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 National Radio Astronomical Observatory (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 *et al.* 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 CBR, these measurements provided strong support for the "Big Bang" interpretation of our earlier discovery.

4.5.2 Robert W. Wilson: Two astronomical discoveries

Bob Wilson shared the 1978 Nobel Prize in Physics with Arno Penzias for their discovery of the CMBR. Wilson is a Senior Scientist at the Harvard–Smithsonian Center for Astrophysics and Technical and Computing Leader of the Sub-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 *et al.* 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-ft 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-ft 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° 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 nonthermal 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 postdoc 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-ft 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 TWMs and make a pair for the observatory. TWMs 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, Venkataraman 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 and Scovil 1959) 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 TWM with a horn-reflector antenna. The horn-reflector was invented at Holmdel by Al Beck and Harald 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 Earth and will be a very low-noise antenna. Therefore, Art Crawford

Fig. 4.12. The 20-ft 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.

built the large (20-ft aperture) horn-reflector pictured in Figure 4.12, to be used with a TWM to receive the weak signals from Echo (Crawford, Hogg and Hunt 1961).

Figure 4.13 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 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-ft 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

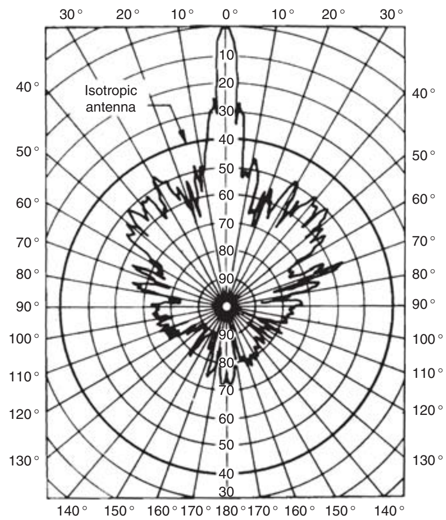

Fig. 4.13. Polar gain pattern of a horn-reflector microwave antenna. The radial units are dB and the gain is normalized to 0 dB at its peak.

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 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 1930s 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 1930s. 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 1930s 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 backside. 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 1930s. When I arrived they were building a replica from the original drawings for the NRAO 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 1940s, 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 tens 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-ft horn-reflector.

One might ask why two young astronomers wanted to work with such a small antenna as the 20-ft horn-reflector with its collecting area of only 25 m^2. 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 TWM 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-ft 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-ft 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 Cas 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 1 or 2% accuracy based on physical temperatures we could measure.

Shortly after arriving I had joined Dave Hogg to make an accurate gain measurement of the 20-ft 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 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 similar to 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 4.14 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 6-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 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

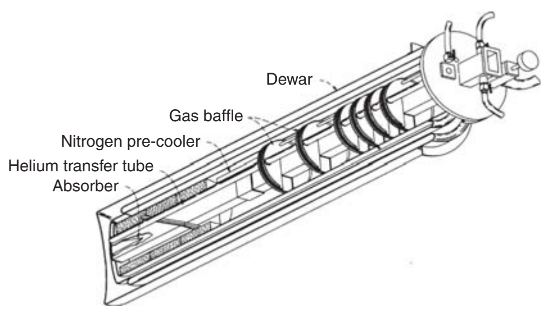

Fig. 4.14. The cold load for our radiometer.

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-l 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 $\sim 5\,\mathrm{K}$ – just eight-tenths of a kelvin above the temperature of the liquid helium. After 15 h 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 4.15 (Penzias and Wilson 1965b). As with most of our astronomical equipment

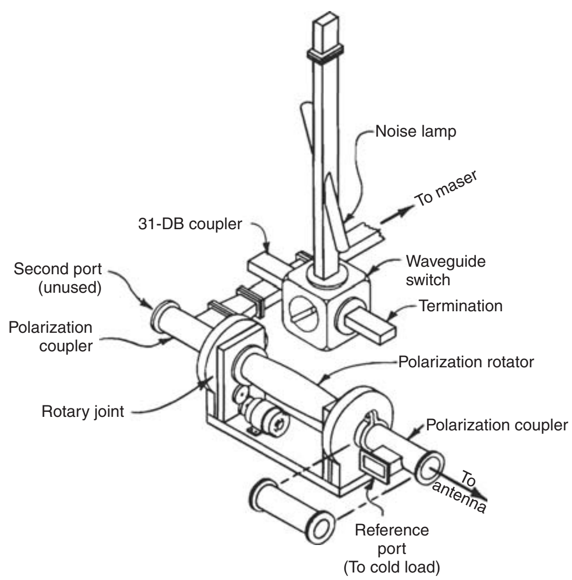

Fig. 4.15. The switch and secondary noise standard of the radiometer used for our measurements of the flux density from the radio source Cas A and the CMBR. The noise injected by the noise tube and its coupler was calibrated in three ways against thermal sources.

at Bell Laboratories, this is somewhat unusual. The 20-ft 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 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 4.16 shows a picture of the actual installation. The rotary joint that allowed the horn-reflector to turn while the receiver stayed stationary 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-ft 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

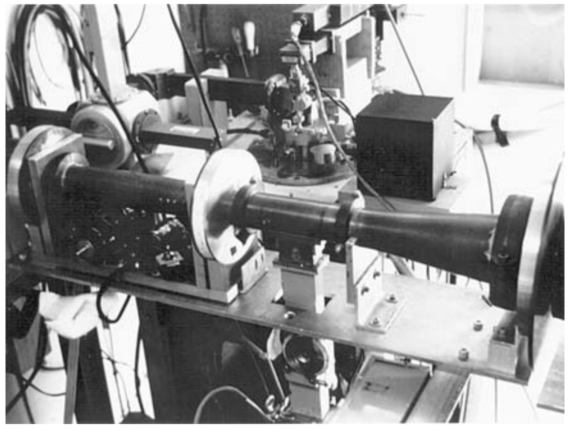

Fig. 4.16. Our radiometer installed in the cabin of the 20-ft horn-reflector.

