From Clark Lake to the Long Wavelength Array: Bill Erickson's Radio Science
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Lessons Learned from the Clark Lake Experience

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Abstract.
It is often said that one learns by one's mistakes. Based upon this criterion, I can claim to have extensive knowledge of low frequency radio astronomy. In this paper I'll describe my experiences in this field in the hope that this may help people avoid a few pitfalls in the design and construction of the LWA. I'll also expand upon the title to include some early experiences before I began work at Clark Lake.

1. Early Experience at the Carnegie Institution of Washington, Department of Terrestrial Magnetism (DTM)

I received my PhD in 1956 at the University of Minnesota under the theoretical physicist, Charles L. Critchfield. About a year before receiving my degree I had occasion to visit Washington, DC and, because I had decided that I wanted to do observational radio astronomy as a career, I visited several observational groups in that area, including NRL and DTM. While I was at the DTM, Bernie Burke and Ken Franklin told me about a strange, sporadic radio source that they had been observing for about two months with the 2° beamwidth, 22 MHz Mills Cross that they had recently completed. The source was certainly sidereal; it crossed the beam about 4 minutes earlier each night, but it was not perfectly synchronized with sidereal time. They were very excited because they had just realized that the reception of these signals fit beautifully with the times when Jupiter passed through the beam of the array. They had set the declination of the array for the Crab Nebula and, serendipitously, Jupiter was at nearly the same declination. This low frequency emission from Jupiter was one of the most spectacular early discoveries in radio astronomy. It gave me the mistaken impression that low frequency work was easy. All that you needed to do was to erect some poles, string some dipoles between them, and great discoveries would result.

About a year later I accepted a Carnegie Fellowship at DTM. By the time that I got there it had been recognized that the Cross had such high sidelobe levels that it could not be used for its primary goal, a deep survey of radio sources. It was being dismantled and my only contribution to the program was to help to pull it down. Most of my time at DTM was spent on a 21 cm survey of neutral hydrogen at high Galactic latitudes.

2. The Convair Scientific Research Laboratory (CSRL)

My thesis advisor, Charlie Critchfield, had gone to San Diego where he accepted a position as Vice President and Director of Scientific Research for Convair, an aircraft and missile manufacturer. He formed the CSRL, a small, company-sponsored scientific research laboratory with a staff of about two dozen PhD scientists plus support personnel. In late 1957 I joined the staff. We were given complete freedom to choose our research projects within the confines of a very limited budget. The staff were expected to act as in-house consultants for the engineering divisions of the company and to carry out basic research to increase the company's competence and reputation in science. There were one or two staff members in most areas of science of interest to the company; theoretical physics, cosmic ray physics, radio astronomy, solid state physics, metallurgy, aeronomy, aerodynamics, chemistry, mathematics, and the like. I was the only radio astronomer. All of us were young, enthusiastic PhDs. I have never worked with a group that had as much esprit de corps. In considering what research program I could undertake, essentially on my own and very cheaply, I thought about the Jupiter discovery at DTM and decided upon low frequency work. So I made plans for the construction of a large decametric array.

My first task was a search for a large flat site in the San Diego area and I found Clark's Dry Lake, a playa about 100 km northeast of the city. It is in the Anza-Borrego desert, a desert formed by the rain shadow of the coastal mountain range. Figure 1 is a 1958 photograph of my first visit to Clark Lake; it is a completely flat area 3 km across. In fact, soil has slumped in around the edges of the playa enough to almost perfectly compensate for Earth curvature across the area, making it nearly a perfect plane. It is located in a valley about 15 km by 35 km in size and it is surrounded by 1800 m (6000 ft) mountains that partly shield the area from interference.

The first antenna built there was a 26.3 MHz dipole array of 3 km aperture (Erickson 1965). It was a rather complex compound interferometer consisting of grating arrays spaced 440 m apart that produced multiple lobes and a filled aperture that, when correlated with the grating array, formed a single beam on the meridian. The beam could be moved north or south by manually changing phases on all of the 312 dipoles. The arrays and their beamshapes are diagramed in Figures 2 and 3. Multiple East-West drift scans of variable solar or planetary sources could be made with the grating array while the single beam could be used for relatively deep surveys of sidereal sources.

In the early days conditions were quite primitive, I worked at Clark Lake either alone or with a technician who was assigned part-time to my project. We had a small motor-generator for power, a small trailer to house the receiving gear (see Figure 4), we slept on the desert under the stars and cooked on a camp stove. After surveying and marking the locations for all of the dipoles and open-wire transmission lines, I invited the whole laboratory staff and their wives out for several weekends of posthole digging and wire stringing (Figures 5, 6, 7) to construct the system. Figure 8 shows the part of the completed open wire transmission lines between the grating arrays and one of the grating arrays is shown in Figure 9.

When the system was completed I found, to my dismay, that it did not work at all well. Its sidelobe levels were intolerably high. I spent weeks or months

Figure 1. Bill Erickson and his 1952 Packard on their first visit to Clark Lake in 1958.

transmitting RF signals into the array and measuring the phase and amplitude at each dipole with a pick-up probe on the end of a long coaxial cable that I dragged up and down the array. The array employed a branching feed system with phase adjustment by moving hundreds of tap points on the open-wire transmission lines. What I found was that, when the array was phased to the zenith, all dipole phases and amplitudes were nearly equal and the array worked reasonably well but, when it was phased north or south, the phase distribution was nonlinear and amplitudes were unequal, especially near the ends of each array. I realized that the problem was caused by mutual coupling between the dipoles. When the array was phased to the zenith, all the induced signals between the dipoles had the same phase and the problem was minimal. When the dipole phases were varied, the induced signals varied in phase. The currents induced in adjacent dipoles were large, about one-third of the proper current. This caused large phase errors and amplitudes that could vary by a factor of two between the conditions when the induced current adds or subtracts from the proper current. Near the center of an array of equally spaced dipoles, the coupling from dipoles located symmetrically on the opposite sides of a given dipole cancel but near the edges of the array, where this cancelation does not occur, the effects become serious.

