COLLECTION OF MEMO'S, NOTES , AND COMMENTS

on

A NEW LARGE STEERABLE RADIO TELESCOPE (NLSRT)*

George Seielstad and I have collected together the various comments know to us which have been prepared over the past few weeks. George will also be distributing in a separate package material which he has assembled from the Green Bank meeting.

KIK/Dec 8

*a.k.a. VLD, BRD, LSD, Big Bird

$ type 3.txt;1
From: CVAX::GATEWAY::"HEILES%BKYIM.HEPNET@LBL.BITNET" 22-NOV-1988 22:23
To: GSEIELST AT NRAO
Subj:

Date sent: Tue, 22 Nov 88 19:11:31 PST
Received: from LBL.Gov by Csa2.LBL.Gov with DECNET ;
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Tue, 22 Nov 88 19:11:31 PST
Message-Id: <881122191131.20809031@LBL.Gov>
To: gseielst@nrao.bitnet
X-ST-Vmsmail-To: LBL::"gseielst@nrao.bitnet"

Dear George,

I think that the idea of a 300-foot replacement is exciting. However, I also think that the astronomical community needs to be convinced--even if, as I presume, the money is "pork barrel" and won't come out of anybody's scientific hide. I would prefer to see a document that presents the scientific rationale in the broadest possible perspective so that any physical scientist, in any field but especially an astronomer working primarily in theory or observational areas other than radio astronomy, can appreciate the signficant contributions such a telescope can make to the broad field of astronomy.

I had a conversation with Jay Lockman tonight and he gave me the list of the people you have invited to the Green Bank meeting. This list consists entirely of radio astronomers. What will surely come out of such a group is a document emphasizing the interests of the group, which will be oriented specifically toward radio astronomy issues. This will not be a balanced document as I described above.

Production of a balanced document requires the participation of a broader segment of the community. For example, you have many pulsar observers but no pulsar theorists or X-ray binary types. You have many VLBI observers, but no optical astronomers who work on quasars or theorists. You have molecular line observers, extragalactic redshift machine builders, but no corresponding optical or theoretical types. You have no gamma ray types, who are interested in overall aspects of pulsars and their relation to what they see with their expensive satellite projects.

In short, you've invited just about every eminent radio astronomer, thus ensuring your broad-based support in radio astronomy (which I'm sure you'd have gotten anyway), but nobody else, thus taking the very risky chance that you won't extend the support beyond the radio community.

I believe that the other segments of the community must be involved from the very beginning. Otherwise, these segments will hear about this proposal by the grapevine. They will wonder where the money is coming from, they will complain that yet another major radio initiative is being launched, they will wonder why the expenditure of large amounts of money is being envisioned on what is, I suspect, perceived now as old-fashioned observational tools and techniques.

They will see the radio astronomers as performing an end run around traditional funding methods without consulting their colleagues who specialize in other subdisciplines.

Even if the money is truly "additional", we need the support of the entire community. Otherwise we run two risks: one, we won't have the broad-based support for this particular project, and without it we run the risk of not being successful; two, we run the risk of fragmenting the community, which makes more difficult the uniting of the entire community in support of future projects.

Yours, Carl Heiles

From: CVAX::BTURNER 25-NOV-1988 15:43
To: PVANDENB,RBROWN,GSEIELST,KKELLERM,JLOCKMAN,DHOGG,MROBERTS,BTURNER
Subj:

Memo to: Paul VandenBout
From : B.E. Turner
Subject: What New Telescope for GreenBank?

WHAT SPECTRAL REGIONS BETWEEN 0 AND 115 GHZ ARE SCIENTIFICALLY IMPORTANT?

The spectral region between 0 and 115 GHz breaks up fairly conveniently into 5 regions so far as science is concerned. I briefly describe that science, then rank the importance of each region on a scale of 1 to 10, using ALL of the scale. "1" is most important, "10" least. I largely omit continuum work; others will hopefully address it.

1) 0 to 1.8 GHz:
Pulsars and 21 cm work. Hardly needs elaboration. (OH is included by going to 1.8 GHz). Requires the largest aperture possible, which is compromised by going to higher frequency. Rank = 1.

2) 1.8 to 5 GHz
Contains CH (3.3 GHz) and H_2CO (4.8 GHz). 100-meter aperture would be nice. Anything smaller means Bonn does it better. Rank = 5. Higher rank if continuum has strong need.

3) 5 to 25 GHz
primarily important for NH_3 (23.8 GHz and lower) and H_2O (22.2 GHz), but C_3H_2 (18.2 and 21.5 GHz) and a few other molecules are of interest also, which cannot be studied with the VLA. The VLA is (and will be) incapable of studying NH_3 in its low-brightness, extended-emission form which characterizes its most useful diagnostic capabilities in dark clouds. NH_3 is one of the 5 most important diagnostic molecules. Rank = 4.

4) 25 to 50 GHz
The only spectroscopic item of significant interest is the SiO masers in circumstellar envelopes (43 GHz). As a single dish item, SiO masers can always be done as well or better at 86 GHz, so the question really involves the VLBI of these objects. Little VLBI has been done at 43 GHz, and the prospects are for even less because of the rapidly improving prospects of VLBI at 3 mm. Rank = 10.

5) 70 to 115 GHz
CO studies of all kinds; molecular spectroscopy of all kinds, at unprecedented single dish resolutions and sensitivities to low surface brightness. A 70 meter class instrument would fill the void in the 3 mm window left by the demises of the NRAO 25-meter and Algonquin 46-meter resurfacing projects. CO studies of all kinds would be superbly addressed. Most of them do not require the 230 GHz lines, the primary need being high resolution and high sensitivity to low brightness; thus the comparison to make involves the 9 arcsec resolution of a 70 m dish at 115 GHz vs. the 11 arcsec resolution of the IRAM 30 m at 230 GHz. Of course, the two would complement each other. Rank = 2.

I conclude that a 70-meter telescope designed to work optimally in the 20 to 43 GHz region is highly mismatched to scientific needs. Scientific needs are best addressed by either a very large low-frequency dish (hope-fully working to 6 cm), or a dish aimed at the 3 mm window. The latter may be politically difficult, and may also be poorly matched to the GreenBank site, but still would be scientifically very important at a good site.

From: TUCVAX::DEMERSON "Darrel Emerson" 8-DEC-1988 11:25
To: KKELLERM
Subj: Very slightly revised LSD memo

Memo:

Subject: Thoughts on the replacement for the 300 ft. telescope.
From: Darrel Emerson

7 December 1988.

300 ft. replacement: a closely packed array of smaller elements

The arguments in favour of building a single, large dish are summarized as:

1) A large collecting area is available, particularly important for pulsars and other confined objects where the highest possible sensitivity is required.

2) High sensitivity to low brightness extended emission. Synthesis instruments perform notoriously badly in this respect. High sensitivity to extended emission requires that the collecting area be centrally concentrated, rather than spread thinly in the UV plane.

3) Capability of mapping low brightness objects over a very extended field. Most synthesis mapping to date has been limited to one, or at most a few, primary beams. We need an instrument able to map much larger fields.

4) Versatility. With a single dish, only one (perhaps two) receivers are required per waveband, and it is much easier to build quickly a single new receiver for the single dish, than to outfit a multi-element array, should some new discovery unexpectedly make a new frequency band important

I suggest that in all above respects, an antenna consisting of a number of smaller elements, phased together, will actually produce higher performance.

