IRAS fluxes and the 12 - 14 μ Cornell detections also showed reasonable agreement.

Even though our paper on the background measurements at 400 μm - 1.3mm was laced with cautionary comments, it gathered wide-spread attention. As we became convinced that the signals were actually due to contamination we withdrew the results but, for the next thirty years, I continued to feel badly about this mistake. Not until Jean-Loup Puget and his group derived the far-infrared flux from COBE scans did I begin to feel relieved (Puget et al. 1996). If the correct analysis of the various cosmic background components had taken more than another quarter of a century and $\sim$ \$500M, roughly five hundred times more money than we had spent in the course of our entire rocket program, it was perhaps not so shameful to have been wrong. Sometimes it may be better to try difficult observations and fail, than not to try at all.

Kandiah Shivanandan

to come...

Rainer Weiss: CMBR Research at MIT Shortly After the Discovery — is there a Blackbody Peak?

Rai Weiss has been at the Massachusetts Institute of Technology since 1950, and is now emeritus professor of physics. His recent research interest is the development of the gravitational wave observatory LIGO.

CMBR research at MIT began in Bernie Burke's radio astronomy group where shortly after the first Princeton (Dicke, Peebles Roll and Wilkinson 1965; Roll and Wilkinson 1996) and Bell Laboratory (Penzias and Wilson 1965) group papers were published a measurement of the CMBR spectrum was made at 32.5 GHz (Ewing 1967). Bernie suggested that my Gravitation Research group try to make a measurement of the spectrum near the blackbody peak.

At the time the group had a program sponsored by the Joint Services in the Research Laboratory of Electronics in studies of the consequences of a scalar component of the gravitational field. The idea had originated with Bob Dicke as a way of incorporating Mach's principle in relativistic gravitation. We had started an active program to see if G, the Newtonian gravitation constant, was changing by as much as one part in 10^{10} per year. Dicke considered this a possibility if there were a scalar field. Prior to this Dirac (1938) had hypothesized a similar change based on the dimensionless scaling of large numbers in nature. Our program consisted of measuring g, the gravitational field of the Earth at its surface, with a new type of absolute gravimeter, good enough to see changes of this magnitude over a year. The gravimeter involved measuring the electric field needed to support a plate against gravity by using the Stark effect in a molecular beam that passed between the plates. Associated with the g-measurement was a need to establish whether the shape of the Earth is significantly changing with time. To enable these measurements we had begun a program of absolute laser frequency stabilization, again using molecular beam techniques. The lasers would illuminate interferometers that measured the strain in local patches of the Earth's surface as a means to establish if the Earth's radius was changing. Altogether it was a somewhat fanciful program which luckily had several spin-offs which were successful while the main effort proved too difficult.

One of the spin-offs was the application of the frequency-stabilized lasers to other high precision measurements. Gerald Blum and I (Blum and Weiss 1967) made a Michelson interferometer to repeat a measurement of the tired photon hypothesis that had been formulated as an alternative explanation of the cosmological redshift. The experiment had originally been carried out with Lee Grodzins (Weiss and Grodzins 1962) using the Mössbauer effect. In that experiment, we passed 14 keV gamma rays through a heated tube emitting blackbody radiation at 1000 K to see if the blackbody radiation caused a frequency shift of the gamma rays.

The concept of this photon-photon scattering experiment came from Finlay-Freundlich's (1954) observation that bright stars seemed to have larger redshifts of their spectral lines than dimmer ones. From this somewhat half-baked observation, Finlay-Freundlich, in typical grandiose astrophysics style, had extended the idea to being an explanation of the cosmological red shift observed by Hubble. The ideas of the Steady State cosmology and its "perfect" cosmological principle asserting homogeneity in time were so powerfully held at MIT at the time that any idea that would provide the redshift other than dynamically was most welcome. In this incarnation of the hypothesis, the photon-photon scattering was to have occurred between the ambient starlight and the light from the observed galaxy exhibiting the cosmological redshift.

We saw nothing in the first experiment. Then after the discovery of the 3 K CMBR, the notion became that the sea of microwave radiation would be the scatterers. That became the motivation for trying the experiment again, this time with microwave photons and visible light. That brought energies closer to those of the actual cosmological situation. Blum and I placed an X-band (3-cm wavelength) microwave cavity in one arm of the interferometer in which we turned the microwave power on and off at a frequency we would look for in the shift of the visible fringes. The interferometer itself was held on a fixed point on the fringe against slow drifts by a servo system with gain at low frequencies and none at the modulation frequency. At the frequency of modulation of the microwaves we were able to detect about 10^{-9} optical radians $\mathrm{Hz}^{-1/2}$, limited by the quantum fluctuations in the phase of the laser light. Again even with a much higher sensitivity than the first experiment and with more relevant photon energies, nothing was seen. The experiment did lead to the idea of detecting gravitational waves by laser interferometery, as in the LIGO project, but that is another story.

At about the time of the photon scattering experiment, I was asked to teach the graduate course in general relativity theory. As was typical in those more callous days, the teaching assignment was made several days before the beginning of the term. I may have been dumb enough in some earlier polling of the faculty to check off an interest in teaching such a course, but I never seriously meant it. Now, with this assignment and with a laboratory dedicated to the study of gravitation, it would seem inappropriate to say that I didn't really know any general relativity. I had taken a course given by Bob

Dicke, with his particular take on GR, and also listened to an idiosyncratic version given by Eugene Wigner, during my postdoc stay at Princeton. But I did not know the subject, most critically I did not understand the tensor calculus and the Riemannian geometry. That was a remarkable term, where I would lock myself into a room with many references and try to understand what I was about to teach. Although I had control of the curriculum, and exploited teaching about the experiments and observations, there came a time when the mathematics had to be tackled. I found Brillouin's book on *Tensors in Elasticity and Relativity* (Brillouin 1964) a good place to learn.

The reason for telling this is that in the class was a very smart student, Dirk Muehlner, who had started at MIT in infrared solid state physics with Clive Perry. Dirk was toying with the idea of going into gravitation or astrophysics. When in the course it came time to explain a gravitational wave and how one might consider detecting one, I gave a homework problem: explore the idea of using laser interferometery as a means of measuring the geodesic deviation induced by gravitational waves. Is there any chance that laser interferometry might be sensitive enough to make a detection? Dirk got quite interested in the idea but then when the course got to using general relativity in cosmology that became even more fascinating for him. It was just about at this time that Bernie Burke made his suggestion to measure the spectrum of the CMBR near the peak. The suggestion came with some real help in that Alan Barrett, who was also one of the leaders of the MIT radio astronomy group, had run a ballooning program to measure atmospheric emission. There was ballooning experience in the group and also the offer of borrowing some critical equipment, in particular, a multi-channel portable instrumentation tape recorder.

Dirk and I decided that it would be interesting to measure the spectrum of the CBR near the peak. Dirk's first research task was to look into the atmospheric absorption (and emission) at 30 to 300 GHz. This range of frequencies would attach to the existing measurements and would break new ground by observing at frequencies past the 3 K blackbody peak. Dirk's first findings from his library visits were that things looked pretty grim. It was clear that the three major atmospheric constituents that would cause trouble were molecular oxygen, water and ozone. Oxygen has a strong magnetic electron and rotation spectrum in the 50 GHz region. Water lines are everywhere starting at frequencies above 20 GHz (the cause of the famous K-band fiasco from WWII) but becoming really awful above 90 GHz with a killer line at 420 GHz. Finally, ozone rotational lines are sprinkled throughout the spectral region but are weaker than the water lines because of the smaller electric dipole moment and larger partition fraction. Once seeing

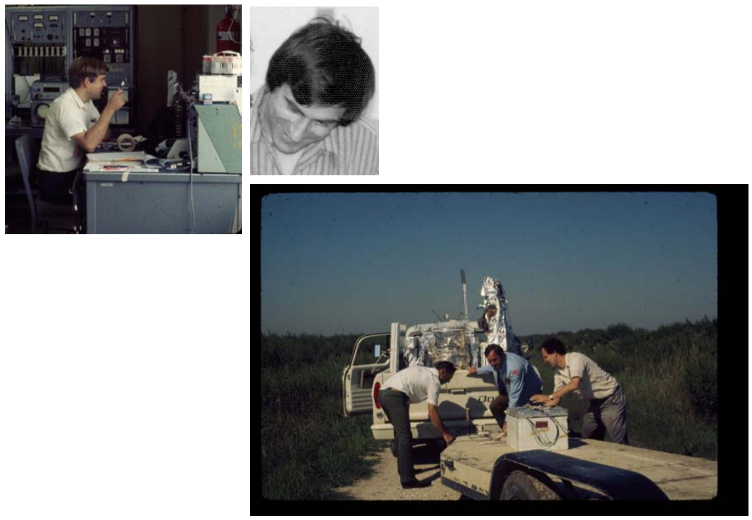

Figure 29: The MIT group, from the left Dirk Muehlner in the Palestine control room in 1971, Richard Benford in the MIT laboratory, and Rainer Weiss with the recovery crew in 1971.

the line structure, we knew that a measurement near the peak required getting above the bulk of the atmosphere. A satellite measurement would have been ideal but clearly was not yet in the cards. Ballooning and sub-orbital rocketry were the only options. We chose ballooning in part because of Alan Barrett's offer but also because we felt that there was just not enough time or real estate in a sounding rocket to do the observation properly. The region around the 3 K peak could, we felt, still be done with some atmosphere in the line of sight.

To both of our amazement, Dirk found articles in the geophysics literature that flatly stated that the atmosphere was getting wetter as one got higher (Grantham 1966, Mastenbrook 1966). There were papers that puzzled about this and were as baffled as we since such a possibility required a source outside the Earth or some complicated reservoir in the stratosphere. We got quite deep into this and began to look hard at the observations. When one said that the atmosphere is getting wetter what was meant is that even though the atmosphere was obeying its exponential dependence of the pressure (and density) with altitude, the fraction of the atmosphere that was water was growing as one got higher. The number of water molecules per unit volume as a function of height was not following the exponential decay with altitude. We smelled a rat.

The Naval Research Laboratory was the principal source of the information from a program that had become routine (always dangerous, but excusable since the main purpose of the program was to search for radioactive fallout from USSR nuclear explosions). They were sampling the atmosphere as a function of altitude by placing an evacuated can in a rocket and then opening the can to the atmosphere while at altitude. After taking the sample the can was sealed and, once back on the ground, was shipped to a mass spectrometer. It was clear to us that the procedure was troublesome when the sample had a low pressure, for then the water adsorbed on the walls of the can during the initial evacuation played an ever-increasing role, until at the highest altitudes the adsorbed water constituted almost all the water in the sample. It was no miracle then that, as one got higher and higher in the atmosphere, the measurements implied that the water concentration was growing relative to the ever reducing density of the atmosphere. We eventually went to visit the NRL and our suspicion was confirmed.

In planning our measurements Dirk and I made the assumption that the fractional water concentration remained constant once in the stratosphere. Even with this assumption, however, it was very clear to us from the absorption and emission calculations for the different lines of the atmospheric constituents, that we would need to go to the highest altitudes attainable with balloons. We also realized that zenith angle scanning to correct for the atmospheric emission, the technique used at lower frequencies from the ground where the atmospheric lines are not saturated, would require a mixture of theory and observation. That is because some of the lines, in particular the water lines, are saturated even at the highest altitudes we could attain with the balloon. Armed with this primitive knowledge of the atmospheric constituents, and a beautiful atlas of the atmospheric line frequencies, strengths and line broadening parameters kept as the Air Force Cambridge Research Laboratory Absorption Line Parameters Compilation, on tape, we made a strategy for the wavelength bands of the observations.

We also realized that measuring the CMBR at and above the peak, to really establish that the spectrum turned over, needed some profound changes from the way the previous ground-based measurements had been carried out. The most serious problem comes from the fact that there is a peak in the spectrum, which simply reduces the amount of power per frequency band. It arises from the vengeance of the quantum theory. The number of modes of the radiation field keeps growing with the square of the frequency. The photon occupation number per mode is kT at low frequencies, as demanded by equipartition in classical statistical physics. This gives the Rayleigh-Jeans part of the spectrum. But the mode occupation number becomes a dying exponential, with exponent $h\nu/kT$, as the frequency increases above the peak. Not that this is bad in its own right, for after all these are the ingredients that produce a peak in the spectrum. The trouble comes from the sources of radiation coming into the beam that are at higher temperatures than 3 K. These warmer sources still are growing in strength unrelentingly with frequency, their blackbody peaks occurring at much higher frequencies. At frequencies below the blackbody peak the contribution of a 3 K and, say, a 300 K blackbody are in the ratio of the temperatures, not great but only a factor of a 100. At the frequency of the 3 K peak (180 GHz), the ratio of the contributions is about 600, while at a frequency of twice the peak (360 GHz) the ratio has become about 5000.

This dramatic ratio is compounded by the fact that the emissivity of metals and many dielectrics increases with frequency, and as mentioned before the atmospheric emission increases with increasing frequency as well. Another factor in the worsening situation is that scattering also grows as frequency in the ratio of the scale of the surface disturbances to the observation wavelength. The scattering from shield- and beam-forming optics becomes important because it can bring in radiation from hot surfaces outside the beam. The only physical optics phenomenon in favor of measurements at the higher frequencies is the diffraction that occurs where the beam size times the beam divergence angle is roughly equal to the wavelength. In particular, one can use smaller aperture optics at higher frequencies.

Given these unpleasant facts Dirk and I made several design decisions to assure as best as we knew that we had things under control. We decided to place the instrument in a large open-mouthed dewar to allow enough room for the beam to be formed by cryogenic optics and not brush against warmer edges. The radiometer itself was placed in a sealed copper can within the outer dewar. The outer dewar was the source of cooling and expendable helium (Fig. 31). To assure that the critical beam-forming optics did not contribute to the radiation measured, we placed all of it in the can filled with liquid helium, which at altitude became superfluid. The superfluid is not only a good thermal conductor but also has a low dielectric constant and is a highly transparent medium. Although the superfluid has no viscosity, there is a critical velocity in the fluid at which vortices form and one begins to experience the transition to a fluid with micro-turbulence, and eventually, after cross-coupling of vortex lines, into a viscid classical fluid. The fluid dynamics set the highest modulation frequency of the mechanical chopper in the beam. Another aspect, not usually encountered in normal fluids, is the 15% change in the density of the fluid with temperature. The instrument can (Fig. 30) is initially filled with normal liquid helium at 4.2 K at atmospheric

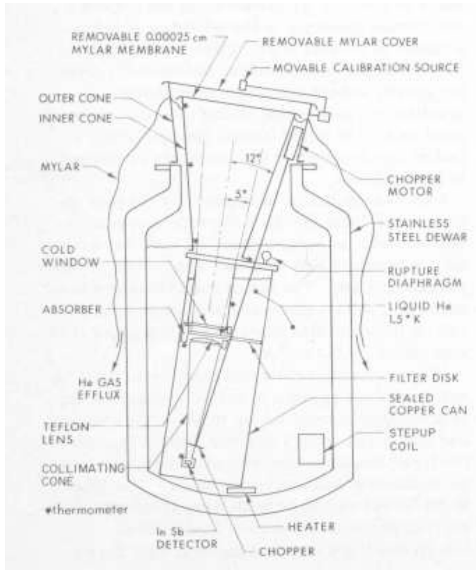

Figure 30: Schematic of the radiometer in the flight dewar. The initial radiometer and the second one, shown here, were similar.

pressure. In our design we let the helium pump down to pressure equilibrium with the ambient atmosphere as the balloon rose. At altitude 39 km the pressure is 2.5 mm and the temperature is 1.4 K, the helium has made the transition from normal to superfluid at the temperature of 2.1 K, the lambda point, and the fluid density has changed from 0.125 to 0.145 g cm^{-3}. To accommodate the density change and still assure that the entrance window of the radiometer can would be in contact with the liquid helium, the window was recessed by 8 cm in the 50 cm can.

Aside from the cryogenic operation the radiometer was a standard system. A set of capacitive and inductive mesh filters defined the band pass. The filters, made of silver patterns evaporated onto polyethylene, were multi-elements fused together thermally. The filters were particularly important in defining the high frequency roll-off (to avoid leakage of light, including the sun). The roll-off was provided by a glass fiber-laced plastic window (Fluorogold) which also served as a cryogenic gasket material. The primary difference between our various flights was in the number and bandpass of the filters. The first flights had three filters while subsequent flights had six, one being a metallic reflector that completely blocked the incoming light. (However, this filter reflected the radiation generated by the detector, an unexpected radiative contribution to be discussed later.) The filters could

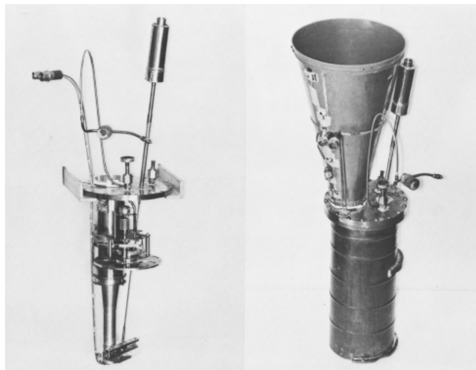

Figure 31: Left: photograph of the radiometer showing the filter transport and chopper; right: the sealed copper can with the radiometer installed.

be changed on command.

The collimator to fill the detector was a cone-lens combination which matched the beam hitting the detector area with a solid angle of close to π to the beam on the sky with an angle of 5 degrees. The cone and lens were in the sealed can. At the bottom of the cone was a smooth disk Plexiglas chopper wheel with aluminum-evaporated sectors to bring the chopping frequency to 330 Hz, high enough to lie above the inevitable 1/f noise of the detector and preamplifier electronics. The chopper was driven by a long thin shaft with periodic bearings from a motor on the outside. The chopper could induce microphonics at the rotation frequency, but was not a serious source at the modulation frequency. Finally, the detector was a 5 mm on a side by 1 mm thick piece of Indium Antimonide which was immersed in the liquid helium. The detection mechanism was the small change in electron mobility in the semiconductor when the electrons absorbed the millimeter wave radiation. The resistance of the material, measured continuously by a small bias current, became smaller as the electrons acquired more kinetic energy from the incoming radiation. The primary resistivity comes from electron coulomb scattering by ionic impurities in the material, where the deflection angle on scattering is reduced the greater the velocity of the electron. The time constant for equilibration of the electron gas to the temperature of the lattice, after it is excited by absorbing radiation, is approximately a microsecond. Although we understood the detection mechanism, we did not fully appreciate the systematic errors the radiation produced by the hot electron gas could make to our estimate of the incoming radiation. In principle, the calibration, if done with a truly non-reflecting load, would eliminate the problem. This is what we used at the end, but these calibrations were done on the ground and only relative gain-sensing calibrations were done in flight. The filter position containing the metal sheet helped in measuring this self-generated radiation by the detector.

The final element of the radiometer using the cryogenics was the amplifying system that converted the tiny voltages developed across the detector (fractions of nanovolts) to amplitudes more easily measured with room-temperature electronics. The low impedance of the InSb detector suggested that we use a passive step-up system to match into the noise of JFET (Junction Field Effect Transistor) amplifiers operated at about 60 K, as cold as possible to reduce (Johnson) thermal noise but still high enough to avoid freeze-out of the charge carriers in the semiconductors. We employed a copper coil inductor and capacitor resonant circuit at the chopping frequency to make the impedance transformation and accomplish the voltage step-up. The Q of the circuit was over 50, taking advantage of the reduction in resistance of copper at lower temperature. The coil, a possible source of noise, was surrounded by a superconducting lead shield and was potted with mineral oil to avoid relative motion between turns and the winding cores, which could be a source of microphonics.

The remaining parts of the instrumentation were more conventional but still fussy. Dirk and I had not previously constructed equipment that could survive the significant accelerations when landing, nor was the conventional lab practice to gain reliability really good enough. Systems had to work unattended and not fail at altitudes where convective cooling no longer applied since the atmosphere had such low density. We learned that on the scale of both costs and care a balloon payload was about 30 times more expensive and difficult than mounting something in the lab. Later I learned with the COBE satellite that carrying out an experiment in space was another factor of 100 times more costly and difficult. (I say this despite Werner von Braun's advertisement that space research would become as easy as ballooning. He actually said this at a committee meeting I attended in the mid 1970s while trying to convince the nation of the values of the space shuttle).

A vignette of the first ballooning campaign I will never forget occurred because of a youthful and rash decision we made not more than several weeks before the package was to be shipped to the Balloon Base in Palestine Texas. Both Dirk and I had become aware of the new integrated circuit operational amplifiers that had just come on the market. The chip was a 709 and looked like a little cockroach with 8 legs. The chance to save enormously on the battery capacity and on the real estate required for the electronics was sufficiently seductive that we embarked on a solid three-day campaign to replace the discrete transistor electronics with these integrated circuits. After this almost complete overhaul of the electronics, the electronics boards passed our altitude tests and we decided, just to be sure, to make a final full system test with the radiometer at liquid helium temperature. To our horror the system worked but had a new and disturbing radiative offset. By luck we happened to have the FM radio in the lab on and noticed that the chopping frequency could be heard almost anywhere on the FM dial. We eventually traced the problem to powerful oscillations between 50 to several 100 MHz generated by the new integrated circuits. The circuits had been compensated with the filters recommended but were oscillating at frequencies past the ability of our instrumentation to detect them. The radiation was getting into the radiometer via the leads and then being detected by the InSb. In the nick of time another integrated circuit chip with internal compensation and therefore no high frequency oscillation, the 741, became available. These operational amplifiers started a long and still active tradition of having the same functional connections on the pins to make these circuits readily interchangeable. Electronics has never been same again.

Calibration of the instrument was accomplished by inserting a horn of known temperature and emissivity into the full beam of the radiometer. The voltage developed by the detector as a function of calibrator temperature was measured for each filter. The curves were consistent with integrals of the black bodies of different temperatures over the instrument response. This was not important if all that was needed was a voltage associated with each temperature blackbody. It became important when trying to solve for the absolute atmospheric contributions from the elevation scanning when some lines were saturated while others remained unsaturated. One needed to know the detector volts generated per watt of incoming radiation at each wavelength, especially those at atmospheric lines. The absolute calibration was so fundamental to the measurement that we thought hard about performing it in flight but in the end found this so complicated and prone to failure that we resorted to a secondary calibration with a small blackbody source that could be brought into the beam by command. In the end such a calibration was only useful to measure an overall sensitivity and we had to trust the apparatus to not change transmission or spectral character. Later the COBE satellite, and Gush Halpern & Wishnow (1990) in their rocket borne observation, deployed in-flight absolute calibration.

The trickiest bit of the experiment design was at the interface between the cold world of the radiometer and the warm world we all live in. Indeed, it was the place that got us into trouble. Several functions needed to be satisfied. On the ground the atmosphere had to be separated from the cold

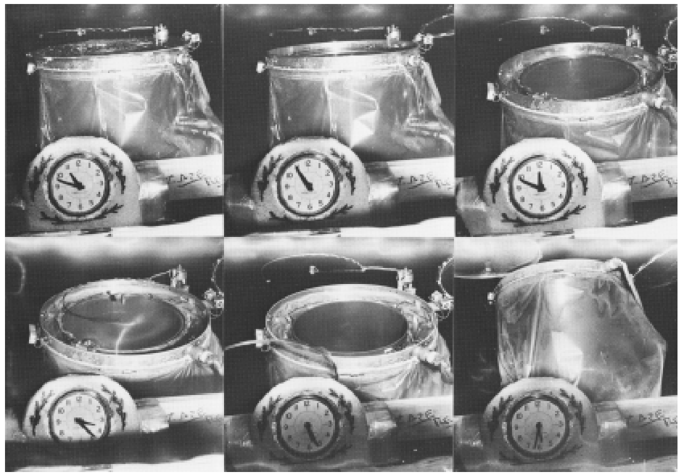

Figure 32: A sequence of photographs taken during the June 1971 flight showing operation of the various covers and bags at altitude. In the top left panel both hoop covers are still in place. The secondary calibrator is visible. Note the debris on the outer hoop cover. In the top middle panel the outer hoop cover has been removed and now rests on the right. In the top right panel the dewar has been moved to bring the radiometer beam closer to the zenith. The inner hoop cover appears to be clean. In the bottom left panel the secondary calibrator has been brought into the beam. Now, about 4 1/2 hours after the outer hoop cover was removed, water frost is clearly visible on the inner hoop cover. The radiation from this water frost can be received by the instrument. In the bottom middle panel the inner hoop cover has been removed and the flow of helium gas is keeping air out of the cone. In later flights, the inner hoop was removed near the beginning of the time at altitude because we found that the helium efflux was sufficient to keep the radiometer input aperture clear of air and water. In the bottom right panel the zenith angle has been increased to make an atmospheric scan. The picture was taken to check whether the hoops flopped around – they didn't.

part of the radiometer to prevent the catastrophe of air falling into the liquid helium-cooled surface and freezing there. At the same time we wanted to observe the atmospheric emission as we gained altitude, in part to verify the model of the atmospheric radiation as well as to establish that the instrument was working. During the ascent from 760 torr to the pressure of around 2.5 torr at float, the helium in the outer part of the dewar was pumped down from 4.2 K to 1.4 K. We needed to have an opening to the atmosphere for the pumping, but the pressure of helium had to be high enough to keep air from back-diffusing into the dewar. Initially, we expected that we would need to heat the helium in the outer dewar to force enough evaporation, but it turned out there was enough efflux of gas due to the radiative load. The separation was done by tailoring a set of polyethylene bags. The bags allowed the helium to be pumped around the dewar circumference. At the top of the dewar, where the beam comes out to observe the sky, there were two hoops stretched with thin Mylar drum heads. The outer hoop sealed against a cowling which also clamped the outer bag. The outer hoop was intended to prevent debris from falling into the system. It was part of a helium-filled shield for the inner bag and hoop system. At altitude the outer hoop was removed, leaving only a shield cone coated with an insulator to reduce its emissivity at grazing angles. The second bag was attached to the outside of the shield cone. An inner hoop sealed the shield cone. The major part of the observations were to be made through the thin mylar of the inner hoop, then at the end of the flight that hoop was removed to determine the radiative contribution of the hoop itself. Several of the early flights were made this way until in one flight a piece of ballast (lead shot) punctured the inner hoop Mylar sheet and we discovered that it was possible to observe at altitude without any cover. That is because the helium efflux from the dewar, if allowed to emerge through the shield cone, was sufficient to purge the air from the radiometer and keep the radiometer clean. Figure 32 shows a set of photographs taken at altitude of the entire sequence of hoop removals and dewar motions to allow the camera to look into the radiometer and observe if there had been air condensation. We observed only a small amount of deposition of nitrogen on the inner hoop after several hours at altitude. In fact to reduce the radiation from this film it was advantageous to remove the inner hoop earlier in the flight.

It turns that that in the initial flights the inner hoop was not far enough away from the main radiometer beam edge. The hoop caused sufficient scattering of radiation from a warmer shield to produce a radiative contribution which appeared to be maximum in one of the filter channels. That indicated that there may be a distortion of the cosmic background spectrum at about 300 GHz. This was our principal blunder. In a way it was lucky that we had a free-fall with this instrument and that we had to rebuild. But I will say more about this later.

