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DAVID TODD WILKINSON
1935—2002

A Biographical Memoir by

BY JOHN MATHER AND P. JAMES E. PEEBLES

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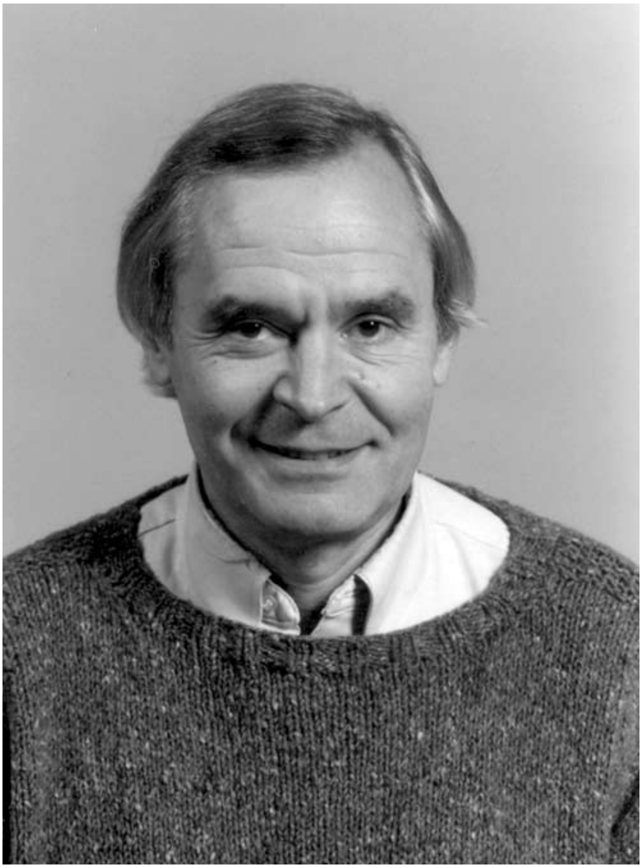
Biographical Memoir



David T Wilkinson

DAVID TODD WILKINSON

May 13, 1935–September 5, 2002

BY JOHN MATHER AND P. JAMES E. PEEBLES

DAVID WILKINSON RECALLED, "My mother [Thelma Todd, d. 1994] 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 [Harold Wilkinson, d. 1994]; that's why I'm an experimentalist. He could build anything and fix anything." Dave Wilkinson made excellent use of those talents. He was a hero to those who knew and worked with him, the sort of expert who could fairly claim to have made all the mistakes and learned from them; but he led by example and inspiration more than by detailed instruction. Dave was passionate about measurements, and he knew how easily Nature could fool our eager selves, so he was the ingenious skeptic who would not accept the quick and easy answer. He was sure that diligent inquiry was going to pay off with important discoveries, and his students and colleagues absorbed that attitude. It served them well, for they went many places and did wonderful things themselves.

Dave's favorite topic was the cosmic microwave background radiation, the CMB, a remnant of the hot big bang. He was part of the Princeton group that was building an instrument to search for the radiation, and in 1965 was scooped by a group at Bell Labs. Dave never expressed regrets that the Princeton group had not been a little quicker; he was much more interested in finding what other secrets Nature might yield. His work on the CMB culminated in the Wilkinson Microwave Anisotropy Probe (WMAP), a NASA mission that produced exquisitely detailed maps and definitive answers to century-old scientific questions, but more about that later.

Dave's scientific accomplishments were widely recognized, though he did not seek fame and glory and he kept his official résumé short. He chaired the Princeton Physics Department from 1987 to 1990. He was elected to membership in the National Academy of Sciences in 1983; he was also a member of the American Academy of Arts and Sciences and the American Physical Society. He received an honorary doctor of science degree from the University of Chicago in 1996 and the National Academy of Sciences' James Craig Watson Medal in 2001 "for elegant precision measurements by Wilkinson, his students, and their students, of universal radiation that is close to blackbody yet wonderfully rich in evidence of cosmic evolution."

Predeceased by an older brother, Ramon (an aeronautical engineer, d. 1999), Dave is survived by his wife, Eunice, of Princeton; a son and daughter, Kenton, of Lubbock, Texas, and Wendy Gordon, of Lambertville, New Jersey, from his first marriage to Sharon Harper, which ended in divorce; three stepchildren, Marla Dowell of Boulder, Colorado, Michael Dowell of Washington, D.C., and Janice Dowell of Lincoln, Nebraska; and eight grandchildren.

Dave's archives have been cataloged (online) by Princeton University and are available for study.

STUDENTS

Dave's preference always was for small research groups that included students. In studies of the CMB and excluding space missions, 13 of his students earned Ph.D.s for work in collaboration with Dave, and an additional 7 postdocs and more senior people worked with and learned from Dave. In other studies, including pulsars and measurements of light from nearby and distant galaxies, the numbers are 12 Ph.D.s and 5 more senior people. One of his undergraduate students, Andrew Lange, in an e-mail message in June 2008, offered this assessment of Dave's ability to attract a consistent flow of capable students:

DAVE'S RULES

• Work on important problems
– better to "fail" at something important than "succeed" at something unimportant

• Make it look fun and easy
– the students won't know any better till it's too late to turn back

• Give the students lots of room (rope?)
– all of the survivors will be great

• Keep an eye out for new technology
– an important problem + great people + new technology = success

• Keep it simple
– you'll be able to move on to the next attempt more quickly

• Be gracious
– nurture everyone's potential

Our poll of a sample of Dave's students suggests all were significant draws. The students Dave attracted loved the chance to use new technology, and even better to apply it to an interesting measurement. But Dave himself used new technology mainly when it added new capability (greater sensitivity, lower noise, and the like), and not when it just made life easier—why use a computer or calculator when a pencil and paper would do, or just estimate the result in your head? Some recall his admonition to choose projects with care—you cannot complete that many—and to present results with care—if you don't point out every peculiarity, others will. Some recall excitement at the idea of contributing to measurements that could change our worldview. Dave's career played out at the center of the growing web of evidence that turned the notion of an expanding Universe into an established part of physical reality. But that change in worldview happened over some 40 years. His students tended to have the more immediate goal of a meaningful measurement, and he proved to have an excellent sense of the subtle pathway between making this happen and allowing it to happen.

