Green Bank Workshop

December 2-3, 1988

Supplementary Materials

This folder contains some of the handouts from the meeting

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE. MASSACHUSETTS 02139

July 21, 1987

Dr. Paul Vanden Bout
Director, NRAO
Edgemont Road
Charlottesville, VA 22903

Dear Paul,

This may be an opportune time to explore the possibility of building a modern replacement for the 140-foot telescope in Green Bank. The current intensive use of that instrument stems directly from the excellence of the NRAO electronics program, which is without peer in the world. Its scientific productivity is still superior to that of the Effelsburg 100-meter telescope because the NRAO receivers have lower noise, the accompanying electronic systems are better, and the electromagnetic interference level at Green Bank is far more favorable than at Effelsburg because of the National Quiet Zone, enhanced by the low population density.

The most important distinguishing quality, however, is managerial: the NRAO is flexible, responsive to the community, and is especially distinguished for its ability to respond to creative individuals who want to do things that have never been done before. This quality is not found in the MPIfR management style, and while one might upgrade the performance of the Effelsburg telescope by shipping NRAO equipment to them (if they would have it) there is, almost certainly, no chance of their adopting our more flexible way of doing things. Thus one cannot expect the 100-m telescope to serve the US scientific community, no matter what inter-agency agreements might be signed. US observers can and do use the German instrument, but it cannot play a meaningful role in enhancing the best qualities of US science.

The Green Bank 140-ft instrument has served well, but its equatorial geometry is antique, its structural flexure is dreadful, its surface quality is inferior, its maintenance is expensive and man-power intensive, and its pointing is sub-standard for short wavelength operation. It is high time that it was replaced by an instrument worthy of the beautiful receivers of the NRAO and of the US scientists who use them.

The Green Bank location, because of its unique status in the National Quiet Zone, is a special resource that is being inadequately used. As time goes by, the floor of the man-made interference will continue to rise at other radio astronomy sites. The VLA and VLBA will survive the perils of that rising tide because they are interferometric systems, and a large fraction of the man-made noise will only add a small amount to their excess noise. Single-dish systems, on the other hand, will suffer ever more serious interference, and most "remote" sites, unprotected by a quiet zone, will suffer at least as badly as the "suburban" sites because of military operations with electronic measure-countermeasure systems of ever-increasing power and bandwidth. Thus, the special qualities of the Green Bank location will be ever more essential, confirming itsstatus as a true national resource.

Interference from space-borne transmitters is already on the rise, and this raises a question of special relevance: when is the US going to develop a large antenna with sidelobes so low that the out-of-band radiation from satellites will be discriminated against? One answer might be that now is the time to develop a truly low-noise instrument in the 70-100 meter size range. It should have a clear aperture and high-quality surface, so that the diffraction and surface-defect sidelobes are low. Green Bank, with its low level of ground-based interference, is a logical place to develop a national prototype, since its inherent ground-generated level is low to start with. The NSF is a logical agency to take the lead in this initiative, which looks forward to long-term general needs and not crash-program mission-related objectives.

In many ways, 70 meters is a good choice for size since it is a standard size that makes it almost "off the shelf". The extensive experience that now exists in the DSN and elsewhere should make the cost estimates fairly secure. Naturally, one would like to edge the size upwards to 100 m; this would double the cost ($50 million est. for 70 m, $100 million, with more uncertainty, for 100 m). There are non-standard aspects; the dish should function at as short a wavelength as possible, and the offset feed will be a new feature (not necessarily expensive). I have heard that the DSN studies have shown that the cost curve for sizes above 70 meters get noticeably steeper, so 70 meters might be a reasonable choice on the basis of economics. The numbers should be reviewed, of course. The larger the dish, the higher the maintenance cost, but I believe that a modern 70-m telescope would cost no more for upkeep than the old 140-ft does.

The basic decision must be founded on scientific considerations. Here, the decision is an easy one, because almost anything the 140-ft dish does, a 70-m (with a better surface) will do better. The subscription rate for the 140-ft is an effective demonstration that it will be a scientifically productive instrument for the foreseeable future, so an upgrade will make the good science even better. A simple review of the last few years of 140-ft activity should show how persuasive the case is, and a small ad-hoc group could easily put together a detailed and persuasive case. Two additional points might be made, however: in millimeter-wavelength radio astronomy, the US is falling behind the rest of the world. The Pico Valleta 30-m, the Nobeyama 45-m, and the James Clerk Maxwell 15-m telescopes, each in their own way, exceed in their capability anything the US has to offer. A 70-m telescope, capable of 2.7 mm operation, would be a significant step in recovering the lost US preeminence. We do have a leading position in interferometry, and a 70-m telescope in Green Bank would help maintain that lead with its VLBI capabilities, but there are also significant areas of research that require a filled aperture, and many of these have also been areas of US preeminence.