The very low levels of radiation that could be tolerated at altitude, and the desire to use zenith-scanning as a means of helping to remove the atmospheric contribution, necessitated special treatment in the ballooning art. A typical high-altitude balloon with payload capability of 500 kg is 100 meters in diameter at altitude. In the beginning, we expected that a balloon packed in powder, to facilitate release from being folded in a box, would carry significant amounts of water into the stratosphere and we would have a local pollution of water lines worse than those in the atmosphere. We also wanted to make elevation scans as close to the zenith as possible. Both of these factors drove us to put about 700 meters of nylon line between the payload and the bottom of the balloon. A flight train as long as this was impossible to launch because of low-level windshear near the Earth's surface. The strategy that the balloon base had developed with prior atmospheric constituent flights, which also needed to be a goodly distance from the balloon, was the use of a mechanical rope payout-reel that by air friction through propellers and capstan friction slowly released 700 meters of nylon line when a command was given after the launch had safely lifted the payload off the ground. The reel enabled the launch of such a complex flight train but also increased the risk of flight failure. We did find the balloon to be a source of water vapor, and the reel was necessary to accomplish our measurement.

Our first flight made in beginners' innocence and with beginners' luck was a complete technical success, but it produced a cosmological mystery by giving a result consistent with the ground-based measurements of the CMBR temperature in the low frequency channel, an excess in the middle channel that embraced the blackbody peak, and an interesting and useful upper limit in a band above the blackbody peak (Muehlner and Weiss 1970). This last result disagreed with large excesses measured by a rocket experiment of the Cornell and NRL groups (Shivanandan, Houck & Harwit 1968; Houck & Harwit 1969; Houck, Soifer, Harwit & Pipher 1972). There was no sensible way to reconcile the rocket excess with our measurements and it was clear that the rocket measurement was in error. Nevertheless, we too had an excess, smaller and now narrowed to a band between 180 to 360 GHz.

The result was so important that we decided to fly the same payload again with some small refurbishments, just enough to fix the damage caused on landing. The same flight techniques were used but this time things did not go so well. The apparatus worked but just as we reached altitude an errant command was given which terminated the flight by cutting the long line at the bottom of the balloon. Once we realized what had happened many commands (all unsuccessful) were given to unpack a parachute on the payload, but to no avail. The instrument package went into free-fall over an East Texas forest. It not found for several weeks, until deer hunting season opened in early November. We had gone home before the package was found and Dirk went back to Texas to gather the pieces. It wasn't worth it: the package had not been designed to deal with an impact with the ground at 250 miles an hour. Not only were all our home-built cryogenics and electronics destroyed. Worse still, the most expensive part of the payload, the instrumentation tape recorder which the MIT radio astronomy group

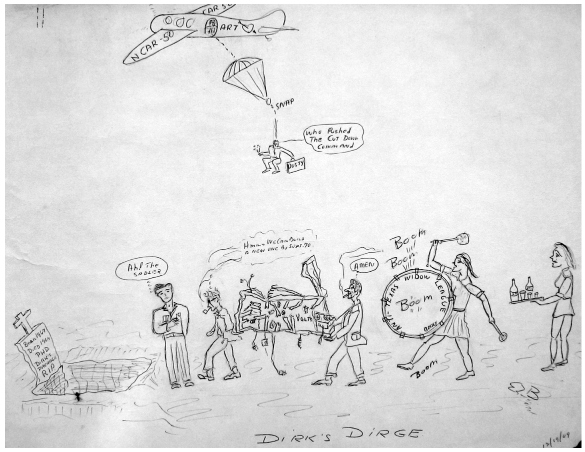

Figure 33: A sketch made by Frank O'Brien of MIT, who I asked to join us for the second flight of the original radiometer. This was the flight that free-fell after an erroneous termination command was given just as we were reaching altitude. Frank, an old friend from Zacharias's atomic beam laboratory at MIT, was an engineer and machinist who taught us the ropes in experimental work. With the free-fall he unfortunately did not have much to do and made this drawing which captured a good bit of what was going on. In the end, as is elaborated in the text, the free-fall was a mischievous gift.

had lent us, was in many pieces, a total loss. We did not have the funds to replace it. The reel, however, did return to the ground in one piece. It had a small parachute that was passively deployed in the flight line and the long line, luckily, did not prevent the parachute from opening.

Much as the free-fall was a rude introduction to the hazards of ballooning, it was also a gift to keep us from confirming an erroneous result. In the process of designing, constructing and testing a new radiometer and payload, we discovered that the excess in the mid band of the first observation was from radiation scattered by the edges of the inner cone and hoop that made up the interface between the cold and warm world. When we were making the second payload we had more powerful and higher frequency sources to make careful beam maps into the sidelobes. We found, not unexpectedly, that the beam profile was different for all spectral channels and, in fact, the spectral filter that was used in the mid channel caused sidelobes with peaks at the angles associated with the edge of the cone and the hoop.

It took about a year to complete and test the new package. Most of the ideas for the radiometer were still valid and many had been proved to work in the first flight. We did increase the number of filter positions to allow better definition of the cutoffs in the blackbody integrals and, later in the flight series, to try to pin down more definitively the various atmospheric contributions. We made incremental improvements in the detection sensitivity and did a much better job of mapping the sidelobes and shielding design of the instrument. Now burnt by experience, we horned in on the ballooning mechanics. We designed a more reliable reel. We took part in the design of the flight train, to avoid packed parachutes and any other command driven-procedure that affected the safety of the payload. A failure in the ballooning was now designed to result in a landing with a parachute irrespective of the failure of commands or noise triggering of the commands. In that year the NSF-sponsored NCAR Scientific Balloon Facility also upgraded its command and data communication electronics. This proved to be a significant step forward that substantially improved the reliability of the flights. In another big step forward Richard Benford, a technical jack of all trades who eventually became a full-fledged engineer, joined our little group. Now it was Dirk, Dick and Rai who went to Texas to enjoy the high life.[24]

We made five successful flights with the new payload in two years. Two were made at stratospheric wind-turnaround in the spring and fall, when the

[24] A comment on the location of the balloon base in Palestine Texas. The balloon campaigns always lasted longer than we told our wives and girlfriends. Even though it rarely took longer than two weeks to get the instrument and flight apparatus together it usually required between five to six weeks to get a flight off and recovered. I used to ponder how this could happen. Weather reigns supreme in the ballooning business. The wind on the ground had to be less than 10 knots to be able to launch and for difficult flight trains the winds needed to be even smaller. The low level winds, those at several 100 feet above the ground could not be large, for otherwise there was the possibility of wind shear that could destroy a balloon. The favored launch conditions were not into clouds so that one could follow the progress of the launch. There were also conditions required in the recovery area. The lack of clouds was important there so that a chase plane could see where the payload would hit the ground after termination of the flight, and high winds and rain were not good for a successful termination. It turns out that East Texas, in such close proximity to the Gulf of Mexico which spawns much of the humid and thunderstorm-bearing weather in the Southern United States, was a particularly bad choice for a Scientific Balloon Facility. Not only was it difficult to achieve the needed benign weather conditions but also a good look at the map would indicate that in the summer when the stratospheric winds travel from east to west direct trajectories out of Palestine to the west all had the unfortunate property of intersecting the Mexican border. We had no reciprocity agreements with the Mexicans so that balloons that threatened to encroach on the Mexcian border had to be terminated. Locating the National Balloon Facility north of Palestine, say in Oklahoma, would have offered significantly higher probability of obtaining good launch conditions and much less probability of a threatened over-flight of Mexico. The trouble was that Oklahoma was not in Texas when the facility was inaugurated during the Lyndon Johnson years. One of Lyndon's gifts to ballooning was a significant number of separations and divorces.

Figure 34: The launch layout for the first flight of the new payload. The package has been lifted by the launch vehicle ASCEND II. The payout reel with the 700 meters of line is on the cart between the launch vehicle and the balloon. At the time when the picture was taken the top of the balloon was being filled with helium. The lower part of the balloon is lying on a tarpaulin between the cart and the clamp that is holding the balloon down. Although they are not visible, there are separate parachutes in the flight train for the reel and the payload. Once the balloon is released the launch crew member with his foot on the cart will follow the path of the balloon as it lifts the reel off the cart. ASCEND II will chase the balloon around the field until it is a little down wind of the vehicle and then release the payload. If the launch master has been successful, the payload will go forward and upward rather than backwards and hit the vehicle. The launch ballet and the balloon dynamics are lovely to watch and much quieter than a spacecraft launch.

upper level winds change sign. This makes possible long flights, extending over a day or two, that remain in radio contact with Palestine Texas. These are highly sought launch times, usually reserved for research that requires long exposures such as cosmic ray detection or measurement of periodic events in the atmosphere. Our helium consumption was large enough to limit useful observations to about 14 hours, so we did not qualify for turnaround flights. Nevertheless, by being ready to go on 10 hours notice we managed to get a turnaround flight as another group had to scrub their flight because of apparatus failure. The long durations allowed us to test for systematics and to carefully measure the contribution to the detected radiation by emission from water vapor generated by the balloon. We could also make zenith scans to help measure the atmospheric emission. Several of the flights used the largest and lightest balloons then being manufactured, and we were able to explore the change in the atmospheric lines with altitude near our flight altitude. This helped in validating our atmospheric emission models. The flights also differed in the extent and elegance of the ground shielding. That helped reduce the possibility of contamination by the radiation from the atmosphere and the ground that fell into the radiometer sidelobes.

As we became more cavalier we began to improvise at the balloon base with new instrumentation that could help in understanding conditions at altitude. One of the most revealing innovations was the use of an old camera that was lying around. We fitted it with a heater and film advance motor to look at the top of the payload with flashbulb illumination. The sequence of pictures taken of the top of the system was timed by a Big Ben clock we had bought in the local drug store. We degreased it so it would work at the low temperatures at altitude. The sequence in Figure 32 shows all the hoop dynamics and the small amount of air frost deposited on the inner hoop. It also shows that there is no discernible deposition of the atmosphere in the radiometer mouth. This validated our experience that the helium efflux gas actually purged the air from entering the cold parts of the radiometer. One of the panels shows the secondary calibrator in position.

The results of these flights (Muehlner and Weiss 1973a) were published in *Physical Review* rather than *Physical Review Letters*. This was a significant mistake but we thought (at the time) it was justified because we felt that all aspects of the observation had to be explained and understood to make the measurement believable. *Physical Review Letters* offers rapid response but not much space, and we were not experienced enough to write our results in a convincing and short manner. To us the results were clear enough. The results of the initial flight were wrong: there is no excess near the peak. Our second set of measurements showed that there is a peak in the CMBR spectrum, the curve turning down at higher frequencies. There was no question that the Rayleigh-Jeans spectrum did not continue to high frequencies. We could reject the idea that the CMBR is produced by a diffuse dust shell around the Earth.[25]

[25]Early on in thinking about the wisdom and importance of taking on the measurement of the spectrum of the cosmic background, I consulted Philip Morrison (Phil), who had recently come to MIT from Cornell to work with Jerrold Zacharias and Francis Friedman on developing new curricula for science education at all levels. I made an appointment with Phil to talk with him in his office and came at the designated time. Phil waved me in with his head in a book and asked me to explain my visit while he continued to read. I explained what I considered the importance of measuring the spectrum near the blackbody peak and also some of the difficulties we would experience in the measurement.

There was still significant skepticism about our result from the people who had done the rocket observations. I remember well discussions with Martin Harwit about the atmospheric corrections that we had to make. Martin kept pointing out that our critical high-frequency data, which is important in determining that there is a peak in the CMBR spectrum, was not all that different from the rocket results if one did not make the atmospheric correction. Furthermore, because this region of the spectrum is dominated by water lines, the simple zenith-scanning technique, so useful for measurements on the ground at longer wavelengths where the lines are not saturated, simply does not work. One had to solve for the radiation from a column of gas with Van-Vleck – Weisskopf (almost Lorentizan) line profiles. The use of zenith-scanning measurements without additional calculation could give two limits. If the lines were fully saturated their radiative contribution would vary as the square root of the column density. An application of this case would over-correct the incoming radiation, leaving an apparently low CMBR contribution. In the other, unsaturated, limiting case the radiative contribution from the atmosphere would vary directly as the column density. Application of this case would undercorrect, leaving an apparently high CMBR contribution. One had to do it right by doing integrals. We did it right but the rocket people did not believe it. In part to make sure that stratospheric ozone was not playing tricks, we made a final flight with new filters to look specifically at more atmospheric lines (Muehlner and Weiss 1973b). which only confirmed our radiative modeling.

When Dirk and I wrote our long paper summarizing the results of the

I was not sure he was truly listening although it became clear that Phil was and that he was actually good at multiplexing his attention. His opinion was strongly expressed and very negative. He thought the CMBR was a mistake, that it was absurd to think that there could be a cosmic background that had equilibrated and that we in effect were embarking on a fools' errand. It was clear that the source of the radiation was something local, he thought possibly from a spherical shell of dust around the Earth. At the time he was still a strong proponent of the Steady State Theory of cosmology, which did not have an easy time with the CMBR. A few years later, after the results of the first balloon flights, I once again encountered Phil but by this time he had been through the conversion from skeptic to believer and he was just as strongly convinced that our result in the middle channel, showing an excess near the blackbody peak, was in error. So it goes with expert advice. Mind you, I have always been very fond of Phil and found him interesting to talk with and imaginative, it is just good that he was not in charge of funding or other serious matters. Several years later I helped him with demonstrations for a Nova program he called the "Whisper from Space." By that time we had made the series of flights with the new radiometer which gave results consistent with blackbody and we were good guys again. An enjoyable volume by which to experience Phil is a collection of essays and biographical sketches he wrote (Morrison 1995).

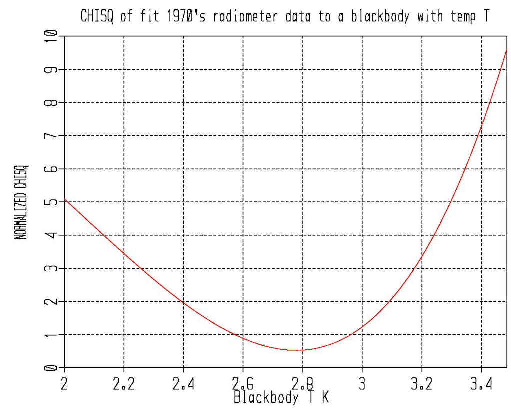

Figure 35: Reanalysis of the data from all five flights made with the second radiometer. The calculated contributions from atmospheric radiation were used. The technique was to assume a blackbody spectrum with the temperature (horizontal axis) as the fit parameter in a χ^2 minimization. The value of χ^2 reduced to the number of degrees of freedom is the vertical axis. The smallest χ^2 is at a fit temperature of 2.78 K. This analysis was done in the mid 1990s to convince Alan Guth that we had measured the turnover in the spectrum. We should have analysed the original data this way, but we didn't know enough.

second radiometer flights, we presented the results associated with the radiation power measured through each of the spectral responses separately, both with the atmospheric corrections and without. These data could be related directly to the absolute calibrations made on the ground. We also attempted to get another handle on the incoming radiation spectrum by taking differences between the channels. This turned out to be troublesome because uncertainties in the atmospheric contributions contributed systematic errors, and differences increase the noise. We did not do the sensible thing of assuming a blackbody spectrum with an unknown temperature and making a χ^2 (least squares) fit to the temperature using the power measured in each spectral response. (Only after teaching experimental technique and data reduction in an undergraduate course did I become aware of the power of the χ^2 minimization).

Several years ago when Alan Guth was writing his book on the history of inflation (Guth 1997) he asked if those early balloon flights really had shown that the CMBR spectrum is blackbody. That led me to apply the χ^2 test to our data. The result is shown in Figure 35. The minimum of χ^2 is at 2.78 K. The value measured by COBE (Mather *et al.* 1990) and by Gush, Halpern & Wishnow (1990) is 2.726 K. At the time of our measurements the best estimate of the temperature was 2.72 ± 0.08 K (Peebles 1971, p 141). Since our measurements probed shorter wavelengths this demonstrated that the CBMR spectrum is close to blackbody over the peak.

By the time we had finished writing up the results of the second radiometer spectrum measurements it was clear that the next step would be to use a Fourier transform spectrometer. This would help eliminate the atmospheric contributions since the water (but not the ozone) would show up as narrow intense lines that could easily be removed from the smoother spectrum of the CMBR. Two groups, one at Berkeley (Mather 1974) and another at Queen Mary College in the UK (Beckman *et al.* 1972) were already quite far advanced in preparing instruments for balloon flights. Herb Gush at the University of British Columbia in Vancouver was preparing a rocket instrument also using a Fourier transform spectrometer. Dirk and I went on to measure the anisotropy of the CMBR near the blackbody peak in a long series of flights that discovered a lot of galactic dust and not much else. But that is still another story.

Yu Jer-tsang: Clusters and Superclusters of Galaxies

Jer Yu is Chief Information Officer at the City University of Hong Kong

I was a graduate student in the Physics Department of Princeton University from 1964 to 1969, and had the good fortune to work with Jim Peebles and David Wilkinson at the time of the discovery of the CMBR. I had the opportunity to observe at first hand the events that occurred during this exciting period, and to participate in some of the early work on the interpretation of this discovery.

I should begin my story with the decision I made in 1962 to go to the US to study. Although I was born in Shanghai, China, my family had moved to Hong Kong when I was seven. Thus, the first part of my education was mostly done in Hong Kong. In 1961, I was admitted by the University of Hong Kong into a programme in pure mathematics. Back then, Hong Kong was nowhere near the international metropolis it is today. The University was fine, but it somehow lacked the excitement and diversity that I was hoping for. I was not sure what I really wanted to do with my life, but I was restless and yearned to go abroad to broaden my horizon. I began to make applications for admission to a number of US universities, from a list that I compiled from the catalogues that I was able to find in the US Consulate. I was offered admission by several universities, and I chose the University of Michigan. UM has a long history of working with students from China, and enjoyed an excellent reputation in this part of the world.

I arrived in Ann Arbor as a transfer student in Engineering Physics in September 1962. Although I had started out not knowing anyone on campus, everyone that I met had been extremely kind and helpful. One of the persons I met was David Wilkinson, who was teaching a course I took. I cannot recall exactly how it happened, but soon I was working as a part-time assistant with Dave and his team in their experiment on nuclear magnetic resonance. This was my first ever paying job, and more importantly, the job gave me my first glimpse into what it was like doing research at the frontiers of science.

I completed my undergraduate degree at the University of Michigan and came to Princeton in September, 1964. Unlike when I first arrived in the US two years earlier, this time there was someone I knew on campus before I came — David Wilkinson. Indeed, Dave's presence on the faculty of the Physics Department might have something to do with my being accepted by Princeton.

Every graduate student in Physics in Princeton was given a research studentship on admission, and was allowed to choose the area in which to work. Naturally, I ended up working in the Gravity Group. There were a number of interesting projects going on to try to prove the theory of General Relativity, one of which was Dave's experiment to detect the cosmic background radiation. Work had already begun to design and build a Dicke radiometer for this purpose. I was helping to assemble the equipment in the basement and to take measurements on the roof top. This was when I had my first lessons in cosmology. It was also when I met Jim Peebles.

Even as a graduate assistant, I could feel the excitement and intensity that were going on inside the group. It seemed like some new ideas would be floated almost every other day on how the cosmic radiation could have an effect on our physical environment, and then plans were being put forward on how to observe such effects. News of the discovery of the radiation in 1965, if anything, had heightened the level of activities in the group.

In the mean time, I was really enjoying the good life of a Princeton graduate student. I had a reasonable stipend from my studentship that kept me free from any financial worries. The work in the Gravity Group was interesting and enlightening. Everyone was very willing to show me how things work and to teach me the theory behind it. The intellectual ambiance in the Physics Department was incomparable. I can recall the stimulating discussions in the weekly brown bag lunch seminar of the Gravity Group, the well presented departmental colloquiums by invited speakers from around the world, the fascinating lectures by the Princeton professors in any number of the courses that I was free to take, or just the daily gathering of faculty and students for tea in the lounge in Palmer Laboratory. Added to this was the comradeship of all my friends in the Graduate College, coming from all over the world and working on so many different disciplines.

I passed my general examination, which was required of all Physics graduate students, in the summer of 1966, and began to look for a topic for my thesis work. Since I knew that I did not have the knack for experimental work, I decided to do something theoretical. So one day, I walked up to Jim Peebles and asked him whether he would be my thesis advisor. Jim looked at me and said yes without the least hesitation. Thus, I had the honor of becoming Jim's first Ph.D. student.

Jim began to talk to me about homogeneity and isotropy in the Universe. He already had his theory of gravitational instability for the development of large-scale structures in the universe, and we began to look for observational evidence to support this theory. He gave me the two catalogues of clusters of galaxies, one compiled by George Abell (1958) and the other by Fritz Zwicky (Zwicky, Herzog and Wild, 1961 - 68), and asked me to study the data to see whether there was any higher level clustering of the clusters.

As a novice researcher, I began to read up feverishly on all the papers on the subject that I could find, some relevant and some not so relevant. I was keeping extensive notes on everything that I had read, and spending a lot of time in the library. However, I was not making much progress with my thesis, until one day, in one of my regular meetings with Jim, he finally said to me something to this effect: Jer, stop reading, start thinking. This one piece of advice had served me well for the rest of my life. You can always read about what other people think, but this is no substitute for your own thinking.

I had never done any observational astronomy myself and had not ever seen the galaxies and clusters (or the photographic plates on which they were recorded) in person. To me, the data were just x, y co-ordinates projected onto a spherical surface. I painstakingly transcribed the co-ordinates into 80 column punched cards so that I could feed them into what was, at that time, still a relatively new tool, a digital computer, for analysis. For the next 6 months, the half box of cards became one of my most precious possessions.

Princeton had one of the more advanced computers of the time (I believe it was an IBM 360/65) housed in the Engineering Quadrangle. I was making the trek between Palmer Lab and the Engineering Quad and submitting my deck of cards to the Computer Centre almost on a daily basis. I was putting the data through all kinds of permutations to determine whether there were any significant patterns. On a good day, I could get two or three runs per day. Initially, I was hoping to find an exact mathematical formulation which would allow me to integrate over the data and come up with a definitive yes or no answer. Unfortunately I was not able to achieve this. In the end, I had to resort to using simulation to create a number of possible distributions, and then to compare the observed distribution against the simulated ones. In this way, I was able to drawn some conclusions by inference (Yu 1968; Yu and Peebles 1969).

Another idea I had was to do a complete simulation of the evolution of the Universe by following the development of some initial density fluctuations through the different epochs, and if everything works according to the theory, should finally be able to see large scale objects emerge like bubbles in boiling water. Again, I was not able to completely solve the problem as I had formulated, probably because the computers in those days were not powerful enough to do what I wanted to do.

I left Princeton in 1969, and after spending one year as a post-doctoral research fellow at the Goddard Institute for Space Studies in New York City, I returned to Hong Kong in 1970. The University of Hong Kong was just then getting its first mainframe computer, and had hired me because of my computer knowledge to manage this installation. Although I have not continued to work in the field of cosmology after I left the US, the experience that I had in Princeton was most memorable and rewarding. After all, there are not that many people in the world who can claim that he was there on the spot when the cosmic microwave radiation was discovered.

Rainer K. Sachs: The Synergy of Mathematics and Physics

Ray Sachs is Professor Emeritus of Mathematics and Physics at the University of California, Berkeley. His analysis with Wolfe of the gravitational interaction of the CMBR with the departures from a homogeneous mass distribution is a central element in the cosmological tests. His current research interest is the mathematical modeling of radiobiological data.

In the 60s, Artie Wolfe and I thought that inhomogeneities in the early universe sufficient to cause the presently observed lumpiness would lead to anisotropies in the observed temperature of the CMBR. Photons from different directions, coming from, and passing through, different time-dependent fluctuations would give information on the nature of the fluctuations. We used linearized perturbations of a spatially flat, general relativistic Robertson-Walker model to analyze density, velocity, vorticity, and gravitational wave fluctuations; their influences on the CMBR, regarded as consisting of test-photons, were worked out with general-relativistic kinetic theory. We overestimated the size of the temperature anisotropies, but some of our ideas (Sachs and Wolfe 1967) were eventually supported by COBE and subsequent observations.

My most vivid recollection of the work concerns the interplay between mathematics and physics. Somehow, it seemed almost like an extension of the interplay between calculus and Newtonian dynamics in a freshman physics course.

The reader probably took such a course and may remember that it did not literally emphasize typical physical systems, whose methodical analysis usually requires additional background. Instead there were Newton's laws plus a magical zoo of idealized ropes, pulleys, weights, projectiles, billiard balls, levers, reaction forces, springs, pendulums, and (best of all) monkeys. Also, we learned how calculus really works, and what an immensely powerful unifying force it is; formal calculus proofs were blessedly absent; intuitive proofs were wonderfully present. The experiments we did in lab were boring to me but the gedanken experiments we analyzed for homework were endlessly fascinating. In optional reading Milne's (1935) beautiful little book on cosmology fit right in, despite (or perhaps because of) the fact that some of his main ideas do not actually work for the real world.

Tensor analysis is no less capable of unifying than freshman calculus, and the geometric approach to general relativity is even more elegant than 3-d Cartesian vector algebra. For me, our CMBR anisotropy work was, much as in a freshman physics course, mainly an exercise in applying mathematics

Figure 36: A part of the history of the standard cosmology.

to a highly idealized situation. Some examples may illustrate the flavor of the arguments.

1. One question concerned history-based vs. law-based explanations in cosmology and in the rest of science. Biological evolution has a strong historical component, rather than being understandable in terms of equilibria or steady states resulting from general laws. We are what we are in good part because some 40 million years ago our lemur-like ancestors were what they were; to explain those proto-lemurs one must go still further back in time; etc. And in biological evolution we may (perhaps) be dealing with one unique process, so that our usual ideas of replicating experiments or assigning probabilities become obscure. Moreover, it seems pretty clear that chance played a significant role; Vice President Cheney is presumably not an inevitable consequence of basic natural laws.

Now is cosmology, contrasted for example with special relativistic quantum theory, also like that? My guess at the time was "yes." To explain lumps in the present universe we must go back to the lumps at the time when radiation and matter decoupled, which in turn can only be understood in terms of earlier lumps, and so on indefinitely. The modern idea, of including some initial conditions as part of basic physical law, could perhaps have saved me a lot of confusion but did not occur to me.

The two other atypical characteristics of biological evolution mentioned above also seemed to have probable counterparts in cosmology. Even more than in the case of biological evolution, the universe's history can probably best be understood as a unique process. Comparisons, probabilistic or otherwise, to possible alternate universes may be useful but probably have no fundamental significance.

Whether there is also anything accidental involved in this putatively unique history was puzzling, especially in those days, before inflation. The spacetime diagram in figure 36 represents part of the history of a perfect-fluid Robertson-Walker general relativistic model. Causality is emphasized, i.e. lightlike geodesics are at 45 degrees and only the conformal structure is considered. The world line represents part of the history of our galaxy and here-now is shown as a dot. The main point is that in such a figure the big bang typically turns out to be a 3-dimensional spacelike surface, not a point. Correspondingly there could be signals, as shown, coming from very near the big bang, which have since been traveling toward us at the speed of light and will eventually arrive with information (and other stuff) that is, for us now, new and unpredictable.

So I felt at the time that the evolution of the universe, like biological evolution, is a process where details of the past have dominant importance, is a one-of-a-kind process, and is in some sense subject to blind chance. That was worrisome, since most of the rest of physics is not like that at all. The main properties of a hydrogen atom result from laws, not its own history; there are many different hydrogen atoms; and blind chance plays no role in their basic structure.

