From: "Alan Bridle" <abridle@nrao.edu>
Subject: Re: The DRAO 10-MHz telescope {External}
Date: Sun, November 27, 2022 7:25 pm
To: "Elizabeth Griffin" <r.elizabeth.griffin@gmail.com>

Hi ELizabeth

I am glad you found all this useful and I very much look forward to seeing the whole book.

The DRAO in the mid-1960's was a great place to be. The Cambridge radio astronomy group was very active and Martin Ryle was an inspirational leader, but in retrospect I think it was quite introverted and it was famously secretive with other groups, even within Cambridge. The DRAO was the first example I saw of a government-run research lab with a very productive and open relationship with a related university community, and its lunch-room was an immensely stimulating place for a junior researcher. I felt that everyone there was an equal - if they had something to add to any discussion they could just jump in. John Galt inspired the sort of anarchy in the place that made it creative and fun. I later saw a similar phenomenon at Green Bank (where the only eatery for about 25 miles was the NRAO staff cafeteria) but the atmosphere of that DRAO lunch room had a lot to do with why I decided to go back to Canada after I got my Ph.D. Queen's U. was "off the beaten track" and quite isolated for an astronomer, but I was able to spend parts of my summers at the DRAO and continue the enjoyable experience I had had there as a student.

So I am very glad that you will add that little story to yours, as it is a nice example of what the DRAO was very good at - and began a tradition they seem to have have built on again with the success of CHIME.

Best wishes

Alan Bridle

On Sun, November 27, 2022 11:34 am, Elizabeth Griffin wrote:
> Hi Alan,
>
> Thank you so much for all those splendid details, and also for the
> time and trouble you have taken in order to help that section of the
> DRAO chapter to be both more accurate and more interesting. Adding
> details (such as the lunch-room discussion at the DRAO) bring the
> whole matter to life in a very beneficial way.
>
> Kind regards,
>
> Elizabeth
>

From: "Alan Bridle" <abridle@nrao.edu>
Subject: Re: The DRAO 10-MHz telescope {External}
Date: Sat, November 26, 2022 3:40 am
To: "Elizabeth Griffin" <r.elizabeth.griffin@gmail.com>

Dear Elizabeth

I attach my comments and suggestions on your DRAO chapter segment about the 10-MHz array. Please do not be alarmed by the length of my text, which is mostly deep background for you about why I am suggesting the changes (which are in bold text should you or Jasper need to speed-read them and skip the rest). The changes I am suggesting are relatively small in size and number as I feel the main thrust of what you have already written is perfectly ok. There are a couple of places at which Rob's perspective from the 22 MHz project differs a little from mine from inside the 10 MHz one, but the specific changes I am suggesting are fairly small so I hope they will be easy for you to accommodate.

I am sure the Canadian radio astronomy community will thank you both for taking on this project, and will look forward, as I will, to seeing the whole book.

Please do not hesitate to contact me again if I can be of further help to you. I will send you individual JPEGs of any photos from my report that you might want to add to this chapter - just let me know which one(s) might be needed.

Best regards,

Alan Bridle

On Wed, November 23, 2022 8:06 pm, Elizabeth Griffin wrote:
> Dear Alan,
>
> I have discussed this matter with Jasper (currently rather unwell,
> unfortunately), and am sending you herewith what the MS of the Book
> currently says about the 10-MHz instrument. The text is based upon a
> document written by Rob Roger and sent to Jasper several years ago.
> We wove it into our DRAO chapter, from where I have lifted the
> relevant pages. There is one brief comment at the veugh your lry end
> of the chapter that refers to the sorry state of the wooden poles,
> which were never creosoted (as were those for the 22-MHz instrument)
> and rotted after about 10 years in the ground unprotected.
>
> If you have time and inclination, please could you look through these
> pages and see if there are glaring omissions or serious errors that
> must be added or corrected? We cannot really make the book very much
> longer than its present length; your own, much longer, account is
> enviably detailed. but it would risk overbalancing other chapters if
> we include too much on this one instrument. A few additional
> paragraphs, up to (say) 2 or 3 pages, could be acceptable, and maybe
> we could think about incorporating one of your excellent photos (I am
> assuming they are your own). Otherwise, we could refer to the
> existence of your report when it has a home in an archive.
>
> We would very much value your help, and hope that our modification of
> Rob R's report is not too poor!
>
> With all good wishes,
>
> Elizabeth Griffin
>

Attachments:

Bridle comments on Wall-Griffin chapter.pdf	
Size:	191 k
Type:	application/pdf

Alan Bridle comments on the draft of DRAO low-frequency section

Section 7.3, first para.