I now consider the important role such an instrument would play in augmenting the VLBA and other VLBI activities. The collecting area of a 70-m telescope is nearly equal to the total area of the VLBA, and this means that studies of faint objects would be significantly upgraded when it would serve, from time to time, as part of the VLBA. One should also note that there is a worldwide "Large telescope array" (LTA?): the three DSN 70-meter dishes, the phased VLA, Effelsburg, the phased WSRT, and the three Soviet 70-meter dishes. The addition of a big Green Bank telescope should offer some interesting scientific possibilities in enhancing such a collaboration. Finally, there is a direct tie-in with space science as orbiting VLBI becomes a reality. RADIOASTRON and QUASAT are forerunner missions, and their successors would profit greatly from having ground-based VLBI terminals of large collecting area.

One might ask why the instrument should be placed in Green Bank, particularly as it might be able to observe CO. The main answer is that the National Quiet Zone is a unique asset that could never be set up again, Green Bank is a superb observatory, with an imposing array of supporting facilities in place, and in winter, after a cold front has come through, it is an excellent place for 3 mm observing. When the weather is adverse, there are many other programs that would be vying for the time. There is an important policy question at issue. I assert that it would be the height of folly to abandon the National Quiet Zone, either by an abrupt action of terminating the Green Bank Observatory or by default, allowing the facility to atrophy by benign neglect. Either action might well have deleterious consequences extending far beyond the immediate loss of the observatory.

In summary, I would urge that you set up a working group to examine the justification, cost, and specifications of a telescope that would replace the 140-ft at Green Bank. It would certainly be prudent to have the plans in readiness as soon as possible, should the proper occasion arise on short notice. The planning procedure should not make large demands on current resources. Furthermore, it would be a prudent action to safeguard a functional and extraordinarily capable arm of the NRAO. I would be happy to help, and I expect that there are many colleagues who would join us.

Very truly yours,

Bernie

Bernard F. Burke

cc: K. Kellermann
G. Seielstad
P. Thaddeus
W.J. Welch

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 2.6^{th} 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 2.6^{th} 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 are 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.

TO: Paul Vanden Bout
FROM: Steve Schneider, Five College Astronomy Dept SES
RE: 300 foot replacement

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

Studies of low-luminosity extragalactic phenomena--including dwarf galaxies, galaxies of low surface brightness, and intergalactic matter--may allow us to better understand galaxy formation, star formation within galaxies, and cosmological questions about biased galaxy formation. Since little visible light is present, the only practical approach to such studies is via 21 cm observations.

The 300 foot was the best available instrument outside of Arecibo's decli- nation range, but even it was beginning to become limited by problems of confusion. I therefore think a larger instrument, optimized for longer wavelength use would be the best choice for the 300 foot's replacement.

From:CVAX::JBREGMAN 28-NOV-1988 17:27
To:GSEIELST
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. Analys HI data should lead to a m
uch
better understanding of the spatial structure of both the hot gas and the neutral gas in the local interstellar medium.

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 H2CO (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 NH3 (23.8 GHz and lower) and H2O (22.2 GHz), but C3H2 (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 NH3 in its low-brightness, extended-emission form which characterizes its most useful diagnostic capabilities in dark clouds. NH3 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 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 (hopefully working to 6 cm), or a dish aimed at the 3 mm window. The latter may be politically difficult, and may also be poor be scientifically very important at a good site.

WHAT SHOULD WE DO?

A 70-meter telescope would have to service the following catagories:

1) Space VLBI (to which it is well matched)

2) SETI (fairly well matjor importance).

3) Pulsars and 21 cm: poorly matched. Arguably a backward step.

4) Spectroscopy: poorly matched, and much worse if it doesn't reach 115 GHz.

5) Continuum

I assumes that each of these ae time. At present, spectroscopy receives ~ 70% of 140 ft
time, plus a small amount of 300 ft time (OH and CH). Pulsars and 21 cm areas received ~ 50% of 300 ft time. Continuum receives ~ 40% of 300 ft time. Thus pulsars and 21 cm would be reduced a factor 2.5 in time and have a smalle a factor
of 2 and likewise have a smaller aperture (more confusion for surveys). Spectroscopy would be reduced a factor 3.5 in time, but would have a ~~telescope superior to the 140 ft; it is difficult to estimate the overall~~ effect on the science by
use the MPI 100-meter to complement their 140 ft work.