Dave set the style and substance for the long campaign of CMB measurements by his own experiments, and by his interactions with students and colleagues and through them their students and colleagues. They now make up a good fraction of the CMB measurement community that has grown into a big science. He was happy to explore speculative ideas about new measurements and unproven technology that might aid the measurements, but he always insisted on close attention to the problems that afflict any instrument; the invention of ways to defeat errors by design, measurement, and analysis; and the critical debate on what is worth publishing for its believability and fundamental interest. He was convinced that the most diligent search for truth is worth doing, that natural complexity and our own ignorance have to be battled every day, and that the battle can be won.

Dave had many interests outside science, and work sometimes combined with family life. His students recall expeditions to the balloon facility in Palestine, Texas, where Dave's father and mother sometimes joined them and they got to admire his dad's mechanical ability in helping with the assembly of equipment. Dave took great pleasure in fishing and camping; skiing and hiking; hockey playing, physics department softball, and motorcycle touring; jazz clubs and bird watching; picnics and family gatherings.

RESEARCH BEGINNINGS

Dave's trajectory is simple to describe: Born in Hillsdale, Michigan, on May 13, 1935, undergraduate and graduate degrees (B.S.E., M.S.E., and Ph.D.) at the University of Michigan, and then to Princeton (an appointment as instructor in 1963, promotion to tenure in 1968, and retirement in July 2002 to Cyrus Fogg Brackett Professor of Physics Emeritus). To judge from his continued interest in the fortunes of the university football team, he had a good experience as a student at the University of Michigan. He completed his doctoral dissertation in 1962, under the direction of Horace Richard Crane, on a precision measurement of the electron magnetic moment, the $g - 2$ experiment.

The elegance and importance of this test of quantum electrodynamics led Robert Henry Dicke to invite Dave to join his group at Princeton University. Dicke persuaded him and another junior faculty member, Peter Roll, to build a radiometer—a device Dicke had invented 20 years earlier—to test the idea that space is filled with radiation left from the hot early stages of an expanding Universe. Dicke's proposal set the direction for Dave's career, an example of how a conversation can change history.

In 1965 word of the Roll-Wilkinson experiment reached Arno Penzias and Robert Wilson at the Bell Labs in Holmdel, some forty miles from Princeton. The story of this event has been told many times. The Bell group was working with a low-noise microwave receiving system that had been built for satellite communications tests, and as scientist-engineers Penzias and Wilson were using it for astronomy. This instrument was known to indicate slightly more radiation from the sky than expected. Penzias and Wilson made a convincing case that the excess radiation at 7.4 cm wavelength could not be attributed to sources in and around the system. They had an effect but no interpretation; they were unaware that George Gamow, Ralph Alpher, and Robert Herman had predicted the existence of the radiation in 1948. The Princeton group knew what they were hunting for, and once the groups made contact it was clear that the Bell system had produced evidence for it.

If this cosmic microwave background radiation (the CMB) is a remnant from the big bang, its spectrum is expected to be close to the blackbody form produced by the thermal equilibrium conditions of the primordial material. By the end of that year the Roll-Wilkinson experiment gave the first test. They detected the radiation at a shorter wavelength, 3.2 cm, and they showed that the spectrum between the Bell and Princeton wavelengths is consistent with blackbody, with $I_\nu \propto \nu^2$ at long wavelengths. It took another 25 years of work to show that the spectrum is very close to blackbody over its broad range of wavelengths. The significance is immense. For many years prior to this discovery the big-bang picture had competed with the steady-state theory and other ideas in vigorous, sometimes almost vicious debates. The CMB settled the issue: the Universe has to have expanded from a state dense and hot enough to have produced this blackbody radiation.

To give a little more detail, we recall that space is observed to be close to transparent at CMB wavelengths—radio-luminous galaxies are observed at great distances at such wavelengths—so radiation cannot have been forced to relax to thermal equilibrium in the Universe as it is now. Relaxation would have to have happened in the early hot and dense conditions of an expanding Universe, when heat radiation would have been emitted and absorbed rapidly compared with the rate of expansion. That would produce thermal equilibrium among all forms of matter and energy. As the Universe expanded the heat radiation would cool but preserve its blackbody form. In the Universe now there are other important radiation fields: light from stars, infrared emission from dust, X rays from exploding stars, and so on. But the photons of the primordial radiation far outnumber all other photons and particles, so the heat radiation was expected—and later observed—to have preserved a blackbody form. The tiniest details of the departures from a blackbody spectrum are important too, of course, as markers for what happened later.

Dave was involved in all the many steps of the measurements that have proved that the CMB really is a remnant of the big bang. The first two tests are whether the CMB is close to the same in every direction, as befits heat radiation from a near-uniform Universe, and whether the spectrum is close to the distinctive blackbody form. The next, still more demanding test is whether the CMB has the expected slight departures from an exactly uniform sea of radiation. These would result from its disturbance by the clumpy distribution of matter in the Universe as it is now and predicted to have been in the past. The measurements by Dave and colleagues have given us a far richer web of evidence of the nature of this cosmic evolution than anything Wilkinson and Roll could have imagined in 1964 when they set out to search for radiation from the big bang.

CMB SPECTRUM MEASUREMENTS

By 1968, three years after the identification of the sea of microwave radiation, 12 ground-based measurements had checked the critical blackbody signature, and there was an important though more indirect check from the spin temperature of interstellar cyanogen (CN) molecules that act as a radiation thermometer. The large number of measurements from groups in England, the Soviet Union, and the United States shows conditions were right to measure the CMB once someone influential enough—Dicke, as it turned out—thought to suggest it. (One can debate whether the measurements could have been done much earlier. Perhaps, if anyone had realized how important it was.) Five measurements came from Dave, with Bruce Partridge and Paul Boynton, fellow junior members of the Princeton faculty, and Dave's first graduate student, Bob Stokes (Ph.D., 1969).[1] This group certainly was not alone, but it was the most tightly focused, and Dave had become the reliable critic people went to with their CMB measurements.