These statements are all true but I feel that the emphasis may be a little misleading. Here - for background - is how I saw the early story from my perspective as a student who was being drawn into the DRAO 10-MHz project.

When I began working at the Cavendish in the Summer of 1963, most of the discussion about the low frequency work involved John Baldwin and Francis Graham-Smith. Both John and Graham were then actively supervising students working on the low-frequency spectrum of the diffuse galactic background at low resolution (Baldwin), solar radio emission from Earth satellites (Graham-Smith) , and both discrete sources and the galactic emission at 1-degree resolution with the 38-MHz aperture-synthesis T. The latter project had been started by Graham-Smith with Carman Costain as his student, then carried on by Baldwin with Sidney Kenderdine and Phil Williams as his students.

Peter Scheuer's interest had been more distant (he had been on sabbatical in Australia) and it was also more specific – he was particularly interested in why the low-frequency spectra of some of the most compact sources "turned over" at frequencies below 80 MHz, and he was studying possible mechanisms for that turnover theoretically. Ryle was of course interested in everything the Cavendish group was involved in - and he controlled most of the funding – but his main interest in the low-frequency work appeared to center around estimating the intensity of the nonthermal extragalactic background "monopole". That intensity, which was then called the "integrated emission" could be used to constrain the interpretation of the source counts – and specifically to show that a significant fraction of the actual monopole had already been counted, so that the highly controversial source count slope cooed not be dismissed as a purely "local" anomaly, as Hoyle was then advocating. Martin was passionate about this topic and was eager for someone to improve the estimate of the monopole intensity by mapping the northern minimum sky brightness at higher resolution and lower frequencies than had been possible for Costain and Turtle in their thesis work. The first attempt to do this had been made by John Pugh, but it was widely believed that Pugh's results were questionable doe to some deficiency in the construction f his array. In my very early days in the group I was charged with getting Pugh's 17.5-MHz array going again and making a new set of measurements. (I subsequently found that Pugh had miswired some of his baluns so he had observed with a broader beam than he thought. I also constructed a scaled replica of the 17.5 MHz array at 81.5 MHz so we could so a proper scaled-array analysis – in that work I was directly supervised by John Baldwin while we both made regular reports on my progress to Ryle.)

There had also been attempts to do galactic background work as low as 5 MHz with dipoles at the old Rifle Range site, but these had failed due to interference and (probably) absorption. Ryle was well aware of Costain's plans for the 22-MHz T at DRAO, and had visited DRAO in about 1961 (there are some fine color photos of that visit in the DRAO archives), but the Cambridge group did not have access to the real estate needed to push discrete-source spectral work lower than 38 MHz, as Baldwin was doing with Kenderdine and Williams. (Purton was working under Graham-Smith's supervision on the galactic spectrum and diode noise standards, Bryan Andrew on the galactic spectrum with dipoles and a small aperture synthesis array at 13 MHz, with - I think - Baldwin also as his supervisor).

So while it is true that Ryle and Scheuer were the most involved in getting funding for the Resolute Bay (mis)adventure and later for Cambridge's part in the 10-MHz array, it was Graham-Smith and Baldwin and their students who were most actively working at low frequencies. It was well understood

that - while interference and absorption had prevented the 5-MHz work on the galactic spectrum that had been attempted by Bryan Andrew) - you could do that sort of low-resolution work with dipoles down to 13 MHz from Cambridge, but scintillations and refraction would compromise work on discrete sources with about 1 degree resolution below 38 MHz. My impression is that Baldwin, Graham-Smith, Ryle and Scheuer were all equally interested in trying to push the envelope to lower frequencies by the time I started work at the Cavendish arrived - and that Baldwin and Graham-Smith who the ones who were most active in trying to do so. Graham-Smith had jumped in early on satellite-born radio astronomy (and he then moved to Jodrell Bank), but in 1963 he was still part of most discussions we had about low-frequency work (he was also the external examiner for my Ph.D. thesis in 1967)