Obviously the space VLBI is the thrust behind the 70-meter concept, and would benefit over the 140 ft at K-band by roughly a factor of 3 in sensitivity. Most other science currently cone at GreenBank stn of the fate of the 140 ft must be considered carefuly.
Descriptions of the 140 ft as being "obsolete", "expensive to maintain", "of inadequate surface accuracy and pointing to service high frequencies", need to be examined. In terms of parts and power, the 140 ft costs only about $1 the major
expense in both cases being personnel). So the question is whether a new telescope would require significantly fewer people to operate it than does
' 140 ft. As to surface accuracy and pointing, the 140 ft performs e..irely adequately up to 25 GHz, and probably up to 30 GHznna. Above these fre quencies
the science is unimportant until one reaches the 3 mm window. It is therefore entirely unclear that more science would be produced by a new 70-meter antenna which offered a factor of 3.5 less timtter is most used. It IS clear that user
frustration in these areas would increase.

Therefore, there seem to be two possibilities for best serving the science:

1) If space VLBI is to be a driving force, we should retain the 140 ft under NSF operation, and attempt NASA funding for a new 70-meter telescope which writies,
endeavours in which NASA has a keen interest. This plan leaves the pulsar/ 21 cm science at a disadvantage.

2) Alternatively, we should build a low-frequency, very large aperture instrument that replaces the 300 ft, only better, and serves the pulsar/21 cm science. If
degree, and space VLBI perhaps to a lesser degree.

In either case, I strongly advocate retaining the 140 ft in its present operation.

	< Diffraction >	
	< 2GHz	> 5 GHz
System Temperature		
Scattering..............	Prime Focus OFFSET - or - THIN FEED SUPPORTS LIGHT WEIGHT	OFFSET CASSEGRAIN Heavy
Spillover...............	PRIME FOCUS	CASSEGRAIN
See-through.............	Mesh OK	SOLID
Rcvr Complexity.........	(Cassegrain)	CASSEGRAIN
Collecting Area		
Aperture................	BIGGER PER $	Smaller per $
Efficiency..............	FEED EFFICIENCY F/D > 0.4 (Symmetric)	SHAPED REFLECTOR
Far Sidelobes............... Interference Stray Radiation	OFFSET - or - THIN FEED SUPPORTS LIGHT WEIGHT	Mild Concern
Sky Coverage................	(Size trade-off)	FULLY STEERABLE
Tracking Time...........	"	"
Spectral Baselines..........	(Offset) Clean Structure	Offset Clean Structure
Frequency Coverage..........	0.3 to 3 GHz	3 GHz to Size Trade-off
Frequency Agility...........	Efficiency Trade-off	Optics and Diplexers
Angular Resolution..........	Moderate	Good
Pointing Accuracy.......	Moderate	Moderate to Stringent
Atmospheric Fluctuations....	Low Concern	Optical Beam Switching
Polarization................	Symmetric if > 25 dB	Offset possible
Near Sidelobes.............. Interference	Minimum Blockage Geosync Orbit Zone	Mild Concern
Field of View............... Number of Beams	High F/D	High F/D Cassegrain
Slew Speed..................	Low to Moderate ($)	Moderate

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.

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

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: 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: 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.

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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

NLSRT Memo No. 1

National Radio Astronomy Observatory
Charlottesville, Virginia

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February 2, 1988

To: Scientific and Engineering Staff

From: K. Kellermann KIK

Subject: New Large Steerable Radio Telescope (NLSRT)

During the past year there has been considerable enthusiasm from some NRAO staff members as well as several outside scientists about the possibility of building a new large steerable radio telescope. I have agreed to investigate the possible characteristics of a modern large steerable filled aperture antenna, and prepare a report for the Director discussing the options that might be available to us.

Among the topics which need to be addressed are:

1. Scientific Justification: Is there a need for a new instrument? What are the major scientific problems which will be addressed? In what way will a new instrument be an improvement on existing facilities? Some of the major scientific uses which have been discussed are:

a. Ground support for space VLBI, other VLBI;
b. Pulsars and other variable sources;
c. Atomic and molecular spectroscopy, particularly large scale features and highly redshifted lines;
d. Microwave background studies;
e. Other large scale phenomenon such as the distribution of galactic HI and HII;
f. Extragalactic HI;
g. Extragalactic source surveys and other studies;
h. SETI;
i. Provision of low spatial frequencies for synthesis observations;
j. Planetary Radar.

2. What is the optimum size, cost, and wavelength limit? Clearly there is no simple answer to this; different people will have different emphases. Do we want a (more expensive) general purpose instrument that will, in line with NRAO tradition, do everything for everyone? Or, as some have argued, do we want to build a (cheaper) more specialized instrument that might be more likely to be funded? The type of instrument we will want to consider will probably be in the range of 30 to 130 meters with wavelength limits of 1 mm to 6 cm. So far most of the interest has been expressed in 70 to 100 m class dish working to 3 or 7 mm wavelength. How important is it to reach CO at 2.6 mm?