My first attempt was to calculate and correct for the mutual coupling effects. Theoretically, this involved setting up a huge matrix describing all the mutual impedances between each dipole and all of the other dipoles in the array, then inverting this matrix to obtain the required corrections. In practice, the coupling

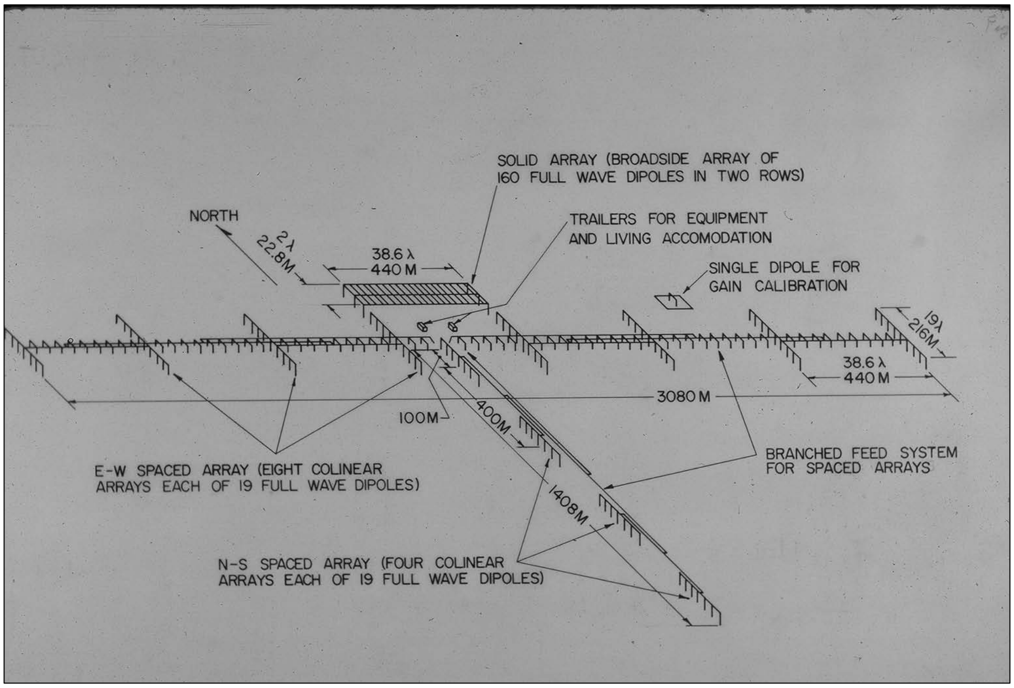

Figure 2. The layout of the 26.3 MHz dipole array.

effects are only appreciable between adjacent dipoles and those separated by one. I tried to calculate the required corrections (using only a slide rule - no electronic calculators existed in those days). This helped, but not enough; probably my understanding of the approximations involved in the formulas that I used was not sufficient.

By this time a full report concerning the design of the highly successful Mills Cross in Australia had been published (Mills et al., 1958). I read it with care and finally understood why Bernie Mills had used a dipole feed system that originally had seemed very strange to me. The basic idea is to make the current in each dipole independent of induced effects and dependent only upon the current impressed by the feed system. For a half-wave dipole one uses a high impedance feed that blocks the induced currents as illustrated in Figure 10. I spent months rebuilding the feed system for my array using a variant of this principle. It worked beautifully. However, by this time (1961) Convair was in a deep financial crisis because of the losses that the company had sustained developing and building the Convair 880/990 transports, four engine jets to compete with those of Boeing and Douglas. The planes did not sell well and the company almost went bankrupt. They decided that they could no longer afford a basic science laboratory. They gave us several months warning and told us that when the laboratory closed they would find engineering positions for us if we desired them. All but one of the staff left for academic or government positions and the CSRL disappeared. Only some solar and planetary work had been produced by Clark Lake by that time.

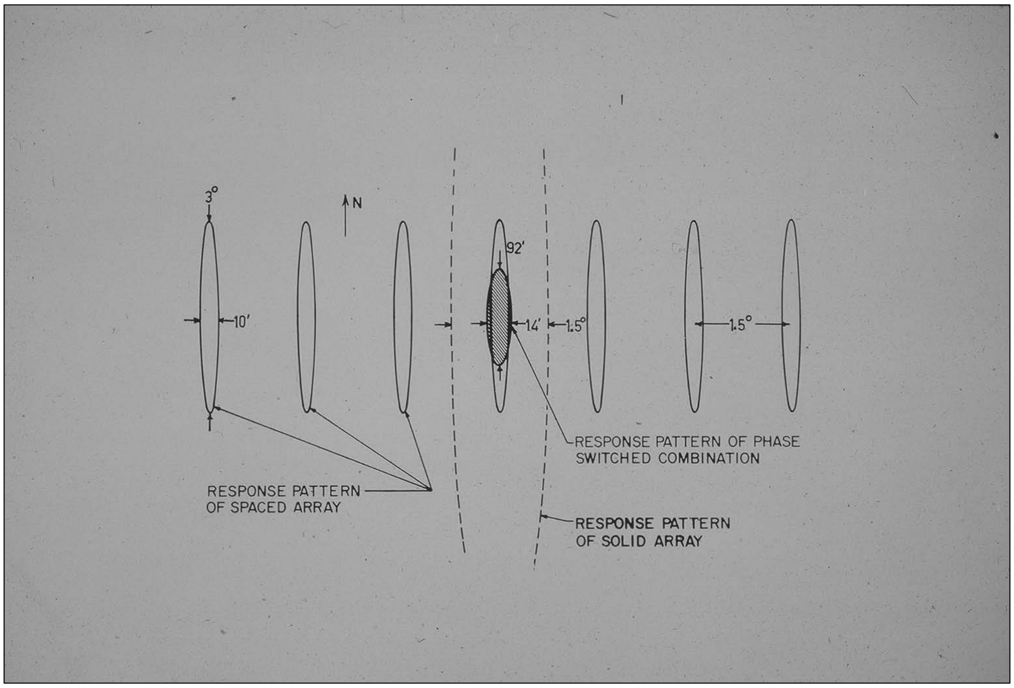

Figure 3. Response patterns of the grating array and the solid array together with their single, correlated beam.