So – I think it would be better to start out something like this: **" In the early 1960's a group including John Baldwin, Francis Graham-Smith and Peter Scheuer working under Martin Ryle at Cambridge were exploring ways to extend measurements of the galactic and extragalactic background and of the brighter discrete sources to lower frequencies during long winter nights in the coming solar activity minimum. Experiments at Cambridge had shown that low-resolution studies of the diffuse radiation with dipole antennas were compromised by interference and by ionospheric absorption below 13 MHz, while ionospheric scintillation and refraction made discrete-source work with larger antenna arrays difficult below about 38 MHz."**

I think it's then of historical interest to mention Ted Hartz as providing the original motivation for the Resolute Bay experiment - both because Hartz was himself an important figure in the history of Canadian radio astronomy, and to show that Scheuer did not simply pull the "polar cap" idea out of his theoretical hat. They went to Resolute Bay on the basis of what seemed at the time like actual experimental evidence that it might work.

I do not know exactly when or where Scheuer saw Hartz's data but I remember him being excited about it when I started work in the summer of 1963 - so it was before that. I asked Peter about it again during a ten-day period when Peter stayed at our house in Charlottesville in January 1994 (at that time I was showing Peter how to use NRAO's AIPS software to make images from VLA data). Peter had got the impression from Hartz that "the ionosphere disappeared" in the polar cap. In retrospect, it looks like Hartz's topside sounder may have seen no return because the peak electron density occurs close to ground level in the polar region so the signal was totally absorbed. My memory of what was said in 1963 was that Scheuer had encountered Hartz on an airplane while going to a conference, but Peter was unsure about the details when we talked 30 years later in 1994. I have wondered since if it was Graham-Smith who actually saw Hartz's data, as he and Hartz were both doing satellite-based experiments, but I am quite sure that Scheuer went to Resolute on the basis what appeared to be a "tip" based on unpublished data from Hartz.

In any case, it was Scheuer who made the first trip to Resolute Bay and set up the dipoles during the waning polar daytime in the early Fall of 1963. while Purton went there in January 1964 to do the actual experiment during the polar night. It was understood from the start that the Resolute Bay experiment needed frequemcy agility so Scheuer built two types of antenna – a 4.5 MHz single dipole to look at the galactic background (more or less matching what Bryan Andrew had tried to do at the Rifle Range) and some fat dipoles that could be used from 7 to 13 MHz as an interferometer to study Cas A scintillations over that whole frequency range (not just at 10 MHz). When Purton actually got data, Cas A was never seen by the interferometer at any frequency, so he cobbled a 28-MHz setup together which did detect Cas A but with strong scintillation and lots of absorption. Data from Jack Belrose's ionosonde, which operated on the ground at Resolute Bay, later clarified what was actually going on, and that the whole effort really was a giant misadventure. The main thing it had achieved

however, was to bring Peter Scheuer more "into the loop" with some Canadian radio astronomers, sowing the seed for the DRAO collaboration that followed.

Bottom line: I suggest amending the second sentence of 7.3 to read

"Following a suggestion from data obtained by Ted Hartz with the topside sounder on Alouette I that the ionosphere might be much less dense at latitudes above the auroral zone, Peter Scheuer went to Resolute Bay in the late summer of 1963 to set up a single dipole for 4.5 MHz and a two-dipole interferometer usable from 7 to 13 MHz to explore the possibilities for polar-cap radio astronomy. Purton (then a research student at Cambridge working under Graham-Smith) followed Scheuer to Resolute in January 1964 to operate this equipment but he quickly found that observing conditions were not what had been hoped. He improvised another interferometer for 28 MHz which barely detected Cas A through severe scintillations and absorption. This experiment made it clear that Resolute Bay was an unsuitable site for radio astronomy even at night near solar minimum, so the Cambridge group turned its attention to the possibility of collaborating with work on a second low-frequency array at the DRAO."

The detailed story of how the Mullard-DRAO axis got started seems to be lost, as all of those involved – Jack Locke, John Galt, Martin Ryle, and Peter Scheuer – have now passed away and so far as I know did not write the details down in any public document I also never talked directly to Jack Locke or to Martin Ryle about this – they were too senior and I was too junior for us to have had such conversations.

I do recall both John Galt and Carman Costain telling me that the 10-MHz array project had become possible because of the sudden cancellation of another project that the Department of Mines and Technical Surveys had planned in the North-west Territories. DMTS had planned and funded a chain of seismic stations, presumably to help them to map density distributions that could be relevant to resource exploration, but had decided to "can" the project when they assessed that ongoing operating costs would be too high. This apparently created a "use it or lose it" situation for the construction money, which Jack Locke was able to divert into funding for laying out and building the 10-MHz array.