3. What novel design features might significantly enhance the performance and/or reduce the construction or operation costs? Some items which have been discussed are:

a. Off-axis designs;
b. Very low sidelobe level to reduce interference from space born transmitters;
c. Spherical primary;
d. Three mirror system;
e. Limited sky coverage;
f. "Permanent" installation of most frequently used receivers and feeds;
g. Focal Plane Arrays for multi-beaming and the correction of surface errors;
h. Remote observing and unattended operation;
i. Active Surface.

4. What is the optimum location? This will depend to an extent on the answers to question (2). Emphasis on the shorter wavelengths might favor a high dry site, but otherwise the existing infra-structure, technical staff, and favorable RFI environment appear to make Green Bank a very attractive location.

5. Funding: We are probably talking of a price tag in the range of 50 to 100 million dollars for a major general purpose instrument; probably much less for a no frills long wavelength antenna. Krupp has estimated that it would cost 80 MDM ($50M) to replace the German 100 m telescope today, in Europe. NRAO is committed to completing the VLBA and to the Millimeter Array as the next NSF funded major project for NRAO. If we want a new large filled aperture telescope in this century, alternative sources of funding need to be investigated? What are they? Is there any support for an international project?

It is likely that any new telescope would probably replace the 140 and/or 300 foot (depending on size and wavelength limit). Thus, there might truly be no new operating costs involved, and we need to find only one-time construction costs. This is perhaps the only bright light in an otherwise apparently bleak funding picture.

6. What do we call it? The VLBA went without a name for years, and we were asked by the NSF to change the name twice during the final preparation of the proposal. I am tentatively using NLSRT because that is so bad that there is no danger that it will, by default, become the final name (as did happen with the VLBA!).

It is not my intention to begin a long drawn-out (e.g., never ending) design study, but rather to establish by the end of this year, or earlier, a conceptual design that can, if appropriate, be used to seek funding. Nor do I plan to organize extensive meetings or workshops, although I do anticipate two or three teleconferences over the course of the year to help organize our thoughts. Most of the work can be, and some already has been done by interested individuals. In order to keep those of us who are working on the study informed, I am reluctantly giving in to the NRAO tradition of starting a memo series. Those of you who wish to receive copies of the NLSRT Memo series should contact S. Mason (804 296-0224). Contributions to the memo series should be sent initially to me. Material which may be suitable for a draft report will be most conveniently received in machine readable form, preferably a **Word Perfect** floppy, or by E Mail to KKELLERM at NRAO.

At this time I would especially like to hear your initial reactions on the need for a modern large steerable dish at NRAO which may replace the aging 140-foot and/or 300-foot antennas. Specifically we need brief descriptions of the scientific need in various areas. In particular, how will the new instrument impact the scientific problems described in Section 1 (or other problems)? What are the alternative solutions? Can existing facilities satisfy, or be modified to satisfy our requirements? Would such an instrument do anything new? Is it necessary for it to do anything new? Anything being done now on the 140-foot, the 300-foot, Haystack, and the Bonn 100 m would be done much better with a modern well supported 100 m dish.

As many of us are aware, NRAO has developed, over the years, several concepts for antennas of various sizes and wavelength limits. Regrettably, none of these were ever built, but they do form a source of material that will be valuable in evaluating our present requirements and capabilities. The following reports, which may be found in the NRAO libraries, may be of interest:

J. W. Findlay, Design Studies of Radio Telescopes, February, 1965.
J. W. Findlay, et al., Progress Report on the Design of the Largest Feasible Steerable Radio Telescope (LFST), January 1966.
A 300 Foot High Precision Radio Telescope, May 1969.
J. W. Findlay and S. von Hoerner, A 65 Meter Radio Telescope, April 1972.
A 25 Meter Radio Telescope for Millimeter Wavelengths, September 1975.
A 25 Meter Radio Telescope for Millimeter Wavelengths II, July 1977.
A 25 Meter Radio Telescope for Millimeter Wavelengths III, February 1982.

Also of interest is the CAMROC proposal for a "Large Radio-Radar Telescope," (1967, 1968) and a Caltech-Berkeley-Michigan proposal for a 300 foot radio telescope. Lovell (The Jodrell Bank Telescopes, Oxford Univ. Press, 1985) gives an interesting account of their unsuccessful attempts to build the MK IV (1000 foot), MK V (400 foot) and MK VA (375 foot) antennas.

