Frontiers of Astrophysics - A Celebration of NRAO's 50^{th} Anniversary
ASP Conference Series, Vol. 999, 2008
Alan H. Bridle, James J. Condon & Gareth C. Hunt

Discovering CO and other Interstellar Molecules with the NRAO 36-Foot Antenna

Robert W. Wilson

Harvard-Smithsonian Center for Astrophysics, USA

Abstract. Bell Labs was an early developer of millimeter-wave technology. In the 60's there was a big push to develop a millimeter wave long-distance communications system to do what ultimately fiber optics has accomplished. As part of this system, Charles Burrus at Crawford Hill developed millimeter-wave receivers by making Schottky-barrier diodes using modern photolithography. Arno Penzias and I recognized that these had a potential use in radio astronomy and with Ken Kellermann proposed to build a receiver with them for use on the then-new 36 foot antenna. Unfortunately this attempt was premature and not successful. In 1970 Arno, Keith Jefferts, and I—with much help from Sandy Weinreb—put together a spectral-line receiver. This was done with the hope of detecting rotational transitions of simple molecules in interstellar space. Since, at the time, only a few people (like Phil Solomon) had any idea that molecular clouds existed, we prepared to detect a weak signal. Our backup strategy, suggested by Pat Thaddeus, was to look for CN, which had been known to exist since the late 1930s. If neither line had been detected, we would have observed the H38α recombination line which is close in frequency to the CO J=1−0 line. As we all know now, however, the signal from carbon monoxide (and even its less abundant isotopes) was remarkably strong. Such measurements have since transformed our ideas of star formation.

1. Introduction

Prior to 1970 the idea of massive molecular regions in the interstellar medium (ISM) was not in the general consciousness, even though Solomon & Wickramasinghe (1969) showed that dense clouds with a hydrogen density $n > 100$ cm^{-3} would all be molecular clouds with molecular hydrogen H_2 as the dominant constituent and very little atomic hydrogen H I. Interstellar CH and CN (McKellar 1940) and CH+ (Douglas & Herzberg 1941) had been known in diffuse clouds from their optical absorption lines since the late 1930s. In a talk in 1955, Charles Townes (Townes 1957) suggested radio astronomers should look for a number of transitions in simple molecules, including OH, NH_3, H_2O, and CO. In 1963 Sander Weinreb (Weinreb et al. 1963) detected OH as the second significant radio line. In 1968 and 1969, ammonia (Cheung et al. 1968), water (Cheung et al. 1969), and formaldehyde (Snyder et al. 1969) were found, but none of these pointed to extensive molecular clouds.

2. Millimeter-wave Work at Crawford Hill

In 1932 George Southworth of AT&T demonstrated the first propagation through a waveguide. The next year, Mead at AT&T and Schelkunoff at Bell Labs separately calculated the attenuation of various modes in waveguide—and both discovered quite unexpectedly that the circular electric modes (TE_{01} is the lowest) have losses that decrease rapidly with higher frequencies in a given circular waveguide. In 1934 Southworth moved to Bell Labs, Holmdel (where Karl Jansky worked) to continue exploring waveguides. That same year Russell Ohl used a spark source to investigate millimeter-wave detectors. The Holmdel group's charter was research on long-distance communication, and they were constantly pushing technology to higher frequencies to get more bandwidth. The idea of using the TE_{01} mode in a highly overmoded waveguide for long-distance communication was thoroughly implanted in their thinking early and, as millimeter-wave technology was developed, they worked on it with such a system in mind. By 1963, the year I joined the group in the newly completed Crawford Hill Lab, a system was under intense development using a two-inch diameter waveguide with a potential band extending from 38 to 120 GHz. By 1975 a separate development group had built enough of the system to show that coast-to-coast communication through waveguide was possible. But the capacity was not yet needed—and optical fibers were on the horizon—so the effort was dropped.