In the first temperature measurements, in the mid-1960s, Penzias and Wilson found $T = 3.1 \pm 1$ K, Roll and Wilkinson $T = 3.0 \pm 0.5$ K. Dave Johnson (Ph.D., 1986), in a balloonborne measurement above most of the radiation from the atmosphere, brought the value to $T = 2.783$ K at 1.2cm wavelength. They presented a "conventional error" in the measured temperature, $\pm$ 0.025 K, from the sum of uncertainties in quadrature, which would put their result 2.3 standard deviations higher than the later satellite measurement. But they also presented a "conservative error," from the sum of magnitudes of uncertainties, $\pm$ 0.089 K. The big difference is a reminder that an accurate temperature measurement is difficult, and it illustrates Dave's cautious attitude.

By 1971 the measured CMB spectrum had been shown to match the long wavelength power law part of a thermal spectrum. The critical question became whether at shorter wavelengths the spectrum is still close to thermal, with a peak and then exponential decrease toward shorter wavelengths.

Checking this is next to impossible from the ground—the atmosphere gets in the way—and heartbreakingly difficult above the ground, though people did try. By 1971 one rocketborne radiometer measurement had indicated that the energy in the CMB is about consistent with a thermal spectrum, but it only roughly constrained the shape of the spectrum. Worse, two other experiments, one rocketborne and one balloonborne, found evidence for significantly more energy than thermal at short wavelengths. In the 1970s and 1980s a sequence of aboveground measurements tightened the precision but left indications of CMB energy in excess of thermal that might be real or might be a systematic error. This unsatisfactory situation at last ended in 1990 with two independent measurements discussed below that showed the spectrum is very close to thermal. This gave us close to tangible evidence that our Universe evolved from a very different condition, a memorable advance.

COSMIC BACKGROUND EXPLORER—COBE

In 1974, just five years after the *Apollo 11* mission first landed on the Moon, NASA sought five proposals for new satellite missions. A meeting to discuss what became the COBE mission to explore cosmic background radiations, in September of that year, included John Mather and Patrick Thaddeus, then at NASA Goddard Institute for Space Studies; Rainer Weiss and Dirk Muehlner from MIT; Dave Wilkinson from Princeton University; Michael Hauser from the NASA Goddard Space Flight Center; and Joe Binsack, deputy director of the MIT Center for Space Research. They planned four experiments (though they didn't have these names yet): FIRAS (the Far Infrared Absolute Spectrophotometer) to measure the CMB spectrum, DMR (Differential Microwave Radiometer) to go after the departures from an exactly smooth sea of radiation, a shorter wavelength version of DMR (that never flew), and DIRBE (Diffuse Infrared Background Experiment) to find the background radiation at wavelengths shorter than where the CMB dominates. Two years later NASA chose a Mission Definition Study Team that included Hauser, Mather, Weiss, and Wilkinson. It also included George Smoot from the University of California, Berkeley, and Sam Gulkis from the Jet Propulsion Lab, both members of teams that proposed versions of what became DMR. Now the team had three DMR experts, any one of whom could have led the development of that instrument. Dave didn't want to be principal investigator, knowing the many meetings at NASA that would require. But later in life he did jump with both feet into another space mission, MAP, later renamed WMAP, a story we tell later.

Dave's most notable role in the COBE was as the referee, the person who had to be convinced before any result could be declared as originating from the team. As the expert who knew of plenty of mistakes that could be made, he always had a feeling for what could go wrong, and insisted that everything had to be checked and rechecked. In the case of the spectrophotometer used to measure the spectrum of the CMB (FIRAS spectrum experiment), he undertook to measure the reflectivity of the calibrator body in its position in the antenna, something NASA was not set up to do. Dave drove his pickup to Goddard, drove home with the antenna and calibrator, and in a few weeks he had real data, something far better than armloads of calculations.

The COBE was launched on November 18, 1989, 15 years after it was conceived, and within weeks had shown that the CMB spectrum is very close to blackbody. Although the whole COBE team was sworn to secrecy until the public announcement, Dave carried a copy of the new spectrum folded up in his shirt pocket, and a few privileged people saw it in private viewings slightly in advance of the announcement. He was very proud of it. The public presentation of this simple but deeply important spectrum drew a standing ovation from the American Astronomical Society.

The initial result came from a simple comparison, that the spectrum of the calibrator—a near blackbody—matched that of the sky when the calibrator temperature was adjusted to about 2.73 K. After years of further work by the FIRAS calibration expert, Dale Fixsen (one of Dave's former students), the final answer from COBE was $T = 2.725 \pm 0.001$ K. Dale is a person to whom a least-squares fit in 4000 variables is properly expressed in tensor notation, like general relativity. Dave was amazed and pleased, but of course permanently skeptical that the very best number had been found.

Only a few weeks after the COBE was launched, Herb Gush and colleagues at the University of British Columbia launched a sounding rocket experiment that obtained a consistent result, $T = 2.736 \pm 0.017$ K. The detector was more sensitive than FIRAS, but the uncertainty is larger because they had just a few minutes of rocket flight. If the rocket had been launched a little earlier, or any of Herb's previous launches had succeeded, history would have been different for the COBE team, too.

CMB SPECTRUM AFTER COBE

The COBE/FIRAS check of the CMB thermal spectrum will be hard to improve: it is difficult to imagine a project any time soon to remeasure the part of the radiation spectrum the FIRAS and Gush experiments reached. We can learn more, however. Since our Universe is not exactly transparent, and matter temperatures now are much larger than that of the CMB, the spectrum has to have been somewhat perturbed from thermal, likely most significantly in the tails at long and short wavelengths, where the CMB energy is relatively small—and outside the ranges of the precision experiments we have now. Measurements of the tails are not easy. Dave kept at it into the mid-1990s, in measurements at relatively long wavelengths, with colleague Norm Jarosik, former graduate student Stephan Meyer, and students Chris Smith (Ph. D., 1997), Suzanne Staggs (Ph.D., 1993), and Ed Wollack (Ph.D., 1994). The effect has to be there, and it will tell us something interesting about the temperature and density history of the plasma the CMB has passed through. These experiments improved the limits on the size of the effect but did not find it. A satisfactory completion of this assignment is a task for the next generation.

CMB TEMPERATURE MAPS—THE ANISOTROPY

After the spectrum, the big issue is what a map of the CMB sky looks like. How important are local sources of radiation at these (centimeter and millimeter) wavelengths? Is there a measurable effect of Earth's motion through the CMB? Are there signs of the effect of the hypothetical lumps in the primordial material that would grow into the observed concentrations of matter in galaxies and clusters of galaxies? Dave and his students worked on all parts of these questions. All the experimenter's skill was required to find the tiny variations of the CMB across the sky behind huge and unstable emissions from the instrument itself, from the atmosphere, from the ground around the observer, and from matter in the Milky Way and other galaxies. Everything hinges on experimental designs that can distinguish between them, and almost all depend on some kind of differential device like the "Dicke switch." The art and science of the CMB observer are devoted to these modulation schemes.