Further background and discussions of the need for a large fully steerable radio telescope is given in the Whitford (1964), Greenstein (1972), and Field (1983) Reports of the National Academy of Science, as well as the two reports of the Dicke Committee (1967, 1969). All of these studies concluded that there is a convincing need for a large fully steerable radio telescope working down to short centimeter wavelengths, but as far as I have been able to determine there have been no proposals to build such an instrument since the mid-1960's.

NLSRT Memo No. 4

May 13, 1988

To: K. Kellermann
From: F. J. Lockman
Subject: The NLSRT: a proposal for a BFD

NLSRT memo No. 1, while ostensibly a neutral "call for discussion", veered off in an unfortunate direction with its reference to the "NRAO tradition" of building telescopes that "do everything for everyone", referring, no doubt, to the 140-foot, the VLA and the VLBA. In this memo I want to invoke the other great NRAO tradition: that of making a quick, decisive move to acquire a new instrument when it would advance our facilities without costing a bundle (by current standards) or requiring a large and potentially devastating operating expense. This tradition has produced the 300-foot telescope, the upgrade of the 36-foot to the 12-meter and, in the same spirit, the HEMPT development effort and the purchase of the first CONVEX computer. I do not have to broaden the category too much to have it also include the development of the Green Bank interferometer and the construction of the 36-foot. These facilities were fairly quick and cheap, and thus could be built along with, *not* in lieu of, other projects. They been wonderfully successful – it is hard to imagine what radio astronomy would be like without them. The extension of the VLA to low frequencies may well be added to this list in the future.

In this spirit I propose that we build a large ($\sim$ 100-meter), offset parabolic reflector with full sky coverage and horizon-to-horizon tracking that would operate only at $\lambda \gtrsim 6$ cm. Let's call this instrument the Big Floppy Dish (**BFD**) to emphasize that it is not an Efflesberg-type telescope, and also because this acronym is certain not to stick. Rick Fisher discusses some technical aspects of a BFD design (including its $\sim$ \$10M cost), and a lot of other important things, in his memo of 7-Dec-1987 which is essential reading and which should be considered the NLSRT Memo No. 0.

There are several "administrative" advantages to a BFD design. First, since it operates at low frequencies, where interference is a problem but the atmosphere is not, the ideal site for the telescope is somewhere in the National Radio Quiet Zone, i.e. at Green Bank. Second, the BFD will do most everything that the 300-foot telescope can do, so there will be no point in maintaining the 300-foot telescope as a general-purpose user instrument. We could continue to operate it for special programs (ones that could be run without an operator, and that did not require frequent equipment changes) but at a very reduced cost. The savings would fund much of the operating expenses of the BFD. At the absolute worst, i.e. if we did not reduce operations of the 300-foot, the new enterprise might cost an additional $\sim$ \$0.5M a year: the cost of telescope operations and maintenance for the 140-foot. This is not a large burden.

To first order, the BFD will be a 300-foot with vastly increased sky coverage and tracking, and with much better sensitivity. This ought to be scientific justification enough. But for the querulous, here are some points to consider:

Sky Coverage. The horizon at GB is at $\delta \sim -50°$; the 140-foot telescope can follow a source for about two hours a day at declination $-47°$. The BFD will thus be able to observe 87% of the **entire** sky! It will be able to study a similar percentage of all galaxies, HII regions, pulsars, globular clusters and OH/IR stars. The galactic center will be above $10°$ elevation for $> 6^{\mathrm{h}}$ a day. In contrast, the 300-foot covers only 2/3 of the sky, and that with limited tracking. It does not reach the galactic center.

VLBI. The BFD will be a superb addition to the VLBA at cm-wavelengths, and will be essential for observations of weak objects, like pulsars to detect their annual parallax, extragalactic supernovae to follow their expansion, and weak OH masers, to measure their proper motion. The VLBA will need the additional sensitivity of a BFD for these and other problems. The BFD will be able to do VLBI with Arecibo, something that the 300-foot does not often do because of the semi-transit nature of each telescope.

Frequency Coverage. The combination of the Radio Quiet Zone and the low sidelobes of an offset design make the BFD unique for work at frequencies outside of protected bands. This is especially important for redshifted HI, but past experience also indicates that every frequency is likely to be in demand sometime, for something not previously anticipated. Who knows, maybe the BFD will be so good that it will be possible to observe the 1612 MHz OH line again. The BFD will certainly be as interference-resistent as a large filled aperture can be.

Sensitivity. The BFD will have about the same sensitivity to point sources as the VLA.