The main lesson that I learned from these experiences was that long wavelength work is not easy as it seems. The equipment looks exceedingly simple, but it can still be tricky and difficult. In particular:

BEWARE OF MUTUAL COUPLING EFFECTS

I'm confident that mutual coupling effects caused the high sidelobes of both the DTM Cross and the first-generation Clark Lake antenna. There exist many possible solutions for the coupling problem but one must be certain to have solved it before construction of any large array can begin.

I accepted a faculty position at the U. of Maryland but, before I moved there, Jan Oort contacted me and invited me to come to Leiden for a year or two to work on the Benelux Cross Antenna Project. That was interesting to me and the Maryland administrators were willing to hold my position open for me, so I went.

3. Leiden

The Benelux Cross Antenna Project, based at the Leiden Observatory, had gone through several stages of design. It began with the desire by astronomers in the Low Lands to exploit radio astronomy with a large instrument of arc-minute angular resolution. Because it had been conceived by astronomers, one of its requirements was that it should produce data like an optical telescope. An astronomer should be able to go to the telescope, take his"exposure", and return

Figure 4. The first equipment trailer with propane tank for AC generator in the distance.

Figure 5. One of the antenna construction parties at Clark Lake.

home with a map of his specified area of sky. To meet these and other require-

Figure 6. Wives of the laboratory staff working on construction.

Figure 7. A theoretical physicist at work; Charlie Critchfield erecting dipoles.

ments, in 1958 Charles Seeger first designed a huge array of dipoles operating

Figure 8. The open wire transmission lines carrying signals from the grating array.

at 75 cm. “Chris” Christiansen joined the project in 1960 as did Jan Hogbom, who had just completed his Cambridge PhD with Martin Ryle. I replaced Chris after he returned to Australia in 1962. By this time plans for the telescope had evolved through large cylindrical paraboloids to dishes for continuum operation at 21 cm. The requirement for producing a map from a single observation still existed so Jan and I, with long distance collaboration from Chris, developed plans for an array of some 103 dishes, each 25 m in diameter, plus a single 70 m dish to fill in the short spacings. It was a monstrosity that none of us liked, including Ben Hooghoudt, the excellent mechanical engineer who designed the dishes and who would be responsible for building them. However, that was what was required to obtain all Fourier components simultaneously to produce an immediate map.

Information about the development of Earth rotation synthesis was coming out of Cambridge, this seemed like a promising approach so Jan Hogbom visited there to learn the details. He returned full of enthusiasm for the concept, Ben and I immediately embraced it and Chris liked it as well. Ben, Jan, and I convinced Oort that it would not be practical to build the telescope unless a much slower instrument was to be made, one utilizing Earth rotation synthesis and requiring multiple 12 hour tracks to produce a map. This was the basic design of what became the Westerbork Synthesis Radio Telescope (WSRT). Oort agreed with the design but he envisioned difficulties convincing other astronomers and funding agencies of our plans. He spent several years on various political problems involved with the instrument. The basic design was more or less agreed upon by the time that I left Leiden in 1963 for my post at the U of MD, but

Figure 9. One of the grating arrays, each consisting of 19 colinear full-wave dipoles.

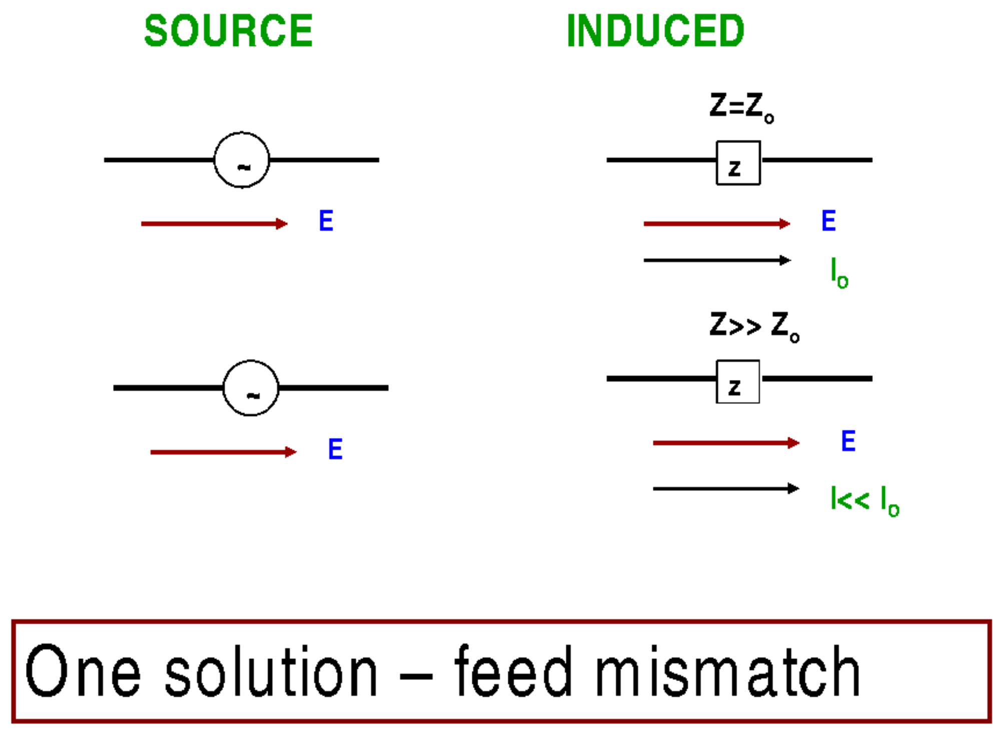

Figure 10. A high impedance feed blocks induced currents to reduce the effects of mutual coupling.

several more years elapsed before funding and siting problems were solved and the actual construction of the WSRT began.

