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The Start of 21-cm Line Research: The Early Dutch Years

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Abstract. This is the text of an after-dinner presentation given at the conference banquet. An anecdotal review is given of the first five years of 21-cm line activities in The Netherlands, highlighting personal experiences.

I am supposed to entertain you for a while after this great banquet. But have we not had entertainment enough with the marvelous series of papers and discussions during this Symposium, and with the tour of the fabulous Dominion Radio Astrophysical Observatory? Does not the fact that the whole interstellar medium seems to be made up of little and big rings provide enough entertainment? But no, the organizers want you to have even more; so here I am keeping you from discussing the night away for another while.

I first want to express my pleasure at seeing so many young faces here. The 21-cm line research appears to be young and vigorous and continues to have a tremendous future.

Now I want to talk a little about the very early years of the Dutch 21-cm line observations and reductions, and the delightfully primitive ways in which we produced maps of our Milky Way Galaxy. We all know the story of the 1944 Leiden Colloquium in which Henk van de Hulst (van de Hulst, 1945) suggested that the neutral Hydrogen 21-cm line might be observable.

After the war, when science was getting back on its feet, Jan Oort determined that we should gear up for the detection and study of this line, in his classical determination to try to solve the riddles of our Galactic System. Yes, Oort called our Galaxy the Galactic System, i.e. the system that produces the Milky Way, and it took very many years before he gave up that name for the name Galaxy, with a capital G for ours, and a small g for other systems. Oort got in touch with the head of the Dutch Post, Telephone and Telegraph services (the PTT) which was also in charge of radio communications, and convinced him to salvage some of the German Giant Wurzburg 25-foot radar dishes, scattered along the coast. One of those was transported to the Kootwijk radio transmitting station in the middle of the country. It was there in 1948 that the Leiden Observatory started the construction of a 21-cm line receiver in a small hut adjacent to the Wurzburg, and also adjacent to a 100-foot hole-in-the-ground antenna built by the PTT for solar observations. The work progressed slowly, far too slowly to Oort's liking, and unfortunately, in March 1950, the hut and all that was in it burned up in a fire.

Undaunted, Oort immediately got Philips electronics labs to build a new receiver, and found one of the most capable young engineers just out of the Technical University of Delft, Alex Muller, in Fall 1950. Muller took the Philips receiver apart, thoroughly improved it, and moved his lab into the cabin under the dish on top of the Wurzburg azimuth drive. The observing room rotated with the telescope. Basically starting from scratch, he had a 21-cm line receiver working in 9 months. Unfortunately, shortly before he was finished, Ewen and Purcell detected the line. Muller made a few observations in the summer of 1951, indicating the existence of distinct spiral arms. At that time, Oort concluded from the fact that the line profiles were symmetrical around zero in the center and anti-center directions that the orbits of the stars in general (the local standard of rest) must be pretty well circular. Muller then spent a year further developing the equipment. In July 1952 he started observations along the Galactic plane as visible from Holland, which continued, with many interruptions for improvements, until June 1953. This series of observations led to the famous B.A.N. 452 (van de Hulst et al., 1954), which for the first time showed the spiral arms outside the solar circle most of the way around the Galaxy. More about that later.

What was observing in Kootwijk like? In the first place, as already mentioned, the observations were made in the shadow of the Dutch radio *transmitting* station, a forest of antennas. The biggest ones were the long-wave (1500 m) broadcast antennas. When you stood by one of the guy wires of the 200-foot high posts and held a screwdriver close, you could hear, in the spark it drew, the voice and music of Radio Hilversum. Some people thought that due to the very high interference levels, Muller had to build enormous shielding in all the parts of his receiver, which in turn served as a guard against spurious signals at intermediate frequencies and thus made the equipment work. This was definitely not true; he was simply a topnotch engineer. But when the Germans built their radio telescope on the Stockert mountain near Bonn, Siemens built them a 21-cm line receiver in which the components were each individually encased in copper boxes, with copper water pipes connecting them to carry the cables. It took a big truck to get it up there!