What great science would this telescope do? Basically, everything that the 300-foot now does, only over more of the sky, and with more sensitivity. Examples include:

HI in Galaxies. It will be the great redshift machine. Even now the 300-foot, with its very limited tracking and only adequate sky coverage, is very much in demand for this work. Observations of extragalactic HI, OH and H_2CO alone justify the BFD.

Galactic HI. As an offset reflector, the BFD would be a unique instrument for galactic HI work. It would have good angular resolution, and not suffer from the stray radiation that contaminates HI spectra from all other large telescopes. This capability will be especially important in the next decade when high quality HI spectra will be needed for the analysis of data from satellites like ROSAT, COBE and AXAF.

Pulsars. The use of pulsars as reference standards, for everything from calibrating clocks, to measuring the gravitational potential in globular clusters, to searching for gravity waves, is great and growing. The BFD will be able to observe > 80% of all galactic pulsars, including those in globular clusters, which are concentrated toward low declinations. With its all-sky coverage, large area, frequency agility, interference resistance and sensitive receivers, the BFD will be the premier telescope worldwide for general pulsar observations.

Zero Spacing Data. The BFD will have an exceptionally clean main beam. It should be useful in supplying zero-spacing data for aperture synthesis telescopes.

How the BFD Fits Into the NLSRT Program. Most of the items in the Scientific Justification section of NLSRT Memo No. 1 have been already discussed. The optimum design of a BFD is treated below. This makes many items in section 3 of Memo 1, *Novel Design Features*, redundant. Very low sidelobe levels follow from off-axis designs. Permanent installation of most frequently used receivers is a fact for the VLA, VLBA, and the 140-foot (for the maser-upconverter systems) and should have little impact on telescope design. The same for focal plane arrays. And our experience both at Green Bank and Soccoro suggests that the issue of remote observing, like the placement of paths on a college campus, will be decided by the users before very long.

The design of the BFD, as noted above, settles the issue of its location, and funding is a problem for administrators, not scientists. Note that the BFD, given its very modest construction and almost negligible operations costs, does not compete with the VLBA or the mm-Array. It hardly competes with the cost of operating the VLBA for a year. Finally, while the BFD may in some sense "replace the aging 300-foot" it is not because of the 300-foot's age. The 300-foot is a lot younger than I am, and is a child compared to the 200-in! The 300-foot will be replaced, or more likely reassigned, because the BFD is a **better**, not just a **younger** instrument.

Final Notes, Design Considerations, etc.. (Much of this will seem obscure if you are not familiar with Rick Fisher's memo.) A telescope with the frequency coverage of a good 25m (0 to 100 GHz) and the effective area of a 100m is a very, very useful instrument. It would certainly help my current research. But I want to argue strenuously against drifting toward a design of that type. (Of course, if some group like NASA or the Soviet Union wants to *give* us an Efflesberg clone we should be ready to receive.) It is probable that the requirement of high surface accuracy and large diameter immediately leads to a > $50M pricetag, with all the problems and delay that implies. It may be too much to ask that a telescope combine a very good surface with a very large surface. At Efflesberg, only the inner portion of the dish is used at the highest frequencies, but they still have to drag the outer, unused portion around, with its contribution to wind loading and cost. Conversely, at low frequencies they don't need the very accurate pointing and surface that must be built in to accommodate high frequency work.

A great increase in cost must come when some large fraction of a surface is required to be solid rather than composed of Sears' best chicken wire. Wind and snow loading become much larger, which requires a stiffer backup structure, stronger bearings, more torque in the motors, and there we are, marching down the path to the 100m. Thus, a good BFD will have a mesh surface and that keeps operations below about 5 GHz.

On the question of offset vs. on-axis parabaloids, however, I come down firmly on the high-tech, off-axis side. Why build a big aperture only to block it? Why construct a telescope with built in far sidelobes when the sky and ground are increasingly filled with transmitters? Why not put the effective area where we want it to be – pointed at the narrowest part of the sky? Main beam efficiencies of order 98% should be achievable and, besides making the telescope a gem for galactic HI, this will make a 10K total system temperature (at L band) really possible. Right now > 25% of the L-band system temperature (on the 140-foot) comes from the ground via scattering and spillover. Rick Fisher discusses how a large, floppy telescope could be made from flat surface panels, and how that reduces the problem that asymmetric designs have a high surface panel cost. The curvature of an off-axis dish is smaller than that of an equivalent size on-axis dish, so flat panels are an even more suitable approximation to the desired shape.

I disagree with Rick on just one point. It is OK to restrict slew rates, coverage close to zenith, and operation in the occasional high wind. But I think that it is not at all satisfactory to have a *lower* limit on the allowed elevation angle. The telescope should be able to observe to negative elevation (i.e. in the dirt) at least to the South. We should not give up sky coverage unless it seriously compromises the design or seriously increases the cost.