The main lesson that I learned in this case was:

BE CAREFUL NEVER TO OVER SPECIFY A SYSTEM - KEEP IT SIMPLE

One must always be certain that all the specifications and requirements of a system are absolutely necessary. It is easy for features to creep into the specifications that are not vital even though they may contribute to the convenience of the system. They quickly are adopted and become so familiar that simpler alternatives are overlooked. More about this later.

4. Maryland/Clark Lake

Convair had continued payments on their five-year lease of the Clark Lake site while I was in Leiden, but the array had remained essentially idle. After I got to Maryland, Convair transferred ownership of the project to the U of MD at nominal cost and we began operating the system from Maryland. I did consider moving the project to some place on the East Coast but could find no suitable site. Good sites existed in Southwestern USA but travel times and costs were not appreciably less than for Clark Lake, so we continued the project there. Work with the 26.3 MHz array produced several PhD theses including a full-sky survey of radio source positions and flux densities.

Over the years several other institutions made use of the site; NASA-Goddard for a Jupiter interferometer and for a collaboration with the U of MD to build a 3 km East-West, 20-60 MHz array of 16 large log periodic antennas for solar work (Erickson & Kuiper 1973), the U of Iowa built the Cocoa Cross there for 34 MHz interplanetary scintillation (IPS) studies (Cronyn & Shawhan 1975), and the UCSD used the site for one of its three IPS stations (Coles & Kaufman 1978). These associations were almost always mutually beneficial but we found that it is necessary to be very cautious about sharing a site between institutions. Problems can arise easily, for example, interference is usually blamed on some other institution, as is equipment that is lost or broken. Even though the people directing the different projects are good friends and understand the situation, workers in the field sometimes come into conflict because of conflicting requirements and orders from the different institutions. Considerable care must be taken to keep the relationships from deteriorating. This should be kept in mind when plans for sharing sites between the LWA, the NRAO, and FASR are considered.

Mukul Kundu joined the U of MD faculty in 1968. We became partners in the project. He directed the solar work and handled much of the administration; I directed the sidereal work and most of the technical development. In particular, he was primarily responsible for analysis of the solar data produced by the log periodic array mentioned above and for the solar work with the TPT.

5. The Clark Lake TPT

5.1. Design and construction

For the dipole array at Clark Lake, moving of the beam required about 3 hours and 8 km of walking to manually adjust the feed points of the 312 dipoles. One had plenty of time to consider better ways of phasing an array, so in the 1970's we designed and constructed a new, highly sophisticated instrument more similar to LOFAR and to the LWA than any system that has yet been built. The array consisted of some 800 fixed log-spiral elements that were electronically phased in order to steer the beam of the array to any point in the sky. The main array contained 720 elements; the others were used for special purposes or as spares. Figure 11 is a photo of the South arm of the "T" shaped system. Since the elements were reminiscent of Indian teepees, the system was called the TPT. Its aperture was 3.0 km E-W $\times$ 1.8 km N-S, the largest aperture that would fit on Clark Lake. Phasing of each circularly polarized teepee was accomplished by rotation of its illumination using a diode switch at its feed point. Figure 12 is an upward looking view of one element showing its eight spiral wires clipped onto eight support ropes and Figure 13 shows one of the diode switches. Rephasing of the array to point its beam in any arbitrary direction required some 22 μs (10 μs to generate the phasing instructions plus 12 μs for the round-trip time to the end of the arms).

In the case of the teepees, mutual coupling was no problem. As shown in Figure 14, circularly polarized antennas induce signals of the opposite polarization.

Figure 11. A view from the end of the south arm of the TPT.

The teepees responded to only left circular polarization, so they did not respond to the induced signals. The coupling between adjacent teepees was very low; it varied between -35 dB at ∼25 MHz to -60 dB at ∼100 MHz.

The layout of the system and its specifications are given in Figures 15 and 16. The elements in each arm were gathered into banks of fifteen; the 480 elements of the East-West arm comprised 32 banks while 16 banks contained the 240 elements of the South arm. Pressurized 7/8" diameter coaxial cables each some 1.8 km in length were buried about 1 m deep to carry the signals from each of the 48 banks to the observatory building. The signal from each of the banks was processed by an electronics chain as shown in Figure 17. The raw spectrum produced by the array varied by some 35 dB across the band because of the Galactic background slope and cable loss. A slope filter flattened this variation to <3 dB, thus greatly reducing the dynamic range requirements for the system.

I use a rule-of-thumb that 70 dB is the minimum dynamic range that can be tolerated for a wide band system operating in this frequency range at a quiet site. If the dynamic range is below this level, system operation will be seriously compromised by intermodulation of the strong RFI below 30 MHz. The TPT had a dynamic range of about 80 dB and was only occasionally disturbed by intermodulation. To reduce intermodulation in the wideband portion of the TPT receivers, the gain was kept as low as possible until after the bandpass filter in the second IF.

The operating frequency was set by the first LO that took about 1 ms to settle on a new frequency. This limited the speed with which the frequency

Figure 12. One of the TeePee elements. Eight ropes support the spiral wires that carry the RF signals.

could be changed. The rapid beam positioning and frequency adjustment were very important attributes of the system.

The adjustable bandpass filters in the second IF turned out to be almost unnecessary. We hardly ever needed to use the two narrower bandpasses and, since we employed no RFI excision techniques, it was difficult to find a 3 MHz region of the spectrum that remained sufficiently clear of RFI for long periods of time. The 3 MHz filters found little use because data taken at this bandpass usually required too much manual editing to eliminate RFI. 1.5 MHz was the standard bandpass that we used; using this bandpass we could normally find frequencies that remained essentially clear for months at a time. Part of the reason for this was that the skirts of the 3 MHz filters were not nearly as steep as those of the other filters.

