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Early photons from the early universe

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Abstract

The 60 years before the announcement of “A Measurement of Excess Antenna Temperature at 4080 Mc/s” witnessed a remarkable number of observational and theoretical close approaches to what is now called the cosmic microwave background, though it had a wider variety of names in the past. We explore some of these, with special attention to the activities of Simon Newcomb, George Gamow, Andrew McKellar, Edward Ohm, and Yakov Borisovich Zeldovich.

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1. Introduction: kinds of history

The history of a scientific question can be traced in at least three kinds of sources: formal publication in books, journals, and observatory reports; informal writings in memoirs, letters, and other documents, some contemporary with the events being chronicled and others looking backwards; and the range from folk tales to oral history. Some portion of each of these has gone into the following account, but the focus is on the formal published record.

Each has its virtues and its failings. The folk tale has great staying power. No one who has heard [Sandage \(2004\)](#) tell the story of how Harlow Shapley wiped off the

Cepheids that Milton Humason had found on Shapley's plates, leaving them to be found a few years later by Edwin P. Hubble, is likely ever to forget it. Oral history is, however, likely to be the victim of selective or modified recall. “There comes a time,” said [Bell Burnell \(1976\)](#) “when you no longer remember what happened, but only how you told the story the last time.” One such story (which appears below and might be titled “Gamow and the graduate student”) has come back to me by an indirect route and in almost unrecognizable form.

The formal, published record leaves us in no doubt about who knew what, when. Thus, [Tolman's \(1934\)](#) classic text makes crystal clear in Eqs. (171.5), (171.6), and (156.1) that black body radiation in an expanding universe will redshift to black body radiation at a lower temperature, not dilute to gray body radiation at the original tempera-

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ture. But the formal record typically omits the thought processes, struggles with instrumentation, and other intermediate stages leading up to the published result.

Informal records – letters, notebooks, first drafts, responses to questions from colleagues and historians – can help to bridge the gap, which is why historians of science are often to be found in archives, attempting to sort through the partially-catalogued papers of our predecessors. But of course the key letter or memorandum may never be found, indeed may not exist, a problem shared by historians of all fields and one which leads to a tie between the subjects to be studied and the papers that have actually been archived. I am indebted to David DeVorkin of the National Air and Space Museum for making this point in connection with the (so far unresolved) issue of how much, if any, of the archives of the *Astrophysical Journal* are to be preserved. One would give, a good deal for, say, a note from George Gamow written around 1950 to some friend mentioning his own understanding of the potential detectability of the radiation predicted by his younger associates Ralph Alpher and Robert Herman. But nothing of the sort has ever been found, leaving the multiple possibilities to be described by historian Kragh (1996).

2. Three classic misunderstandings

The proverbial 20–20 hindsight makes it easy to see at least three branch points, where a misunderstanding held back discovery of what eventually had the full panoply of names, the isotropic, cosmic, microwave, black body, 3° , background, thermal, relict radiation. We explore Gamow and Newcomb and Herzberg and McKellar here, deferring Zeldovich and Ohm to a later section.

At one (pre-Edisonian) time, astronomers seem to have supposed that the night sky was completely dark between stars, if you looked away from the galactic plane, zodiacal light, and any aurorae that might be going off nearby, and you had the good luck to be far from fires, fireflies, or other light sources. That this is not so for visible light was shown by Newcomb (1902), who generally does not otherwise get very good press. He was the first president of the American Astronomical Society and arguably the “Learned Astronomer” of the poem. Newcomb, looking at the sky from “the home of A. Graham Bell, near Baddeck, Cape Breton Island” on 16 September 1901, using dark glass and diffused images, found that the brightness of the sky near the galactic pole is the equivalent of a little less than one 5th magnitude star in a circle 1° in diameter. The region of the sky he examined falls within Sir Patrick Moore’s (2005) constellation Supellex Cubicullii (The Bedroom Utensil), with some of the calibration stars in *Instrumentum Quod Se In Mechanicum Insertit* (The Spanner).

Using somewhat more systematic data, Eddington (1926) estimated that the local energy density in starlight is 7.67×10^{-13} ergs/cm³, corresponding to an effective or bolometric temperature of 3.18 K. He called this “the tem-

perature of interstellar space,” but made completely clear that the radiation has the wavelength distribution of dilute starlight, a gray body, not a black body at that temperature. Not long after, Regener (1933) found that cosmic rays contribute a comparable local energy density. It remains true, incidentally, that the local densities (or pressures) due to cosmic rays, starlight, magnetic fields, and gas turbulence are all about the same in the galactic disk, and all are about equal to the CMB energy density. This last is surely a coincidence. The others presumably not. Gamow seems to have supposed that the starlight would compete with “the temperature of the universe,” (Kragh, 1996). And Alpher and Herman, 1948 in their first, 5 K, prediction paper mention the starlight and primordial radiation so close together as to invite confusion.

