

From: "Tim Robishaw" <tim.robishaw@gmail.com>
Subject: Re: DRAO History
Date: Thu, December 17, 2020 8:14 pm
To: "Alan Bridle" <abridle@nrao.edu>

Hello Alan!!!!

Thanks so much for this history! I'm looking forward to reading it after all my meetings this afternoon.

No worries at all on the delay---I was just very worried that my emails were ending up in the spam folder... I've had reports from other folks that I've been reaching out to about DRAO history that my emails have been flagged.

So glad to make contact again. More shortly. Best Wishes -Tim.

On Wed, Dec 16, 2020 at 3:17 PM Alan Bridle <abridle@nrao.edu> wrote:

Hello Tim

I am sorry to be slow responding, but my NRAO email box is somewhat flooded at the moment as I have been spending more time recently keeping in touch with family than I have with my astronomical mail. Mary and I are thoroughly locked down at home at the moment, so almost all of our contact with the outside world is by email and video which is wonderful to be able to do but also quite time consuming, and I have not been into my NRAO mailbox as often as usual.

To answer your questions ... my official Ph.D. supervisor at the Cavendish Lab was Tony Hewish. However that was an assignment made before I actually started work there, at a time when Martin Ryle thought I would be working on the 4C survey. What actually happened was that on the first day I showed up at the lab as a graduate student I was asked to help Bryan Andrew and Chris Purton start construction of the 13 MHz array at Lords Bridge that Bryan eventually used for his Ph.D. thesis. That was strictly manual labor, bending copper wire into shape for the wide dipole elements, hanging them on Dexion structures, pounding angle iron into the ground for guy wires, making resin pits for baluns, etc etc. It was meant to be a few-day assignment but while I was doing it Martin came up with the idea for redoing Carman Costain's estimate of the extragalactic radio background (Olbers radiation) at higher angular resolution than had been possible in Carman's thesis. As I was now "highly experienced" in low frequency array construction after a couple of weeks of bending wire and pounding angle iron with Bryan and Chris I was diverted to the old Rifle Range site off Grange Road and tasked with going the low frequency scaled array experiment that became the first part of my thesis, whose goal at the time was solely to confirm and improve on Carman's project.

That had the effect of taking me out of the 4C survey effort (which Derek Wills worked on instead) and accidentally launched my time as a low frequency specialist, hence my time at Penticton and thus incidentally my entire career in Canada.

I never really talked about my own science with Tony Hewish, although owing to a series of clerical oversights in the Cavendish Lab system he was never formally replaced as my Ph.D. supervisor of record so I am sure the Cambridge archives still list me as his student and he did actually convey my thesis to the board of studies and organized my oral exam in 1967. In practice I was supervised for the scaled array work by John Baldwin and to some extent by Peter Scheuer who was sort of the resident theoretician and also had particular interest in what could be done at the extreme low frequency end of the spectrum.

It was Peter Scheuer who got the whole Canadian connection going for us. My recollection was that he accidentally met Tom Hartz on a flight to some conference and they talked about Tom's early results from the Alouette satellite topside sounder that were (mis)interpreted as saying that the ionospheric electron content went to near zero in the magnetic polar cap. That lead to the (retrospectively crazy) Resolute Bay episode which took Peter and then Chris Purton to Resolute in pursuit of extremely low frequency observations through an ionospheric layer that was basically at ground level and impossible to observe through at those frequencies. I had become friends with Chris as a result of the wire bending and angle iron hammering done on my first days at Lords Bridge and it was Chris and Peter who came up with the idea of me following Chris to Penticton when Chris had to return to Cambridge to write up his thesis. The point was that Chris had experienced very good observing conditions near the end of his stay in Penticton although the solar minimum was officially over, but he had not been able to get much science done

with the 10 MHz array because it was late coming online owing to construction errors made by the telephone linemen who had built it. It was a totally last minute improvised idea for me to go out to DRAO to help John Galt sort out the problems with commissioning the 10 MHz array, and the fact that I had completed the scaled array experiment inside 2 of my 2 scheduled years as a graduate student made it possible for me to do that.