2. The whole concept of "now" was likewise worrisome. The dot in the figure is indispensable for the discussion just given, and similar "now-dots" appear to be needed in many other arguments. But the mathematical analysis, which incorporates relativity of simultaneity, does not use any corresponding preferred events. In our analyses, we tried to think through essentially this point not only in considering what "blind chance" might mean but also when we ran across the following question. Suppose the observed microwave temperature in a given direction is a bit larger than in other directions. Can this be attributed to, for example, time-dependent density inhomogeneities, or could it just mean that in the indicated direction we are somehow managing to look a bit further out, and thus a bit further back in time to a hotter epoch of the universe? Eventually we did find a consistent, coordinate independent answer that does not implicitly invoke absolute simultaneity.

Quite generally, "now-dots" like the one in the figure are really not legitimate in relativity. You only need to draw a Minkowski spacetime diagram with a few timelike world lines in it to realize that no consistent, systematic assignment of now-dots is possible without in effect imposing some version of absolute simultaneity. Indeed a key part of the geometric approach to relativity is to consider a physical process as a unified history, past, present and future. Spacelike slices through a history are at best conveniences and are often more misleading than useful. It is this 4-d aspect that accounts for the truly extraordinary simplicity (leaving quantum phenomena apart) of the actual relativistic universe compared to a hypothetical universe governed by vintage 1900 physics. Fundamentally the latter is much more complex because it is less unified logically (with space and time being distinct things instead of two different aspects of the same thing, energy and momentum conservation being separate laws, etc., etc.).

My guesses about "now" were (and are) the following: (a) For the reasons just given, "now" can't be allowed into a relativistic theory, and it is really highly embarrassing for such theories that all of us very strongly believe now is somehow very different from the rest of our history. (b) Perhaps when two people talk there are (especially if one of the two has extra frequent flier miles) small discrepancies between their perceptions of "now." It is tempting to blame this fact for the style of conversation one hears in a Berkeley cafe. But airplane speeds are not that large; the potential discrepancies generated are considerably less than a microsecond, and thus pretty harmless. The discrepancies could easily, in a highly interactive community like the human race, be subordinated to an implicit agreement on some kind of ad-hoc consensus simultaneity. (c) In principle, however, there seems to be an important conflict between the impermissibility of "now-dots" and our overwhelming intuition that now is special. This conflict could be, as were Olbers' and Gibbs' paradoxes, an obscure signal of the need for some basic paradigm shift; if there is a simple resolution of the conflict I am not aware of it.

3. Some of the mathematical tools we used to analyze the behavior of photons were, truth to tell, motivated at least as much by formal analogies as by physics. I had learned from Jurgen Ehlers about the elegant way relativistic hydrodynamics treats fluid expansion, shear, and vorticity. The formal generalization to light beams was almost automatic; that approach eventually led to generalizations in terms of Liouville's theorem in an appropriate phase space, with which one can track the microwave photons as they come from and through distant matter inhomogeneities to us.

4. The main tool we used was likewise based in good part on a mathematical analogy – to first order time-dependent perturbation calculations in quantum theory. Specifically, we found that by linearizing the Einstein field equations around a Roberston-Walker perfect fluid spacetime (a method used earlier by Lifschitz 1946) one gets very instructive time-dependent solutions, identifiable parts of which (essentially normal modes) correspond to gravitational waves, to vorticity, or to density and velocity fluctuations. The way in which each relevant physical aspect had just the appropriate mathematical counterpart, and vice-versa, seemed very satisfying. The perturbation solutions, not being restricted by the artificial symmetry assumptions essential to get fully non-linear solutions explicitly, gave perspective on how inhomogeneities in the universe evolve in time and how they can influence

anisotropies of the observed CMBR.

In summary, to me the most interesting aspect of possible CMBR anisotropies was the way the processes involved illustrated a synergy between mathematics and physics – to paraphrase Einstein, one of the most incomprehensible things about the universe is that mathematics can help us comprehend it. I was thus actually less interested in the universe than in the methods used to analyze it. In retrospect that seems odd, but that is what happened.

Arthur M. Wolfe: CMBR Reminiscences

Art Wolfe is professor of physics and director of the Center for Astrophysics and Space Sciences at the University of California, San Diego. His research interest is galaxy formation, with particular attention to gas-rich galaxies observed at high redshift.

I was a graduate student in physics at the University of Texas (Austin) when the CMBR was discovered in 1965. Although most cosmological models at that time were based on the assumptions of homogeneity and isotropy, there was little empirical support for either assumption. I was working with Ray Sachs, my PhD thesis advisor, on devising techniques useful for placing quantitative limits on departures from homogeneity and isotropy. After all. the universe of galaxies is observed to be quite lumpy, and it was unclear to us whether the global smoothness of the models was consistent with observations. Ray had been attending lectures by the astronomer G. de Vaucouleurs who emphasized the presence of superclusters of galaxies on length scales which, though not generally accepted at the time (they are of course accepted today), might have implications for the large-scale structure of the universe. In fact de Vaucouleurs always adhered to a model with zero mean density and structures on ever increasing scales; that is, a universe that was highly inhomogeneous on all scales.

I think it is important to emphasize that cosmology in 1965 was not an empirically based branch of Physics. Besides the Hubble expansion, we knew very little else about the universe. While the competing steady state cosmology was running into difficulties with the steep slope of the radio-source counts, Hoyle and collaborators were extremely resourceful in finding plausible scenarios to explain these data. In retrospect, the quasar redshift distribution indicated an evolving universe that was inconsistent with the steady-state, but at the time the origin of the quasar redshifts was controversial, so this was not regarded as a definitive argument against the steady state model. As a result, the subject was in a state of flux with the big bang competing head-to-head with the steady state model. This was also reflected in the lack of good textbooks available for graduate students struggling to understand the field. Fortunately for me, I attended an excellent course on cosmology given by Engleburt Shucking. Unfortunately, most of the textbooks available at the time emphasized mathematical elegance at the expense of physics. A notable exception was the excellent monograph on cosmology by Bondi (1960a), one of the architects of the steady-state.

Prior to the discovery of the CMBR, the only way to assess the large-scale structure of the universe was through observations of low-redshift galaxies.

Ray Sachs and Jerry Kristian had computed a local power-series approach to this problem (Kristian and Sachs 1966). They showed how departures from pure expansion in the form of shear and vorticity could be inferred from measurements of galaxy shapes. But because this was a power series expansion around here and now, their model was valid only for redshifts $z \ll 1$. The results of this exercise were illuminating in that Ray and Jerry found that the observations were consistent with a shear as high as 20% of the expansion rate, and with an even larger value for the vorticity. As a result, because observations of galaxies in the 1960s were not sensitive measures of large-scale kinematics, the data were consistent with significant deviations from the widely accepted idea of pure expansion.

Detection of the CMBR by Penzias and Wilson (1965) changed everything. Nobody I knew had been thinking about the CMBR even though it had been predicted earlier by Gamow and collaborators. I first heard about the Penzias and Wilson discovery at a 1965 seminar given in Austin by Nick Woolf. Because the CMBR was not mentioned in any of the text books or courses I took, I had to learn about an entirely new field from the bottom up. Fortunately for me, Ray handed me a paper copy of the 1965 preprint (electronic preprints did not yet exist!) article by Jim Peebles on the "primeval fireball" and its implications for galaxy formation. This preprint was really a blueprint for Jim's research for many decades to come. It contained terminology such as "the last scattering surface", "Thomson drag", etc. which, though familiar now, were revolutionary concepts in 1965. Jim's article was very different from most of the previous literature in cosmology. It was filled with physical rather than purely mathematical ideas. It introduced me to the concept that the CMBR was a truly global radiation field. I was astounded when I first realized that its mean temperature was an average over the present spatial particle horizon.

About this time Ray became interested in using the CMBR as a tool to study large-scale structure. The idea was to perturb Friedmann models to first order and see what effect gravitational perturbations had on the CMBR. The main reference in the study of linear perturbation theory was the classic paper by Lifshitz (1946). While Lifshitz solved the problem for the full suite of Friedmann models, we focused on the Einstein-deSitter model: due to its mathematical simplicity the solutions to the perturbation equations could be expressed in terms of simple algebraic functions. This made it easier to compute light-like geodesics to first order. To calculate the effects of the perturbations on the CMBR temperature, we used the Liouville theorem for radiation to find that the present CMBR temperature in any direction is inversely proportional to $1+z$, where z is the redshift in the same direction. By computing the lightlike tangent vectors we were able to find first-order corrections to temperature in terms of an integral along our past light cone over functions of the perturbed metric and its time derivatives. Later in my thesis I repeated this calculation by solving the collisionless Boltzmann equation for radiation to first order and obtained the same answer. Our solution for the temperature fluctuations included contributions from vector and tensor terms, which are physically related to vorticity and gravitational radiation. We focused instead on the scalar terms because they contained a first-order gravitational potential that was a solution to the Poisson equation with density perturbations as the source. As a result we derived an expression in which the temperature perturbation $\delta T/T$ is proportional to the density perturbation $\delta\rho/\rho$.

To estimate $\delta T/T$ we assumed $\delta\rho/\rho \approx 10$ % on scales $d \sim 300$ to 1000 Mpc. In the 1960's little was known about the density structure of the universe on large scales. In retrospect we were influenced by de Vaucouleurs' claim of significant density structures on scales of hundreds of megaparsecs. In any case, we concluded that $\delta T/T \approx 0.005$.

Publication of our result (Sachs and Wolfe 1967) had a mixed reception. The major figures in the field of cosmology were very interested. During a trip to Moscow in 1971, Zel'dovich and Sunyaev told me how excited they were about our work. In the west, Peebles, Rees, and Silk turned their attention to the problem of temperature anisotropies. On the other hand, most astrophysicists showed little enthusiasm for this subject: forty years ago astrophysical research centered on topics such as stellar evolution and the physics of radio sources and quasars rather than the large-scale structure of the universe. A revival occurred in the 1980s with the advent of dark-matter cosmologies. In 1982 Peebles combined our formalism with his newly derived cold-dark-matter power spectrum to make a more realistic estimate of $\delta T/T$ of $\sim 10^{-5}$ on large angular scales (Peebles 1982). This was ultimately confirmed by the COBE satellite (Smoot 1992). The result was flood of interest in our work. While I cannot speak for Ray, I was both astonished and gratified by the amount of research our work has generated. At the time of its publication in 1967, neither of us had any idea about the impact it would have.

Joseph Silk: A Journey Through Time

Joe Silk is Savilian Professor of Astronomy, University of Oxford. He is an active contributor to physical cosmology and author of five books on the subject; the latest is Infinite Cosmos (2006)

I began my research career at a propitious time. Cosmology had been stuck in a rut for decades, but it was about to explode. I arrived at Harvard in 1964 as a beginning graduate student who was eager to become a cosmologist. This intention was nurtured by two events in my life. I had been studying for a mathematics degree at Cambridge, and was not overenthused by my lectures. I was completing Part 2 of the Mathematics Tripos, so named I was told because in earlier times, the students were examined individually by their professors, while precariously perched on a three-legged stool. I accidentally stumbled into a Part 3 course given by Dennis Sciama. I heard him lecture on Mach's Principle, Einstein and the origins of General Relativity. I was captivated. The universe may not have been rotating that day, but my head was certainly spinning from the new vistas that were opened on a wet Cambridge morning.

Leaving Cambridge behind, I went north to Manchester to continue my studies by enrolling in a fourth year course in physics. The next event occurred when I was studying in the library and getting progressively more and more bored. Perusing at random the pages of *the Astrophysical Journal*, I was impressed by the choice of the first article of each issue, invariably on cosmology. And one of these fascinated me further. The article in question applied the virial theorem to the universe and to the growth of structure (Layzer 1963). The very notion of a cosmic virial theorem captured my imagination. The author was a cosmologist on the faculty at Harvard. Many years later, his theorem was to form the core of an important cosmological probe for weighing the amount of dark matter in the universe. This required data, which did not then exist. So it was to theory that my attentions turned, and I set about getting a fellowship from ESRO, the research-orientated predecessor of the European Space Agency. I took the fellowship to Harvard to work with my idol, Professor David Layzer.

Layzer agreed to supervise my research on the topic of how galaxies formed in the expanding universe. I soon discovered that Layzer was an arch proponent of the cold big bang. It did not take me long to explore the possibilities of galaxy formation in an initially cold universe. Indeed, I found the outcome for galaxy formation was entirely satisfactory. However the issue of data soon posed a serious challenge. The cosmic microwave background radiation, the fossil radiation from the beginning of the universe, was discovered by Arno Penzias and Robert Wilson the year I started graduate school, 1964. The problem was that the cosmic microwave background radiation argued strongly for a hot big bang. The timing was truly optimal for a confrontation of theory and data.

But first, there were confrontations between the rival theorists. Many refused to accept the cosmological nature of the cosmic microwave background radiation. Local origins were strongly advocated, especially in a cold big bang. Relations between my Harvard supervisor and the leading proponent of the hot big bang, James Peebles at Princeton, were tense. News of the rivalry filtered down to the dark and dank basement office at Harvard College Observatory, where the graduate students were sheltered. I slowly migrated away from the theory of a cold big bang. My first paper struggled with Mach's principle in an unusual setting. Going back to my cosmological roots, I tackled the problem of galaxy formation in Godel's rotating universe. But this research direction seemed to have little future. Nor for that matter did the concept of a cold big bang.

To his immense credit, Layzer was remarkably open-minded and encouraged me even when I eventually became disillusioned with his increasingly baroque attempts to incorporate the newly discovered cosmic microwave background radiation into the context of a cold universe. The CMBR did seem to be most simply interpreted as the fossil blackbody radiation from a primordial thermal fireball.

I spent part of the summer of 1965 at a summer school on the Cornell University campus in upstate New York, organised by the American Mathematical Society. Cosmology was at the transition between a branch of general relativity and one of astronomy. The theme was the rapidly emerging subject of what would now be called physical cosmology. My fellow students included Jim Gunn, Bruce Peterson and Arthur Wolfe, all to subsequently leave their marks in cosmology via the eponymous effects associated respectively with tracers of the ionisation history of the universe in quasar spectra, and the large angular scale fluctuations in the cosmic microwave background radiation that are associated with the observed large-scale inhomogeneity of the universe (Gunn and Peterson 1965; Sachs and Wolfe 1967).

During the following summer of 1966, I was employed as a research assistant at American Science and Engineering, an MIT spin-off company started by Bruno Rossi and Riccardo Giacconi that had recently launched an X-ray satellite to search for fluorescence X-rays from the Moon. As often happens in science, the serendipitous discoveries of the first X-ray source Scorpio X-1 and the diffuse X-ray background overshadowed the initial goal.

My summer brief was to develop a theory for the X-ray background. This radiation had to be of cosmic, indeed of extragalactic, origin as a consequence of its observed isotropy on the sky. The X-ray background was relatively uniform, and so had to be produced by many distant galaxies. One could speculate freely, on the basis of one known galactic X-ray source! This was how how I developed a taste for studying diffuse backgrounds, a topic that was ripe for investigation and was to play a central role in much of my future research.

At this point in time, I almost became an observer. Harvard in those days required its budding theorists to undertake an observational project. Armed with the approximate coordinates of a new x-ray source in the constellation of Cygnus, I spent many cold nights that winter at Harvard's Agassiz Observatory in Harvard, Massachusetts. My mission was to use the 36 inch reflector to photograph the star field several times per night at the location of the X-ray source. I would develop the plates myself, then the following day I would bury myself in the depths of the Harvard College Observatory, huddling over a blink comparator device to search for short time-scale variability. The recently discovered Scorpio X-1 counterpart, a bright blue star, varied and flickered on timescales of nights and perhaps even hours, and the logic went, so should Cygnus X-1. There was a theoretical argument, based on the scaling with the ratio of X-ray to optical luminosity of Scorpio X-1, that suggested one should be seeing a variable, blue, 12th magnitude star.

Of course, my mission failed, and I was eventually scooped by professional observers who had the advantage of clear skies, the world's largest telescopes, and most importantly, extensive experience. Theoretical prejudice was found, not for the first or last time, to be detrimental to the observer's health. By way of consolation, I was not alone in being led astray: Alan Sandage, who had previously identified Sco X-1 as a flickering 13th magnitude blue star, was searching for an 18th magnitude counterpart to Cyg X-1 (Giaccnoi *et al.* 1967)!

Cygnus X-1 turned out to be the brightest star (9th magnitude) at the centre of my plates. It was even a previously catalogued star, HD 226868. My plates were certainly well-centered, but somehow I missed the variations. In fact, the images I took were mostly in terrible seeing, trailed and out of focus. Little surprise that I could hardly compete with the experienced astronomers on the Mt. Palomar 200 inch telescope.

I was increasingly frustrated from my attempts at astronomical observations, and felt that I most likely suffered from a version of the Pauli Principle: whatever could go wrong in an experiment that I undertook did go wrong, with even my proximity seemingly having a malign influence on the outcome and even the functioning of the experiment. So I resolved to become a theorist. My doctoral thesis was to be entitled *The Formation of the Galaxies*. But I still had to write it.

Meanwhile, the debate on the interpretation of the cosmic microwave background intensified. This was still at a time when the steady state universe had a vocal band of supporters. Much of the debate came to a climax at the second conference I attended, in early 1967 at the Goddard Institute for Space Studies in New York. This for historical reasons was the *Third Texas Symposium on Relativistic Astrophysics*, following earlier meetings in the series at Dallas and Austin. Those were heady days. Quasars as superstars highlighted the first Texas meeting in 1963, but their true distance and nature was still being hotly debated. What stole the show for me however was the question of the origin and nature of the cosmic microwave background radiation. I even recall encountering George Gamow surrounded by a small crowd and declaiming in his curiously high pitched voice that he had lost a penny, Penzias and Wilson had found a penny, and was it his penny?

Despite the new developments in cosmology being pioneered by Jim Peebles and that further developed the theory of the hot big bang, I found no better solution to understanding the origin of galaxies until in the summer of 1967 I found myself at Woods Hole, Massachusetts. The occasion was my enrollment as a student in the Woods Hole Oceanographic Institute Summer School. Traditionally, WHOI held an annual summer school on applications of fluid dynamics. That year, the chosen field was astronomy. The topic was astrophysical fluid dynamics and I was fortunate to hear lectures by such luminaries as Richard Michie and Ed Spiegel. But my true inspiration came from George Field, who lectured on Galaxy Formation. The summer project that I chose under Field's direction was to incorporate the newly discovered cosmic fossil radiation into galaxy formation theory. I was inspired, and worked day and night. I studied the coupling of the matter and radiation in the early universe., and in particular, the transition from optically thick to thin regimes at a redshift of 1000. I used the adiabatic mode of density fluctuations, described by sound waves in the baryon-photon plasma prior to matter-radiation decoupling, to evaluate the associated radiation density fluctuations. Within a few months, I had produced my first paper on this topic, presciently entitled Fluctuations in the Primordial Fireball (Silk 1967). To form the galaxies, the initial density fluctuations must have had a finite amplitude, that left a potentially observable trace in the CMBR via the acoustic imprint in the temperature fluctuations on sub-degree angular scales.

There was one initial hiccup. I was almost scooped again, so I felt when I first saw the paper by Sachs and Wolfe (1967) that appeared later that same year. But my spirits lifted when I realised that their predictions of large angular scale fluctuations were based on an extrapolation of the observed large-scale irregularity of the universe. This was an observation with no accompanying theoretical explanation. The irregularity was seen in the observed large-scale structure of the galaxy distribution, but did not have to be there. By studying the coupling and growth of primordial density irregularities, the temperature fluctuation strength could be predicted. It was a phenomenological prediction.

My predictions, on the contrary, were focussed on the theory of galaxy formation. This, after all, was the title of my doctoral thesis. But how was one to test such a theory, in the era before the advent of the very large telescopes and the space telescopes? The solution came from the prediction of small angular scale temperature fluctuations. These provided a crucial missing link in the connection between the initial conditions and the formation of the galaxies. The irregularities arose from a fundamental theoretical argument. The fluctuation strength could be predicted via the requirement that galaxies must have formed by the gravitational instability of tiny density fluctuations whose amplitude was calculated from the theory laid down in the pioneering paper of Lifschitz (1946). Fluctuations grew in strength via the effects of gravity in the expanding universe. Without such fluctuations there would be no galaxies. Not that it was particularly clear at the time, or indeed for decades later to anybody beyond a select handful of cosmologists, but I had come up with a theoretical prediction that was fundamental to our understanding of the big bang as a cosmological model of the observed universe. In fact, my results were entirely complementary to those of Sachs and Wolfe, who had concentrated on the superhorizon scales where the primordial density ripples are imprinted. I studied the interaction of matter and radiation on subhorizon scales, where the physics of acoustic waves modifies the primordial fluctuation spectrum, and boosts its amplitude. I evaluated the characteristic angular scale of the fluctuations that seeded galaxies and galaxy clusters. My predictions were further refined a year later (Silk 1968) when I evaluated the minimum scale of surviving adiabatic density fluctuations due to the coupling with the radiation field. There was a corresponding minimum angular scale above which the temperature fluctuations could survive and be detectable.

Of course, history had the last word in 1992 when the Cosmic Background Explorer satellite (COBE) verified, to within a factor of two, the Sachs-Wolfe prediction on angular scales in excess of 7 degrees (Smoot *et al.* 1992). It was to take almost another decade before the fine angular scale anisotropy predictions on sub-degree scales were confirmed.

The idea was straightforward. If galaxies formed by gravitational instability from primordial infinitesimal density fluctuations, the inferred radiation dominance of the early universe meant that they had to have a finite amplitude given the limited time available in the matter-dominated regime for fluctuation growth. So I predicted, initially very naively, that the required amplitude of temperature fluctuations on angular scales of tens of arc-minutes or less in order to form large-scale structure such as clusters of galaxies had to be about 3 parts in 10000. The fluctuations could not be any smaller, otherwise the galaxies and galaxy clusters would not have had time to form. The argument was remarkably simple. The growth factor since last scattering was 1000 in a flat, matter-dominated universe. Hence the initial density fluctuations to form clusters by today had to be of order 0.1 percent. The temperature fluctuations were correspondingly of order a third of this, for the adiabatic mode in which density is proportional to the cube of the temperature. One byproduct of the calculations was the damping of the fluctuations as the last scattering surface was traversed. The angular scale associated with the so-called last scattering surface denotes the angle subtended by the transverse projection of the finite time of recombination, converted to a comoving length scale. Below the minimum angular scale of a few arc minutes, where the damping sets in, one would not expect much in the way of primordial temperature fluctuations in the cosmic microwave background radiation.

Over the next two decades, there was a very small and select group of theorists who pursued and refined these calculations, pioneered by Peebles, Sunyaev, Zel'dovich and collaborators. One early critique was that the adiabatic density fluctuations would be erased by the finite thickness of the last scattering surface and that velocity modes would predominate. This turned out not to be the case once a more sophisticated treatment of fluctuations was developed. The notion of acoustic peaks in the matter was developed in a classic paper by Peebles and Yu (1970), and independently proposed that same year by Sunyaev and Zel'dovich (1970), and in the radiation intensity by Doroskevich, Sunyaev and Zel'dovich (1978). The latter paper improved on the earlier discussion by Sunyaev and Zel'dovich in 1970, but was itself later substantially corrected and refined in the first rigorous treatment of the subject by Silk and Wilson (Silk and Wilson 1981; Wilson and Silk 1981; Wilson 1983). I had taken up a faculty position at Berkeley in 1970, and with my student Michael Wilson, I developed the first modern relativistic treatment of temperature fluctuations by solving the coupled Boltzmann and Einstein equations in a curved background. From now on, one could hope,

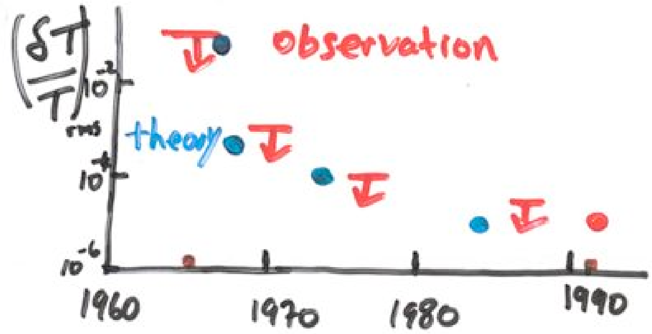

Figure 37: Upper limits on the fractional CMBR temperature anisotropy $\delta T/T$, demonstrating that theory remained ahead of the observations for some 3 decades. From a transparency dating from 1992.

at least in principle, to measure the curvature of the universe by studying a map of the sky.

Of course, the ultimate verification was to take a long time and painstaking effort. There were several generations of cosmic microwave background radiation experiments. A prolonged period followed when the improved experimental limits were above the progressively refined theory (figure 37). Each time there was a major experimental improvement, as happened with the pioneering attempts of Partridge and Wilkinson (1967), then of Uson and Wilkinson (1981), the theoretical hurdle was raised with the advent of more precise calculations. Until the mid-1980s, the select band of theorists who worked on the CMBR were lonely voices in the wilderness. I distinctly recall at one conference during this period how Geoff Burbidge labelled us the "background brigade", arguing confidently that the absence of detectable temperature fluctuations proved that the gravitational instability theory of structure formation was wrong, and thereby cast doubt on the big bang itself.

The final theoretical refinements came with the introduction of cold dark matter. The large-scale CMBR anisotropy in the CDM model was computed by Peebles (1982), and the small-scale anisotropy was computed independently by myself and Nicola Vittorio at Berkeley (Vittorio and Silk 1984) and by Dick Bond and George Efstathiou at Cambridge (Bond and Efstathiou 1984). Nor was it long before the cosmological constant was probed via these predictions (Vittorio and Silk 1985). The weakly interacting cold dark matter allowed fluctuations to grow despite the tight baryon-photon coupling once the universe was matter-dominated. The prediction of temperature fluctuations arising from structure formation was now an order of magnitude or so lower than the early predictions, 3 parts in 100000 at the first acoustic peak at an angular scale of about 30 arc-minutes, and substantially lower on smaller angular scales where the damping played a role. It was to take another 5 or 6 years before a ground-based experiment (TOCO) and the balloon-borne experiments (BOOMERANG, MAXIMA) provided strong confirmation of the elusive signal.

Nor even then was the solution completely definitive. Refined data were needed for the next step. This was the prediction that one could measure the curvature of the universe (Sugiyama and Silk 1994) in the sky. It turns out that in the cosmic microwave background alone, there are significant parameter degeneracies (Efstathiou & Bond 1999). Indeed, the simple addition of a Hubble constant as measured by the Hubble Space Telescope key project (72 km $\sec^{-1}$ Mpc^{-1}) leads to the highly significant inference that the universe has close to zero spatial curvature. This result was greeted with joy by many theorists who regarded it as a prediction of inflationary cosmology. I personally am less convinced by the predictive power of inflation, recalling the equally vocal band of inflationary theorists in the 1990s who welcomed the low density, spatially curved, universe then favoured by observational cosmologists with suitably tuned inflationary models.