AETHER DRIFT—THE COSMIC DIPOLE

The Universe defines a preferred rest frame in which the CMB and the galaxies are observed to be as close as possible to isotropic. An observer moving relative to this frame sees in effect a wind: the CMB is warmer in the direction toward which the observer is moving, cooler in the opposite direction, varying by the fractional amount $\delta T/T = (v/c) \cos \theta$. Here v is the velocity relative to the preferred frame, c is the velocity of light, and θ is the angle between the line of sight and the direction of motion. This reminds one of the old notion of an aether drift, but one that does not violate relativity; it is motion relative to the CMB.

In 1967 Penzias and Wilson reported that the velocity effect is limited to $\delta T/T \lesssim 3 \times 10^{-2}$. Wilkinson and Partridge improved this to $\delta T/T \lesssim 2 \times 10^{-3}$, which showed we are moving no faster than about 600 km s^{-1} relative to the preferred frame. Penzias and Wilson obtained their bound by comparing the radiation detected at different times of day, when their instrument was pointing at different parts of the sky. The inevitable drifts in instrumental noise and sensitivity and the changing amounts of radiation from the ground and sky that find their way into their instrument limit the accuracy of this measurement. The familiar remedy in experimental physics is a switching experiment: measure the difference of the antenna temperatures at different positions in the sky by switching the instrument beam between positions as rapidly as possible. But beware that switching might disturb the instrument, introducing an artificial difference of sky temperatures.

Wilkinson and Partridge used two switches: a large metal plate reflector moved the beam of a fixed instrument, and a faster switch compared their main horn antenna with one pointed to the zenith. They got within a factor of two of what proved to be the dipole amplitude, but they were stuck there. One problem was instrument sensitivity. Advancing technology solved that. Another was radiation from the atmosphere. Water vapor is a big and highly variable source of microwave radiation, and the measurement site, the rooftop of the Princeton University geology building, Guyot, wasn't exactly dry. To solve that they moved the instrument to a place that weather records show to be among the driest in the United States: Yuma, Arizona. The site, an army base, was convenient for the experiment, apart from the occasional black widow spider. But there was still too much water in the atmosphere, likely streaming in on high from the Gulf of California. Later, Dave's teams moved to higher and drier sites: mountains, balloons, the plains of Saskatchewan, Canada, and eventually to space. A third problem was the clumsy method of beam switching by swinging a large mirror. Wilkinson recalls that "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 and went up there and dismantled our reflector." There were no such escapades in Yuma, but the switching was still too slow.

Ron Bracewell at Stanford University with his graduate student, Ned Conklin, found a much quicker switch: point two horn antennas to different parts of the sky and use electronic switching to compare temperatures in the two directions. They reduced the problem with radiation from atmospheric water vapor by going to White Mountain near Owens Valley, California. It is a pretty good site, and other CMB groups, including Dave's, used it for measurements of the spectrum. But Conklin and Bracewell's horns were not easily moved; they instead used Earth's rotation to scan a band of the sky. To do better one wants to scan more sky and do it faster.

Paul Henry (Ph.D., 1970) did that by using a balloon to lift the detector, with the Conklin-Bracewell two-horn design. In flight the instrument rotated with a period of 0.8 minute, a whole lot faster than 24 hours. The spin caused the two horns to scan very nearly the same circle around the vertical, and Earth's rotation moved the circle, so through the night the instrument scanned a good part of the sky. The high balloon altitude suppressed radiation from the atmosphere, and instrument drifts were strongly suppressed by the double switching: rapid comparison of the two antenna temperatures and a 0.8-minute scan of the sky. The strategy is effective: it was used in the COBE and then the WMAP satellite that, as we will describe, obtained the most precise CMB anisotropy measurements so far.

But, proving that the expert is the person who has already made all the mistakes, Dave learned something very important from this project. One might have thought that in the tranquil environment of the upper atmosphere the rotation of the instrument would have no effect on the antenna temperature difference that originates within the instrument. But Henry's apparatus used a magnetic field to polarize a ferrite bead in the switch, and Earth's magnetic field had a slight effect on the switch that varied with the orientation of the instrument. Unfortunately, that variation looks a lot like the CMB anisotropy signal Henry was looking for. This systematic error limited him to only a marginally significant separation of the CMB anisotropy. A lesson for subsequent experiments, including COBE and WMAP, was clear: think of everything that could go wrong, put a lot of effort into designs that minimize systematic errors, and think of ways to analyze the systematic errors that inevitably remain.

This project also tells us a bit about how Dave dealt with his students. Henry had most of the responsibility and was largely left alone to design and run the experiment, with Dave looking on, stepping in only when problems got serious. Henry's first flight was spoiled by inadequate shielding of radiation from the Moon. Dave approved another flight, and he "volunteered" to come along this time to help. Henry recalls that he had "constructed a screen according to the design that I [Henry] 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 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." Dave's presence often was crucial, but he also expected his students to play a major role.

Conklin and Henry both detected the dipole, we believe, at $\delta T/T \simeq 10^{-3}$, but neither result was compelling enough to draw the attention of the community. Dave is recalled as having said that if he didn't find the anisotropy due to our motion around the Milky Way galaxy he would keep pushing until he could see Earth's motion around the Sun, as in the aether drift experiments a century ago. It turned out not to be necessary, but the annual variation in the dipole anisotropy is seen in satellite measurements.

In a balloon measurement with Henry's double-switching strategy and better control of the crucial systematic error from instrument offset, Dave and Brian Corey (Ph.D., 1978) obtained a clear detection of the dipole. They presented their first measurements in 1976 but only at a conference; the refereed publication three years later reported more measurements at three wavelengths. That was important because one has to correct for radiation from our Milky Way galaxy, which is quite irregularly distributed across the sky. Fortunately, galactic radiation and the CMB have different spectra, so the multifrequency measurement can distinguish them. Meanwhile, at the University of California, Berkeley, George Smoot, Marc Gorenstein, and Richard Muller reported measurements from a U-2 airplane. That experiment had one wavelength, but it was short enough that the correction for the galaxy was not a serious problem. Their measurement gave a clear demonstration of the dipole anisotropy from our motion relative to the rest frame defined by the CMB.