3. The People Involved

Arno Penzias I first met Arno when I interviewed at Bell Labs and attended the winter AAS meeting at the end of 1962. He had joined Bell Labs in 1961 after finishing his thesis at Columbia with Charles Townes in which he searched for hydrogen in clusters of galaxies using a MASER amplifier that he had built for the purpose. At the time I met him, he was searching for emission from the OH radical in the direction of the Galactic anticenter with a small horn reflector on the side of Crawford Hill while waiting for the 20 foot horn reflector to be available for astronomy. In 1966, after our cosmic microwwave background radiation (CMBR) discovery, Roy Tillotson (our lab director) reminded us that we had agreed to do astronomy half-time and do projects for the Bell System with the other half. We worked on company projects together and separately, but by 1968 I was spending most of my time on a Sun tracker I had devised to measure 1 and 2 cm wavelength Earth-space propagation for Tillotson's proposed domestic satellite system. However Arno had continued more astronomical work and had developed an interest in exploiting the available mm-wave technology for astronomy. We had continued work with the 20 foot horn reflector with the help of Arno's Princeton graduate students and Gerry Wrixon.

Keith Jefferts Keith Jefferts was an atomic physicist who had worked on ion trapping with Hans Dehmelt at U. Washington and then continued a very difficult measurement of the hyperfine structure of H_2^+ at Bell labs, Murray Hill. He joined with Arno, Ed Lilley, and others at the Harvard College Observatory to search for those lines in the ISM (Jefferts et al. 1970).

Charlie Burrus Charlie Burrus was a physicist who worked a few doors down the hall from Arno and me. His thesis had been on millimeter-wave spectroscopy with Gordy at Duke. In addition to his other abilities, he had an unusual talent for assembling small things under a microscope. He was working on receivers for the millimeter-wave waveguide system at the time. Photolithography was relatively crude then and micron-sized features were not yet possible. Still they were needed for millimeter-wave diodes. Charlie took masks that were made for silicon target vidicons with 8 micron holes on 20 micron centers, used an inverted microscope lens to make holes of 1 to 2 microns diameter in an SiO_2 layer on GaAs, and electroplated gold through these holes to produce gold-gallium arsenide Schottky-barrier diodes (Lee & Burrus 1968). He mounted these in Sharpless wafers (Sharpless 1963) and contacted one of the dots with a wire "whisker" to make the lowest-noise millimeter-wave receivers of the time.

Sandy Weinreb After detecting OH in the ISM, Sandy built the first digital autocorrelator for radio astronomy and made a heroic effort to detect neutral deuterium. He then joined the NRAO as head of electronics, where he made significant improvements in their receivers. Dave Heeschen accomplished many important things for the NRAO and U.S. radio astronomy—and hiring Sandy was one of them.

4. 1968 Observations with Ken Kellermann

In 1968 Charlie Burrus made an 80−90 GHz mixer using his Schottky-barrier diodes in Sharpless wafers in a WR-12 waveguide block. Arno and I took it to Kitt Peak to use on the then-new NRAO 36 foot telescope. This was a cooperative effort with Ken Kellermann with the intent to extend spectral measurements of radio sources among other things. We had been granted two months of observing time if I remember correctly. The receiver used a directional coupler to inject the local oscillator and a bending waveguide beam switch that was suited to the desire to measure small-diameter sources (Figure 1). A small absorber could be mounted so as to cover the feed horn in the off-source position and thus make it a load switch.

Unfortunately the 36 foot was not quite ready for serious observations. The pointing was not well understood. Moreover, while the data were integrated digitally, they were recorded on paper tape and there was no facility for reading or using that tape at the telescope site. I attempted to write such a program to use a large University of Arizona computer, but interpretation of the data was only done after we returned to Bell Labs. Two papers resulted from that effort, but not about source fluxes. One was an attempt to see a pulsar (Maran, Penzias, & Schraml 1968). The other was a deep integration along a line that showed that if the CMBR was actually generated by a large number of point sources, that number was really large (Penzias, Schraml, & Wilson 1969).

In the end, we did not use all of the time allotted, but left the receiver and decided to wait until the 36 foot was better developed. Charlie Burrus provided NRAO with Schottky-barrier diodes for a number of years afterward.