1) Sensitivity to confined sources (pulsars, etc.). This just goes as collecting area. The smaller elements would be phased together to produce a single i.f. . A single backend (de-disperser etc.) will suffice for the complete phased array. More collecting area is likely to be possible with a number of small antennas, than with a single element (money, engineering problems of huge structures, wind, gravity etc.) It is assumed that the cost of building a dish of diameter D increases faster than the square of dish diameter.

2) An arrangement of closely packed antennas, using the zero-spatial-frequency responses (i.e. total power) as well (perhaps) as the cross-correlation terms, will have at least as good a sensitivity as a single dish of the same collecting area, but will have a potential observing efficiency advantage because of the additional multiple-beaming (synthesis) possibilities. Some thought needs to be given to the design such that the effect of shadowing of adjacent dishes is minimized. Because the auto-correlation components will be used as well as the cross-correlation terms, there will be o absolutely no compromise in measurments of structure more extended than the individual primary

beams. Spatial frequency terms corresponding to baselines greater than a dish diameter but smaller than the gap between adjacent dishes would be attenuated, so the separation between elements should not exceed twice the dish-diameter. This should easily be realizable, with minimal degradation from shadowing at low elevations.

3) (a) Phasing a number of smaller elements together to produce a single beam, which can be scanned both electically and mechanically, will provide excellent large-scale imaging capabilities. There is much more control over the effective beam shape, and algorithms such as CLEAN will work even more effectively (due to the better UV sampling) than on the VLA. In practice, several simultaneous "single beams" will be available.

(b) Treating the collection as a more conventional synthesis instrument, but using auto-correlation as well as cross-correlation terms, large scale mapping will be possible using the techniques already being developed for the MMA.

The phased elements will produce SIMULTANEOUSLY mapping beams corresponding both to the individual element primary beam and to the synthesis beam of the whole array.

4) Since the antenna will be used at relatively low frequencies (~< 50 ?) the problem of maintaining high performance receivers on a relatively large number of antennas is relatively trivial. (!) This is not true at higher frequencies. Today's generation of low-frequency receivers are relatively simple, broadband, and virtually noiseless.

5) Reliability. If one of the individual elements fails, then the effect on the overall performance of the system is minimal - just a small fractional decrease in total collecting area. If one element from a conventional synthesis array fails, the effect on UV coverage is much more serious, and of course if a receiver on a single large dish fails, then the whole system is down.

6) Should there be a desire to go for higher frequencies (say 70-115 GHz, or even just the 30-50 GHz band), this can be achieved more practically with a number of small, high-precision dishes than with one, huge, high-precision dish.

7) It would be practical to extend the collecting area of this closely-packed array, at some future date, simply by adding more dishes. Clearly the collecting area of a single, huge dish could never be increased beyond the initial design.

Conclusion:

It seems to me that in every aspect of performance that would cause one to chose a large single dish, a phased array design would be superior to a single large dish. The array should be designed at the outset for high sensitivity to extended structure, and NOT, as is the case for all existing arrays, for high resolution. The optimum number and size of the individual elements is an engineering choice - e.g. 16 VLBA-type elements would give double the collecting area of a 70-meter dish, but probably 25-m is not the optimum diameter.

There is NO compromise in performance with this closely-packed array, compared with a large single dish. The techniques to be devloped for this (large scale mapping algorithms, multi-channel correlators) will in addition contribute to the future MMA.

It will, I believe, be possible to obtain a larger collecting area for lower cost, in a shorter time, with a much higher upper frequency cut-off, with an all-round superior performance, if a closely packed phased array is constructed in preference to a large single dish.

Doc. 6

Memo to: P. Vanden Bout, G. Seielstad
From: K. Kellermann
Subject: Antenna Costs

The following estimates of the costs of large steerable antennas have become available since my Nov 28 memo (number in parenthesis is wavelength limit):

1) 100 meter (2cm) Lee King has scaled the VLBA design to estimate the cost and performance of a 100 meter antenna. Adding the cost of the subreflector, foundation, and contingency I come up with 59 M. This design is limited by gravitational deformations. Operation at 2 cm requires moving the subreflector to keep it a the optimum position. A further improvement in performance can be achieved by using the order of 60 motors to adjust the surface. This could be cheaper than introducing an homologous design.

2) 100 meter (1.3 cm): Scaling the cost of the above design by f-0.7 (JPL empirical law) suggests a cost of 80 M for a 1.3 cm antenna.

3) 300-ft (6 cm): RSI has estimated the cost of replacing the 300-ft as 6.74 M plus an additional 2.9 M to make it steerable in azimuth. Allowing for contingency would bring this to a total of 11.6 M. Note that this structure still has a 30 degree elevation limit. Considering this constraint, the RSI estimate is roughly consistent with the cost of an all-sky 6 cm 300-ft instrument estimated by the Fisher method of 15.8 M.

4) 450-ft (6 cm): Scaling the 300-ft dish by a 2.7 exponential law increases the cost to 35 M for the limited elevation instrument and 47 M for the full sky instrument.

5) 100 m (3 cm): JPL has a cost estimate from Ford Aerospace of 91 M. This is much higher than the numbers we have been considering, but can probably explained by the DSN requirements on slew speed, operating under high wind conditions and other gold plating that distinguishes JPL antennas from radio astronomy antennas.

6) 100 m (1.3 cm): MAN in New York has given an estimate of 38 M for reproducing the Effelsberg telescope in the United States as a joint effort between MAN and an American company. Note that this is about 10 M less than the estimate received about a year ago via MPIfR. I had incorrectly assigned this earlier estimate to Krupp/MAN whereas in fact it cam from Krupp (Germany) only.

The reality of the MAN estimate may be reflected in the cost of the 32 meter dish that MAN is actually building at Cambridge for VLBI. MAN gives the cost of this antenna as 7.1 million dollars, which I find to be very inconsistent with their estimate of 38 million dollars for a 100 meter antenna with similar specifications. Using a 2.6 power exponential scaling law, the

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cost of the Cambridge antenna would suggest something like 140 million dollars for a 100 meter antenna.

Summary:

We can probably build a copy of the 100 meter Bonn dish for 40M to 50 M. or for the same price a fully steerable 140 meter telescope good to 6 cm. For 50 M to 60 M we can make it a little better than the Bonn dish, or a little bigger; but probably not both. For reference the following dish efficiencies (referred to 100 m aperture) have been measured at Bonn (Altenhoff and Wink 1988).

Wavelength	Efficiency
6 cm	47%
2 cm	36%
1.2 cm	21%
0.7 cm	16%
3.5 mm	5%

From: OUTBAX::VAX3::TCORNWEL 6-DEC-1988 14:38
To: PVANDENB,TCORNWEL
Subj: 300' replacement

Dear Paul,

Along with everyone and his brother, I thought that I should send you some comments on the 300' replacement. I shall try to concentrate on arguments which have not been made before.