The detection of the aether drift shows us how fast we are moving through the Universe. This can be turned into a quantitative test of ideas: compare our velocity to what would be expected from the gravitational acceleration computed from the distribution of mass around us. But the measurement of this mass distribution is difficult, so the comparison of the measured speed with the theoretical prediction still isn't very accurate. The way ahead proved to be to apply the experience gained so far to measurements on smaller angular scales.

INTRINSIC COSMIC LUMPINESS

If the big-bang theory is right and we have the right idea of how galaxies formed through the action of gravity, then there must have been primordial seeds of present-day structure. In many areas of physics small disturbances grow exponentially, so that all traces of the initial conditions are lost. The hypothetical butterfly that flaps its wings in Brazil and causes a hurricane in the Caribbean is an extreme example. But in cosmology the effect of the expansion of the Universe is to produce a near power law (rather than exponential) growth of departures from exact homogeneity. That means we ought to be able to find traces of initial conditions in the distribution of galaxies and in tiny disturbances to the CMB. We now characterize all this with power spectra, for the distribution of matter in three dimensions, and for the CMB sky map in two dimensions.

Matter and radiation interact largely by gravity and by the scattering of radiation by free electrons. The physics is simple, and the physical conditions are reasonably simple, too, because the important interactions between matter and the CMB happened in the early Universe. Since the expanding Universe is unstable, the early Universe has to have been very close to homogeneous to have ended up as close to uniform as we see it. The small early departures from homogeneity thus can be analyzed in linear perturbation theory, which makes the problem easy enough to solve. The basic concepts had been worked out around 1970 by Peebles and Yu and in the Soviet Union by Yakov Zel'dovich and his colleagues.

That left three big questions. First, what are the principal dynamical actors. What is the Universe made of? Now, in 2009, we have learned that we must add two components, nonbaryonic dark matter and dark energy—the new name for Einstein's cosmological constant—and that it is safe to ignore others, such as cosmic strings at the present level of the evidence. But none of this was at all clear in the 1970s. Second, what is the nature of the departures from exact homogeneity in the early Universe? One could imagine different primeval distributions of matter and radiation. The evidence now is that matter particles and photons had to have been distributed in very nearly the same way, in a near scale-invariant, random Gaussian process. Third, do our laws of physics apply to the fantastic conditions within the big bang, or more precisely, how far can we extrapolate the established laws? For example, we have demanding tests of the standard theory of gravitation, general relativity, on scales ranging from the laboratory to the Solar System at $\sim 10^{13}$ cm. We are applying this theory on scales ranging to the edge of the observable Universe, at $\sim 10^{28}$ cm, and to the extreme conditions of the very early Universe. It is remarkable that our extrapolation works at all, but the evidence now is that it does.

MEASURING THE LUMPS

When the CMB was identified in the mid-1960s, none of the issues underlying the interpretation of an accurate CMB sky map were well understood, so experimenters like Dave just went hunting for whatever Nature might have provided. In the end the intrinsic cosmic lumps were tough to find, being only tens of parts per million of the rather faint CMB, and a huge challenge to the experimenter. Dave spent the largest part of his research career on this challenge. It took real courage: for the first quarter-century we had only upper bounds, not detections, of anything in the CMB sky beyond the dipole anisotropy from our motion through the radiation.

The advances in the measurements drove improvements in the theoretical situation that started to offer interesting targets for the experiments. For example, for a while in the early 1980s there appeared to be evidence for a large-scale variation of the CMB temperature across the sky, in the form of a quadrupole anisotropy (the next term in the systematic spherical harmonic expansion of the anisotropy spectrum to progressively smaller angular scales). Dave, his colleague Steve Boughn, and a student Ed Cheng thought they saw the quadrupole in measurements from three balloon flights from Palestine, Texas. It apparently confirmed the quadrupole detection from a balloon flight from Sicily announced by Francesco and Bianca Melchiorri and their group in 1980. That led to a theory to fit the quadrupole; it was easy because there were many options. But in their dissertations Cheng (Ph.D., 1983) and Dale Fixsen (Ph.D., 1982) withdrew the apparent detection and replaced it with tighter upper limits. So the Princeton group withdrew the theory and introduced another, CDM, so named for the important role played by the hypothetical nonbaryonic cold dark matter. With the addition of dark energy—Einstein's cosmological constant—it became the present standard cosmology.

And so it went: experimenters and theorists pushed and pulled and stumbled, and in the process established our new worldview with two rather startling parts, dark matter and dark energy, that (so far) only astronomers can measure. Dave did not hesitate to take part in this tumultuous process, but he insisted on clarity in the part he could control: he made sure his group's measurements were well documented, and when his measurements had gone wrong, as inevitably happens, he made sure the world knew it.

Some of the early measurements used general-purpose radio telescopes to search for fluctuations of the CMB sky temperature on relatively small angular scales, around a few minutes of arc. The Princeton group contributed, as did quite a few others. Early measurements used a 15-foot telescope at the Aerospace Corporation in California (1967), the large Pulkovo radio telescope in the USSR (1971), the 64-m Goldstone facility in California (1973), the Parkes Radio Telescope in Australia (1974), the National Radio Astronomy Observatory (NRAO) 42-m telescope in West Virginia (1978), the 91-m NRAO telescope in West Virginia (1980), the 11-m Kitt Peak National Observatory telescope in Arizona (1973), the Owens Valley Radio Observatory 40-m telescope in California (1980), and the 25-m × 38-m Jodrell Bank telescope in England (1983). (The dates are approximate, and refer to first measurements when there were several at the same instrument.)

What drove all this work at so many telescopes? These measurements were an important early check of the idea that the CMB might not be cosmic but rather radiation from objects in the Universe as it is now. If so the objects would be resolved by a big enough telescope. There was also the general conviction that the clumpy distribution of mass in galaxies and concentrations of galaxies had to have had some effect on the CMB. But our impression is that what drove this work was much simpler: the measurements could be done using existing facilities, so they ought to be done.