The design of the teepees and the analog electronics was carried out by Rick Fisher (then a graduate student), John Hubbard (an electrical engineer), and me. Our original design (Erickson & Fisher 1974) called for the banks of each arm to be combined in seven fan beams that would then be correlated to form 49 pencil beams. We had only a small work crew of three or four so construction of the antenna elements and buried transmission line system required several years. By the time that they were complete, digital technology had advanced to the point that cross correlation of all of the banks and mapping of the whole primary beams of the banks was practical. This required a 1024 channel correlator that was initially designed by Beat von Arx and Mengia Caflisch and completed by Karl Erb and Mike Mahoney (Erickson, Mahoney, & Erb 1982). This correlator

Figure 13. The 16-diode switch at top of each TeePee that rotates its excitation and changes its phase.

is illustrated in Figure 18. Phase quadrature components of each of the 16 South arm signals were correlated with each of the 32 East-West arm signals. We digitized 10 MHz IF signals with 12 MHz samplers employing the 2nd Nyquist zone. At 1.5 MHz bandwidth the signals were four times oversampled. Our system used the correlator chips that had been developed for the VLA; in fact, we obtained a test run of chips that were delivered to us before the VLA obtained their chips. The 800 antenna rotation switches, analog electronics system and the correlator were all built by Stig and Iva Johansson.

After correlation, the bits were accumulated in an up-down counter. We counted up, then reversed a phase switch in the fixed frequency second LO and counted down. This removed many baseline offsets, such as sampler offsets. Time delays between the banks were compensated in discrete steps of 83.3 ns (one 12 MHz clock cycle) with 128 tap digital delay lines. Because of the limited aperture and low operating frequency, fringe rotation rates were low and the fringes were stopped in software.

Unfortunately, the system was designed and built before self-cal was developed and we performed no correlations between the banks within a single arm. To implement phase closure and self-cal a different correlator architecture and 1232 more correlators would have been required. During periods of good ionospheric stability we calibrated the total phases and amplitudes of the 48 channels at each frequency using strong astronomical sources. The cables and amplifiers were extremely stable and we found that these astronomical calibrations were valid for weeks or months. Basically, the calibrations were valid until some problem

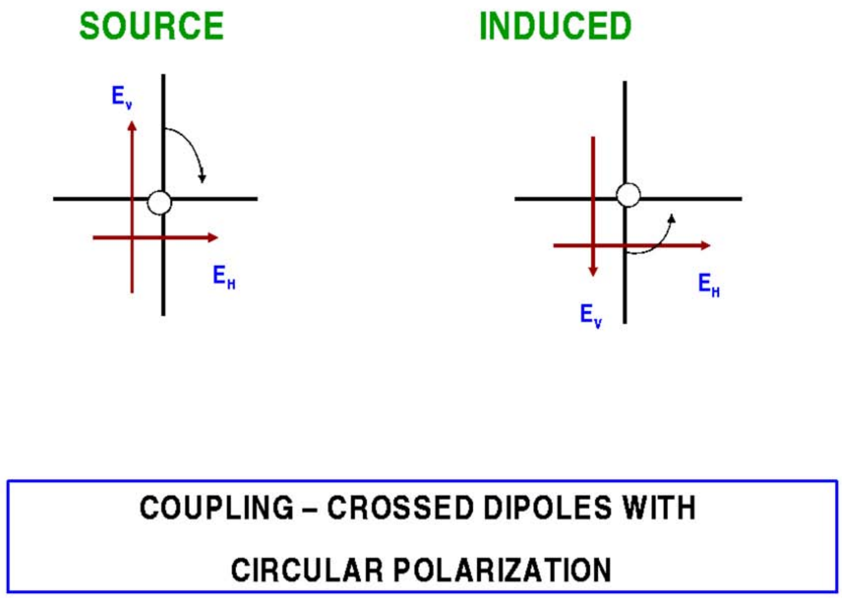

Figure 14. Circularly polarized antennas couple with opposite polarizations.

occurred that required hardware changes. We could not use ordinary self-cal to compensate dynamically for ionospheric variations although we did make a first order ionospheric refraction correction. Without self-cal, the dynamic ranges of our maps were limited to about 1000.

By the time that the system was completed, the Observatory had developed to a reasonable level of living comfort and convenience. Figure 19 is an outdoor view of the buildings while Figures 20 and 21 show the receiver room and the computer room.

5.2. Operation

With the initial operation of the system a completely unanticipated problem arose. We found time variable, spurious output correlations well above the 1 Jy design sensitivity of the system. Many months of tests were conducted in order to determine the origin of these spurious correlations. When the cable system was disconnected and the signal inputs were loaded where they entered the laboratory building, the correlations disappeared. When the underground cables were connected, but with loads on their far ends, the spurious correlations reappeared. It seemed that there was some kind of cross-talk or signal leakage between the buried cables but tests made by injecting strong signals into one cable and measuring the leakage into the others showed that this was not the case. Perhaps some low level signals that were induced on the shields of the cables were leaking into the system and causing the low level correlation. We