In 1940, McKellar (1940) correctly identified an absorption feature in the Mt. Wilson spectrogram of Zeta Ophiuchi at 3874.62 Å as arising from the ground state of interstellar cyanogen (CN), and he predicted that there should be line at 3874.00 Å due to the first rotationally excited state. Adams (1941) quickly found the predicted line and sent that data back to McKellar (1941), who used the estimated line ratio of 5:1 between ground and excited state absorption to calculate a rotational excitation temperature of 2.3 K. He also noted that intensity ratios between 2.5 and 10 to one still yield temperatures between 1.8 and 10 K. He does not remark either on the similarity of this temperature to the Eddington interstellar value or mention any particular excitation mechanisms.

Enter Herzberg (1950), who had also participated in the identification of interstellar molecular absorption features and was the only one of this crew to exit from the process with a Nobel Prize (Chemistry, 1971, for his lifetime of work on molecular spectroscopy). But in 1950, he wrote, “From the intensity ratio of the lines with $K=0$ and $K=1$, a rotational temperature of 2.3K follows, which has of course only a very restricted meaning.” Not surprisingly, graduate students who were witness to the 1965 discovery spent a good deal of time going around assuring each other that their ideas had only very restricted meanings. Adams appears in standard astronomy books for work on spectra of giant stars. McKellar is largely forgotten, even at DAO, in whose publications one of his crucial papers appeared.

3. The theorists take a hand

The first people to take an early universe seriously must have been Friedmann (1924, with a number of variant spellings) and Lemaitre (1927), whose solutions to the Einstein equations expanded, the latter from a “cosmic egg” (an image that has probably contributed to the common misunderstanding of the big bang as an explosion occurring at some specific place in existing space). Next comes George Gamow, whose first paper dealing with element creation in a cosmic context is Gamow (1935). Nearly always cited, however, is “Alpha, Beta, Gamma” (Alpher

et al., 1948). The paper is noteworthy in at least three ways relative to our story. First, it appeared in the April 1st issue of *Physical Review*. Second, the footnote explaining that Hans Bethe had participated only in absentia was somehow lost from the published version. And third it says not a word about left over radiation or a temperature, but addresses only the production of the full range of chemical elements. Oh, and fourth, the initial conditions are wrong. “The early state of matter” is described as “a highly compressed neutron gas,” which partly decays to protons and electrons. These run around capturing the left-over neutrons until a few nuclei reach the lead–bismuth peak and still fewer perhaps uranium and thorium.

The primordial substance was dubbed ylem (by Alpher, not Gamow), and it was still the assumed initial condition in the Alpher and Herman (1948) first calculation of a temperature of the universe. The word sounds vaguely Greek, but, says the OED, it derives from medieval Latin. All three revisited the temperature issue several times in the next few years, finding mostly larger temperatures of 6, 7, 28, and 50 K (see Kragh 1996 for references).

Hayashi (1950), better known to most astronomers for calculations of the pre-main sequence evolution of stars, did the first calculation of early universe processes that began with protons, neutrons, electrons, positrons, neutrinos, and antineutrinos expanding out of thermal equilibrium. But once again he addressed only helium production (finding an answer not very different from either the Gamow value or later computations or indeed from contemporary and later observations). There is no mention of residual radiation. I came perilously close to writing, “But he was interested only in helium production...” This is, however, precisely the sort of information the published record does not provide. We know only what he chose to write about, not what he was interested in, unless unpublished or oral material on the point exists.

4. Mapping the radio sky

The first studies of the radio astronomical sky were those of Karl G. Jansky in 1932–1933 (Jansky, 1935) and Grote Reber from 1937 on (Reber, 1940). The former was attempting to understand interference on the recently inaugurated trans-Atlantic radio-telephone service on behalf of his employer, Bell Telephone Laboratory, and the latter, also a radio engineer, had read of Jansky’s results and found them interesting enough to build his own parabolic reflector and produce contour maps. Then there was a war, during which radar operators in, at least, Denmark, the USA, England, Australia, and New Zealand independently discovered radio emission from the sun (Orchiston, 2005), and after which many of the same radar dishes were turned upward and their operators became the first generation of radio astronomers. You already know that the telecommunications industry along with various military organizations and research-oriented scientists contributed to the development of radio astronomy, and where

one of those industrial projects led. But when you next see the names of Penzias and Wilson, our story will be over.