Anisotropy measurements on larger angular scales use special-purpose instruments built to carefully crafted strategies to pick out the intrinsic CMB anisotropy, $\delta T \lesssim 10^{-4}$ K, in an environment near room temperature, $T \simeq 300$ K. An example of the subtle design strategies is Peter Timbie's (Ph.D., 1985) interferometer that rapidly compares temperatures at angular separation $\simeq 2°$. Timbie's site, the roof of the Princeton physics building, was appropriate for the development of this technique but not very good for measurements. Timbie and Wilkinson took their instrument to the plains of Saskatchewan, where the winter air can be excellent: cold, dry and stable. The Saskatchewan team later included Mark Devlin, then a postdoc; Lyman Page, then a new member of the Princeton group; Ed Wollack (Ph.D., 1994); Barth Netterfield (Ph.D., 1995), Lyman's student; Jarosik; Wilkinson; and later Angélica de Oliveira-Costa and Max Tegmark, then at the Princeton Institute for Advanced Study. The group was growing but still organized in the style and scale Dave preferred.

These certainly were not the only degree-scale measurements: other combinations of people and institutes were taking data from balloons launched from Sicily, Texas, New Mexico, and Australia. Other measurements came from the Observatorio del Teide in the Canary Islands; from Antarctica, where the air is thin and really cold and dry; and from Cambridge, England, where elaborations of the interferometer technique we mentioned earlier overcame the less favorable observing conditions. By the mid-1990s it was clear that the anisotropy had been detected—to perhaps 25 percent—on angular scales ranging from about 0.3° to 3.0°. But the pursuit of anisotropy also required much larger operations.

COBE ANISOTROPY MEASUREMENTS

The search for the best place for anisotropy measurements of course included space. The first CMB satellite mission, RELIKT in the Soviet Union, was launched in 1983. It detected the dipole anisotropy and placed a useful upper limit on the quadrupole. Dave was a charter member of the science team for the second, COBE, launched in 1989. In 1992 the Differential Microwave Radiometer (DMR) group on COBE announced the first clear detection of the intrinsic CMB anisotropy, about a part in 100,000 rms, measured on angular scales greater than about 7°. This was a profoundly important advance: we at last had a measure of what happened to the CMB as cosmic structure formed. If there were not these "little zits on the face of the Universe," as Dave called them, we would not exist because no part of the Universe would have stopped expanding to form a galaxy for us to live in.

As in the COBE/FIRAS spectrum measurement, Dave played a critically important role in the DMR work, mainly as a wonderfully creative thinker about what could and would go wrong. When Ned Wright first reported to the science team his analysis indicating the COBE anisotropy detection, Dave was among the most vigorous skeptics. He had a long list of things for the team to check, and he went over all the computations. To be certain the COBE team did every important calculation at least twice, with independent programs. The biggest uncertainty, about which Dave might never have been convinced, was the effect of diffracted radiation from Earth that might creep over the edge of the COBE sunshield and into the DMR antennas. When the DMR discoveries were published, Dave didn't object, but he showed that he was still "worried," one of his most common words in these discussions.

WMAP—THE WILKINSON MICROWAVE ANISOTROPY PROBE

In early 1991 Dave started pushing for a new satellite mission that would be far more powerful than the COBE/DMR. Considering his reluctance to go to endless meetings with NASA, this was a strong statement on his part: he was determined to get to the end of things, to resolve those nagging worries by new and better measurements.

Dave's first two space proposals were submitted before anyone knew for sure that there is a detectable intrinsic anisotropy.[2] In the second proposal there is the remark that anisotropy "in the CMB at angular scales of $5'$ to $5°$ carries the imprint of density fluctuations which eventually led to large-scale mass structure—clusters of galaxies, sheets, and voids." At the time, the CDM model for how this structure formation happened was widely discussed—it led to a promising way to understand the properties of galaxies—but it was not the only theory on offer: people were considering cosmic strings, explosions, and still other lines of thought. Dave was driven largely by the experimenter's wish to find the CMB intrinsic anisotropy, not to use the measured anisotropy to test a specific model for how structure formed.

The state of thinking about CMB anisotropy measurements from space is illustrated by the report of a NASA Medium-class Explorer (MIDEX) Workshop on March, 27, 1991. Three CMB mission concepts were presented: from Phil Lubin at the University of California, Santa Barbara; George Smoot, Andrew Lange, and Paul Richards at the University of California, Berkeley; and from Dave Wilkinson. All considered a single beam defined by an off-axis reflector, all had a scheme for sky scanning to suppress effects of instrumental drifts, and all mentioned the use of the new technology of HEMTs (high electron mobility transistors, from the National Radio Astronomy Observatory) that had considerably better sensitivity than the mixer receivers on COBE/DMR. In Dave's design, which was the simplest and most optimistic, the beam formed by a primary reflector would scan a 10° circle in the sky. There would be no switching to a reference load; he proposed that the benign environment might keep the instrument stable enough that a one-minute scan by the rotating instrument would take care of instrument drifts. His science goal was limited to two 20°-diameter sky maps at the galactic poles where the foreground galactic radiation is least, using three radiometers to sample a range of frequencies (40 GHz to 90 GHz) to be used to measure and remove foreground radiation.

In August 1991 Page and Wilkinson submitted a second concept, which was closer to what flew, in a proposal to NASA for funding to support development work with Jarosik and Wollack. They proposed a differential instrument with symmetrical beams, using waveguide techniques and correlation detectors to produce nearly instantaneous measurements of the sum and difference of the intensities arriving at the two antennas, thus eliminating the need for mechanical or ferrite switches. The idea of correlation radiometers goes back at least to the early 1950s, but improved technology and systematic error control made the 1991 proposal very interesting. Twenty years earlier Conklin, Bracewell, and Henry had shown the power of two sky beams. Two sky beams were used in later measurements, including the Soviet RELIKT and NASA COBE/DMR space missions. The Page and Wilkinson addition of fast coherent differencing would have helped reduce the effect of instrumental drifts that were worse than Dave had assumed in his first proposal. But what flew was still better: a phase-sensitive amplification and differencing strategy developed by Jarosik, Page, and Wilkinson.

By 1993 discussions among groups considering CMB anisotropy measurements from space had led to the first foundations for what became the Planck CMB anisotropy mission. In addition, Chuck Bennett at the NASA Goddard Space Flight Center had been arguing for a MIDEX-class program within NASA, and also for Goddard support for a CMB mission proposal. After initial discussions with the Jet Propulsion Laboratory and several potential industrial partners, Dave decided to try his luck with Goddard, and formed a partnership with Chuck.