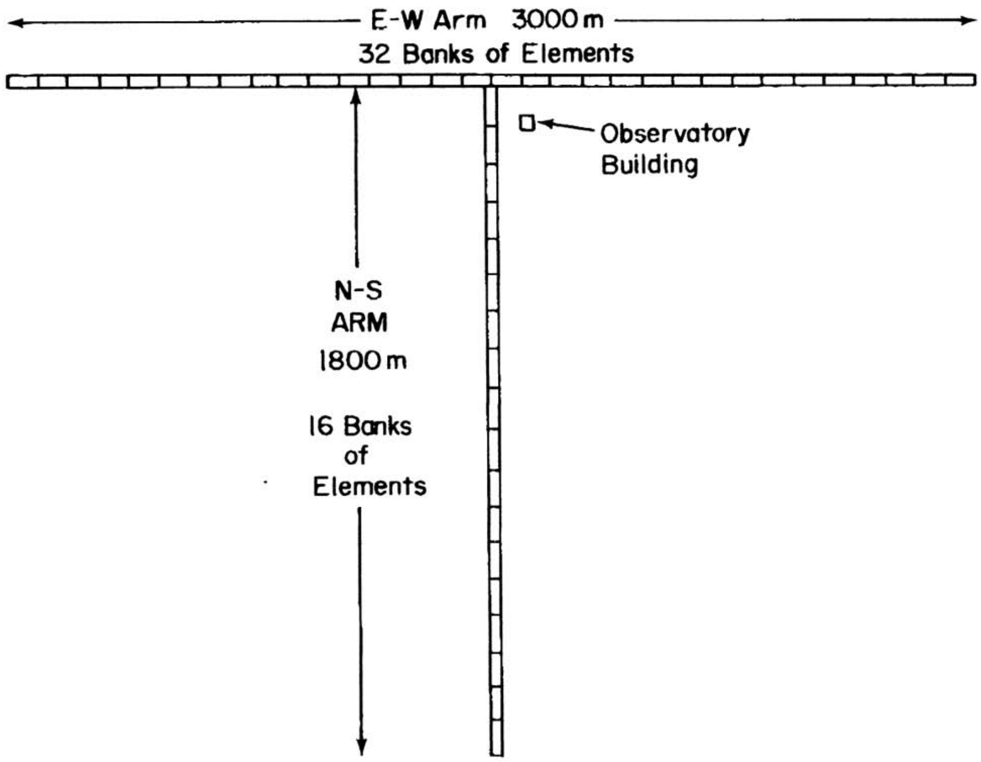

Figure 15. The layout of the TPT.

were never able to find the origin of the problem. Finally, we simply eliminated it by rapidly rotating the antennas in the arms of the array to produce phase inversions of the sky signals. These phase inversions were then demodulated by extra switching cycles in the second LO. Thus the problem was eliminated without ever being solved and this leads to another lesson:

EXPECT THE UNEXPECTED

Only a very small number of systems for low frequency radio astronomy exist in the world. For higher frequency work there is a much larger background of technology and experience. This means that unanticipated problems can more easily arise in low frequency work.

The pointing and frequency agility of the system allowed us to develop many different observing modes for different projects. In daytime the system was used for the Sun and we jumped quickly between a number of fixed frequencies in order to make multifrequency maps of solar bursts. For ionospheric work we rapidly pointed the beam to a dozen or more strong sources to make nearly simultaneous measurements of ionospheric refraction in different parts of the sky. Our most common observing mode at night was sidereal and involved long integrations of single source regions. To achieve high sensitivity we needed to average over both noise fluctuations and sidelobe responses. In order to map a

TPT SPECIFICATIONS

Parameter	Value
Frequency range	15–125MHz
Instantaneous bandwidth	0.15–3 MHz
Total collecting area	$250\lambda^2$
Resolution	
20 MHz	17′
110 MHz	3′
Steering and frequency changing time	$\ll 1$ ms
Sky coverage	$< 45°$ zenith distance
Sensitivity (and confusion limit)	~ 1 Jy at all frequencies
Polarization	left circular

Figure 16. The specifications of the TPT.

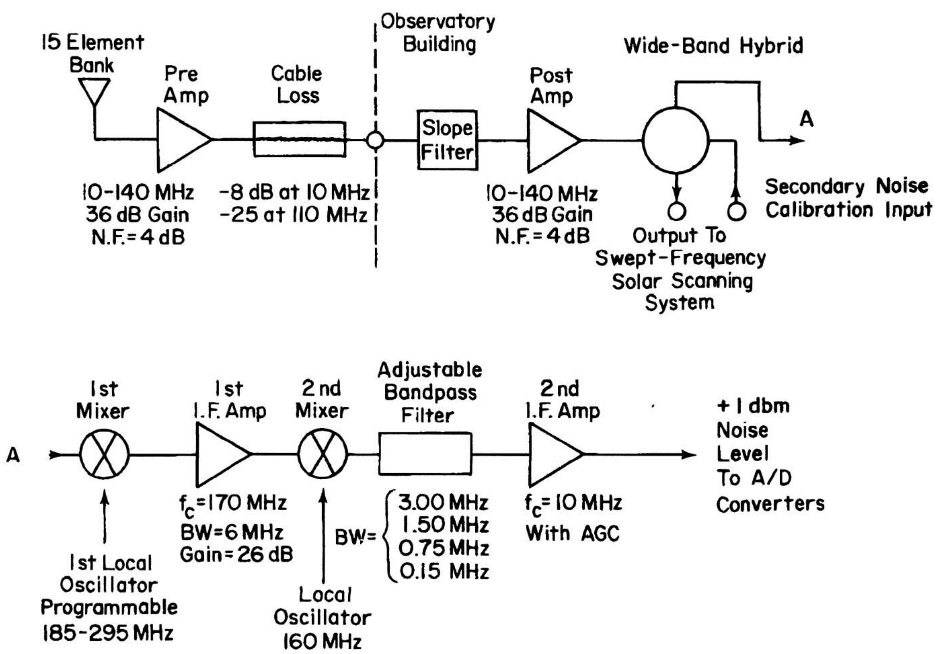

Figure 17. One of the analog electronic channels attached to each of the 48 banks.

given region we pointed the beam 1/16 of a East-West bank beamwidth ahead (west) of the map center. We took drift scans leaving the system completely