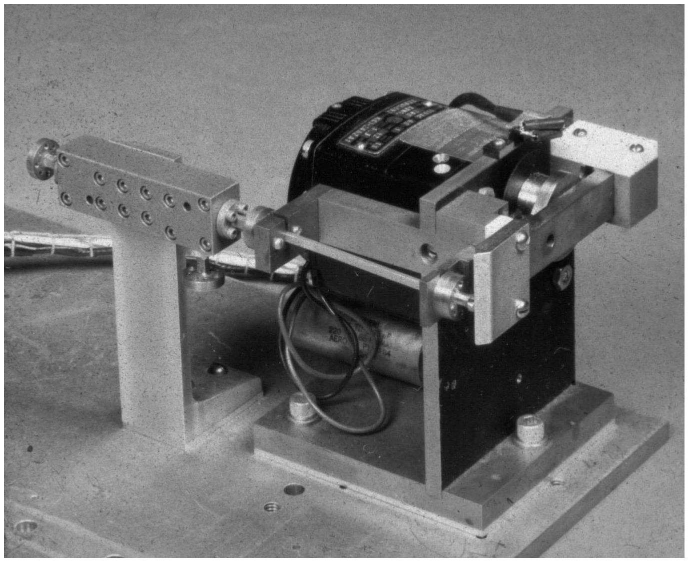

Figure 1. The 80−90 GHz receiver we used in 1968. The directional coupler for injecting the local oscillator is on the left and the bendable waveguide extends across the middle. To the immediate right of the horn, a metal plate holds a microwave absorber, thereby converting the beam switch into a load switch.

5. The Discovery Observations

In 1970, after working with Keith Jefferts on other projects, Arno invited him to join us in an effort to do spectroscopy with the 36 foot antenna. Keith could not only interpret results and predict frequencies, he also had the budget to buy the expensive, short-lived klystrons we would need.

Arno had been a student of Charles Townes and was quite aware of his list of likely molecules in the ISM. We consulted Phil Solomon (who also suggested CO) and Pat Thaddeus, who pointed out that CN has lines near CO. After struggling with other types of lines, the idea of observing rotational levels of simple molecules made of C, N, and O was quite appealing as it would allow a much more systematic approach to molecules. We were aware that the hydrogen recombination line H38α would be very close to the CO 1−0 line that was our primary target. Since formaldehyde had been seen in the ISM, I thought that of the molecules we were considering, CO was quite likely as it would be a breakdown product of H_2CO.

Buhl and Snyder were also proposing to look for rotational transitions of molecules in the ISM, and Sandy Weinreb made plans for spectroscopy with the 36 foot. He designed and built a phase-lock system to control the frequency of millimeter-wave klystrons with a provision for frequency switching. This was complicated by the cathode and heater of the klystrons running at a potential of −2500 volts and the reflector at up to another 1000 volts more negative. Much of the rest of the equipment Sandy assembled was similar to that used for 21 cm observations. He provided a Micromega paramp at 1420 MHz center frequency for the IF preamp to minimize the noise from the front end. The spectrometer was a filter bank consisting of fifty 2 MHz wide filters with 1 MHz spacing, each with its own detector, lock-in amplifier, and integrator. The lock-ins required some form of front-end switching. The data recording system had a multiplexer that would either provide a real-time display of the integration building up by cycling through the channels and sending the sequence of voltages to an ordinary oscilloscope or digitize the channels in sequence and record the results on magnetic tape. Once a new integration was started, the previous results were not to be seen again until the tape was taken to a big computer. Figure 2 shows views of the 36 foot control room.

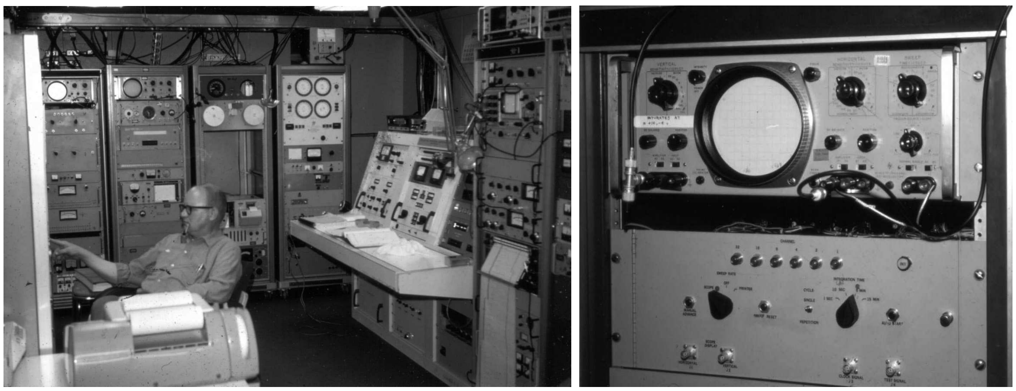

Figure 2. On the left is a picture of the 36 foot antenna's control room with George Grove poking at the DDP-116 drive system computer. On the right is a close-up of the multiplexer control panel and the oscilloscope.