Peter Scheuer applied for a Royal Society grant to help send me to DRAO, but it had not been adjudicated in time for me to leave and replace Chris. I therefore went to Canada on the strength of a personal check for travel and expenses written by Peter out of his private bank account. Fortunately the grant came through... otherwise my stay at DRAO would have been short! But the officialdom at Cambridge was not used to students disappearing to Canada without supervision, and they must have asked Martin Ryle to appoint a local supervisor for me at DRAO. As Martin and Carman were good friends he persuaded Carman to take me on for what was supposed to be just a few months but which turned into a whole year as the good observing conditions continued.

Carman and I became very close. We were both hands-on our arrays at night and spent long hours together at the observatory. Everything about observing at 10 MHz was about 4 times more difficult due to scintillations and absorption than it was at 22. If Carman was having difficulties with scintillations at 22 it was hopeless for me at 10. I typically started my night shift with Carman looking at the output of the 22 MHz system and only drove out to the 10 if conditions looked promising. We talked, and I learned, a lot of astronomy and ionospheric science in those hours. I had also been set the task of making the absolute calibration of the 10 MHz data using the two-dipole interferometer for Cas A transits and Carman came up with the computer code for calculating its gain and made sure I was doing the measurements properly. John Galt was also heavily involved but he was busy being observatory director by day and the night time sessions with Carman were where I got my truly close supervision. He was also very interested in the first part of my thesis, which had morphed into mapping the variations in the galactic spectrum as well as estimating the intensity of the extragalactic background. So although I only formally interacted with Carman for a little less than a year of my Ph.D. time, he really was the senior person who had the most interaction with me for the final year, until I returned to Cambridge and actually wrote the thesis, when John Baldwin was again my unofficial Ph.D. supervisor

Bottom line: the University archive probably says my supervisor was Tony Hewish but in the real world it was Baldwin for the scaled array work, and a mixture of Carman Costain and John Galt for the 10 MHz work at DRAO. In practice at the DRAO end John Galt supervised the commissioning of the array, finding the baluns that been installed with the wrong polarity, dipoles that has been connected backwards etc. but he was less involved than Carman was with the detailed science that I did with it.

The commissioning problems were that the construction crew were telephone linemen who had not been given quite enough instructions about how to wire the thing up and many mistakes had been made. There was a theoretical plan for calculating the gain of the array after measuring the mutual impedances of representative dipoles within it. Implementing that was physically difficult as it involved driving the cherry picker around in the array while opening and short circuiting dipoles while measuring their input impedances with a Wayne Kerr bridge. I believe that Chris Purton and John Galt had spent a lot of time on this which was part of why Chris's astronomical observations were delayed and incomplete by the time he had to return to Cambridge to write up. The additional problem was as Chris described to you, the 10 MHz dipoles were hung in the worst possible configuration for minimizing the interactions so the gain computation was very tricky.

It was during the discussions I had with Carman that we decided that as many of the discrete sources at high galactic latitudes were showing power law spectra down to 22 MHz (a preliminary result at the time but one that stood the test of time) then if I could measure enough sources at 10 MHz we could probably find a high latitude set that could be plausibly assumed to have power law spectra down to 10 MHz and that a "calibration" of the array gain on that basis would be about as reliable as any we could do from a computer calculation based on a difficult and not totally reliable set of mutual impedance measurements. I wrote my thesis on that basis and later revised the detailed calibration of the array gain as a function of zenith angle to make it consistent with the curves that Chris had computed from his measurements. The final calibration basically normalized a set of theoretical curves calculated by Chris to an envelope that was constrained by the power

law extrapolation assumption that Carman and I came up with.

I subsequently worked with Carman and Rob Roger on a re-evaluation of the whole low frequency flux density scale and one byproduct of that was to again confirm that there is a set of high galactic latitude sources that suffer no significant absorption by interstellar HII and have sensibly power law spectra and that those sources could plausibly be used as secondary calibrators for the gain of the 10 MHz array as a function of zenith angle. Yes the 22 MHz array was better understood and more amenable to modeling because of the orientation of its dipoles. And no, I was never involved in modeling the gain of the 22, that was all Carman and Rob, and the modeling of the gain of the 10 was all done by Chris. It is indeed a very hard problem and it will be interesting to see if modern phased arrays and computers can solve it convincingly. What I did for my calibration was a kludge but in retrospect it still looks like a pretty good one, especially in the context of trying to get a thesis written on time!