I remember this topic specifically because it was my own first encounter with the "use it or lose it" funding principle, and John Galt was sensitive and vocal about all the difficulties that arise when operational funding does not follow construction funding at an appropriate level. I also recall Carman Costain being critical of the haste in which the 10-MHz project got started, and of bad decisions that had been taken because of that – such as the failure to creosote the 10-MHz poles. A lot of my information about these early days came from Carman while we were both watching strong sources scintillate at 22 MHz and deciding whether it would be worthwhile for me to drive out through the cold and snow to the 10-MHz T to operate it hands-on for a whole night.

So - I suggest slightly modifying the start of the second para of your Section 7.3 along these lines:

"During the time that Purton was at Resolute Bay in early 1964, the sudden cancellation of another project at the Department of Mines and Technical Surveys had created the opportunity for DRAO director Jack Locke to fund the design and construction of a second array at White Lake, operating at 10 MHz. This array was to be a simpler, less expensive instrument than the 22-MHz one, and with a shorter anticipated life. The budgetary "windfall" had come without the funding needed to operate the new array, but Locke and Ryle had agreed that the DRAO could fast track the building of the array if the Cambridge group could supply some back-end

instrumentation and - more importantly - manpower to operate the array in the form of graduate students; the rest of the DRAO team was already over-committed with bringing the first array into operation, and could offer little more than advice and design ideas. John Galt would oversee the project at the DRAO, while Scheuer and Purton would provide the initial contribution from Cambridge, funded by a "start-up" grant from the Royal Society."

Then continue with your text "as is" until the detail about the commercial baluns. As I mention in my history, the situation with those baluns was much worse than your account now describes. Not only were many of the joints poorly soldered, but the phase and attenuation characteristics of the baluns were poorly standardized, so that the "rookery" would have been a dysfunctional mess without the insertion of individually customized phase and attenuation corrections specific to each balun. Just redoing the soldered joints would have been relatively easy, but each balun had to be individually measured and a compensating circuit then had to be added in front of it. This was a huge job, and was mostly performed by Scheuer in the downstairs electronics lab at White Lake, where I am told he literally worked day and night to complete the mind-numbing task. (A small subset of the baluns had been delivered to the Cavendish, where I recall Purton doing the same task on a smaller scale on a workbench in the Mullard group's attic before he left for the DRAO.) To many who now think of Peter Scheuer only as a theoretician, the level of his practical involvement in the 10-MHz project often comes as a surprise. So I think it will be good to spell out the whole messy detail on these baluns, by merging some of my text with yours as follows:

"An unexpected complication in the building of the Rookery arose when it was found that many of the soldered joints within the 260 commercially-supplied hybrid networks were defective and had to be re-soldered. To make matters worse, the attenuation and phase shifts associated with the individual transformers had unacceptably large variance, so the characteristics of every transformer had to be measured and individualized attenuators and phase shift networks put in place before each transformer. This lengthy and tedious task was carried out by Scheuer at the DRAO and on a smaller scale by Purton at the Cavendish Laboratory in the summer of 1964."

Then it's fine until the part in 7.3.4 about refraction, which I feel is a little misleading, in the sense that MOST of the source transits at 10 MHz had to be, and were, corrected for refraction. (I think what is said is much more appropriate for 22 MHz than for my experience at 10 MHz). The E-W refraction at 10 MHz was seen mainly within an hour or so of sunrise or sunset, and indeed affects only the apparent transit time – except under extraordinary conditions (I once saw Virgo A transit the array meridian three times in the same night, each time with a "beamshape" about half the width of the actual array beam due to to slow scintillation on top of the refraction). The N-S refraction was more significant as it could only be allowed for by recording the apparent amplitude of the sources in adjacent 10-MHz phasings. It was rarely less than 30 arc min and occasionally exceeded 2 degrees. It was ALWAYS corrected for, either directly from the adjacent-beam observations, or by using a value interpolated from observations of nearby brighter sources for which the correction could be determined more accurately. I suggest replacing the current paragraph with the following:

"Refraction of point sources caused by ionospheric electron density gradients could amount to a significant fraction of the 10-MHz beam. The E–W component would show up through the time of transit of a known source and was greatest near sunrise or sunset. In principle, having contiguous multiple beams in declination enabled the detection of any N–S component of refraction for bright sources, but this procedure became inaccurate for observations with low signal-to-noise. All 10-MHz source observations were corrected for N-S refraction, roughly half using individualized corrections and half with corrections interpolated from observations of

nearby, brighter, sources. The corrections were rarely less than 30 arc min and some were as large as several degrees."