However, while one can always find inflationary models to explain whatever phenomenon is represented by the flavour of the month, it is certainly true that the generic predictions, associated with the vast majority of the models of inflation on the market, have had two immense successes. One of these is the verification of the flatness of space. Another stems from an achievement of the 3-year data from WMAP, which has succeeded in eliminating one of the rival hypotheses to inflation, the Harrison-Zel'dovich prediction of the scale-invariant nature of the primordial density fluctuations. This asserts that the spectral index of the scalar fluctuation power spectrum $n_s = 1.0$, on the basis of simple but compelling scaling arguments. However this is one situation where simplicity has to be abandoned when confronted with reality. The new result from the WMAP satellite (in 2006) is that $n_s = 0.95 \pm 0.02$. This is expected as a consequence of the finite duration of inflation with smaller and smaller fluctuations exiting the horizon later and later as inflation peters out and the fluctuation distribution gradually rolls over in power.

Nowadays, cosmology seems rather boring. All measurements converge on the standard cosmological model with hypothesised ingredients of dark matter and dark energy that are themselves poorly understood. It requires immense hubris to be confident that we have found the final solution, given our woefully inadequate mastery of the first instants of the big bang The ultimate theory of cosmology will surely include our standard cosmological model as a component.

R. Bruce Partridge: Early Days of the Primeval Fireball

Bruce Partridge is a cosmologist turned radio astronomer who has taught at Haverford College for 36 years. He spent 5 years, 1965-70, in the fabled Gravity Group at Princeton working on the "Primeval Fireball" (the CMBR) and primeval galaxies. He also served six years as the Education Officer of the American Astronomical Society, and even survived 8 years as an academic administrator at Haverford.

I will start as I propose to continue, in a quite personal and even anecdotal tone. I'll begin with my interest in astronomy, awakened in my teen years by building two reflecting telescopes with my father. In my college years, I bounced back and forth between history, physics and astronomy. In retrospect, I can see that these were pointing towards my eventual fascination with the evolution of the universe and how we can determine it. Physics ended up as my major, but I got some grounding in astronomy as an undergraduate. In the early 1960s, Princeton University was just developing an undergraduate astronomy track. To gain admission, one had to take an elementary astronomy course designed primarily for the dimmest of football players. We used a text coauthored by the professor, a text that mentioned the word "universe" only twice, both times misidentifying it with the Milky Way galaxy. Fortunately, my subsequent courses were with George Field, a master teacher as well as a visionary astronomer. In 1960 I took from him a course that dealt in part with cosmology; that section of the course was based on Herman Bondi's (1960a) thin book, *Cosmology.* Bondi's book was a fair representation of the state of cosmology at the time: attention was focused on cosmological models and possible observational tests of them. The largest scientific question in the field was whether the steady state model fit the (meager) data better than what we now call big bang models.

It is perhaps a mark of how small a dent cosmology made on me that I elected to do research with George Field in the areas of interstellar grains and radio astronomy instead. But my main focus in my last year at Princeton and thereafter at Oxford was in quantum physics (my Oxford D.Phil. was on optical pumping in helium gas). Nevertheless, fascination with large-scale questions in astronomy was ticking away in the background. I recall attending, in 1964, a meeting of the Royal Astronomical Society to hear about the newly discovered phenomenon of quasars. It was at that meeting, incidentally, that I first encountered Dennis Sciama, and noted both his wonderful ability to explain scientific principles clearly and his collegial treatment of a very young Stephen Hawking.

So, when it came time to apply for postdoctoral positions, I looked to groups in both Britain and the USA that were bringing techniques of physics to bear on astronomical or cosmological questions. My Princeton background led me to send an application to Bob Dicke. Bob's invitation to join the fabled "Gravity Group" was the crucial event in my scientific career.

As a 25 year old with a scant knowledge of cosmology, I walked into Bob Dicke's office in the late summer of 1965. I knew of Bob's ongoing work on the Eötvös experiment, but his enthusiasm in 1965 was more firmly directed toward either explorations of solar oblateness (as a test of relativity and the scalar-tensor variant) or the newly-discovered microwave background radiation. Generous as always, he offered me a free choice, and then took me to see the two experimental setups. We went first to the solar oblateness experiment, housed in a small wooden hut down by the Princeton Observatory. The hut was crowded with complicated electronics, many of them lock-in amplifiers, a Dicke invention I came to love and rely on. But the assembly of electronics was rather daunting. In contrast, the microwave background apparatus looked comfortingly simpler and even familiar — I'd used microwave techniques in my thesis research. And I thought Dave Wilkinson would be a fine person to work with. Boy, was I right!

With great good fortune, I chose as my first effort in the Gravity Group to work with Dave on designing and running what became the first real CMBR anisotropy experiment. The way that experiment was planned and carried out provides some useful lessons on how one should — and should not — design an experiment.

Dave and his colleague Peter Roll (see his contribution on page 144) had a year or so earlier designed an experiment to detect the radiation left over from the big bang. This instrument, shown in Figure 38, was specifically designed to make an absolute measurement of temperature or intensity of the CMBR radiation.

To measure or put limits on the anisotropy of the radiation requires a quite different approach. On the one hand, anisotropy measurements are easier, since they can be made comparatively (is this part of the sky hotter than that part?). On the other hand, Dave and I recognized that to be meaningful, such an experiment needed to be much more sensitive, and to produce temperature measurements accurate to a few parts in 1000. Penzias and Wilson (1965) in their discovery paper had already noted that the "excess noise" they picked up is approximately isotropic, with any variations in intensity below about 10%. We aimed to improve this limit by nearly two orders of magnitude. The plan was to scan a circle in the sky at constant declination over a long enough period so that any diurnal variations would

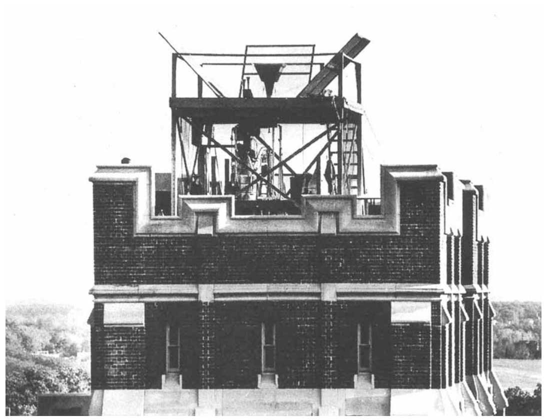

Figure 38: The former pigeon coop atop the Geology Building that housed the Roll-Wilkinson (1966) CMBR spectrum measurement and the 1965-67 Princeton "isotropometer."

cancel out. A dipole distribution in the CMBR temperature would then produce a 24-hour variation (in sidereal time), and a quadrupole moment a variation at 12-hour period.

As anyone who has lived in New Jersey knows, however, the atmosphere over Princeton is not exactly stable. To cancel out the atmosphere to first order, we needed to make calibration observations of a stable, unmoving region of the sky through a comparable air mass. We thus elected to switch the beam (observing direction) between the north celestial pole (the fixed point) and a point an equal angular distance away from the zenith to the south. We thus ended up scanning a circle at declination $\delta = -8°$. There were two levels of beam switching. First, we switched at about 1,000 Hz back and forth between our main horn antenna and a smaller antenna pointed towards the north. As a further control, we switched the beam of the primary antenna itself every few minutes by raising a reflecting sheet to divert the beam to the north celestial pole. This was the Princeton "isotropometer" housed in an unused pigeon coop on a tower of Guyot Hall (see Figs. 38

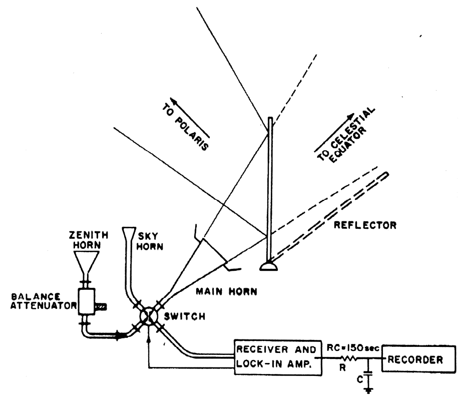

Figure 39: Schematic of the "isotropometer," showing the moving reflector used to zero the instrument.

and 39).

The kilohertz signal was phase-sensitively detected, and plotted out using a pen and ink chart recorder. (Mentioning a pen and ink chart recorder to scientists today must be the functional equivalent of telling my children that I walked 3 miles each day to catch the school bus. Both are true.) Dave and I and a handful of undergraduate students working with us then read the output of the chart recorder by hand to determine the differences between the declination $\delta = -8^\circ$ circle and our constant calibration point, the north celestial pole. We ran this experiment for substantially more than a year to help average out diurnal effects. Some of those results appear in Figure 40.

It soon became clear that atmospheric noise was completely dominating the signal. By late 1966 we were planning improvements. It would have helped, for instance, if we had been able to switch the main beam more rapidly, but we were aware that the ferrite devices used for switching are themselves a source of noise and potential systematic error, a problem later encountered in another anisotropy experiment by Dave Wilkinson and Paul Henry (Henry 1971). So we took another approach to doing a better experiment, trying to find a place where the atmosphere is more benign. We probably should have leapt immediately to the conclusion that we needed to get above the atmosphere altogether, as Dave later did in his pioneering balloon experiments, and as George Smoot and his colleagues later did with their U-2 experiments (Smoot, Gorenstein and Muller 1977). But we were a frugal pair, so we decided instead to find the place in the United States with

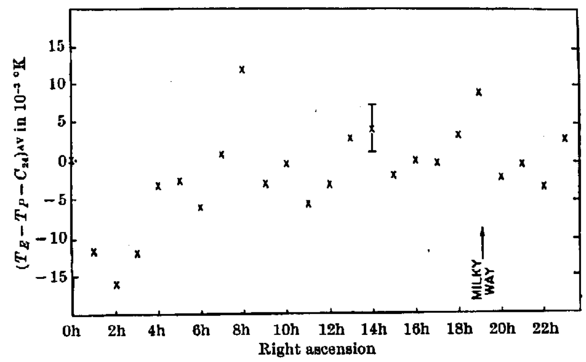

Figure 40: Results of the scan of a circle at declination $\delta = -8°$. The fractional temperature fluctuations are $\delta T/T \sim 3 \times 10^{-3}$ (Partridge and Wilkinson 1967).

the least cloud cover. Dave discovered that that is southwestern Arizona, and I found out that there is an Army base at Yuma, smack in the middle of this relatively cloudless zone. Through my father's Army connections, I got us permission to move an improved isotropy measuring device to the Army's Yuma Proving Ground.

We faced some constraints in designing the equipment. Both of us were busy teaching and could not spend much time in Yuma. So we needed to design a fully automated station that would take data and record it and that needed no daily maintenance. The equipment was designed with the main horn antenna pointed down, to prevent the collection of dust, rain, dead moths, etc. We also designed the equipment to scan two circles in the sky as well as the constant reference point, the north celestial pole. We also took much greater care to prevent radiation from the ground entering the main antenna through its side lobes — see the ground screens identified in Figure 41. I took charge of designing the structure to support the main antenna, as well as the rotating beam-switching device, a tilted, elliptical mirror. I recall bringing my designs to Bob Dicke, who took a brief look at them and said, "Well, it is certainly sturdy." By that he meant that I had over-designed the strength of the contraption by several orders of magnitude — I suspect it was at least as "sturdy" as the Army's top line tank!

Now, if you're designing a remote experiment, you need to have it in a place where casual hikers or hunters are not likely to poke around in it. The management at the Army's Yuma Proving Ground suggested that we use a securely fenced area at the outer edge of the base. It was securely

Figure 41: A refined experiment to look for anisotropy in the CMBR. Note the inverted horn and the use of ground screens to minimize stray radiation from the ground.

fenced because it was the site at which the Army tested the integrity of nerve gas shells. There were racks and racks of nerve gas shells of various sorts lying about in the desert, left out to see whether or when they would leak. Needless to say, the area was both securely fenced and patrolled.

So, in the summer of 1967 we packed the monstrosity I had designed plus some additional equipment (see below) into a large U-Haul truck, and set out for the west. Dave used the trip as a family vacation; I got to drive the U-Haul. When we arrived at the Yuma base, Dave was appointed a temporary captain, and I got to be a lieutenant. The U-Haul was costing us a fortune to drive back and forth to our remote site, so we bought an item designated as a "personal transport device" to the NSF, otherwise known as a moped, for me to commute to the instrument.

We soon had the equipment up and running. Since useful computers were still a ways in the future, the basic control mechanism for the experiment was derived from a re-wired digital clock, and the data were printed out on a line printer (whose values still needed to be recorded and sorted by hand).

While we were installing the Yuma apparatus, Dave and I were finishing up two papers on the results of the first anisotropy experiment at Princeton. One of those was written in a crummy motel room in Yuma using the only available horizontal surface, the top of a beer cooler. I remember sitting on the dirty green shag carpet, drinking Coors, and the excitement of reaching milli-K levels in anisotropy.

We also had some time to learn about the nerve gas from soldiers on duty near our site. They explained the use of gas masks — we were issued with them in case the nerve gas really did leak — and told us that the first way to detect leaks of nerve gas was to check out the rabbits. Near each stack of nerve gas shells there was a hutch containing several standard laboratory rabbits. These were placed there since rabbits are highly sensitive, it appears, to nerve gas. So the first alarm for leakages was increased rabbit mortality. Well, in our brief time in Yuma, the rabbits started to die. There was considerable consternation, not least on our part, until a wise veterinarian pointed out that all the rabbits had been bought at the same time, and all of them appeared, entirely naturally, to be reaching the rabbit equivalent of three score and ten. Since we'd escaped the nerve gas, Dave and I joined the soldiers' favorite game of sitting in a cargo container while someone set off a military-strength tear gas grenade. The game was to see who could last the longest before bolting for fresh air. We were young then.

As the Yuma experiment came on line, Dave and Bob Stokes left for the second main leg of the summer's work, a refined, multi-frequency measurement of the spectrum of the CMBR. I later joined them, traveling through the desert on my trusty "personal transportation device."

The idea here was to measure the temperature of the CMBR at three different frequencies — later extended to four by Paul Boynton — using very similar apparatus, so that the temperature measurements could be securely intercompared. In particular, the hope was to show that the spectrum of the radiation we were studying is not an exact Rayleigh-Jeans form, with energy density that varies with frequency ν as $u_\nu \propto \nu^2$, but instead shows some curvature as the peak of a blackbody spectrum at temperature about $3\,\mathrm{K}$ is approached.

So we designed radiometers having similar beam sizes, all able to couple to a common calibration cold load. To prevent systematic errors, we designed the main horn antenna to look downwards at an angle, making it easy to couple to a tilted dewar containing the cold load without moving the apparatus (Fig. 42; Stokes, Partridge and Wilkinson 1967) Thus, to deflect the beam to the zenith in order to measure the CMBR, we needed to use an oversize reflector. We also arranged the reflector to be movable, so that the main beam could be cast through different zenith angles, enabling us to measure the atmospheric emission with the same equipment used for the absolute temperature measurements. The three radiometers used in the 1967 campaign are shown in Figure 43; Figure 44 shows the experimental

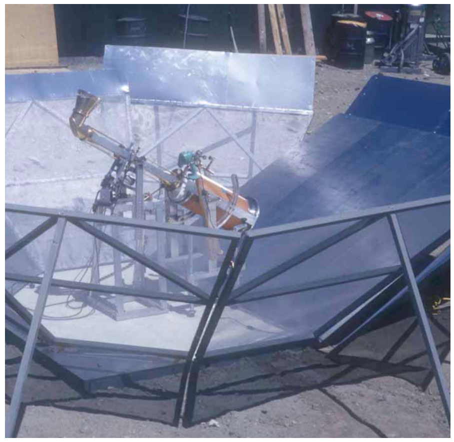

Figure 42: Photo of one of three radiometers used on White Mountain, California, to measure the spectrum of the CMBR (Stokes et al., 1967). The horn antenna is coupled to a large-diameter cold load.

setup.

It is worth mentioning the care we took to avoid systematic error. Dave Wilkinson, as all who knew him will attest, was extraordinarily careful about finding and eliminating, or at least modeling, sources of systematic error. We took great precautions, for instance, to control emission from the ground leaking into the side lobes of the antennas we used. We were conscious that emission from the walls of the calibration cold load could present a problem, and for that reason we expanded the beam and used a large "over-moded" cold load immersed in liquid helium. I have already mentioned quasi-simultaneous measurements of the atmospheric emission. And we also took account of the possible emissivity of the reflecting surface.

The result of this work was to produce temperature measurements at three wavelengths with substantially smaller error bars than previous workers had been able to obtain. The error bars were small enough to show rather convincingly that the spectrum of the CMBR does indeed begin to turn over at high frequencies, as expected for a 3 K blackbody (Fig 45). And the final temperature we derived from combining observations at the three frequencies gave a value $T = 2.68^{+0.09}_{-0.14}\,\mathrm{K}$, in remarkably good agreement with the COBE satellite results that came along nearly two decades later (Stokes, Partridge and Wilkinson 1967; Wilkinson, 1967).

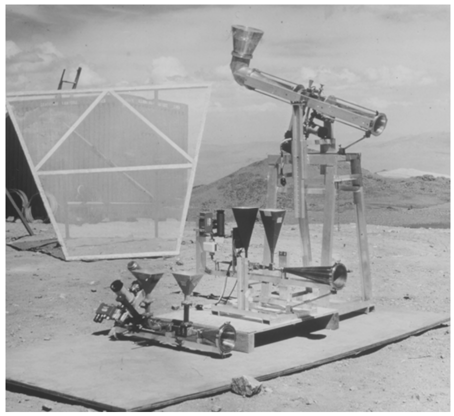

Figure 43: The three radiometers used by Bob Stokes, Dave Wilkinson and me to measure the CMBR temperature at wavelengths $\lambda = 3.2$, 1.6 and 0.96 cm.

The spectral observations were carried out at the highest place in the United States with electrical power, the White Mountain Research Station maintained by the University of California. Not surprisingly, other groups had figured out that this was an excellent place from which to observe the microwave background. When we arrived, we discovered Bernie Burke and his colleagues busy assembling apparatus that looked an awful lot like that shown in Figure 43 (Ewing, Burke and Staelin 1967). Our group and his agreed to work entirely independently, so as not to influence one another's results. Yet another group, Welch, Keachie, Thornton and Wrixon (1967), also recognized the value of high altitude observations. However, they en-

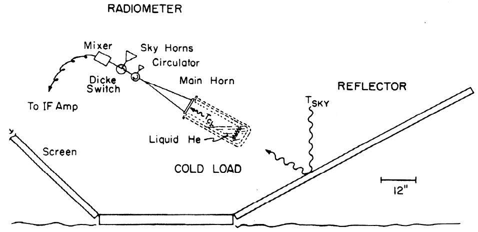

Figure 44: A schematic of the radiometers in Figure 43.

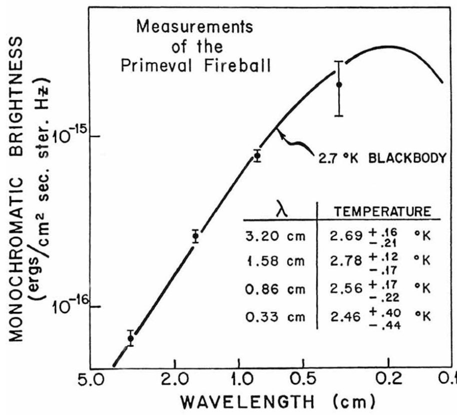

Figure 45: Results of the Princeton measurements on White Mountain, showing a departure from the Rayleigh-Jeans $u_\nu \propto \nu^2$ law.

countered problems with the design of their cold load calibrator, and perhaps as a consequence came up with too low a value for the CMBR temperature. On page 191 Welch describes how that turned out.

Even for these measurements, there was a constant struggle against the atmosphere. Uncertainties in the amount of emission from the atmosphere, particularly from water vapor, dominated the error budget. Throughout the experiment, we were worried about possible frequency-dependent systematic errors that could bias our results. I suspect it was at this stage that Dave came to recognize the value of balloon experiments, and even more of a satellite experiment to get out the atmosphere altogether. Nevertheless, working with Bob Stokes and Paul Boynton, Dave went on to do one more ground-based temperature measurement in these years, the measurement carried out at 0.33 cm wavelength at the High Altitude Observatory in the Colorado Rocky Mountains (Boynton, Stokes and Wilkinson 1968). They found $T = 2.46^{+0.40}_{-0.44}$ K, which within the errors is consistent with the modern value. And, as a footnote, I went on to join an Italian-Berkeley-Haverford team that returned to White Mountain 15 years later to make refined spectral measurements at five wavelengths, 0.33 to 12 cm (Smoot *et al.* 1985).

What were we trying to accomplish with these early experiments? With the wisdom of hindsight, it is clear that we were beginning the process of mining the CMBR for cosmological clues. But in the years 1965-68, the full value of spectral and anisotropy measurements was far from appreciated.

The beautiful and influential theoretical work on the power spectrum of CMBR fluctuations, for instance, lay years in the future. So what were we really trying to accomplish?

First and foremost, we were trying to establish that the microwave radiation detected by Penzias and Wilson (1965) is indeed cosmic, and not coming from more local sources in the Solar System, the Milky Way, or galaxies or some other class of extragalactic objects. In the mid-1960's there were plenty of skeptics, and numerous noncosmological explanations of the "excess noise" reported by Penzias and Wilson. We recognized that strong proof of cosmic origin lay in two fundamental tests: the blackbody shape of the spectrum of the radiation and its isotropy on both large and small scales.

Electromagnetic radiation pervades the universe. At radio wavelengths it is dominated by the emission from galaxies and quasars. Could the "excess noise" detected by Penzias and Wilson simply be the high frequency tail of this background? The spectrum holds the key to the answer. Emission from radio galaxies is typically dominated by the synchrotron process, producing a power law spectrum $u_\nu \propto \nu^{-\alpha}$ with α generally in the range 0.5 to 1.0. This is very different from a thermal spectrum where $u_\nu \propto \nu^2$ at long wavelengths. Another possibility is "free-free" emission from a thin plasma with nonrelativistic electrons, which typically produces a power law spectrum with $\alpha \simeq 0.1$. Such a spectrum, too, is easy to distinguish from the truly thermal or blackbody spectrum expected from radiation left over from a hot, dense state of the early Universe.

More difficult to distinguish from a true blackbody spectrum is *graybody* — emission fron an optically thin but higher temperature source. At wavelength $\lambda \gg 0.3/T$ cm, with temperature T measured in kelvin, graybody emission can have the same ν^2 dependence as true blackbody emission, but the spectrum peaks at shorter wavelengths. To confirm true blackbody emission at $T \simeq 3$ K, we needed both to confirm the ν^2 dependence at long wavelength and find evidence for the peak expected near 0.1 cm wavelength.

It is worth repeating how unlikely it is to find a purely thermal spectrum in the cosmic setting, where densities tend to be very low. Only if the universe were many orders of magnitude denser than it is now could true thermal equilibrium have been established. If the microwave background radiation truly does have a thermal spectrum, it not only establishes the cosmic origin, it shows that the early properties of the Universe were radically different from those prevailing today.

By 1967 we had the answer: we were seeing curvature in the spectrum consistent with a peak at a wavelength of about 1 mm (Stokes et al., 1967;

Wilkinson, 1967).

Isotropy is the second test. An observer not moving with respect to the comoving coordinates of the universe discussed in Chapter 2 (page 14) would see that radiation left over from the big bang appears isotropic (apart from the disturbances caused by the departure from a smooth mass distribution).[26] On the other hand, a Solar System origin would be expected to produce intensity variations tied to coordinates fixed to the direction to the Sun. Sources in our galaxy would presumably produce radiation that peaks in the direction of the galactic plane, in an anisotropic distribution akin to the concentration of bright stars in the plane of the Milky Way. Such a distribution would introduce a large dipole moment, and particularly a quadrupole moment, into the distribution of CMBR intensities. If the radiation were somehow produced by a myriad of extragalactic radio sources, as suggested for instance by Wolfe and Burbidge (1969), it would be "grainy" on a small scale. More precise limits on anisotropy on both large and small scales would, we hoped, kill off these noncosmological explanations. This hope motivated our work, and we soon showed (Partridge and Wilkinson 1967; Smith and Partridge 1970) that the radiation is indeed highly isotropic on both large and small angular scales.

Nor were challenges to the cosmic origin of the CMBR mounted solely by inventive theorists. At least one experimental result, the pioneering rocket measurement of Shivanadan, Houck and Harwit (1968; see Harwit's piece here) seemed to favor a graybody spectrum. The results naturally raised doubts about the cosmic origin of the microwave background.

All of these results, attacks on the very notion of the "primeval fireball," were very much on our minds as we mounted the experiments described above and wrote up our results.

One anecdote encapsulates the skeptical air of the times. In 1969, as I recall, I gave a talk on our Yuma experiment at a meeting at Caltech. In the question period a formally dressed, middle-aged man in the back asked, in effect, "Given that you see no change in emission as the sky passes overhead each day, how do you know your equipment is even switched on?" Fortunately — since the questioner was Charles Townes — I gave an appropriate answer, describing in detail the care we took to calibrate the instrument.

So I would say that in the 1960s, we were on a mission to convince the skeptics, an attitude that strongly colors an early review of the primeval

[26] Parenthetically, this would not be the case if the universe itself were expanding in an anisotropic way. Ellis (page 288) notes that this idea was of considerable interest in the 1960s.

fireball published in the spring 1969 issue of the American Scientist (Partridge 1969). I would suggest, however, that there was another influence at work. We were, after all, working for Bob Dicke, acknowledged as the master of the beautiful null experiment. These are experiments designed to to test the absence of some physical effect by establishing more and more stringent upper limits on the magnitude of the effect. Dicke's ultrasensitive version of the Eötvös experiment, for instance, showed that there are no differences in the way gravity acts on different chemical elements to a level of roughly one part in 10^{11}. I will speak only for myself here, but part of the motivation driving me was to do better and better null experiments on the CMBR, in particular to establish lower and lower upper limits on possible fluctuations in the CMBR (see Partridge 2004). In other words, I am confessing to having been driven less by theoretical concerns or predictions than by an experimenter's lust to do the best possible experiment, and to cover as much parameter space — in this case sensitivity and angular scale — as possible. As Dave and I were planning better experiments to push down limits on the amplitude of the temperature fluctuations on degree scales, I was also thinking about ways to limit anisotropies on small angular scales. This required the use of larger aperture devices, since the angular scale goes approximately inversely as the diameter. Others — Ned Conklin (1967), Eugene Epstein (1967) and Penzias, Schraml and Wilson (1969) — had already set upper limits on arcminute-scale fluctuations; Paul Boynton and I realized we could reach both smaller angular scales and higher sensitivity using a 36-foot telescope operated by the National Radio Astronomy Observatory. Our results turned out to be only mildly interesting, and I mention them simply because they reflect at least one person's motivation in these early years — to set the best possible limits on fluctuations at all angular scales.

That I was not alone in this aim is reflected in the way in which anisotropy measurements were presented in these early years, and for at least a decade afterwards (see Fig. 46). What is shown is basically a plot of upper limits on the fractional temperature fluctuations $\delta T/T$ across the sky, with little reference to any underlying theory of what the angular spectrum of anisotropies might be (though we did know that the overall amplitude would be affected by the mass density fluctuations). Also reflecting the focus on upper limits is the fact that a paper I wrote with Italian colleagues in the early 1980s was initially rejected solely on the grounds that the upper limit we established was not as low as the upper limit somebody else had established, despite the fact that we were working at degree angular scales and the "better" experiment was at arcminute scales.