The mixer Charlie made for us was in RG-138 waveguide suitable for 90 to 140 GHz operation. The mixer block and wafer are shown on the left of Figure 3 and the assembled receiver plate that we used for several years afterward on the right. The klystron had to be frequency-controlled, so there was a second diode to act as a harmonic mixer for Sandy's phase-lock loop. This receiver used a chopper wheel instead of the beam switch for calibration and a resonant cavity for injecting and filtering noise in the sidebands from the local oscillator. I remember that the actual receiver used in the CO detection still had the beam switch and it also had an avalanche diode (supplied by Charlie Burrus) that could be switched at 10 Hz to inject noise as a secondary calibrator. Spectral observing was done in frequency-switch mode and pointing checks used the beam switch. The evolution to the receiver shown in Figure 3 probably occurred within the first year. We realized that absorbing-chopper-wheel calibration would to a first approximation compensate for atmospheric opacity—and be much more reliable than the beam switch.

Sandy shipped the front-end box (without a receiver) to Crawford Hill along with its rack of power supplies and control equipment about 2 weeks before our scheduled observing run. Keith and I started spending long days wiring our receiver, beam switch, etc. to work in the NRAO box and testing things. At one point in this process Charlie Burrus remarked that Arno had the two best technicians at Bell Labs working for him. In any case, we did not quite finish

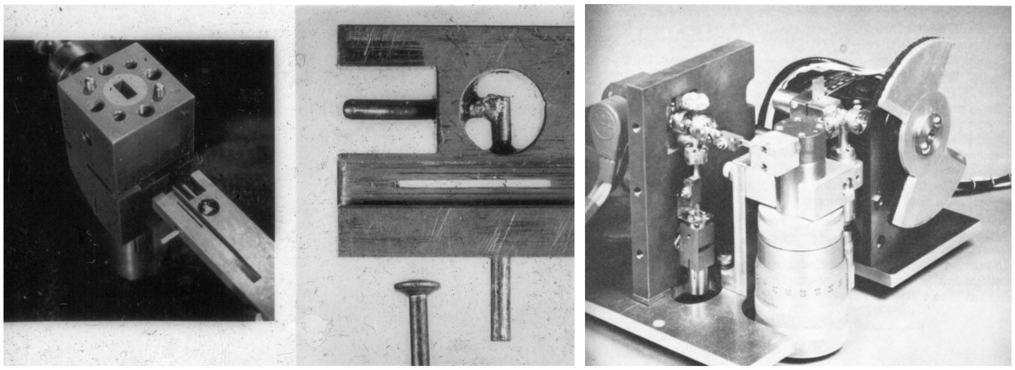

Figure 3. The 2 mm receiver we used at the 36 foot antenna for several years. The waveguide block that held the Sharpless wafer and a wafer are shown on the left. A magnification of the working part of the wafer is shown in the center. The white horizontal slit in the middle is the reduced-height waveguide, and the whisker that contacted the diode can be seen crossing the waveguide. The array of diodes is at the top of the whisker in this view. The wafer is thinned in the region of the waveguide. The complete receiver is on the right, with the klystron at the far left of that picture and the chopper wheel at the right The prominent large micrometer in the center drives a wavemeter to help in tuning the klystron to the correct frequency. (Reprinted, with permission, from the Annual Review of Astronomy and Astrophysics, Volume 11 (c)1973 by Annual Reviews `www.annualreviews.org`)

before the time we had to ship it all to Tucson for observing. Sandy, Keith, and I went to the 36 foot to get the whole assembly working early one week.

Each piece of the front end had its own way of failing and each frequently did so. The Sharpless wafers had the advantage that we could have a stash of contacted diodes ready to go (provided by Charlie), but they were not quite as sturdy as was needed. The diodes were also very sensitive to static discharge. The result was that the diodes frequently lost contact or shorted.

The klystrons cost more than $5,000 each and were only guaranteed for 500 hours. They frequently did not last that long. They would become noisy or have tuning characteristics at some frequencies that defied the PLL to lock them. They would sometimes spark and then stop oscillating, giving us a sinking feeling. If shut down promptly and slowly brought back up to their operating voltage, they would often work again. The sparks would sometimes blow transistors in Sandy's lock box. It was often difficult to find gain and bandwidth values for the lock box that would allow it to hold lock securely. The heater supply for the klystron was a switching supply, and its switching transients sometimes seemed to make the lock unstable.