George Gamow played no part in what came next, though arguably he had a chance to do so. In fall 1949, the University of Maryland hired a professor of electrical engineering, who had just resigned his commission in the US Navy, where he had been in charge of electronic countermeasures. The appointment was contingent on his obtaining a Ph.D., and the institution clearly had to be someplace in the greater Washington area. One of the places he visited was George Washington University, and one of the people he had talked with was George Gamow, then a professor there. A question on whether Gamow had any good thesis problems in mind elicited a reciprocal question about the younger man’s (by 15 years) skills. I am a microwave spectroscopist, said the interviewee. No, I don’t have any good problems, responded Gamow, so the potential graduate student went on to Catholic University, completing a thesis on the inversion spectrum of ammonia with Keith Laidler a couple of years later. This is at least first-hand oral history, because the Maryland professor was my late husband, Joe Weber.

Could the background radiation have been seen then? In a technical sense, perhaps. Weber had already used Dicke switching radiometers in his radar work. But the rest of the world was no more ready than Gamow to regard hunting for it as an interesting, doable project. A later “when could it have been done?” vignette follows shortly.

Gamow makes one last appearance in our story at the January, 1967 Third Texas Symposium on Relativistic Astrophysics, the first not actually held in Texas, and the only one for which there are no printed proceedings (the manuscript having disappeared into a publisher’s bankruptcy like the plates for John Bevis’s *Atlas*, with the first image of the Crab Nebula). Gamow was asked to share his thoughts on the microwave background from the podium. Among them was the remark, “Well, I lost a nickel and you found one. Who’s to say it’s the same nickel?” Recall that the first Alpher and Herman (1948) prediction was 5 K (hence, I suppose, the five cents), and wonder ever after whether Gamow truly did not believe that the prediction, resting on his own earlier work, had been confirmed. Could I have asked? Yes, because he bought me a couple of drinks that evening (and sketched me as “the red shift girl”). Did I? No. Would the answer have been useful? Probably not. Those were not his first drinks of the day, and he died the next year.

Between 1950 and 1965, very many people mapped the radio sky, for many different reasons. I am aware of six cases that have been put forward as pre-discoveries of the relict radiation and would be interested to hear about any others that may be lurking in under-cited papers or unplumbed memories. The six are discussed not chronologically but from least well documented to, arguably, best.

Novikov (2001) notes in a single sentence that he was informed that measurements in the early 1950s by Japanese radio astronomers had probably seen the 3 K background.

In the one piece of historical research undertaken for this talk, I emailed about two dozen members of the IAU Commission on Radio Astronomy with current addresses in Japan, asking them for further information. No one answered.

Kragh (1996) remarks with equal brevity that William K. Rose (later of the University of Maryland but then at MIT) attempted a deliberate search in 1962 using facilities of the Naval Research Laboratory. The estimated result, 3 K was very uncertain, not confirmable, and never published (see Brush, 1993).

Better documented is the work of T.A. Shmaonov in Russia, reported in his 1957 PhD dissertation (Shmaonov, 1957) and discussed by Novikov (2001) and in a forthcoming chapter of Partridge and Peebles (in preparation). He mapped the sky at 3.2 cm using a horn antenna. After careful allowance for noise, he had left over a radiation background of 4 ± 3 K, not varying with direction (away from known sources) or time. Although he worked with a well-established group of Soviet radio astronomers, the work had been forgotten by the time Novikov and Doroshkevich inquired among the community in 1964 for any measurements that might be relevant to their prediction of radiation left from the early universe.

Pribery is not very accessible, but many readers will find *Comptes Rendus de l'Academie des Sciences (Paris)* in their real or virtual institutional libraries. And there, in volume 244 (Denisse et al., 1957), you will find a brief account of the thesis work of Emile Le Roux, who also surveyed the sky (at 33 cm, using a WWII surplus German radar dish) and had 3 K left over, with an uncertainty of 2 K and considerable isotropy. He suggested extragalactic origin in the thesis, and the work was later (Le Floch and Bretenaker, 1991a,b) described as a pre-discovery. Curiously, Le Roux did not get to be first author on the CR report of his work, and indeed Kragh (1996) and Le Floche and Bretenaker disagree about the precise identities and ordering of the authors. The paper was communicated to CR by Andre Danjon (who was eventually succeeded as director of the Observatoire de Paris by Le Roux co-author Denisse). Intriguingly, the reconsideration (Le Floch and Bretenaker, 1991b) was communicated by Jean-Claude Pecker, long an opponent of conventional hot big bang cosmology (but a vigorous supporter of French astronomers!).