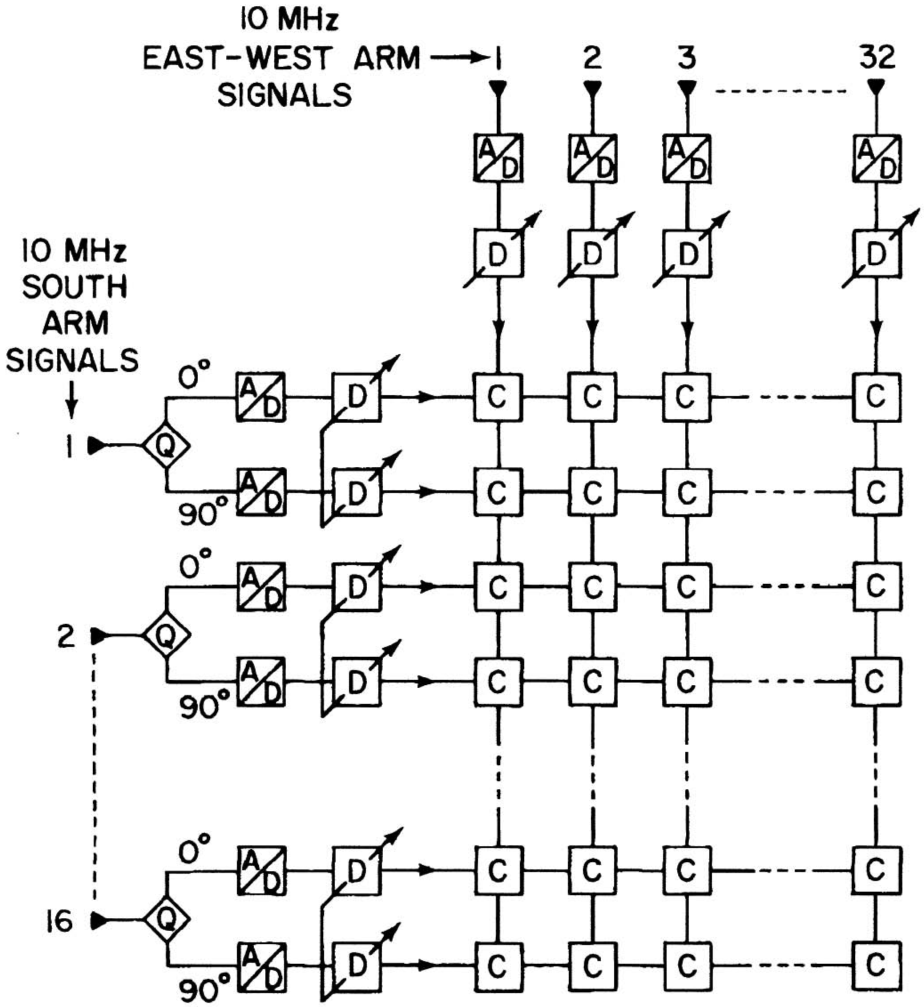

Figure 18. Architecture of the 1024-channel correlator.

unchanged until the beam was 1/16 of beamwidth east of the map center. This was called a "scan". The change of the gain of the banks over the ± 1/16 beamwidth of a scan was negligible and, when unexpected things occurred (like RFI, for example), it was useful to know that they were not a result of any beam motion or sidelobe motion caused by rephasing. I would suggest that this is a good observing mode for any scanning antenna. On a TV we displayed the aperture plane data and its sky-plane transform each second. This was often very useful for recognizing different problems. Watching this display for hours at a time made one very good at doing FFTs intuitively!

Because of foreshortening effects, the map region expanded and rotated when observing in any direction away from the zenith. We found it most convenient to work in the image plane rather than in the uv plane. During a scan, the sky-plane data were averaged and written to tape each second. In off-line processing

Figure 19. Buildings and living quarters at Clark Lake, 1980 era.

Figure 20. Receiver room with the analog receiving system and the correlator at left, solar equipment at right.

Figure 21. Computer room, processors, tape drives, and disk drives.

the dirty beam was calculated for each second and averaged as well. At the end of each scan a coordinate transformation was made to the map of the data and the dirty beam consistent with data taken at the zenith. These data were interpolated onto the zenith map grid and those parts of the observed map that were beyond the zenith map were discarded. [1] The maps were then accumulated but, before adding each new map to the accumulated map, it was slid in each coordinate to maximize its correlation with the accumulated map. This made a correction for uniform ionospheric refraction. Differential refraction effects across the area of the map were not corrected. The map size was relatively small, 2.6° at 60 MHz, and differential refraction was not normally a problem. At the end of an observation, the accumulated dirty beam was used to clean the accumulated map. This procedure accounted for the beamshape variations caused by foreshortening of the fixed banks of elements and also made a first order correction for refraction. A sample output map is shown in Figure 22.

The system was quite productive in both solar and sidereal science but it was limited by its rather low angular resolution and sensitivity. To significantly raise these limitations required a large new project with multiple sites like LOFAR or the LWA but funding for all radio astronomy installations became very stringent in the late 1980s and a large new project could not be realistically contemplated.

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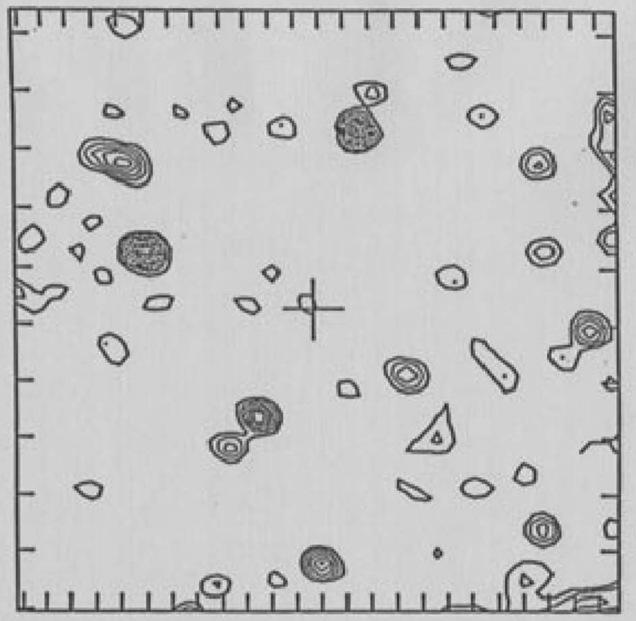
[1]Foreshortening only makes the arms of the array appear to be shorter than at the zenith. Therefore, all map regions off the zenith are larger than ones at the zenith and they include the zenith map area



Of the dozen or so sources detected in this 57.5 MHz radio image from the Clark Lake Radio Observatory, only two are known to have been detected by any other radio telescope in the world. Nearly 5,000 fields this size (2.7 degrees square) are observable from Clark Lake, but only a small fraction have actually been observed.