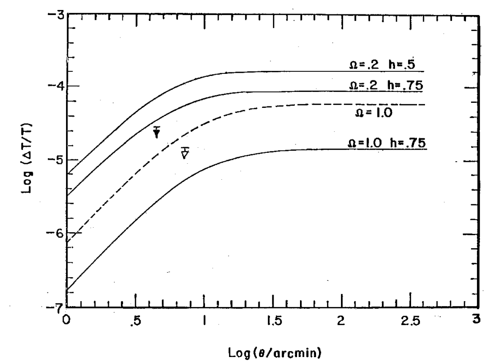

Figure 46: An early (and poor) way of representing upper limits on anisotropy in the CMBR on various angular scales.

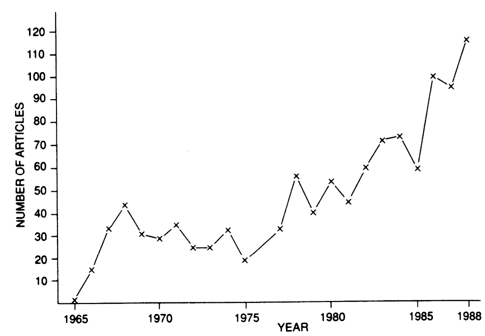

Figure 47: The number of CMBR-related papers published each year. Note the lack of activity in the years 1969-77.

Is it possible that the drive to set lower and lower limits on CMBR anisotropies has a contemporary analogue in the drive to determine the cosmic equation of state parameter w that is supposed to describe the evolution of the dark energy density? Does the (expensive) effort to improve limits on this effect parallel our efforts 40 years ago to improve limits on the CMBR temperature anisotropy? Indeed, the same question could be asked about plans to measure another cosmic parameter, the ratio, r, of tensor to scalar anisotropy perturbations. Clever scientists are designing better and better methods of refining measurements of r and w, but without much theoretical guidance (especially on r).

Finally, I would like to explore a phase change that occurred in the field in the early 1970s. It may surprise some of you, who see the CMBR as a giant growth industry, to learn that by the end of the 1960s interest in it was waning, or at least changing. That is reflected in the rate of publication of papers dealing with the CMBR shown in Figure 47, taken from the "history" chapter in my book, 3 K (Partridge 1995). Before exploring some reasons for temporary dwindling interest in the CMBR, let me make a much more positive point: this is the time when many other groups here, in Europe and in Russia began to take an interest in improved CMBR experiments.

The emergence and success of these groups is linked to one of the reasons I see for the cooling of ardor at Princeton. By the late 1960s we had, in effect, done all the easy experiments. The experiments Dave and I did were, as I liked to say, "one-Cadillac scale," costing of the order of $10 to 20,000 each. They were constructed almost entirely from commercially available components. To improve these experiments, new detectors and optics were needed, as were instruments designed specifically for the detection of CMBR anisotropies or the precision measurement of the CMBR spectrum. In addition to new technology, better observing strategies were needed (recall my remarks above on problems created by the atmosphere). The same strictures applied to the use of existing radio telescopes: observers had pushed them to their technological limits as well.

New groups brought new techniques and new technologies to bear. I want to mention specifically the introduction of bolometric detectors into the field, and to praise the foresight of people like Paul Richards and Francesco Melchiorri, and of Rai Weiss, who has written for this volume. Francesco was a pioneer in the field, who unfortunately passed away in 2005.

So there was a pause while new technologies and techniques were brought to bear. Along with new groups joining the field, Dave Wilkinson wisely moved in the direction of balloon experiments. I got interested in the use of radio-frequency interferometry to probe yet smaller angular scales. The introduction or exploration of these new techniques took time, and that is in part responsible for the drop in activity in the CMBR field in the early 1970s.

Another factor, at least in the case of Princeton's Gravity Group, was the explosion of other interesting things to do in astrophysics, ranging from pulsar timing to searches for "primeval galaxies." The experimentalists of the Gravity Group found lots of other intriguing things to do while we waited to sort out new CMBR technologies and techniques. Dave, for instance, began to explore limits on extragalactic optical backgrounds and oversaw Marc Davis's pioneering search for primeval galaxies. I mounted a separate search for primeval galaxies, and got interested in observational tests of the

Wheeler-Feynman (1945) absorber theory (Partridge 1973) and searches for bursts of radio-frequency emission. Both Dave and I, joined by Ed Groth and Paul Boynton, spent a lot of time from the spring of 1969 on making precision timing measurements of the optical pulses of the Crab Nebula Pulsar. In an ironic twist, we felt we had discovered evidence that the Crab Nebula pulsar is slowing down due to the loss of energy by gravitational radiation; it turned out that Nature had thrown us a curve ball in the form of a glitch in the pulsar period. But another, cleaner pulsar system would reveal energy loss by gravitational radiation and win the Nobel Prize for Russell Hulse and Joe Taylor.

I will end by floating an idea that may be strongly colored by retrospective wisdom. Could the lull in CMBR activities have been in part influenced by the fact that we were beginning to pay some attention to theoretical predictions as to the properties of CMBR anisotropies and spectral distortions? That is, instead of blindly trying to set better and better limits on both anisotropy and spectral distortion at a range of wavelengths and scales, were we, I wonder, beginning to recognize (a) how hard it would be to see meaningful spectral distortions and (b) that the amplitude of anisotropies would in general be very small except on certain angular scales? Frankly, my recollection of my mood in the late 1960s and early 1970s is now a little too hazy for me to say for sure. What I can say is that the five years, 1965 to 70, were not only the years that truly established physical cosmology, but were a hell of a lot of fun!

Ronald N. Bracewell and Edward K. Conklin: Early Cosmic Background Studies at Stanford Radio Astronomy Institute

Ron Bracewell has been at Stanford since 1955 and is now professor emeritus. Ned Conklin was a graduate student at Stanford at the time described here. After a number of years at NRAO and Arecibo, he co-founded FORTH, Inc. and worked in the field of scientific computer programming until retiring recently.

When the existence of the CMBR was announced in 1965, we discussed various kinds of measurements that might be made. Ned had been looking for a thesis topic, and this was an interesting and brand new field. The most obvious measurement was the absolute amplitude at one or more new wavelengths, but that would have been an extremely difficult experiment. It's relatively easy to make a measurement of the apparent temperature relative to that of a known absorber when an antenna is pointed at empty sky, but that's only the start. Then all the other possible sources of antenna temperature such as atmospheric losses, losses in the system and the antenna, unwanted pickup in the sidelobes, etc. must be accurately estimated or measured and subtracted from the observation to yield the residual (if any) due to the CMBR itself. In nearly all cases these unwanted sources of radiation are substantially higher than the few degrees of the CMBR, and so the subtraction process is prone to errors. Further, we recognized that, once a measurement had been published at one frequency and a thermal spectrum was posited, there would be the unconscious bias towards confirming it at other frequencies, leading to a situation where one might think of extraneous radiation sources contributing to a measured value, subtract their effect until the CMBR temperature was reached and then stop looking quite as diligently, so that a secondary measurement would not be truly independent.

Discarding the absolute value left measurements of the angular structure (if any) of the radiation field. In their original paper Penzias and Wilson (1965) reported that the CMBR was isotropic, but the precision of their measurements was low. Two possibilities presented themselves (Bracewell 1966; Conklin 1966), each with its own set of experimental problems – measuring the fine scale on the order of arc-minutes (inhomogeneity), and measuring the large scale on the order of degrees (anisotropy). In the end we pursued both.

The weak cosmic background radiation detected by Penzias and Wilson was reported as isotropic. So it was with cosmic rays, but they were quickly found to vary with altitude, latitude, and season, so we thought it likely that the cosmic background also might prove to be not isotropic. Any anisotropy of the remote cosmic background would be hard to address but in the summer of 1967, when these measurements were undertaken, Stanford Radio Astronomy Institute was by chance well equipped to look for inhomogeneity in the newly announced microwave radiation. A 735-foot minimum-redundancy microwave array of five interconnected 60-foot paraboloids and associated electronics was already under construction (Bracewell et al. 1973). With particular attention to symmetry and matching at a frequency of 10,690 MHz, a pair of identical feed horns was installed at the focus of one 60-foot dish, one horn pointing to the zenith, the other down on the level paraboloid. The paraboloid was tilted very slightly off zenith so as to cause the radio source Cygnus A to pass through the center of the fixed antenna beam and serve as a calibration source.

A waveguide tee junction, built to connect the two horns, incorporated a two-state ferrite circulator; in one state the signal received from the radiometer was that from the upward-looking horn whose beamwidth was about 80°, and in the other state the signal was from the horn that fed the paraboloid forming a resultant beamwidth of about $13'$. The circulator was subjected to square-wave switching at 37 Hz. The signal delivered to the output arm of the tee, after preamplification, mixing, intermediate frequency amplification, and detection thus consisted of a noisy square wave jumping between the antenna temperatures in the 80° beam and the $13'$ beam formed by the paraboloid. One can understand that in the state of electronics of the day extreme care was needed to deal quantitatively with such a small jump.

The response to the upward-pointing horn was expected to be 900 K (the noise temperature of the radiometer) plus the mean intensity of the cosmic background, now known to be approximately 2.7 K, plus a few more degrees from any atmospheric radiation and ground radiation in the antenna sidelobes. The response of the horn looking into the reflector and focused into a pencil beam $13'$ wide would be nearly the same and the difference would reveal any local departure from isotropy as the sky rotated overhead. Clearly, constancy with time of the 900 K noise temperature of the receiver is also of the essence of the experimental design, as well as constancy of the atmospheric radiation.

The experimental differential results did show both a long-term trend (over a period of hours) which was evidently caused by thermal effects in the radiometer and switched circulator, and short-term fluctuations which did not repeat from day to day and which correlated fairly well with changing atmospheric conditions. The long-term trend was removed by subtracting a half-hour running average from the data; the effect of this was to limit the inhomogeneity analysis to beamwidths less than about 7.5° (30 m. of right ascension), not a serious limitation. The short-term atmospheric fluctuations, being random, could not be removed and simply added to the effective r.m.s. antenna temperature. Out of 65 days of observation, the data from 29 days were rejected entirely as having excessive short-term fluctuations, and the remaining 36 days of data were averaged and analyzed.

The results did not show any noticeable inhomogeneities in a strip of sky located at 40.6° N declination and from 11 to 19 h of right ascension. The mean observed r.m.s. antenna temperature of the data, although somewhat higher than expected, decreased as $N^{-1/2}$ up to the limits of our data, where N was the number of days in the average, indicating that the results were purely statistical and that no floor of intrinsic CMBR inhomogeneity was limiting the observed results. At the $13'$ beamwidth of the paraboloid, the 3σ limit to any intrinsic inhomogeneities in our data was about 7.9 millikelvins. By integration of the data, even lower limits could be set for larger angular scales Θ up to a few degrees, and limits could also be set for angular scales smaller than $13'$, recognizing that their amplitudes had been reduced by the spatial smoothing in the antenna beam. The final published results were $\sigma = 103/\Theta$ for $0' < \Theta < 13'$, and $\sigma = 28\Theta^{-1/2}$ for $13' < \Theta < 120'$, where σ is the r.m.s. intrinsic inhomogeneity in millikelvins (Conklin and Bracewell 1967). These limits were low enough to be useful constraints on various competing theories of the CMBR.

The very-low-noise radiometers widely available today were not common in the late 1960's and since statistical r.m.s. fluctuations unfortunately decrease only as the square root of the integration time, improving the inhomogeneity limits below the values quoted above would have required excessive observing time. Also, the borrowed equipment being used for the experiment was needed for the completion of the five-element array, so at this point we turned our attention to the anisotropy question. Here we had at least one likely positive result on theoretical grounds, albeit at a very low level. If the Earth were moving with respect to the rest frame of the CMBR, then there would be an increase in the CMBR temperature in the direction of motion, and a similar decrease in the opposite direction. (The theory is discussed below.) A speed of 300 km s^{-1} would result in a temperature change of only one part in a thousand, or about 2.7 millikelvins, quite an experimental challenge in that era (and still not easy).

Measuring this kind of effect with any type of moving or scanning antenna was obviously out of the question. Varying ground pick-up in antenna sidelobes and changes in atmospheric re-radiation would far outswamp the few millikelvins from the CMBR itself. The only hope was to construct some

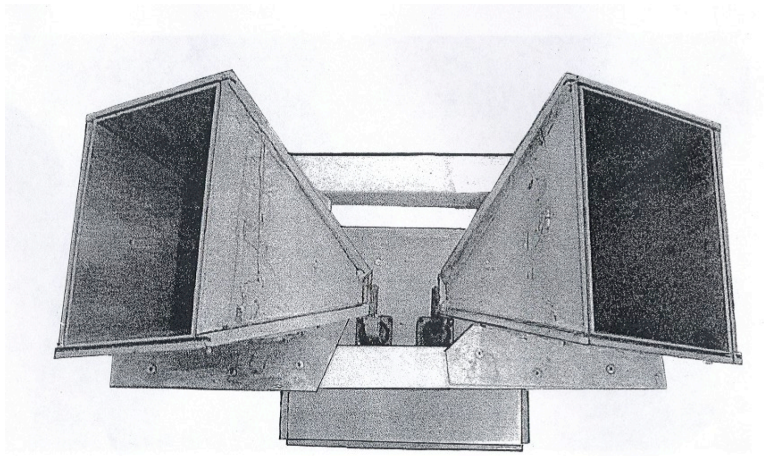

Figure 48: Historic horns through which flowed the radiant energy (estimated at roughly half an erg during the integration time of 425 hours) that revealed the Sun's motion through the cosmos.

kind of system that was fixed with respect to the ground and let the sky sweep through the beam. The dipole anisotropy would then appear in the antenna temperature as a sine wave with an amplitude proportional to the velocity and a period of one sidereal day.

The apex of the Earth's motion (if any) with respect to the CMBR was completely unknown. But unless it were closely aligned with the north celestial pole, there would be an equatorial component which could be measured most easily. As with the inhomogeneity experiment, a radiometer would need to be switching between two sources with antenna temperatures as nearly equal as we could devise, and yet be sensitive to the dipole anisotropy. Because of the extremely low amplitude, it was desirable that no integration time be spent on reference loads or reference patches of sky such as the north celestial pole.

We ended up with an antenna system consisting of two identical horns (Fig. 48) with 14° beamwidths directed east and west respectively at a zenith angle of 30° and enclosed in a truncated conical screen to intercept radiation from the ground. To limit reception of unwanted thermal radiation from the surroundings these horns incorporated, at their rims, the short-circuited quarter-wave transmission line chokes familiar from microwave radar practice.

As the sky passed overhead any temperature difference between the two patches of sky pointed at would contribute a component to the recorded system temperature. As the earth rotates a given region of sky will pass first through the eastern antenna beam and then, several hours later, through the western beam. Since the polarity of the antenna temperature is opposite for the two antennas, this configuration is measuring the finite difference of the temperature distribution in the sky. It can be shown that the original sky distribution is recoverable from the finite difference record except for the mean value, which is not of interest here. The amplitude of any dipole anisotropy is reduced somewhat by the finite differencing, but the sensitivity remains higher than for a fixed reference system.

An extremely sensitive and stable radiometer was essential for this experiment. Receivers with very low system temperatures (such as masers) were just not in the budget, as well as being difficult to keep stable. Since the CMBR is essentially an extremely wide bandwidth thermal signal, an alternative for increasing the sensitivity was to increase the bandwidth of the receiver. We were fortunate in obtaining a very wide bandwidth, fairly low noise tunnel diode amplifier on loan from NASA, and the radiometer system was built around it. The wide bandwidth made the radiometer susceptible to airborne radar, but that was sporadic, and easily detected on a chart recorder and eliminated from the data.

The noise figure of the tunnel diode amplifier was such that it was theoretically capable of reaching an r.m.s. output fluctuation of 5 millikelvins for a one-minute average. It took months of patient experimentation to construct a complete radiometer system that would stably operate at that level, but eventually we succeeded. A key feature that was added at this time was that the entire radiometer including the antennas and front-end electronics was mounted on a turntable. At five minute intervals the turntable rotated 180°, interchanging the east and west antennas. This second differencing removed the last small asymmetries and drifts in the electronics and left us with a system that appeared capable of detecting millikelvin variations in the CMBR.

Just a few days of observation near sea level at Stanford were enough to show that operation there was hopeless because of fluctuating atmospheric absorption and re-radiation at the 8 GHz frequency of our system. We needed an observing site with extremely low water vapor, and that meant in general high altitude and ambient temperatures below zero degrees C. Again we were fortunate in finding a reasonably accessible high-altitude facility at the University of California's White Mountain Research Station. The entire radiometer and associated data-taking electronics were installed in a small trailer and towed up to the Barcroft station at latitude 37° N in October

1968. Here, at an altitude of 12,500 feet and atmospheric pressure less than two-thirds of that at sea level, the receiver performed acceptably close to its theoretical limit (although the human observer had difficulties!).

Because of solar radiation in the antenna sidelobes, useful data could only be taken at night time. Two month-long observations were made in October 1968 and April 1969 and were combined to obtain a complete 24-hour record. It was immediately evident that non-thermal galactic radiation was affecting the data; this was removed by extrapolating an all-sky map of the galactic antenna temperature to our 8 GHz observing frequency and subtracting it.

Details of the reduction procedure are given by Conklin (1969), along with a preliminary result based on the first observing run. The final reported result for the first detection of the dipole asymmetry was an amplitude of 2.28 mK (formal standard error ± 0.68 mK, total estimated error ± 0.92 mK) at a phase corresponding to right ascension 10 h 58 m (Conklin 1972), indicating a solar velocity in the equatorial plane of 255 ± 76 (formal), ± 103 (total) km s^{-1}. No significant smaller-scale anisotropies were seen in the data, which covered a region of sky from about 25° to 39° N declination and the full range of right ascension. The precision achieved in the light of determinations years later with much improved electronics, was quite respectable.

The theoretical impact of the discovery that the Sun possessed an absolute motion through the Universe was striking, both to the scientific community and to science writers (Sullivan, 1969) addressing the general public. When the letter appeared in *Nature* a letter to Bracewell from Professor Jakob L. Salpeter in Adelaide reported on a paper written by Kurd von Mosengeil (1907) where we read:

> Alle Versuche, einen einfluß der Erdgeschwindigkeit auf die elektrodynamischen Erscheinungen festzustellen, haben ein negatives Resultat ergeben. Um dies zu erklären, haben H.A. Lorentz[1]) und in noch allgemeinerer Fassung A. Einstein[2]) das ,,Prinzip der Relativität" eingeführt, nach welchem es prinzipiell unmöglich ist, einen derartigen Einfluß aufzufinden.

> All attempts to establish an influence of Earth's velocity on electrodynamic phenomena have given a negative result. To explain this H.A. Lorentz (1904), and in greater generality A. Einstein (1905), have introduced the principle of relativity, according to which it is in principle impossible to discover such an influence.

We now know more about special relativity. In 1996 textbooks of thermodynamics did not mention moving observers; one could only wonder what appearance the microwave sky would present. The two reports (Bracewell 1968) deduced that the brightness observed in the forward direction would increase and that the spectral distribution would still be that of a black body, but with a temperature apparently higher than that seen by a stationary observer. Naturally the spectral components would be shifted to higher frequencies by Doppler effect, but that alone would not result in a Planck spectrum; two other effects are involved. Stellar aberration would reduce the solid angles subtended by sky elements on or near the apex of solar motion and this would result in an increase in brightness as measured in watts per square meter per hertz per steradian. Finally, the electromagnetic field strengths of both the electric and magnetic fields would be increased a little by the relativistic Lorentz transformation. Combining these three effects we found (Bracewell and Conklin 1967) that the spectrum would preserve Planck's blackbody form. In the forward direction, if the observer was moving at one thousandth of the light velocity c, the apparent temperature would be greater by one-thousandth than the temperature T seen by an observer at rest. For an observer moving with velocity v we found that, in a direction making an angle θ with the direction of motion, the observed temperature would be $T[1 + (v/c)\cos\theta]$. If the observer's velocity was not negligible with respect to c then the observed temperature would be $T[1 + \beta\cos\theta/\sqrt{1-\beta^2}]$, where $\beta = v/c$.

Though a literature search did not uncover this result it was reasonable to assume that it was known; the internal reports (Glints) were for the edification of the graduate students. It was therefore a surprise when Condon and Harwit (1968) reported that the spectrum seen by a moving observer would not be that of a black-body. The internal memorandum was dusted off and submitted for publication to the appropriate journal but, being rejected, was resubmitted to *Nature* (Bracewell and Conklin, 1968). Shortly after that we learned from Prof. Salpeter's letter, that the same conclusion had been reached by von Mosengeil in 1907. Our discovery of the Lorentz/Einstein undiscoverable naturally made a wide impression. In due course Corey and Wilkinson (1976) of Princeton University launched many balloon flights and extended Conklin's results to a range of declinations, and Smoot, Gorenstein, and Muller (1977) of the University of California at Berkeley made many flights with the Kuiper flying observatory stationed at NASA Ames Laboratory in Mountain View, California. These more detailed endeavors neatly bracketed the original Stanford discovery (Fig. 49). The remarkable detail discernible in the Cosmic Background Explorer satellite

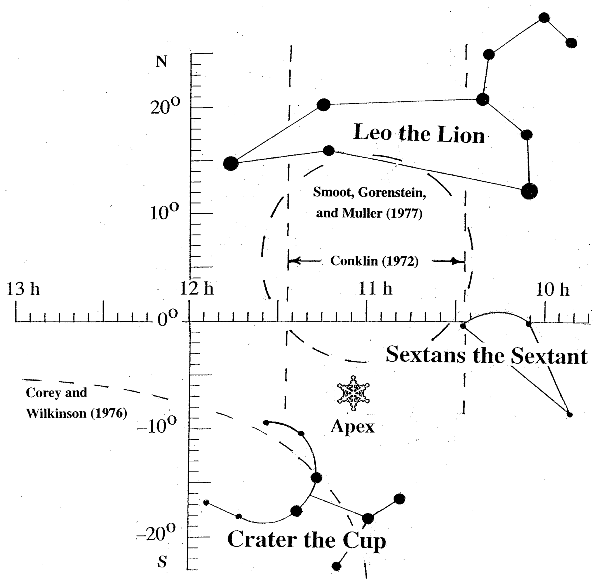

Figure 49: Constellation chart showing a naked-eye observer on the bridge of Spaceship Sun that we are heading toward the point labeled "Apex." The coordinates have been refined by spacecraft observations. Also shown are the earliest reports from Stanford, Princeton, and Berkeley.

images of later years have continued to grip the lay imagination.

The Sun's motion might reveal itself in other ways, for example it should be evidenced by a dependence of density of distant galaxies on θ; they should be less tightly packed in the general direction of Pisces than in the direction of Sextans. Counts of faint galaxies in the zones $\theta < 15°$ and $165° < \theta < 180°$ should differ by about a part in a thousand.

Meanwhile, the effort to discern a dipole component against a spatial noise background had left open the question whether spatial fluctuations existed, on a fine angular scale, in the background radiation. Careful observation did not reveal any such pattern but did allow an upper limit to be placed on the size of any such departures, as averaged over small solid angles. A technically remarkable low limit of 0.005 K for beamwidths broader than 10 arcminutes was reported (Bracewell and Conklin 1967).

This was less than exciting for an aspiring Ph.D. candidate but caught the attention of theoretical cosmologists and was quoted for several years as a constraint on assorted cosmological theories.

Thus in these early days, three significant contributions were made to the future and continuing studies of the fundamentally significant cosmic background radiation.

When the National Science Foundation withdrew support from universities in order to fund the National Radio Astronomy Observatory the staff of the Stanford Radio Astronomy Institute dispersed, some to NRAO, some to radio astronomy elsewhere, and some to industry. The remaining graduate students eked out their dissertations with funding support for medical imaging, solar thermovoltaic energy, theory of dynamic spectra, etc. The mothballed observatory was torn down on March 11, 2006.

Postscript: On Being the First to Know (EKC). In the summer of 69 I was reducing all the data that had been taken in our two month-long observing runs looking for CMBR anisotropy. This involved hundreds of IBM cards filled with five-digit numbers, each one indicating the value of a one-minute integration from the radiometer, and a complex program that combined the observing runs, aligned and averaged the data, subtracted the estimated galactic radiation and Fourier-transformed the result. Because the amplitude of the dipole component was expected to be very small, there was no way to get a preliminary indication along the way of any statistically significant result; it was all or nothing. Late one evening I was at the Computer Center, ready at last with the run that would have the final result. I submitted the deck of cards to the mainframe computer operator, and in about half an hour, back came the stack of output paper. This would have two important numbers in it that represented about 18 months of effort: the dipole anisotropy and its standard error. Did I have something significant? Yes!

At that point I recalled an article I had read recently, I believe by Philip Morrison, that one of the joys of research like this is that for a moment, until you choose to tell someone else, there is something you know that no one else in the world knows. I went home that night elated to have had that happen to me, and although all of this occurred over 35 years ago I have never forgotten it.

Stephen Boughn: The Early Days of the CMBR: An Undergraduate's Perspective

Steve Boughn is professor of physics and astronomy at Haverford College. His expertise includes the theory and detection of gravitational waves, extragalactic astronomy and cosmology.

I'm not sure why Peebles and Partridge invited me to participate in this collection of remembrances since I was only an undergraduate in the 1960's and most of what was swirling around above my head was just that, over my head. Of course, I've learned a great deal about Big Bang cosmology and the CMBR since then, and I have been involved in CMBR research off and on for the last 40 years. However, I'll endeavor to relate as accurately as possible what was going on in my mind back then in the hope that the recollections of a novice might be of some interest or at least provide some amusement.

I enrolled as an undergraduate at Princeton University in 1965, the same year as the announcement of the discovery of the CMBR. I had been enticed by special relativity early in high school and so arrived at Princeton committed to becoming a physics major. Astronomy had also been a source of fascination for me since I was very young. It was an exciting time in astronomy; quasars had just been discovered and pulsars were soon to follow. However, my entire knowledge of cosmology consisted of Hubble's discovery of the expansion of the Universe and the notion that Einstein's theory was capable of "explaining" what was going on. Still that was what excited me most. Even so, for my first three years at Princeton I busied myself studying physics (no astronomy - even though today I'm a professor of astronomy, I've never actually taken a course in astronomy!). Then in 1968, at the end of my junior year, it was time to choose a senior thesis topic. Of course, I immediately pestered the people in Dicke's Gravity Group for possible projects. I still remember the suggestions. Dicke suggested a project having to do with solar oblateness and its relation to Brans-Dicke theory. John Wheeler suggested two projects, one having to do with the dragging of inertial frames (at Stanford, Francis Everett and my future PhD advisor Bill Fairbank were already deeply involved in what is now known as Gravity Probe B to test this effect). Wheeler knew I was from Wyoming and so described this to me in terms of a cowboy's lariat. The second project he suggested was experimental. He thought it would be interesting to try to measure the advanced potential implied by the Feynman-Wheeler absorber theory of radiation using one of the CMBR radiometers of Dave Wilkinson and Bruce Partridge. Partridge and Wilkinson suggested a project also involving one of their microwave radiometers, but used in a more standard way to attempt to measure the dipole and quadrupole moments of the CMBR. This is what I chose and that decision had a lasting impact on my professional career. I hope I realized at the time how fortunate I was to have the choice (as an undergraduate) to work with such wonderful scientists, but I probably didn't.