Fifth (in case you might have lost track), come the M.Sc. projects of Jasper Wall, Donald Chu, and their advisor J.T. (Alan) Yen (Wall et al., 1970). Wall and Chu and horn antennas were set to measure the absolute value of the galactic temperature at 320 and 707 MHz. About the time they finished, Yen decided that he had told them to measure the darkest parts of the sky, not the brightest. In that case, the extra 3 K would have been a good deal more conspicuous in their data, though not necessarily more likely to have been recognized as cosmologically important. In summary (Wall, 2006) “At the time of the discovery of the CMB we were not looking for it; and we had not made a claim at the time to have seen it. Nor have we claimed

afterwards that we ‘saw’ it before 1965. It was there in our data all right, though. The truly amazing point in hindsight was the realization of just how easy it would have been to do the right experiment for \$500, any time after say 1960, when cheap low noise transistors came on the market.” With (unfortunately) inimitable modesty, Wall concludes that “this is hardly as big as a footnote to the CMB discovery, more like a toe-note.” The results were not published until 1970, after two years in press.

And, finally, we return you to Bell Telephone Laboratory, where Edward Ohm is, like Jansky, again scanning the sky to measure backgrounds or noise against which signals will have to be detected, this time from the Echo communications satellite. He used a horn antenna at 2390 MHz and has reported both the process and the results in detail (Ohm, 1961). It is, I think, possible to interpret his numbers in different ways, depending on which portions of the text, figures, and tables you look at. If you know you are looking for a predisccovery of the CMB, you will focus in Table II (p. 1080) and the text immediately following. The table predicts a total system temperature of 18.90 ± 3.00 K, including sky emission at the zenith (as derived from zenith to horizon scans). But, says the text, the lowest observed temperature anywhere was consistently 22.2 ± 2.2 K. The difference is, of course, 3.3 K, and the uncertainties add in quadrature to 3.7 K, clearly not a statistically significant detection, though it almost certainly could have been turned into one with some additional work.

What has become of the major players in these quarter-final games? I have no information about the Japanese contingent. William K. Rose is professor emeritus of astronomy at the University of Maryland, though, after a stroke, no longer, as I write, in the best of health. Novikov reports (private communication) that he has had no further contact with Shmaonov since 1983, when the latter was at The Institute of General Physics in Moscow and told Novikov about his 1957 work.

Emile Le Roux appears in IAU directories in 1958–1967 (though not as a member of C40, radio astronomy, or any other commission) and is gone from the 1970 directory, not exiting via the necrology. Le Floch and Bretenaker (1991a,b) gave no indication of any further contact with him in connection with their re-examination of the 1955 map. Jasper Wall, nominally retired, is to be found in your IAU and RAS directories and will answer messages as soon as he deals with the other six higher priority tasks on his to-do list.

Ed Ohm remained at BTL, working on satellite communications, through the official discovery of the CMB and well beyond, retiring in the mid 1980s. He wrote in 1994, in response to a letter from the present author, saying that there was simply no justification, given the error bars, for a 1961 report of anything more than he reported – a sky (or at least an antenna) somewhat warmer than he expected. He also indicated that he was probably related to the Ohm of the unit (George Simon 1787–1854) as the families had come from the same region of northern Germany. The

father of my maternal great grandmother was a Captain Ohm, from the Schleswig–Holstein region, and it is not improbable that I am connected to the same lineage, though “our” Ohm’s daughter was adopted by a family named Rasmussen and ended up on the Danish side after the partition of Schleswig–Holstein. It has nothing to do with this story, but Heinrich Christian Schumacher, the founder of *Astronomische Nachrichten*, our oldest extant journal, was a near life-long resident of Altona, Schleswig–Holstein and was greatly distressed by the upheaval.