In Section 7.3.5, where it talks about the absolute measurements of Cas A and Cyg A, I am not aware that any absolute measurements by Chris Purton went into the calibration of the flux density scale in the paper by Bridle and Purton. (I am not sure how Chris got the absolute scale for his thesis work, and the one 10-MHz flux density that he published separately from our compendium - for NGC1275 – was based on a scale he got by extrapolating Virgo A's higher-frequency spectrum to 10 MHz) He may have independently measured Cas, but the numbers that went into our joint paper were those from the interferometer data that I published in The Observatory. Chris had made a very important contribution to those measurements in the form of the noise diode standard that was used to calibrate the power level, but if he did make his own independent estimate of Cas A, I don't think it was ever published separately. I suggest in place of your text saying

"Both Purton and Bridle attempted absolute measurements of the 10-MHz flux densities of the two strong sources, Cas A and Cyg A, to establish the gain of the array."

it would be much more correct to say:

"Bridle (1967) made absolute measurements of the 10-MHz flux densities of the two strong sources, Cas A and Cyg A using a small interferometer whose gain could be calculated from theory and a coaxial noise diode standard constructed by Purton to calibrate the received power level. The measurement of Cas A was used to establish the gain of the 10-MHz array near its declination, and from there to bootstrap a more general calibration based on extrapolating to 10 MHz the spectra of sources whose higher-frequency power laws were well determined. After considerable effort of hand-scaling chart recordings and selecting the best-quality meridian scans for averaging, Bridle and Purton (1968) compiled a list of the flux densities for 124 sources observed during the two seasons."

(The second half of that para, which is still much as you wrote it, is in my opinion a satisfactory simplification of the process that we actually went through, which I don't think the historically-oriented reader really needs to worry about! The added reference to Bridle (1967) is **Bridle, A.H., "Flux densities of Cassiopeia A and Cygnus A at 10.05 MHz", The Observatory, vol. 87, pp. 60-63 (1967).**

Further down in 7.3.5, in the first full paragraph on p.132, the sentence starting "While not perhaps appreciated at the time" is jarring to my ears because, as I explained in my earlier email to Elizabeth and in my "personal history" of the project, the search for synchrotron self-absorption in compact emission from quasars had been a prime motivator for the whole project in Peter Scheuer's view. The fact that there were easier and more efficient ways to identify new quasars was not apparent in the early days. If there is something that was not appreciated at the time, I would say it was the structural complexity of the typical powerful extragalactic source - which we now know can have compact components both in the AGN and in the hot spots of the extended radio lobes, so we can end up seeing slight changes in the total spectral index with observing frequency, rather than the complete "turnover" spectra that Peter had hoped to find new examples of. I think you should definitely drop the "while not perhaps appreciated" clause and (perhaps_ elaborate the rest of the sentence to say

"This may indicate that parts of the emitting regions of quasars are small in extent, and that the most compact emission components seen at higher frequencies are prone to synchrotron self-absorption at low frequencies."

This section may also be the best place to add a mention of what I feel is a unique way that the 10-MHz array added to our knowledge of the interstellar medium of our galaxy – the fact that we saw evidence for 10-MHz absorption in the spectra of almost all the extragalactic sources at LOW galactic latitudes. This was not surprising a priori, as the 10-MHz array was the most sensitive machine for detecting ionized hydrogen at low emission measures that had ever been built (which was something that Carman Costain emphasized in just about every discussion we ever had about looking for galactic absorption effects). I believe one of the most interesting results along those lines sprang from a DRAO lunch-room discussion that involved Bill Shuter, Rob Roger, Venugopal, Ed Argyle and me about things we could use (the then newly-discovered) pulsars for, especially those pulsars whose distances were known from 21-cm absorption studies. As I recall it, Venu and I went almost directly from the lunch room to the DRAO library and quickly sketched out the first draft of the paper that we published as an article in Nature, combining my 10-MHz low-latitude absorption estimates with data on pulsar dispersions and hydrogen column densities (which were what Venu's thesis was about, supervised by Bill Shuter) to support a "thick disk" model of the dilute electron gas towards the Anticenter. That model, although simplistic by today's standards, was at the time a premonition of the "thick disk" model that has persisted to this day. I think this was a good example of the unique leverage that working as low as 10 MHz gave on this kind of problem, and also a great example of how DRAO served as a meeting-point for people from different ends of the Canadian radio astronomy community in the late 1960's. There was probably no other place where a young assistant professor from Queen's would have formed an ad hoc collaboration with an Indian graduate student working at U.B.C. in such a stimulating environment. So I'd like to suggest that you add something to your text that alludes to that aspect of the 10-MHz source data, perhaps along the lines of the following (as the penultimate para in 7.3.5)