How to get it done. I would like to see some size vs. rms surface accuracy graphs for designs of a *fixed cost*! What exactly are the compromises necessary to reach 1 cm, or 6 cm for that matter. How much smaller does a telescope have to be if the same bucks are spent for an offset or on-axis design? What is the cost for the ability to track within 10° of the zenith? How about 20°? These are questions that a curious engineer could answer. If we can get a little money to such a person then we could begin. The important thing is to move quickly, not let it get out of hand, and not make a BFD out of the BFD.

report from the Associated Press, however, it was not a slapdash expansion of a smaller dish; the latter was actually a separate, 42.5-meter instrument of similar vintage that is still in operation.

As for deferred maintenance, says Vanden Bout, that is a real problem for the observatory. "But we confine it to things like roads," he insists, not items that would really matter to the science. In particular, he says, "the 300-foot got inspected regularly. We repainted a section of it in rotation every summer, like on a bridge. We kept the bolts in good condition. I can't say that because of lack of money we didn't do what we needed to do."

Whatever the final explanation, he says, it will probably have to await the results of a formal inquiry now being organized jointly by the National Science Foundation, which funds NRAO, and by the Associated Universities, Inc., the university consortium that operates the observatory on behalf of NSF.

Meanwhile, the 300-foot telescope itself will be sorely missed by the astronomical community. It is by no means the only large radio telescope in the world. The 305-meter radio telescope at Arecibo, Puerto Rico, for example, is more than three times larger. But it is the only one to combine such a large size and sensitivity with the ability to see all the sky in the northern celestial hemisphere. (Arecibo is immobile, and is thus comparatively restricted in what it can see.)

Perhaps its most dramatic finding came in 1967, shortly after pulsars were discovered: it was the first radio telescope to detect the furiously rotating pulsar at the center of the Crab nebula, which is the remnant of a supernova that exploded in 1054. But in the main, says Vanden Bout, "it was not an instrument for big breakthrough discoveries. It was a survey instrument, a road map instrument." Indeed, on the night of its collapse it was within a week of completing a new map of the entire northern sky at the 6-centimeter wavelength. It was much in demand for such activities as a survey of galaxies at high red shifts, or a survey of neutral hydrogen in our own galaxy and in other galaxies, or in one notable case, a survey of radio sources that might prove to be new gravitational lenses.

Ironically, at the time of the collapse, the NRAO had already embarked upon a study of possible replacements for the 300-foot telescope, as well as for the 42.5-meter instrument. Officials are hesitant to say what form the replacements might take—or how much they would cost—but the events of 15 November have clearly given them an incentive to complete their report.

■ M. MITCHELL WALDROP

Collapse of a Radio Giant

"It was a very pretty telescope," says National Radio Astronomy Observatory (NRAO) director Paul Vanden Bout, with more than a trace of sadness in his voice. "It was light. It had a lacy structure." It rose out of a remote mountain valley near Green Bank, West Virginia, overshadowing its companions at the NRAO facility there. It was one of the largest radio dishes in the world.

And at 10 p.m. on the clear, calm night of 15 November, it collapsed. With no warning whatsoever, its two supporting pylons gave way. The great white mesh paraboloid, 300 feet (92 meters) in diameter, crumpled downward into a tangle of steel spaghetti. The falling debris tore open the roof of the control room underneath, sparing the computers and other equipment inside, and leaving the telescope operator frightened, but unscathed.

"We're baffled," Vanden Bout told *Science* shortly after his initial survey of the wreckage. "There are probably as many ideas around [about what happened] as there are astronomers."

One conjecture is that the telescope may have been shoddily built in the first place. Another is that the telescope may not have been properly maintained, particularly with NRAO's chronically tight budgets in recent years. Vanden Bout, however, does not subscribe to either conjecture at this point. It is certainly true, he says, that when the telescope was built in 1962 it was considered a stopgap instrument, a way to get the then-fledgling observatory up and running as quickly as possible. Construction was rapid and cost only $850,000—cheap even then. Contrary to an early


Wide World

After the Big Bang.

Wednesday
November 30, 1988

the Charleston Gazette

Partly cloudy today with highs in the mid-40s. Variable cloudiness tonight with lows in the 20s. Details on Page 8A.

PARTLY CLOUDY 30¢

Telescope replacement speed urged

WASHINGTON (UPI) — Sens. Robert C. Byrd and Jay Rockefeller said Tuesday they want to see a proposal by January for the replacement of a destroyed radio telescope at Green Bank, W.Va.

The West Virginia senators met this week with officials of the National Science Foundation and the National Radio Astronomy Observatory to discuss the replacement of the 300-foot telescope, which collapsed two weeks ago.