I have heard a report on the GB meeting last week so I think I understand what the science is. It seems that with the possible exception of the nearby HI observations, all the science can be done with either a conventional big single dish or a very compact array. I do think that it is ridiculous to suggest building a very novel telescope of either type. This means that a 100m offset paraboloid is out! My feeling about the comparison between the two types of telescope is that, theoretically, both could do a good job. By this I mean that for the single-dish we would have to develop focal plane array technology substantially before it could compete with the array in imaging speed, data quality and the ability to correct certain errors like phasing and RFI. To take one example, selfcal for single dishes requires critically-sampled focal plane arrays, the like of which we will not be able to build reliably for at least a decade. Selfcal for arrays works now. Similarly, imaging (which is not a huge part of the science, but is important) is slightly more awkward with focal plane arrays than with a compact synthesis array. No doubt we can improve this but it will take time and effort. RFI rejection with a synthesis array will always be better, not because of fringe/delay descrimination which also exists for a single dish (it corresponds to different focal points--that's all), but because first, the elements can be build with very low sidelobes if desired, and second, we know exactly what the synthesized beam is at any point on the sky (WSRT can remove Cygnus A 50 degrees away--try that with a single dish). This does, however, require some development. Overall, the technology behind a compact array is conservative; we could build one now:

- 25 x VLBA dishes	= $40M, say
- Correlator from VLBA	= $5M, (ballpark)
- Computing	= $10-20M (do it right)
	$55-65M

Some other odd points in favor of an array which have not been raised before are:

- It would give us an additional high frequency VLBA site even when the whole phased telescope is not used.
- It would be a good test bed for MMA techniques such as mosaicing. We would get good short-spacings for the VLA straightaway.

Beyond that I have nothing more to say about the technical arguments for a compact array. Darrel's memo of last week summarizes these very well. I agree whole-heartedly that there is no scientific compromise in building a compact array, and there are a great number of advantages. We could build a single dish, but, compared to the array, it would be rather poor in a number of areas.

Putting aside the technical arguments, it seems to me that there are a number of arguments for building a single dish. In decreasing order of importance:

- All our interferometer people are busy with the VLBA. We don't want to distract them now. Hiring more interferometer people is essentially impossible: all the good ones are taken by us or by other places like the AT.
- It is probably cheaper to operate. This may not be true if it were to be equipped with good focal plane array(s). Building something with high operating costs may well destroy the whole of NRAO since we would then not be able to do anything well, but rather we would be in an exaggerated version of the current situation of doing a number of things poorly.
- Byrd and Rockefeller may feel happier with a big single dish flanked by the flags of the US and of West Virginia. Only you can judge this one: given the irrational way this whole thing has gone, this may indeed be an important point. We should not destroy NRAO over a dispute between single dishers and interferometrists.
- We have not shown that an array can be easily used for spectral work. Now this could go both ways. The MMA must be an interferometric array and it must do spectral line well and simply. So we have to solve these problems. However, solving it now would be a big distraction from the VLA/VLBA.

I don't really want to support one or the other: the array wins easily on technical grounds but the other arguments for the single dish are quite strong. If the 300' have not fallen down, and if we were not in this terrible situation of having to build this new telescope while the VLBA is being built, and if it did not have to go in Greenbank, then I would be totally opposed to a big single dish. In the current situation, I cannot say that the choice is so obvious, but, I prefer the single dish option.

I do suggest that if we do build the single dish, we should find some way of setting up a group at Greenbank to work on advanced single dish techniques such as the use of focal plane arrays. Rick Fisher once suggested something like this at one of our workshops on future instrumentation long ago. We are building up Socorro to be the center of excellence in interferometry, so why not sell the idea of Greenbank as a center of excellence in single dish observations. Obviously, if this project goes ahead, NRAO will be at Greenbank for at least 25 more years, so we should attempt to do it properly.

Tim Cornwell

Xerox:

KIK

Brown

Bridle

Tim

From: CVAX::AWOOTTEN "Al Wootten" 6-DEC-1988 10:11
To: GSEIELST
Subj: Replacement dish comments from G. Heiligman (XRAY@LL.ARPA)

Subj: New NRAO dish requirements

I have a lot of thoughts about the Green Bank replacement dish. I'm not sure any of them are worth beans, but here are the chief ones.

1) Design the instrument for cm-waves primarily, but give it a "light bucket" mode at higher frequency. Thus you take advantage of the National Radio Quiet Zone-- which is Green Bank's big plus over other sites-- but still have a front-line research facility. Green Bank is probably well suited to operations on a two-season system: cm and meter waves in the summer and fall, 1.3 cm and mm waves in the winter and spring.

2) As far as I can tell, pointing ANY dish to an accuracy better than 20 arcsec is a very tough proposition; so I would go for 20+ arcsec resolution rather than try to push the technology in this rather inelastic dimension. This means that the full dish should be nearly diffraction-limited at 1.3 cm; maybe you want to make the inner 50 meters good at 43 GHz; and beyond that it's purely a light bucket.

3) For pulsars, variable QSO's, etc., keeping continuity with the 300' archival data is very important. You want to make sure that you can repeat with the new dish just about any experiment you did with the older one. A minimum 300-ft aperture should be a hard requirement; 101.6 m (4000": the largest full steerable telescope on earth) has a certain political appeal to it.

4) For Heaven's sake, keep the QUASAT/Radioastron people out of this! A new NRAO instrument, at $5"M, is 1/10 of QUASAT... and the annual operating budget is probably 1/100 as much. If the VLBI people need a big ground-based dish before QUASAT data is really useful, fine; build it with QUASAT money from NASA, not 300-ft money from NSF.

5) KISS (Keep It Simple, Stupid!) The temptation to design the "ultimate" radio telescope must be fought; astronomy is much better served with a modest instrument soon than with a superb instrument later.

5) Why not spend 10% of the money on an interim dish while the 300-ft replacement is under construction? This worked in 1962, it might work again.

Gary

From: CVAX::ABRIDLE 5-DEC-1988 12:39
To: @REPLACE,ABRIDLE
Subj: My reaction to Dec 2/3 meeting

Here are some observations and conclusions based on what I heard at the Dec 2/3 meeting at Green Bank.

A. Array vs Single Dish

The advantages of an array are:

1. Can provide large total aperture without the structural design innovation needed for equivalent monolithic antenna. This dominates choice if the required total aperture much exceeds equivalent of 100-m diameter.

2. Reduces pointing problems, wind loads for given final resolution.

3. Small elements might use conventional offset-feed geometries to minimize aperture blockage and get very clean primary beam.

4. Can place some control of beam shape in hands of observer.

Tradeoffs are about even on:

1. Speed, and complexity of electronics, for large-area surveys (if the single dish uses array feeds for such work).

2. Initial construction cost (at about 100-m effective aperture); dish needs more structure, array needs electronics and computing. Much above 100-m aperture, array should win easily because dish requires pioneering design.

3. Self-calibration of atmosphere. Dish must have array feeds and a large correlator; array has what it needs anyway. Techniques are better developed for arrays, but principles are well understood for dish also.

4. Both can provide high surface brightness sensitivity and zero spacing data if all auto and cross correlations are used in the array.

The advantages of a single dish are:

1. Can keep electromagnetic path very clean by dismounting all unwanted receivers and feeds whenever it is important to have low sidelobes, little stray radiation and RFI, flat spectral baselines. Array elements get cluttered in practice because there is operational pressure to leave equipment for all wavelengths in place on all elements all the time.