The Goddard and Princeton groups formed the Microwave Anisotropy Probe (MAP) collaboration. Bennett was to be principal investigator, overseeing all aspects of the project. Wilkinson was to be "instrument scientist," a title Bennett made up to reflect Dave's core interest in the instrument and the issues that could affect the scientific outcome. (In later years Lyman Page, who always played a centrally important part, almost entirely took over this role.) Dave was delighted that the Princeton physics machine shop would fabricate key parts, including the elegant corrugated horn feeds. He had said that the machine shop was more important to him than the university libraries.

The MAP collaboration spent the better part of a year working out the measurement strategy, with Dave once again in the role of referee who insisted on being convinced that all options had been considered, that the best was chosen, and that the best was capable of doing the job. The choice of HEMTs seems to have been easy. An alternative (bolometers) has better sensitivity and stability but requires cooling, which is expensive, and this was the time of "faster, better, cheaper" at NASA. There was more discussion of strategies for scanning the sky: a single beam or a sky temperature difference. The notes we have seen suggest Dave continued to like a single beam because it makes the data analysis conceptually simpler. But by mid-1994 it had become clear from the experience in Saskatchewan and from Jarosik's JPL-supported studies that HEMTs are not stable enough for Dave's first concept, even in his unicorn design that would spin at an exceedingly fast one revolution per second. That left a single sky beam referenced to an internal load, or a two-beam sky temperature difference measurement. The Planck mission uses the former strategy for their HEMT detectors, MAP the latter. On the issue of the satellite orbit Wilkinson's notes from 1991 show he was paying careful attention to the advantages of the Sun/Earth Lagrange point L2 that puts the Sun, Earth, and Moon in the same part of the sky and always well away from where the instrument would look. But as usual he wanted to be convinced. His memos in 1994 return to the advantage of an Earth orbit; the shorter distance would simplify data transmission. Planck will go to L2, which was the plan for the Soviet mission RELIKT II, and it was the right choice for MAP. Also characteristic of Dave is a June 1994 memo in which he mentioned that he would be "delighted if the medium scales could be mapped without a satellite." All this might have been a little trying for colleagues who wanted to get on with the project, but the style can be effective, and the tight collaboration of Princeton and Goddard certainly was successful.

The MAP MIDEX proposal was submitted in June 1995; NASA approved it in April 1996. The competition was serious, against two similar and excellent proposals from California, one from JPL, and one from Caltech, as well as against missions with other science goals.

Dave's colleagues recall some tense moments in the formalities. NASA's approach assumed the Princeton group would be considered a contractor, but Dave naturally assumed equality in a partnership. NASA insisted on certain formalities in the area of quality control that were intended for huge aerospace firms and didn't fit Princeton at all. Dave would have none of that. Chuck Bennett quieted the waters by sending a brilliant quality assurance manager to Princeton to explain with clear examples why there is more to quality assurance than bureaucracy. His instructions were to come back with a quality assurance plan that satisfied both Princeton and NASA, and somehow it happened.

The mission was launched on June 30, 2001. By this time a considerable number of other measurements had shown that the anisotropy spectrum is close to what would be expected in the CDM model for structure formation, and that the anisotropy peaks about where CDM would put it if the Universe were cosmologically flat (with curved spacetime but space sections that have Euclidean geometry). Many theorists liked that because it would agree with the idea that inflation—a time of exceedingly rapid expansion of the very early Universe—ironed out spacetime curvature fluctuations to produce the observed near homogeneity of the Universe, in the process ironing out large-scale space curvature as well. The mission considerably improved the precision of anisotropy measurements on angular scales greater than about 0.2°, and it straightened out the measurements on larger angular scales that are difficult at lower altitudes where Earth gets in the way. The measurements may be expressed as impressively precise constraints on the combinations of cosmological parameters such as space curvature to which the anisotropy spectrum is sensitive. But more important is the consistency of these constraints with a growing number of cosmological tests based on measurements of other phenomena. That consistency among independent ways to look at the Universe established the case that the parameter measurements are accurate as well as precise.

Dave Wilkinson died in 2002, 16 years after diagnosis of cancer. The first-year results from the CMB space mission were released a half year after his death, but Dave saw preliminary measurements of the map of the microwave sky and of the anisotropy spectrum and knew the probe was taking data to his satisfaction. Before the first data release the science working group proposed a new name, the Wilkinson Microwave Anisotropy Probe, WMAP. Bennett recalls that renaming the mission is not something that is lightly proposed in the large NASA operation, but the merits of the case were seen and accepted. Dave's family attended the renaming ceremony at NASA. His unease with bureaucracy and his lifelong disinterest in seeking credit and recognition lead us to wonder how he would have felt about this honor. We think he would have been a bit embarrassed by the attention, but he would have to agree that it was merited.

Dave was the one person always at the center of the experiments from the first exploratory measurements of the CMB to the extraordinary exercises in precision in the COBE energy spectrum and the WMAP anisotropy spectrum. He was there as these measurements drove the growth of cosmology from a speculative science with a modest empirical basis to a tight web of evidence that shows our Universe is evolving and the evolution is well approximated by the general relativity theory Einstein found nearly a century earlier.

CMB POLARIZATION AND THE FUTURE

The CMB is very slightly polarized, we now know, and Dave seems to have been the first to design experiments to look for it. He knew about Martin Rees's remark in 1968 that anisotropy with free electron scattering produces CMB polarization, but it is typical of Dave that he was more strongly motivated by the possibility of a measurement. That led to three senior theses for independent study, by

William F. Baron (1969), Donald W. McCarthy (1970), William N. Cunningham (1971), and a doctoral dissertation by Pete Nanos (Ph.D., 1973). Suzanne Staggs, who was one of Dave's graduate students and is a member of the Princeton faculty, is leading one of the groups aimed at improving the measurements of the polarization and seeing what it tells us. There is no way Dave could have anticipated that his notion of an interesting experiment in the 1970s would grow into a part of big science, but that is how the enterprise of science sometimes evolves.