I began reading about the CMBR right away but didn't know about general relativity or about cosmological models expect for very qualitative descriptions. So for me, my isotropy project soon became more a matter of getting the apparatus to work and to make a reliable measurement than of thinking about the cosmological consequences. I'm sure that others will make this same point, that is, that while the motivation of an experiment is extremely important, getting the experiment to work and making the best possible measurement usually takes over and determines the measure of success — at least in one's own mind. If this weren't the case, I suspect that experiments would not be as successful as they are nor would science advance as rapidly as it does. I do remember being extremely careful to track down all the possible sources of systematic error, something I'm sure I acquired from Partridge and Wilkinson, and that has held me in good stead as an experimentalist.

The instrument was an 8.6-mm radiometer used by Dave to measure the CMBR spectrum and the plan was to compare the temperature of two points in the sky separated by 90° on the celestial equator. The 90° separation was picked to maximize the sensitivity to a quadrupole signal in the CMBR — I believe that one motivation was a possible anisotropic expansion of the universe, but to an undergraduate these were just words. The real motivation was to do an isotropy experiment at a shorter wavelength than other experiments to see if the previously measured CMBR isotropy was independent of wavelength. At the time, extragalactic sources for the CMBR had not been completely ruled out and one might expect that these sources would exhibit anisotropy at higher frequencies. I'm sure some Big Bang enthusiasts might say we were wasting our time; however, such null tests are all part of the important "network of measurements" that validate any scientific model. It now seems hard to believe that the data were recorded on many, many rolls of Esterline-Angus, pen and ink, strip chart paper that were painstakingly analyzed by hand. Of course, we found no anisotropy at our level of sensitivity, about 0.4 percent, a respectable limit but certainly not the best at the time. The design of the size and shape of the reflector was left entirely up to me. Since the beam was required to switch by 90° on the sky, the reflector should be switched by $\pm 22.5°$, or so I thought. That would only be true if the beams were reflected in a direction perpendicular to the axis of the reflector. They were not. As a result the beam throw on the sky was 64°, a value that prompted several inquiries after our paper was published (Boughn, Fram & Patridge 1971). It was a both an embarrassing lesson for me and a testament to the involvement that Partridge and Wilkinson expected from their undergraduate students.

I assembled the radiometer in the Gravity Group haunt, a large area in the northwest corner of the basement of Palmer Laboratory, the home of the Physics Department. The place was an absolute maze with endless piles of equipment punctuated by several desks supporting stacks of papers that often spilled onto the floor. One evening I brought my two month old daughter with me while I worked on the radiometer. I tucked her in her carrier in a safe place among the jumble of apparatus as I worked on the radiometer. Some time later Dicke wandered in to search for something and uttered, "Well, what have we here!" as he happened, with delightful surprise, upon my daughter. During the day the place was a beehive of activity with three other undergraduates (Mike Smith, David Payne, and Bill Baron) working on CMBR related projects, one of which was the first attempt to measure the polarization of the CMBR; two senior graduate students (Paul Henry and Karl Davis), who were building more radiometers, one of which was to be the first balloon-borne instrument; three more junior graduate students (Ed Groth, Jim Cambell, and Dave Fram), who had just arrived at Princeton; three postdocs (Jer Yu, Paul Boynton, and Neil Rasband), who were also working on the CMBR; and our leaders, Dicke, Wilkinson, Peebles, and Partridge. The excitement was palpable and the community spirit of the quest ever present as everyone helped with each other's projects. It was no wonder that I had a somewhat inflated notion of the importance to physics of what was going on there.

By the time I left Princeton in the spring of 1969, I was finally beginning to learn a little general relativity and began to think a little more deeply about the CMBR. After reading some of the fundamental papers on gravitational lensing I began to wonder if perhaps gravitational lensing of the CMBR by massive galaxies might not result in some anisotropy. I don't know if I mentioned this to Wilkinson or Partridge, but if I did I suspect they would have told me (with a smile) to go away and think about it some more. I did and after a laborious calculation was surprised that everything canceled out and lensing could not, in fact, generate any anisotropy whatsoever. It seems there is something called the "brightness theorem" that is valid even in the presence of gravity. Even though I was beginning to think more deeply I was, alas, still a novice. It turns out that collapsing or expanding concentrations of mass can generate anisotropies in the CMBR via the gravitational redshift, as was predicted by Sachs and Wolfe (1967), but it wasn't until 3 years ago that I and others detected this "integrated Sachs-Wolfe effect" and found its amplitude to be consistent with the existence of a cosmological constant, another thread in the "network of measurements".

I was involved in two more rounds of anisotropy experiments, one in the late 1970s and the other in the late 1980s. These took place well after the decade that is the subject of this book; however, I think there is an important point regarding these and other null CMBR anisotropy measurements of that 25 year period. It's understandable that the measure of success of a given measurement was, in part, determined by the upper limit it set on the level of the fluctuations in the CMBR. However, it seemed that some people (usually theorists) in the field took these limits very seriously, distinguishing between experiments that yielded upper limits that differed by as little as 20 or 30 percent. Most experimentalists realize that differentiating observations on this basis is nonsense. I once conferred with a statistician about the best and most robust statistic that I should use to set an upper limit to CMBR fluctuations. He seemed mystified by my use of statistics and responded that what I should properly do is to report the sensitivity of my instrument and then say whether or not I detected anything. Setting upper limits, he maintained, is not a proper use of statistics. Hmmm.... I once asked Wilkinson about what I described as over-interpreting the statistics of null results. He said not to worry. This situation was just the result of anxious cosmologists biding their time until CMBR fluctuations were actually observed. Sure enough, this came to pass. However, I do see a hint of the same problem returning, with some people judging cosmological observations by their usefulness in reducing the errors on the various parameters of the currently favored cosmological model and paying scant attention to the diversity of those observations, a diversity that will be absolutely necessary in ushering in any new understanding of our universe.

I know now that cosmology certainly wasn't considered to be one of the important areas of physics research in the 1960s and I was well aware then that there were other great discoveries being made. The professor of my very first physics course at Princeton was Val Fitch, who had the year before discovered CP violation in particle physics, a discovery for which he and Jim Cronin would later receive the Nobel Prize. (The second semester of the course was taught by M.L. Goldberger, another notable figure in particle physics and future President of Caltech.) Yet, from my limited point of view (in the biased environment of Princeton's Gravity Group) I perceived cosmology as one of the most important and fascinating areas of all fundamental science. I still do.

Paul S. Henry: A Graduate Student's Perspective

Paul Henry is a Member of the Technical Staff of AT&T Laboratories. His thesis experience at Princeton sparked a lifelong career in telecommunications research, which continues to this day.

The last thing I remember from that day is Dusty Rhoads and Gene DeFreece depositing me at my motel room. Dave Wilkinson, my thesis advisor, was waiting for me and was none too happy. *Where in hell have you been?* he demanded. *I've been looking all over for you. We have an interview with the Hobbs paper in half an hour.*

Not now, Dave, please. You can cover for me. And at that point I passed out.

So that's what it had come to. After four years of grad school — most of them spent laboring in Bob Dicke's Gravity Research Group (the "GRG"), preparing for what was supposed to be a definitive experiment in observational cosmology — I had ended up in a Hobbs, New Mexico motel, tanked to the gills with 190-proof Everclear grain alcohol.

The trouble had begun about two years earlier, when Dave and Paul Boynton (a postdoc in Dicke's group) recognized that ground-based measurements of the isotropy of the CMBR would forever be plagued by atmospheric effects, especially emissions due to water vapor. High altitude was the key to success. After rejecting satellite- and aircraft-based platforms, they concluded that a balloon-borne experiment, flying above 99% of the water vapor in the Earth's atmosphere, could be a cost-effective approach. A low-noise, wideband radiometer, slowly rotating to scan the sky, could collect enough data in a 10-hour flight to yield a good estimate of the 24-hour (dipole) anisotropy of the CMBR. (Ned Conklin and Ron Bracewell at Stanford University had already done a lovely experiment to measure the equatorial component of the anisotropy [Conklin 1969], but the polar component, and therefore the total magnitude, was still unknown.) No one in the GRG had ever worked with research balloons before, but Dave had discovered that a federal agency, the National Center for Atmospheric Research (NCAR), could provide the equipment and services needed to conduct high-altitude research experiments. He asked me to dig a bit deeper.

I had come to Princeton solely because a college professor had mentioned Dicke's work in cosmology as proof that you didn't need to be in the then-fashionable mainstream of big-time, high-energy physics in order to do exciting research. Believing then, as I do now, that less is more, this comment sounded like high praise to me. I had zero knowledge of cosmology (Who's Hubble?); it was the possibility of doing small-scale experimental physics, not cosmology *per se*, that excited me. The experiment proposed by Dave and Paul was just what I had been looking for.

The next thing I knew, I was up to my eyeballs in the details of microwave radiometers, thermal design, telemetry, and failure-mode analysis. Dave and Paul apparently trusted me to manage the project on my own, or suspected that it was going to be a huge sink of time they'd rather not invest. Either way, they gave me more than enough rope to hang myself.

As I labored through the endless details of design, construction and testing of my equipment, I began to sense that my little project, though extremely modest by the usual standards of experimental physics, was part of a much bigger picture, a brewing revolution in cosmology. Time and again, I'd overhear the big shots of the GRG (to me, anyone with a PhD qualified as a big shot) discussing things like scalar-tensor gravitation, helium formation, and radiation decoupling. Something momentous was clearly in the works. They'd invite me to join their conversation, but I always demurred. With the myopia typical of so many grad students, I was happy to let the experts debate deep cosmological matters — all I wanted to do was get on with my experiment so I could satisfy Dave and get my degree. Truth be told, I was in heaven with my thesis project and wanted nothing more. Every circuit design, every test run was a labor of love. Long hours in the lab, deep into the night, gave me a sense of accomplishment that I had never known before. Leave the big picture to the experts; in my own small way, by performing the GRG's first balloon experiment, I was going to be a pioneer too. That was enough for me.

As with any experiment, almost nothing went smoothly. In most cases I managed to deal with problems as they arose, but occasionally I'd get stuck. That's when Dave would step in to save me. My gondola design, for example, was as light as I could possibly make it, but even so it turned out to be far too heavy for the balloon we planned to use. I was stumped. Dave made some calls, found an expert in lightweight structures who could help me with a re-design, and arranged for a fabrication shop to do the construction. One problem solved. Alas, countless more to go.

One by one, the problems yielded. The most memorable part of my graduate experience was about to begin. In late fall of 1969 I packed up my experimental apparatus along with a bunch of test gear and took it to the NCAR balloon base in Palestine, Texas, where I was greeted by Dusty Rhoads, the facility supervisor. He was friendly, but obviously very busy with other research groups preparing their own balloon experiments. He showed me to my assigned work-space, assured me that if I needed help, I could ask any of the staff, and then left to tend to one crisis or another.

Despite Dusty's assurances, and despite the dozens of people working around me in that same building, I felt alone and, for all intents and purposes, totally lost. Luckily for me, Paul Boynton had arranged to travel to Palestine to help me get started. He met me the next morning, and together we unpacked my equipment and ran some initial tests that confirmed that the radiometer was in good shape. He could only stay a couple of days — for the rest of my time in Palestine I'd be on my own — but before he left he introduced me to Rai Weiss and Dirk Muehlner, who had the work-space next to mine, and arranged to have them "babysit" me, to be available to help should an emergency arise. They were preparing their own CMBR experiment, an effort far more sophisticated and ambitious than mine. On the one hand, I was delighted to be hooked up with guys who, unlike me, actually knew what they were doing. But on the other, I was utterly intimidated by their clear command of just about everything I wished I knew, but didn't. In any case, I needn't have fretted. Not only were Rai and Dirk always eager to offer advice and assistance whenever I needed it, it turned out that they were delightful dinner companions as well. We were all staying at the Sadler Motel, and as there weren't many eateries in Palestine, we usually had dinner together at the motel restaurant. The menu was limited — basically your choice of fried chicken, chicken-fried steak or chicken-fried pork chop — but the meals were a treat all the same. Rai was a gifted raconteur. Drawing from a bottomless well of stories about goings-on in the MIT physics department — tales too scandalous (and delicious!) to repeat here — he kept us enthralled for hours.

As I continued preparations at the balloon base, I was delighted to discover that, despite my hurried introduction to the facility by Dusty Rhoads, both he and his entire staff were, in fact, absolutely committed to "customer service" for their visiting researchers. One day, for example, Earl Smith from the electronics shop stopped by to ask how things were going. I was generally in pretty good shape, I told him, but I had some concerns that a telemetry interface box I had built wasn't compatible with NCAR's equipment. I would have to re-design and re-build much of it on-site. He asked a few more questions, wished me well, and went back to his shop. The next morning he presented me with a new telemetry package that he had specially modified to be compatible with my experiment. I didn't have to do any re-design at all — just plug and play. Earl was typical of the entire NCAR staff. They would recognize when help was needed, step in without overstepping, and do whatever it took to move a project forward.

After a week of fixing one problem only to find yet another, flight day arrived, though not by my choice. Dusty told me the evening weather forecast was for clear skies and calm surface winds — conditions too good to pass up; I was to be ready for launch at sunset. (I needed a night flight to avoid microwave radiation from the sun.) He introduced me to Gene DeFreece, the launch director, who ran through a final check-list. He nodded with approval when he saw that I had followed Rai's advice and secured a six-pack of beer in the telemetry section of the gondola. Over the course of the night at 75 thousand feet, the beer would chill down to a temperature just right for drinking by the recovery crew, who would have followed the path of the balloon in a pickup truck, in order to retrieve the gondola after it parachuted to Earth. A little gesture to repay — or at least acknowledge — the courtesy that the NCAR staff had shown me over the past week.

Shortly before sunset Gene and his crew mounted my gondola on the launch truck and secured it to the huge, uninflated balloon stretched out along the ground. From the launch truck, he ordered his team to start filling the "bubble" at the top of the balloon with helium. I stood with Dusty at the edge of the launch pad as he communicated via walkie-talkie with Gene and the staff in the telemetry room, who were synchronizing their equipment with the signals from my radiometer. When Dusty got word from Gene that the balloon was ready, he turned to me: "Are you ready, Paul?"

"I don't know."

"We can't launch till you say you're ready."

"How do I know when I'm ready?"

... A long pause ...

"You are now," he decided for me. "Let it go," he radioed to Gene.

Seconds later the bubble slowly started to lift the huge polyethylene bag connecting it to my gondola on the truck. As it climbed above the truck, the driver followed underneath until Gene sensed that the bubble was pulling up hard enough to support the payload, at which point he cut the gondola free. Launch was complete.

After the stress of the launch, the rest of the flight was an anticlimax. I worked in the telemetry room, monitoring the radiometer, performing periodic calibration runs, and following the track of the balloon as it drifted east. The experiment functioned beautifully throughout the night. As expected, shortly after dawn solar radiation appeared in the radiometer output; no more useful data could be taken. Dusty ordered cutdown. The pickup crew found the gondola immediately after it landed, reported that the beer was just right, and delivered the payload to Palestine that evening.

Dave had built enough money into our project budget to pay for a second flight if it should be needed, so I left my equipment in Palestine — just in case — and returned to Princeton to analyze the data. It didn't take me long to discover that all was not well. I traced an unexpected periodic component in the data to radiation from the Moon. The screens I had designed to shield the radiometer from this effect had apparently been inadequate. The effect was too small to have been seen in sea-level pre-flight tests, but in the near-perfect conditions at altitude it was clearly visible and strong enough to render much of my isotropy data useless. I asked Dave for permission for another flight. He agreed, and also volunteered to come along to help. (Or maybe to ensure that I didn't screw up again!) This time, we scheduled our flight for a moonless night.

I designed a new screen system and did some quick tests. Meanwhile, the NCAR balloon operation had moved to its winter site in Hobbs, New Mexico, a few hundred miles west of Palestine, in order to accommodate stronger high-altitude winds. They had brought my apparatus from Palestine, so when Dave and I arrived in Hobbs, everything was ready for us. I constructed a screen according to the design that I had developed in Princeton, but as soon as we started testing, Dave spotted problems with it. Moonless night or not, the screen had to be right. I couldn't explain why it wasn't working, but Dave suspected diffraction from the screen edge — a problem he had seen in some of his earlier CMBR experiments. He re-positioned the edges and the trouble disappeared! A problem that could have held up launch for days was dispatched in an hour.

Launch day for flight #2 arrived. I was as stressed for this one as I had been for flight #1, but this time I at least had the courage to say "Go." And go we did. The flight was uneventful — minor problems with thermal control and telemetry, but the backup systems took over and got us through the night. The next morning, after cutdown, we all felt pretty satisfied. The balloon crew wanted to celebrate, as they often did after a successful flight, but Dave and I were exhausted and just wanted to sleep. Dave managed to excuse himself and headed back to the motel, leaving me at the mercy of Dusty & Co. Half asleep, I vaguely recall riding with them for several miles until we came to a roadhouse that appeared to be one of their favorite spots. Inside, at what I guess was their usual table, we were welcomed by the waitress, who was clearly glad to see us and greeted the crew members by name. Pointing to me, Gene told her, "He'll have an orange blossom." I had no idea what that was, but when it arrived it looked like a glass of orange juice, so I downed it in a gulp and asked for another. A few minutes later the table exploded in laughter when I reported that the right side of my face was numb. I don't know how long I continued to provide amusement for the group, but apparently Dusty and Gene finally decided that they had helped me celebrate enough, and took me back to my motel, where Dave was seething. As he greeted me with a few choice words of disapproval, he no doubt realized that my plea to skip the interview with the "Hobbs Daily News" was in everybody's best interest, so he went without me.

One hangover and several airline connections later I was back in Princeton with stacks of telemetry tape, followed shortly by my radiometer, which arrived via truck. Over the next nine months I processed data, analyzed instrumental quirks that I should have spotted long before, and tracked down anomalies. I had to throw out some suspect data, but fortunately there was enough left over for my thesis. And what did I have? I had the first measurement of the polar component of the anisotropy of the CMBR, as well as its total magnitude. As Conklin and Bracewell had concluded before me, the radiation is not isotropic. There is a hotspot in the sky. But it is no hotter than could be accounted for by the likely rotation of the local supercluster of galaxies, of which our galaxy is a member. Our confidence that the CMBR is a cosmological phenomenon, not just a local effect, notched up a bit.

My work was good enough for a thesis and a brief piece in the journal *Nature* (Henry 1971). I felt relieved, and lucky to be finished. I knew my experiment had been far from perfect — there were many things I could have done better. I was thankful to Dave for showing mercy in my time of need. It wasn't until a decade later that I sensed that maybe my work had not been so bad after all. I was visiting the Electrical Engineering department at Stanford, where I passed a display case featuring Ron Bracewell's long career. There was a large collection of papers, propped open to key pages, showing a few of his many contributions. One of them showed the isotropy measurements that he had made with Ned Conklin. And there, right beside it, opened to Figure 1, was my *Nature* paper. I was in good company.

George F. R. Ellis: The Cosmic Background Radiation and the Initial Singularity

George Ellis is Professor Emeritus of Mathematics at the University of Cape Town, where he has run a relativity and cosmology research group since 1973. He continues to write on relativity theory and cosmology, but also nowadays writes on the emergence of complexity and the way the human mind works.

The Department of Applied Mathematics and Theoretical Physics (DAMTP) at the University of Cambridge was lucky to get Dennis Sciama as a University Lecturer in 1961. His passionate love of physics, astrophysics, and cosmology was matched by his enthusiastic understanding that what mattered most in a research group was finding and supporting bright students, who were where the future of the subject lay. Furthermore, as was true also of John Wheeler, he believed that some of the deepest advances would come from describing physical effects through precise mathematical formulations, combining a good understanding of physical effects with a knowledge of the latest mathematical techniques. Thus as well as encouraging the kind of approximation techniques that lie at the heart of much physical understanding, he also encouraged the search for exact mathematical theorems that could express important physical results. But he insisted that theory should have relevance to the real world, so one should explore all possible observational aspects and subject them to rigorous test. Theorems on black holes or cosmology were little use without some link to possible testing by astronomical observations. He would always push one on this point: How is it observable? How do you test it?

A key issue at that time was the possible existence of space time singularities: whether they occurred at the start of the universe on the one hand, and at the endpoint of gravitational collapse of astronomical objects on the other. In both cases simple general relativity models with exact symmetries indicated there was indeed a spacetime singularity, and John Wheeler (1964) in particular emphasized that this was a major crisis for theoretical physics, because such a singularity indicated a beginning or end not just to space, time, and matter, but also to physics itself, and hence represented the limit of physical understanding. The question was whether the implication of a singularity was a result just of the simplified models used, which excluded rotation for example, and so would go away if more realistic models were used. This was precisely the kind of area where the combination of new mathematical techniques with deep physical insight could be expected to pay off. One route was investigating the occurrence of singularities in spatially homogenous but anisotropic universe models, and (inspired by Dennis and informed particularly by Engelbert Schücking when he visited King's College, London) Stephen Hawking and I obtained useful results in this regard (Hawking and Ellis 1965), as did Larry Shepley (1965), who was working under Wheeler's guidance. However these were rather special models and did not include the effects of inhomogeneities.

The *annis mirabilis* for the subject was 1965 — the same year the cosmic microwave background radiation was discovered — when Roger Penrose (1965) published his extraordinarily innovative paper on the existence of singularities at the end of gravitational collapse. Working very much on his own, he combined methods from topology, geometry, and analysis to show singularities would occur under realistic astrophysical conditions, provided a generic energy condition was satisfied by all matter and fields present. The key geometric concept he introduced was a *Closed Trapped Surface*: a 2-sphere in spacetime such that light emerging and expanding outwards from the sphere had an area that decreased with time, instead of increasing as happens in flat spacetime. This would generally be associated with the existence of an event horizon, whose formation would show a black hole had come into being. When such a closed trapped surface existed and some auxiliary conditions were satisfied, it was inevitable that a singularity would occur; and these conditions would be likely to occur in gravitational collapse situations, because they occur in Schwarzschild spacetimes. Because the requirements of satisfying the energy conditions and existence of a closed trapped surface are both *inequalities*, they can occur in realistic real-world situations: they are stable to perturbations of the model. Thus his theorem gives the needed kind of generalization of previous results from special geometries to generic situations: these conditions implied existence of a singularity, a spacetime boundary.

The paper, while clearly written, was obscure both because it was very brief, and because it introduced a combination of new mathematical techniques into general relativity studies that were not in common use at the time. This led to a flurry of activity in which relativity research groups like that at DAMTP in Cambridge, including Sciama, Brandon Carter, Stephen Hawking, and myself, and that at King's College London, including Herman Bondi and Felix Pirani, scrambled to get up to speed. We ran a series of joint seminars in London and Cambridge where we explained to each other what Penrose had done and the underlying mathematical ideas, with useful input from others such as Bob Geroch and Charles Misner. Penrose himself of course also gave seminars on the topic, but one needed more background than we had at that time to comprehend fully what he had done.

Stephen Hawking's first major insight was that a closed trapped surface

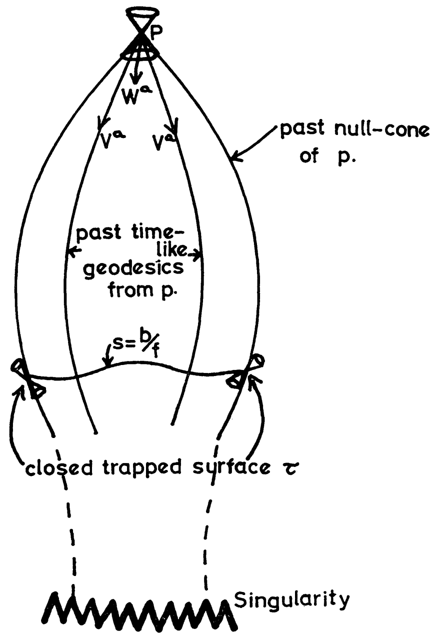

Figure 50: Past-directed timelike geodesics from p starting to converge again before the surface $= b/f$ [which may be chosen so that the primeval plasma has combined to atomic hydrogen and helium, making space close to transparent to the CMBR].

would occur in a time-reversed sense in the early universe, and this could be used to extend Roger Penrose's theorem to the cosmological context. This followed from Fred Hoyle's discovery that there would be a minimum angular diameter for the observed size of an object of fixed size as it was moved back to earlier and earlier times in cosmology. The result was codified in Alan Sandage's (1961) magisterial paper on using the 200-inch telescope at Palomar for cosmological observations, and so was by then well known. This feature is obscured in the usual conformal diagrams used to indicate causal relations clearly; it is obvious when one uses proper distance coordinates instead, revealing the true onion-like shape of the past null cone shown in Figure 50 (from Hawking and Ellis 1968; see also Ellis and Rothman 1993).

The figure shows that the past light cone refocusses as one goes back into the past, hence there are indeed time-reversed closed trapped surface in the standard Friedmann-Lemaître models of cosmology. But this means they will also exist in perturbed such models, implying closed trapped surfaces exist also in these more realistic universes, and so implying an initial singularity in these cases too. Stephen produced a series of theorems that applied specifically to the cosmological situation: under various slightly different sets of conditions, a universe that is expanding and filled with matter and energy obeying a physically acceptable equation of state must have have had a beginning sometime in the past, regardless of any lack of symmetry today. In his words, "...time has a beginning."

This is a pretty important conclusion, so one should consider it carefully: this is a convincing argument for an initial singularity, but it is not an observational proof. What would be a good observational link? Enter here the CMBR and its recently discovered importance for cosmology. I do not recall specific seminars on the topic, but obviously it would have been widely discussed. One of the key things that Dennis insisted on is that a research group should have a fixed coffee time each day when they could regularly meet together, providing a natural setting for discussing the latest papers and data with each other. Coffee tables were provided with white tops intended to be written on: at hand would be pens and table-top cleaners. Dennis would often come in at coffee time clutching some new scientific paper in his hand and ask, Did you see this? What does it mean? And the same would have happened with the CMBR. Dennis immediately understood the significance for cosmology of the CMBR and its interactions with matter, and in particular how it provided evidence for a hot early state of the universe.

But did it provide evidence of an actual singularity? The realization came from discussions *inter alia* with Dennis, Martin Rees, and Bill Saslaw that, unlike matter, the CMBR permeated *all* spacetime and so was a dominant dynamical feature not only in the early universe but in empty space in the recent universe. Hence one could see if it alone would imply existence of a singularity, a great advantage being that its nature and density were well understood, while that of the matter was much less clear: was it hot or cold, molecular or ionised for example? And just what was its density and atomic composition? What fraction was uniformly distributed and what fraction clumped? The beauty of theory based on the CMBR was that its nature was so simple: being blackbody, just one number sufficed to determine all its properties. And its high degree of isotropy showed it was very smoothly distributed.