The Wurzburg was hand cranked. Yes, it did have fast motors to quickly change elevation and azimuth, but fine setting was by two little cranks, which protruded through the wall (for elevation) and the floor (for azimuth) of the observing cabin. The elevation and azimuth coordinates could be interpolated to an accuracy of 0.1 degrees on divided circles directly attached to the axes, which had divisions every 0.5 degrees visible through small windows. Now how do you follow a point in the sky? Integration times were long, as the noise figure was rather high (10, about 2500 degrees!!). To get an entire line profile took two hours. The Leiden Observatory, having a staff of "computers" (these were PEOPLE who used hand-cranked and electrical calculators), set out to calculate handwritten tables of azimuth and elevation as a function of hour angle, for every few degrees in Galactic longitude and latitude, and for every half hour of time. The observers then made observing sheets giving the coordinates for every 5 minutes of sidereal time, with telescope corrections applied. These were again handwritten on thick legal size paper, with $^1/_2$ cm squares, hundreds of them, neatly sorted in folders. This work was ongoing for the first few years, tables being made on request whenever the observational planning required new longitudes and latitudes. The observer then interpolated the azimuth and elevation for every $2\,^1/_2$ minutes, and turned the two cranks every $2\,^1/_2$ minutes. Since the beam width was 1.9×2.7 degrees, a setting every $2\,^1/_2$ minutes corresponded to $^1/_4$ of the beam at the equator. We worked mostly at night, because traffic noise increased during the day and occasionally caused problems. But at 5 p.m. the PTT radio station staff went home, so we observed from 5 p.m. to 7 am.

Those were the days! We took the train from Leiden to Apeldoorn, bringing our bicycles with us, and then cycled 40 minutes from the station to Hotel Kootwijk Radio, on the grounds of the radio station. The hotel owner was always very accommodating, preparing midnight lunches, and allowing us access to the hotel refrigerator for cold drinks or milk. I was once heavily chastised for having poured milk into a beer glass! That was a mortal sin, and the beer glass was summarily destroyed. We also cycled from the hotel to the telescope. Lex Muller and his two technicians cycled every day from their homes, some 30 minutes or more away (rain or shine!), and sometimes had to return in the middle of the night.

I had no experience building or maintaining radio equipment - my only claim to fame was a 1950 photoelectric amplifier built under Walraven's direction, which was insensitive to fluctuations in the AC power from the wall outlet. Therefore, when something went wrong, the technician - or Muller himself - had to be called out of bed and had to cycle the distance in the dark of night. I remember one angry technician whom I summoned because the frequency markers stopped being recorded on the chart. After some grumbling about "those incompetent students" he opened the appropriate rack, touched some connections with a screwdriver, drew a spark while doing that, slammed the rack shut and cycled home: "It's fixed." It was. A week later the same thing happened. I decided I was not that incompetent, opened the rack, touched some connectors with a screwdriver, drew a spark and slammed the rack shut. It was fixed. I told the technician and Muller proudly the next day how I had fixed the equipment without having to call them out of bed. Did I ever get a tongue-lashing: "Keep your dirty hands out of the equipment or we send you straight back to Leiden".

That series of observations made between July 1952 and June 1953 were all sent to Henk van de Hulst in Leiden for reduction. Initially, he did all the work alone, then with two graduate students, skimming the top off the data to come up with immediate results. This allowed Oort to present the first rudimentary spiral map of the Galaxy two months after the start of the observations at the IAU meeting in Rome, September 1952. Those reductions were not easy, as everything, including calculations and plotting, had to be done by hand on large sheets of paper. The line profiles were provided with occasional tick-marks indicating the frequency setting (read from a dial attached to the shaft of the tuning condenser) and put on by hand by the observer pressing a button. The baseline was curved. The oscillator was swept at a rate of 1 MHz per hour and the profile appeared twice, once straight up and once upside down. The switching frequencies were not far enough apart (100 to 700 kHz, later stabilized at 648 kHz = 137 km/s), so the end of the wider lines ran into the start of the inverted part of the line. The frequency markers had to be interpolated and corrected for nonlinearity in the frequency scale. They were then converted into a grid of radial velocities with respect to the local standard of rest, requiring rather complex calculations to prepare tables, with an electric or hand-cranked calculator, and "looking up" trigonometric functions. Then the intensities had to be read off at these radial velocity points (every 5 km/s, the bandwidth being 8.5 km/s), corrections for the effect of the baseline and the profile overlaps made, and the two profiles per tracing (one right-side-up, the other upside down) added.

After the entire reduction was done meticulously for a number of key profiles, some time in late 1953 the entire student body was herded into the Leiden lecture room and the eight of us plus some staff members reduced all the charts. These data represented over 1500 hours of hand-observing. The week-long reduction binge was real drudgery as most students were more or less unaware what enormously important work they were doing. Henk van de Hulst acted as the slave driver to keep up the pace.