Polarization was acquired when the CMB last bounced off electrons: if the radiation field seen by the electrons has a quadrupole anisotropy, the radiation we receive has a small linear polarization. So the measurements are capable of probing the CMB distribution when the Universe was young, not just today. The observed level of polarization is an order of magnitude below the CMB temperature anisotropy, and there may be another recognizable pattern of polarization another order of magnitude or two below that, induced by very long-wavelength gravitational radiation produced during inflation. Dave absolutely would have enjoyed the challenge of devising new modulation schemes to measure these faintest whispers of the big bang, and he would have been interested, but perhaps not entirely convinced, by the theoretical explanations.

The Planck mission is the next in space to probe the CMB. It will improve on WMAP by using shorter wavelengths to gain higher angular resolution and more information to compensate for foreground emissions, and it will improve on the WMAP polarization measurements. And around the world other groups are working on ground- and balloon-based experiments that may measure that tiny polarization from the big bang and scoop the satellite folks. Dave would have liked that, too.

SCIENCE OUTSIDE THE BIG BANG

Dave certainly was not entirely occupied by studies of the CMB, and neither were his graduate students: about half wrote theses on other topics. Here are some highlights of what he and his students were doing when not thinking about the CMB and the big-bang cosmology.

When Dave came to Princeton he joined Bob Dicke in a group working on measurements of the Earth-Moon distance by the roundtrip timing of laser pulses sent to the Moon and returned by an array of corner reflectors placed on the Moon for this purpose by the *Apollo 11* astronauts. The group was large by the standards of the day for gravity experiments, but their style fitted Bob and Dave: the pulse-timing system and reflector array were built for the well-defined purpose of testing general relativity theory. By 1970 the group reported lunar distances measured to about 2 m. Now distances from the Moon and geodesic satellites to places on Earth are measured to 1 cm, with improvements in progress. That affords demanding tests that, so far, Einstein's theory passes.

In the 1970s the astronomy community was moving from photographic plates to the much better quantum efficiency and linearity of digital photon detectors (with the penalty of much smaller fields of view). A list of dissertations gives some flavor of the new physics and astronomy opened up by this transition. Ed Groth (Ph.D., 1971) used a photomultiplier detector for precision timing of the recently discovered optical pulsar in the Crab Nebula. He saw the slowing of the rotation rate by magnetic drag, and glitches marking accommodations of the neutron star shape to its slowly increasing rotation period. Mark Nelson (Ph.D., 1972) used a similar setup to search for other optical pulsars. Marc Davis (Ph. D., 1973) used a digital vidicon panoramic detector that was under development at the Princeton University Observatory for possible use in a space telescope. Davis looked for galaxies at high redshift, seen as they were when they were young because of the light travel time. Dave also inspired a related search for young galaxies by Bruce Partridge. This has become a big topic of research that has taught us a lot about the evolution of the galaxies. Bill Stoner (Ph.D., 1974) measured the diffuse light—presumably starlight—between the galaxies in one of the nearest rich clusters, the Coma Cluster. This intracluster starlight was and still is an interesting indication of past interactions among the cluster members. The relatively large angular size of the cluster required a return to photographic plates, but with grids placed across the images to allow the switching that was so familiar to Dave's group.

Roger Dube (Ph.D., 1976) found bounds on the mean optical brightness of the extragalactic sky, an important measure of the cosmic mean mass density in stars. Dube separated foreground radiation by colors and separated stars by masks in front of a photomultiplier system. At about the same time, Michael Hauser, who had earlier worked with Dave in Princeton, became principal investigator of the COBE/DIRBE experiment, which was being set up to make the same measurement, extended to longer wavelengths, from space. This exceedingly difficult project was a success, to Dave's great pleasure. Indeed, Dave (inspired by the Partridge-Peebles papers on light from early galaxies) was one of the strongest advocates for including this instrument in the COBE. Ed Loh (Ph.D., 1977) worked with Dave on an early adaptation of the charge-coupled detector (CCD) to astronomy. These detectors now dominate optical astronomy. Loh, and later Peter Saulson (Ph.D., 1981) and Bernie Siebers (Ph.D., 1981) applied CCDs to the measurement of the diffuse starlight around edge-on spiral galaxies. This is an important indication of how the galaxies grew: infalling objects of appreciable mass would have gravitationally disturbed the disk, moving stars into the halo. The evidence is that the effect is real but modest. Stephan Meyer (Ph.D., 1979) and Marshall Bautz (Ph.D., 1980) used CCDs to measure the spectra of galaxies in distant clusters. These objects are seen as they were in the past when they were young. The use of such measurements to estimate when stellar populations formed also has grown into an active line of research. We summarize Dave's motivation for all this work in a borrowed and adapted precept: "It's amazing what you can see when you can look."

Dave took very seriously the task of communicating science to nonspecialists, from children to school teachers and judges. He particularly enjoyed teaching the Princeton undergraduate introductory physics courses; he had a flair for presenting physics demonstrations that kept the students on the edges of their seats, and as always he took pleasure in the students' reactions, which they in turn could sense and appreciate. In 1996 he received the Princeton President's Award for Distinguished Teaching. He was a member of working groups on undergraduate education at the National Academy of Sciences and the American Physical Society. His demonstrations of soap bubbles and vortex rings entranced children and adults; his work with the Harvard group on the optical SETI search for intelligent life in the Universe energized the local amateur astronomy community.

CONCLUSIONS

Four points only, to summarize an entire career:

First, Dave was fascinated with new technology that might allow better science. For example, we mentioned that modern versions of the CCD detectors Dave's group were developing and using in the 1970s have vastly advanced what can be done in optical astronomy

Second, he was willing to tackle speculative projects, as long as they had some promise of becoming meaningful. For example, Marc Davis's search for young galaxies proved to be a quarter of a century ahead of its time. But it made perfect sense to try the measurement then, and it didn't damage Marc's successful career

Third, he was an inspiring leader and teacher of students, not by detailed instruction but by example and attitude

And fourth, he was very aware of the world around us and cared deeply about it; he put his time and heart into communicating to all of us the thrill of the science of the real world.

NOTES

1. Here and in what follows we enter the date of completion of the thesis, and for brevity "a student" means "Dave's student." Where there is no date the thesis was on another project.
2. The second proposal, with Lyman Page, was submitted in August 1991. At about the same time, Ned Wright first concluded that the DMR one-year data showed there really is a CMB anisotropy; his e-mail message to the COBE science working group announcing this result is dated August 17, 1991.

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