2. Can make better use of state-of-the-art receivers, i.e. can run with prototypes and/or devote all maintenance resources to keeping a small number of packages in tip-top shape. Faster response to innovative receiver design is possible.

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3. Re-engineering of feeds and receivers is much cheaper because there are fewer of them.

4. Can be maintained and operated by less people, as there are fewer item to be maintained and attended to.

B. RFI performance

Green Bank's "trump card" as a site is the Quiet Zone, and much of the exciting low-frequency science (high-redshift HI, multifrequency pulsar work, etc.) requires exemplary RFI rejection capabilities. We should plan eventually to do whatever we can toward interference excision by signal processing. But we must get off to the best possible start by emphasizing RFI performance and primary beam cleanliness in the design of the antenna(s). The RFI environment will only get worse with time, so we must invest as much as possible now in design that will reduce far-out sidelobes.

The enormous generic advantage of interferometers for RFI rejection is based on fringe rate and delay discrimination. These advantages vanish asymptotically for compact arrays, though some use can still be made of them in practical finite arrays if the RFI is impulsive.

The worst RFI signals are from satellites, against which very clean beams are needed as the first line of defence. An array of small elements could use offset-feed technology to maximize clear aperture and so minimize RFI acceptance through far-out sidelobes. But an extremely compact array might negate this for much low elevation work because of aperture blockage and scattering off adjacent dishes. RFI rejection would be best for a not-too-compact array of offset-feed dishes working near the zenith, or at azimuths and elevations for which blockage had been specially optimized (e.g. as one might do for the Galactic Center).

It will be difficult to use offset-feed technology for apertures of order 100m, except by illuminating off-axis sub-apertures from an on-axis minimum-blockage feed support (as was proposed for galactic HI work with the 300-ft before its demise). A new single dish should minimize use of massive feed supports, and perhaps maximize use of non-conducting guy wires with dielectric constants as close to unity as possible (are there any suitable strong materials ?) The single-tower geometry used on the Jodrell bank MkI, and the two-leg+guys geometry used on the 300-ft are preferable to a tripod or tetrapod, and modern versions of these should be considered.

A compact array would keep all the feeds closer to the ground than would a conventional dish with the same total aperture. This protects against local sources of interference getting directly to the feed, which may be an important problem at the lowest frequencies. The main RFI disadvantages of a compact array are dish-to-dish blockage, scattering and cross-talk. Most practical compact arrays (e.g. VLA D-array) have severe cross-talk problems, but none was aggressively designed to reduce this. We should be sure that we know how to eliminate self-interference before committing to a compact array.

C. Designs we should elimate now

The scientific goals presented at Green Bank ask for large apertures at low frequencies, but significant residual aperture at 3mm. I think we should therefore eliminate the following options:

1. A single 70-m class antenna going to 3mm. This will be too small to do exciting science at the low frequencies for which the Quiet Zone is an ideal location.

2. A large-aperture array of many cheap dishes operating only to 5 GHz, e.g. off-the-shelf cm-wave communications antennas. This will be cheap to construct but relatively expensive to operate, and will not service the high frequency applications.

3. A single 100-m class antenna with a conventional off-axis feed geometry. The feed tower will be prohibitively tall if the path lengths from to the dish are equalized enough that the dish can be illuminated satisfactorily by a broad-band feed with a reasonably symmetric beam. We should however remain open (for a while) to suggestions for clever geometries that would reduce the tower height without exacerbating the illumination problem.

D. What's left ?

Two possibilities occur to me:

1. An inner-panel, outer-mesh dish giving 100-to-130-m aperture at low frequencies and about 70-m aperture to as high a frequency as we can afford. We should shoot for useful performance at 3mm, but back off to 1 cm if this cannot be done at reasonable cost. The dish should have an on-axis but minimum-blockage design; we should plan to support optional slightly off-axis feeds to illuminate a fully clear sub-aperture for work that requires the ultimate in sidelobe suppression.

2. An array with one (central) element that operates up to 3mm and a surrounding ring of about 6 equal-sized elements that operate only up to about 5 GHz. The outer elements might be off-the-shelf communications antennas, and would not be used for the highest frequencies. The ring might be made reconfigurable to meet the blockage and resolution requirements of different experiments. The element size should probably be about 40-m. Possibly we could use an offset feed clear-aperture design at this diameter.

I suspect that the array would be more scientifically flexible for a given construction cost, but that it would cost more to operate, and to keep equipped with state-of-the-art receivers, in the long run. If it was provided with a "generous" computer capacity at the outset, the computer might also contribute significantly to the VLA/VLBA computing problem, and thus give Green Bank an extra role as an array computing center.

I marginally favor (1) because it would be cheaper to operate as a state of the art instrument, and so might be a better "matched filter" to the likely budget. But array options deserve a further hearing in-house, at least for a few more weeks.

From: TUCVAX::DEMERSON "Darrel Emerson" 2-DEC-1988 22:58
To: CVAX::KKELLERM,DEMERSON
Subj: RE: 300 ft. replacement telescope

I would assume that some focal plane capability would be built into the the beginning. That of course would win back some of the intrinsic speed advantage that any synthesis instrument in effect possesses.
As a small detail, I suspect that the packing efficiency of any kind of focal-plane feed system would be rather worse than the packing efficiency of a number of small antennas, but that is not really the main point.
In my mind, it all hinges on whether the cost of building an antenna of diameter D goes as D**2 or faster.
The use of focal plane arrays or now XXX not is really a separate issue.
It would always be possible to put a focal plane array on each of the smaller antennas. It would be interesting to do a cost analysis by differentiating so and looking for the minimum, taking account of focal plane arrays both in a large single dish antenna an on each of a multi-element array.

I apologize for the awful typing. I'm actually typing this on a Commodore C64 at home, and I haven't found out which is the "delete" key on this terminal emulater yet!!
I'm sorry I couldn't get to the GB meeting. Can't wait to hear how it all went. By the way, no-one in Tucson ever received a copy of your working-group report. Whichever way it goes, good luck!
Cheers,
Darrel.

From: CVAX::JLOCKMAN 4-DEC-1988 11:27
To: KKELLERM
Subj: A solution to the "long arm" problem for offset reflectors

One way to design a large offset reflector is the way they did at Bell Labs -- just take a look at the horn-reflector (the one used by Penzias and Wilson) and imagine it with less horn. There is no "long arm", there seems to be little penalty for a very long focal length, all the receivers could be in the cabin at the focus, etc. This may be the solution. The trick is to keep the focus at a constant elevation.

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December 1, 1988

Dr. Paul Vanden Bout
NRAO
Edgemont Road
Charlottesville, Va. 22903

Dear Paul:

It turns out that I will not be able to attend your meeting in Greenbank after all. There have been too many things going on here of late, and I have just run out of time. On the other hand, I do have strong feelings about the situation at Greenbank, and I want to communicate those thoughts. Apparently, an emergency allocation from congress to replace the 300 foot in Greenbank is a real possibility, and we must certainly make the best of this.