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 been careful measurements of horn-reflectors with TWMs at Bell Laboratories. First, before going to the trouble of building a 20-ft 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 *et al.* 1959). 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 4.17 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.

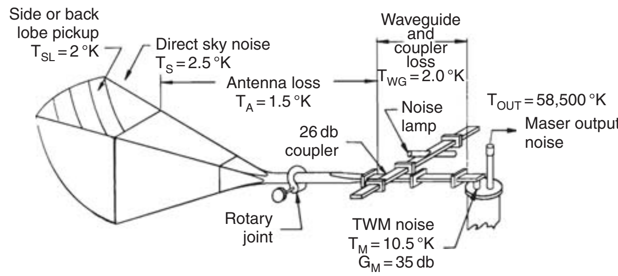

Fig. 4.17. The assignments of contributions to the system noise temperature in the De Grasse *et al.* (1959) radiometer.

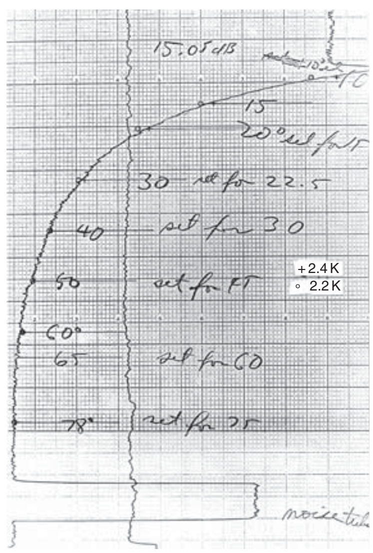

Fig. 4.18. A measurement of the radiation from the atmosphere by a tipping experiment with the 20-ft horn-reflector. The small circles and crosses are theoretical fits of the atmosphere to the measurements and show excellent agreement with the measurements.

This group had measured the atmospheric radiation (sky noise) by the same technique that Dicke had first reported on in 1946 (Dicke *et al.* 1946). Figure 4.18 shows a chart of such a measurement Arno and I made with the 20-ft 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-ft 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 4.19 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 4.20 is the first measurement 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 0.04 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.

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.

Fig. 4.19. Ed Ohm's (1961) tally of instrumental noise. Reprinted with permission of Lucent Technologies/Bell Labs.

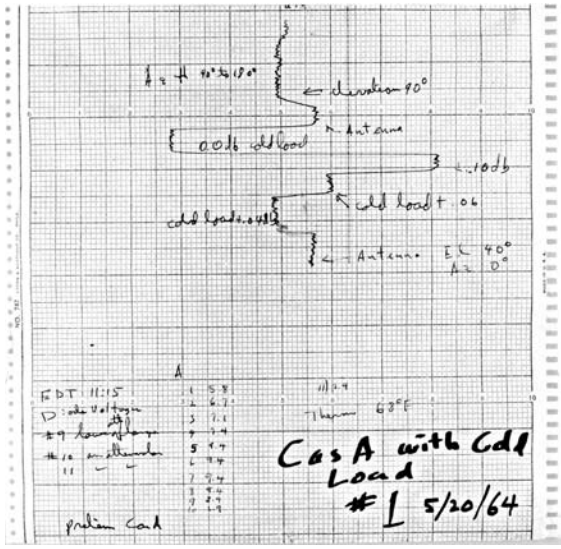

Fig. 4.20. Record of our first 7-cm observation.

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 4.18 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 1967a).

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 superthermal 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 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 electroformed copper and we measured waveguides of the same material in the lab to determine the loss 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 the 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. Afterward our two groups wrote separate letters to *The Astrophysical Journal* (Dicke *et al.* 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-ft 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-ft 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-ft 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 blackbody spectrum rather than graybody, and thus definitively from the early universe. The details of these early measurements are covered by other essays in this chapter.

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 Wilkinson 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,

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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 0.1 K 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-ft 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-μ 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 μ 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-cm 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 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 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-ft 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 ft. We returned with a higher frequency Burrus receiver. Arno talked Keith Jefferts (a Bell Labs atomic physicist interested in millimeter-wave spectroscopy) and me into integrating the Bell Labs receiver into an NRAO receiver box that would fit at the focus of the 36-ft 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 to the BN/KL region of the Orion Nebula where two nebulae which are bright, one in the near infrared and the other in the far infrared, would be in our beam. 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-ft 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-ft antenna at Kitt Peak.

I then became project director for the 7-m antenna. It was designed to do millimeter-wave astronomy when the weather was good and satellite propagation measurements at 1-cm and 2-cm wavelengths 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 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.

4.6 The Bell Laboratories–Princeton connection

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

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Let me start out with some personal background. When I was a graduate student at MIT, 1950–1953, 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 (Telecommunications Research Establishment) 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