By November 1953 the receiver was perfected: a straight baseline, automatic frequency markers, and the switching frequencies far enough apart, avoiding overlap of profiles. From then until June 1955, Muller and I embarked on a major observing program covering the entire Milky Way as visible from The Netherlands, at roughly $2\,{}^1/_2$ degree intervals over about a 20-degree wide strip. Many undergraduate and graduate students from Leiden participated in the Observations, and were sent to Kootwijk for two-week stints cranking the hand cranks and labeling the records.

Very early in that period, another graduate student, Kwee Kiem King, and I measured the terminal velocities of the line in the inner parts of the Galaxy and produced the first Galactic rotation curve. This allowed Oort to make a new mass model of the Galaxy, use it to predict the rotation velocities outside the solar circle, and use these velocities in the final reductions of that first survey. We found wiggles in the curve, which we attributed to the presence of two spiral arms inside the solar circle. We were right, but the wiggles were later found to be mostly due to the gas motion caused by the spiral density waves. This was published in BAN 458 (Kwee et al., 1954).

Talking about rotation curves, the big bump in the rotation curve close to the center mentioned earlier in this Symposium by Lockman, was added by Oort later. He argued that the mass-to-light ratio in the center of the Andromeda Nebula might be the same as in our Galaxy, indicating a large mass concentration in the Center, and therefore the rotation curve should go way up there.

By spring 1955 I had the majority of the data reduced. This still required hand reading of the charts and making long tables of intensities as a function of radial velocity, for which I acquired two undergraduate girls who spent a year with Maarten Schmidt and myself in the Leiden Observatory archives, where large tables allowed the spreading out of charts. It must be said, we had some fun with those girls and often frustrated them no end, but they were hard workers. I also had the services of some of the "computers". One was 63 years old and had worked at the Observatory since he was 12. His task, for three months, was to calculate second differences of the 3-digit tables of intensities. He felt he should do that not using any mechanical means, but he occasionally had to write some intermediate numbers down. For that, he brought scraps of brown wrapping paper from home, as using Observatory paper for something he thought he should be able to do without paper was a sin. Finally, I had him meticulously draw line profiles, explaining what it was he was doing, and expecting this to be a welcome variant to the dull work of calculating second differences. After a week he asked me, could he please go back to second differences, that was fun work; those "graphs" were very tedious!!

These second differences were part of a deconvolution for random cloud velocities, which I estimated having a dispersion of 6 km/s. The profiles were corrected for bandwidth and beamwidth and then converted to optical depth using a spin temperature of 125 K. There was a long discussion about the possible pitfalls of this choice, given the presence of both cold and warm H I (this was in 1955!). I used 125 K as a reasonable harmonic mean over the line of sight in all parts of the Galaxy.

This conversion to optical depth and especially the drastic correction for random cloud velocities really sharpened up the line profiles, and provided the considerable details in the final map. The difference is clear when you compare the Northern and Southern spiral structure in the Oort, Kerr and Westerhout review in Monthly Notices (Oort et al., 1958). There is much less detail in the Southern part. Kerr refused to correct for random cloud velocities, calling that "arbitrary". Of course it WAS a very rough treatment, but it certainly helped highlighting all the minute details in the line profiles.

At that point I could draw cross-sections of the Galaxy, with the Sun as center, at intervals of 5 degrees in longitude and up to 10 degrees in latitude. How to produce a 3-dimensional model of the spiral structure? I drew the cross-sections on Plexiglas, and set these Plexiglas panels on a plywood board, about 1 by $1\,^1/_2$ meters. It had to be transparent, so the densities were provided by painting between the contours with transparent paint. How to get transparent paint, and paint that would stick on Plexiglas? Near Leiden was a well-known paint factory, Sikkens, so I cycled over there and discussed the problem with the head honcho. He brought me to the lab where an engineer mixed, in small medicine bottles, all sorts of colors and transparencies. I finally chose the colors I needed. They then wanted to know when would the order for production come in and how much paint did we need. I said: "these 6 little bottles are all I need, thank you very much," put them in my pocket, got on my bicycle and went back to the Observatory. Later, Oort wrote them a nice thank-you letter.

Around that time it became clear how unbelievably flat the Galactic plane was in the inner parts of the Galaxy. Between 4 and 9 Kpc it deviated not more than 25 pc from a mean plane. This, with confirming data from the Southern hemisphere, plus the fact that it was clear we would soon know the "exact" position of the Galactic center, caused the IAU in 1955 to establish a subcommittee to recommend a new Galactic coordinate system. Combining Northern and Southern data, Gum, Kerr and Westerhout (Gum et al. 1960) presented new values for the Galactic pole and center, and proposed a new Galactic coordinate system which was adopted by the IAU in 1958 and put into use in 1961.