"Another correlation evident from the 10-MHz discrete source data was that extragalactic sources at low galactic latitudes had systematically lower-than-expected flux densities relative to their high-frequency power laws. Expressing the flux density deficiencies as an optical depth at 10 MHz, Bridle (1969) found that the data were consistent with a disk-like distribution of the absorbing medium in the local spiral arm. A vigorous discussion in the DRAO lunch room between Rob Roger, Ed Argyle, Alan Bridle and V.R.Venugopal (then a graduate student working with Bill Shuter at U.B.C. on a neutral hydrogen survey of the galaxy with the DRAO 25-m telescope) then led to a paper by Bridle and Venugopal (1969) which combined the low-latitude 10-MHz source data with 21cm data on the neutral hydrogen distribution and the dispersion measures of pulsars with measured distances to suggest a "thick disk" distribution for diffuse ionized gas in the Milky Way. This work, which supported a much thicker (equivalent thickness 600 pc) for the electron gas than for the neutral hydrogen, exploited the unique sensitivity of the 10-MHz array to the presence of ionized gas with very low emission measure. It also illustrated the special role played by the DRAO in those early days as a focal point for radio astronomers from across the country."

References added:

Bridle, A.H., "10 MHz absorption in the local spiral arm", Nature, vol. 221, pp 648- 649 (1969).
Bridle, A.H. and Venugopal, V.R., "Distribution and Temperature of Interstellar Electron Gas", Nature, vo. 224, pp.544-547 (1969).

I do want to commend you for doing an excellent job on pulling this together, and I very much look forward to seeing the whole book. Please let me know if you want any of my photos of the 10 MHz array to add to this section, and I will provide a separate JPG file for that purpose.

I will just add one further item about Peter Scheuer, who I feel was not fully appreciated by many of his peers. One of Peter's endearing characteristics was his sense of humor, and all who worked with or around him in the Cavendish were familiar with how the corridor would ring with his unique laughter. The rubber spider that he installed in the 10-MHz Rookery was a memorable example of his mischievous side that not everyone in the astronomy community got to see. For that reason alone I would appreciate it if you could find room to include it near the description of the 10-MHz Rookery - the photo was taken by John Galt but it is misidentified in the DRAO photo archive as being the 22-MHz phasing network. For some reason Peter named the spider "Olivia", but we never knew why. The original is in the custody of Chris Purton to this day. The "low frequency gang" were all very fond of Peter and the way he threw himself into this project, first at Resolute and then at DRAO, was quite inspirational.

From: "Alan Bridle" <abridle@nrao.edu>
Subject: Re: The DRAO 10-MHz telescope {External}
Date: Thu, November 24, 2022 2:52 am
To: "Elizabeth Griffin" <r.elizabeth.griffin@gmail.com>

Dear Elizabeth

I am very sorry to hear that Jasper is again unwell and hope that he will be feeling better before long. Please pass on my best wishes to him.

I have read the chapter extract quickly, and while it is a good account of the 10-MHz project generally, as expected from Rob's knowledge of it at the time, there are some aspects that I feel are a little misleading and a couple of statements that are not correct. I will send you my suggestions and comments as soon as possible.

I understand that at this stage it would not be possible for your account of the project to go into as much detail as mine, but for the most part the length of mine comes from personal details I am describing that would clearly be out of place (or proportion) in your more general history. So I will keep my comments and suggestions short. As for the photos, I do identify in my own document which of the pictures of the array are from official photos from the Dominion Observatory archives and which are mine - basically most of the black and white photos are official photos for which a permission would be needed for publication (I am sure that could readily be obtained) while all of the color photos are mine (scanned from 35mm slides). You will be welcome to include any of mine without further ado, and I will be happy to send you an individual JPG.