The memo from Ken's committee discusses two options that were considered. Plan A, the preferred, is a 70m class dish good down to 7mm, or possibly 3mm. I find this option entirely uninteresting. Plan B, a 100-150m low frequency replacement for the 300 foot alone, is very attractive. All of the arguments on pages 11 and 12 of the memo are compelling and need not be repeated here. A true replacement of the 300 foot antenna with a surface useable to 20 cm wavelength, and no lower, and a diameter of possibly 150m would provide a truly unique instrument for low frequency research. This instrument must reach all declinations down to the galactic center, and, if possible, should be capable of about two hours of tracking. Really new capability will be provided by such an instrument. No such large antenna is available anywhere in the world, except for Arecibo, and it can reach only about 40 percent of the sky. This telescope could reach high redshift galaxies in H and OH and study the largest population of quasars, providing important information both about the quasars and the ionized component of the ISM.

The site is a key issue here. It is the National Radio Quiet Zone. An inspection of the Frequency Allocation Chart shows that ground based interference is mostly concentrated at frequencies below one GHz. Here the NRQZ is a real asset. At frequencies above about one GHz, the allocations shift more to satellite and meterological transmissions. These signals from the sky go everywhere, and the quiet zone is less effective. Thus it is natural to exploit our quiet zone resource with a large low frequency antenna at Greenbank.

This unique low frequency telescope can probably be built for 5-10 million dollars. This represents a sensible high quality replacement for 300 foot telescope.

Plan A, the 70m telescope is a poor choice from every point of view.

1. It is not unique. The Russians are building one or more of these antennas. There is already a 100m in Germany.

2. It will be expensive, 50 million dollars. Perhaps that much money is available for this emergency. However, next year and in the future the congress and the scientific community will regard it as another 50m to the NRAO (and for radio astronomy) and will not welcome any further requests from that quarter.

3. The site is perhaps the strongest argument against this plan. Paragraph 5 of the memo notes that Greenbank has the poorest weather of any site in the U. S. for observations at short centimeter and (obviously) millimeter wavelengths. The Bell Labs 7m millimeter dish has done well for its very small group of users by being useable for a few months in the dead of winter. A national instrument must be more available. When the planned 70m telescope is put to work at low frequencies during all that bad weather at Greenbank, it will be a small but expensive antenna. If we are serious about a facility for short wavelength work, we must put it in a good location.

4. The scientific program is not very appealing. (a) The best program is space VLBI. But here we are too little and too late. The space VLBI is apparently going to be done by the Europeans and/or the Japanese. They both have or are building 70m class antennas and don't need us. (b) Pulsars can better studied with the large low frequency telescope of Plan B. (c) The microwave background must be studied at short cm or millimeter wavelengths. Greenbank is a poor site for these wavelengths, particularly for low brightness continuum, as experience has shown. (d) Extragalactic HI will be done better with the Plan B antenna. (e) Atomic and molecular spectroscopy. Most of the molecular work is at millimeter wavelengths, where Greenbank is a poor site. At centimeter wavelengths where interference is more of an issue, the VLA offers both a clearer sky and good RFI rejection. Of course, it has high sensitivity and resolution and modest extended brightness sensitivity in the D array. It is often argued that the VLA does not have good frequency agility. This is a very out of date argument. Centimeter wave receiver technology is very mature, and the cost of equipping the VLA with receivers for any wavelength is tiny compared with the cost of a large single dish. Note that the Australians have figured out how to use octave bandwidth feeds and one to 32 GHz is five octaves, the same as the number of bands now in use on the VLA. (f) Galactic HI and HII. The Plan B telescope will do a better job on HI and the low frequency recombination lines. The higher frequency more compact HII regions are being done at the VLA, both in the continuum and the recombination line, at the needed high resolution. The single dish cannot compete here. There is an enormous amount of high resolution HI work to do at the VLA. It is hard work, but that is no excuse for not doing it. (g) SETI. SETI needs collecting area, more than that of a 70m telescope. The 150m telescope will be more valuable for this program.

At this point it may be worth considering the relationship of this proposed replacement antenna with the Arecibo telescope. For that 40 percent of the sky which it can see, the Arecibo telescope has no competition. With its upgraded feed, it will be our major cm wavelength telescope for deep studies. It's present surface is 2mm RMS, and because the individual panel RMS is 0.5mm, it can probably be further improved. That makes it a solid telescope down to 2cm wavelength with an effective diameter of about 250m. Where is it weak? It is clobbered by interference at low frequencies, and its sky coverage is limited. A low frequency 150m full sky coverage antenna at (radio quiet) Greenbank is a perfect complement.

There is one technical point that I would like to comment on. The 70m antenna is to be shaped for high gain and also be able to carry focal plane arrays. For all the shaped antennas that I am aware of, these two requirements are incompatible. The shaped systems have very small regions of good image quality in the focal plane. The VLA antennas are an example. I am not certain that this is fundamental, but someone had better demonstrate the feasibility before any proposal is written up.

Let me summarize. The fact that money for a replacement antenna at Greenbank is probably available is certainly an opportunity that must be taken. A large low frequency antenna will provide a unique instrument that will best exploit the best qualities of the site. It's cost will be modest, 5-10M. Let us not buy an expensive cm wave antenna that is not unique and is a mismatch to the site, just because the money might be there. Let us not mortgage our future plans.

Good luck with the meeting.

Best regards,

Jack

Wm. J. Welch

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From: TUCVAX::PJEWELL "Phil Jewell" 2-DEC-1988 17:17
To: GSEIELST, DEMERSON, BTURNER, PVANDENB, AWOOTTEN, PJEWELL
Subj: Thoughts on a new, centimeter-wave dish(s)

2 December 1988

George -- Sorry I couldn't arrange to attend the on-going workshop on a new, centimeter-wave dish. As a user of Green Bank facilities, I am very interested in such a project and would like to provide some input on what capabilities a new dish should have. Here are some thoughts from my perspective.

Without a doubt, a new facility should have a strong capability at L-band. I am interested in work on the OH lines, for example. Some of the most interesting science these days is at higher frequencies, however, namely K-band and Q-band. We have never had a truly world-class capability in either of these bands, at any facility in this country. (The capabilities of the 140' and Haystack in these bands are very good, but we could surely use higher efficiencies and more collecting area.) I would also urge that serious thought be given to a capability in the 3 mm band, all the way to 115 GHz. I think that the weather in GB would allow observations in this band during the winter months. There are a number of ways that a 3 mm capability could be achieved without compromising low frequency performance (e.g., by using the dense-packed, phased-array proposed by Darrel Emerson [to be sent to you] or by making the inner portion of a large dish good to high frequency). Under normal circumstances, a 3 mm capability might be considered extravagant, but the usual constraints may not apply in this case.

Concerning the 30 - 50 GHz band: I support Al Wootten's contention that this is a scientifically valuable, and previously unexploited band. To Al's list of important molecular transitions in this band, I would add the 2(1,2) - 1(1,1) transition of HNCO at 43.8 GHz, which is a good diagnostic of dense regions. I must also disagree with Barry Turner's comments about the 1-0 SiO line at 43 GHz. The information in this line is NOT duplicated in the 2-1 line at 86 GHz. SiO maser studies are now concentrating on understanding the differences from transition to transition in the J ladder and the ground state transition is obviously among the most important. Also, single dish work will not be superceded by VLB work at 43 GHz. The baselines of the VLBA are simply too long for most of the nearby, well-studied maser stars. For example, in previous VLBI studies of SiO masers on the Haystakk,Quabbin baseline, much of the emission was already resolved out. This will be even more the case for 3 mm VLBI.