I think I was the only student in residence at DRAO during the winter I was there but I did meet some students from UBC who were working with Bill Shuter. My recollection is that they came for specific observing programs with the DRAO dish and then mostly went back to Vancouver. I am sure I first met Venu that way but I got to know him much better during my first year on faculty at Queen's. I spent a few weeks at DRAO in summer 1968 and then most of the summer of 1969, while I was still an assistant professor at Queen's and using my research grant to fund the stay. 1969 was also the year that Peter Dewdney lived in one campground on the East shore of Skaha Lake, while Mary and I lived in another, closer to OK Falls. Peter was on a studentship and I was trying to make my grant stretch to a whole summer away from Queen's, so we were both minimizing our expenses! I had not interacted with Venu very much about his thesis work but I think you are right in that he did spend a long time in Penticton while getting the data for the 21cm survey and I can remember meeting his wife. My strongest memory is of conversations in the DRAO lunch room in those days which were very wide ranging and spontaneous and all kinds of science got discussed by ad hoc groups over our brown bags.

My recollection is that the idea that some parameters of the interstellar medium could be extracted from pulsar and 21cm data sets came up during a lunch discussion and that Venu and I did the sums and wrote the paper very quickly in the DRAO library ... in a matter of only days. I was quite surprised when its inference of large scale height for the ionized gas disk attracted as much attention as it did. We probably know too much to make so many simplifying assumptions today! I remember Venu as a very intense but rather shy person. He probably needed to be intense to work alongside Bill Shuter who was very tightly wound and also a prodigious source of new ideas. Pulsars were new and we were all talking about them a lot and thinking about ways to use them. I did not get to know Venu personally very well and I rather lost contact with him after he returned to India. I recall Govind Swarup telling me that Venu had played a very important role in running radio facilities in India. The time we spent putting the Nature paper together was our main overlap.

The student I came to know best in 1965 was Rick Choquette, a summer student from Calgary. He had a big truck and took me all around the mountains that summer. His father was a retired petroleum geologist who owned a gorgeous ranch in the Kootenays near Nelson and I learned a lot about the geography of BC and Alberta from trips taken with him. I think he was working on electronics projects so possibly some of the engineers will remember him.

The everyday lunch room discussions at the DRAO were always something I looked forward to and they had a big influence on me as a young researcher. Students and experienced researchers from DRAO and elsewhere were exchanging ideas and asking questions very freely and informally about whatever came into our heads. I particularly remember Ed Argyle getting into some very stimulating and occasionally heated discussions. He has an interest and opinion about almost every topic that came up, whether scientific or political. Coming from the Cambridge group which was famously secretive about its current projects, especially with outsiders, I found the open-ness of the discussions between staff and students and visitors at DRAO very stimulating and felt very privileged to have experienced it while a student. Being an embedded astronomer in the Physics Department at Queen's was a very different experience, and a somewhat lonely one scientifically much of the time, so I look back on the summer of 1969 as one of the most formative periods of my own career.

However, with Green Bank being a (long) day's drive from Kingston and a

gateway to the world wide radio astronomy community at that time, I was increasingly drawn there instead and then back into interferometry and aperture synthesis.

I had known Jim Caswell as a student in Cambridge and I also had some ongoing interactions with him while he was at the DRAO, especially about things we could do with the DRAO 10 MHz data, and the growing discrepancies between the DRAO radio source measurements and those from the Ukrainian array.

I was never involved directly with the 22 MHz array, although I saw a lot of its early results in analog form looking at the chart records with Carman ... so Peter D's recollection of my involvement is the one that's a little off ... but there was a period when Rob Roger and I worked a lot together on the source spectra obtained from both arrays and there was a paper on flux density scales and low frequency spectral classification that was written by Rob, me and Carman partly at a distance and partly during one of my later summer stays at the observatory. I recall we had the temerity to suggest a revision to the Cambridge flux density scale at 178 MHz as a result of that which we were rather nervous about but in the end they came to the same conclusion themselves so our scale became the generally accepted one, at least for a while.

Carman remained my closest DRAO contact after that summer and it was a great shock to me when he died so suddenly. We had stumbled together upon some of the steep spectrum sources in clusters of galaxies that have since been identified as "fossil relics" but of course we lacked the angular resolution to be sure what they were. One of many explanations we thought about had for them is pretty much the current one, but it has taken multifrequency imaging and polarimetry to show the regions we wondered about clearly for what they are.