Less-serious problems included the beam switch whose waveguide would crack from metal fatigue and the DDP 116 control computer of the 36 foot that would crash at inopportune times. The antenna's brakes were a problem, and the antenna itself was known to lose efficiency rapidly if the sun shone on its surface for long.

After three days of attempting to get the whole receiver to work, Sandy had to return to Charlottesville, and he since has told me that he left very discouraged. Miraculously Keith and I got everything working at the same time in the control room later that day—and it continued to work even after we carried the receiver box up and put it at the focus of the antenna with much longer cables connecting it to its control rack. I looked at our list of possible sources and the Orion BN-KL region was the most likely one that was up, so I asked the telescope operator to point there. Meanwhile I went over the back end and set the integrators to the mode where they acted as two-second time constants instead of integrating. The multiplexer was displaying the 50 channels on the scope, but since each channel had its peculiar offset and gain, the display was a very noisy constellation of points near the horizontal centerline. As I was waiting, I suddenly noticed that some of the points near the center of the display had risen higher. I asked the operator if the antenna had reached the source, and he looked and confirmed that it had. I then asked him to move off the source and saw that the points went back down. We had to check our frequency calculations including our use of DOPSET velocities again, but in fact we had discovered CO in a few seconds once everything worked.

After seeing that the Orion source was extended and exploring until it set, I remember being tired and saying that we were prepared for everything but success, and we put off further measurements until the next morning. In retrospect it is remarkable that we first looked for CO in what we now know is the brightest CO source in the sky and that having made the discovery we slept through what we subsequently called dead time—when the Milky Way is not visible—to awaken and then find CO in other Galactic HII regions.

Sandy called on Friday to check in and was surprised at our success. Arno arrived later that day, and we announced the discovery which then appeared in the Tucson papers on Saturday. Soon afterward we started hearing from astronomers, the most memorable being Bart Bok who told us to look at his globules. During that observing run, I measured the baseline of the spectrum shown in our first paper (Wilson, Jefferts, & Penzias 1970) by observing at two different local-oscillator settings on either side of the center frequency and patched them together back at Crawford Hill (Figure 4). We also traced CO over a region almost a degree wide. My one regret is that I did not recognize that the line wings in the original spectrum were not an extension of the main line but indicated higher-velocity gas. A high-velocity bipolar outflow at that position was recognized years later (Zuckerman, Kuiper, & Rodriguez Kuiper 1976). Arno and I, of all people, should have looked carefully at, and believed, what our receiver was telling us.

In the observing session after ours Buhl & Snyder (1970) discovered HCO^+ (called X-ogen at the time since they were unable to identify it) while looking for HCN and $H^{13}CN$. Millimeter-wave astronomical spectroscopy was off to a roaring start.

6. Follow-on Work

During our two Spring 1970 observing sessions, we found CO in a number of "HII" regions and made strip maps in some sources. We became increasingly

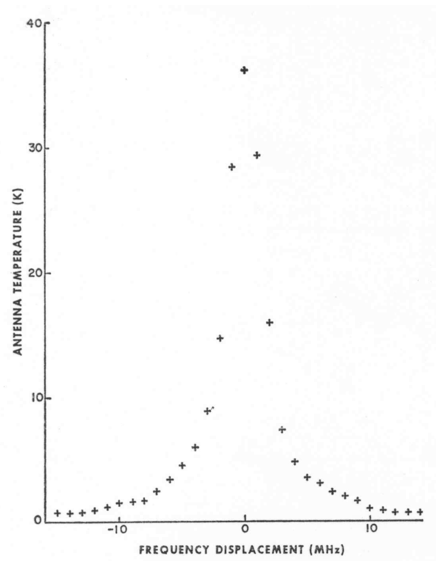

Figure 4. The spectrum of CO in the Orion Nebula from our first publication. (Reproduced by permission of the AAS.)

annoyed by the inability to analyze our data while exploring at the telescope. Keith had ordered a Nova minicomputer for his lab at Murray Hill, and we decided to construct a data recording and analyzing system with it for our Fall runs. I had programmed a PDP-8 computer to record propagation data at Crawford Hill, so I agreed to write the program in assembly language, and Keith made the hardware to interface with the multiplexer at the 36 foot. The Nova did not have a disk, so most of its 8K of memory was used to store the last 100 spectra. The program was able to average spectra, normalize them with calibration spectra, and fit and remove baselines. In our first observing session of the Fall we used the new computer system and started measuring the less-abundant isomers ^{13}CO and $C^{18}O$. A year later the NRAO had a much nicer system programmed by Chuck Moore in FORTH, a language he had invented at least partly for that purpose, so we quit using the Nova at the 36 foot, but the program lived on at the McDonald Observatory for a while. In the first two years after the discovery of CO, we found CN, SiO, CS, CH_3CN, OCS, and H_2S and many isotopic substitutions. Buhl and Snyder had found HCO^+, HNC, and HNCO.