Phil Jewell

c: D. Emerson
B. Turner
P. vanden Bout
A. Wootten

From: 42221::BACKER 30-NOV-1988 13:12
To: NRAO::GSEIELST
Subj: Comments on New GB Dish

Here are a few comments about pulsars and a new GB dish:

D. C. Backer
29 November 1988

The key word is Sensitivity.

There are three broad areas of research: searches for new pulsars, timing known pulsars, and other pulsar investigations.

Searches will always be limited by sensitivity at decimeter wavelengths. There are some 10**5 pulsars in our galaxy and we have detected less than 500. Assuming optimum receiver/feed technology, senstivity is established by collecting area. Collecting area is probably optimized at decimeter wavelengths by an array -- the increased complexity of electronics is offset by decreased cost of elements. For some the array may provide flexibility for decimeter multibeaming, while for others there is the loss of a clean beam; perhaps it's a draw. Complete declination coverage is essential, while tracking beyond a few hours is a luxury. At 75 cm there are 77,500 beam areas over half the sky; one can survey these with 10 minutes per beam using a 10 beam instrument in 54 days. The primary field of 0.5 sr along the plane could be surveyed at 21 cm in the same amount of time.

Timing of pulsars has become an increasingly broad field with implications in fundamental physics, interstellar medium dynamics and space geodesy. The pulsar timing array experiment that we are conducting on the 140ft telescope is an attempt to establish a reference frame of millisecond and binary pulsars around the sky. This data will be modeled by standard parameters for the individual pulsars and global parameters for time, space and a primordial gravitational wave background. The global parameters have monopole, dipole and quadrupole signatures over the sky. The quality of this data is ultimately limited by the sensitivity of the 140ft at decimeter wavelengths, although at present we are limited by funds and effort required to construct data acquisition hardware that uses the full bandwidth presently available. In our most recent 140ft observation we detected a decrease in the dispersion measure of PSR 1937+21 by using observations spanning 800-3200 MHz. Multifrequency capability for monitoring is essential. Observing an array of pulsars does not require full hour angle tracking; in particular observing the globular cluster pulsars 1620-26 and 1821-24 is necessarily restricted to several hours per day at the latitude of the 140ft.

Other pulsar studies cover a wide range of activities. While studies of the intrinsic properties of pulsar radiation have been few in recent years, there continue to be good projects considered. Use of pulsars to investigate the microscale properties of the interstellar medium have produced many exciting new results in recent years. These complement parallel attacks using VLBI techniques and source variability studies. Full declination coverage is essential and of course sensitivity. In this case sensitivity cannot be replaced by bandwidth because many of the phenomena studied are narrow band processes. Unlike the areas of research discussed above long hour angle coverage is often useful for these `other' pulsar studies.

I conclude that the possiblity of a 300ft/140ft replacement with regard to pulsar studies is best satisfied by a decimeter antenna array with total collecting area equivalent to a 140m dish (root 2 times 100m). Optimization of receiver/feed is assumed. Full declination range and limited hour angle coverage (~4 hr) is required. Hopefully this could be done for a fraction of the cost of the VLD 70m. Perhaps there is room for a hybrid solution of one element of the array working to higher frequencies.

From: CVAX::BCOTTON 1-DEC-1988 08:40
To: KKELLERM
Subj: Use of the VLD in VLB

Ken,

The proposed VLD in Greenbank will be an extremely important, occasional participant in the VLBA. One of the major criticisms of the VLBA is it's relatively small collecting area. While the details of the VLD have not been worked out, it will likely have more collection area than the entire VLBA. Thus, it's addition to the array will substantially improve the sensitivity for those experiments for which extreme sensitivity is required. The addition of the sensitivity of the VLD to the outer portions of the UV coverage obtained by the VLBA will be very important for imaging complex sources.

Such a major instrument in Greenbank would also take over the role of the 140 ft. in current VLBI arrays. The 140 ft. provides sensitivity to the shortest trans Atlantic baselines which greatly facilitates the incorporation of the EVN into North American arrays.

Critical considerations for the design of the VLD for it's use with the VLBA are: 1) frequency agility and 2) rapid source changes. Also the necessary VLBA backend should be budgeted from the beginning. I would strongly urge that its useage in VLBI arrays be considered in the design of the VLD.

-Bill Cotton

From: OUTBAX::VAX1::AROTS 30-NOV-1988 17:30
To: PVANDENB, MGOSS, GSEIELST
Subj: 300-ft

Here are just some random thoughts on the 300-ft replacement. They neither pretend to be profound, nor complete, but may help in the discussion.

As I see it, there are three types of instruments that could replace the 300-ft, but before I get to that I would like to stress the issue of frequency coverage. There is an obvious lack of low-frequency capability in the U.S. The VLA now covers the 327 MHz band, and maybe we'll have 75 MHz some day, but that is a far cry from covering everything between, say, 75 and 1420 MHz. My own interest, of course, is red-shifted HI. The Green Bank site has some unique properties in this respect, and I think we should take full advantage of them and emphasize low-frequency work there. What the maximum frequency should be will depend on the type of instrument we build. For a single dish it should st least be 15 GHz, but for a synthesis instrument we may not want to go any higher than 5 or 8 GHz.

In trying to define the role of the instrument in the whole of the NRAO facilities, I feel very strongly that it should be the "zero spacing" instrument - whether or not its vata are actually combined directly with those of the larger arrays (VLA, VLBA) or not; call it the high-sensitivity/ low-resolution telescope, if you wish.

1. The first type that comes to mind is a high quality large single dish, the most direct replacement of the 300-ft. The surface should be more accurate (Sebastian von Howrner's homology design - finally?) and possibly larger, it should be fully steerable, there should be feed arrays, etc. I don't think I have to elaborate this type of instrument, but it also should be capable of supplementing VLA data with short-spacing information.

2. The second type is what I would call a single-structure synthesis instrument: either multiple dishes mounted in a single plane, or a large single dish with multiple feeds illuminating different parts of the surface. This would be truly a short baseline synthesis array. Its advantages for measuring low spatial frequencies are obvious and some interesting designs could be envisaged. As far as sensitivity/speed is concerned it would out-perform the single dish design. However, it may be a little cumbersome when used at low frequencies.

3. Finally one could envisage a compact synthesis instrument, with dishes in the 10 to 15 m class. This could give the present single dish users the same capabilities they have now (or, rather, had last month) and more. With a well-designed configuration and flexibility in observing modes (mosaicing, nodding, etc.) it would be excellent for wide-field mapping and for obtaining short baseline information. Its emphasis should be on low frequency spectral line (either "real" spectral line or continuum in line mode), up to 2 (5, 8?) GHz, and almost continuous frequency coverage - say, from 75 to 1700 MHz.

I would obviously favor the third option. In my opinion it would provide the astronomical community with the most versatile and supplementary instrument possible. In addition, it would take some of the pressure off the VLA, because it could replace (or out-perform) the VLA for D-array spectral line work. An obvious problem with this proposal would be the number of antennae and the associated front-end electronics, this especially in connection with desirable frequency agility.

If the number of antennae would not be greater than 27 (but then tHesize would have to be at least 15 m), one might even consider the following: give it the current VLA correlator - which would be very well suited for the purpose - and build a new one for the VLA. You will realize that this remark is extremely tentative; I know quite well that such a thing would be very sensitive and I'm not even sure it's a good idea. But it ought to be considered and I only mention it here because this is not meant to be a public document.