I do have some pictures scanned from 35mm slides that I took in the 1960's and I will send them along to you as soon as I can.

Chris P's recollection about the 10 MHz array matches mine. He had done a set of mutual impedance measurements that allowed the beamshape to be computed in principle. But I don't think he had the resources to do the calculations in Cambridge and he did them during the time he was at NRC in Ottawa before he went to York U. We had some get togethers while I was newly at Queen's and he was in Ottawa to see if we could harmonize his detailed computations about the array with the empirical way I had calibrated it in my thesis. I am pretty sure we agreed in the end that the detailed calculation could not be done convincingly with the data we had in hand so our best shot was a hybrid of what he had calculated for the gain curves normalized to match what was in my thesis on the average. It's a problem the low frequency SKA will have to get right with much better tools and measurement than we had in the late 1960's and I wish them luck with it!

Peter Scheuer's time in Canada would have been some mixture of 1963 and 1964. At that time he was a junior staff member at the Cavendish and a lecturer at Cambridge and was getting some Royal Society money for his travel to Canada on the various low-frequency adventures. He may also have had some money for that from Ryle. I know he lived for some months in the Apple Grove Motel on the south side of Penticton because that was where I lived in the winter of 1965 and the proprietors had fond memories of him.

It was his idea to use a Butler matrix for beam forming in the 10 MHz array and I believe he did a lot of the assembly of the physical matrix while at the DRAO. Chris P had built some of the components in the Cavendish and shipped them out to him but John Galt was probably who he interacted with most during the construction of the back end. Being around Peter Scheuer was like drinking from a firehose of ideas in astrophysics and cosmology and I imagine the lunch room conversations during his time would have been pretty exciting. Peter was primarily a theorist not an experimentalist but he was willing to get his hands dirty, cut and put plugs on phasing cables or wield a soldering iron all night when needed. I remember Martin Ryle saying once "I wish it was possible to ask Peter a question without getting all of human knowledge on the topic for an answer" I got the impression the people who saw him in action at DRAO were pretty fond of him, and he is one of those people whose reputation is much higher among those who have had direct contact with him than anyone one would guess from counting publications.

I'll sort out some pictures to send and if you have any more questions please feel free to ask them.

Best regards,

Alan Bridle

On Thu, December 3, 2020 7:31 am, Robishaw, Tim wrote:

> Hello Alan!

>

> I hope you've been well! (Sorry if you receive this email twice... I tried
> sending from my GMail account, but others have reported my emails were ending
> up in their spam folder, so thought I'd try to send this from my NRC email
> address as well.)

>

> I'm drafting up a brief history of DRAO and wondered if I could ask you some
> questions?

>

> (1) We have a copy of your PhD thesis here in the DRAO library. I wasn't able
> to suss out who your PhD supervisor at Cambridge was from this document. I
> know you've told me that you were in Ryle's group, but it's not always clear
> to me whether Ryle was the supervisor of everyone who was in his group---it
> seems like a number of folks who I thought worked in Ryle's group were
> actually supervised by Baldwin (Purton), Graham Smith (Costain), or Paul Scott
> (Gower), but maybe I'm misunderstanding who was actually working in Ryle's
> group. I've confirmed with Ann (Neville) Gower that Martin was her advisor.

>

> Back in 2015 you had stated, "Cambridge insisted that I needed a Ph.D.
> supervisor on the spot to be at DRAO that long and as Carman had a Cambridge
> Ph.D. he was asked to take responsibility for advising me even though John
> Galt was the scientist in charge of the 10 MHz project." It wasn't clear to
> me if Carman was your formal advisor for the entirety of your PhD or only for
> the year that you were at DRAO, so wondered if you had a formal faculty
> advisor at Cambridge?

>

> (2) I have your time at DRAO from Sep 1965 to Aug 1966. I was wondering if
> you overlapped during this time with any other students, postdocs, or visiting
> researchers at DRAO? Do you recall any DRAO staff at that time other than
> John Galt, Ed Argyle, Carman Costain, Rob Roger, and Dave Lacey?

>

> (3) I see that you published a paper with Venu in 1969 while you were at
> Queen's. I was wondering if you can provide any memories of Venu? I've
> reached out to his family and sadly found that he passed away in 2016. It
> looks like Venu was a student of Shuter's, and his family says that they spent
> most of his UBC grad studies living in Penticton.