I will write my full comments and suggestions separately as that will be easier to do off-line than in this e-mail window, and I will try to get them to you very soon - tomorrow is Thanksgiving Day here in the U.S. so I may not be able to finish until Friday, but I will do my best to give you a quick turn-around on this.

The main points about the science that stood out to me were

(a) I do not think it is right to imply that we did not think of synchrotron self-absorption in the compact components as a reason for spectral flattening in the extragalactic sources at low frequencies: indeed, as I spell out in my account, I saw a major impetus for the 10-MHz project coming from Peter Scheuer's idea to use low-frequency spectra as a way to identify quasars (at a time before identifying them by direct optical identifications using accurate radio positions "took off"). In fact synchrotron self-absorption was always thought to be the prime candidate for producing spectral flattening in quasars, although Peter had studied all of the alternatives theoretically and he hoped that with good enough measurements we might be able to distinguish them via the spectral shapes. In hindsight that was too optimistic - given the uncertainties in the measurements from all of the ionospheric effects and also given what we now know about the structural complexity of the sources. But very early on in the project there was thinking in the Cambridge group of the 10-MHz array as a potential "synchrotron self-absorption detector" - which is why I found it a little ironic that in the end we only found ONE clear new example of what Peter was looking for. Obviously the project was well worth doing for many other reasons, and it turned out to be a good example of the principle that if you open the astronomical observing window wider, you will find new phenomena that you did not anticipate, e.g. the steep-spectrum sources in the rich clusters.

(b) the list of "interesting results" from the array omits the finding of systematic 10-MHz absorption in the spectra of LOW-latitude extragalactic sources, which we showed was consistent with a thick-disk model for the ionized medium in the Anticenter. At the time that Venugopal and I wrote our Nature paper this was a new result in a part of parameter space that could only be explored at the lowest frequencies, where we had unique sensitivity to the most diffuse ionization. The "thick disk" and its ionization have since been studied by other means, and the details are - not unexpectedly - more complicated than the simple model that came out of the DRAO work, but the 10-MHz array did open the door to that as a topic for further investigation. So I believe that topic deserves a mention to complete the overall picture of what the project produced.

I will send you some detailed comments and suggestions a.s.a.p., and thank you for the opportunity to do so. I am very glad that you and Jasper took on the huge task of putting the book together, and I am sure it will be much appreciated by all Canadian radio astronomers!

With best regards,

Alan Bridle

On Wed, November 23, 2022 8:06 pm, Elizabeth Griffin wrote:
> Dear Alan,
>
> I have discussed this matter with Jasper (currently rather unwell,
> unfortunately), and am sending you herewith what the MS of the Book
> currently says about the 10-MHz instrument. The text is based upon a
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> paragraphs, up to (say) 2 or 3 pages, could be acceptable, and maybe
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> existence of your report when it has a home in an archive.
>
> We would very much value your help, and hope that our modification of
> Rob R's report is not too poor!
>
> With all good wishes,
>
> Elizabeth Griffin
>

From: "Elizabeth Griffin" <r.elizabeth.griffin@gmail.com>
Subject: The DRAO 10-MHz telescope {External}
Date: Wed, November 23, 2022 8:06 pm
To: abridle@nrao.edu

Dear Alan,

I have discussed this matter with Jasper (currently rather unwell, unfortunately), and am sending you herewith what the MS of the Book currently says about the 10-MHz instrument. The text is based upon a document written by Rob Roger and sent to Jasper several years ago. We wove it into our DRAO chapter, from where I have lifted the relevant pages. There is one brief comment at the veugh your lry end of the chapter that refers to the sorry state of the wooden poles, which were never creosoted (as were those for the 22-MHz instrument) and rotted after about 10 years in the ground unprotected.

If you have time and inclination, please could you look through these pages and see if there are glaring omissions or serious errors that must be added or corrected? We cannot really make the book very much longer than its present length; your own, much longer, account is enviably detailed. but it would risk overbalancing other chapters if we include too much on this one instrument. A few additional paragraphs, up to (say) 2 or 3 pages, could be acceptable, and maybe we could think about incorporating one of your excellent photos (I am assuming they are your own). Otherwise, we could refer to the existence of your report when it has a home in an archive.

We would very much value your help, and hope that our modification of Rob R's report is not too poor!