As I said, there is nothing particularly profound about what I have written here. Others have said very similar things and, I'm sure, many more have had similar thoughts. But I felt that opnions had to be voiced, given the urgency and the fact that I will not be able to attend the Green Bank meeting. If any of you want me to expand on this I'll be happy to.

From: CVAX::GATEWAY::"FCLARK@UKCC" 29-NOV-1988 11:50
To: GSEIELST AT NRAO
Subj: potential big disk for GB

Date sent: Tue, 29 Nov 88 11:42:47 EDT
Received: by UKCC (Mailer X1.25) id 7954; Tue, 29 Nov 88 11:48:46 EDT
To: George <GSEIELST@NRAO>

Greetings George:

Al told me to address my opinions to you about a possible new big disk at GB to replace the 300'. I support such an idea very strongly. I would advocate a 1000 meter (i.e. full size replacement) fully steerable antenna. We need such an instrument. If anyone raises thhe issu of Bonn, the Bonn antenna is a cripple. The instrumentation has never been developed for that antenna, and one is not permatued to observe some lines on it (e.g. OH) (tell that to Barry T!).

I would argue for a fully steerable disk fo 300' size, with a surface which is good at least to 45 GHz. If I can provide any input, let me know. I have used the 140' and the Bonn antenna, and we sure could use an antenna of that class at low frequencies here in the US.

Frank

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

MEMORANDUM

TO: K. Kellerman

FROM: B. Burke BFB

DATE: 29 November 1988

RE: Your 300-foot replacement memo of 22 November 1988

This is my initial reaction to your preliminary draft. The scientific applications, summarized in Section II, are a good overview, and make a good case for a VLD. Comments on individual items: (a) The VLBI applications will surely prove highly interesting, and even without OVLBI, the large area is a significant enhancement of the VLA, and with OVLBI the large area gives a significant enhancement to the sensitivity of all systems; (b) The Pulsar observing possibilities are outstanding -- big collection area is crucial and the US has been in the lead worldwide (millisecond pulsars got a good lift at Jodrell Bank, but the US was a prime initiator). Arecibo covers so little of the sky that the case for a VLD almost makes itself; (c) The fluctuations in the microwave background must surely be detected some day, on some scale -- here the VLD is a good bet (for a Nobel prize maybe!) but not a sure thing except for gamblers (but the odds are favorable); (d) Extragalactic HI is good, solid justification; (e) Spectroscopy -- I would like to hear the case from the experts, but I'll bet it will be hard to get time on the prime mm-wave telescopes to study lines at wavelength longer than 1 cm, so the VLD need is there; (f) Galactic HI, HII -- including He, especially ^{3}He -- we've been world leaders here also, and a VLD will keep us there; (g) SETI -- of course, but in a <u>sotto voce</u> kind of way.

The parametric tradeoffs between size and precision need to be known better. I must confess to a slight retreat from my earlier position, when I favored a high-quality 70-meter instrument. If I review points a-g, above, only (e) gives a strong push for that kind of instrument, and it is not clear to me that the advantage over the NRO, JCMT, and IRAM instruments will be significant, since at millimeter wavelengths they fill their beam in many instances. I would not go for a special-purpose HI dish either; VLBI support is too interesting. Recall Von Hoerner's theorem: a (100-meter) dish that won't blow down in the wind and won't fall down when it snows will automatically be a K-band dish.

There is a further consideration, more strategic in nature. A VLD capable of millimeter-wave performance could well be confused with a millimeter array in the minds of planners and politicians who only deal with large concepts and bottom lines. We should avoid such a possibility at all costs, and a K-band VLD would therefore be a prudent choice.

With this truth in mind, I would aim at a 100-m VLD -- 101 to make it the world's largest? -- with twelfth-wavelength precision at 1 cm (i.e. a loss of 4 dB in area from surface errors). The outline of the "LCSPA" (low cost special purpose antenna) does not necessarily specify a sloppy antenna. An antenna with K-band performance may well be possible with standard steel members, simple joints, simplest possible machinery, and reasonable accuracy specifications (these should be consistent with steel erection practices, perhaps a half inch or so, with the final corrections from the panel settings). Here is a good challenge for the engineers, and maybe for the NSF who pretend to like engineers these days: design a homologous dish within standard steel construction practice. As I remember, Sebastian's homology theorem showed that the problem is vastly over-determined; some young (or old?) Sebastian should look at that one.

I have considered, with somewhat the same depth as your memorandum, an optimistic possible cost of a 100-m telescope, scaling your numbers on pp 14-15, with allowances for lower tolerances (main savings: surface rms accuracy 0.7 mm; panel cost down by a factor of two; construction -- standard steel erection - scale the construction and erection by the 2.6th power law, then subtract 10%; same for subreflectors; Foundations and track -- scale by cube and subtract 10%). The Engineering/design, forms, and rotation amount fixed; scale cabling linearly; no service tower. There is also a "pessimistic" set of costs, with no reductions from the 2.6th power law, and with panels and focal adjustments also scaled up. The costs then are, with 15% contingency:

(Millions of dollars)

	"optimistic"	"pessimistic"
Engineering/design	1.82	1.82
Construction	30.44	33.82
Erection	4.91	5.46
Panels	4.27	10.79
Subreflector	.23	.25
Foundation and track	2.62	2.92
Installation and cabling	.49	.49
Focus and rotation mount	.25	.63
SUBTOTAL	45.03	56.18
Contingency	6.75	8.43
TOTAL	51.78	65.61

The "optimistic" estimate generated in this way may be low, but it recognizes that the pointing and rigidity requirements <u>are</u> relaxed. I have not factored in the extra cost of an off-set feed. This provision would be forward-looking, and make the project much more interesting from an engineering point of view. If one built an existing design such as the MAN 100-m, the increment would be large, but starting from a new design, it is not obvious to me that it involves much more than relatively minor structural changes.

Finally, some expressions of opinion. The NSF is our main hope; NASA and the Navy are possible friends, but neither will stand the whole cost and both are big, tough operations that know how to toss logs in our way if there is a move to make either stand the whole bill. It's an NSF problem primarily, and it is our job to seize the current opportunity and push hard and fast. This can be treated as a national emergency if all factors are considered.

From: CVAX::JBREGMAN 28-NOV-1988 17:25
To: JLOCKMAN
Subj: GB meeting

From: J.N. Bregman

To: P. Vanden Bout, R. Brown, K. Kellerman, G. Seilestad, J. Lockman

Re: Comments on a new telescope

First, my apologies for not being able to attend the meeting.

The area that has been limited greatly by telescope constraints is the structure of galactic hydrogen. Single dish surveys have provided basic information about the distribution of the ISM, but have yielded only limited information about the general structure of the cold ISM. Surveys, such as the Hat Creek survey by Heiles, revealed some "bubbles" and "worms" in the HI, but the amount of information that could be extracted from the data was limited by both spatial resolution and dynamic range. The study of detailed structure in HI both in and out of the plane is greatly limited by sidelobe contamination. Tremendous scientific advances in these areas could be made if the dynamic range of an instrument could be improved by at least an order of magnitude. In addition, one would like to have resolution better than the Hat Creek survey. These scientific goals can be met with a 100 m class dish (or larger) that is designed to minimize the contamination from sidelobes and the like. A point to note is that significant advances have been made in nearly every field when the sensitivity or resolution has been improved by an order of magnitude or greater. This type of improvement is possible for the galactic hydrogen problem.