"We told them we want to see a replacement telescope in West Virginia," Byrd and Rockefeller said in a joint statement.

"The message we delivered is that this type of telescope is important to scientific research, we think the telescope should be replaced as quickly as possible with state-of-the-art equipment, and we want to see it replaced in Green Bank," the senators said. "Now it's up to the experts to come back with their recommendation."

Officials told Byrd and Rockefeller the cause of the instrument's collapse has not yet been determined. The telescope was one of the largest of its type in the world.

"Green Bank is a unique research site — and an ideal location for a radio telescope — because it is a national radio quiet zone. We cannot afford to lose any time in moving forward with replacing this important scientific resource," Byrd said.

Observatory officials told Byrd and Rockefeller that plans for a replacement telescope would be drawn up following a meeting at Green Bank later this week. At the meeting, scientists from around the country are expected to discuss the needs of the scientific community.

"This is an enormously important facility. I believe that the scientific community worldwide wants to see a new telescope constructed at Green Bank. The basic data base of astronomy came from that dish, and now there is a void," said Rockefeller, who toured the site of the collapsed telescope last week.

THE POCAHONTAS TIMES
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GREEN BANK

From Senators Byrd and Rockefeller

Washington, D. C. — U. S. Senators Robert Byrd and Jay Rockefeller have directed the National Science Foundation and its scientific advisors to "fast-track" plans to replace the collapsed 300-ft. radio telescope at Green Bank.

At a meeting in Byrd's U. S. Capitol office last Monday the two Senators called on the directors of the National Science Foundation and the National Radio Astronomy Observatory to develop a proposal by early January to replace the telescope at Green Bank.

"We told them we want to see a replacement telescope in West Virginia," both Byrd and Rockefeller said.

"The message we delivered is that this type of telescope is important to scientific research. We think the telescope should be replaced as quickly as possible with state-of-the-art equipment, and we want to see it replaced in Green Bank," the Senators said.

"Now it's up to the experts to come back with their recommendations."

The telescope, one of the largest radio telescopes in the world, collapsed two weeks ago. Officials told Bryd and Rockefeller that the cause of the collapse has not been determined.

"The loss of this telescope dealt a severe blow to the scientific community and to the research being conducted at the National Radio Astronomy Observatory in Pocahontas County," the Senators said.

"We need to put plans for replacement on the 'fast-track.'"

Among those in attendance were Erich Bloch, Director of the National Science Foundation, Dr. George Seielstad, Site Director at Green Bank, Paul VandenBout and Robert Hughes, both officials of the organization that oversees university-sponsored scientific research at Green Bank and elsewhere.

"Observatory officials told Byrd and Rockfeller that plans for replacement of the telescope would be drawn up following a meeting at Green Bank later this week with scientists from around the county to discuss the needs of the scientific community.

Rockefeller, who toured the site of the collapsed telescope with Observatory officials, said "This is an enormously important facility. I believe that a majority of the scientific community worldwide wants to see a telescope constructed at Green Bank because the basic data base of astronomy came from that dish, and now there is a void."

"Green Bank is a unique research site—and an ideal location for a radio telescope—because it is a National Radio Quiet Zone. We cannot afford to lose any time in moving forward in replacing this important scientific resource," Byrd said.

Almucantar Radio Telescopes.

The 300-foot radio telescope at Green Bank, WV, was a transit telescope steerable in altitude. Its recent collapse raises anew the question of whether steerability in azimuth might not be better suited to the construction of large radio telescopes. Azimuth steerability ensures the constancy of the gravity vector relative to all structural elements at all times. Pointing and tracking is achieved without working against gravity, and without significant loss of sky coverage. The reduction of structural stress promotes inexpensive construction of larger reflectors to a given tolerance. Any source on an almucantar (i.e. a parallel of altitude) is accessible to the beam, while the Earth's rotation sweeps the almucantar across the sky. Sources are accessible on both sides of the meridian, and a modicum of tracking is afforded by azimuth steerability and movable feeds. The burden of feed support may be displaced from the reflecting surface and its infrastructure, and borne entirely by the ground.

As early as 1961, Bracewell and Drake had considered azimuth-steerable telescopes with segmented surfaces (1). In 1963, an azimuth-steerable paraboloid was proposed in order to rescue the 600-foot radio-telescope at Sugar Grove, WV from the feared collapse of its infrastructure (2). In the same year, North American Aviation Inc. studied the feasibility of such a telescope (3). By 1967, further studies had been made by Talen (4), and several designs had been developed by the Largest Feasible Steerable Telescope group at the National Radio Astronomy Observatory (5).

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