With all good wishes,

Elizabeth Griffin

Attachments:

ch7.pdf	
Size:	777 k
Type:	application/pdf

From: "Elizabeth Griffin" <r.elizabeth.griffin@gmail.com>
Subject: Fwd: Fw: Carman Costain memoir and other documents] {External}
Date: Sun, November 20, 2022 5:03 pm
To: abridle@nrao.edu

Dear Dr Bridle,

I am so sorry you have had to wait this long for a reply to your very interesting email. I have been on an extended visit (7 weeks) to the UK for personal reasons, and was unable to access my Canadian NRC email account during that time (a new policy of the NRC, in whose building I work as a visitor). I returned home only 2 days ago, and am still ploughing through the email backlog. I hope you had not given me up for lost.

I believe we are in fact exact contemporaries. We met on one occasion at some sort of reception outdoors in Whewells Court of Trinity College; I was present (in those days I used my first name, "Rita") as the then girl-friend of Russell Cannon, and you had your girl-friend (was she also "Rita"?) as your guest. The tripos results had just been released and you and she were spinning another to celebrate high grades. Whenever I see mention of your name in print, I recall that scene!

The History of "Canadian Radio Astronomy" is actually nearly ready to be submitted to Springer, our publisher, but details of the 10 MHz instrument (which I believe was a sort-of combined experiment between the DRAO and Mullard, at the instigation of Ryle and Scheuer) may well be important to bolster what we already have. We should certainly add your obit of Carman Costain to the literature list, and cite it for reference because every obit is different inasmuch as it comes from a different, often personal, angle. There is a published one (I think it was written by Jack Locke), but yours does have quite a fresh slant.

I need to discuss your suggestion of details for the 10-MHz telescope with Jasper Wall, and will do that pdq. Again, I do apologize for this very late response.

With best wishes,

Elizabeth Griffin (formerly Rita Gasson)

On Sun, 20 Nov 2022 at 08:38, Griffin, Elizabeth <Elizabeth.Griffin@nrc-cnrc.gc.ca> wrote:

>
>
> _____
> From: Alan Bridle <abridle@nrao.edu>
> Sent: October 29, 2022 1:48 PM
> To: Griffin, Elizabeth
> Subject: Re: Carman Costain memoir and other documents]
>
> ***Attention*** This email originated from outside of the NRC. ***Attention*** Ce courriel provient de l'extérieur du CNRC.
>
> Dear Dr Griffin
>
> I am writing to you in your capacity as chair of the CASCA Heritage Committee.
> I am a retired radio astronomer and an almost-contemporary of yours from
> Cambridge, a charter member of CASCA, and I did part of my Ph.D. thesis work
> at the DRAO in Penticton, where Carman Costain was my local Ph.D. supervisor
> for a year.
>
> I have written the attached biographical memoir of Carman, who at one point in
> his career was President of CASCA. I would like to donate this to the CASCA
> historical archives in whatever format you think would be most appropriate.
> If you think it might be of sufficient interest to the current CASCA
> membership, perhaps it could be passed on to the current editor(s) od
> Cassiopeia as well.
>
> I have some other materials that may be of interest to you in connection with
> the book on the history of Canadian radio history that I understand you are
> writing with Jasper Wall.
>
> One of these is a personal history of the 10-MHz T array project at the DRAO,
> with which I was involved as graduate student starting in 1965. This array
> was, at the time, the lowest-frequency survey instrument in the world, but it
> was demolished in the early 1970's owing to its having become a hazard to
> navigation on the public road passing near the DRAO at White Lake. I gather
> that its role in the development of low-frequency radio science at the DRAO is
> now pretty much unknown to the current generation of radio astronomers, but as
> low-frequency radio astronomy is experiencing something of a renaissance at
> present this may be a good time to remember it. If you or Jasper Wall would
> be interested in seeing this history, or in adding it to the CASCA historical

> archive, I will be happy to send you a copy.
>
> Another is a biography of Dr Barry Turner, who was a Canadian-born and trained
> radio astronomer who was one of the pioneers of the field of astrochemistry of
> the interstellar medium using radio spectroscopy. Although Barry spent most
> of his professional career at the NRAO and got his Ph.D. from Harold Weaver's
> group at Berkeley, his earlier education was in Victoria and at U.B.C. and his
> first contact with radio astronomy was working under Tom Legg in Ottawa in
> what was then the Radio and Electrical Engineering Division of the NRC. I
> therefore believe it may be quite appropriate for this biography to be added
> to the CASCA historical archive as well as to the NRAO's, and I will send you
> a copy if you are interested in having it.
>
> With best wishes,
>
> Alan Bridle
>
>
>