7. Comments

The NRAO and Sandy Weinreb played a critical role in opening up millimeter-wave astronomical spectroscopy. That led to the discovery of molecular clouds and the star-forming regions within them. Since the rotational transitions of many simple molecules are at millimeter wavelengths, much more systematic observations could be made than at longer wavelengths.

When I look back at Phil Solomon's 1973 paper in Physics Today (Solomon 1973), it is clear that we and the growing community of millimeter-wave observers had gotten a lot right in two and a half years. By comparing the lines of molecules with different dipole moments and molecules with more- and less-abundant isotopes, it was understood that the excitation of the molecules was by collisions with hydrogen molecules. ¿From that cloud densities and therefore cloud masses could be determined. Molecular clouds were known to be centers of star formation, more massive than the associated HII regions, and an important component of the Galaxy's mass. Dark clouds were seen to contain CO extended over degrees. Mapping of them was beginning and was showing intense emission in spots. Isotope ratios were seen to be near terrestrial except for deuterium, for which chemical fractionation was a proposed explanation. The formation of diatomic molecules in the gas phase was beginning to be understood.

The 1970's were an amazing time and I had a lot of fun. We had many joint projects with Pat Thaddeus, Phil Solomon, and their students. We worked hard and were constantly alert to equipment failures (I still jump when I hear a Sonalert). The Bell Labs group had the only receiver above 100 GHz for several years, so we were pretty much free to observe what we wanted to and that contributed to rapid advances. We had a lot of instant gratification with new sources, new molecules, or new insights from every observing run. After a while I found that 24 hours of searching for something new and not finding it became depressing.

I consider myself very lucky to have discovered interstellar CO and participated in the science that followed, but if we had not done it, someone else would have before long—however, Bell Labs' technology certainly accelerated the process.

References

Buhl, D., & Snyder, L. E. 1970, Nat, 228, 267
Cheung, A.C., Rank, D.M., Townes, C.H., Thornton, D.D., & Welch, W.J. 1968, Phys.Rev.Lett, 25, 1701
Cheung, A.C., Rank, D.M., Townes, C.H., Thornton, D.D., & Welch, W.J. 1969, Nat, 221, 626
Douglas, A.E., & Herzberg, G. 1941, ApJ, 94, 381
Jefferts, K.B., Penzias, A.A., Ball, J.A., Dickenson, D.F., & Lilley, A.E. 1970, ApJ, 159, L15
Lee, T.P., & Burrus, C.A. 1968, IEEE Trans. Microwave Theory and Techniques, MTT-16 1968
Maran, S.P., Penzias, A.A., & Schraml, J. 1968, Ap&SS, 2, 334
McKellar, A. 1940, PASP, 52, 187
Penzias, A.A., Schraml, J., & Wilson, R.W. 1969, ApJ, 157 L49
Sharpless, W.M. 1963, Bell System Technical Journal 42, 2496
Snyder, L.E., Buhl, D., Zuckerman, B., & Palmer, P. 1969, Phys.Rev.Lett, 22, 679
Solomon, P. M. 1973, Physics Today, 26, 32
Solomon, P.M., & Wickramasinghe, N.C. 1969, ApJ, 158, 449
Townes, C.H. 1957, in *Radio Astronomy*, IAU Symposium No. 4, ed. H.C.van de Hulst, (Dordrecht: Reidel), p. 92
Weinreb, S., Barrett, A.H., Meeks, M.L., & Henry, J.C. 1963, Nat, 200, 829
Wilson, R.W., Jefferts, K.B., & Penzias, A.A. 1970, ApJ, 161, L43
Zuckerman, B. Kuiper, T.B.H., & Rodriguez Kuiper, E.N. 1976, ApJ, 209, L137