Stephen asked me to help work this out. He had developed a singularity theorem where by imposing a slightly stricter local condition than existence of a closed trapped surface (*the reconvergence condition*: there was reconvergence in the past of all timelike geodesics through one spacetime point), he relaxed a global requirement of Penrose' original theorem: existence of a Cauchy surface (a spacelike surface that intersects every timelike and null curve in the universe). This auxiliary requirement was reasonable in the context of local gravitational collapse, but not in the context of cosmology. The question was if we could show the reconvergence condition was true in a realistic universe model. We knew a lot about the geometry of timelike curves and null surfaces, and particularly the focussing power of matter — this had been made clear by Ray Sachs, Jürgen Ehlers, and Roger Penrose. So could this radiation alone imply sufficient focussing to imply the reconvergence condition? Yes it could. Its near isotropy showed the universe was almost Robertson-Walker in the observable region,[27] so one could base one's calculations on such a geometry. Then where would reconvergence take place: before the last scattering surface, and so in the domain we can actually see, or beyond it, and so in the domain inaccessible to direct observation, where the universe might be quite unlike a Robertson-Walker geometry? Stephen's insight was to note that either (if the matter density was low) the radiation itself would cause the needed refocussing at a low enough redshift, or else (if the matter density was high) the matter that thermalised the radiation, as implied by its very accurate blackbody spectrum, would do so. In both cases reconvergence would occur within the universe domain we can see, and so where we pretty much understand the broad nature of the geometry; and this would be true even if the radiation were re-scattered by reheated intergalactic gas at relatively recent times. The result, being an inequality, would remain true if the universe were not exactly Robertson-Walker, but something like it in the observable domain, and possibly quite unlike it in the hidden domains at very early times (rotation and shear might dominate there, for example). So the mere existence of the CMBR with near isotropy and a thermal spectrum would do the job of ensuring reconvergence, without requiring exact symmetries of the spacetime and without knowing details about the matter present. Thus Hawking's exact mathematical theorem applied to the real universe if the matter obeys the energy conditions, because the existence of the CMBR shows its geometrical conditions are satisfied. The CMBR alone would therefore show that a cosmological singularity — a start to spacetime — must exist (Hawking and Ellis 1968,).

Dennis was very pleased with this result, which was followed by a similar but more elegant calculation given in the summary volume *The Large Scale Structure of Spacetime* published in 1973 (see Hawking and Ellis 1973, Section 10.1). It was of course recognized at the time that this result must

[27]See Section 4.2.2 of Ellis 2006 for a recent discussion and references.

not be taken too literally: it is based on general relativity theory, which will break down if a quantum gravity epoch is encountered in the early universe. Indeed the implication was generally taken to be that what was predicted in physical terms was that a quantum gravity epoch could not be avoided in the early universe: effects like rotation could not turn the universe around at fairly early times so that it never entered such a domain. Whether quantum gravity effects would avoid a singularity in the quantum gravity epoch was unknown (and remains so to this day, but with some evidence from loop quantum cosmology that this is possible).

Does the result still hold today? Not really. Guth's (1981) introduction of the inflationary universe idea established that an effective scalar field could be a plausible dominant form of energy in the very early universe, that would violate the energy conditions of the singularity theorems; indeed this is what makes the accelerating expansion of the inflationary epoch possible. This has become the dominant paradigm of present day cosmology (e.g. Kolb and Turner 1990; Dodelson 2003). The implication of singularity existence therefore no longer follows, because one of the major conditions for the singularity theorem to hold is no longer believed to be true: it fails at times when quantum field effects dominate. Indeed it is possible not merely that there was no singularity, but even that there was no quantum gravity domain: the universe might never have reached the densities needed for such a domain to occur. Explicit examples of Eddington-Lemaître type universes where this is the case can be found in Ellis and Maartens (2004). These examples have been criticized because they start off in a rather special state (at very early times they are asymptotic to an Einstein static universe); and indeed the choice may ultimately be between a space-time singularity or a very special initial state. Which is more undesirable is a philosophical argument; whether one can observationally discriminate between them is an open question.

However that argument works out, acceptance of the inflationary universe kind of dynamics means that the existence of the CMBR no longer necessarily implies the existence of a singular initial state at the start of the universe. Its existence does however still imply not only that there was a hot big bang era in the early universe, but that this era extended back till times when quantum fields became dynamically dominant. That remains an important conclusion.

Chapter 5. Bond and Page: Cosmology since the 1960s

Glossary

Abell, George O. (134, 232) Author of a widely used catalog of rich clusters of galaxies (Abell 1958).

absolute magnitude (45) measure of the luminosity — the rate of emission of energy — of an astronomical object. The measure is logarithmic, such that a difference of five magnitudes is a factor of 100 difference in luminosities. The sign is set so that the fainter the object the larger the absolute magnitude. At 10 pc distance the absolute magnitude of an object is equal to its apparent magnitude.

active galactic nucleus (63, 161, 162, 181, 205) Source of radiation and energetic particles in the center of a galaxy, likely powered by flow of matter around a massive black hole.

acoustic peaks *See* CMBR angular power spectrum.

adiabatic initial conditions (246) Density fluctuations in the very early universe that would be produced by adiabatically compressing and decompressing parts of an initially exactly homogeneous universe. That leaves a spatially uniform entropy per particle.

aether drift *See* CMBR dipole anisotropy.

AGN *See* active galactic nucleus.

Alpher, Ralph A. (27, 28 – 33, 42, 43, 60, 78, 92, 119, 122, 127, 129, 166, 204).

AMI (188) Arcminute Microkelvin Imager for measurements of CMBR temperature variations on small angular scales.

antenna temperature (84, 108, 186) Microwave energy flux from the antenna of a radiometer, measured in equivalent Rayleigh-Jeans temperature.

apparent magnitude (45) A logarithmic measure of the brightness in the sky (the rate of arrival of energy per unit area) of an astronomical object. Neglecting relativistic corrections and obscuration, the apparent magnitude of an object with absolute magnitude M at distance D measured in megaparsecs is $m = M + 5\log_{10} D + 25$.

Applied Physics Laboratory *See* the Johns Hopkins University.

atmospheric noise (40 – 43, 53, 55, 105, 105, 117, 190, 196, 203, 213 – 230, 261) Electromagnetic radiation from the atmosphere. *See* tipping experiment.

back and side lobes (178, 186, 224) A measure of the response of a radio telescope to radiation incident from directions well away from the main direction of observation. *See* ground noise.

baryon mass density (24, 30, 155) The standard estimate of the present value (Table 1 page 21) is $\rho_{\rm b} = 4.2 \times 10^{-31}$ g cm^{-3}.

baryonic matter (22) The neutrons and protons in the nuclei of hydrogen and the heavier chemical elements, along with enough negatively charged electrons to balance the positive electric charge of the nuclei. Other unstable forms of baryons need not concern us.

beam switching (254) *See* Dicke switch, phase-sensitive detection.

Bell Laboratories (39, 43, 81 – 91, 96 – 113, 114, 117, 118, 122, 124, 128, 139 , 144, 185, 189, 282) Bell Laboratories communications experiments led to a convincing detection of the CMBR.

Bell Laboratories 20-ft horn reflector at Crawford Hill (54, 73, 83, 85, 97).

big bang cosmology (7, 18, 18, 45, 65 – 68, 89, 94, 110, 118, 122, 123, 127, 137, 144, 148, 153, 159, 161, 240, 163, 166, 167, 170, 252, 277) A near homogeneous and isotropic expanding model universe described by general relativity theory with standard local physics. The name is unfortunate because a "bang" suggests a localized event, such as an explosion, while the model deals with the evolution of a near homogeneous universe expanding from very high density.

big crunch (45) End of the universe as we know it in a gravitational collapse to exceedingly large density.

big freeze (45) End of the universe in expansion continuing into the indefinitely remote and empty future.

black hole (22, 78, 161, 289) The general relativistic prediction of the singular state approached by the collapse of a mass concentration such as that observed in a dying star or in the center of a galaxy.

blackbody radiation *See* thermal radiation.

Bolton, John (95, 101)

Boltzmann constant (31, 319) The constant $k = 1.38 \times 10^{-23}$ J K^{-1} $= 1.38 \times 10^{-16}$ erg deg^{-1} that relates a temperature T to its characteristic energy kT.

Bondi, Hermann (8, 18, 47, 44 – 49, 67, 115, 157, 164, 170, 188, 202, 240, 252, 289) Biography: Mestel (2005).

BOOMERANG (250) Balloon Observations of Millimetric Extragalactic Radiation and Geophysics (Lange *et al.* 2001).

bouncing universe *See* oscillating universe.

Boughn, Stephen (277 —281).

Boynton, Paul (194 – 198, 258 – 267, 279, 282, 284).

Bracewell, Ronald N. (268 – 276, 282, 287, 287).

Brans-Dicke theory *See* scalar-tensor theory.

brightness theorem (279) In standard local physics, the flow of energy in a beam of free radiation per unit area, time, solid angle and frequency interval varies as $i_\nu \propto (1 + z)^{-3}$, where the frequency of a packet of the radiation varies as as the redshift factor, $\nu \propto (+z)^{-1}$.

Burke, Bernard F. (60, 87, 92, 109, 114 – 121, 122, 128, 146, 211, 260).

Burbidge, Geoffrey A. (46, 47, 49, 65, 155, 163 – 167, 249).

California Institute of Technology (65, 83, 95, 116, 130, 153, 202, 263, 280)

Caltech *See* California Institute of Technology.

Cameron, Alistair G. W. (47, 154) Pioneered, with Burbidge, Burbidge, Fowler and Hoyle (1957), the theory of nucleosynthesis in stars.

Cassiopeia A (84, 88, 101, 108, 115, 185) A supernova remnant that is the strongest radio source in the sky and outside the Solar system.

CAT (188) The Cambridge Cosmic Anisotropy Telescope for measurement of angular variations of the CMBR on scales $\sim 0.5^\circ$.

C-band (118) Microwave radiation in the frequency range 4 to 6 GHz.

CDM model (21 – 25, 79, 162, 249, 250) Big bang cosmology in which the mass in matter is dominated by nonbaryonic near collisionless cold dark matter. The primeval departure from homogeneity is adiabatic, near Gaussian and near scale-invariant. With the addition of Einstein's cosmological constant this model is the standard ΛCDM cosmology. In this model the mean mass densities of baryonic and cold dark matter, dark energy, the CMBR and the relict thermal neutrinos at the present epoch are in the proportions indicated in the table on page 21.

celestial equator Projection of Earth's equator onto the celestial sphere.

Clausen, Carl (85, 99)

clusters of galaxies (81, 101, 134, 162, 188, 231) The largest gravitationally bound concentrations of galaxies. In Abell's (1958) definition the mean number density of galaxies within a sphere of 2 Mpc radius is greater than about 100 times the global mean density.

CMBR (7, 13, 19 – 21, 23, 37, 39, 42, 123, 253, 277) Also termed the CBR: the thermal cosmic microwave background radiation at temperature 2.725 K that nearly uniformly fills space. Outside opaque bodies the universe at its present temperature contains 420 CMBR photons per cubic centimeter. Half of the energy density in this radiation is at wavelengths less than $\lambda = 1.50$ mm. The measurements that demonstrate that the spectrum of the CMBR is very close to thermal were independently obtained at almost the same time by Mather *et al.* (1990) and Gush *et al.* (1990). The measurements are shown in Figure 2 on page 19.

CMBR angular power spectrum (169, 246 – 248, 250) The mean square fluctuation of the CMBR temperature across the sky as a function of angular scale. The spectrum is

$$C_l = \frac{l(l+1)}{2\pi}\langle |a_l^m|^2\rangle, \tag{20}$$

where the expansion coefficients a_l^m are defined in equation (21). The spectrum C_l is close to the temperature variance per logarithmic interval in l (or of angular scale π/l). In the standard cosmology the oscillation of the power spectrum as a function of l reflects the pressure or acoustic oscillations of the coupled CMBR and plasma, which behave like a single viscous fluid prior to recombination.

CMBR anisotropy (111, 113, 141, 162, 181, 247 – 250, 253, 263, 264) The inhomogeneity in the CMBR spatial distribution observed as the variation in the radiation temperature and polarization across the sky. *See* CMBR dipole and quadrupole anisotropy.

CMBR cosmic ray drag (128) Dissipation of the kinetic energy of an energetic cosmic ray proton or other particle by collisions with the CMBR photons that are energetic in the rest frame of the cosmic ray particle.

CMBR dipole anisotropy (148, 142, 181, 270 – 276, 254, 278, 282, 287) The fractional front-back temperature difference $\delta T/T \simeq 2v/c$ largely caused by our motion at speed v relative to the rest frame defined by the CMBR.

CMBR local source model (132, 159, 162, 165, 168 – 169, 173, 262, 278) The microwave radiation incident on the Earth has a small contribution from the Milky Way and other galaxies. An idea under discussion in the 1960s was that these sources produced all the CMBR. This is now convincingly ruled out by the cosmological tests.

CMBR polarization (279) The electric and magnetic fields of a beam of electromagnetic radiation are perpendicular to each other and to the direction of propagation of the radiation. In linearly polarized radiation the radiation intensities are systematically different for electric field directions parallel and perpendicular to the plane of polarization. The CMBR has a small linear polarization caused by scattering by free electrons.

CMBR publications history (266)

CMBR quadrupole anisotropy (254, 278) The term following the CMBR dipole in a spherical harmonic expansion — to successively smaller angular scales — of the variation of the CMBR temperature across the sky.

CMBR spectrum (19, 140, 169, 180, 186, 189, 194, 199, 211, 258) The radiation energy density in the CMBR as a function of wavelength. The measured spectrum is very close to thermal, as shown in Figure 2 on page 19. To see why the expansion of the universe preserves this spectrum suppose the universe is periodic in some large volume and decompose the electromagnetic field into the modes of field oscillation that fit in this universe. At thermal equilibrium at temperature T the occupation number $\mathcal{N}$ (the mean number of photons) in a mode with wavelength λ, frequency $\nu = c/\lambda$, is function of the quantity $h\nu/kT$ (as shown in eq. [25] on page 319). As the mode expands with the general expansion of the universe the wavelength is stretched as $\lambda \propto a(t)$, where a is the expansion factor (eq. [2] on page 20). When we can ignore the interaction of the radiation with matter — an excellent approximation at low redshift — $\mathcal{N}$ does not change: the photons are stuck in the mode. So as λ increases the mode temperature decreases as $T \propto 1/a(t)$ (as in eq. [5]). Since all modes cool in this same way the radiation remains thermal. *See* thermal radiation.

CMBR spectrum anomaly (121, 134, 199, 211) From the late 1960s to 1990, apparent evidence that the CMBR spectrum is significantly different from blackbody.

CMBR spectrum peak (61, 198, 211 – 230, 258, 259, 262) *See* Wien peak.

CMBR spherical harmonic expansion The components a_l^m in the expression for the CMBR radiation temperature (or polarization) as a function of angular position θ, ϕ in the sky in the spherical harmonic expansion

$$T(\theta, \phi) = \sum a_l^m Y_l^m(\theta, \phi). \qquad (21)$$

The angular distance between zeros of the spherical harmonic $Y_l^m(\theta, \phi)$ is close to π/l (except near the poles, where the zeros of Y_l^m for $m \neq 0$ crowd together, but where this happens the value of Y_l^m is very small).

CN *See* cyanogen.

COBE (7, 113, 143, 170, 190, 219, 220, 235, 242, 247, 259, 274, 307) The Cosmic Background Explorer satellite mission. The FIRAS experiment on COBE first demonstrated that the CMBR spectrum is thermal (Mather et al. 1990). The DMR experiment first detected the small departure from isotropy, $\delta T/T \sim 1 \times 10^{-5}$ on angular scales of a few degrees, produced by the interaction of the radiation with the growing departures from a homogeneous mass distribution (Smoot et al. 1992). The DIRBE experiment probed the cosmic infrared background radiation (Dwek et al. 1998).

cold big bang cosmology (32, 33, 34, 70, 89, 126, 132, 172 – 174, 243) A relativistic big bang cosmological model with a low initial temperature. Excessive element formation would be prevented by the postulate of neutrino degeneracy, or of a large gravitational interaction at high redshift that makes the rate of expansion and cooling of the early universe much larger than in the standard model.

cold dark matter Nonbaryonic matter with small cross sections for collisions with itself and baryons, and cold in the sense that the primeval velocity dispersion is small. *See* dark matter.

cold load *See* load.

Columbia University (81, 114, 137, 193)

Coma Cluster (23) The nearest rich cluster of galaxies, at 100 Mpc distance.

comoving observer (14, 263) Motion such that the galaxies are observed to be receding in the same way (apart from local departures from homogeneity and isortrsopy) in all directions.

Compton scattering (131) Electron-photon scattering at energies large enough to produce a significant photon frequency shift.

Conklin, Edward K. (268 – 276, 282, 287)

continual creation (44, 47, 67, 68) The assumption in the steady state cosmology that matter is spontaneously created at a steady rate that is on average independent of position.

Cornell University (116, 153, 199, 223, 244)

cosmic background radiation (13, 19 – 21, 26, 28 – 51, 71, 78, 101, 125, 204 – 209, 244) The sea of intergalactic radiation produced by all sources.

cosmic equation of state *See* equation of state.

cosmic microwave background radiation *See* CMBR.

cosmological constant (14, 17, 45, 280) The constant illustrated in Figure 1 on page 13 and appearing in equation (4).

cosmological density parameter *See* density parameter.

cosmological principle (13, 44, 126, 135, 171, 232, 240) The assumption that the observable universe is close to homogeneous and isotropic. In the standard cosmology the mass distribution averaged over the Hubble length departs from homogeneity by about one part in 10^5.

cosmological redshift (15, 23, 44) The measure in equation (2) on page 15 of the wavelength shift resulting from the general expansion of the universe. The cosmological redshift is the integrated effect of the Doppler shifts seen by a sequence of observers along the line of sight who sample the light as it moves from source to detector.

cosmological singularity (37, 288 – 293) In general relativity theory, the singular start of expansion of the universe. *See* singularity theorems.

cosmological tests (51, 280) The network of astronomical and laboratory tests of the cosmological models.

cosmology (9, 7, 13, 44 – 51, 87, 109, 185, 240, 243, 283) In this book, the study of the structure and evolution of the expanding universe on scales ranging from about 10 kpc to the Hubble length. The study of the structures of galaxies on scales smaller than about 10 kpc might better be termed extragalactic astronomy.

Crawford, Arthur B. (53, 54, 83, 97, 99)

Crawford Hill (54, 55, 83, 87, 96 – 113, 189) Site in Holmdel Township, New Jersey, of Bell Laboratory research facilities, including the horn antennas that played a key role in the identification of the CMBR.

cyanogen (38, 57 – 58, 59 – 61, 75, 89, 110, 132, 166, 189) The molecule CN consisting of a carbon and nitrogen atom. Observations of the ratio of numbers of interstellar CN molecules in the ground and first rotationally excited levels served as an important thermometer for early measures of the intensity of the CMBR at 2.6 mm wavelength.

Cygnus A (59, 115, 184, 185, 269) This galaxy is the strongest radio source in the sky and outside the Milky Way.

Cygnus X-1 (245) A binary stellar-mass X-ray source in the plane of the Milky Way.

dark energy (22, 25, 250) Einstein's cosmological constant, Λ, or a form of uniformly distributed energy that behaves like it.

dark matter (22 – 25, 79, 30, 81, 126, 133, 174, 242, 243, 249, 250) Also known as cold dark matter, or CDM: new names for missing mass. In the standard cosmology nonbaryonic dark matter dominates the mass in clusters of galaxies and the mass in the outer parts of galaxies outside clusters. Most of the baryons also are dark, in the sense that they are not in readily observable forms such as stars, gas and the plasma concentrated near galaxies and in clusters of galaxies.

Davis,Marc (266)

DDO (177, 179) David Dunlop Observatory, Richmond Hill, Ontario.

declination Position in the sky measured as angular distance from the celestial equator.

decoupling (130, 182, 246, 283, 292) At redshift $z = 1400$ the primeval plasma has completed combination to neutral hydrogen and helium and a trace amount of molecular hydrogen. The disappearance of almost all free electrons has eliminated radiation drag on the baryons and Thomson scattering of the radiation. Prior to this epoch of decoupling the baryons and radiation behaved as a viscous fluid.

degeneracy energy At given temperature T and a sufficiently high number density of fermions such as neutrinos the exclusion principle forces occupation of almost all allowed states up to the Fermi degeneracy energy, which exceeds kT, where k is the Boltzmann constant.

De Grasse, Robert W. (43, 53, 82, 105, 189).

density parameter (21, 149) A measure of the cosmic mean mass density. This measure is based on the first integral of equation (4) on page 16, which may be written as

$$\begin{aligned} \left(\frac{1}{a}\frac{da}{dt}\right)^2 &= \frac{8}{3}\pi G\rho \pm \frac{1}{a^2R^2} + \Lambda \\ &= H_0^2\left((1+z)^3\Omega_m + (1+z)^2\Omega_k + \Omega_\Lambda\right). \end{aligned} \tag{22}$$

In the first line aR is the radius of curvature of space sections at fixed world time. If this term is positive, space is curved in the fashion of the balloon analogy in Figure 1, meaning the circumference of a circle of radius r drawn in this space section is smaller than $2\pi r$; if negative, space is curved so the radius is larger than $2\pi r$. In the second line H_0 is Hubble's constant. The density parameters are the fractional contributions to the square of the present expansion rate, with the matter density parameter Ω_m representing the mass in nonrelativistic matter, the density parameter Ω_Λ representing dark energy, and the parameter Ω_k representing the effect of space curvature. The more complete list of values of density parameters in the table on page 21 includes the similarly defined density parameters in starlight and the CMBR.

Department of Terrestrial Magnetism of the Carnegie Institution of Washington (114, 118, 122)

de Sitter, Willem (14, 17) Biography: Blaauw (1975).

deuteron The atomic nucleus of the heavier of the stable isotopes of hydrogen, consisting of a neutron and proton bound together by the strong interaction. *See* primeval deuterium.

de Vaucouleurs, Gérard (240, 242, 318) Biography: Burbidge (2002).

Dicke, Robert H. (7, 49, 57, 73, 74, 89 – 91, 109, 114, 116, 122, 123 – 129, 132, 133, 137 – 140, 144 – 149, 166, 183, 193, 213, 253, 256, 264, 279; early CMBR Temperature bound: 30, 42, 43, 49, 57, 106, 114, 118, 125; entropy from a bouncing universe: 37, 87, 89 , 109, 137, 149; Gravity Research Group: 26, 60, 122, 123, 128, 149, 193, 232, 253, 277, 282; radiometer: 39, 40, 88, 103, 120, 121, 124, 137, 144, 177, 185 – 187, 190, 194, 232, 283; role in recognition of the CMBR: 74, 87, 109, 118, 123, 128, 137, 139, 144, 166) Biography: Happer & Peebles (2006). *See also* Eötvös experiment, evolving parameters of physics, Mach's Principle, scalar-tensor gravity theory, solar oblateness experiment.

Dicke microwave radiometer Phase-sensitive detection of incident radiation using a reference source radiating at a known effective temperature reduces the effect of receiver noise in a radio telescope. *See* Dicke.

Dicke switch *See* Dicke.

distance scale (30) In extragalactic astronomy, the calibration of values of distances to other galaxies. Ratios of distances are estimated from differences of apparent magnitudes of objects that appear to be similar enough that their luminosities are likely to be close to the same. The determination of physical distances is considerably more challenging. *See also* Hubble length.

Doppler effect (15) The shift of wavelength of light caused by the relative motion of source and observer, or of the wavelength of sound caused by the motion of source or observer relative to the air.

Doroshkevich, Andrei G. (50, 71 – 76, 78 – 79, 90).

DTM *See* Department of Terrestrial Magnetism of the Carnegie Institution of Washington.

Echo Project *See* Project Echo.

Eddington-Lemaître model (293) Lemaître's (1927) solution for a matter-filled universe that asymptotically traces back to Einstein's (1917) static model. Lemaître (1931) abandoned this picture in favor of a primeval atom or big bang cosmology, but Eddington (1931) continued to prefer the idea of expansion from a non-singular state.

effective temperature *See* Rayleigh-Jeans spectrum.

Einstein, Albert (13, 17, 18) Biographies: Pais (1982), Overbye (2000).

Einstein-de Sitter model (124, 162) The relativistic big bang model with negligibly small values of space curvature and the cosmological constant, that is, density parameters $\Omega_k = 0$ and $\Omega_\Lambda = 0$ in equation (22) on page 302.

Ellis, George F. R. (157, 160, 288 – 293).

entropy perturbations (79) Primeval departures from a homogeneous universe that differ from the adiabatic initial conditions of the standard cosmological model.

Eötvös experiment (144, 149, 253, 264) Precision check that the gravitational acceleration of a small particle is independent of its composition.

equation of state (265) In this book, the ratio $w = p_\Lambda/\rho_\Lambda$ of the dark energy pressure and energy density. In the limiting case of Einstein's cosmological constant the equation of state parameter is $w = -1$.

erg Unit of energy: 10^7 ergs = 1 Joule = 1 Watt of power applied for one second.

evolving parameters of physics (35, 44, 123, 211) Dirac's (1938) idea that the values of dimensionless parameters of physics may change as the universe evolves. A parameter that has attracted particular attention is the ratio of the strengths of the gravitational and electromagnetic interactions (eq. [15] on page 35) At the time of writing no variation of dimensionless parameters has been observed.

excess antenna temperature (56, 85, 87, 108, 109, 181, 189, 253)

expanding universe (14) The evolving spacetime illustrated in Figure 1 on page 13.

expansion parameter (16) The measure $a(t)$ of the history of expansion of the universe. The wavelength of freely propagating light stretches in proportion to the value of the expansion parameter, $\lambda \propto a$ (eq. [3] on page 16). If galaxies are not created or destroyed the mean distance between galaxies increases in proportion to the expansion parameter.

Fermi, Enrico (32, 63, 153) Biography: *Atoms in the Family: my Life with Enrico Fermi*, Fermi (1987).

Field, George (57, 59 – 61, 64, 66, 89, 110, 189, 246, 252).

flux density (84 – 86, 101 – 111, 145, 191) For a discrete source, rate of reception of energy per unit area.

fossil (4 – 51, 125, 135, 159, 175, 244, 246) In this book, remnants of processes operating when the universe was much denser than now, including most of the isotopes of hydrogen and helium, the spectrum and distribution of the CMBR, and the galaxies.

Fowler, William (Willie) A. (9, 65, 128, 153, 155, 163) Autobiography: Fowler (1993).

fractal cosmology (240) The postulate that the tendency of matter to appear in a hierarchy of concentrations within concentrations continues to the largest observable scales. This picture was widely considered to be attractive, but it is now convincingly ruled out.

free-free radiation (175, 262) Electromagnetic radiation from a plasma produced by collisions of free nonrelativistic electrons with the ions. The free-free spectral index in astronomical sources typically is $\alpha \simeq -0.1$.

Friedmann, Aleksandr A. (17) Biography: Grigorian (1972).

Friedmann-Lemaître models (241, 290) Relativistic homogeneous and isotropic solutions of Einstein's field equation that include the standard big bang cosmology.

Friis, Harald T. (53, 97, 100, 114) Autobiography: *Seventy Five Years in an Exciting World*, Friis (1971).

gain pattern (54, 81, 97, 100, 107, 212) Sensitivity of an antenna as a function of position of the source. *See* back and side lobes.

galaxy An island universe of stars, with effective radius (that contains half the observed starlight) less than about 10 kpc.

galactic radiation (180, 184) In this book, radio radiation from our Milky Way galaxy. Dominant sources at CMBR wavelengths are the radiation from interstellar electrons at temperature $\sim 10^4$ K accelerated by ions and the radiation from relativistic electrons accelerated by the interstellar magnetic field.