5. Revivals of theoretical cosmology

Let me coin the phrase “phenomenological cosmology” to describe the process of trying to distinguish one Friedmann model from another (and, of course, to rule out Steady State during its heyday) by comparing numbers, brightnesses, and angular sizes of galaxies and radio sources at the largest observable redshift (meaning at most 0.5 then) with those here and now. This flourished under Hubble and again beginning not long after World War II, with the optical work concentrated in California and the radio investigations in Cambridge England (though with major contributions from Australia and elsewhere). The critical points were (a) that the optical observations were not definitive, though Sandage (1961) long cherished hopes that the 200” could change the situation and (b) that the counts of radio sources had already ruled out steady state before 1960, though the statistical technique involved, called P(D) and originated by Peter August Georg Scheuer (1955, see Ryle, 1968, for a backward view), took some understanding. And a German astronomer, Mattig (1958, still to be found in the 2003 IAU directory) did a much more thorough job than anyone had before in deriving and presenting in usable form the equations relating observable quantities to redshift and deceleration parameter in FRW universes. None of these had much to do with the early universe or possible fossils of it, though Bondi (1952) occasionally remarked upon the potential significance of helium. And let it not be forgotten that the first modern calculation of helium production in the early universe came from Fred Hoyle and Roger Tayler (1964). Because the total emission of extragalactic sources between 17 and 178 MHz corresponds to a brightness temperature of 2.8 ± 0.7 K (Ryle, 1968), the radio astronomers had an extra opportunity to confuse things, but did not.

Interest in the early universe and calculating things about it seems to have arisen in three places more or less simultaneously, in Cambridge (UK) centered around Dennis Sciama, in Princeton (NJ) centered around John A. Wheeler but with primary input from Robert Dicke and his younger colleagues, and in Moscow (USSR), centered around Yakov Zeldovich.

None of these stories has a particularly happy ending. Enmity in Cambridge between theoretical astrophysics and cosmology on the one hand and radio astronomy on the other dated back to the early days of Steady State

and persisted until a younger generation paved over the chasm around 1970 (I am inclined to credit Martin Rees, who was Sciama’s student on the theory side, and Malcolm Longair, who was Ryle’s and Scheuer’s student on the radio side, but you could prefer other candidates). In Princeton, work was preceding apace, a prediction had been made, and work was going forward on a suitable device to look for primordial microwave radiation (Dicke et al., 1968), when the answer came in from elsewhere in the state.

At least as unhappy is the case of Zeldovich in Moscow. Communication between East and West was exceedingly limited in the 1950s and early 1960s, and what had apparently reached Moscow was either just the abstract or a poor summary of Ohm’s (1961) paper, rather than the complete text with Table II and the surrounding words. Zeldovich took the paper as meaning that there was, at most, “temperature space” for 1K radiation left from a hot, dense early universe. He concluded, therefore, that the Gamow theory had to be wrong, and presented a cold big bang in several papers (Zeldovich, 1962, 1963, 1965). These include explicit plots showing the best estimates (for the time) of the radiation background at 1–100 GHz from galaxies (synchrotron), dotted lines for relict radiation at 0.1, 1.0 and 10.0 K, and a firm dot with arrow pointing downward representing Ohm’s “limit”. He makes the additional point that very high energy cosmic rays (found in 1963) would not be able to travel very far through such a photon sea. This remains, of course, true and was said more firmly by others after 1965 (Greisen, 1966; Zatsepin and Kuzmin, 1966).

Zeldovich’s (1965) review is remarkably prescient on a number of issues and includes brief discussions of the fraction of the critical density that could be present in black holes (collapsed objects to him), neutrinos, and even gravitational radiation. None will dominate, he said. Very strongly in the plus column goes the encouragement Zeldovich gave his younger colleagues to work on details of a hot big bang. The last pre-discovery published predictions of background radiation came from Yuri Smirnov (1964) and from Andrei Doroshkevich and Igor Novikov (1964), the latter drawing attention to the Holmdel horn as a source of past and potential future data, though they suggest that satellite observations would be preferable, owing to the multiplicity of things on earth warmer than 3 K.

Kragh (1996, p. 344–345) has a slightly different take on the significance of Zeldovich’s cold big bang, based (I think) on a misunderstanding about the extent to which steady state vs. evolutionary cosmologies could be made consistent with dialectical materialism, where progress is supposed to happen.

The official discovery paper (Penzias and Wilson, 1965) was one of the last important radio astronomy papers in which the frequency unit was the Mc/s. By 1968, Ryle was using MHz, and the transition period, according to Scheuer, lecturing in 1966, was the “megacycle, sorry, Hertz.”

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