>

> (4) This is a real stretch, but I was wondering if you happened to have any
> photos or slides from your time at DRAO?

>

> (5) You came up in some reminiscences recently and can probably clear up some
> confusion below:

>

>>From Peter Dewdney:

> I spent quite a lot of time when I first arrived at DRAO with one of the
> people mentioned, Alan Bridle. Alan and Mary were there almost day and night.
> I learned a lot from Alan. As I remember, he was mostly engaged in trying to
> figure out the detailed beamshape of the 22 MHz array, taking into account
> interactions between dipoles (what are now called ?embedded element patterns?
> in current SKA parlance). I don't think he ever succeeded but I am not sure.
> Certainly this is still considered one of the ?hard problems? for the
> low-frequency SKA array currently.

>

> A response from Chris Purton:

> Re: Peter's comments on Alan Bridle, here's my take on his time at
> DRAO --

> To put it in context, Alan was one of a series of Cambridge people
> who spent time at DRAO on the 10 MHz project. In chronological
> order they were Peter Scheuer, myself, Alan Bridle, and Jim Caswell. Peter
> Scheuer was a theoretically-minded Cambridge radio astronomer who got
> involved hands-on in interesting projects. He went to Resolute Bay in the
> Canadian Arctic summer '63 to install a couple of antennas that I was to use
> the following winter (site-testing for a low-frequency array ? there is a
> picture of me and the receiving equipment in the last chapter of my thesis,
> which may be the picture Peter D. was referring to), and subsequently worked
> with John Galt designing the 10 MHz array. At some point in '64 he relocated
> to Penticton to help bring the array to completion, I'm not sure exactly when
> but he was already here when I arrived in September with my wife Sharon and a
> 3-week old baby girl who had been born halfway between Cambridge and
> Penticton as part of our relocation, and who, many years later, was hired as
> a summer student at DRAO. I joined in the task of getting the array finished
> and operating at which point Peter Scheuer left and I spent what was left of
> the winter observing with it. Alan was sent out to observe through the
> following winter, and Jim the winter after that.

> I was surprised to learn that Alan was involved with the 22 ?
> thought he was strictly 10. Preliminary work on the effect of
> mutual interactions between dipoles in the 10 MHz array is in my
> thesis with an analysis based on measurements available in the
> literature of mutual interactions between thin dipoles and it had
> been left to Alan to measure the interactions between folded
> dipoles while he was at DRAO, and to refine the analysis. However
> I don't know what he actually did . . . in the Bridle & Purton
> paper on source flux densities there is brief mention of the
> problem + a terse comment that the effects ?can be evaluated
> precisely from a knowledge of the properties of the antenna and
> feeder system?, and that's all. I also recall (warily !) that this
> was a 10 MHz problem because the dipoles were aligned north-south,
> but not a 22 MHz problem because those dipoles were aligned
> east-west, but I may well be right out to lunch on that one.

>
> I wasn't aware of Peter Scheuer's time at DRAO. Do you happen to recall who
> his PhD advisor was at Cambridge? Chris is estimating Peter's time at DRAO as
> beginning in '64; it's not so clear from Chris's description when Peter left.
> I can reach out to Chris again for an estimate.

>
> (6) Looking back at our previous correspondence, it looks like we were never
> able to figure out how to successfully share those Ryle Super-8 films. I
> spoke with Alison Ryle recently and she was happy to let me share the videos
> with you, so here they are:

> <https://youtu.be/UZhePuttdmw>

> <https://youtu.be/fhbv4lCi7jA>

> I also found this neat short with Martin and Ann:

> <https://www.youtube.com/watch?v=A1kSDnjF1DQ>

> (7) I recently found a bunch of old VHS tapes in the DRAO library and
> digitized them; in case you're interested in these:

> https://www.youtube.com/playlist?list=PLbvmO2TFFPk-h_XzevRDht_ndq_xcJ63a

> Time has flown since we were last in touch and I don't think that I ended up
> sending you all the videos from our 2016 Canadian historyfest:

> <https://www.youtube.com/playlist?list=PLbvmO2TFFPk-EhU1Pcysw71LT9TO7pNva>

>
> Sorry this has turned into such a big email!!!

> Best Wishes -Tim.

> =====

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Attachments:

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Size: 28 k

Type: text/plain