Gamow, George (astrobiology: 58; cool big bang cosmology: 33; hot big bang cosmology: 28 – 33, 42, 43, 47 – 51, 60, 63, 65, 73, 78, 89 – 93, 119, 122, 126, 127, 129, 138, 153, 163, 166, 171, 241, 246; structure formation: 31, 51, 130; steady state cosmology: 46) Autobiography: *My World Line*, Gamow (1970).

Gamow condition (30, 43) The constraint on the baryon matter density at temperature $T_{\rm crit} = 1 \times 10^9$ K for thermonuclear production of an interesting abundance of elements heavier than hydrogen.

Gaussian scale-invariant initial conditions In the standard ΛCDM cosmology the primeval departure from an exactly homogeneous mass distribution is a particularly simple random process that is determined by one free function. The function is chosen so that the mass distribution produces space curvature with near constant mean value per logarithmic interval of length.

general relativity theory (9, 16 – 18, 22, 28, 44, 36, 37, 51, 96, 122, 123, 126, 130, 133, 153, 160, 172, 212, 213, 278, 232, 235, 243, 288 – 293) Einstein's (1915) theory of gravity as an effect of spacetime curvature. It has passed demanding tests on length scales ranging from the laboratory to the Solar System, and, recently, extending to the scale of the Hubble length.

George Washington University (119) Gamow was Professor of Physics at GWU from 1934 to 1956.

GHz Unit of frequency: 1 GHz = 10^9 cycles per second.

gigaparsec (162) A unit of length in cosmology: 1 Gpc = 10^3 Mpc = 10^9 pc.

Gold, Thomas (8, 18, 67, 115, 132, 157, 159, 164, 170, 171, 175, 201, 203) Biography: Burbidge and Burbidge (2006).

graybody radiation (110, 194,262) A mixture of blackbody radiation spectra belonging to different temperatures. If the temperature mix has a positive nonzero lower bound a graybody spectrum has the Rayleigh-Jeans form $u_\nu \propto \nu^2$ at long wavelengths.

gravitational instability picture (51, 130, 232, 247 – 249) Gravitational growth of the large-scale structure observed today out of small departures from a homogeneous mass distribution in the very early universe.

gravitational radiation (212, 213, 267) Freely propagating spacetime fluctuations.

Groth, Edward J. (134, 267, 279)

ground noise (43, 41, 56, 83, 97, 270) In microwave measurements, the radiation emitted by the ground that finds its way to the detector through the antenna side- and back-lobes.

ground screens (256 – 256) Conducting sheets placed to suppress ground noise.

Gunn-Peterson effect (244) Scattering of radiation from distant galaxies by the Lyα resonance line of intergalactic atomic hydrogen (Gunn and Peterson 1965).

Gush, Herbert (19, 230) Author of a series of measurements of the CMBR spectrum. This work, based at the University of British Columbia, culminated

Holmdel Laboratory *See* Bell Laboratories.

horn antenna (40, 53, 54, 75, 81, 96 – 112, 117, 122, 139, 145, 186, 190, 254 – 258, 258) Microwave or radio antenna shaped like a horn, which affords strong suppression of back and side lobes. It is not practical to make the horn size as large as a reflector antenna, so the horn has inferior angular resolution and sensitivity to point-like sources, but horn and reflector have the same response to isotropic radiation, and the stronger rejection of radiation from the ground by a horn antenna is a great advantage for measurements of the CMBR.

horizon (160, 241, 247, 250, 289) In general relativity theory, an event horizon is the boundary of events in spacetime that that can in principle be observed by a chosen observer, and a particle horizon marks the set of (conserved) particles that are in principle observable. In the standard cosmology, these definitions often are applied to our spacetime subsequent to inflation.

hot big bang (7, 23, 67, 71, 78, 109, 137, 185, 244, 293) The now standard relativistic cosmological model with the material contents, including the CMBR, listed in the table on page 21.

Hoyle, Fred (8, 35, 46, 116, 153, 159, 163, 173, 200; background radiation: 49, 132, 166, 168, 170, 188; big bang cosmology: 18, 138, 290; at Caltech: 65, 96, 109; at Cambridge: 201 – 203; little big bangs: 35, 165; nucleosynthesis: 47, 163; origin of helium: 35, 49, 67, 127 – 129, 153 – 155, 165, 167; at Princeton: 65; quasi-steady state cosmology: 36, 132, 169; steady state cosmology: 18, 46, 64 – 68, 109, 115, 157, 164, 167, 168, 240) Autobiography: *Home is where the wind blows: chapters from a cosmologist's life* (1994); biographies: *Fred Hoyle: a Life in Science*, Mitton (2005); *Fred Hoyle's Universe*, Gregory (2005); *The Scientific Legacy of Fred Hoyle*, Gough (2005)

HST (69, 175) Hubble Space Telescope.

Hubble, Edwin P. (15, 17, 115, 171, 277) Biography: Whitrow (1972).

Hubble length (18, 89, 115) The distance $c/H_0 \sim 4000$ Mpc at which Hubble's law formally extrapolates to the velocity of light. In the standard cosmology this sets the order of magnitude of the largest observable distances.

Hubble's constant (15, 250) The coefficient in Hubble's law. Recent estimates of its value are $H_0 \simeq 70$ km s^{-1} Mpc^{-1}.

Hubble's law (15, 16, 240) The linear relation between recession velocity and distance (eq. [1] on page 15). Named after Edwin Hubble's (1927) early evidence for the relation, a modern example is shown in Clocchiatti et al. (2006). Motions of galaxies relative to the general expansion of the universe cause a scatter around Hubble's law of about ± 300 km s^{-1}. The deviation from the linear relation at cosmological redshifts greater than or comparable to unity depends on the cosmological model. *See* redshift-magnitude relation.

Hz Unit of frequency: 1 Hz = one cycle per second.

inflationary cosmology (18, 79, 148, 236, 250, 161, 293) The scenario described by Guth (1998) and Linde (1990) for what happened before the standard big bang model could be an adequate approximation.

insertion loss (56, 268) The absorption of radiation in a microwave detector and the consequent production of thermal radiation.

Institute of Theoretical Astronomy Now part of the Institute of Astronomy; *see* University of Cambridge.

integral Sachs-Wolfe effect *See* Sachs-Wolfe effect.

interstellar molecules (93, 112, 191) Molecules in interstellar gas clouds in galaxies, detected by their characteristic line radiation. *See* cyanogen.

isotropometer (253) The Partridge-Wilkinson 1965-67 method of searching for CMBR anisotropy.

jansky A standard unit of flux density: 1 Jy = 10^{-23} ergs cm^{-2} Hz^{-1} = 10^{-26} watts m^{-2} Hz^{-1}.

Jeans length (51, 130) The size of a gas cloud in which the force of attraction of gravity balances the pressure gradient force.

Johns Hopkins University (109, 118, 122, 127, 146)

Jordan-Brans-Dicke theory *See* scalar-tensor gravity theory.

Kaufman, Michele (174, 175)

K-band (114) Microwave radiation in the wavelength range $\simeq 0.3$ to 2.5 cm.

K-band atmospheric absorption (40, 114) Strong atmospheric absorption and emission by water vapor at wavelengths near 1.35 cm.

kiloparsec Unit of length: 1 kpc = 10^{-3} Mpc = 3.09×10^{21} cm.

Kitt Peak National Observatory (93, 112, 163)

Kragh, Helge (11).

ΛCDM cosmology *See* CDM model.

last scattering surface *See* decoupling.

Layzer, David (34, 64, 89, 171 – 174, 175, 243)

Layzer-Irvine equation (243) Relation between the kinetic energy per unit mass associated with the motion of matter relative to the general expansion of the universe and the gravitational potential energy per unit mass associated with the departure from a homogeneous mass distribution (Irvine 1961; Layzer 1963).

Lemaître, Georges (17, 36) Biography: *Un atome d'univers: La Vie et l'oeuvre de Georges Lemaître*, Lambert (2000).

lepton (31, 35) One of the electron, muon or tau particles, or their antiparticles, that do not interact by the strong interaction, or one of the associated neutrinos or antineutrinos that interact only by gravity and the weak interaction. The lepton number is the number of leptons minus the number of their antiparticles. Under the conditions considered in this book the lepton number is conserved. The discussion in Chapter 3 ignores considerations of the three families of leptons, though they are relevant to calculations of helium production in a hot big bang.

lepton degeneracy *See* neutrino degeneracy.

Lequeux, James (50) Astronomer at L'Observatoire de Paris.

Le Roux, Émile (49)

Lifshitz, Evgenii M. (70, 123, 172, 241)

Lightman, Alan (10).

LIGO (212) The Laser Interferometer Gravitational-Wave Observatory is a project in progress to detect gravitational waves, particularly from merging neutron stars or black holes.

Limber, Nelson (67).

little bangs (35, 154, 167) The theory of helium production in massive exploding stars under conditions that approximate a hot big bang (Hoyle and Tayler 1964).

load (39, 39, 40, 56, 84, 85 – 88, 96, 101 – 102, 107. 120, 121, 139, 144, 177, 178, 186, 187, 189, 190, 218, 258 – 261, 271) In this book, a reference source of radiation at known temperature for calibration of a microwave radiometer. *See* Dicke microwave radiometer, maser.

Local Supercluster *See* superclusters.

lock-in amplifier (253) Phase-sensitive detection method pioneered by Dicke and now widely used.

Longair, Malcolm S. (46)

look-back time (45, 46, 158) In cosmology, the time taken for radiation to travel from source to observer.

loss *See* insertion loss.

Lyman α photons (59) Radiation at wavelength 1215 Å emitted by atomic hydrogen in the one-photon transition from the first excited level to the ground level.

Mach's principle (36, 123, 211, 243, 244, 332) The influential but not yet well-defined idea that local physics is related to the large-scale nature of the universe, and — in some circles — that local physics may therefore evolve as the universe expands.

maser amplifier (53, 81, 83, 95 – 96, 105. 189, 272) Microwave amplification of radiation by stimulated emission, used for low noise receivers.

Massachusetts Institute of Technology (114, 120, 175, 200 – 203, 211 – 213, 227, 244, 284; Radiation Laboratory: 40, 39, 114, 118, 144).

MAXIMA (250) Millimeter-wave Anisotropy Experiment Imaging Array (Hanany *et al.* 2000).

McKellar, Andrew (39, 57, 75, 89, 166) Biography: Beals (1960)

megaparsec (13) A standard unit of length in cosmology and extragalactic astronomy: 1 Mpc = 10^6 pc, or about three million light years.

Melchiorri, Francesco (266)

Michie, Richard W. (130, 246).

micron (205) $1\mu = 10^{-4}$ cm.

microwave radiation (7, 19) Electromagnetic radiation, usually with wavelength in the range 1 mm to 30 cm.

Milky Way (13, 18, 22, 24, 83, 96, 101, 141, 179, 252, , 263) A spiral galaxy named for the band of light across the sky from the stars in its disk. We are in the disk about 8 kpc from the center of this galaxy.

Misner, Charles W. (160, 289)

missing mass The old name for dark matter.

MIT *See* Massachusetts Institute of Technology.

mixer (196) In this book, device for conversion of microwave radiation to an intermediate (lower) frequency.

mixmaster cosmology (160)

Morrison, Philip (133, 228, 276).

Mpc *See* megaparsec.

Muehlner, Dirk (213 – 228, 284).

Murray Hill (53, 55, 96, 99) Bell Laboratory, in Murray Hill, N. J.

Narlikar, Jayant V. (36, 46, 163, 167 – 170).

National Aeronautics and Space Administration (20, 54, 96, 193, 197, 206, 272) NASA was created in 1958 for research into problems of flight within and outside the earth's atmosphere.

National Balloon Facility *See* National Center for Atmospheric Research.

National Center for Atmospheric Research (196, 225, 282 – 286) An NCAR Scientific Balloon Facility is based in Palestine Texas, and a High Altitude Observatory is in Climax Colorado.

National Radio Astronomy Observatory (116, 117, 93, 99, 112, 182, 268, 276, 264) The NRAO is based in Charlottesville Virginia.

National Science Foundation (115, 203, 193, 257, 276) NSF is a USA federal agency for the support of science.

Naval Research Laboratory (81, 116, 117, 122, 199, 203, 215)

NCAR *See* National Center for Atmospheric Research.

neutrinos (23, 23, 154) The partners of the electron and its more massive analogs in the lepton families. The mass density in thermal neutrinos from the hot big bang is listed in Table 1 (page 21).

neutrino degeneracy (32, 35, 70, 126, 155, 174) In a big bang cosmology, the assumption that the number density of neutrinos or of antineutrinos is large enough that the degeneracy energy significantly affects light element production.

Nobel Prize (118, 128, 146, 267).

noise temperature (53, 82, 97, 105, 186) A measure of system noise in terms of the equivalent temperature in the Rayleigh-Jeans limit of the blackbody spectrum.

quasi-steady state cosmology (36, 132, 167, 169) Variant of the steady state cosmology with a constant long-term mean general expansion interrupted by relatively short-term cycles of expansion and contraction. The CMBR would be produced by the absorption and thermalization of starlight during the denser parts of each cycle. If the cycles are deep enough this picture approaches an oscillating big bang model universe.

Queen Mary, University of London (230)

radiation drag (124, 130) In cosmology the CMBR drag force on plasma caused by Thomson scattering by the free electrons. *See* Silk damping, decoupling.

Radiation Laboratory of MIT Laboratory for research on radar during the Second World War. *See* Massachusetts Institute of Technology.

radiometer *See* Dicke microwave radiometer.

radio source counts (45, 158, 159, 184, 185) In cosmology, counts of extragalactic radio sources as a function of flux density (or apparent magnitude). The counts played an important role in early discussions of the cosmological tests and figure now in measures of the cosmic evolution of the radio luminosities of galaxies.

radio stars (84, 158, 184) In the 1960s, a term for objects detected at radio wavelengths and variously applied to sources in the Milky Way, other galaxies and quasars.

Radlab *See* Radiation Laboratory.

Rayleigh-Jeans spectrum (37, 215, 227, 312) The limiting form of the spectrum of thermal radiation at low frequency, $\nu \ll kT/h$. In this limit, the mean energy of a mode of oscillation of the electromagnetic field is $\mathcal{N}h\nu = kT$ (eq. [25] on page 319), the expression one would predict from classical mechanics, and the energy per unit volume and frequency interval is

$$u_\nu = 8\pi kT\nu^2/c^3. \tag{23}$$

It is conventional to use this relation to express radiation energy density or flux in terms of the equivalent Rayleigh-Jeans temperature.

recombination *See* decoupling.

redshift In astronomy and cosmology, a wavelength shift that may be caused by relative motion (the Doppler effect), the expansion of the universe or a time-varable gravitational potential. *See* cosmological redshift, noncosmological redshift.

redshift-magnitude relation (45) Relation between the redshifts and apparent magnitudes of extragalactic objects that all have the same absolute magnitude. Measurements of this relation are an important test of cosmological models.

Rees, Martin (157 – 162, 242, 291).

Rees-Sciama effect (162) Perturbation to the CMBR by the time-variable gravitational potential belonging to the departure from an exactly homogeneous mass distribution. *See also* CMBR anisotropy.

reflector antenna (83) Incident radio or microwave radiation is directed by one or more reflecting surfaces to a much smaller horn antenna leading to the detector. The size of the primary reflector can be made large to improve angular resolution and sensitivity to a point-like source at the expense of poorer rejection of radiation incident from directions well away from the source. *See* back and side lobes, horn antenna.

relativistic collapse *See* black hole.

Rice University (95)

Richards, Paul L. (266)

right ascension Position in the sky measured as the angular position along the celestial equator.

Robertson-Walker line element (235, 292) Expresses the geometry of a homogeneous and isotropic world model in the form

$$ds^2 = dt^2 - a(t)^2 \left[\frac{dr^2}{1 - r^2 R^{-2}} + r^2 \left(d\theta^2 + \sin^2\theta d\phi^2 \right) \right]. \qquad (24)$$

The expansion parameter $a(t)$ appears in equation (22) on page 302. The physical radius of curvature of a space section of fixed world time t is $a(t)R$. Since the Robertson-Walker form follows from the symmetry it applies to the steady-state cosmology, with the stipulation that $R^{-2} = 0$ because in this model no global quantity, including the radius of curvature of space sections, can vary with time. In the relativistic big bang model the constant R^{-2} figures in the definition of the density parameters in equation (22). In the standard cosmological model $a(t)|R|$ is much larger than the Hubble length, meaning space sections have close to flat Euclidean geometry.

Rogerson, John B. Jr. (64 – 67).

Roll, Peter G. (37, 57, 60, 87, 110, 124, 128, 137, 144 – 152, 193 – 195, 253)

Ryle, Martin (114, 115, 158, 159, 164, 167, 178, 184) Nobel Prize in Physics 1974.

Sachs, Rainer K. (130, 235 – 239, 240, 280, 292).

Sachs-Wolfe effect (130, 162, 242, 244, 247, 247, 280) The variation of the CMBR temperature across the sky caused by the gravitational effect of the departures from a homogeneous mass distribution observed at the present epoch as the concentrations of mass in galaxies and clusters of galaxies. The effect was predicted by Sachs and Wolfe (1967) and detected by the COBE satellite (Smoot *et al.* 1992). The integral Sachs-Wolfe (ISW) effect is the anisotropy produced by the time-variation of the gravitational potential along the line of sight.

Sandage, Allan R. (45, 89, 245, 290).

S-band Microwave band at wavelengths $\sim$ 2 to 4 cm.

scalar-tensor gravity theory (36, 60, 126, 149, 193, 211, 253, 277, 283) A modification of general relativity theory in which the measured strength of the gravitational interaction is a function of time.

Schwarzschild, Martin (64 – 68, 127) Biography: Ostriker (1997).

Schwarzschild solution (289) Karl Schwarzschild's spherically symmetric solution to Einstein's field equation in empty space that describes the spacetime geometry around an isolated spherical mass concentration, or a black hole with no angular momentum.

Sciama, D. (8, 132, 133, 153, 157 – 160, 162, 168, 202, 243, 252, 288, 289, 292) Biography: Rees (2001).

Scientific Balloon Facility (225) *See* National Center for Atmospheric Research.

Scovil, H. E. Derek (53, 55, 96)

screens (186, 286) In microwave detectors, conducting sheets that reflect antenna side lobes from unwanted sources of radiation.

shaggy dog (114, 121) Calibration radiation source for a microwave detector.

Shakeshaft, John R. (54, 93, 95, 108, 110, 133, 184 – 188).

Shklovsky, Iosif S. (89, 110, 57, 74) Autobiography: *Five Billion Vodka Bottles to the Moon* (Shklovsky, Zirin and Zirin 1991).

Shmaonov, Tigran A. (50. 75, 76)

Silk damping (130, 248) The dissipation of initially adiabatic inhomogeneities in the plasma and CMBR before decoupling by diffusion of the photons through the radiation.

Silk, Joe (130, 242, 243 – 251)

singularity theorems (133, 157, 160, 288 – 293) In observationally relevant situations — the end points of massive stars, the centers of galaxies, the expanding universe — and under broad but not entirely general assumptions about the physics of matter and radiation, demonstrations that the spacetimes of general relativity theory are incomplete.

sky noise *See* atmospheric noise.

Smirnov, Yu. N. (34, 49)

solar oblateness experiment (137, 193, 253, 277) Dicke's program of measurement of the shape of the Sun (Kuhn, Libbrecht and Dicke 1988 and earlier references therein) for the purpose of checking the possible effect of the departure from a spherical mass distribution in the Sun on the orbit of the planet Mercury.

space curvature (22, 250) *See* Robertson-Walker line element.

special relativity theory (9, 14, 274) General relativity theory applied to flat spacetime (Lorentz 1904; Einstein 1905).

spectral index (180, 187, 194) Parameter α in the power law model $u_\nu \propto \nu^\alpha$ for radiation spectrum expressed as energy per interval of frequency ν. The Rayleigh-Jeans spectral index is $\alpha = 2$.

Spitzer, Lyman (59, 60, 64, 90, 116)

Stanford University (116, 153, 268, 277, 282)

standard cosmological model (13 – 51) The relativistic hot big bang cosmological model with the contents listed in Table 1 on page 21. *See* CDM model.

standard model (32) In physical science a well tested and successful theory that is it is hoped approximates a better one to be discovered.

steady state cosmology (8, 18, 21, 32, 35, 44 – 47, 64, 67, 68, 96, 109, 110, 115, 127, 132, 133, 137 – 140 148, 154, 157 – 159, 164 – 170, 171, 178, 184, 185, 202, 204, 212, 228, 240, 246, 252) Postulates that continual creation of matter preserves a steady mean mass density in a near homogeneous universe that is expanding at a steady rate.

Stokes, Robert A. (193 – 198, 258 – 263)

Sullivan, Walter (89, 273) Science editor for the New York Times.

superclusters (162, 231, 240) The largest distinct concentrations of galaxies. We are on the outskirts of the de Vaucouleurs Local Supercluster, roughly 20 Mpc from the center.

Sunyaev, Rashid A. (248, 242).

Sunyaev-Zel'dovich effect (131, 188) Scattering by the electrons in a plasma pushes the CMBR spectrum up relative to blackbody at shorter wavelengths, down at longer wavelengths.

synchrotron radiation (118, 180, 187, 262) Radiation from electrons moving at relativistic speeds through a magnetic field. The spectral index for astronomical sources is typically $\alpha \sim -0.7$.

system temperature (84, 105) Received radiation plus that generated in a telescope expressed as the equivalent Rayleigh-Jeans radiation temperature. *See* noise temperature.

Tayler, Roger J. (35, 49, 49, 127 – 129, 153 – 155, 157, 167)

Telstar Project (43, 55, 83, 100) A Bell Laboratories demonstration of transmission of a television signal via a satellite.

Thaddeus, Patrick (57, 73, 90, 110).

thermal radiation (19) The blackbody radiation that fills a region of space when relaxed to thermal equilibrium. In a cavity bounded by reflecting walls the electromagnetic field may be expressed as a sum of the normal modes of oscillation that fit in the cavity. For a mode with frequency ν or wavelength λ at thermal equilibrium at temperature T the mean number of photons in the mode — the occupation number — is given by Planck's function,

$$\mathcal{N} = \frac{1}{e^{h\nu/kT} - 1} = \frac{1}{e^{hc/kT\lambda} - 1}, \tag{25}$$

where h and k are the Planck and Boltzmann constants. The energy of a photon is $h\nu$, and the mean energy of the mode is $\mathcal{N}h\nu$. The sum over modes yields the mean thermal energy per unit volume in the frequency interval $d\nu$,

$$u_\nu d\nu = \frac{8\pi h\nu^3}{c^3}\frac{1}{e^{h\nu/kT} - 1}d\nu. \tag{26}$$

The maximum value of u_ν is at wavelength $\lambda_p = 5.1/T$ mm at temperature T kelvin. *See* Wien peak, Rayleigh-Jeans spectrum.

Thomson scattering Nonrelativistic scattering of photons by free electrons.

tipping experiment (42 – 43, 106, 108, 187, 190, 215, 258) Measurement of the radiation emitted by the atmosphere at the zenith from the variation of radiation received as a function of angular distance from the zenith.

tired light (44, 211) The idea discussed by Zwicky (1929) that the redshift of the light from distant galaxies is shifted to the red by loss of energy rather than by the expansion of the universe.

TOCO (250) Mobile Anisotropy Telescope on Cerro Toco for measurement of the CMBR anisotropy (Miller *et al.* 2002).

Tolman, Richard C. (20, 36) Biography: Kirkwood, Wulf and Epstein (1952).

trapped surface (160, 289)

Trimble, Virginia (119) Astronomer at the University of California, Irvine.

Turner, Kenneth C. (109, 118, 122, 128).

TWM Traveling wave maser amplifier.

University of British Columbia (20, 176, 230, 306)

University of California, Berkeley (60, 116, 189, 230, 235, 248, 275)

University of California, San Diego (163, 240)

University of Cambridge (54, 95, 155, 157, 161, 164, 184, 202, 243, 288)

University of Michigan (64, 115, 136, 199, 231)

VLA (181) The Very Large Array, in New Mexico, yields high angular resolution radio images by means of the time correlations of signals from well-separated telescopes.

VLBI (180) A Very Long Baseline Interferometer has even higher angular resolution from more widely separated radio telescopes than the VLA.

VSA (188) The Very Small Array telescope for measurement of angular variations of the CMBR on angular scales down to $\sim 0.2^\circ$.

Wagoner, Robert V. (128, 153 – 156, 167).

Wall, Jasper (176 – 184).

waveguide (85, 88, 101, 102) Conducting channels, usually with rectangular or circular cross section, for transmission of microwave radiation.

Weber, Joe (119) A pioneer in the theory and practice of gravitational wave detection. Biography: Yodh and Wallis (2001).

Weinberg, Steven (11)

Weiss, Ranier (121, 143, 211 – 230, 266, 284).

Welch, William "Jack" (189 – 191).

Weymann, Ray J. (66, 131, 187).

Wheeler, John A. (36, 122, 135, 161, 193, 277, 288, 314) Joseph Henry Professor of Physics Emeritus, Princeton University.

Wheeler-Feynman absorber theory (267) Attempt to account for the time-asymmetry of the world in classical physics that is time-symmetric.

White Mountain Research Station (121, 190, 194, 258, 260, 261, 272) University of California research facility with stations in and above Owens Valley, California.

Wien peak (216) The wavelength λ_m at maximum intensity of a thermal radiation spectrum, $\lambda_m \propto T^{-1}$. *See* thermal radiation.

Wilkinson, David Todd (5, 7, 26, 37, 40, 57, 60, 87, 90, 92, 109, 110, 121, 124, 128, 129, 131, 132, 136– 143, 144 – 149, 166, 183, 189 – 191, 193 – 196, 198, 211, 231, 249, 253 – 261, 263, 266, 274, 277 – 280, 282) Biography: Weiss (2006).

Wilson, Robert W. (37, 44, 73, 83 – 94, 95 – 113, 117, 118, 120, 122, 128, 139, 144, 166, 189, 244, 246).

WMAP (7, 131, 136, 148 – 149, 183, 191, 198, 250) Wilkinson Microwave Anisotropy Probe. A satellite placed in a near-stable orbit beyond the moon for the precision measurement of the angular distribution of the CMBR (Bennett *et al.* 2003, Hinshaw *et al.* 2006).

Wolfe, Arthur M. (130, 235, 240 – 242, 244, 280).

Woolf, Neville J. (57 – 58, 61, 241).

world time The mean time kept by observers moving so the universe is seen to be isotropic. This time appears in equation (4) on page 16.

X-band The microwave band at wavelengths ~ 8 to 12 cm.

Yale University (137, 150)

ylem (63) The name Gamow and colleagues used for the early stages of expansion of the hot big bang.

Yu, Jer-tsang (130, 134, 231 – 234, 279)

Zel'dovich, Yakov Borisovich (7, 34, 70 – 77, 78, 131, 132, 161, 166, 242, 248) Scientific biography: Sunyaev (2004); collected works: Ostriker, Barenblatt and Sunyaev (1992).

Zwicky, Fritz (23, 211, 232) Astronomer noted for perceptive and iconoclastic ideas. He appears in this book in connection with the tired light interpretation of galaxy redshifts and an important catalog of galaxies and clusters of galaxies.

2C and 3C catalogues (115, 95, 158, 159, 185) Second and Third Cambridge Catalogues of radio sources.

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