It is worth noting that ROSAT, a European (and American) X-ray satellite will be make a detailed all sky survey in the soft energy bands (near 100 eV) at a resolution considerably greater than the Wisconsin effort in years past. This type of survey is very sensitive to absorption by neutral gas. With the less detailed soft X-ray surveys of the past, the comparison of HI and X-ray data has played a crucial role in understanding the structure of the local ISM. Analysis of the upcoming X-ray survey in conjunction with better HI data should lead to a much better understanding of the spatial structure of both the hot gas and the neutral gas in the local interstellar medium.

From: CVAX::AWOOTTEN "Al Wootten" 29-NOV-1988 16:33
To: GSEIELST,PVANDENB,RBROWN,KKELLERM,BTURNER,JLOCKMAN,HLISZT,FOWEN,RMADDALE,PJEWELL,JMANGUM,AWOOTTEN
Subj: DIR/NEW

Memo to: Paul VandenBout, Barry Turner, H. Liszt, J. Lockman, K. Kellerman & others
From : Al Wootten
Subject:NX$UY6escope for GreenBank?

A. Only a big dish can rmamly replace the 91m.

A recent memo from Barry listed some priorities for spectral regions and conclusions based on them supporting continued support for the 43m in Green Bank. I pretty much agree with his conclusions, i. e. that the scientific grist which kept the 91m going suggests a new telescope should also have a very large aperture available at low frequencies. At moderate frequencies, 5 to 25 GHz, the 43m works admirably well--it must be responsible for the lion's share of published data at frequencies of 2cm to 1cm, and hardly needs replacing.

B. The 25-52 GHz band IS scientifically quite interesting.

Because of the inclusion of this band in the paradigm MMA design, and because of the VLD discussion last spring, I have thought a bit about its uses. In the 25 to 52 GHz band, I think the scientific case is somewhat stronger than just the observation of SiO masers in late-type stars. Molecules heavier than 30-40 amus such as HC3N are excellent probes of the structure of dense cool clouds. At temperatures of 10-20K and densities below 5x10(4) or so, the strongest transitions of HC3N are the 3-2, 4-3 and 5-4 lines at 27, 36, and 45 GHz. The higher lines, at 72 GHz and above, are quite weak as typical clouds lack the density to excite them. The densities of these clouds would be well-constrained by observations of these lines. The fundamental C3H2 1(1,1)-0(0,0) line lies at 51.8 GHz, and several other diagnostically useful lines also lie in the band (2(1,1)-2(0,2) at 46.7 GHz and the 3(21)-3(12) line at 44.1 GHz, for two examples). The fundamental CS J=1-0 line at 49 GHz is also useful. In the US, this band is at present addressed by the Haystack telescope and FCRAO. I believe the 43m could operate very usefully in at least the lower part of the band, and hope that it soon will.

The low end of the band can be sensitively observed with the maser receiver at Onsala, and the high end may be observed at Bonn or Nobeyama. The combination of the Nobeyama array with the 45m is a particularly potent tool for observations in this range, but in practice the NRO instruments and the Onsala intrument are usually employed at higher frequencies except during poor weather in the summer months. I believe the common perception of this band as a scientific wasteland is due to its relative inaccessibility and consequent lack of exploitation. It's a little like the 2mm band at higher frequencies in this respect. I would rank its potential alongside the 1.8 to 5 GHz band (I would rank 7)
and above anything between
5 and 12 GHz (apologies to Rood and Bania but this band defines absolute 10 on my scale). I rank 25-52 Rank=5.

C. Since scientific priorities are strongest at lowest and highest frequencies, and the antenna deficiency is at lowest frequencies, we need a very large low frequency telescope. The 43m should be maintained and upgraded.

The VLD 70m design is a good upgrade for the 43m,
but targets scientific problems which are currently adequately addressed by the 43m, Haystack, FCRAO and potentially the MMA in the US and several facilitie
s
abroad. I believe the 43m should be maintained and its upper frequency envelope expanded to at least 36 GHz. I think access to higher frequencies will draw in more users interested in star formation and the structure of dense clouds, considerably increasing the pressure on the instrument.
I have no doubt that the user interest in the 70m described in Ken's report would be lively with most pressure at the higher frequencies, but little time would be available for pulsar or 21cm work on this smaller (than the 91m) instrument.

From: CVAX::DHOGG 29-NOV-1988 15:57
To: PVANDENBOUT,GSEIELSTAD,DHOGG
Subj: 300-ft Meeting

I am very sorry that I will miss the Green Bank meeting. I hope that both new ideas and enthusiasm for another dish will come out of the meeting,so that much of what I now send to you will be irrelevant.

The documents now being circulated offer a choice between a high frequency dish that is smaller than 300 ft and a less precise dish of order 300 ft. Much of the impetus for the "70-m" class telescope arose as a replacement for the 140 ft,and indeed it would serve admirably in that role. It is not especially well-suited to the site; it would be much more effective during its high frequency operations were it located at a higher,drier, more cloud-free site. However,apart from its role in the space VLB work,it is not in my view such a major step forward in the opportunities it offers for scientific reasearch that the high cost is justified.

Of course,replacing the 140 ft is now not the problem. I believe that with the loss of the 300 ft research at centimeter and decimeter wavelengths has been seriously set back,as I am sure will be emphasized in Green Bank. What then seems to be needed is a powerful centimeter wavelength dish that can build upon the work of the 300 ft. This requires in my opinion a telescope of comparable power,not one that is significantly smaller. Thus I endorse as a concept the BFD of Lockman,because it has the potential of being a major research tool in the fields of galactic HI,extragalactic HI, and pulsars. It might also be useful in galactic continuum,depending on its polarization characteristics,but that field is relatively less important. I think that such an instrument would have a long research life. I note in passing that a telescope on this kind is extremely well-matched to the Green Bank site,because of the radio quiet zone.

The problem with all of this is that no design exists for the instrument. I do not know what the external forces are,and how they will affect the decision-making process. I hope that we will have enough time to forge a reasonable consensus about the scientific need for a new telescope,and time to do a reasonable design effort on something like the BFD. Perhaps there are ways to be innovative and clever with it,rather than just going down the same old path.

l Vanden Bout
eve Schneider, Five College Astronomy Dept
foot replacement

tand that various possibilities for a replacement
e for the 300 foot are under consideration.
ically, I think one of the most important considerations
e for extragalactic 21 cm work. With the Arecibo
e's narrow declination range and the Bonn telescope's
success due to interference, there is no other instrument
ble of making sensitive measurements of HI over most of

of low-luminosity extragalactic phenomena--including
alaxies, galaxies of low surface brightness, and
actic matter--may allow us to better understand galaxy
on, star formation within galaxies, and cosmological
us about biased galaxy formation [illegible]
s present, the only practical approach to such studies is
cm observations.

foot was the best available instrument outside of
's decli- nation range, but even it was beginning to
limited by problems of confusion. I therefore think a
instrument, optimized for longer wavelength use would be
t choice for the 300 foot's replacement.

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