Figure 22. A typical output map from the TPT.

The VLA, for example, had trouble securing enough support to maintain its rail lines for changes in its configuration.

5.3. The end of the TPT

All of the other smaller university-operated radio observatories had been forced to close by 1988 when the NSF terminated funding for Clark Lake. At that time we had just begun full sky radio source surveys at two frequencies. Utilizing the fast switching capabilities of the system these surveys were conducted in a novel fashion. We divided the observable sky into thousands of overlapping fields, then the system observed these fields randomly, jumping between them once per minute. The observation probability for each field was biased by its elevation (to avoid spending most of the observation time near the horizon) and by the total length of previous observations (to observe all fields equally). In this fashion we hoped to build up a relatively uniform survey in spite of ionospheric effects or other problems. However, we were too clever for our own good and, when we were forced to close, we only had a very poor, useless surveys of the whole sky at two frequencies but we did not have any useful well-observed sections.

Ours was a small project that did not attract any large amount of support in the radio astronomy community. The main reason for this was because of the system's limited angular resolution and sensitivity combined with its low operating frequency. At that time, most astronomers considered low frequencies to be uninteresting. We did our best to attract outside observers but only a small number of them came to the site and made use of the system. Its modes of operation and capabilities were unfamiliar to most people and it was located at a remote site with harsh weather and few creature comforts. We could not afford to cover travel costs or to provide telescope operators, maids, or cooks. An outside observer was forced to run the system and process the data himself, buy and cook his own food, and clean his own room. This tended to make the system rather unpopular.

Our project received good support among the Astronomy faculty at the U of MD, but little or no support from the University administration. The owners of the land at Clark Lake, the Burnand family of Borrego Springs, had donated the site to the U of MD some years earlier. The administration was anxious to sell this asset. Therefore, the electronic and computer systems were donated to the Mauritius project, the antenna system was dismantled (see Figure 23) and sold for scrap and other movable assets were sold or given away. The buildings were demolished and today the site looks exactly like it did in Figure 1 when I first saw it. I was disappointed by this whole process so I took an early retirement and moved to Tasmania.

Figure 23. The wrecks of dismantled TeePees.

The most enduring legacy of Clark Lake is the students that were trained there:

PhDs:
Will Cronyn
Mel Viner
Tom Kuiper
Rick Fisher
Tom Gergely
George Resch
Rick Perley
Bob Hanisch
Namir Kassim

plus MA students and many undergraduates who were first exposed to research at Clark Lake and went on to distinguished careers in science .

6. Lessons for the LWA

6.1. Keep it simple

Since funds are limited, every feature of the system must be evaluated to be certain that it is actually required scientifically.

Modern digital and electronic technology provide many opportunities that did not exist when we began designing the TPT system thirty years ago. However, I feel that often one is tempted to include features in a system because they are clever and relatively easy, not because they are vital. (We have all seen too many "clever" features in Microsoft programs that tend to obstruct rather than facilitate processes.) These sorts of features must be avoided, especially when they cost time or money.

Multiple beams for the LWA are certainly useful, but they tend to be expensive and are not imperative scientifically. If providing multiple beams complicates the system appreciably, the alternative of time sharing should be seriously considered. Time sharing costs almost nothing and it is a powerful technique for many purposes. It basically involves trading data acquisition speed for simplicity. That is what was done when the Benelux Cross became the WSRT. Trading speed for simplicity could reduce costs enough to bring the LWA into a realistic funding range. I have not seen a careful cost analysis of this alternative. This may be caused by the fact that most people have never operated an electronically-switched, time-sharing telescope. Note that with time sharing between n programs, each program loses sensitivity by a factor of $\sqrt{n}$, but if this were to save enough funds to allow a system to be built with m times the collecting area, then each program would gain sensitivity by a factor of m. If m were greater than $\sqrt{n}$, each of the programs would actually gain in sensitivity. The tradeoff is not obvious.

Phase shifting of the station elements by electrical rotation of the circularly polarized antennas may be a simple and practical alternative to the provision of delays in all the transmission lines. As seen from my distance from the project, the fractional bit corrections required for sufficiently accurate delays seem to be complicated in comparison with phasing.

6.2. Mutual coupling

There are several ways to solve the mutual coupling problem, but it is important that it be recognized and solved early in the design of the system. For the LWA it should not be a problem if the active baluns are designed in such a way as to effectively block the currents induced by nearby dipoles. I believe that this can be done but I find it difficult to be absolutely certain because I always apply the principle of reciprocity to these problems. This principle does not apply to active systems. Careful analysis of any possible problems should be made at the earliest possible time.

6.3. Expect the unexpected

The LWA will not have the long (1.8 km) buried cables of the sort employed by the TPT and problems like those that appeared on the TPT should not occur. I have never encountered any similar problems with ∼100 m cables of the type that will be needed within an LWA station. Of course, LWA will have very long optical fiber cables but it seems unlikely that they will cause problems. Optical fibers do not involve metallic connections and the technology of their use over long distances is very mature.

Since the LWA is a wideband system, some intermodulation problems can be expected, but unexpected problems may also arise in this area. The LWA is designed to measure extremely small correlated signals. RFI will be quite well correlated across the whole aperture of the array and low level, partially correlated intermodulation products may be generated. The normal specifications for comparing the linearity of devices are their IP2 and IP3. These specifcations are useful, however, I have not found them to be reliable for an actual calculation of the strengths of intermodulation products. When devices are actually connected to an antenna, the intermodulation products are often stronger than I would calculate using the concepts of IP2 and IP3. Perhaps this may be caused by the fact that IP2 and IP3 pertain to the products generated by two or three simultaneous signals. In the radio astronomy case at frequencies below 30 MHz, many hundreds of signals are simultaneously present.

Ionospheric calibration and the corrections for ionospheric refraction are, by far, the most difficult problems faced by the LWA. In this area I am certain that many unanticipated problems will arise.

7. A final thought

GOOD LUCK
The LWA is a truly exciting system to contemplate.

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