At the 1955 Jodrell Bank Symposium (IAU Symposium 4, 1957) followed by the IAU meeting in Dublin we first presented the 3-dimensional model described earlier and all the other results, including Maarten Schmidt's separation of the spiral arms in the inner part of the Galaxy. I had a very large plywood box made for that model, with straps, which I took by plane to Jodrell Bank and to Dublin. It made quite a stir, and impressed one of the young ladies on the

Institute For Advanced Studies Organizing Committee so much that she decided to marry me. Judith is still around and sends her greetings.

All this material was published in BAN 475 (Muller and Westerhout, 1957, Westerhout, 1957, Schmidt, 1957). What a fabulous time: "This *is* the spiral structure of our Galaxy." One had no doubt and there were no young whipper-snappers like Jay Lockman around who said, "We know nothing!" (Comment earlier in this Symposium). This brings me to the question: Why has there never again been a large-scale attempt to disentangle the spiral structure of our Galaxy? We again have H I data all over the sky, and we have data on dust, H II regions, molecular clouds, lots of background sources, hot background gas, and an insight into some of the fine structure of the medium, by no small means due to the great work of the DRAO. I asked that question of a number of people. Several times I got the answer that the problem is too hard; there are too many variables to be sure of getting any believable results. And then on top of it all we heard today from Ron Allen (this Symposium) that he does not think the Hydrogen is original: It was formed in the arms. So, when you are finished with all your current "easy" problems, could someone please tackle the really hard one, and resolve once and for all the large-scale Galactic structure problem?

I should stop here, but some people asked for a few anecdotes about the early Dwingeloo times (what I described above dealt with Kootwijk and the Giant Wurzburg). Well then, here are a few. In November 1955, when the 85-foot dish was still under construction, Charles Seeger and I wanted to observe two occultations of the Crab Nebula by the Moon. The dish was up, but only movable with the fast Azimuth motor. The contractor agreed to stop work for a day or two, let us mount a 400-MHz feed at the top of the focus mast, and clamped the dish at an elevation of 7 degrees. By slowly lowering that central mast by cranking the top guy wire, we could follow the Crab in elevation for a few degrees. I calibrated the guy wire with paint marks using the Crab Nebula and Cygnus A the previous day. I mounted an alarm clock on a little axle sticking out of the Azimuth Drive box (what the little axle was for I do not know) and calibrated the Azimuth. Thus, by hand-cranking the 85-foot dish we were able to observe both occultations and conclude that the radio Crab Nebula is the same size as the optical (Seeger and Westerhout, 1957). Would that we had had a pulsar receiver!!

The second anecdote plays in April 1956 when the Queen came to put the telescope into operation. The coordinate converter (the "pilot") was not yet working properly so we arranged that when, positioned outside, she pressed a red button, a light inside the control room would be the signal for one of the technicians to start moving the telescope to the prearranged position. He goofed. The telescope went the wrong way and I have never run so fast in my life, up the steps and into the control room, to change directions! Then the Queen wanted to climb the steep stairs to the apex platform, not to see the telescope but to see the hundreds of sheep - real sheep - assembled on the heather in front. "Your Majesty, that is very difficult", said Oort, but Her Majesty insisted. Unfortunately, a new coat of aluminum paint had been put on the banisters the previous day, so you can imagine the Queen's face when she came down and looked at her white gloves.

And then finally the story of the high-velocity clouds. As we heard at this meeting, Oort ruled them out as clouds in 1954, but he still felt there should be material falling in. So in 1960 Ernst Raimond and I were instructed to measure at random high-latitude positions between 50 and 250 km/s. We saw only noise, but Oort, when he sat down with the initial results saw "a peak here.... and another one there..."

Our comment: "But Professor, those are baseline wiggles, not real data".

"Go and measure again in those locations".

Different baseline wiggles, so we were allowed to give up. But: it became the first program with the new low-noise paramp receiver in 1963 and of course the high-velocity clouds were "discovered" - or should I say "detected" as Oort fully expected them.

I have talked long enough. This is what happens to astronomers who get enthusiastic and have the tremendous advantage of being somewhere just at the right time, at the forefront of new and exciting discoveries. They get married, after a while move to the USA, and finally end up as the after-dinner speaker in Penticton!

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Part 2. New Observational Advances and Future Prospects